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Polarized Federalism: Activists, Voters, and the Resurgence of State Policy in the U.S.

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Polarized Federalism:
Activists, Voters, and the Resurgence of State Policy in the U.S.

by

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Abstract

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This dissertation investigates the causes and consequences of policy *variation* and policy *polarization* in the U.S. states. Chapter 2 describes policy change over time. Since the 1970s, state governments have implemented important policies while national policymaking has slowed, such that Americans' relationships to government are increasingly determined by their states of residence. This policy variation is increasingly associated with party control of state government, and carries major consequences for the lives of residents. Chapter 3 further investigates the measurement of policy outcomes, suggesting that prior measures may understate policy polarization in recent years. Chapter 4 estimates the relationship between public opinion and policy outcomes. Results suggest that this relationship varies widely by policy issue area, with marijuana and LGBT rights policy showing strong responsiveness. While public opinion may play an inconsistent role in state policy change, Chapter 5 suggests that changes in activist group behavior may influence legislative behavior and policy outcomes in the states. The implications of this dissertation challenge traditional theories of federalism and state politics, suggesting that organized and well-resourced political actors, not ordinary voters, have collapsed American politics into a single national arena of contestation over the direction of public policy.

For Mamá, Dad, Matty, and Aimee

And for the Culture.

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Chapter 1

Introduction

1.1 The States Matter

In a 2011 phone call with a radio host impersonating David Koch, Wisconsin Governor Scott Walker explained that he was part of a national movement of conservative governors who "got elected to do something big" across their states (Newell 2011). Democratic governors have similarly called for coordinated efforts by Democratic state governments to oppose initiatives by the Trump administration and Republican Congress. If their rhetoric is to be believed, politicians at the state level believe they are engaged in major struggles over the direction of public policy in the United States.

Despite the contentious rhetoric, political scientists have suggested that state governments are relatively marginal policymakers. Researchers have long seen the states as "the runt in the American governmental litter" (Allen 1949; Sharkansky 1968; Teaford 2002, 2), with policy agendas that are highly constrained by economic realities (Peterson 1981) and low legislative professionalism (Kousser 2005). Recent research largely continues this characterization. While some studies report important changes in state policy in the polarized era (e.g., Kousser 2002; Hertel-Fernandez and Skocpol 2016; Hertel-Fernandez 2016), the most comprehensive recent studies in this area conclude that state policy outcomes have been generally "stable" over the years (Caughey and Warshaw 2016, 7), and that party control of government still plays only a "modest" role in policy differences between states (Caughey, Xu and Warshaw 2017, 1).

Yet there are reasons to expect that the role of states in American federalism has expanded since the 1970s. From the 1930s though the 1970s, the Federal Government used policy to decrease differences between states in civil rights, the welfare state, and the regulation of business. Since the 1970s, however, the Federal Government has polarized and produced fewer major policies that standardize laws across states (Binder 2003; Hacker and Pierson 2010). When federal policy does pass, it is more likely to delegate discretion to the states, and the federal judiciary is an increasingly "state friendly arena" (Waltenburg and Swinford 1999, 2). Policy demanders, faced with federal gridlock, may turn to the states to pursue

their policy priorities (e.g., Baumgartner and Jones 2010). Indeed, journalists describe a strengthening relationship between an individual's state of residence and her legal right to obtain an abortion, own a firearm, join a labor union, or use drugs, as well as her tax burden, environmental regulatory regime, and generosity of the welfare state (e.g., Fehrman 2016).

This dissertation investigates the causes and consequences of this *state policy resurgence*. Chapter 2 provides empirical evidence that public policies have become increasingly varied across the states, and that this variation is driven by party control of government. Chapter 3 investigates why established research may understate the extent of interstate policy variation and polarization in recent years. Chapter 4 asks whether this state policy resurgence has been responsive to public opinion. Finally, Chapter 5 offers an alternative explanation for policy polarization in the states: the consolidation and coordination of activist groups.

1.2 Party Control Matters

Our first task is to answer two empirical questions about *policy change* in the states. First, has *policy variation* increased across the states? Expanding policy variation means that individuals' relationships to government is increasingly determined by their state of residence. Given the major slowdown of national policymaking in recent decades, this would represent a major shift in American federalism.

Second, is this policy variation driven by *party control of government*? It is well known that the parties have polarized in Congress, and there is evidence that roll-call votes have similarly polarized in the states (Shor and McCarty 2011). However, scholars argue that the *policy* consequences of legislative polarization are much less significant (Caughey, Xu and Warshaw 2017).

I argue that scholarship has understated policy variation and polarization in the states. In Chapter 2, I build upon large datasets of state policies to estimate policy variation and polarization in the states since 1970. I first use one established and three new unidimensional "left-right" measures of policy outcomes to show the expanding differences in state policies in recent decades, as well as their relationship to party control of government. Whether I use raw averages or Bayesian IRT ideal point models for measurement, the new measures show greater polarization than prior measures due to my expanded state policy dataset.

More important, I argue, is to estimate policy variation and polarization using *issue-specific* measures. These issue area measures, in areas such as abortion or tax policy, are considerably more interpretable than uni- or bi-dimensional measures, and serve to demonstrate the real world consequences of policy change.

When analysis is disaggregated in this way, we observe that policy polarization between Democratic and Republican states has occurred in 14 of 16 issue areas, but not in two important areas: education and criminal justice. Given that the states are the constitutional locus of education and criminal justice policy, this carries major social consequences. I show that, in contrast to polarized issue areas such as health policy, both Democratic and Republican state governments have driven the rise of mass incarceration in recent decades.

Chapter 3 further investigates the differences between measures of state policy outcomes. Again, I show that my expansion of the number and temporal coverage of state policies in the dataset leads to larger estimates of policy polarization. In addition, I ask whether latent dimensional measurement models such as Bayesian IRT are applicable to the measurement of state policies. I find that parameters from these model fits—which determine the ideological direction and magnitude of each policy in the data—often do not conform to substantive understandings of the ideological content of policy. I argue for increased qualitative attention and expert coding in the creation of summary measures of policy.

1.3 A Story of The Mass Public?

What is driving the expansion of policy differences across states? Our first suspect is the mass public. Indeed, that *state* governments are especially responsive to their constituents is a deeply entrenched piece of conventional wisdom in both the academy (e.g., Erikson, Wright and McIver 1993) and beyond.

And it is plausible that changes in public opinion over the past generation could be the cause of state policy resurgence. Since the 1970s, mass opinion and voting behavior have undergone important changes. Americans' party identification and partisan vote choice have sorted such that they are increasingly associated with ideology, policy attitudes, and racial identities. Split-ticket voting declined. Have these trends generated greater differences between the median voters of different states? And, more crucially, have such shifts in state opinion led to shifts in state policy outcomes?

On the other hand, other recent research suggests that there are major obstacles to policy responsiveness to public opinion. Conventional wisdom suggests that state-level politicians may be more responsive than national politicians because they are "closer" to their constituents—but this "closeness" may also be a barrier to accountability. Voters and media appear increasingly inattentive to state politics (Hopkins 2018). Meanwhile, there is new evidence that organized interests and activist groups have made major investments in state level politics (e.g., Anzia 2011; Hertel-Fernandez 2014)—and when these groups' preferences diverge from those of the mass public, the less professional and lower information environment of state politics facilitates these groups' influence.

I estimate policy responsiveness to public opinion in Chapter 4. Again focusing on issue-specific measures, I find that state policy changes are only weakly related to policy change. While state policy outcomes have transformed in recent decades, mass opinion in the states has remained mostly static in issues such as abortion, labor, and gun control.

However, this analysis again uncovers important variation by issue area. In two issue areas, public opinion has shifted greatly over the past generation: LGBT rights and marijuana policy. Changes in opinion in these two areas predicts important policy changes in the states. I argue that these issue areas are more straightforward and less complex, facilitating mass influence over policy. Moreover, these areas have not seen the same investment from concentrated interests that other areas, especially economic issue areas, have experienced.

1.4 A Story of Organization

If public opinion is not driving state policy resurgence, what is? A pathbreaking body of research from Alexander Hertel-Fernandez, Theda Skocpol, and others has shed light on major political investments by deep pocketed organizations such as the American Legislative Exchange Council (ALEC) and groups in the Koch network such as Americans for Prosperity (AFP). However, there has been less attention to a different form of organization that has grown increasingly active in state politics: *activist networks*.

In contrast to the super-elite organizations of the Koch network, activist networks combine financial and organizational resources with a broader base of intense issue or ideological activists. The National Rifle Association (NRA), anti-abortion groups, and environmental groups are examples of these sorts of activist networks.

As I show in Chapter 5, the rising tide of activist networks is evident in campaign finance. Individuals who donate money to state legislative candidates are now ten times more likely to be affiliated with single-issue or ideological activist interest groups like the NRA. Analysis of survey data suggests that these group-affiliated activist donors have more extreme and consistently partisan policy and ideological attitudes—and that they are more likely to contact their legislators. Organizations may further facilitate the influence of these networks by providing informational and coordination resources.

It is notoriously difficult to uncover evidence that campaign contributions affect politicians' behavior, but I find suggestive evidence that the rise of activist donor networks has had a significant influence on state governments. State legislative parties in government, as well as individual state legislators, make more consistently partisan or extreme roll-call votes when they rely on greater concentrations of activist donors.

1.5 Polarized Federalism

The implications of this study suggest a growing need for research on American federalism in the age of hyper-polarization. Both scholars and activists should be attentive to the nationalization of state politics and its implications for parties and groups who hope to shape public policy in the United States. Rather than a decentralized federalist system with vertical differences across levels and horizontal differences across regions, American governmental institutions look increasingly like a single arena of partisan combat over public policy.

Chapter 2

The Party Effect

Little research has investigated shifts in the substance of state policy over time.[1] In this chapter, I investigate two dynamics in policy in the U.S. states: increased policy *variation* (the substantive differences between states) and policy *polarization* (the relationship between party control and policy outcomes). Importantly, I investigate the substance of policy change across 16 distinct issue areas such as abortion or tax policy. I collect data on 36 state policies, to which I add data from Caughey and Warshaw (2016) and Jordan and Grossmann (2016) to create a dataset of 135 major state policies from 1970 to 2014 (see the Appendix for full descriptions of policies).[2]

The analyses show a large increase in policy variation and a tightening relationship between party control and policy change in recent years. Across each issue area, the range of state policies has increased. For instance, the difference between the most restrictive states for abortion and the least restrictive states has expanded since *Roe v. Wade* (1973). This variation is increasingly related to party control of government: Prior to 2000, whether a state was controlled by Democrats or Republicans said little about the policies it would adopt, but the parties have implemented highly divergent policy agendas after 2000.

Issue area analysis shows two important areas of exception, however, where policy outcomes have not polarized: education and criminal justice. I corroborate this finding with analysis of its socioeconomic consequences. Health and welfare policy has sharply polarized in recent years, and I find that party control of state government increasingly predicts rates of health insurance coverage. However, in the non-polarized area of criminal justice, I find no change in the relationship between party control and incarceration rates.

[1]Recent research on temporal dynamics in state policy summarizes policy with unidimensional left-right ideal points (e.g., Caughey and Warshaw 2016), which are difficult to interpret in substantive policy terms.

[2]Caughey and Warshaw (2016) include data on 102 policies for the 1970 to 2014 period, and fewer years of data coverage for many policies.

2.1 The Minimalist View of States

Nearly three decades after Elazar (1990) predicted resurgent states in an emerging "neo-dualist" era of federalism, observers point to intensifying battles over public policy at the state level. However, there has been little empirical investigation of systemic policy changes in the states over time. Though scholars are now less likely to call them the "backwaters" of American politics (Winston 2002, 106), recent literature may only focus on the states as a means to increase one's N to 50 in order to "address a domain of questions with greater statistical rigor because of the large number of states" (Brace and Jewett 1995, 655).[3]

Institutional, developmental, and historical research, in contrast, engages directly with temporal dynamics in federalism and public policy (e.g., Mettler 1998; Allen, Pettus and Haider-Markel 2004). Overwhelmingly, however, this research takes a *minimalist view* of state policymaking. John Kincaid (1990, 144), former director of the U.S. Advisory Commission on Intergovernmental Relations and a prominent scholars of federalism, describes a 20th century in which the role of states shrank and U.S. federalism became "more adaptable to policy preferences defined increasingly by the national government"—where the Federal Government moved from "senior partner" to "commanding partner" (see also Posner 2007; Zimmerman 2009).[4] By 1975, even the predominant federalism scholar William Riker (1975, 143) suggested that the existence of lower levels of government "makes no particular difference for public policy."

Additional research lends credence to the minimalist view by highlighting the constraints that face lower levels of government in federalism. Fiscal federalism implies that the threat of exit from businesses and wealthy residents exerts downward pressure on taxation, redistribution, and regulation (Peterson 1981; Oates 1999), which reduces the potential for variation across states.[5] Fiscal federalism implies that state governments have little policy discretion compared to the Federal Government: They face a greater threat of exit, and with no ability to manipulate a floating currency, they face economic forces beyond their control and greater

[3]Researchers have correspondingly used the state level as a way to increase their N to 50 in cross-sectional studies of the roles of public opinion (Erikson, Wright, and McIver 1993; Lascher, Hagen, and Rochlin 1996), interest groups (Gray and Lowery 1988), descriptive representation (Bratton and Haynie 1999; Sanbonmatsu 2002), or institutional rules and legislative organization (Chubb 1988; Poterba 1995; Carey, Niemi, and Powell 1998; Barrilleaux and Berkman 2003; Overby, Kazee, and Prince 2004). There are certainly advantages to increasing one's N of institutional venues to 50, but analysis of a cross-sectional "snapshot" is unlikely to detect systemic changes that occur over time (Pierson and Skocpol 2002; Pierson 2004).

[4]A few conservative commentators counter the minimalist view, arguing, for instance, that liberal state governments like that of the "failed state of California" are too active in attempts "to regulate the internet, to tax corporations on profits earned in foreign jurisdictions, and to impose sales tax collection obligations on internet sellers domiciled elsewhere" (Greve 2011, 6).

[5]However, some research challenges the prediction of a "race to the bottom" in the states (Volden 2002; Konisky 2007).

pressure to balance budgets.[6]

State legislators also lack the policymaking resources of members of Congress (Kousser 2005). Lower salaries increase the incentive to spend time earning money outside of their political offices, and fewer staff limit the ability to research and draft legislation. Even if state legislators face equivalent pressures from voters and interest groups as members of Congress, we would expect those in state capitals to be less productive due to these resource constraints.

Despite these constraints, however, roll-call voting in state legislatures has polarized in recent years (Shor and McCarty 2011). Whether the prior cause of polarization stems from voters, interest groups, or politicians themselves, greater polarization implies greater distance between the policy preferences of Democrats and Republicans,[7] and thus increasing polarization of policy outcomes in the states. Yet the most comprehensive studies of state policy polarization over time, those of Caughey and Warshaw (2016) and Caughey, Xu and Warshaw (2017), again conclude in favor of the minimalist view of state policy. While "Democrats and Republicans may disagree consistently and even violently," Caughey, Xu and Warshaw (2017, 27) conclude that "the actual policy consequences of these disagreements are far less dramatic." The increasingly partisan and ideologically consistent rhetoric of Democratic and Republican governors and state legislators is just that—talk, with little consequence for public policy (for other examples of minimal effects of party control see Garand 1988; Erikson, Wright and McIver 1993; Jacobs and Carmichael 2002; Konisky 2007).

This article's theory and empirical analysis challenge this line of research. I turn to measurement in a later section, but here I contend that theories of state policy polarization have neglected institutional dynamics. Research in "new institutionalism" and formal theory has shown that institutions structure and influence preferences and incentives, and that this is especially true of institutions associated with federalism (e.g., Riker 1964; Pierson 1995). There has been little theorization, though, of how polarization operates not merely at the federal level or the state level, but in a federalist system.

2.2 Polarized Federalism

There a number of reasons to expect a resurgence of the state level as a locus of major policymaking and policy conflict in recent decades. Traditional perspectives of the states as 50 separate polities would point to causes of policy polarization that occur within each state. For instance, an observed increase in state policy variation and polarization could be the result of partisan sorting and polarization of state electorates.

[6]Researchers highlight the inability to devaluate a currency as a major barrier to fiscal policy in lower income Eurozone countries (e.g., Krugman 2013).

[7]This is true at least to the extent this polarization is ideological, as well as to the extent that non-ideological partisan brinksmanship incentivizes ideologically distinct policy agendas (Lee 2009).

But evidence suggests that the median voter is an unlikely cause of state policy change. The electoral connection is weak in the states because voters pay little attention to state politics (Anzia 2011; Hopkins 2018). State legislative elections are dominated by national tides, suggesting that state policy is not central to vote choice (Rogers 2016). Moreover, the precipitous decline of state politics journalism in recent years may make policy even less "traceable" for voters (Arnold 1992) such that state politicians are unlikely to face electoral punishment for pursuing policies that are "out of step" with voters. Accordingly, empirical research has found limited policy congruence and responsiveness to public opinion in the states (Lax and Phillips 2012).[8] In the next chapter, I use multilevel regression with poststratification (MRP) to estimate policy-specific public opinion over time, and find little evidence that policy responds to opinion.

While theories of 50 atomized states, especially those focused on the median voter, are likely to be incomplete, a more interactive theory involves what I call *polarized federalism*—polarization within multilevel federalist institutions driven by shifts in investments by policy demanding groups. The multilevel structure of federalism matters for policy polarization in the states for two reasons. First, polarization at the *national* level increases the cost of policy change through Congress, which in turn incentivizes policy demanders to venue shift and use lower levels of government as a "safety valve." Second, when policy demanders shift their focus to the states, state level institutions may advantage coordinated intense policy demanders over diffuse voters.

Federalism may serve as a "safety valve" for policy demanders who are stymied in Washington, and this safety valve grows more valuable as polarization in the Federal Government increases. Polarization reduces the ability of the minority party and their aligned policy demanders to influence or extract compromises from the majority party, increasing the relative benefit of shifting their focus to the states. Frustrated climate activists may turn their hopes to the states (Rabe 2004), as might organized labor (Meyerson 2014), LGBT rights activists (Lax and Phillips 2009), or antistatist and business interests (Skocpol and Hertel-Fernandez 2016).

When combined with divided party control, polarization leads to policy gridlock (Binder 2003). As has been well documented at the national level, gridlock prevents agenda items from legislative success (Binder 1999) and generates policy "drift" (Hacker 2004)—and higher costs of national policy change for policy demanders. Again, all else equal, this national gridlock increases incentives for policy demanders to venue shift to the state level.

Additionally, gridlock in the Federal Government prevents national legislation from preempting, overturning, or standardizing state policies, further increasing the attractiveness of state venues for policy demanders. An expansion of national policy from the New Deal through the early 1970s "centralized" governance and standardized the welfare state and civil rights law across the states (Melnick 1996; Mettler 1998; Campbell 2014). Although New

[8]There is some evidence of policy responsivess when opinion and policy are reduced to one or two dimensions (Erikson, Wright and McIver 1993; Caughey and Warshaw 2017), but such responsiveness rarely appears when more policy-specific measures are used (e.g., Lax and Phillips 2012)

Deal programs allowed states to exclude many black Americans from benefits (Weir 2005; Katznelson 2013), landmark policies like the Social Security Act of 1935 and the Civil Rights Act of 1964 decreased interstate policy variation by establishing or raising legal and economic baselines.[9] However, in the years since the 1970s, polarization has increased in Congress and divided Federal Government has become a more frequent occurrence (McCarty, Poole and Rosenthal 2006). With a gridlocked Federal Government, state level policy is more likely to persist.

Even in the rarer moments when important federal policy does pass, polarization and divided government may increase incentives for members of Congress to delegate authority to the states (e.g., Mooney 2000; Feeley and Rubin 2009; Chatfield and Rocco 2014). A legislator who would ideally implement his or her ideal policy across all 50 states may accept a decentralized policy as a second-best option if it moves the average outcome (such as the policy regime for the average state or average individual) toward his or her ideal. Moreover, the district-based electoral connection in Congress can improve the relative appeal of the second-best option because "representatives know that when they delegate to state and local agents, policy for *their* constituents will be set by representatives elected by those same constituents" (Chatfield and Rocco 2014, 4). Indeed, the rise of polarization in Congress has coincided with what scholars call a "devolution revolution" (e.g., Soss et al. 2001; Grogan and Rigby 2008; Kelly and Witko 2012). In a similar fashion, the federal judiciary has undergone a "federalism revolution" in which the courts are an increasingly "state friendly arena" (Whittington 2001; Waltenburg and Swinford 1999, 2) precisely during an era of increasingly partisan and narrow (5-4 split) decisions (Baum 2015).[10]

The heterogeneity of states makes the obstacles to policy change less severe—especially for policy demanders with the capacity to make political investments across states. As the parties polarize, policy demanders are incentivized to ally themselves with one side (Bawn et al. 2012), but fortunately for them there will (virtually) always be at least one state government controlled by their aligned party.[11] Diffuse voters are immobile, but states allow coordinated groups to "venue shop" in search of fertile pastures to implement their agendas (Baumgartner and Jones 2010). A group with the ability to target and influence the agenda of many state governments controlled by their aligned party can make major policy gains while the U.S. Congress stalls.

Indeed, such an environment is likely to provide political advantages to well-resourced, mobile policy demanders over diffuse voters. A classic literature argued that concentrated

[9]This process of centralization continued in the welfare and regulatory buildup of the 1960s and 70s. Landmark federal policies that decreased state variation during this era include the Social Security Amendments of 1965 and 1972, the Gun Control Act of 1968, the National Environmental Policy Act of 1969, and the Clean Air Act of 1970.

[10]Various measures of fiscal activity show an expanding role of state government since the 1970s. I plot total state government spending, employment, and average state tax rates as a percentage of federal spending, employment, and tax rates in Appendix Figure A.2.

[11]There were no unified Republican states in the year after the Watergate scandal (see Appendix Figure A.1).

and elite interests are advantaged at lower levels of government (e.g, Schattschneider 1960; Riker 1964; McConnell 1966, 139-155), which diffuse and mass interests can counter by "extending conflict" to higher levels (Schattschneider 1960, 63). Relative to voters, who are often cross-pressured and inconsistent (Converse 1964), concentrated interests and organized activist groups are likely to have intense and consistent preferences (Bawn et al. 2012; Skocpol and Hertel-Fernandez 2016)—which, to the extent they are implemented, increase interstate policy variation.

Recent studies harken back to the classic literature. They argue that well-resourced organizational networks have increased their investments in state politics with a focus not on their home states, but on cross-state agenda setting and advocacy. Organizational and technological innovations have allowed these groups to lobby and provide "model bills" to state legislators (Hertel-Fernandez 2014; Hertel-Fernandez, Skocpol and Lynch 2016). Such influence is relatively low-cost because state politicians face considerable informational and human resource constraints relative to members of Congress (Kousser 2005).

Finally, the Federal Government has also been more likely to be under divided party control than state governments in recent decades. Between 1970 and 2014, the U.S. House, Senate, and Presidency have only been under unified party control about 27% of the time (12 of 45 years), whereas the average state has been under unified control about 50% of the time (see Appendix Figure A.1). Regardless of whether this difference in the likelihood of unified party control is due to federalist institutions or historical happenstance, we would expect relatively less gridlock in the states as polarization increases, and, in turn, a relative growth in the role of state governments as major policymakers.

2.3 Measuring Policy Outcomes

Polarized federalism posits that national polarization interacts with federalism to produce greater divergence in state policy. But is state policy consistent with polarized federalism? Do policy outcomes diverge over time, and is this divergence related to party control of government? In this section, I describe my strategy to measure policy outcomes and estimate the changing relationship between party control and policy.

This study employs the most comprehensive dataset of state policy outcomes since 1970.[12] To build it, I collect data on 35 policies, to which I add data from Jordan and Grossmann (2016), Caughey and Warshaw (2016), and Boehmke and Skinner (2012) to create a dataset of 135 policies. (I also extend years of coverage for 16 policies from the other datasets.) Caughey and Warshaw (2016, 4-5 and Supplemental Material) provide a detailed description of many

[12]I start in 1970 for two reasons. First, no modern state law is substantively comparable to those of the Jim Crow regime, which entailed mass disenfranchisement of black Americans and a *de jure* racial caste system. Second, although Caughey and Warshaw (2016) employ data going back to 1937, their model's parameters for early state policies may be substantively invalid, and state ideal point estimates also have high degrees of uncertainty in the pre-1970 period.

of the policies, which can be binary (e.g., Right to Work laws), ordinal (e.g., mandatory parental notification or consent for a minor's abortion), or continuous (e.g., marginal tax rate on high incomes). Table 2.1 lists the policies, and I provide descriptions and sources for each policy in the Appendix.

The data I collect covers policies of considerable importance. They include voter ID laws, state capital gains taxes, as well as various regulations related to public sector unions (Anzia and Moe 2017), abortion rights and coverage (Guttmacher Institute), campaign finance (Barber 2016*b*), and immigrant workers (National Council of State Legislatures).

Of particular importance is my data collection of criminal justice policies. Although some research focuses specifically on criminal justice (e.g., Yates and Fording 2005), research that summarizes policy across issue areas has neglected incarceration. For instance, aside from a few drug-related policies (e.g., medical marijuana laws), the Caughey and Warshaw (2016) dataset only contains data on four criminal justice policies: death penalty repeal, the establishment of probation (only for the 1936-1939 period), animal cruelty as a felony, and age span provisions for statutory rape cases (i.e., the decriminalization of sex between consenting teenagers of similar ages). These policies are generally orthogonal to the rise of mass incarceration. I collect data on laws that criminal justice research considers central to the rise of mass incarceration (for a review see Travis, Western and Redburn 2014, Chapter 3): truth-in-sentencing laws, which require individuals to serve a minimum percentage of their original sentence; three strikes laws, which increase penalties for an individual's third felony; and determinate sentencing laws, which specify mandatory minimum sentences.

To measure party control of government, I use variables that indicate whether a state is under unified Democratic control, unified Republican control, or divided control (Klarner 2013). While control of executive branch or one or more legislative chamber may have an independent or partial effect on policy outcomes (Smith 1997), the polarized federalism theory focuses on unified control because polarization and divided government interact to produce gridlock (Binder 1999). I provide additional models with measures of control of the governorship, lower house, and upper house in the Appendix.

Key to the analyses is the comparison of the party-policy relationship across time. Because policy change is rare compared to other political dynamics, estimating a completely dynamic party effect (i.e., by year) is difficult. Precision and clarity are greatly improved by estimating an average party effect for different eras that span multiple years (e.g., Caughey, Xu and Warshaw 2017, Table 3). I primarily compare the association between party control and policy change during two eras: the 1970-1999 period and the 2000-2014 period.[13] In practice, this entails interacting the party control variable with a dummy variable for the 2000-2014 period to estimate the marginal effect of party control on policy change during the different eras. Temporal breaks in time-series models can also be estimated empirically. Chow tests reject the null of no structural break in the party-policy relationship between

[13]Setting this threshold between 1999 and 2000 strikes a balance between periods that are long enough (precision) and highlighting the potentially preciptious increase in policy polarization in the most recent years of hyper-polarization (Mann and Ornstein 2013). In the Appendix I provide additional estimates using the 1970-1993 and 1994-2014 periods.

Table 2.1: State Policies (1970-2014)

Abortion	DNA motions	Senior prescription drugs	Gay marriage ban
Abortion insurance restriction	Three strikes	TANF eligibility	Hate crime law
Abortion legal	Truth-in-Sentencing	TANF payment level	LGB discrimination ban public accomodations
Consent post-Casey	**Education**	Welfare drug test	LGB employment discrimination ban
Consent pre-Casey	Charter school law	Welfare time limit	Sodomy ban
Emergency contraception	Higher ed spending	**Housing/Transportation**	**Marijuana**
Gestation limit	K-12 spending	Growth management	Marijuana decriminalization
Medicaid covers abortion	School choice	Lemon law	Medical marijuana
Parental notice	**Environment**	Rent control ban	**Taxes**
Partial birth abortion ban	Bottle bill	Tort limit	Corporate tax rate
Physician required	CA car emissions	**Immigration**	EITC
Waiting period	Endangered species	Drivers licenses for undocumented	Estate tax
Campaign Finance	E-waste	English official language	Income tax
Corporate contribution ban	GHG cap	E-verify	Sales tax
Dollar limit on individual contributions	Renewables fund	E-verify ban	Tax burden
Dollar limit on PAC contributions	Solar tax credit	In-state tuition for undocumented	Top capital gains rate
Limit on individual contributions	State NEPA	State cash benefits for recent immigrants	Top income rate
Limit on PAC contributions	**Gun Control**	State food benefits for recent immigrants	**Voting**
Public funding elections	Assault weapon ban	State health benefits for recent immigrants	Absentee voting
Civil Rights/Liberties	Background checks (dealers)	**Labor (Private Sector)**	Early voting
Bible allowed in public schools	Background checks (private)	Disability insurance	Motor voter
Corporal punishment ban	Brady law	Local minimum wage ban	Voter ID
Discrimination ban public accomodations	Dealer licenses required	Local sick leave law ban	**Other**
ERA ratification	Gun registration	Minimum wage	Animal cruelty felony
Fair employment comm.	Open carry	Paid family leave	Beer keg registration
Gender discrimination ban	Sat. Night Special ban	Paid sick leave	Bike helmet required
Gender equal pay law	Stand Your Ground	Prevailing wage	Casinos
Moment of silence in public school	**Health and Welfare**	Right to work	Cigarette tax
No fault divorce	ACA exchange	Unemployment comp.	Drinking age 21
Physician-assisted suicide	AFDC payment level	**Labor (Public Sector)**	Grandparent visitation
Public breast feeding	AFDC Up	Ban on agency fees (state)	Living wills
Religious Freedom Rights Amendment	CHIP eligibility (children)	Collective bargaining (firefighters)	Lottery
Reporters right to source confidentiality	CHIP eligibility (infants)	Collective bargaining (local)	Mandatory car insurance
State ADA	CHIP eligibility (pregnant women)	Collective bargaining (police)	Mandatory seatbelts
State ERA	Expanded dependent coverage	Collective bargaining (state)	Motorcycle helmet required
Criminal Justice	Medicaid adoption	Collective bargaining (teachers)	Smoking ban (restaurants)
Death penalty repeal	Medicaid expansion	**LGBT Rights**	Smoking ban (workplaces)
Determinate sentencing	Pre-BBA CHIP eligibility	Civil unions and marriage	Zero tolerance underage drinking

Note: Issue area categories in bold.

1999 and 2000 with $p < 0.01$ for every policy measure used in this study. In a subsequent section, I also empirically locate the most years with the greatest likelihood of structural breaks.

Unidimensional Measures

Political scientists often summarize public opinion, legislative votes, and more recently, policy outcomes on a unidimensional left-right dimension. Recent unidimensional policy measures provide a summary of the ideological content of policy on a dimension typically described as "policy liberalism" or "the role of government" (Erikson, Wright and McIver 1993; Caughey and Warshaw 2016).

As a first cut at the data, I estimate policy variation and polarization with four unidimensional left-right measures of policy outcomes. The first is the State Policy Liberalism (SPL) measure from Caughey and Warshaw (2016), a set of state-year policy ideal points generated from a dynamic Bayesian IRT model. Second, I estimate the same ideal point model with my expanded policy dataset to produce an Expanded SPL measure. The third and fourth measures are Substantive Scales, simple additive indices (averages) that are the sum of a state's liberal policies minus its conservative policies in a given year. These measures serve as expert-coded alternatives to the Bayesian IRT latent dimension estimates and are analogous to the "Policy" measure from Erikson, MacKuen and Stimson (2002, Chapter 9). One of the additive indices weights policies equally, while the other is the average of issue area-specific indices. (Subsequent sections address how the ideological direction of policies are determined.) All measures are normalized to a range between 0 and 1.

I calculate two measures of policy *variation* with these unidimensional scales: the range and the standard deviation of policy ideal points across states in each year. I plot yearly estimates from 1970 to 2014 in Appendix Figure A.3. The spread of ideal points widens greatly since the 1970s. The range and standard deviation estimates are remarkably similar across the measures. The range of ideal points is at least a third larger in the 2010s than in the 1970s and 80s, and the standard deviation is at least two thirds larger.[14]

These measures suggest that policy *polarization* has similarly increased. Using dynamic panel regressions, Appendix Figure A.4 plots the marginal effect of unified party control of government on change in ideal points for the 1970-1999 period and the 2000-2014 period. All of the estimates show at least a twofold increase in the magnitude of the relationship between party control and policy ideal points (see also Caughey, Xu and Warshaw 2017).[15]

The expanding variation and polarization evident in the unidimensional analysis motivates the investigation of *issue-specific* policy dynamics. Unidimensional ideal points serve as

[14]The Substantive scales, which do not use data from earlier years to smooth ideal points over time like the Bayesian IRT measures, show slightly larger increases in range and standard deviation over time (starting from slightly lower in the 1970s and ending slightly higher in the 2010s).

[15]As is described in detail in Chapter 3, the three measures that employ my full policy dataset show larger party effects after 2000.

strong summary measures, but generally, they may create obstacles to inference by obscuring multidimensional variation or conflating extremism and consistency (Broockman 2016), and they rely on relatively strong assumptions about the comparability of policies across issue domains. More importantly, it is difficult to draw conclusions about the substantive content of policy—its effect on members of the polity—from unidimensional ideal point estimates. Policy scholars may be interested in more specific temporal dynamics in residents' relationship to government. Are state abortion laws more or less restrictive? In which direction have state tax rates, restrictions on campaign contributions, and the generosity of welfare benefits moved in recent decades?

Additionally, although they may be advantageous in the study of roll-call votes (e.g., Clinton, Jackman and Rivers 2004), there are two reasons to prefer straightforward additive indices over latent dimension estimates (e.g., factor analysis or Bayesian IRT) for the measurement of *policy outcomes*. First, historical, normative, and policy scholarship provides clear priors about the ideological content of policy. Empirically deriving model parameters (the ideological content of policy) from the data rests on the joint assumption that (a) liberal states are liberal because they pass liberal policies, and (b) that liberal policies are liberal because liberal states pass them. When this assumption is violated historically (e.g., during the 1960s and 1970s conservative Southern states were early adopters of liberal abortion laws), the model may produce parameters that do not conform to substantive understandings about the ideological content of policy. For instance, I show in Chapter 3 that parameters for some relatively inconsequential policies (e.g., mandatory registration of beer keg rentals) are larger than those of more important policies, and that some similar laws have parameters that point in opposite directions.[16] Second, the real world consequences of substantive policy is, for the most part, additive. Whereas latent dimension estimates rely on the correlations between policy items to provide "relative" measures of policy outcomes (Caughey and Warshaw 2016, 7), averages can provide absolute measures of policy outcomes.

Policy Indices by Issue Area

Issue area measures provide a clearer picture of historical changes in policy substance. Although many studies have employed summary measures of policy outcomes in a single issue area (e.g., Norrander and Wilcox 1999; Hero and Preuhs 2007), mine is the first to compare across many issue area indices. I group the policies into 16 discrete issue areas: abortion, campaign finance, civil rights and liberties, criminal justice, drug policy, education, environment, gun control, health and welfare, housing and transportation, immigration, labor (private sector), labor (public sector), LGBT rights, taxes, and voting.

In each area, I calculate a simple substantive measure of average policy outcomes: the number of liberal policies minus the number of conservative policies (see also Erikson, MacKuen and Stimson 2002, Chapter 9). Because policies can be binary (e.g., medical marijuana

[16]The discrimination parameter for occupational licensing for beauticians is liberal but the parameter for licensing for nurses is conservative

laws), ordinal (e.g., voter ID laws, which can be strict or non-strict), or continuous (e.g., minimum wage level), I normalize each policy to range from 0 to 1. A binary policy, which a state either has or does not have, takes on the values of 0 or 1, whereas an ordinal or continuous policy, such as a tax or minimum wage, is transformed to the $[0, 1]$ scale. A state's score in an issue area index is therefore the sum of the liberal policies minus the sum of the conservative policies.

This kind of measure relies on three assumptions: first, the ideological "direction" of policy (whether it is liberal, conservative, or neither); second, that policies are of equal substantive importance; and third, that the direction and importance remain constant over time. These assumptions are unlikely to be satisfied in practice, especially equality of substantive importance.[17] However, I argue that these simple index measures strike a balance between agnosticism, precision, transparency, risk of bias, and substantive interpretability.

Determining the *ideological direction* of more than 130 policies is a difficult task. The primary left-right ideological dimension, or "what goes with what" has changed over time, but for the most part political observers characterize policies on the *left* to be those that 1) expand the use of state power for economic regulation and redistribution (Rawls 1971; Foner 1984; Weir 2005; Wang 2005; Brinkley 2011), or to increase or protect the rights of historically marginalized groups in society (black Americans and other nonwhite racial groups, women, LGBT individuals, immigrants, and religious minorities) (DuBois 1935; Foner 1988; Kessler-Harris 2001; Shelby 2005; Kollman and Waites 2009); and 2) restrict the use of state power for the punishment of deviant social behavior (Simon 2007). Policies on the *right* do the opposite (Himmelstein 1992; Brinkley 1994; Harvey 2007). Although there is considerable nuance throughout political and intellectual history, in short, left policies promote social libertarianism and economic interventionism, while right policies promote traditional (incumbent) social values and oppose state intervention in markets.

Yet even with this large body of historical and normative scholarship, there is still no objective, unifying test of whether a certain moral principle, political action, or legal statute is on the left or right. Many scholars argue that the first dimension of politics represents the "size of government" (Poole and Rosenthal 1997), but this is not always the case. For instance, policies that expand rights and protections for black Americans, which are understood to be liberal, can involve expansions of state power (e.g., anti-lynching laws) or restrictions on state power (e.g., laws that reduce prison sentences). The same is true of abortion laws, where Medicaid coverage of abortion and bans on "partial birth abortion" both involve greater state intervention, but are quite ideologically distinct. It is thus no surprise that there is an ongoing debate about whether the clustering of policies along partisan and ideological lines is due to "natural" ideological or psychological principles (e.g., Haidt 2012), or whether they are the products of idiosyncratic historical coalition partnerships between interests in society that over time became path-dependent (e.g., Karol 2009; Bawn

[17] One might argue, for example, that voter ID laws are more substantively and normatively consequential than motor voter laws, and should thus be weighted more heavily in calculating the issue area indices.

et al. 2012).

Table 2.2: Ideological Content of Policy Issue Areas

Issue Area	*Concept*
Abortion	Legal right to and cost of emergency contraception and abortion
Campaign Finance	Restrictions on individual, corporate, PAC contributions; public funding of elections
Civil rights & Liberties	Penalties for discrimination based on race, gender; religious privileges
Criminal Justice	Punitiveness
Drugs	State legality of federally illicit drugs (especially marijuana)
Education	Spending; public vs. private control
Environment	Restriction on emissions, chemicals; protection of species
Guns	Legal rights to purchase, own, or carry a firearm
Health & Welfare	Generosity (eligibility, benefit levels)
Housing & Transportation	Command and control
Immigration	Legal right to public services for undocumented; regulation of hiring undocumented
Labor	Right to unionize; wage laws
LGBT	Protections or penalties for homosexuality
Taxes	Marginal rate; progressivity
Voting	Cost, access to voting

I argue that an *issue-specific* left-right conceptualization can improve inference for studies of policy dynamics. Rather than assuming that issues "go together" in unidimensional space, Table 2.2 shows conceptual dimensions that determine the ideological direction of policies within each issue area.[18] The left-right dimension for abortion policy, for example, represents the legality and costs (broadly defined) of obtaining an abortion. Other issue areas represent multiple related concepts. Tax policy, for example, is comprised of two concepts: absolute rates and progressivity (the distribution of marginal rates across income levels), and health and welfare policy is comprised of both benefit levels and the strictness of eligibility. I base a policy's *direction*—left, right, or, in a small number of cases, neither—on its expected effect on the issue-specific dimension. This issue-specific conceptualization also helps to avoid the problem of sorting and shifts over time regarding which issues "go together" on a single left-right dimension. While the cluster of issues on the left and right has shifted over the 19th

[18]Within an issue area, a policy can be on the "left" or "right," but these terms are simply shorthand for the concepts described in Table 2.2.

and 20th centuries (e.g., Schickler 2013), issue-specific assessments (e.g., whether a policy restricts or broadens access to abortion) have largely remained constant.[19]

2.4 Interstate Policy Variation

In this section I estimate change in state policy since 1970. Figure 2.1 plots each issue area policy index. The grey lines represent the policy outcomes for each individual state over time.

States' policy outcomes within each issue area (the grey lines) diverge greatly over time; this represents increased *overall variation* in state policy outcomes in each area. Compared to the 1970s, the policy regime under which an individual lives is increasingly determined by her state of residence. For instance:

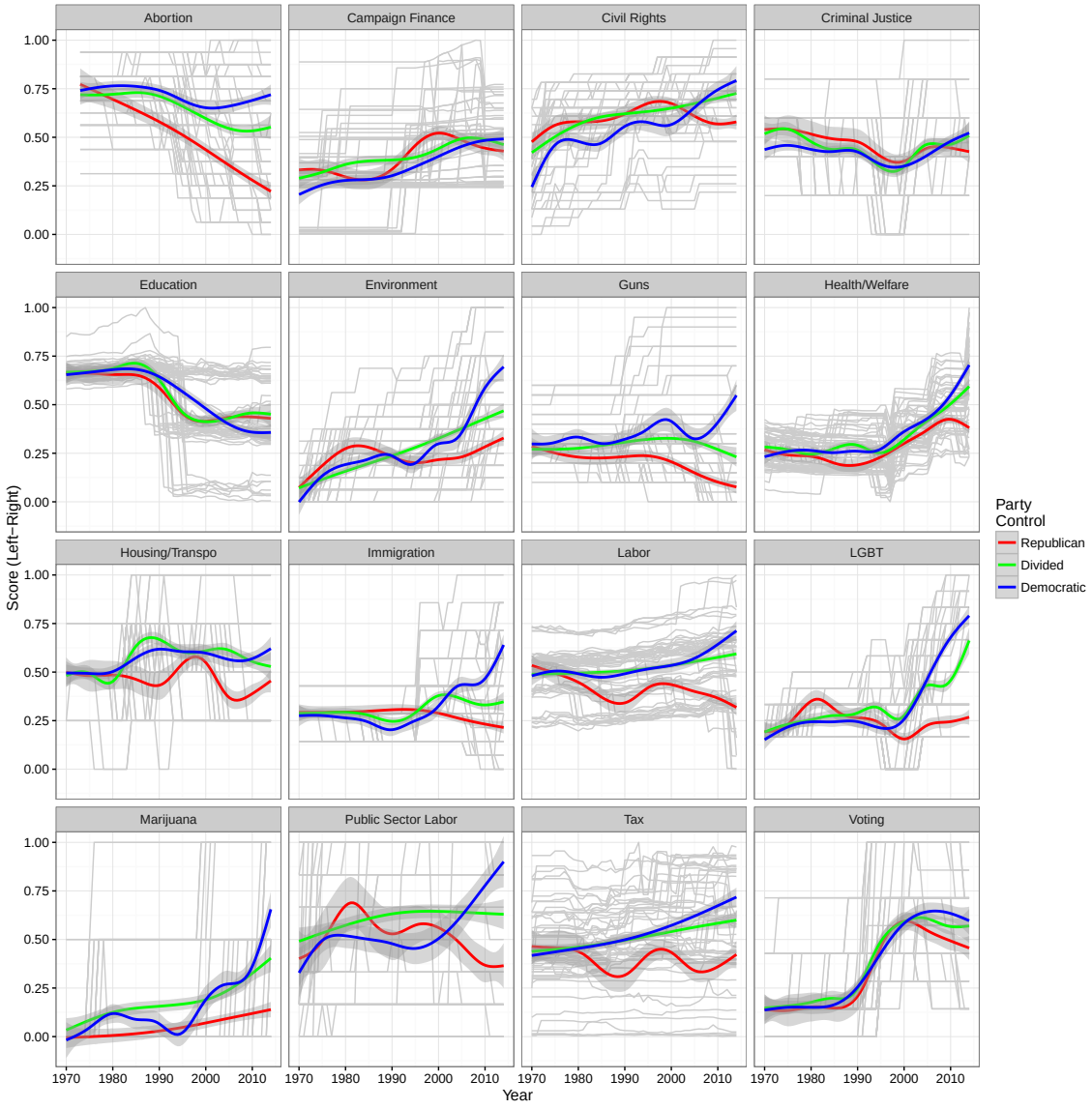
- **Abortion:** In 1973, states only differed in Medicaid coverage for abortion and other minor regulations. By 2014, the most restrictive states mandate waiting periods, parental notification, counseling, licensed physicians, a 20-week gestation limit, and restricted insurance coverage for abortion.
- **Environment:** In 1970, the greenest states had state EPAs and endangered species laws. By 2014, they had strict regulations of greenhouse gas emissions for cars and utilities, solar tax credits, and a plethora of recycling programs.
- **Gun Control:** In 1970, the least strict states allowed open carry and the strictest states required dealer licenses and purchaser background checks. By 2014, the least strict states had added had Stand Your Ground laws, while the strictest states banned assault weapons and mandated registration and waiting periods for purchases.
- **Health and Welfare:** In 1970, states varied in AFDC benefits and Medicaid adoption. By 2014, Massachusetts offered generous TANF and SCHIP benefits and had expanded Medicaid, while Alabama did not expand Medicaid, requires drug tests for public benefits, requires a monthly income below $268 for a family of three to qualify for TANF.[20]
- **Immigration:** In 1970, states mostly varied in laws establishing English as official state language, and all legal immigrants were eligible for public welfare and health programs. By 2014, only some states provide public benefits to new legal immigrants.[21]

[19] Of the 135 policies shown in Table 2.1, I exclude the 15 in the Other category because they (a) have unclear issue-specific ideological content (e.g., animal cruelty felony), (b) are socioeconomically inconsequential (e.g., beer keg registration), and/or (c) are insufficiently varied or numerous to create an issue area (e.g., state lotteries).

[20] $268 per month is about 16% of the Federal Poverty Level for a family of three.

[21] 1996 welfare reform made legal immigrants ineligible for federal benefits for the first five years of residency; some states then moved to cover these new immigrants in their Medicaid, TANF, and SCHIP programs using only state funding (Hero and Preuhs 2007).

Figure 2.1: Issue Area Scales by Party



Note: Plots show average scores on issue area scales by party control (loess). The abortion measure begins in 1973 due to *Roe v. Wade*.

Some states provided in-state tuition for undocumented college students, drivers licences for undocumented immigrants, and banned the use of e-verify for employment, while other states require all employers to use it.

- **Taxes:** In 1970, some states had no income or capital gains taxes, while the highest tax state, Vermont, had a 5.54% top capital gains rate and 14.88% top income rate. By

2014, many states continued to collect no income or investment taxes, but California had a 14.1% top capital gains rate and a 14.1% top income rate.

Some areas, such as environmental policy, become more liberal over time on average. All of the major policies in this area increase environmental regulation or public spending in pursuit of environmental quality, and the most conservative states on the environment simply do not pass the major environmental laws that the "green" states do. Abortion policy, in contrast, tracks more conservatively since *Roe v. Wade* (1973). A few states become more liberal on abortion over time as they pass laws to provide Medicaid coverage for abortion and over the counter emergency contraception. This liberal trend, however, is swamped by the spread of abortion restrictions in states, such as mandatory parental notice for minors and bans on "partial birth abortion." Though not included in this analysis, prior research finds similar dynamics for Targeted Regulation of Abortion Provider (TRAP) laws, which "single out abortion providers and impose on them requirements and regulations that are excessive and more stringent than those imposed on other medical practitioners" (Medoff and Dennis 2011, 955). A third set of issue areas, such as immigration and labor, sees similar growth in *variation*, but does not become more liberal or conservative on average since the 1970s.

Each issue area shows growing *policy variation* across states, but they also show partisan *policy polarization*: Policy outcomes in Republican states are more distant from those in Democratic states. In particular, Figure 2.1 shows the correlation between party control and policy outcomes in each area (with the blue lines representing unified Democratic states, the red lines representing unified Republican states, and the green line representing divided states). There are two issue areas that do not fit this pattern, where increased overall variation is appears nonpartisan: criminal justice and education. The averages of Republican, Democratic, and divided states in Figure 2.1, however, are simple correlations, so the growing policy divergence by party control could be simple sorting—states with conservative policies becoming Republican and states with liberal policies becoming Democratic. To test the changing relationship between party control and policy *change*, in contrast, I estimate dynamic panel regressions and compare the marginal effect of party control on policy outcomes for the 1970-1999 period and the 2000-2014 period. Figure 2.2 plots these results.

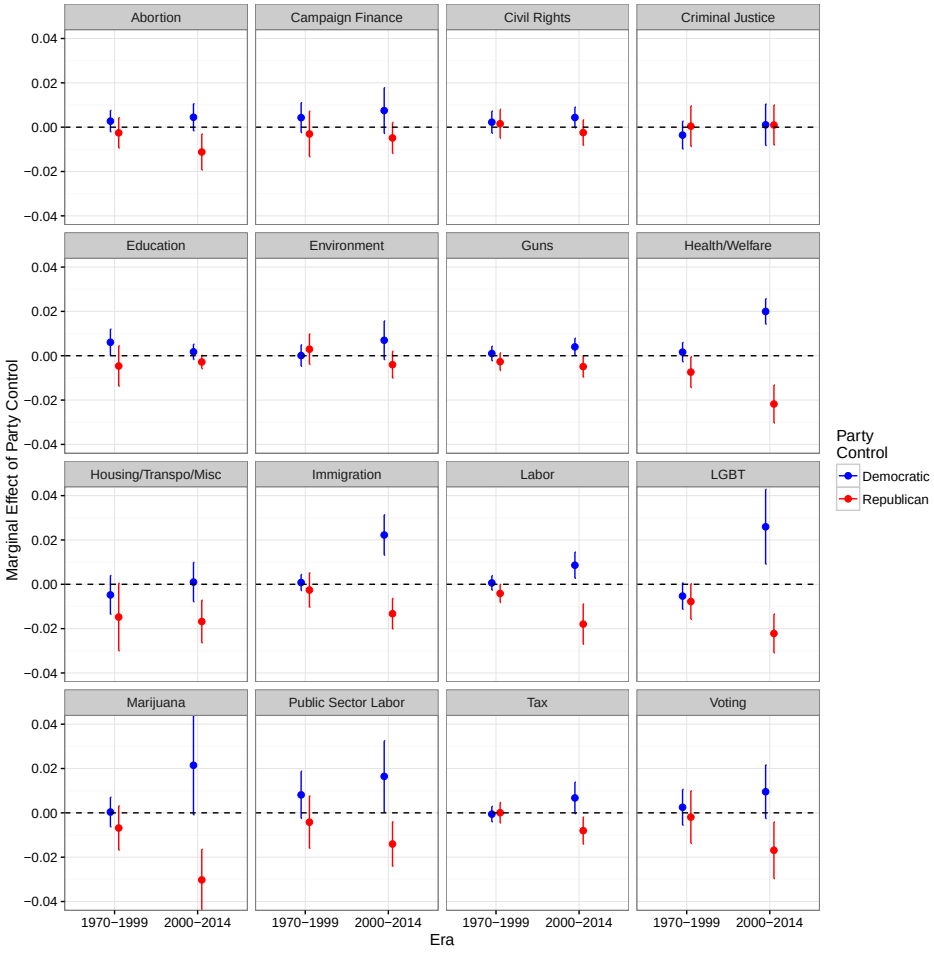
2.5 Partisan Policy Polarization

Figure 2.2, which tests the relationship between party control and policy *change*, corroborates the correlations shown in Figure 2.1.[22] Again, in 14 of the 16 issue areas, the party effect polarizes after 1999: There is a greater difference in the effect of unified Democratic control relative to that of unified Republican control in the 2000-2014 period than in the 1970-1999 period. The amount of polarization depends on the partisanship of policy—that

[22]I follow the dynamic panel models of (Caughey, Xu and Warshaw 2017), who add lagged dependent variables for year $t-1$ and $t-2$ to traditional two-way fixed effects models to improve fit. Alternative specifications are provided in the Appendix.

is, whether, for instance, Democratic states increase taxes relative to Republican states. But the overall amount of policy activity in a given area matters. For example, states become less active on civil rights and liberties as time progresses, but more active in areas like drug policy, LGBT rights, and voting rights (see Figure A.11 in the Appendix for counts of policy changes).

Figure 2.2: Party Effect on Issue Area Scales



Note: **Plots show the marginal effect of party control on policy outcomes across 16 policy issue areas.** Estimates are derived from models that include state and year fixed effects and lagged dependent variables for years $t-1$ and $t-2$. Robust standard errors are clustered by state.

Figure 2.2 shows that party control is no better at predicting policy change in criminal justice or education in recent years. Both before and after 2000, party control does not predict change in criminal justice policies. States controlled by Democrats pass punitive and liberal criminal justice policies at similar rates to divided and Republican states. In both eras, states controlled by Democrats are slightly more likely to pass liberal education

policies (e.g., increase spending in K-12 or higher education) and less likely to pass school choice, voucher, and charter laws. However, party control becomes slightly *less* predictive of education policy changes after 2000. In both of these issue areas, the static or decreasing predictiveness of party control stands in contrast to the other 14 issue areas in which party control increasingly explains policy change.

But does this policy polarization matter for the lives of these states' residents? Does it matter for socioeconomic outcomes that there is polarization in 14 issue areas, such as tax and health policy, but *non-polarization* in criminal justice and education?

The Socioeconomic Consequences of Policy Polarization

The polarization of policy carries major socioeconomic consequences for residents. In the polarized areas of health and environmental policy, party control of state government increasingly predicts rates of health coverage and carbon intensity of a state's energy supply, respectively. In the non-polarized areas of criminal justice and education, however, party control *does not* increasingly predict rates of incarceration (overall or among black residents) or graduation rates, respectively. In this section, I focus in depth on health and criminal justice policy. Analysis of socioeconomic outcomes in education and environmental policy is provided in the Appendix.

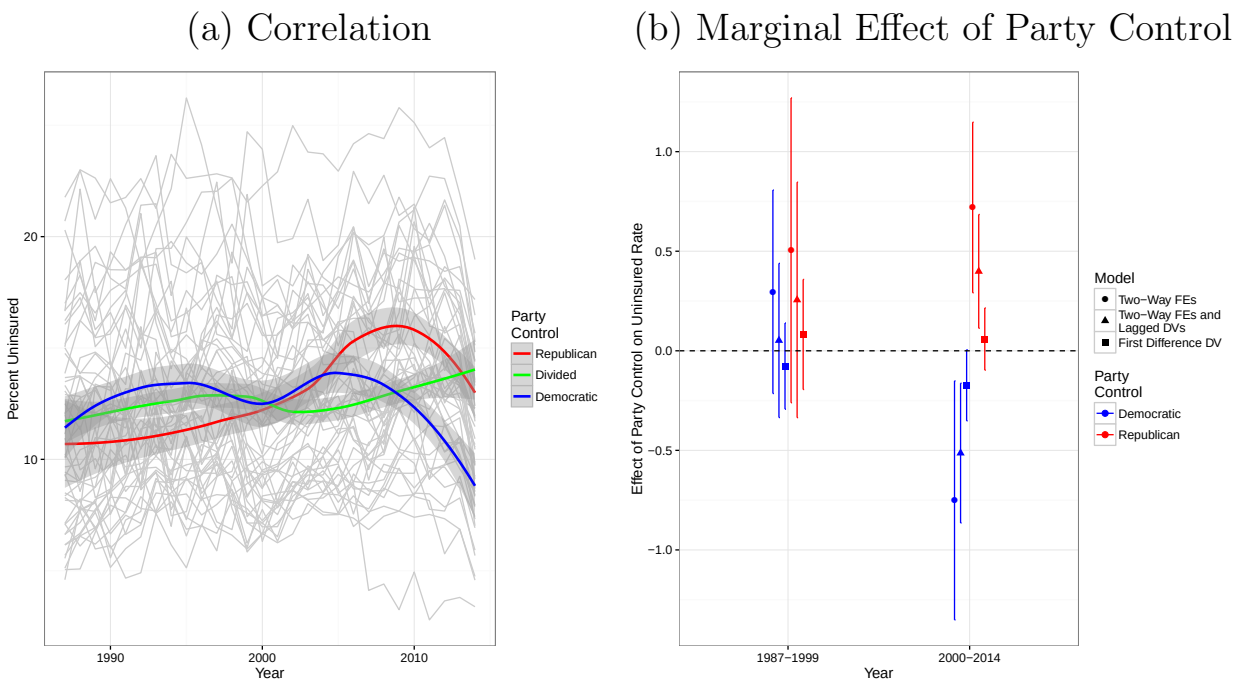
The health policy agendas of the national Democratic and Republican parties have been distinct since at least the 1930s. Health policy in the states has been similarly polarized for decades, as Democratic states tended to have more generous Medicaid eligibility and benefits. As the role of states in health policy expanded with the development of state prescription drug benefits for seniors, as well as federal grants for the State Children's Health Program (1998) and Medicaid expansion under the Affordable Care Act (2014), state health policies increasingly varied—and this variation was increasingly related to party control of government.

Socioeconomic *outcomes* related to health policy polarized accordingly. Figure 2.3 shows the relationship between party control and the *uninsured rate*. Plot (a) displays state uninsured rates (the grey lines) and the average Republican (red), Democratic (blue), and divided (green) state from 1987 through 2014. Plot (b) shows the marginal effect of party control for the 1987-1999 and 2000-2014 periods from different time-series regression models.

In both the correlation and the regressions, party control of government is increasingly associated with health insurance coverage in more recent years. Whereas prior to 2000, party control does *not* predict change in the uninsured rate, after 2000 unified Republican control is associated with a 0.75 percentage-point increase in the uninsured rate and unified Democratic control is associated with a 0.75 percentage-point decrease in the uninsured rate. These differences in coverage are of considerable social consequence. Health policy scholars, for instance, "estimate the number of deaths attributable to the lack of Medicaid expansion in opt-out states at between 7,115 and 17,104" (Dickman et al. 2014).

In contrast, education and criminal justice policies are—uniquely—non-polarized. In education, Democratic state governments pass school choice and charter school laws, and

Figure 2.3: Party Control and Health Insurance Coverage



Note: **Party control increasingly predicts health insurance coverage over time**. Plot (a) shows the average percent uninsured by state party control over time (using loess). Plot (b) shows the marginal effect of unified Republican control on the uninsured rate for the 1987 to 1999 period and the 2000 to 2014 period across three time-series model specifications.

spend at similar rates to Republican state governments.[23] In criminal justice, Democratic and Republican states both instituted "tough on crime" laws that led to mass incarceration. The lack of polarization in these areas relative to others has largely reflected the positions of the national Democratic and Republican parties, and a substantial literature describes the bipartisan history of policymaking in these areas (e.g., DeBray 2006; Hursh 2007; Weaver 2007; Alexander 2012; Wolbrecht and Hartney 2014).

Mass incarceration—the internationally unprecedented number and proportion of Americans, disproportionately black, under correctional control—has drawn increasing scholarly attention with respect to its origins (Weaver 2007; Lacey 2008; Wacquant 2009; Alexander 2012) and consequences (Western 2006; Manza and Uggen 2008; Weaver and Lerman 2010). Mass incarceration is in large part the result of changes in law and bureaucracy *in the U.S. states*. Of the powers reserved to the states in the 10th Amendment of the U.S. Constitution, *police powers* are the most prominent and likely the most socially consequential. State and

[23]However, I do find a modest increase in polarization in K-12 spending per pupil (but not higher education spending), with Democratic governments spending more than Republican governments after 2000.

local agencies account for the overwhelming majority of law enforcement, and the federal prison system houses less than six percent of the U.S. incarcerated population. There is new but limited research focusing on the interaction of mass incarceration and federalism (Miller 2008; Lacey and Soskice 2015; Miller 2016).

Despite the social importance and comparative punitiveness of American criminal justice policy, its politics has been mostly bipartisan as the parties compete to be perceived as "tough on crime." Weaver (2007, 261) discusses how after 1968 "even liberal Democrats did not talk about civil rights without deploring crime." Alexander (2012, 55-56) places responsibility on not only the Republican Party, but also on Democrats, for adopting "tough on crime" policies, especially during the 1990s. As shown in Table 2.3, punitive criminal justice policy has not polarized in the states. An exception is the repeal of the death penalty; five Democratic states repealed the death penalty between 2000 and 2014.[24]

Table 2.3: Criminal Justice Policies by Party Control

	Policies Passed (Repealed) by Party Control					
	Democratic		*Divided*		*Republican*	
	1970-1999	*2000-2014*	*1970-1999*	*2000-2014*	*1970-1999*	*2000-2014*
Three Strikes	8	1	11	1	5	0
Determinant Sentencing	6 (1)	1	11 (1)	0	2	0 (2)
Truth in Sentencing	5	0	4 (2)	0	1	0
Death Penalty Repeal	2	5	1	1	0	0

Note: Democratic and divided state governments passed more punitive criminal justice policies than did Republican governments, though removals of the death penalty mostly occurred in Democratic states. Numbers in parentheses represent repeals.

Yates and Fording (2005) find a significant association between Republican control of government and incarceration rates for white and especially for black people between 1978 and 1995, and I similarly find a statistically significant effect of unified Republican government for the 1978-1999 period.[25] The substantive effect, however, is modest and inconsistent across models: The two-way fixed-effect model (the least strict test) shows an increased incarceration rate of about 30 people per 100,000 residents, but the other models show no effect (see Plot (b) in Figure 2.4). An increase in a state incarceration rate of 30 individuals

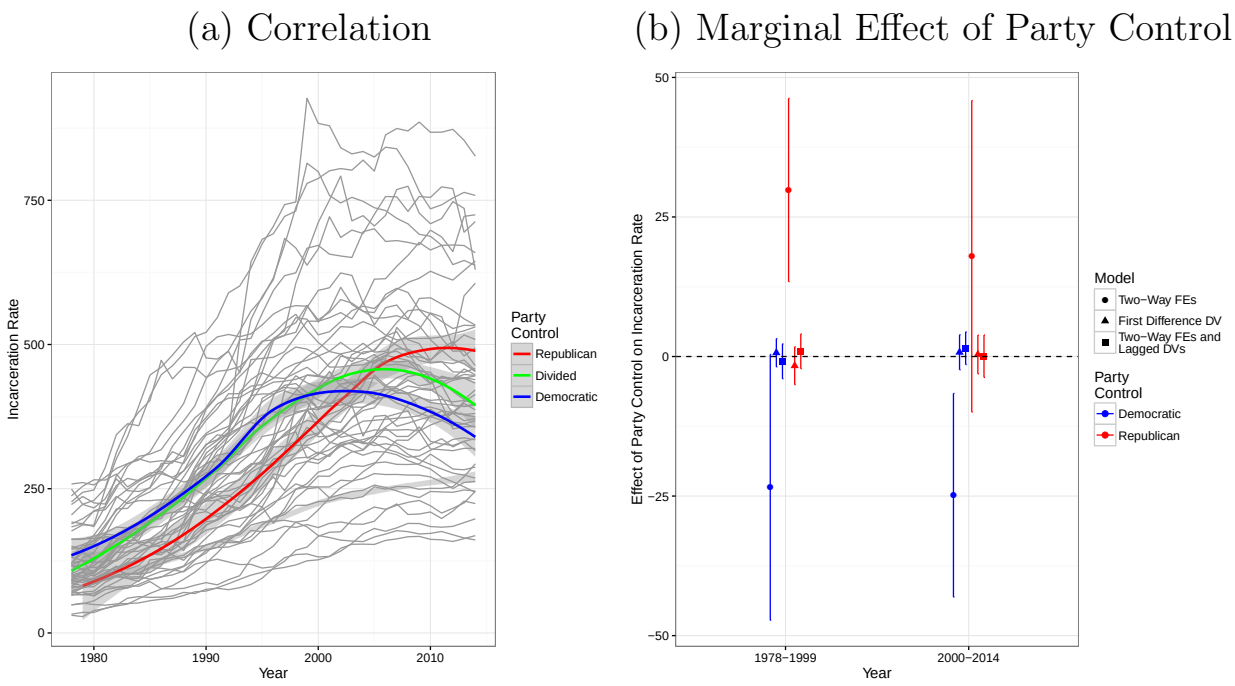
[24]Though the states' execution of 1445 individuals since 1976 is of great social consequence, it is less related to mass incarceration than the other policies because in all likelihood these individuals would have been given a life sentence had the death penalty not been in effect. Moreover, the death penalty is unique because Texas is responsible for nearly a third (542) of the executions in the U.S. since the death penalty was ruled constitutional in 1976.

[25]Data on incarcerated populations is from the Bureau of Justice Statistics (ICPSR 36281). Yearly state population estimates by race are from linear interpolation of decennial Census numbers (Weden et al. 2015).

per 100,000 residents is substantively minuscule in a society in which one in 36 adults are under correctional jurisdiction.[26]

More importantly, there is no evidence of a polarization of incarceration rates by party across time. This *decreased* effect of Republican control in the post-2000 period is a stark contrast to the rapid polarization in other policy areas.

Figure 2.4: Party Control and Incarceration



Note: **Incarceration *does not* polarize by party over time**. Plot (a) shows the average incarceration rate per 100,000 residents by state party control over time (using loess). Plot (b) shows the marginal effect of unified Republican control on the incarceration rate for the 1978 to 1999 period and the 2000 to 2012 period across three time-series model specifications. Models control for the crime rate at year $t-1$ (see Yates and Fording 2005).

I also provide estimates of the relationship between party control and the incarceration rate for black people in Appendix Figure A.12. Even more than for the overall incarceration rate, the black incarceration rate becomes *less polarized* after 2000. For most models prior to 2000, Republican control is associated with an increase in the black incarceration rate of about 100 per 100,000 residents, but the party differences decrease after 2000.

Overall and black incarceration rates do not appear to polarize in the states, but recent years have seen growing partisan conflict over the use of *private* prisons (Price and Riccucci

[26]This is the 2014 estimate from the Bureau of Justice Statistics, and it includes people on parole or probation.

2005).[27] The use of private prisons may be more polarized than overall incarceration because it may generate conflict not only over crime and punishment concerns, but also over profit incentives for punitiveness, reports of inhumane conditions, and the fundamental role of the state and the social contract (Shapiro 2011). I estimate the relationship between party control and the percent of inmates who are housed in privately owned facilities in Appendix Figure A.13, but only beginning in 1999 due to a lack of available data. Analogous bivariate and panel regression analyses suggest a modest relationship between party control and private prisons. After 2010, Democratic states have significantly lower proportions of inmates in private facilities. However, the panel regressions show at most a small effect of party control (less than 1%), which is only statistically significant in the model employing the first differenced dependent variable (not the two-way fixed effects or lagged models).

Health policy and criminal justice are substantively important and illustrative cases in which major socioeconomic outcomes are polarized to the extent that relevant *policies* are polarized. This pattern generalizes further. Like diverging tax rates, state governmental revenue and spending have polarized over time, with Democratic control predicting greater increases relative to Republican and divided states in recent years. In the polarized environmental policy area, carbon efficiency is also predicted by party control of government (see Appendix Figure A.14). In contrast, non-polarized policy in the issue area of education, like criminal justice, appears to be associated with non-polarized socioeconomic outcomes: Party control does not predict high school graduation rates any more in recent years than it does in earlier years (see Appendix Figure A.15).[28] Estimates of partisan differences in graduation rates are reduced further with the inclusion of state poverty rate in the time-series regressions.

2.6 State Resurgence

There are strong historical and theoretical reasons to expect state governments to be marginal players in American policymaking. Compared to the Federal Government, states face greater threat of exit from business and wealthy residents. Their legislatures have are poorer in terms of the time, money, and information required to change policy. Major interstate differences in policy, such as the legality of racial segregation or gender discrimination in employment, have been washed away by landmark federal policies. Yet this minimalist characterization of states has grown antiquated.

While the Federal Government grew more gridlocked, states implemented major policies that shape the lives of their residents. Federal laws from the 1930s through 70s decreased

[27]Price and Riccucci (2005) test the cross-sectional relationship between partisan and ideological variables and private incarceration for the year 1990. To the author's understanding, this is the first test of this relationship across time.

[28]The dependent variable is the average freshman graduation rate, the percentage of an entering freshman class that graduates high school in four years. Data is from the National Center for Education Statistics.

interstate variation in all of the policy issue areas described in this article. Since 1970, in contrast, interstate variation increased as some states implemented restrictions on guns, abortion, labor unions, welfare, and voter eligibility, while others loosened restrictions. Moreover, some of the most significant recent federal policies have served to increase interstate variation rather than decrease it. In addition to welfare devolution in 1996 (Soss et al. 2001), the Supreme Court ruling in *NFIB v. Sebelius* (2012) gave states great discretion in the implementation of the Affordable Care Act, the choice of whether to expand Medicaid and create a state-run health insurance marketplace (Beland, Rocco and Waddan 2016).

There are notable exceptions where Congress and the federal courts have decreased variation in state law, however. In a famous example of "coercive federalism," the National Minimum Legal Drinking Act of 1984 threatened to withhold federal highway grants from states that did not increase their drinking age to 21. The area of LBGT rights is also prominent. *Lawrence v. Texas* (2003) invalidated state sodomy bans. Though not included in this article because it occurred after 2014, the *Obergefell v. Hodges* (2015) case legalized same-sex marriage by invalidating state marriage bans.

The upward trend in interstate policy variation is not inevitable, however. In 2017, the Federal Government came under unified Republican control. The Trump administration has signaled a desire to act against state and local immigration and marijuana policies. Recent decades saw the buildup of considerable interstate policy variation, but an aggressive Federal Government may move the center of policymaking in American federalism back to the national government. Further research should investigate interbranch conflict in the polarized era.

Chapter 3

How Should We Measure State Policy?

3.1

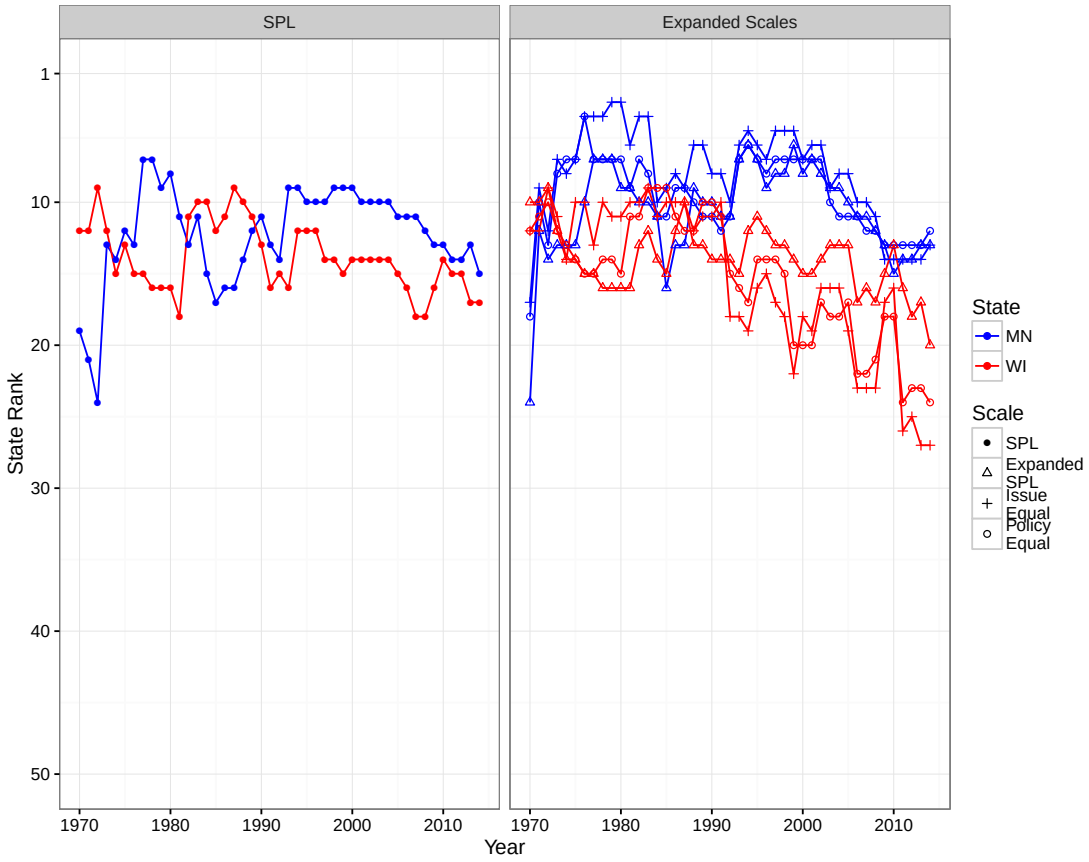
Scholars and journalists point to the divergence between Wisconsin and Minnesota as an extreme case of the polarization of state policy (Kersten 2011; Jacobs 2013; Hertel-Fernandez and Skocpol 2016). The *Milwaukee Journal Sentinel* has called policy change since 2010 "the greatest reordering of Wisconsin's politics in a century" (Stein 2015). Since 2010, while Wisconsin has restricted collective bargaining rights for state employees, cut taxes for corporations and high earners, banned abortions after 20 weeks of pregnancy, and rejected Medicaid expansion, neighboring Minnesota has accepted Medicaid expansion, raised the minimum wage, significantly increased taxes on high earners, and preserved its rights to abortion and collective bargaining.

A recent Bayesian IRT measure of "state policy liberalism" (SPL) represents a considerable advance in the study of policy dynamics (Caughey and Warshaw 2016), the role of party control in policy outcomes (Caughey, Xu and Warshaw 2017), and policy responsiveness to public opinion (Caughey and Warshaw 2017). However, the measure challenges the interpretation of policy divergence between Minnesota and Wisconsin. According to the SPL measure, relative to other states, Wisconsin was *more liberal* in 2014 than in 2008, and Minnesota and Wisconsin are *more similar* in state policy liberalism in 2014 than they were from the mid-1990s through the mid-2000s (see the left panel in Figure 3.1). As seen in the right panel, three alternative measures produced in this article show more divergence between the states after 2010.[1]

Why does the SPL measure obscure recent policy polarization in the Midwest? In this article, I discuss two issues in the SPL measure that speak broadly about the estimation of latent ideology. First, measurement error can arise due to limitations in the number or

[1]Here I plot the ideal point's rank, not the estimate itself, because Caughey and Warshaw (2015, 7) suggest that their measure is relative, not absolute.

Figure 3.1: Comparing Minnesota and Wisconsin



Note: **Plots show liberalism rank of Minnesota and Wisconsin 1970-2014**, with 1 representing the most liberal state. Expanded scales (right panel) show a greater divergence in both relative and absolute terms between Wisconsin and Minnesota after 2010.

quality of the items (in this case, state policies) in the data. Unlike roll-call scales, which typically employ the entire population of roll-call votes for one or more legislative sessions, policy data necessarily involves sampling. I show that ideal point estimates are sensitive to the sample of policy data. When I expand the sample of policies with 45 additional items and greater temporal coverage and apply an identical Bayesian IRT model to produce an *Expanded SPL* scale, the new ideal point estimates show greater growth of policy polarization in recent years, substantially more of which is driven by the Republican Party (compared to Caughey, Xu and Warshaw 2017).

A second issue is related to the assumptions of the latent dimension model (LDM) itself, that policy choice is spatial and the resulting latent dimension represents "ideology." Latent dimension models such as Bayesian IRT and factor analysis (hereafter called LDMs) derive the parameters for political actions—how far left or right each political action (e.g., a roll-call

vote) moves an actor's ideal point—from the empirical distribution of the data. One potential benefit of LDMs is the promise of parameters that are derived from the data and thus rely on fewer assumptions from researchers about how much a given political action should affect ideal point estimates. Substantively, however, the assumption of spatial behavior implies that a) liberal actors are liberal because they take liberal actions, and b) liberal actions are liberal because liberal actors take them.

This assumption is often violated in real world politics. For instance, Mississippi—a state with otherwise conservative policies—was the first U.S. state to legalize some abortions. When these violations occur, I find that the SPL model produces discrimination parameters that at times do not conform to substantive considerations about the ideological content and relative historical importance of policies. Modeling these violations on an additional dimension would require that many states act on the same additional dimension (Lauderdale 2010), but this is unlikely to occur. The SPL scale offers a unique opportunity to assess validity because, unlike roll-call scales, it relies on a medium-N set of recognizable, substantively important policies about which historical and normative scholarship provides strong priors.

I provide an alternative strategy to LDM for summarizing policy outcomes: simple additive indices (i.e., weighted averages) that I call *Substantive* measures. Whereas SPL and other LDM measures outsource the job of determining the ideology and importance of policy to the data, the Substantive measures rely on explicit assumptions from the researcher about the ideological content of policy prior to estimation. Like many traditional indices (e.g., see Erikson, Wright and McIver 1993), the Substantive measures require trust in the validity of expert coding, but the small number of policy items in the data makes this feasible. I provide two Substantive measures with relatively agnostic weighting assumptions for policies, and also simulate Substantive measures under a wide range of assumptions about the relative importance of different policies. The simulations show that LDM techniques may overestimate policy polarization earlier years and underestimate it in more recent years.

Scholars have increasingly turned to LDM techniques to derive measures of latent ideology from roll-call votes (Clinton, Jackman and Rivers 2004; Bailey 2007), judicial rulings (Martin and Quinn 2002), opinion surveys (Tausanovitch and Warshaw 2014), media outlets (Ho and Quinn 2008), characteristics of political systems (Treier and Jackman 2008), and, more recently, policy outcomes (Caughey and Warshaw 2016). While this shift has generated important insights for the study of politics, this article suggests that scholars should be aware of the assumptions and item parameters in LDMs before employing them for substantive studies.

3.2 Measures of Policy Liberalism

Effectively summarizing political activity on a left-right dimension is no easy task. The payoff, however, is considerable, and unidimensional summary measures have opened the possibility of testing major theories of politics. Similarly to ideal point measures based

on legislative voting, the SPL measure has already generated key insights about the roles of partisanship, institutional rules, and public opinion on political outcomes. But given the consequentiality of these debates for researchers and the public alike, the validity of the measure takes on additional importance. Analogous ideal point measures such as DW-NOMINATE are downloaded and used by substantive researchers, journalists, and practitioners, often with little knowledge of how the measures are constructed.

The SPL measure represents a major advancement in the study of policy outcomes and state politics, but it is one of many possible ways to summarize policy outcomes—and, like all measures, it has advantages and disadvantages. Time-series ideal deal point measures can differ in at least three ways. First, they can differ in the sample of policy items in the data. I collect data on additional policy items and extend the years of coverage for existing policy items. Second, the dimension can represent *relative* or *absolute* liberalism based on whether item parameters vary over time. Finally, a measure's item parameters—which determine the marginal affect of a policy on a state's ideal point—can be derived from the empirical data in the case of LDMs, or they can be set by the researcher in the case of additive indices such as the "Policy" measure from Erikson, MacKuen and Stimson (2002).

Table 3.1: Description of Measures

Measure	Type	Policy Items	Absolute or Relative	Equation
SPL	LDM	148	Relative	$y^*_{st} \sim N_j(\beta_j\theta_{st} - \alpha_{jt}, \Psi)$
Expanded SPL	LDM	193	Relative	$y^*_{st} \sim N_j(\beta_j\theta_{st} - \alpha_{jt}, \Psi)$
Factor Analysis	LDM	193	Absolute	$\theta_{st} = \sum w_j y_{jst}$
Substantive by Policy	Additive index	193	Absolute	$\theta_{st} = \sum u_j y_{jst}$
Substantive by Issue	Additive index	193	Absolute	$\theta_{st} = \sum (1/\sum w_{jg}) u_{jg} y_{jgst}$

Table 3.1 describes the characteristics of the SPL measure and the four alternative measures developed in this article. The Expanded SPL measure replicates the SPL measure with an identical Bayesian IRT model, but with additional policy items and data coverage across years. The Factor Analysis measure similarly derives item parameters from the empirical data, but estimates absolute liberalism because item parameters are static. The final two measures, called Substantive Indices, represent absolute liberalism, and, importantly, rely on expert-coded item parameters.

All of the measures summarize state policy outcomes on a left-right dimension across time. As suggested earlier by the case of Minnesota and Wisconsin, however, their different

constructions yield different estimates and substantive conclusions about dynamics in state policy.

Sampling Policy Data

As with any statistical model, the quality of the underlying data shapes the researcher's inferences. Among LDMs, those that measure public policy outcomes require special care in this regard. Whereas LDMs of roll-call votes and judicial rulings typically employ the entire population of votes in one or more legislative or judicial sessions (e.g., Poole and Rosenthal 1997; Martin and Quinn 2002; Clinton, Jackman and Rivers 2004; Treier and Jackman 2008), the complexity of statutory law necessitates that studies of policy change *sample* from a population of policies. The SPL scale appears to be the first published unidimensional LDM measure of public policy outcomes.

If one assumes that the latent dimension being measured represents a unified concept (e.g., liberalism), then the goal is to build a dataset of representative, unbiased sample of policies. Importantly, increasing the number of policies and years of coverage of the policies in the dataset will reduce measurement error in expectation (Ansolabehere, Rodden and Snyder 2008). In classical statistics, sampling error is reduced as the sample size increases, and in this basic sense it follows that increasing the number of policies in the sample increases the precision of ideal point estimates of policy liberalism.[2] This is also true in the IRT framework, where increasing the number of test items generally decreases standard errors of parameter estimates (e.g., Wingersky and Lord 1984).

As a first step, I expand the number of items in the policy dataset. I collect data for 45 major state policies, with which I augment data from Jordan and Grossmann (2016), which itself is in part comprised of data from Caughey and Warshaw (2016) and Boehmke and Skinner (2012), to create a dataset of 193 policies (135 of which cover the 1970-2014 period). The data I collect include policies of considerable historical importance. They include the most consequential shift in state health policy in a half-century, Medicaid Expansion under the Affordable Care Act, as well as the three principal state policies understood to be central to the rise of mass incarceration since the 1970s (Travis, Western and Redburn 2014), determinate sentencing laws, three strikes laws, and truth-in-sentencing laws. I also collect policies in the areas of labor (e.g., paid sick leave and paid family leave laws), abortion (e.g., gestation limits), campaign finance (e.g., limits on individual and PAC contributions), voting rights (e.g., voter ID and felon disenfranchisement laws), and many more. The Appendix provides descriptions and sources for each policy.

Relative or Absolute Liberalism?

The SPL model draws upon the Martin and Quinn (2002) and Clinton, Jackman and Rivers (2004) Bayesian IRT models to estimate ideal points for states based on their policy

[2]Similarly, in classical test theory, Chronbach's α is a measure of test reliability that is an increasing function of the number of test items (Terwee et al. 2007).

outcomes. In particular, the distribution of policy outcomes for state s in year t, y^*_{st}, is a function of the state's latent policy liberalism for that year, θ_{st}, as well as the policy's fixed discrimination parameter β_j and time-variant difficulty parameter α_{jt}. Subscript j denotes different policies, which are analogous to test questions in the IRT framework. In this equation, N_j is a normal distribution with j dimensions (as there are j policies). Ψ is a $J \times J$ variance-covariance matrix:

$$y^*_{st} \sim \ N_j(\beta_j \theta_{st} - \alpha_{jt}, \Psi) \tag{3.1}$$

This model equation contains important determinations about how ideal points will be estimated. The first is the time-variant nature of the difficulty parameters α_{jt}, which is modelled with a dynamic Bayesian random walk. Due to the time-variance of α_{jt}, Caughey and Warshaw (2015, 7) describe their scale as a measure of *relative* liberalism:

> [O]ne should bear in mind that the model allows the difficulty parameters α_t to evolve over time. As a result, aggregate ideological shifts common to all states will be partially assigned to the policy difficulties. Since states did adopt increasingly liberal policies over this period, the model partially attributes this trend to the increasing difficulty of conservative policies (and increasing "easiness" of liberal ones)...The price of this flexibility is that states' policy liberalism scores are comparable over time primarily in a relative sense.

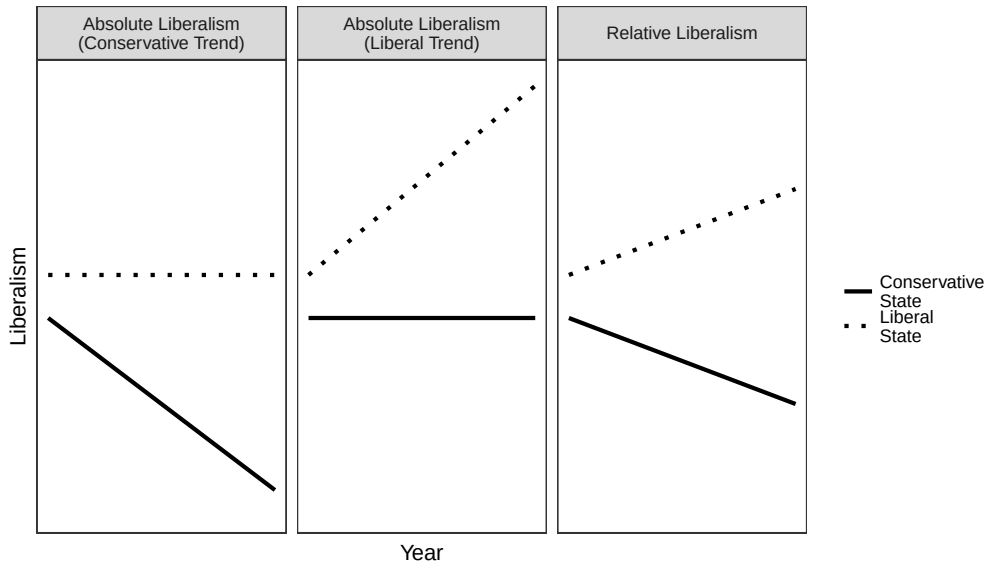
The benefit of time-variant a_{jt}, according to Caughey and Warshaw (2015, 7), is that "it helps avoid the interpretational difficulties of assuming that policies have the same substantive meaning across long stretches of time." The empirical data—how many states in year t have policy j and the amount of variance explained—changes the substantive meaning of a policy across time. The difficulty parameter for medical marijuana laws, for example, decreases substantially as the number of medical marijuana states grows from one in 1998 to 20 by 2014.

Substantively, this time-variance matters because, all else equal, the ideal points of states that adopt the policy early will be more influential in determining the policy's discrimination parameters, and the ideal points of early adopters will also be more greatly affected by policy change than late adopters. The normative assumption that early adopters of a liberal policy should be more liberal than late adopters of the policy is plausible, but not developed in the literature. In addition, the ideal point of state s not only change as the state adopts different policies; it changes as a function of the policy decisions of other states.

The *relative* nature of the SPL measure may complicate analyses of policy responsiveness to public opinion. In a spinoff paper, the authors aim to estimate policy responsiveness to public opinion using Bayesian IRT "measures of *absolute, not relative*, liberalism" of the mass public (Caughey and Warshaw 2017, 14, emphasis added). This research strategy generates two obstacles for inference. First, it is unclear how regressing their "relative" policy liberalism scale on "absolute" measures of public opinion (called "mass liberalism") produces an interpretable test of democratic responsiveness. The ideal point of state s is

affected by the changing status quo, but mass liberalism is a measure of liberalism "in absolute terms over time, independent of the policy status quo" (Caughey and Warshaw 2015, 202).[3]

Figure 3.2: Responsiveness to Relative vs. Absolute Liberalism



Note: The first two panels show increasing polarization driven by rightward movement of conservatives or leftward movement of liberals, respectively, measured in absolute liberalism. The last panel shows the same increase in polarization, but measured in relative, not absolute terms.

Figure 3.2 illustrates this concern with hypothetical trends in polarization. The first two panels represent *absolute* liberalism, in which polarization is driven by a rightward shift of conservatives or a leftward shift of liberals, respectively. However, as shown in the third panel, *relative* liberalism is determined by the distance between liberals and conservatives, obscuring the system-wide trend in absolute liberalism. The right panel, in other words, accurately represents the trend in *relative* liberalism in both the first and second panels because the distance between the conservative and liberal state is equivalent. Since public opinion is measured absolutely and policy outcomes are measured relatively, analyses may systematically over- or underestimate policy responsiveness.

Second, the parameters for policy items in the SPL scale may differ substantially from the parameters for survey question items in the mass liberalism model. Correlating two separate Bayesian IRT models leaves open the possibility that the parameters of the survey questions in the Caughey and Warshaw (2017) "mass liberalism" model do not correspond

[3]This issue is distinct from existing—and compelling—criticism that studies of ideological responsiveness (on one or two dimensions) conflate consistency with extremism, obscure important multidimensional variation, and provide little ability to assess whether politicians respond to the desires of citizens (Broockman 2016; Ahler and Broockman 2017).

well to the parameters of the state policies in the SPL model—e.g., the possibility that survey questions about abortion strongly influence state positions on the "mass liberalism" scale but abortion policies only weakly discriminate between ideal points on the "policy liberalism" scale—making it difficult to tell whether policy responded to what the public actually wanted. The next sections investigate policy item parameters in the SPL model.

Dynamic ideal point measures of roll-call votes, such as DW-NOMINATE, are similarly measures of relative liberalism because they treat multiple roll-call votes on the same bill as separate choice items.[4] For policy outcomes, however, substantive researchers and journalists typically speak in terms of absolute liberalism and the "past accumulation of policy" (see Erikson, MacKuen and Stimson 2002, Chapter 9). Policy outcomes are worthy of study because they affect the lives of members of the polity, and their effect is absolute, not relative. The elimination of Jim Crow laws, for instance, produces virtually no increase in policy liberalism (for the average state or average Southern state) according to the SPL measure.

The alternative measures, in contrast, are designed to represent *absolute* rather than relative policy liberalism. Policy item parameters are time-invariant such that the adoption or repeal of policy$_j$ has the same effect on a state's ideal point regardless of time period.

The Spatial Choice Assumption

The sample of items and the time-variance of item parameters are not the only ways that ideal point measures may vary. The item parameters themselves can be determined by the distribution of the empirical data in the case of LDMs, or by researchers' expert coding in the case of more traditional additive indices (e.g., Soss et al. 2001; Marshall and Jaggers 2002; Gadarian 2010).

Even sophisticated readers and substantive scholars may assume that researchers define the parameters of the survey responses, roll-call votes, or state policies used to estimate ideal points in LDM measures. Instead, they are derived from the data by generating ideal points or scores that best predict the empirical data.[5] For a roll-call scale, for instance, the modeller does not determine which roll-call votes are liberal or conservative.[6] The

[4]The marginal effect of voting on identical bills to repeal the Affordable Care Act, for instance, depends on the particular vote.

[5]The conceptualization of ideal points as the solution to a prediction problem is closely related to the blossoming "culture" of machine learning and algorithmic analysis in which "accuracy and simplicity (interpretability) are in conflict" (Breiman et al. 2001, 206).

[6]The modeller must set the polarity of a small number of parameters because the parameter space must be constrained in order to estimate a Bayesian IRT model (Clinton, Jackman and Rivers 2004). In dynamic Bayesian IRT models for time series measures, modellers also sometimes impute starting ideal point values for a small number of actors (Martin and Quinn 2002; Caughey and Warshaw 2016). The SPL model constrains spending policies, Equal Rights Amendment ratification, minimum wage for women, antiinjunction, fair employment and prevailing wage laws to have positive discrimination parameters, and constrains right-to-work laws and bans on interracial marriage to be negative. Overwhelmingly,

statistical model estimates the parameters for each vote as functions of which and how many legislators vote for it, and the parameters of the other roll-call votes that those legislators also supported. In other words, the *actions of political units* determine the parameters of each action. Specifically, for a roll-call scale, the parameters of each floor vote are a function of the particular combination of legislators that votes yea on each bill (*clustering*) and the number of legislators who vote yea (*uniqueness*). In the case of the Caughey and Warshaw (2016) SPL model, which direction and how far particular policies push a state's "policy liberalism" score are determined by the timing of and combination of state that adopt the policies.

The derivation of ideal points from empirical data is an appealing feature of LDM techniques because it may require weaker assumptions from researchers about the ideological content of political actions. LDM measures, in the words of Bonica (2013, 306), "allow the data to speak to the question at hand" without assumptions. Caughey and Warshaw (2016, 7) offer an additional justification of LDM methods for historical data, suggesting that it "helps avoid the interpretational difficulties of assuming that policies have the same substantive meaning across long stretches of time."

LDMs, however, rely on the assumption of *spatial choice*: All actors are assumed to make decisions that minimize the difference between observed choices and their true ideal point. In the case of state policy, a state chooses to adopt a binary policy if it moves the state's outcomes closer to its ideal point. Closely related is the assumption of *local independence* (Treier and Jackman 2008): Conditional on the ideal point θ_{st}, item variables y_{jst} are uncorrelated. In substantive terms, LDMs assume that *liberal actors are liberal because they take liberal actions* and *liberal actions are liberal because liberal actors take them.*

The spatial choice assumption allows scholars to interpret ideal points produced by LDMs as measures of ideology. Caughey and Warshaw (2015, 4) call their scale "policy liberalism" and claim that the measure "summarizes the global ideological orientation of state policies." Liberalism, in their words, "involves greater government regulation and welfare provision to promote equality and protect collective goods, and less government effort to uphold traditional morality and social order at the expense of personal autonomy" (3).

Like roll-call votes (Clinton 2012), however, public policy outcomes may not always be the result of spatial considerations. Some *conservative* policies are more likely to be adopted by *liberal* states, and vice-versa. For example, conservative states might increase sales taxes as a substitute for more progressive income taxes. Because sales tax increases are correlated in the empirical data with other conservative policy items, the LDM model would estimate that sales tax increases make state ideal points more conservative—despite our qualitative understanding of sales taxes as a liberal policy (though probably less liberal than income taxes). Other examples are abortion policy, where otherwise conservative Southern states were first to adopt liberal abortion laws, and anti-communist laws, which arose in liberal states. But because LDM models assumes spatial behavior and fit the observed data

however, the observed data drives model estimates.

accordingly, violations of the assumption of spatial behavior produce ideal point estimates that do not reflect "true" ideal points.

The fitted LDM model estimates ideal points, but it also estimates *parameters* for the political actions in the observed data. These parameters provide an opportunity to assess the substantive validity of an LDM, such as an IRT model. In its most popular current form, IRT models allow items j to vary by two parameters: the item's *difficulty* α_j (based on the percent of actors who answer the question correctly) and its *discrimination* β_j (based on how well it separates actors on the latent dimension). A particular roll-call vote (e.g., the vote on the Affordable Care Act in the U.S. House in 2009) or survey question (e.g., "Do you support additional restrictions on immigration to the U.S.?"), like a test question in educational research, gets a difficulty parameter based on the percentage of individuals who vote or respond affirmatively to the question and a discrimination parameter based on how well a correct answer predicts an individual's placement on the underlying scale. Discrimination parameters, which can be negative or positive, also determine the ideological "direction" of an item. To measure a latent "liberalism" ("conservatism") dimension, then, all else equal, giving an additional liberal (conservative) answer or roll-call vote should increase the individual's ideal point estimate while an additional conservative (liberal) answer will decrease it.

In the next section, I discuss parameter estimates that suggest a breakdown in the spatial choice assumption, and, in turn, that the measure does not represent ideology.

Policy Parameters

In this section, I show how the policy parameters produced by the SPL model sometimes do not conform to historical and normative considerations about the ideological content of policy. When qualitative judgement about the ideological content of an item contradicts the item's estimated parameters, there are two potential ways to respond. The first option is to reaffirm the spatial choice assumption and conclude that our qualitative judgement is wrong; the item's parameters represent the true ideological content of the item. The second is to reject the spatial choice assumption and impose more expert judgement in defining item parameters.

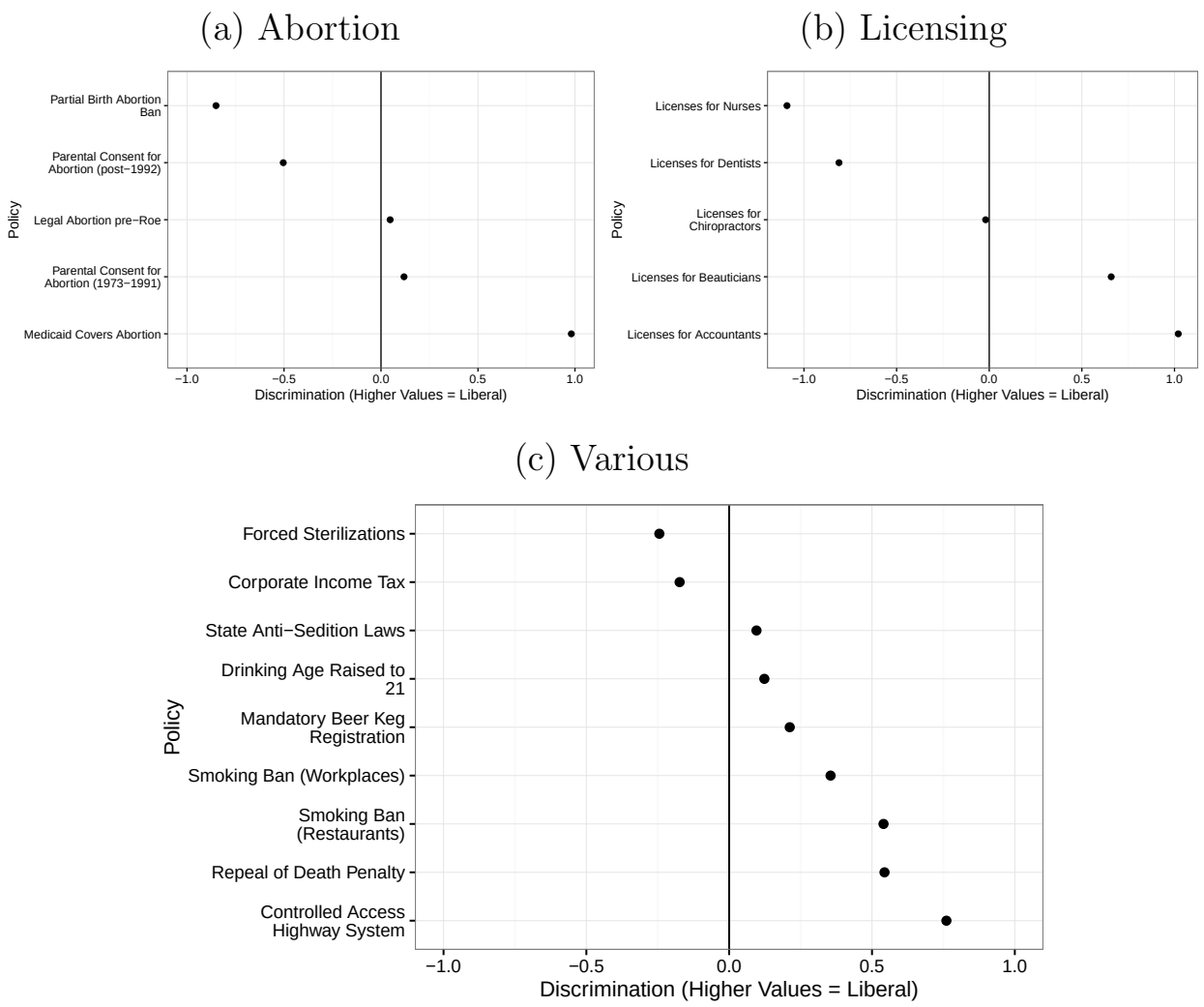
A policy's discrimination parameter determines whether passing the policy increases or decreases a state's policy liberalism, and by how much. (The difficulty parameter is a scaler that, all else equal, also affects the magnitude, but not the direction, and action moves ideal points.) One way to judge the measure is with expert validation of model parameters and ideal point estimates. Indeed, expert validation played a large role in the development of NOMINATE and other LDM measures.[7] I replicate the SPL model and plot a sample of discrimination parameters in Figure 3.3 below. The parameters in Figure 3.3 are chosen for

[7]In the early development of NOMINATE, for instance, "Poole and Rosenthal...relied upon their knowledge of American political history to see if the output made visual sense" (Carroll et al. 2009, 265).

purposes of conceptual illustration of the spatial choice assumption; I report all policy item parameters in the Appendix.

The dimension is dominated by policies related to racial discrimination and the welfare state. The discrimination parameters for policies such as fair employment laws, Social Security general assistance payment levels, worker's compensation, and bans on racial discrimination in public accommodations are by far the largest in magnitude.[8] As expected, the discrimination parameters are large and positive for these policies, which means that a state's estimated policy liberalism *increases* as the policy takes effect.

Figure 3.3: Discrimination Parameters (Caughey and Warshaw 2015)



[8]Curiously, however, the parameter for post-1964 fair employment laws is many times greater in magnitude than that of segregation in public schools.

Table 3.2: Examples of Issues with Policy Parameters

Wrong Direction	*Magnitude Too Small*	*Magnitude Too Large*
Corporate income tax	Forced sterilizations	Mandatory beer keg rental registration
Occupational licensing policies	Legal abortion pre-Roe	Smoking ban (restaurants)
Parental consent for abortion (1973-1991)	Repeal of death penalty	Fair employment laws (post-1964)
State anti-sedition laws		

As shown in Figure 3.3, however, other discrimination parameters are substantively questionable. Plot (a) shows discrimination parameters for the abortion policies in the Caughey and Warshaw (2016) model. While Medicaid coverage for abortion procedures makes states more liberal and bans on partial birth abortion make states less liberal, laws that mandate parental consent to obtain an abortion point in different directions for the eras preceding and following the *Casey v. Planned Parenthood* Supreme Court decision. The pre-*Casey* and post-*Casey* laws are substantively similar, but their discrimination parameters point in opposite directions due to the cluster of states that happened to pass the laws. States that passed the pre-*Casey* consent laws tended to have more liberal ideal points (due to the other policies they also had on the books) than did the states that passed the post-*Casey* consent laws—resulting in a liberal discrimination parameter for pre-*Casey* consent laws and a conservative one for post-*Casey* consent laws. This does not reflect the substance of the laws: Many pre-*Casey* laws were stricter (i.e., less "liberal") than post-*Casey* laws (Ziegler 2014). It is easy to imagine a substantive scholar drawing problematic inferences because he or she incorrectly assumed that the SPL measure treats pre-*Casey* consent laws as conservative.

Moreover, laws that legalized abortion before *Roe v. Wade* barely register on the dimension at all. Substantively, the model weights Medicaid coverage for abortion (discrimination = 0.98) *twenty times greater than legal abortion itself* (discrimination = 0.047). On average, a state that legalizes abortion receives a tiny increase in its liberalism score, but one that provides Medicaid funding receives a huge jump. Why is this the case? Again, the answer is found in the particular combination of states that passed the policies. In 1966, Mississippi, a conservative state in other policy areas (e.g., civil rights, labor relations, welfare, and taxation), was the first state to decriminalize some abortions, and subsequent early adopters of legal abortion (e.g., North Carolina) were not particularly liberal states either (Tatalovich 2015).

Other issues arise in the parameters of policies about which policy researchers have weaker priors. Plot (b) shows discrimination parameters for occupational licensing policies, a generally non-ideological policy area characterized by the politics of concentrated interests (Willbern 1954; Lowi 1967). Due to the idiosyncratic historical patterns of licensing policies, policies mandating licenses for beauticians and accountants have large *positive* discrimination parameters, while licenses for nurses has a large *negative* discrimination parameter. One might not expect licensing policies to be captured well by this "liberalism" dimension, but the size of the discrimination parameters gives pause. In fact, its discrimination parameter makes nurse licensing the *fourth most conservative* policy in the entire Caughey and Warshaw

(2016) model, behind only racial segregation in schools, bans on interracial marriage, and right to work laws. The magnitude is larger than those of historically important conservative laws, such as bans on sodomy (Halley 1993), Stand Your Ground laws (Lave 2012), and laws that make English the official state language.

Plot (c) displays various other discrimination parameters that are individually or collectively unexpected in their relative magnitudes. *Forced sterilizations*, which represent one of the most insidious uses of state power in U.S. history, have a mildly negative (conservative) discrimination parameter—a parameter whose magnitude is almost eclipsed by that of relatively inconsequential policies mandating that beer keg rentals be registered. Compared to mandatory beer keg rental registration, raising the drinking age to 21 (which involves greater use of state authority and affects many more people) has a smaller parameter. The discrimination parameter for repealing the death penalty is equivalent to that of restaurant smoking bans, and much smaller than that of controlled access highways (for a comparative analysis of the death penalty see Hood and Hoyle 2015). Relative to restaurant smoking bans, workplace smoking bans have a much smaller discrimination parameter.

Finally, the discrimination parameters for state anti-sedition laws and corporate income taxes are in the wrong direction. Anti-sedition laws, which states used to prosecute communists, increase a state's liberalism score—again, because states with other liberal policies were disproportionately the ones to implement them. Similarly, the corporate income tax receives a conservative discrimination parameter because Southern states (e.g., Alabama, Arkansas, Georgia, Mississippi, North Carolina, Tennessee, and Virginia) were especially likely to have the policy.[9]

The parameter for anti-sedition laws offers an intuitive example of a violation of the spatial choice assumption. It is likely that relatively liberal states passed the laws because that is where alleged communists lived. The presence of communists may be endogenous to other liberal policy outcomes. This does not, as LDM models assume, imply that anti-sedition laws are therefore liberal; instead, it suggests that policy choice is nonspatial.

These and other examples show how the peculiar historical record of which states implement which policies at which time determine the ideological content of policies (the discrimination parameters), which in turn are used to calculate states' SPL ideal points across time.

Traditional methods of assessing the validity of LDM measures are unlikely to be helpful. The most common validity check is to estimate an LDM's predictive capacity. If the model fits the data well, most state policies in a given year can be predicted by a state-year's SPL ideal point, θ_{st}. The Caughey and Warshaw (2016) model performs moderately well, correctly classifying about 82% of state policy outcomes for binary yes/no policies. Yet the percent of policies correctly predicted by the model is not necessarily a good metric of the quality of the model for substantive inference. It could be the case that the most

[9]It is possible that the corporate income tax served as a more regressive alternative to other taxes such that its adoption does indeed relfect conservatism, but it does not follow that a state with a corporate income tax has more conservative policy relative to a state with no taxes of any kind.

substantively important policies are incorrectly predicted, while relatively inconsequential policies are nearly perfectly predicted.

Furthermore, the problem of violations to the spatial choice assumption cannot be solved by modeling additional latent dimensions. As Lauderdale (2010) shows with respect to roll-call measures, additional dimensions require that *many* states act spatially on the same additional dimension. There are too few states and too few major policy items to estimate, for instance, an additional dimension for abortion, anti-communist, or occupational licensing policies.

3.3 Additive Indices

If the data cannot simply tell us the ideological content of policy, how should it be determined? Instead of deriving the ideological content from the potentially idiosyncratic clustering and uniqueness of actions across political units, researchers would have to justify the ideological content of political activity—of voting yea on a bill, of answering "yes" to a survey question, of contributing to a political candidate, or of adopting a state law. Unlike subjects on which research have weak priors about the relative and absolute importance of particular political actions in an abstract concept (e.g., Treier and Jackman 2008, 201), researchers have strong priors about major policies and a large body of historical scholarship to inform them.

In political science, LDM estimation arose in large part due to the implausibility of substantively coding the ideological direction and magnitude of thousands of roll-call votes, many of which have limited, contradictory, or obscure ideological content (e.g., omnibus and procedural bills). Policy data is different. There are hundreds of bills that reach the floor in each congressional session and thousands per year in state legislatures (Shor and McCarty 2011), but even the most ambitious datasets of policy outcomes have observations numbering only in the low hundreds (e.g., the "landmark law" dataset from Mayhew (1991) contains 262 laws, and the Caughey and Warshaw (2016) data contains 147 policies).[10]

I offer Substantive measures, simple averages of the liberalism of policy outcomes based on expert coding of the ideological content of policy items. These measures do not rely on the spatial choice assumption, but they necessitate trust in the "hard graft" of expert coding (Laver and Garry 2000, 619). Though it is unrealistic for roll-call votes, it is within the realm of possibility for scholars to gain deep substantive understanding of this medium-N set of policies. Major studies of policy change and responsiveness have spent considerable effort on qualitative and expert judgement in collecting and coding samples of laws (e.g., Mayhew 1991; Erikson, Wright and McIver 1993; Erikson, MacKuen and Stimson 2002; Maltzman and Shipan 2008; Lax and Phillips 2012).

The Substantive Scales potentially offer two advantages over LDM measures. First, their parameters are transparent and customizable. Substantive scholars can easily tweak

[10]Thus, in practice the sample frame is usually the policies for which there is available data from government agencies, interest groups, research organizations, and extant literature.

parameters according to their research question, and decisions about parameters can be more easily discussed and justified or criticized with normative, historical, and socioeconomic considerations. Second, the additive nature of the Substantive Scales allows the researcher to seamlessly separate a unidimensional measure into issue area-specific measures (e.g., an environmental policy scale) while maintaining the original policy parameters. In contrast, estimating a new LDM model with a subset of items produces an entirely new set of item parameters, which are not easily comparable across models.

Calculating Substantive Measures

Whereas the SPL scale is based on policy parameters derived from the data, the estimation of substantive policy scales requires the researcher to specify three parameters prior to estimation. Individual policies, such as the state tax rate on the wealthy or a ban on late term abortions, are assigned two parameters: its *ideological content* (its "direction"), and its relative *substantive importance* (its "weight"). Issue areas, categories of individual policies (e.g., tax policy or abortion policy), can also be assigned their own weight parameter based on relative *issue area substantive importance*.

We start with a vector of policies j for state s at year t, called X_{jst}. Because policies can be binary, ordinal, or continuous, I standardize their range. Each policy j in X_{jst} is scaled from $[0, 1]$. A binary policy, which a state either has or does not have, such as a right to work law, takes on the values of 0 or 1, whereas an ordinal or continuous policy, such as a tax or minimum wage, is transformed to the $[0, 1]$ scale with the function $(x_{st} - min(x))/(max(x) - min(x))$.[11]

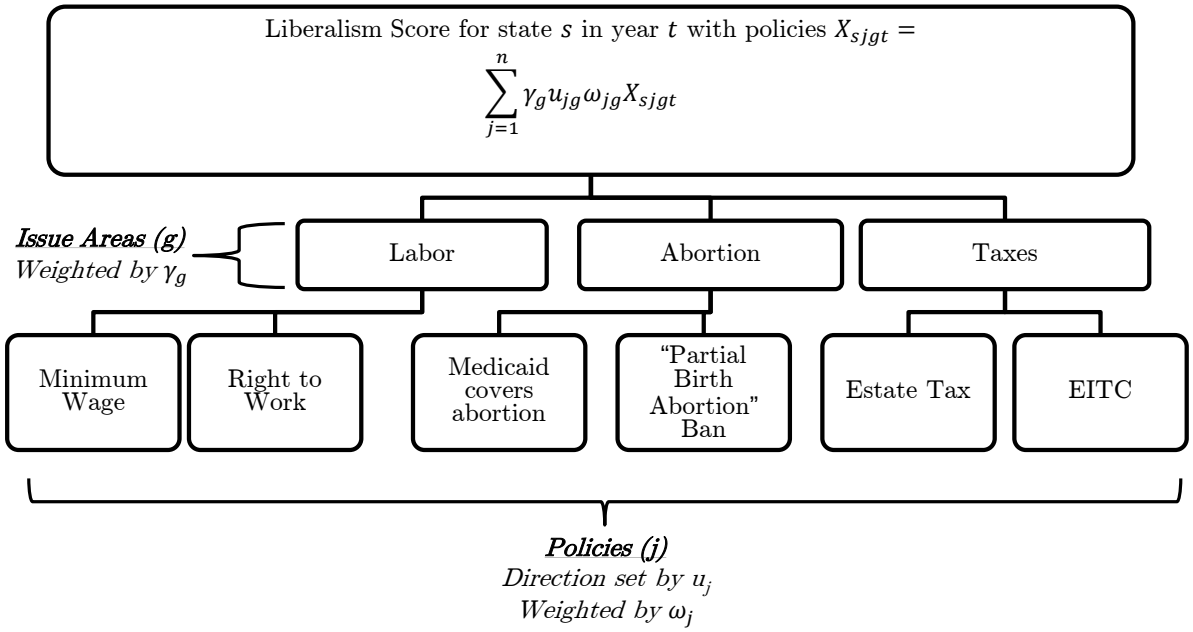
The estimation strategy then proceeds from the bottom up in two parts in order to estimate a score for state s in year t. First, I construct additive scales for each issue area. The issue area score of state s within issue area g are an additive function of its policies X_{jgst} (again, where for binary policies $X_{jgst} = 1$ if a state has the policy in year t and 0 otherwise). As described, each policy j has two parameters, its ideological content $u \in \{-1, 1\}$ (its "direction"), and its weight ω_j, where $\omega_j \geq 0$. The researcher assigns liberal policies a u of 1 and conservative policies a u of -1, analogous to the direction (sign) of a discrimination parameter in the IRT framework.[12] The score of state s in issue area g at year t is thus $score_{gst} = \sum u_{jg}\omega_{jg}X_{jgst}$. As the default, a researcher can weight each policy equally by keeping each policy weight ω equal to 1.

It is straightforward to aggregate up to unidimensional left-right estimates for each state, which incorporate all policies. Importantly, policy parameters in the unidimensional measure remain the same as those in the issue area measures—something that is not possible to do

[11]Range normalization is not required; subtracting the mean and dividing by standard error is a viable alternative, but liberal policies will need to be restricted to positive values for the measure to remain absolute rather than relative.

[12]The dimension does not have refer to the traditional "left-right" concept, however. A researcher can assign ideological content u_j based not on liberalism, but instead based on another normatively informed dimension like libertarianism.

Figure 3.4: Substantive Scaling Procedure



Note: Diagram describes the procedure for producing an additive left-right scale under researcher-defined parameters for policies j and issue areas g.

with data-driven techniques because they require the estimation of a new model with new parameters. Individual policies j are grouped into issue areas g, such as abortion policy, tax policy, and environmental policy. The researcher assigns each issue area g an issue area weighting $\gamma_g \geq 0$. A state's left-right score y^*_{st} is thus equal to $\sum \gamma_g u_{jg} \omega_{jg} X_{jgst}$. Issue area weights γ_g are a function of ω_{jg}, of course, and the equation can therefore be further simplified (i.e., the inclusion of γ_g is mathematically redundant). However, separating the policy-specific ω_{jg} from the issue-specific γ_g increases the transparency of the researcher's theoretical justification for the parameter values. Justifications based on the substantive importance of specific policies are easy to express with ω_{jg}, while theoretical rationales based on the specifics of issue areas can be expressed through γ_g.

I then construct two substantive measures, each of which prioritizes different considerations to produce a reasonable summary of left-right policy outcomes. First, assigning all $\gamma_g = 1$ again weights each policy equally, because the equation reduces to $y^*_{st} = \sum u_j X_{jt}$ (i.e., a weighted sum of a state's number of left policies minus its number right policies in a given year). I call the scale derived from this specification the *Equal Policy Scale*.

Second and in contrast, the researcher can weight each *issue area* equally by assigning issue area weights γ_g to be the inverse of the sum of the policy weights ω_{jg} in issue area g (i.e., such that $\gamma_g = 1/\sum \omega_{jg}$). I estimate liberalism scores with this specification, and call it the *Equal Issue Area Scale*.

Table 3.3: Substantive Scale Specifications

Specification	*Policy Weights* ω_{jg}	*Issue Area Weights* γ_g
Equal Policy Scale	$\omega_{jg} = 1$	$\gamma_g = 1$
Equal Issue Area Scale	$\omega_{jg} = 1$	$\gamma_g = 1/\sum \omega_{j \in g}$

The researcher can further customize the specification based on theoretical and normative judgement. Many policies are of obviously greater social consequence than others. For instance, among environmental policies, greenhouse gas emission caps and car emission standards probably deserve greater weight in determining a state's ideal point than do e-waste recycling programs or deposits for glass bottles. This can be done easily by increasing the policy weight ω_{jg} for policies that the researcher determines to be especially significant. As seen earlier, SPL policy parameters do not correspond to substantive importance, but the additive measures allow a researcher to fully customize parameters prior to estimation.

Finally, I normalize each left-right scale to the $[0, 1]$ interval for ease of interpretation (though the research can choose any rescaling procedure she prefers).

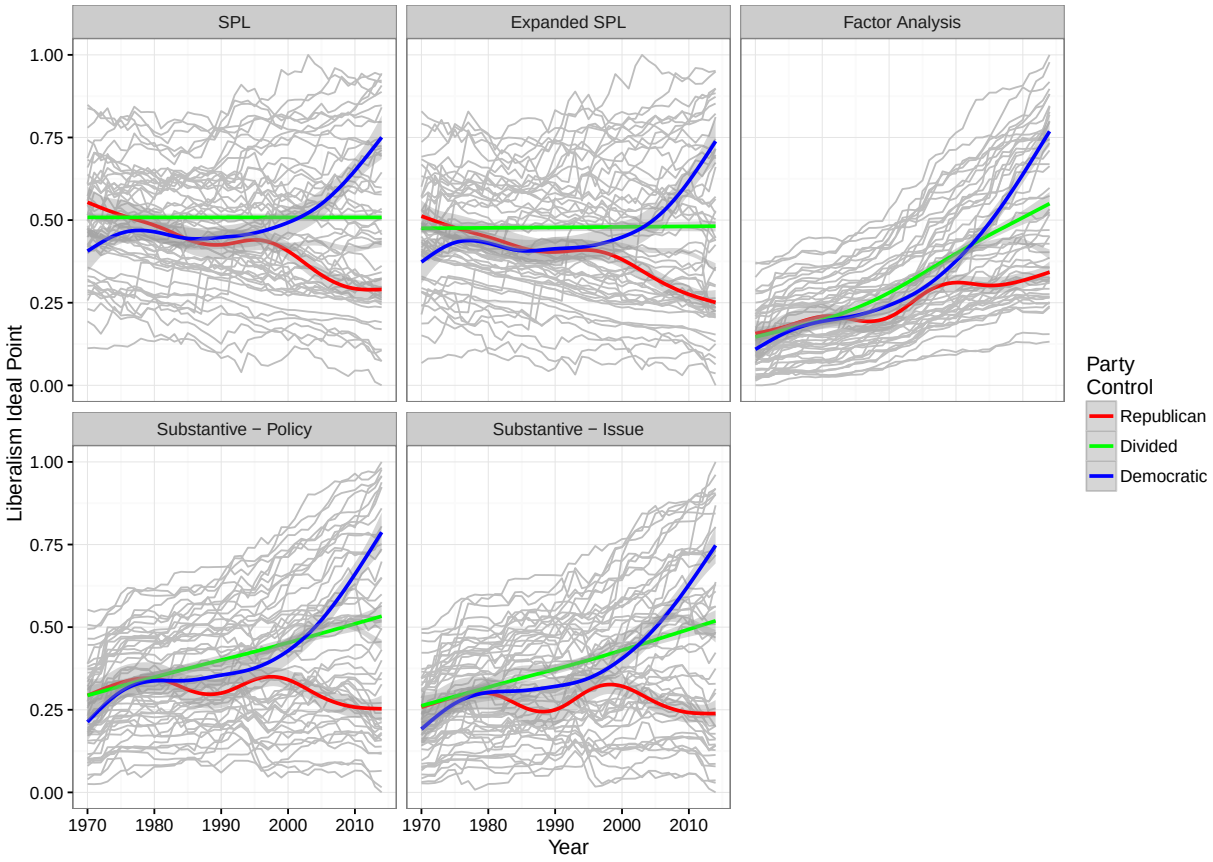
The Appendix compares the SPL and alternative measures in greater detail. Of particular importance is the correlation of the measures. The Substantive measures and the SPL measure are highly correlated (with an average yearly correlation of 0.93, see Appendix Figure B.5). Although the correlations between the measures are large, the next section compares analyses of policy polarization—and shows important differences across the measures.

3.4 Estimating Policy Polarization

But how do the measures perform in testing theories of polarization? Of primary interest is the increasing likelihood of Democratic state governments to pass liberal policies (and not conservative ones) and the increasing likelihood of Republican state governments to pass conservative policies (and not liberal ones). There are strong reasons to expect increasing policy polarization in the states. At the federal level, the Democratic and Republican parties have polarized in their legislative voting behavior (McCarty, Poole and Rosenthal 2006), as well as in their substantive policy agendas (Hacker and Pierson 2005). There is also evidence that this polarization is asymmetric, where Republicans vote along party lines in Congress more consistently (McCarty, Poole and Rosenthal 2006), promote more extreme policies Hacker and Pierson (2005), and employ more scorched-earth parliamentary tactics than do Democrats (Mann and Ornstein 2013). Although recent research investigates polarization in state legislative voting (Shor and McCarty 2011) and the declining regionalism of politics (Hopkins 2018), there has been little work that addresses the policies that the parties in government implement at the state level. Recently, Caughey, Warshaw and Xu (2017, 1)

test the effect of party control of state government on the SPL measure and find that the "the policy effects of party control have approximately doubled in magnitude."

Figure 3.5: Party Control and Policy Liberalism Across Measures



Note: Grey lines represent state ideal points. The colored lines plot the average ideal point for states under unified Democratic control (blue), Republican control (red), and divided government (green) using loess regression.

Figure 3.5 plots state ideal points from 1970 through 2014 and the average ideal point of states under unified Democratic control (blue), Republican control (red), and divided government (green) using loess. All of the measures show polarization between Democratic and Republican policy outcomes, especially after the year 2000. Of note is the difference between the relative measures and absolute measures. The SPL and Expanded SPL measures, which represent relative liberalism, show virtually no shift in average liberalism of state ideal points over time; the other measures, which represent absolute liberalism, show an approximate doubling of average liberalism (or quadrupling in the case of the Factor Analysis measure).

To analyze polarization over time, however, these correlations are insufficient. It is necessary to test the changing effect of party control on policy *change*. Using various panel regression specifications, I estimate the marginal effect of unified party control of government on policy outcomes for two time periods, the 1970 to 2000 period and the 2000 to 2014 period.[13] Figure A.4 plots marginal effects from dynamic panel regressions that include fixed effects for state and year and lagged terms for dependent variable, and regressions with a first difference dependent variable representing $\Delta Liberalism$.[14] The Appendix provides results for additional model specifications, such as more traditional two-way fixed effects models.

At first glance, the estimates in Figure A.4 appear quite similar (perhaps with the exception of the estimates using the Factor Analysis measure)—but a closer look shows important differences across the measures. Across the measures, the results vary in three important ways: the size of the estimated party effects, the difference between the effect before and after the year 2000, and in the symmetry of the Democratic and Republican estimates. Recall Figure 3.1, where the Expanded SPL and Substantive measures showed greater policy divergence between Minnesota and Wisconsin in recent years than did the SPL measure. Figure A.4 suggests that the alternative measures show greater policy polarization more generally.

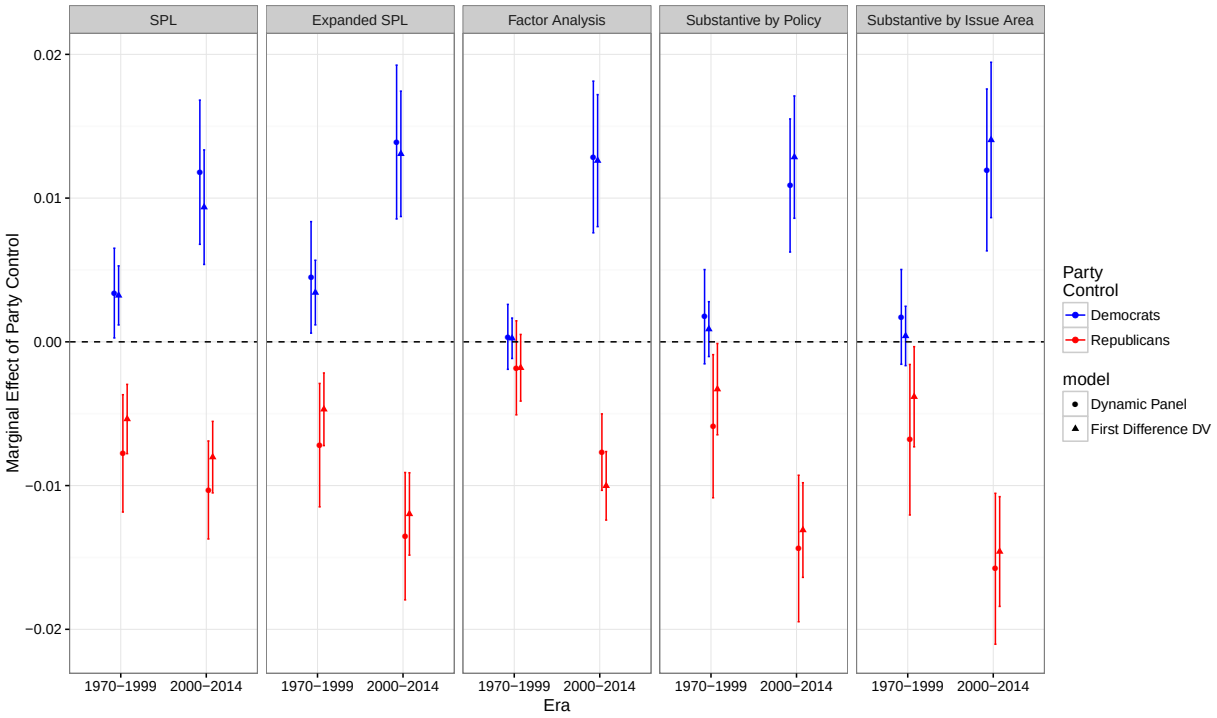
The SPL and Expanded SPL measures show very similar party effects prior to 2000, but the other models yield smaller estimates for this period. The Substantive measures show smaller pre-2000 effects, especially for Democratic states. The Factor Analysis measure stands as an outlier, with pre-2000 party effects that are close to zero. From 2000 onward, the party effects are much larger and the differences across measures much greater. The Democratic effect remain largely static across the measures, but this is not so for the Republican effect. With the exception of the Factor Analysis measure, the alternative measures show an effect of Republican control in recent years that is at least 30 percent larger than the SPL estimate.

Importantly, the SPL scale shows *no significant increase in the effect of Republican control in the post-2000 period*. This is an unexpected conclusion in light of qualitative studies of the dramatic policy changes that have occurred more frequently in red states in recent years (e.g., Kersten 2011; Jacobs 2013; Bulman-Pozen 2014; Hacker and Pierson 2016; Hertel-Fernandez and Skocpol 2016), which, if true, would have dramatic consequences for our understanding of state politics and partisanship. In terms of the difference between the pre- and post-2000 effect of Republican control, the alternative measures show an increased effect of at least

[13] I choose the year 2000 to strike a balance between the precision gains of longer time periods and the highlighting of the hyper-polarization of American politics in the most recent years. I provide analogous estimates using other cutoff years for the eras (e.g., pre- and post-1992) in the Appendix. In addition, I use unified party control because the combination of divided government and polarization may produce legislative gridlock (Binder 2003).

[14] Transforming the dependent variable into its first difference protects against spurious relationships when a time series is non-stationary (Levin, Lin and Chu 2002). I also execute the Levin, Lin and Chu (2002) test, which tests a null hypothesis that the panels (in this case, each state time series) contain a unit root. The null is rejected for a test with a time trend ($p < 0.001$) and with both a time trend and subtracting the cross-sectional means ($p < 0.05$).

Figure 3.6: Effect of Party Control Pre-2000 and Post-2000



Note: **Substantive scales show a larger increase in party effect before and after 2000, and greater effects of Republican control (both in absolute terms and relative to the effect of Democratic control).** Dynamic panel models include state and year fixed effects, state × era fixed effects, and lagged dependent variables for years $t-1$ and $t-2$. First difference models include year fixed effects. Standard errors are clustered by state in all models.

double the magnitude of the increase for the SPL estimates. The differences between the SPL measure and the alternative measures is partly due to the expanded sample of policy items employed in the alternative measures. However, there are additional differences due to variation in measure specification.

In terms of the proportional increase in the party effect before and after 2000, the differences are starker. The SPL measure suggests that the Republican effect from 2000 onward is larger than the pre-2000 effect by a factor of 1.3. The ratio of this increase is about 1.9 for the Expanded SPL measure. The Substantive by Policy and Substantive by Issue Area estimates, however, suggest that the ratio of this increase is about 2.5 times and 2.3 times, respectively. The Factor Analysis measure, again an outlier, suggests that the ratio is 4.2 times.

In terms of partisan symmetry, all of the LDM measures (those that derive item parameters from the data) show a larger Democratic than Republican effect on policy outcomes. In contrast, the Republican estimates are of greater magnitude than the Democratic estimates

for the Substantive measures.

These different estimates lead to different substantive conclusions about policy polarization and its (a)symmetry, as well as the importance of party control of government in determining outcomes more broadly. For scholars and practitioners who want a simple number or statistic to guide their analysis in the increasingly contentious arena of state politics, these differences matter greatly.

3.5 Policy Polarization across Many Measures

Although the expert coding of item parameters for the Substantive measures provides advantages over LDM techniques, scholars are often concerned about introducing human bias in coding items. Are my particular biases, ideological or otherwise, driving estimates and the differences between the LDM measures and the Substantive measures? Other expert coders may view the ideological direction and weight of policy items differently. They may want to exclude from the measures certain policy items because they lack ideological content or are socioeconomically inconsequential, or develop a highly specific weighting scheme that prioritizes certain kinds of policies over others. What do estimates of policy polarization look like across many different additive indices?

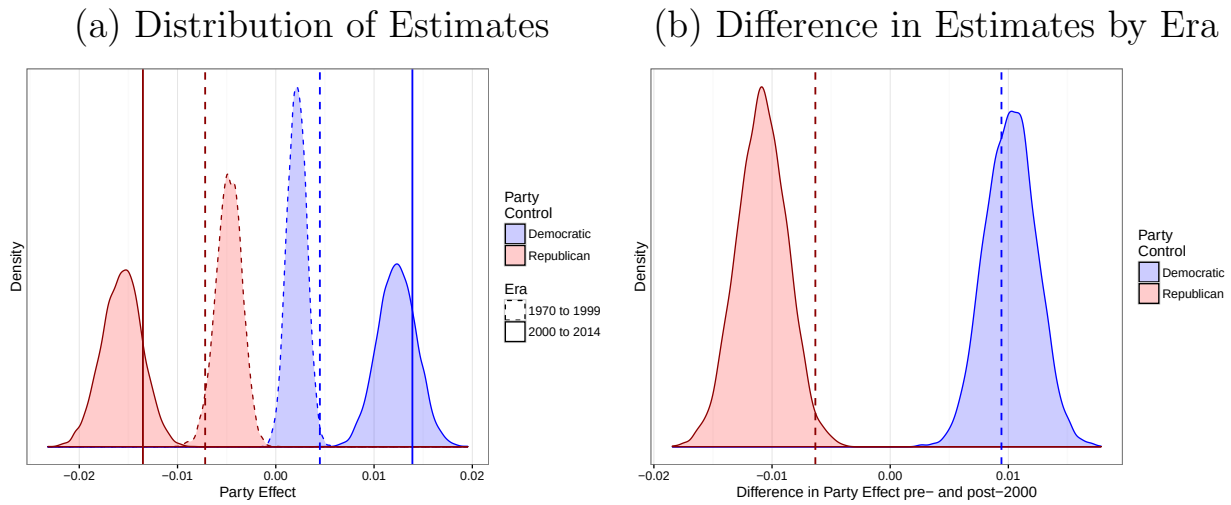
I address this concern with Monte Carlo simulations. In particular, I simulate 10,000 different additive measures of policy liberalism based on randomly generated item parameters. I randomly assign each policy j a weight ω_j between 0 and 1 (inclusive) from the standard continuous uniform distribution $U(0, 1)$ in each of 10,000 iterations. For the 15 nonideological policies that were not included in the Substantive by Policy or the Substantive by Issue measures, I again assign a random weight between 0 and 1, but I also assign a random direction $u \in \{-1, 1\}$.

In expectation, of course, the simulated measures are equivalent to the Substantive by Policy measure. What this simulation analysis does, however, is provide a *distribution* of measures under many possible weighting schemes. The many randomly generated measures will inevitably mimic all kinds of measures—those that assign larger weights to certain policy issue areas, such as tax policy, or those that prioritize more partisan or nonpartisan policies. In some simulated measures, one or more policy items will receive a weight close to 0, essentially excluding them from the measure.

I estimate policy polarization with a dynamic panel regression for each of the 10,000 simulated measures. The distribution of estimates allows one to assess how "rare" the LDM estimates are compared to those from traditional additive indices. Figure 3.7 shows the distribution of these regression estimates of the marginal effect of party control for the pre- and post-2000 periods.

Plot (a) in Figure 3.7 shows the distribution of marginal effect estimates, and compares them to the estimates using the Expanded SPL scale (the vertical lines). Compared to the distribution of pre-2000 estimates with the simulated measures, the Expanded SPL estimates are relatively large in magnitude: in the 99th percentile for Democrats and the

Figure 3.7: Party Effects Across Many Indices



Note: **Plots show estimates of the change in marginal effect of party control between the pre-2000 period and the post-2000 period.** The left panel uses only data from Caughey and Warshaw (2016) and the right panel uses the expanded policy dataset to construct 10,000 policy scales generated with random weights for each policy. Dashed vertical lines show estimates using the Expanded SPL scale for comparison. Estimates are derived from models that include state and year fixed effects, state × era fixed effects, and lagged dependent variables for years $t-1$ and $t-2$.

96th for Republicans. For the 2000-2014 period, the Expanded SPL estimate for Democratic state governments is in the more modest 78th percentile of simulated estimates. In contrast, the 2000-2014 estimate for Republicans is on the *low end* of the distribution of simulated estimates, in the 14th percentile.

The difference in estimates is partly due to the differences in the policy item parameters for between the Expanded SPL model and the additive indices. In the Expanded SPL model, substantively conservative policies that arise after 2000 receive discrimination parameters of smaller magnitude than do liberal policies during this period. For instance, state laws mandating paid family leave have a large discrimination parameter (5.91), but the parameter for voter ID laws is more than an order of magnitude smaller (-0.46).[15] In contrast, the simulated substantive measures assign these policies parameters of opposite directions with symmetric magnitudes in expectation.

Plot (b) shows estimates of the *difference* in the marginal effect of party control before and after 2000. Again, the Expanded SPL scale yields estimates of the difference in the marginal effect of Democratic control that are on par with those from the simulated additive indices (38th percentile). But difference in the Republican effect is still much smaller in the Expanded SPL scale than for the simulated additive indices (2nd percentile). The Expanded

[15]These policy items do not appear in the original SPL data. These parameter estimates are from the Expanded SPL model.

SPL scale shows an increase in the Republican effect that is smaller than 98% of the estimates derived from simulated Substantive Scales.

In the Appendix, I execute the same simulation analysis using *only the policy data included in the original SPL model*. The SPL estimates for the pre-2000 period are again at the top end of the distribution of simulated additive measures. The SPL estimates for the post-2000 period, as well as the difference between the pre- and post-2000 party effects, again tend to inflate Democratic estimates more than Republican estimates when compared to the simulated indices.

3.6 Substance Matters

Ideal point estimates command great scholarly attention, and political scientists have used innovative techniques to summarize political attitudes and behaviors quantitatively. In particular, the allure of LDM ideal point estimation strategies is great in a discipline that hopes to "drop the proper nouns" in order to produce generalizable theories of politics (Kellstedt and Whitten 2013, 31). In the case of state policy outcomes, however, the drawbacks of LDM methods like Bayesian IRT and the benefits of more traditional measure construction are especially acute. There are strong normative and theoretical reasons for skepticism of estimates from LDM models—substantively questionable parameters that are derived from the clustering and uniqueness of political actions across units and time that are, with little interrogation, believed to produce estimates of "ideology" or "liberalism." Applied researchers download and use datasets of LDM ideal points, often assuming that the measures correspond to more substantive conceptualizations liberalism based on normative, historical, and socioeconomic considerations.

Even more critical are the substantive conclusions that researchers draw from the ideal point estimates produced by these scales. The SPL scale suggests that, counter to the findings of qualitative and policy scholars, Republican control of state government in the hyper-polarized 2000s makes a state little or no more conservative than Republican control did during the less polarized 1970s and 80s. It also suggests that Wisconsin and Minnesota became more similar in policy outcomes in the post-Scott Walker era than they were in the mid-1990s through mid-2000s. Major obstacles to substantive inference impede LDM-based analyses of policy responsiveness to public opinion.

Although it is not central to this article, additive indices allow researchers to seamlessly aggregate and disaggregate items into separate issue area dimensions such as abortion, environmental, tax, welfare, and criminal justice policy. The item parameters, which the expert coder sets, can be carried over from a unidimensional measure into issue-specific indices. In contrast, scholars using LDM techniques must run a new LDM model for each dimension, yielding entirely new item parameters that are incomparable to those from the unidimensional measure.

Nearly a decade ago, Pierson (2007, 146) argued that "[s]trong research programs gain from sustained communication within and across communities of scholars pursuing a range

of strategies, including both quantitative and qualitative ones" (see also Brady and Collier 2010). To heed this advice, further research, especially that which examines policy outcomes or political actions that number in the medium-N or less, should make explicit researchers' ex-ante parameters for political actions. This substantive orientation toward the study of politics could bridge the gap between research that assumes politics exists on a latent ideological dimension from which deviations are considered "errors" (e.g., Poole and Rosenthal 1997; Bonica 2013; Tausanovitch and Warshaw 2014; Caughey and Warshaw 2016) and research that focuses on the substance of particular policy areas (e.g., Hill and Leighley 1992; Gerber 1996; Hero and Tolbert 1996; Soss et al. 2001; Berry, Fording and Hanson 2003; Fellowes and Rowe 2004; Hacker 2004; Yates and Fording 2005; Hero and Preuhs 2007; Karol 2009; Grossmann 2014; Broockman 2016; Beland, Rocco and Waddan 2016; Anzia and Moe 2017).

Chapter 4

The Public or the Party

4.1

In 2015, Governor Scott Walker signed a bill into law that bans abortions after 20 weeks of pregnancy in the state of Wisconsin. Wisconsin for decades had been a moderate state on abortion policy: more restrictive than socially liberal California but more permissive than evangelical Kansas. However, with the passage of the 2015 law, Wisconsin ties a dozen states for the strictest gestation limit. Because state policies are traditionally understood to be highly responsive to public opinion (Erikson, Wright and McIver 1993, 2006; Maestas 2000; Lax and Phillips 2009; Caughey and Warshaw 2017), one might assume that the Wisconsin law reflected the preferences of the mass public. It did not. Wisconsin remains near the median in terms of public opinion on abortion, and in the years leading up to 2015, support for abortion rights had actually *increased* slightly in the state.

Cases like abortion policy in Wisconsin—the passage of a substantively significant policy bearing little association to public attitudes—may be increasingly common. Nearly a half-century after the long buildup of federal policymaking capacity, a surprising twist occurred in American federalism: Recent state governments have passed a number of significant policies while the federal government has been largely gridlocked. A large number of state policies passed since 2000 are the most socially and economically consequential state policies in a generation. They include Kansas' abortion restrictions, California's environmental regulations and increased tax rates on high incomes, Colorado and Washington's legalization of recreational marijuana, and the restriction of union bargaining rights in the Midwest. Even the most significant federal social policy in a generation, the Affordable Care Act (ACA), gives states the authority to refuse large parts of it after the Supreme Court's ruling in *National Federation of Independent Businesses v. Sebelius*. These policy changes at the state level have increased variation in policy outcomes across states. But what determines which states pass which significant policies? Are the states responding to the will of their constituents while the federal government stalls?

The tools developed by studies of policy responsiveness can shed light on the origins

of significant state policies. Analyses of responsiveness have tended to rely on correlations between opinion and policy since Miller and Stokes (1963). Although correlations across states (cross-sectionally) and across time between state public opinion and policy outcomes leave open the possibility that elites lead and public opinion follows (Zaller 1992; Lenz 2013), American scholars have historically tended to embrace the *democratic responsiveness* theory that the public's policy opinions are—a few distortions notwithstanding—translated into policy. Correspondingly, recent shifts in significant state policies could be explained by changing constituency demands due to partisan, ideological, or residential sorting (Bishop 2009; Levendusky 2009).

However, in an era of polarized parties in government, divergence in policy outcomes has occurred in states with generally similar electorates. In 2012 President Obama won a virtually identical proportion of the electorates of Minnesota, Wisconsin, and Michigan—but red Wisconsin and Michigan have cut taxes dramatically and restricted union activity while blue Minnesota implemented its largest tax increase in history. To test policy responsiveness to party control and public opinion, I use multilevel regression with poststratification (MRP) to estimate state level opinion over time in twelve issue areas: abortion, civil rights and liberties, criminal justice, education spending, environment, gun control, health and welfare, immigration, labor, LGBT rights, marijuana, and taxes.

In most issue areas since the 1980s, I find that policy change is uncorrelated with dynamics in public opinion. Interstate variation in opinion has not expanded nearly as dramatically as policy outcomes have; state level policy opinions in the 2000s tended to follow national trends with little increase in interestate variance of opinions. The large party effects I find suggest a need for deeper investigation into party agenda-setters at the state level—as the passage of significant state policies is explained more by party control than dynamics in public opinion.

However, in two issue areas, LGBT rights and marijuana, policy outcomes are tightly associated with public attitudes. I suggest that characteristics of these issue areas, such as social movement attention and relative incomplexity, help to explain why policy in these areas is comparatively responsive to mass attitudes.

4.2 Responsiveness Across States and Time

Theories of democratic responsiveness require two correlations as necessary conditions. First, there is *cross-sectional* responsiveness (or congruence), which asks whether political units with more conservative opinions are more likely to have conservative policy outcomes (Erikson, Wright and McIver 1993; Gray et al. 2004).[1] Cross-sectional responsiveness requires variation in opinion across states. If only a certain subset of states have a given policy, one would expect that aggregate opinion in those states with policy should be more supportive than opinion in states without the policy.

[1] A related question is whether binary policy outcomes are congruent with opinion majorities (e.g., Lax and Phillips 2012).

Second, *over time* responsiveness asks whether temporal *dynamics* in opinion are associated with policy *changes* (Lowery, Gray and Hager 1989; Stimson, MacKuen and Erikson 1995; Caughey and Warshaw 2017). For policy changes to be responsive to public opinion across time, there must be movement in opinion; one cannot explain change with a constant. Dynamic panel models, such as those that employ lagged dependent variables or state fixed effects, are often used to estimate the association between two dynamic variables over time.

As seen earlier, party control of government has large effects on policy outcomes in the states. But this pattern may itself be driven by public opinion. I investigate whether party control explains policy outcomes above and beyond that which is predicted by public opinion. Policy attitudes in states may leave little variation in policy outcomes unexplained, because politicians who are "out of step" on policy issues are voted out of office (Canes-Wrone, Brady and Cogan 2002; Downs 1957; Erikson, Wright and McIver 1993) and incumbent politicians self-sanction in order to avoid anticipated electoral punishment (Stimson, MacKuen and Erikson 1995). If party control has a substantial influence over policy outcomes *net of public opinion*, it will be important to investigate other potential causes for why the parties in government propose and pass distinct policy agendas in the states.

The Strong Correlation

Theories of democratic responsiveness hypothesize a *strong correlation* between constituency opinion and policy change, both cross-sectionally and over time. Classic theories of democratic responsiveness predict that political candidates will be responsive to the policy attitudes of the general electorate's median voter in order to maximize their chances of reelection. A number of influential studies find considerable dyadic responsiveness between legislative behavior and constituent opinion at the federal level (Miller and Stokes 1963; Bartels 1991; Canes-Wrone, Brady and Cogan 2002), as well as collective responsiveness at the systemwide level (Page and Shapiro 1983; Stimson 1991; Erikson, MacKuen and Stimson 2002).[2] Some elite-driven theories of mass opinion—quite distinct from theories of democratic responsiveness—could also be congruent with a high correlation between opinion and policy, but this time because the masses "follow the leader" (Zaller 1992; Lenz 2013).

A number of major analyses of politics at the state level appear to be even more supportive of the democratic responsiveness theory, both at the single issue level (Clingermayer and Wood 1995; Lax and Phillips 2009), and on one or two left-right dimensions (Erikson, Wright and McIver 1993; Caughey and Warshaw 2017). As Erikson, Wright, and McIver (1993, 81) argue, "even small differences in state ideological preferences appear to have major policy consequences"—a relationship between opinion and policy so strong that it is nothing short of "awesome" (Erikson, Wright and McIver 1993, 80).

The effect of party control on policy is expected to be largely subsumed by the effect of public opinion, because voters choose partisan politicians for their policy positions. Erikson,

[2]Comparative analyses of democracies find similar policy congruence and responsiveness (e.g., Brooks and Manza 2008; Soroka and Wlezien 2010).

Wright, and McIver (1993, Ch. 5) address the tug of war between the "extreme" pull of party elites and the moderating influence of voter ideology, but largely conclude that Downsian convergence occurs as both Democratic and Republican elites are more conservative in conservative states and more liberal in liberal states. For the most part, elite behavior is explained by public opinion. I depart from recent literature by comparing the influence on policy of both opinion and party. By holding opinion constant in models, I estimate the association between party and policy *net of state policy opinion* and also the association between opinion and policy *net of party control*.

Significant state policy changes have increased variation in policy outcomes across states in recent years. In this situation, cross-sectional responsiveness requires that opinion in states that implement these significant policies is relatively more supportive than opinion in states that do not. Similarly, over time responsiveness necessitates opinion divergence in the corresponding policy area to accompany the divergence in policy outcomes. A correlation between opinion and policy outcomes over time is possible if at least one of these two changes occur: 1) state policy opinion becomes more liberal (or conservative) in states where the liberal (or conservative) policy is passed; 2) state policy opinion becomes more conservative (or liberal) in states where the liberal (or conservative) policy does *not* pass. A plausible reason for diverging state attitudes is geographic sorting. Bishop (2009) argues that Americans have increasingly opted to live in communities that tend to share their political views (see also Sussell and Thomson 2015).

The Weak Correlation

In contrast, some theories predict a *weak correlation* between mass opinion and policy change. With an electorate sorted along party lines and polarized primary constituencies, politicians may rationally be most responsive to subgroups such as their primary constituency or co-partisans at the expense of the general electorate's median voter (Fenno 1978; Jacobson 1992; Clinton 2006).

A weak correlation may also suggest a more fundamental breakdown of the opinion-policy relationship. Legislators' own backgrounds, such as their social class, influence their behavior in biased ways (Carnes 2013). State legislators systematically overestimate the conservatism of their districts on policy questions (Broockman and Skovron 2018). At the same time, constituents may select politicians not on the basis of policy positions, but on the basis of their party identification derived via socialization into a party "team" using social group identities that may only loosely correspond to policy preferences (Green, Palmquist and Schickler 2002). Policy agendas can be determined at the elite level by "intense policy demanders" like partisan activists and interest groups that are influential in the nomination process (Bawn et al. 2012). The "electoral blind spot" theorized by Bawn et al. (2012) provides politicians considerable wiggle room to implement policies that are at odds with their constituents' policy attitudes, particularly given the lack of traceability of policy (Arnold 1992), voter

myopia (Bartels 2009), and the prevalence of "nonattitudes" in the public (Converse 1964).[3]

Challenges to Studying Responsiveness

Responsiveness is key to democratic theory, but successful estimation and inference in studies of responsiveness is no simple task. There are particular challenges posed by the investigation of each type of responsiveness, cross-sectional and over time. A major drawback of cross-sectional responsiveness is the reliance on the relative, not absolute, position of opinions and policy outcomes across states. Moreover, estimating the absolute responsiveness of policy is impossible when opinion and policy are measured on different scales, as they are throughout this literature. There may be a systemwide responsiveness "deficit," as Bartels (2015) finds in regard to social welfare spending in developed countries. Across all 50 U.S. states, it could be the case that policy in some issue areas is intercept-shifted such that it is "off center" (Hacker and Pierson 2005), but it would be difficult to know.

Unlike cross-sectional responsiveness, the estimation of over time responsiveness allows for all political units (i.e., states) to be responsive to a global (i.e., national) trend in opinion even when the relative opinion of units do not change position. However, again related to the absolute responsiveness of policy, a challenge for over time responsiveness is its use of a baseline from which to measure change. A state's pre-existing policy—the baseline—may be unresponsive such that a policy change improves responsiveness even when opinion is static.

Other challenges arise from choices in measurement. Although a large literature contends that there is a strong link between the liberalism of a state's population and the state's policy outcomes, measuring the masses' opinions on a single liberal-conservative dimension may create obstacles to aggregation and inference (Broockman 2016). Bartels (2015, 3), for instance, goes so far as to describe the single-dimensional analysis the relationship between "public mood" and policy of Stimson, MacKuen and Erikson (1995) as "provid[ing] no way to assess the degree of congruence between what citizens wanted and what they got."[4] (Analogously, the previous section described how a recent composite measure of state policy outcomes understates changes.) A stronger test of democratic responsiveness provided in this paper is whether the public's policy preferences are translated into policy change in the corresponding issue area. The drawback here is that policy issue preferences may be measured more noisily than is a single left-right dimension of ideology.

Finally, some challenges are largely insurmountable. Polling introduces measurement error, though aggregate opinion may cancel out random error and have more stability and

[3]Proponents of the view that voters are "cognative misers" who utilize heuristics to vote as if they had coherent policy preferences may argue that democratic responsiveness is healthy in the face of weak correlations between policy opinions and policy outcmoes. However, there is evidence that cognative cues can introduce bias and actually decrease the probability of a "correct" vote for some (Lau and Redlawsk 2001). In addition, "low information rationality" is generally less normatively desirable than responsiveness to policy opinion.

[4]Despite the difficulty in substantively interpreting responsiveness on a single dimension, I test the assocation between the "policy mood" measure of state opinion by Enns and Koch (2013) and significant policy outcomes in the Appendix.

"rationality" (Page and Shapiro 2010). Correlations are unsatisfying for questions of causal inference, and there are times where exogenous variation cannot plausibly be exploited. However, as I have described, some theories include correlations as necessary conditions, and estimates of the correlation between opinion and policy can shed light on the largely unstudied origins and representational consequences of recent significant policy changes in the states.

Recent literature overcomes some of the challenges to estimating responsiveness, and this study attempts to build upon previous advancements. Lax and Phillips (2012) offer a particularly thorough multi-dimensional analysis of policy congruence to constituent majorities at the state level, but congruence only at one snapshot in time. Lewis, Wood and Jacobsmeier (2014) study the relationship between judicial behavior and gay rights opinion over time. However, this study is the first to investigate the relationship between temporal trends in opinion and state policy outcomes in many different issue areas (see also Lewis and Jacobsmeier 2012).

4.3 Data and Models

Data

I use repeated policy-related questions from the American National Election Study (ANES), Gallup, and the General Social Survey (GSS) to estimate temporal dynamics in the policy opinions of residents of the 50 states. The sample of policy areas is determined by the intersection of the author's estimation of the policy's substantive social and economic significance and the availability of repeated measures of public support. I list the policy questions for which I estimate state level support, along with the survey-years from which the questions are taken, in Table 4.1.[5]

As I did with policy outcomes in Chapter 2, I measure public opinion by issue area. (Table 4.1 shows the issue area that corresponds to each survey question.) Most studies of responsiveness have measured opinion and policy at the level of the single policy (Clingermayer and Wood 1995; Lax and Phillips 2009), and or on a single left-right ideological dimension (Erikson, Wright and McIver 1993). Scholars have argued that dimension aggregation reduces measurement error in opinion estimates (Ansolabehere, Rodden and Snyder 2008). However, in light of new evidence that dimension reduction may conflate consistency with extremism and lead to spurious relationships between opinion and outcome variables (Broockman 2016), new research tends to disaggregate attitudes into a "social" and an "economic" dimension measures of opinion and policy (Caughey and Warshaw 2017; Caughey, Dunham and Warshaw 2018). I argue that these dimensions are too broad to capture important variation in public attitudes on policy: within the large buckets of social and economic

[5]For the feeling thermometer quesitons, the top 51 levels are coded as 1.

Table 4.1: Survey Data

Question	**Source**	**Policy Area**
Abortion legal	ANES	Abortion
Abortion legal	Gallup	Abortion
Abortion rape exception	GSS	Abortion
Abortion legal	GSS	Abortion
Equal employment for black people	ANES	Civil Rights & Liberties
Affirmative action	ANES	Civil Rights & Liberties
Aid to black people	ANES	Civil Rights & Liberties
Women equal role	ANES	Civil Rights & Liberties
Legal rights of accused	ANES	Criminal Justice
Support death penalty	Gallup	Criminal Justice
Support death penalty	GSS	Criminal Justice
Courts too harsh	GSS	Criminal Justice
Spending on public schools	ANES	Education
Environment thermometer	ANES	Environment
Spending to protect environment	ANES	Environment
Support gun ownership	GSS	Guns
Licenses for gun ownership	GSS	Guns
Assault weapon ban	Gallup	Guns
Government health assistance	Gallup	Health & Welfare
Government health insurance	ANES	Health & Welfare
Government spending on services	ANES	Health & Welfare
Spending on the poor	ANES	Health & Welfare
Spending on welfare	ANES	Health & Welfare
Spending on elderly	ANES	Health & Welfare
Childcare assistance	ANES	Health & Welfare
Spending on the poor	GSS	Health & Welfare
Government health assistance	GSS	Health & Welfare
Increase immigration	ANES	Immigration
Undocumented immigrant thermometer	ANES	Immigration
Labor union thermometer	ANES	Labor
Big business thermometer	ANES	Labor
Government support for employment	ANES	Labor
LGBT adoption	ANES	LGBT
LGBT in military	ANES	LGBT
LGBT anti-discrimination	ANES	LGBT
LGBT thermometer	ANES	LGBT
Legalize marijuana	Gallup	Marijuana
Legalize marijuana	GSS	Marijuana
Tax high incomes	GSS	Taxes

issues, many individuals are likely to hold unconstrained policy attitudes that reflect real attitudes.[6]

Measurement Models

In order to increase the precision of state subsamples, I employ varieties of the popular multilevel regression with poststratification (MRP) method. MRP involves the estimation of a multilevel model with individual level effects nested within states and regions, and then the use of Census weights for poststratification. The method has been extensively validated in recent years in samples of approximately 1,500 respondents nationwide.[7]

The multilevel models first estimate the effect of individual demographic factors on opinion using random intercepts.

$$Pr(y_n) = logit^{-1}(\beta^0 + \alpha^{income}_{c[i]} + \alpha^{race}_{d[i]} + \alpha^{gender}_{g[i]} + \alpha^{age}_{h[i]} + \alpha^{education}_{j[i]} + \alpha^{race*income}_{k[i]} + \alpha^{state}_{m[i]}) \tag{4.1}$$

The individual model is nested within a model with fixed effects for state level characteristics. I use state Democratic vote share in the last presidential election and state income as predictors along the lines of Gelman (2009). I also include a state's percent of evangelical residents (see Lax and Phillips 2009).

$$\alpha^{state}_s = N(\alpha^{region}_{r[s]} + \beta DemVoteShare + \beta StateIncome, \sigma^2_s), \text{for } s = 1, ..., 50 \tag{4.2}$$

States are nested within regions.

$$\alpha^{region}_r = N(0, \sigma^2_r), \text{for } r = 1, ..., 4 \tag{4.3}$$

Finally, estimates are population reweighted at the state level using Census Current Population Study (CPS) data downloaded from the Census' *Data Ferret* program. MRP uses partial pooling of the data based on demographics and region. Because Census weights vary as geographic concentrations of demographic groups change over time, the model is able to pick up variation in state level opinion even in cases where respondents in state survey subsamples answer questions identically across time.[8]

[6]For instance, among social issues, an individual may hold liberal views on LGBT rights and conservative views on abortion. Similarly, among economic issues, an individual may oppose labor unions but support raising taxes to support the poor. While such unconstrained survey responses may at times reflect "errors," they often signify policy attitudes with consequences for vote choice (Ahler and Broockman 2017).

[7]The cluster sampling design of the ANES (meant for representative national, not state samples) makes estimates using ANES questions less reliable than the CCES, but state estimates are a vast improvement over raw ANES subsamples (Stollwerk 2012).

[8]I execute a validity check against the raw CCES state subsamples using support for the State Children's Health Program (SCHIP) as a policy example in Appendix Figure C.1.

For each state-year, I average the MRP estimates in each issue area. These issue-area averages are the primary opinion measures used in subsequent analyses.

I also use the same survey data to fit two additional kinds of models that smooth estimates across time. The first alternative measure is a dynamic MRP model, in which a state's MRP estimate for a survey question in year t is the result of a "random walk" using a Bayesian prior of the state's estimate in year $t-1$. As before, I construct issue area measures for each state-year by averaging the estimates for each survey question in the issue area. The second alternative measure is the result of a dynamic group level IRT model. Rather than weighting each policy question equally in each issue area, this method derives difficulty and discrimination parameters (which determine the ideological content of the policy question) for each question within an issue area. These alternative models are implemented using the *dgo* package in *R* (Dunham, Caughey and Warshaw 2017).

I merge my opinion estimates with the measures of issue-specific policy outcomes used in Chapter 2. For use in regression models, I recode policy outcomes and/or opinion such that policy liberalism is matched by poll question liberalism, where liberal policies are coded as 1 and conservative policies as 0 (see Caughey and Warshaw 2017).[9] A positive coefficient for opinion thus *always signifies greater responsiveness within policy areas*. In contrast, a positive coefficient for party control means that the party is more likely to make policy more liberal (i.e., the same interpretation as the models of policy in Chapter 2), such that the coefficient for Democratic control will probably be positive, and that for Republican control probably negative.

4.4 Results

In this section, I present four types of analyses of the opinion-policy relationship. First, I describe *national* responsiveness to public opinion by plotting the average state opinion and average state policy in each issue area across time. This helps gauge systemwide dynamics in opinion and state policy over the past generation. Second, I compare *cross-sectional* responsiveness—whether states with more liberal opinion have relatively more liberal policies—in 1988 and 2012. This analysis addresses whether the relative positions of state policy corresponds to their relative positions in opinion, and whether this correspondence has grown stronger or weaker over time. Third, I estimate *dynamic responsiveness* to opinion, which asks whether state policy responds to opinion change over time. Finally, to address the possibility that party control of government mediates the opinion-policy relationship, I execute *mediation analysis*, a generous test that adds together the direct effect of opinion with its indirect effect via party control.

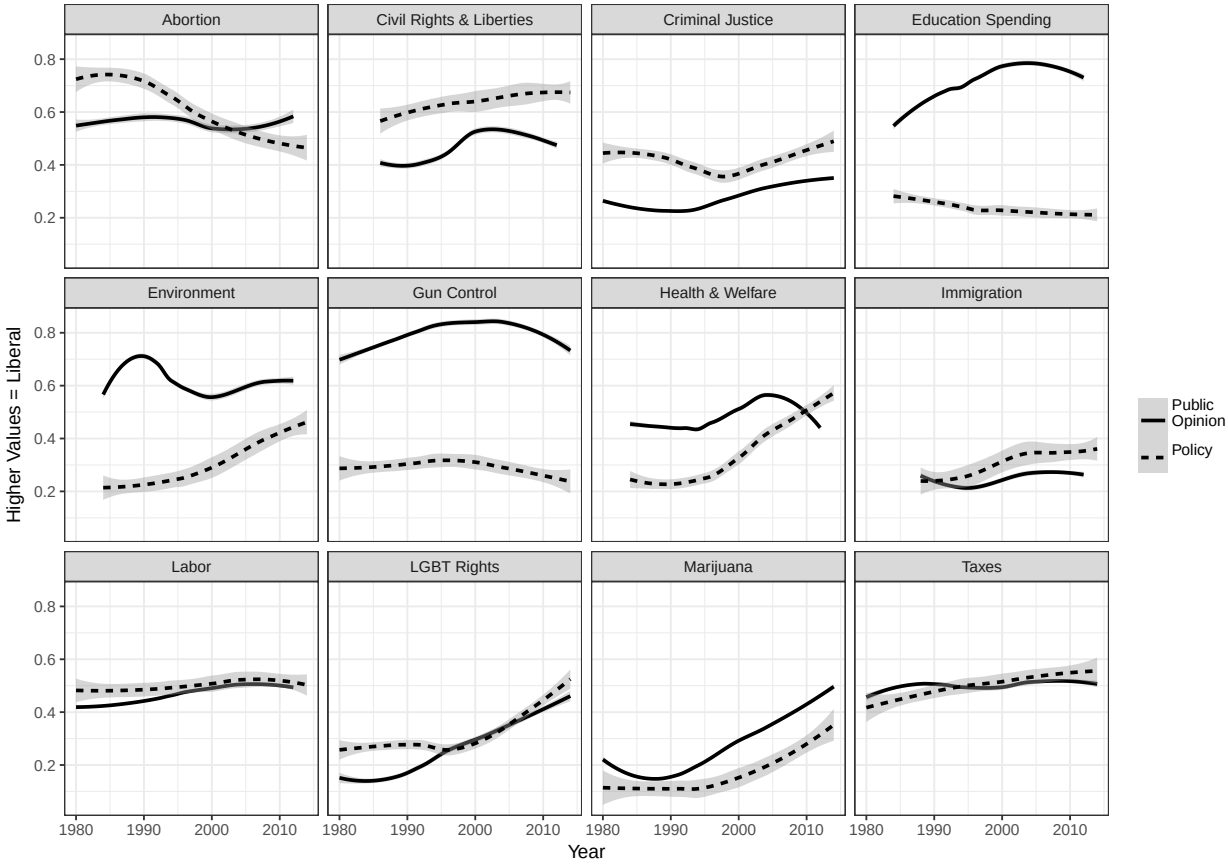
[9]For example, because the labor unions question asks for respondent support for labor unions and Right to Work laws are oppositional to organized labor, I recode the MRP estimate as opposition to labor (i.e., 1−support for labor).

Describing Opinion and Policy Over Time

I plot average state opinion and average state policy outcomes over time in Figure 4.1. We are interested in the temporal correlation between opinion and policy—whether opinion change is associated with policy change across time. Average state opinion is shown in the solid line, and average state policy is shown in the dashed line. In many issue areas, while there may be temporary fluctuations, opinion is largely static since the 1980s, such as abortion, environment, health and welfare, labor, and taxes.

Policy change cannot be well explained by static opinion, and yet, as we saw in Chapter 2, some of these issue areas experienced sea changes in policy but little change in mass attitudes. Abortion policy is a clear example. Since *Roe v. Wade* (1973), many states innovated and implemented increasingly restrictive abortion policies—but Figure 4.1 suggests that, on average, opinion has remained stable during this time period.

Figure 4.1: Opinion and Policy Across Time



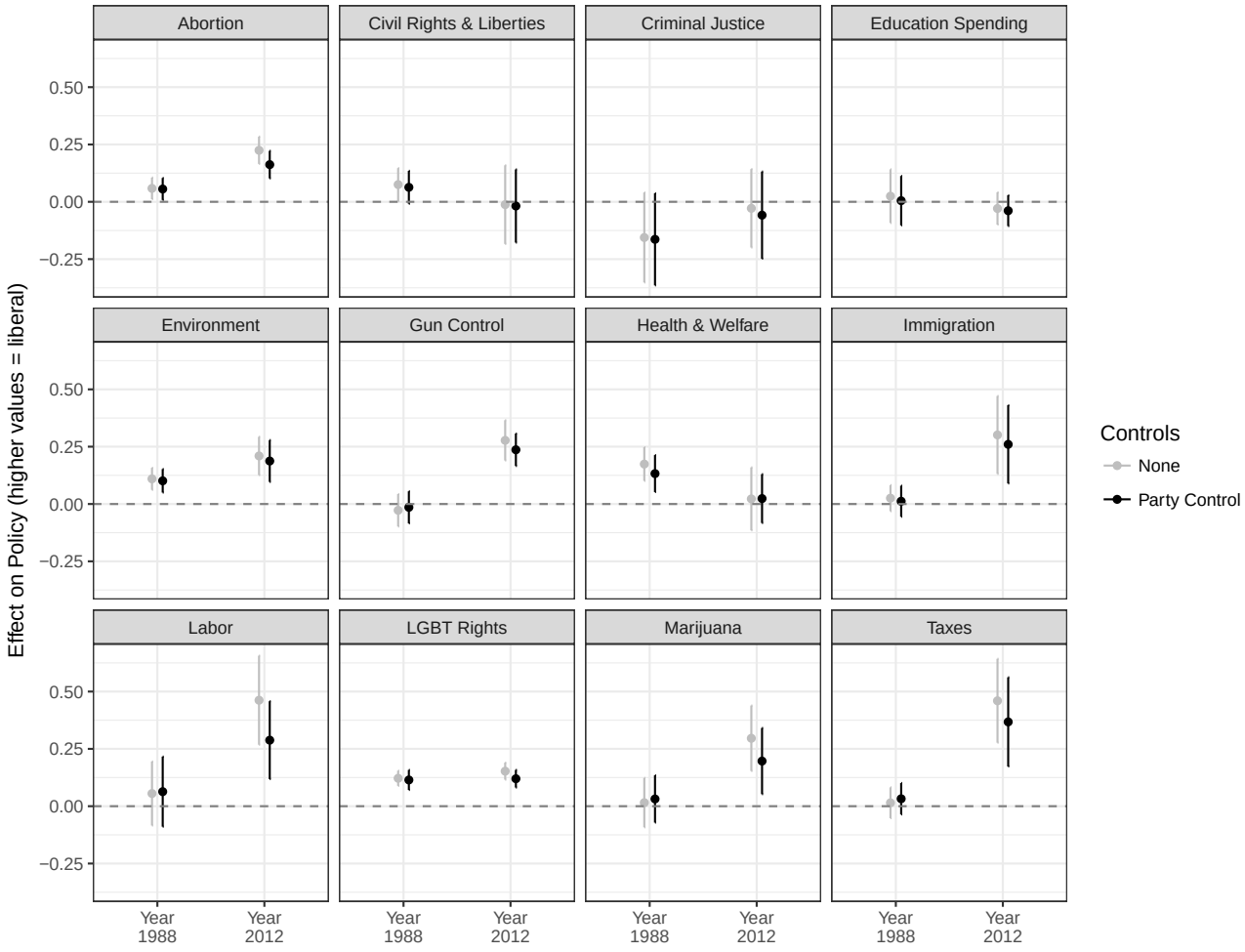
Conversely, some major changes in average state opinion are associated not with correspondent policy change, but with policy stasis. Average state opinion becomes much more

generous on education spending since the 1980s. It is commonly known that increased education spending is a perennially popular item in the mass public, but it becomes even more popular in recent decades. However, as more Americans desire increased education spending, education spending *decreased* in the average state.

Cross-Sectional Responsiveness

The next step is to estimate the *cross-sectional* relationship between opinion and policy. Figure 4.2 plots the results of cross-sectional regressions of the opinion-policy relationship by issue area. Positive coefficients suggest that public support for a policy (relative to other states) makes a state significantly more likely to implement the policy.

Figure 4.2: Cross-Sectional Responsiveness to Public Opinion



Cross-sectional responsiveness is modest in 1988. Only in abortion, environment, health and welfare, and LGBT rights do we observe evidence that states with more liberal opinion are significantly more likely to have more liberal policies in the issue area. The rest of the issue areas show policy outcomes that are unassociated with state opinion.

The cross-sectional opinion-policy correlation tends to be stronger in 2012. In abortion, environment, gun control, immigration, labor, marijuana, and taxes, the coefficients are significantly greater than zero—and significantly greater in 2012 than in 1988. As argued earlier, cross-sectional responsiveness is a necessary but insufficient condition for democratic responsiveness overall.

Some issue areas see no change in cross-sectional responsiveness during this time period. The correlation between opinion and policy in LGBT rights is evident in both time periods. In contrast, cross-sectional responsiveness remains minimal in both 1988 and 2012 for criminal justice and education spending.

However, in health and welfare policy, and to a lesser extent civil rights, cross-sectional responsiveness *weakens* between 1988 and 2012.

Taken as a whole, these findings on cross-sectional responsiveness are a potentially hopeful sign. Even if state opinion is static across time, such an increase in cross-sectional responsiveness between 1988 and 2012 may mean that policy has *come into alignment* with public opinion. This might occur, for example, if a state's median opinion on abortion was more conservative than its policy status quo in 1988, and abortion policy moved rightward to meet this opinion. Furthermore, as we observed in Chapter 4, the range of policy outcomes in the states has expanded greatly in recent decades because *state governments are doing more important and variable policymaking.* Statistically, increasing the variation of a variable y improves the correlation between x and y—so this greater cross-sectional responsiveness is in part thanks to the greater variation in state policy outcomes across time.

But there are important limits to assessing responsiveness cross-sectionally. As described earlier, cross-sectional responsiveness is *relative.* Because opinion and policy are not anchored to a specific policy status quo, we cannot confidently conclude that an improvement in cross-sectional responsiveness is evidence of incongruent opinion becoming congruent in an absolute sense (i.e., the relative positions of states can improve while mass opinion remains far outside of the range of state policy outcomes). A potentially more effective test of the opinion-policy relationship is to estimate *dynamic responsivess*, the relationship between opinion change and policy change within each state. More generally, an important tradition of literature argues that voters adopt policy positions from elites (e.g., Lenz 2013; Achen and Bartels 2016; Broockman and Butler 2017). Such studies have criticized cross-sectional studies of responsiveness (e.g., Erikson, Wright and McIver 1993) for being susceptible to reverse causality.

4.5 Dynamic Responsiveness

In this section, I estimate dynamic responsiveness in the states. The models use state and year fixed effects to estimate the within-state relationship between opinion and policy. In contrast to estimates of cross-sectional responsiveness, we see here that dynamics in opinion within states is not significant predictor of policy change. Within a given state, increases in policy support are negligibly associated with an increased likelihood of passing the policy.

Figure 4.3: Dynamic Responsiveness to Public Opinion

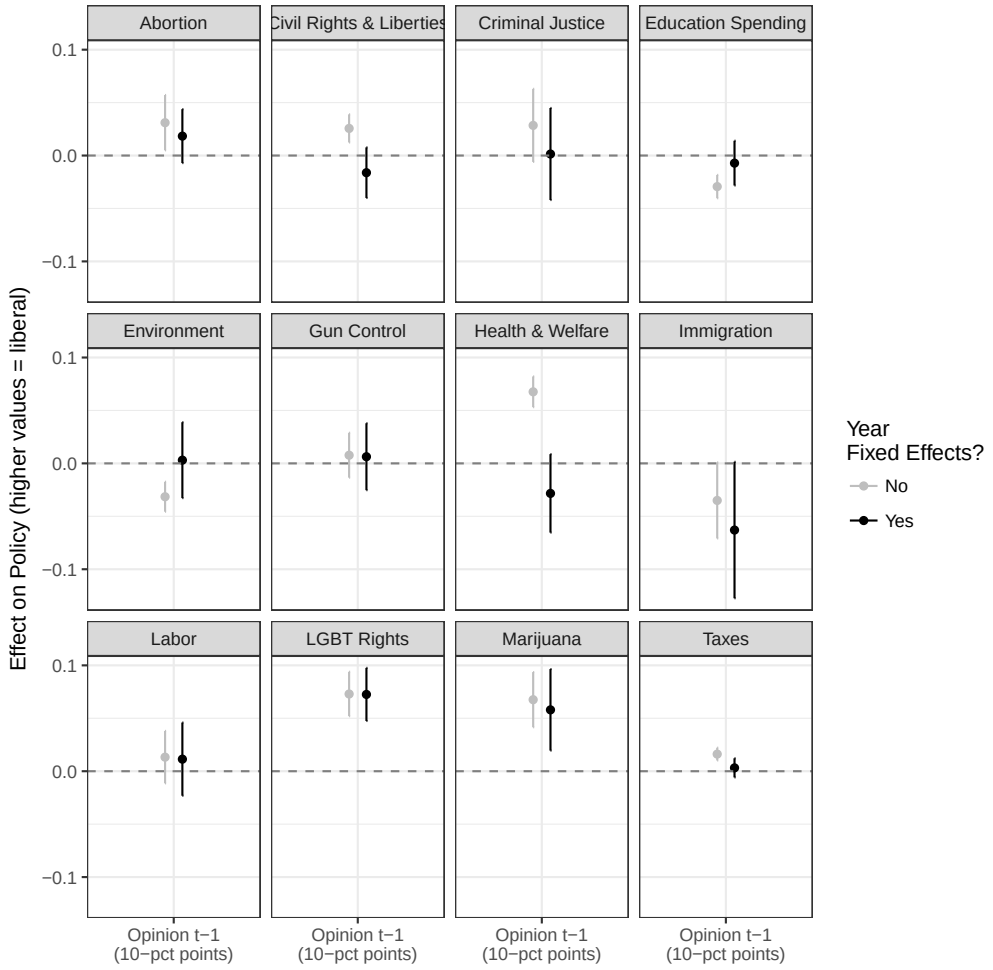


Figure 4.3 displays the results. 9 of 12 issue areas show no responsiveness—or in some cases a negative relationship between opinion and policy.

Abortion, but most of all the areas of LGBT rights and marijuana policy, show strong dynamic responsiveness. A one standard deviation increase in a state's public support for LGBT rights is associated with a 0.075-unit shift in LGBT rights policy. This is not a small

amount. Recall that policy outcomes are measured on a 0-1 scale so this shift is equal to 7.5% of the range between the most and least liberal LGBT rights policy observed in a state since 1970. Marijuana policy shows a similarly strong responsiveness coefficient of about 0.06.

I also plot the results of analogous models without year fixed effects in grey. These models do not control for shifts in the mean of all states' policy, allowing the opinion coefficients to predict national-level variation in state policy. These models without year fixed effects may be preferable to the extent that we consider responsiveness to these global mean shifts in opinion to be normatively important in assessing democratic performance. In these models, the opinion-policy relationship appears considerably stronger for civil rights, health and welfare, and taxes. This is because, as seen in Figure 4.1 earlier, mean state opinion and mean state policy (across all states) trend more liberally through this time period. Education spending and environment, however, show significantly less responsiveness when year fixed effects are removed.

The Influence of Party Control

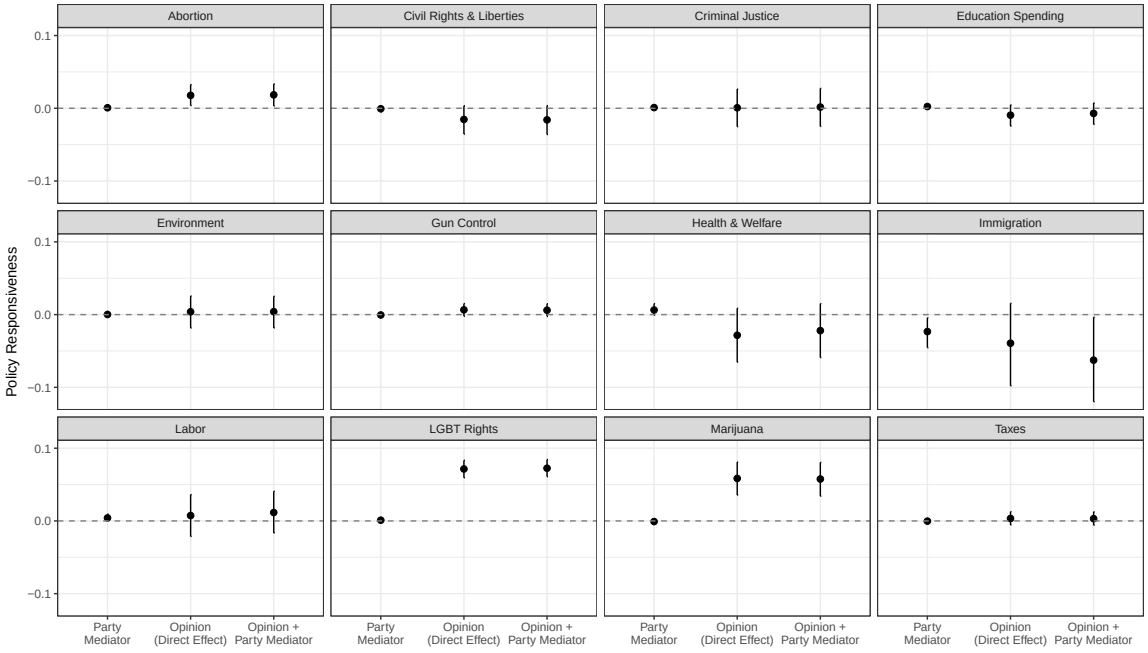
Although there is minimal evidence of dynamic responsiveness to public opinion in the states, Chapter 2 showed large effects of *party control of government* on policy. These large effects of party control persist even when controlling for dynamics in state opinion—suggesting that party control matters net of public opinion.

But because opinion is typically believed to be causally prior to party control, party could be a *mediator* for public opinion. State public opinion on policy questions could lead voters to elect politicians from the party that will implement their preferred agenda. I therefore estimate causal mediation models analogous to the cross-sectional, lagged dependent variable, and state fixed effects models in Table 4.4.

The mediation models are likely to find larger effects for opinion than the analyses presented earlier, because they combine the direct effect of opinion on policy to the mediated effect of opinion on policy through party control. The estimation process can be described in three steps. First, the models estimate the direct association between opinion and policy. Second, they estimate the effect of a one standard deviation increase in opinion on the mediator, *party control*, in order to estimate the effect of this change in the mediator on policy outcomes. Finally, the direct effect of opinion on policy and the mediated effect of opinion through party control are combined. Results are presented in Figure 4.4.

In each issue area shown in Figure 4.4, the left estimate represents the mediated effect of party control. This is determined by the correlation between opinion and party control. Because public support for conservative policies (and opposition to liberal policies) is usually positively correlated with unified Republican control and negatively correlated with Democratic control, these estimates should be positive. While they are indeed positive (the only exception is immigration), the estimates are minuscule because *dynamics in policy opinion are not strongly correlated with party control.*

Figure 4.4: Party Control as Mediator of Public Opinion



The middle estimates represent the "direct" effect of opinion on policy. Most importantly, the rightmost estimates are the total effect of opinion on policy: the sum of the direct effect of opinion and the effect of opinion as mediated by party control of government. This makes mediation analysis quite a generous test of dynamic responsiveness.

Yet even under the favorable conditions of mediation analysis, public opinion remains an inconsistent predictor of policy in the states. Again, only LGBT rights and marijuana policy show strong responsiveness to dynamics in state opinion.

4.6 Why LGBT Rights and Marijuana?

LGBT rights and marijuana policy are the two issue areas that show consistent evidence of opinion change leading to policy change—a key health metric of democratic responsiveness to mass attitudes. The strong responsiveness we observe in these areas stands in stark contrast to the weak or even negative relationship between opinion and policy in other areas. What explains this variation across different issue areas? Political scientists increasingly summarize many policies and issue areas on one or two left-right dimensions, but the results of this study suggest that responsiveness operates differently depending on the issue. In this section, I argue that there are three explanations for the distinct politics in LGBT rights and marijuana over the past generation.

First, public opinion has shifted greatly on LGBT and marijuana policy over the past

generation. Such a sea change in opinion implies greater issue salience, which may produce a clearer signal to politicians of public attitudes. Popular culture surrounding LGBT individuals and relationships, as well as the use of marijuana, have transformed since the 1970s. As late as the 1990s, television sitcoms portrayed marijuana use as a dangerous pathology; by the mid-2000s, the U.S. version of *The Office* featured an episode in which the other characters mock uptight Dwight for his strict anti-marijuana attitudes. To an even greater extent, the 1990s and 2000s saw the rise of positive portrayals of LGBT individuals in shows such as *Will and Grace* and *Ellen*.[10]

Second, LGBT rights and marijuana policy are social issues that feature greater partisan polarization in the mass public than do economic policies. Despite substantial *policy* polarization between red and blue states on issues like the minimum wage, many economic policies designed to support lower and middle income Americans remain popular and less polarized across red and blue Americans, as well as red and blue states. Hopkins (2017) suggests that this partisan geography strengthens the incentives for national parties to pursue these distinct social policy agendas.

Third, LGBT rights and marijuana policy are relatively simple to understand. Many scholars have suggested that policy complexity shapes politics (Makse and Volden 2011), especially by advantaging information-rich actors and disadvantaging more information-poor voters (e.g., Bartels 2009; Lenz 2013). Compared to taxes and environmental regulation, LGBT rights and marijuana policy are straightforward problems with relatively straightforward policy solutions (e.g., same-sex marriage and medical marijuana). Politicians often claim credit for economic outcomes that may have little to do with their or their party's policy decisions (Arnold 1992), but this is less possible in the areas of LGBT rights and marijuana.

4.7 Explaining Party and Policy

My estimation of the influence of public opinion and party control on significant state policy outcomes leaves much of the effect of party to be explained elsewhere. American politics has become hyper-polarized, and interparty divergence in policy outcomes reflects this fact. Yet polarization on its own does not imply a breakdown of policy responsiveness if opinion is diverging alongside policy. However, in this polarized era, I find that party control explains significant state policy outcomes *above and beyond public opinion*. Party control is tightly associated with policy outcomes even when holding public opinion constant. While an individual's social and economic policy regimes are increasingly determined by her state of residence, her policy preferences are not.

Further investigation of the roots of interparty divergence in significant policy outcomes should be a priority. This study suggests that little of the significant interparty divergence in recent years is explained by dynamics in public opinion. No longer political backwaters or

[10]Note that I am not making a normative comparison between marijuana policy and LGBT rights, or between LGBT individuals and people who use marijuana.

sites of the particularistic politics of unprofessional politicians, state governments appear to have been integrated into national politics—where the parties' policy agendas are polarized and well understood. Instead of shifts in mass opinion, these significant policy changes may be the result of *increasing national party coordination.* National party coordination could be the result of increasing interstate coordination and political investments from ideological activist groups, organized interests, donors, national party organizations, and state politicians. Hertel-Fernandez (2014), for instance, shows how the American Legislative Exchange Council (ALEC) developed and disseminated "model bills" that were ultimately implemented in many states.

In an era when the production of significant policy is increasing in the states relative to the federal level—and Americans' socioeconomic policy regimes are increasingly conditional on their state of residence—the question of "Who governs?" takes on a new life. Parties, voters, and interest groups may be influencing policy outcomes across venues, both horizontally across states and vertically across levels of government. Who is advantaged by the nationalization of American politics?

Chapter 5

Activist Donor Networks

5.1

Have changes in candidates' sources of fundraising contributed to legislative polarization? Recent studies suggest that limits on different sources of contributions—from interest groups, party committees, or individual donors—affects the behavior of legislators in office (La Raja and Schaffner 2015; Barber 2016*b*). A prevailing theory has emerged: the balance of fundraising from individuals relative to organizations has contributed to legislative polarization in the U.S. states. Individual contributions appear to polarize, while interest group contributions appear to moderate, state legislatures. Curiously, however, as polarization has grown precipitously, there has been little shift in the aggregate share of candidate fundraising from individuals compared to interest groups.

I argue that variation in the *types of individual donors* who contribute to candidates offers a more complete explanation for legislative polarization in the states. In particular, it matters to what extent that candidates' individual donors are affiliated with and coordinated by interest groups. While previous work focuses on whether individual donors are different from PAC donors (Barber, Canes-Wrone and Thrower 2017; Barber 2016*a*,*b*), individual donors are often affiliated with political organizations like political action committees (PACs). Indeed, interest groups such as Americans for Tax Reform, the National Rifle Association, and MoveOn.Org *are comprised of individuals*. I argue that conceptualizing individual donors who are affiliated with interest groups as *interest group activists* (hereafter IGA donors) may help to explain the nationalization and polarization of state politics in recent years.

I first provide evidence that IGA donors are distinct from otherwise similar copartisan donors. Survey data suggests that IGA donors report more extreme ideological and policy attitudes that otherwise similar individual donors. Moreover, benefits of organization, such as coordination and informational resources, may lower the cost of political influence for IGA donors. Consistent with this theory, I find that IGA donors are significantly more likely to report contacting legislators.

I then use campaign finance data to investigate the relationship between IGA donors

and legislative behavior in the states. Over time, the average donor to a state legislative candidate has become much more likely to be an IGA donor. I find that the proportion of legislators' contributors that are IGA donors has large and significant effects on their legislative behavior—as large as the effect of public opinion or contributions from formal party committees and interest groups. Since 2000, increases in the proportion of donors that come from ideological group extended networks are associated with legislative polarization. I use a novel dataset of state legislative primary dates to estimate the effect of IGA contributions during the primary and general election periods. Consistent with theories that emphasize the role of parties, groups, and activists in the nomination process (Bawn et al. 2012; Hassell 2016), I find that the effect of IGA contributions is mostly concentrated in primary elections.

While IGA donors may be polarizing agents, actors affiliated with party establishments are expected to be moderating influences (La Raja and Schaffner 2015; Hassell 2018). However, I find that *party insiders*—individual donors affiliated with state and national party committees such as the Democratic Congressional Campaign Committee (DCCC) or (RNC) (Hassell 2016)—have no consistent relationship with legislative behavior.

Although the potential for endogeneity merits caution, this investigation of individual affiliates of interest groups complicates the prevailing view of organizations as moderate and individuals as extreme, and provides a partial explanation for partisan polarization of legislative behavior and policy agendas in the states.

5.2 Individuals and Organizations in Campaign Finance

Political observers are often surprised to learn that dominant theories in political science maintain that money—at least in the form of campaign contributions—exerts little influence in politics.[1] Indeed, numerous studies have found minimal effects of campaign contributions from political action committees (PACs) (Wawro 2001; Ansolabehere, de Figueiredo and Snyder 2003). Individual donors, who may contribute for ideological reasons, are dismissed as facing collective action problems that limit their influence (Ansolabehere, de Figueiredo and Snyder 2003; Bonica 2013, 2014, 2015). Puzzlingly, a large body of evidence suggests that politicians are more responsive to the preferences of wealthy individuals and interest groups than those of ordinary citizens (Gilens 2012; Gilens and Page 2014; Bartels 2009; Page, Bartels and Seawright 2013).

Two emerging bodies of scholarship challenge the traditional minimal effects theory. Instead of buying the roll-call votes of opponents, one literature finds considerable evidence that interest groups, particularly business groups, seek to buy access to policymakers in order to influence policy outcomes (Fouirnaies and Hall 2015, 2016; Grimmer and Powell 2016). The access theory is a strong alternative to theories of vote-buying (Ansolabehere,

[1] Legal scholars, journalists, and pundits have echoed these arguments (Schuck 2014; Porter 2012; Smith 2016).

de Figueiredo and Snyder 2003) or ideologically "moderate" business groups (Bonica 2013, 2014).[2] In addition to complicating unidmensional analyses of business in politics,[3] the access theory widens our understanding of tools that individual campaign donors may use to influence legislative behavior (Kalla and Broockman 2016).

In addition to the research on contributions and access, a second literature finds that spending limits on different forms of contributions (e.g., corporate PACs, party committees, or individuals) affects the behavior of state legislators (La Raja and Schaffner 2015; Barber 2016*b*). This research, like the study at hand, suggests that shifts in the sources of campaign contributions can shape the behavior of parties in government over time.

Nevertheless, like the earlier work in search of legislative vote-buying, the new literatures may continue to obscure the broader effect of campaign contributions on legislative outcomes. Prior studies draw a sharp line between atomized individual campaign donors on the one hand, and official party committees and interest group organizations on the other (La Raja and Schaffner 2015; Barber 2016*a*,*b*; Grimmer and Powell 2016). Barber (2016*b*, 297-298) argues that individual donors are motivated by ideology, whereas interest group donors are motivated by a desire for access to important legislators. Correspondingly, he finds that individual contribution limits are associated with moderation and PAC limits with polarization.

However, there has been little change in the proportion of fundraising from individuals versus PACs in recent years. As shown in Panel (b) of Figure 5.1, the average state legislative candidate's share of funds from PACs increases from 36.7% in 2000 to 38.1% in 2012 (approximately 3% of one standard deviation in candidates' share of funds from PACs). Among only winning candidates, the share decreases from 48.6% to 48.5% during this period. These small changes are unlikely to be major causes of the precipitous rise of legislative polarization in the states during this period, shown in Panel (a) with NP-Scores based in state legislative roll-call votes (Shor and McCarty 2011). Not shown in this figure, the proportions of funds from party committees and party insiders also remain constant during this time period.[4]

In contrast, Panel (c) shows a rapid increase in candidates' share of fundraising from IGA donors. In 2000, the average candidate received less than 0.25% of her funds from IGA donors. By 2012, IGA donors comprised nearly 4% of the average candidate's funds—

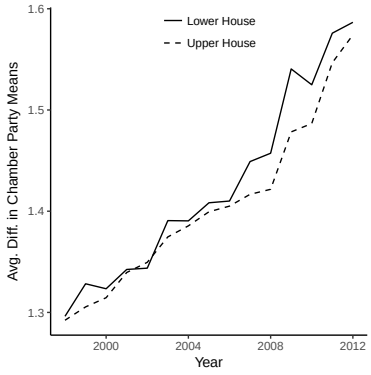
[2]Vote-buying is unlikely on salient issues, though the rise of "dark" money after *Citizens United v. FEC* may allow for more overt issue conversion. Business organizations often combine campaign contributions with lobbying to shape the less salient details of policy and use negative agenda control to exploit policy "drift" (Hacker 2004; Hall and Deardorff 2006; Hacker and Pierson 2010).

[3]Fossil fuel companies, for instance, may donate to Democrats in oil and coal producing states to buy access and ultimately limit environmental regulation, but the Democrats' liberal positions on unrelated issues like gay rights or abortion make such Democrats—and by extension, their corporate donors—appear moderate on a single dimension. In addition, scholars of structural and instrumental business power have long chronicled the ability of business to shift and redefine the "center" in ways that will not appear in measures of relative ideology on a single dimension (Lindblom 1982; Hacker and Pierson 2002, 2010).

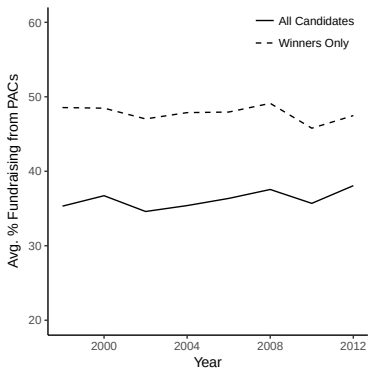
[4]The McCain-Feingold (Bipartisan Campaign Reform) Act of 2002 restricted party committee's use of "soft money" in campaigns, which La Raja and Schaffner (2015, 111–112) argue decreased the relative influence of formal parties in state politics.

Figure 5.1: Trends in Contributions and Legislative Polarization

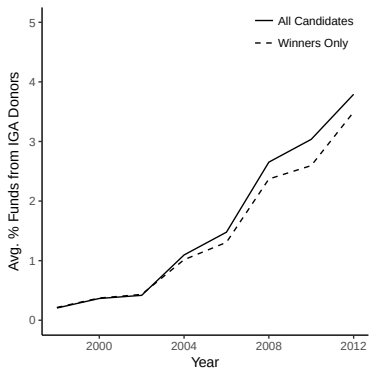
(a) Legislative Polarization



(b) Share of Funds from PACs



(c) Share of Funds from IGA



Note: Legislative polarization (difference in party chamber means) has increased (a), but the aggregate share of contributions from PACs has remained static (b). In contrast, the share of contributions from IGA donors has increased (c).

an increase of over tenfold. Although IGA donors remain a relatively small proportion of candidates' overall fundraising, this shift from virtual nonexistence to a clear presence in state legislative campaigns may influence candidate incentives if IGA donors are systematically different from other individual donors—and if increased IGA contributions is a proxy for other forms of group activist participation, such as volunteering on campaigns and lobbying legislators.

Individual Donors as Group Activists

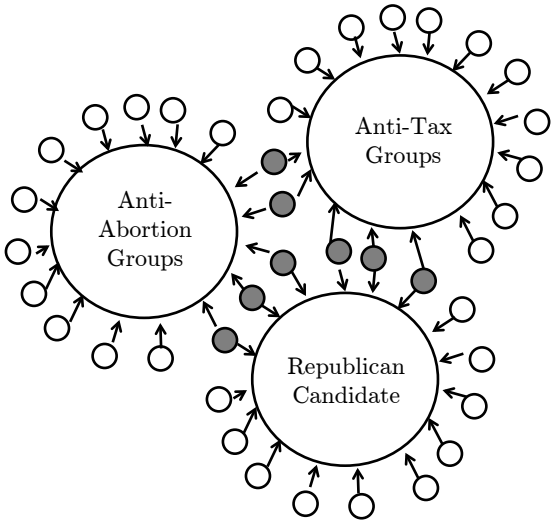
I argue that the distinction between individuals and organizations is considerably fuzzier than recent studies have suggested. Organizations have more political resources at their disposal than the amount that their official PAC contributes directly to campaigns. A group's most important resource, which has been largely neglected by political scientists, is its donor network—the individuals that provide the funds that enable the formal 501(c) organization to exist and persist (Walker 1991). Groups are more than simply their formal 501(c) organizations; they are better understood in addition as *extended networks* of donors, activists, and members. Indeed, extended networks are not unfamiliar to political science; a long tradition of research has defined political parties as coalitions of policy demanding groups in society (Key 1947; Schattschneider 1960; Truman 1951; Karol 2009; Masket 2009; Bawn et al. 2012; Hacker and Pierson 2014; Achen and Bartels 2016).

Groups seek access to politicians in order to shape policy outcomes (Fouirnaies and Hall 2015, 2016; Barber 2016*a*; Grimmer and Powell 2016), but people closely affiliated with these

groups can also contribute money directly to candidates. Groups may have a larger effect on legislative behavior indirectly through the contributions of affiliated individuals than they do from the contributions that come directly from the group's legally incorporated 501(c) organization. These affiliated individuals are in both a politician's donor network and an interest group network; that is, the candidate and the group's networks overlap.

It is well known that donors are different from non-donors (e.g., Barber 2016*b*). However, there are two reasons to expect differences between donors who give to both legislators and groups and those who give only to legislators (shown schematically in Figure 5.2). First, an individual's donation to an interest group signals interest in the group's goals, which, in the case of activist groups, centers around ideological and policy outcomes (Bawn et al. 2012). Second, organized groups can overcome obstacles to collective action more effectively than unaffiliated, atomized individuals. Interest groups can hire staff, provide information, and marshal resources that help to coordinate individuals within their network and prevent free riding (Olson 1965). In this way, organizations can amplify the political voice of their members beyond those of atomized, unaffiliated individuals.

Figure 5.2: Group and Candidate Donor Networks



Note: Large circles represent interest group PACs or a legislative candidate. Small circles represent individual donors. Shaded donors are those that contribute to both a legislator's campaign and to at least one interest group PAC.

Recent research suggests that campaign contributors have greater access to politicians than individuals that do not contribute. In a field experiment, Kalla and Broockman (2016) find that legislators and their staff are more likely to grant donors a meeting than non-donors. Such access may provide individual donors with the opportunity to influence legislative behavior. What often goes unreported in discussions of the Kalla and Broockman (2016) study, however, is that the donors seeking access to state legislators were affiliated with a

national progressive interest group, CREDO Action, which maintains a superPAC. With resources and coordination provided by the organization, the signals that such a group of donors sends to politicians are likely to be more coherent and effective at influencing behavior than those of unaffiliated, atomized individual donors.

The methods that groups use to coordinate members in order to achieve political goals are varied. A tradition of research on parties suggests that the "extended networks" of party coalitions—comprised of "policy demanding" activists and organizations—are influential in the nomination process (Schattschneider 1960; Karol 2009; Masket 2009; Bawn et al. 2012; Hassell 2016). These activists and organizations have informational and other resource advantages over ordinary voters in primary elections (e.g., Anzia 2011). Given the overwhelming uncompetitiveness of state legislative general elections (Klarner 2015), supporting their preferred candidates in the nomination process is a viable way to ensure that activists' policy goals are pursued by parties in government.

Lobbying candidates and incumbent officeholders is another potentially fruitful method of influence for groups of activists. Groups use a variety of strategies to facilitate lobbying from their activist members. Some organizations sponsor trips to legislatures and town hall meetings. Others contact members to generate large amounts of phone calls to legislative offices before key legislative votes. The National Rifle Association (NRA), for example, provides an extensive array of guidelines, information, and resources for individuals to contact lawmakers and lobby for gun rights effectively. The group's website even allows members "to identify and contact [their] lawmakers directly from [the] site."

These coordinating mechanisms may lower the costs of lobbying candidates and legislators for organizationally-affiliated individuals relative to unaffiliated individuals. In addition, group affiliation may increase the influence of individuals' political signals to candidates and legislators. By wearing an NRA hat and drawing upon a common activist language, for instance, an individual firearm activist may be perceived as a greater potential electoral threat to candidates.

Ideological Activists and Party Insiders

Ideological or single-issue groups are expected to support candidates who are ideologically pure, consistent, and active on their pet issues (Wilcox 1989; Bawn et al. 2012). Outside of business and labor groups, these ideological groups include the vast majority of what political observers and social scientists consider to be politically active interest groups: conservative Christian groups, environmental organizations, women's rights groups, antiwar groups, libertarian groups, anti-tax groups, Tea Party groups, politically active African American and Latino organizations, and many more.

In addition to interest group activists, of particular interest is the role of formal party committees in the polarization of American politics (Bonica 2013; La Raja and Schaffner 2015; Barber 2016*b*; Hassell 2018). In contrast to policy demanding ideological groups, party committees are expected to prioritize electability above other considerations (for an alternative explanation see Hassell 2018). A popular theory thus posits that contributions

from party organizations lead to more moderate politicians (La Raja and Schaffner 2015; Schuck 2014).[5] However, in addition to the weak electoral connection in the states, party gatekeepers themselves appear to prefer to recruit ideologically consistent candidates over moderates (Broockman et al. 2014). Hassell (2016) provides evidence that party committees can direct party insiders to support candidates, but there has been as yet no empirical analysis of the relationship between party insider donors and legislative behavior in office.

In this study, I investigate the effect on legislative behavior of donations from IGA donors, individuals who contribute to legislators *and* to single-issue interest group PACs, as well as the effect of donations from *party insiders*, individuals who contribute to both legislators *and* to state or national party committees.

5.3 Donor Attitudes and Participation

There are strong theories for why party committees and PACs are likely to support moderate candidates in contrast to individual donors (e.g., La Raja and Schaffner 2015; Barber 2016*b*). However, there has been little empirical focus on variation in attitudes and behavior among individual donors (but see Barber, Canes-Wrone and Thrower 2017; Barber 2016*a*). In this section I investigate the political attitudes and self-reported participation of different kinds of individual donors. This task helps to further solidify our theoretical expectations about how IGA donors and party insiders may influence legislative behavior. I first examine whether IGA donors systematically differ in ideological and policy attitudes from otherwise similar individual donors. I then ask whether IGA donors are more likely to contact legislators, a potential method that donors may use to influence legislative behavior.

Donor Attitudes

Do donors affiliated with activist groups have distinct ideological and policy views from similar copartisan donors? My analysis of data from the Cooperative Congressional Election Study (CCES) suggests that they do (Ansolabehere and Pettigrew 2014). As seen in Figure 5.3, co-partisan non-donors, legislative donors, party committee donors, and IGA donors differ significantly in their self-reported ideological placement.[6] These predicted ideologies are derived from the models that include the constituent terms for these interactions as well as demographic controls, all of which are omitted here for brevity.

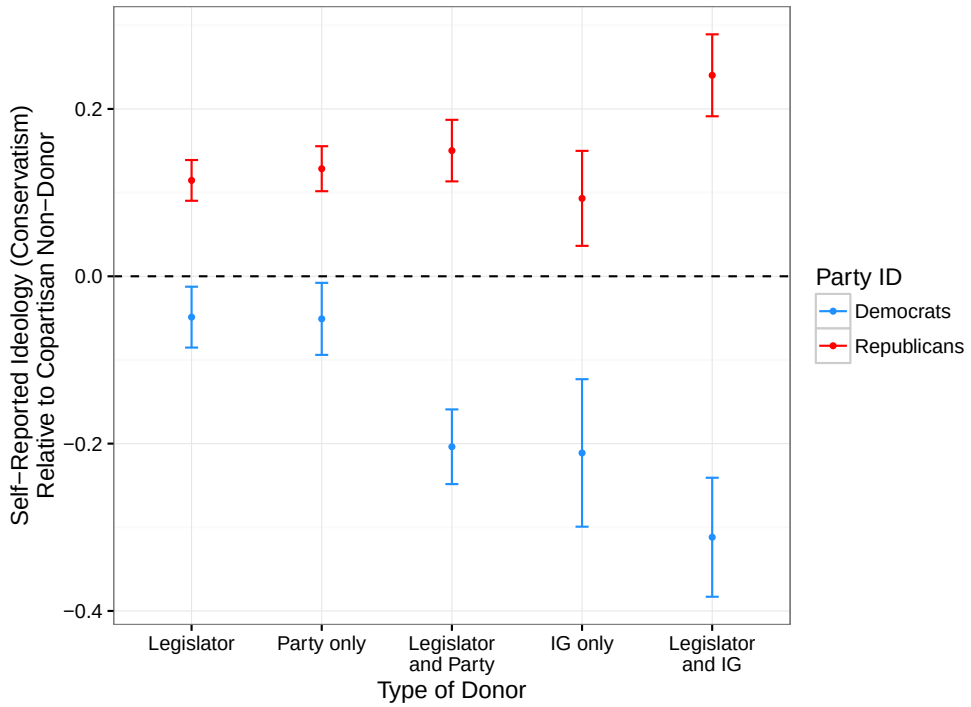
In the era of sorted and polarized electorates, party identification is the most powerful predictor of attitudes, as expected, but the within-party differences are non-trivial. Republican IGA donors are significantly more conservative than Republican non-donors and

[5]For a countervailing theory, see Malbin (2017, 545–547). Malbin (2017) suggests that the *Citizens United* ruling allowed party leaders to use 501(c) organizations to spend unlimited amounts on campaigns. This spending appears to have been concentrated in general elections, not primary elections in support of moderate candidates.

[6]Individuals who report donating to a legislative candidate and a "political group" are coded as IGA donors in the CCES data.

legislative donors; Democratic IGA donors are significantly more liberal. On the 7-point ideological self-placement scale, the average Democratic IGA donor is about 0.31 units more extreme (liberal) than Democratic non-donors and 0.26 units more extreme than Democratic legislative donors. For both parties, donors who give to party organizations (or party organization and legislators) are a middling group, with ideological self-placement in between legislative donors and IGA donors. Similar analysis presented in Appendix Table D.1 using the American National Election Survey (ANES) provides a robustness check. The ANES estimates are less precise, but this pattern is largely consistent (with the one exception being that Republican party donors are more conservative than interest group donors).

Figure 5.3: Donor Self-Reported Ideology



Note: **Interest group activist donors have more extreme ideological self-placement.** Estimates are derived from full interaction models (separated by party ID) with covariates including race, gender, age, education, and family income (see Table D.2 in the Appendix).

Non-donors, legislative donors, party donors, and IGA donors also differ significantly in their policy views. Again, the IGA donors are the most extreme (or consistently partisan). Figure D.1 in the Appendix displays predicted policy attitudes in six policy areas. Democrats who donate to both legislative candidates and interest groups are more supportive of abortion rights, gay marriage, and a path to citizenship for undocumented immigrants than their non-donor and legislative donor counterparts, while IGA Republican donors are more conservative on abortion, cap and trade, gun control, immigration, and the minimum wage. Republican

IGA donors are about 8 percentage-points less supportive of increasing the minimum wage than Republican legislative donors, and nearly 17 percentage-points less supportive than Republican non-donors. Republican IGA donors are 6 and 3 percentage-points less likely to support legal abortion rights than Republican non-donors and legislative donors, respectively. In every other policy area, Republican IGA donors are consistently more conservative and Democratic IGA donors consistently more liberal than their legislative donor counterparts, but the differences are not significant at the $p < 0.05$ level.

IGA Donors Contact Legislators

Not only do IGA donors hold more consistently extreme attitudes than their co-partisan counterparts—they are also more likely to contact their legislators. Figure 5.4 shows the predicted probability of contacting the incumbent legislator (again holding constant respondent race, gender, age, education, and income). Fewer than 50% of Republican and Democratic non-donors contact their legislator, and about 65% of those who donate to legislators also do so. In contrast, more than *three in four IGA donors* report making contact with his or her legislator.[7]

As described, contacting legislators is a plausible mechanism by which interest group activist donors can influence the behavior of legislators across time. Because contacting legislators is a costly action, this finding is consistent with arguments that organizational coordination can serve to reduce the costs (e.g., with informational resources as in the NRA example described earlier) or increase the social benefits of participation.

Challenges to Causal Inference

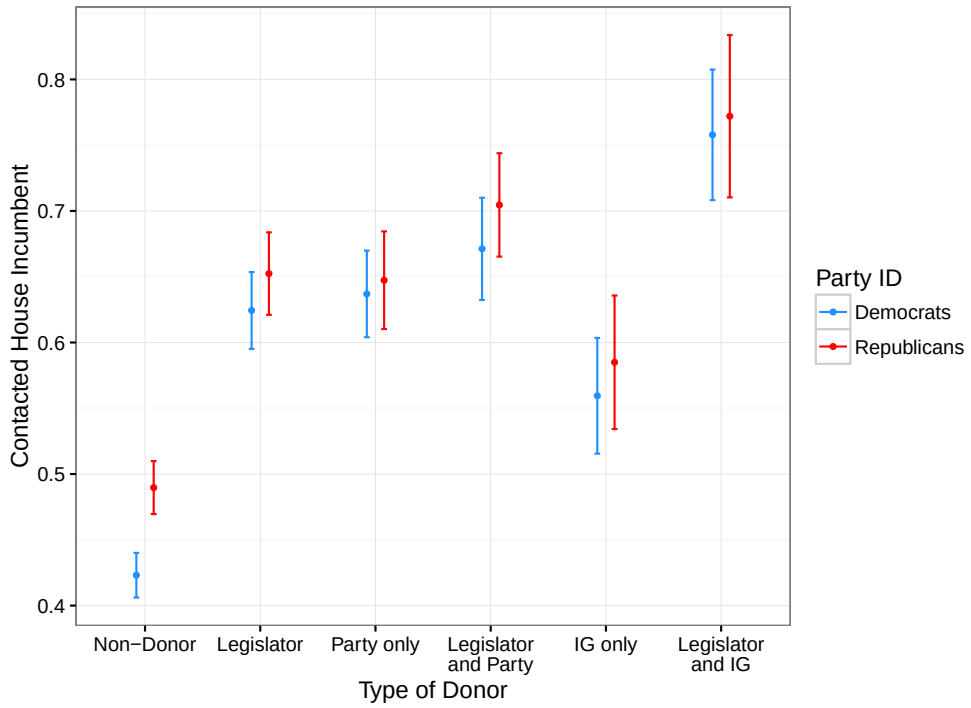
The previous section established that IGA donors are more ideologically extreme and more likely to contact legislators than other donors—patterns that lead us to expect IGA donors to play a role in the polarization of state legislatures. However, the causal relationship between contributions and legislative behavior is likely to be multidirectional. As previously argued, IGA donors may systematically support more extreme candidates, and influence the behavior of incumbent legislators through lobbying or threatening to support a primary challenger. Legislators may also become more extreme partly in order to *attract* increasingly numerous group activist donors, and party gatekeepers may recruit candidates for their ability to build networks of activist donors.

Candidate and legislator behavior, however, may be the prior cause of increased IGA contributions. State legislators may have other incentives to become more extreme, such as a desire to advance to higher office in an increasingly nationalized and polarized partisan context. This extremism, in turn, may galvanize IGA donor activity.

Although there will remain some degree of uncertainty over the direction of causality, establishing a correlation between IGA contributions and legislative behavior suggests a central

[7]The survey question asks about contacting the U.S. House incumbent, which I use as a proxy for contacting state legislators.

Figure 5.4: Contacting Legislators



Note: **Interest group activist donors are more likely to contact their legislators.** Estimates are derived from models in Appendix Table D.3.

role for IGA donors in polarization. Even if candidates and legislators are the prior cause of increased IGA contributions, candidates may only be able to become extreme because they know they can rely on a funding base with increasing concentrations of IGA donors. Overall, regardless of whether politicians' behavior lead IGA donors or vice-versa, it is likely the case that IGA donors are necessary for the observed equilibrium of legislative behavior.

5.4 Data and Methods

Calculating the Composition of Donor Networks

I use the Database on Ideology and Money in Elections (DIME), which provides consistent contributor and recipient identifiers for nearly all campaign contributions at the state and federal levels from the Federal Elections Commission (FEC), Center for Responsive Politics and National Institute on Money in State Politics (Bonica 2013).[8]

I code a contributor as an ideological activist for a given election cycle if during the cycle the individual donated to an interest group PAC defined by the Center for Responsive

[8]The FEC requires disclosure of individual donors who contribute over $200 in an election cycle.

Table 5.1: Variables: Composition of Donor Networks

Variable	*Description*
Percent IGA	Percent of legislators' funds from donors who also donated to an FEC-registered "ideological/single-issue" PAC
Percent Party Insiders	Percent of legislators' funds from donors who also donated to a party committee
Percent Ideological PACs	Percent of legislators' funds from ideological/single-issue PACs
Percent Party Committees	Percent of legislators' funds from party committees
Percent PAC	Percent of legislators' funds from PACs
Percent Individual	Percent of legislators' funds from individuals
Total Money Received	Total dollar amount of fundraising during the election cycle
Total Number of Donors	Total number of unique donors during the election cycle
Democratic Presidential Vote Share	Share of two-party presidential vote received by Democratic candidate
Outcome: NPscore	Estimate of state legislator ideology at year $t+1$, with larger values indicating greater conservatism (Shor and McCarty 2015)

Politics as "ideological/single-issue." The Appendix provides a list of ideological/single-issue PACs in the data. Analogously, I define *party insiders* as individuals who have donated to a state or national party committee in a given electoral cycle (see Hassell 2016).

Legislative Behavior

The outcome of interest is a measure of legislator ideology derived from roll-call votes from Shor and McCarty (2011), analogous to the DW-NOMINATE scale developed by Poole and Rosenthal (1997).[9] Although, as explained previously, such single-dimensional scales are problematic for the analysis of the influence or preferences of campaign contributions from business groups, they are an extremely useful tool for analyses involving parties and partisan ideological interest groups.

[9]I hesitate to call DW-NOMINATE or the Shor-McCarty scale measures of ideology because of the difficulty of disentangling the role of ideology and partisanship in the polarized era (Lee 2009). However, whether the scales measure ideology or partisan consistency—two highly correlated variables in recent years—does not affect this particular study.

Legislative behavior is measured in year $t+1$, where year t is the election year. I thus test the effect of the composition of candidates' donor networks during an election season on their behavior while in office. Table 5.1 summarizes the variables I employ in subsequent statistical tests.[10]

For ease of interpretation, all variables are rescaled to have a mean of 0 and standard deviation of 1.

I construct traditional time-series cross-sectional (TSCS) regression models to estimate the effect of dynamics in the composition of donor networks on legislative behavior. As described, I depart from some recent literature on state legislative behavior by estimating separate models for Democrats and Republicans. All models contain state fixed effects in order to restrict the analysis to within-state variation across time, as well as legislative chamber fixed effects (i.e., a dummy variable for upper legislative chamber).

The main models include year fixed effects to control for time trends. Because the percent of IGA contributions to candidates has risen precipitously since 2000 (as previously shown in Figure 5.1) and state legislatures have polarized during the same period (Shor and McCarty 2011), models that include year fixed effects are likely to show a much smaller correlation between ideological activist donors and legislative behavior. Year fixed effects protect against potential confounding variables that may influence both the composition of donor networks and legislative behavior over time. However, it may be the case that the universal increase in proportions of ideological activist donors is exogenous; in this case, estimates from the models without year fixed effects are preferred.

Primary Elections

In order to test the extent to which potential influence over legislative behavior arises from primary election or general election fundraising, I fit additional models that separate contributions into primary and general election periods. To do so, I calculate candidates' funding amounts from different sources in the primary and general election periods using a new dataset of state legislative primary dates.

A research assistant collected state legislative primary dates between 2000 and 2012 from state government websites. The primary dates dataset includes special elections that vary by legislative district. To my knowledge, this is the first nationally comprehensive dataset of state legislative primary dates.

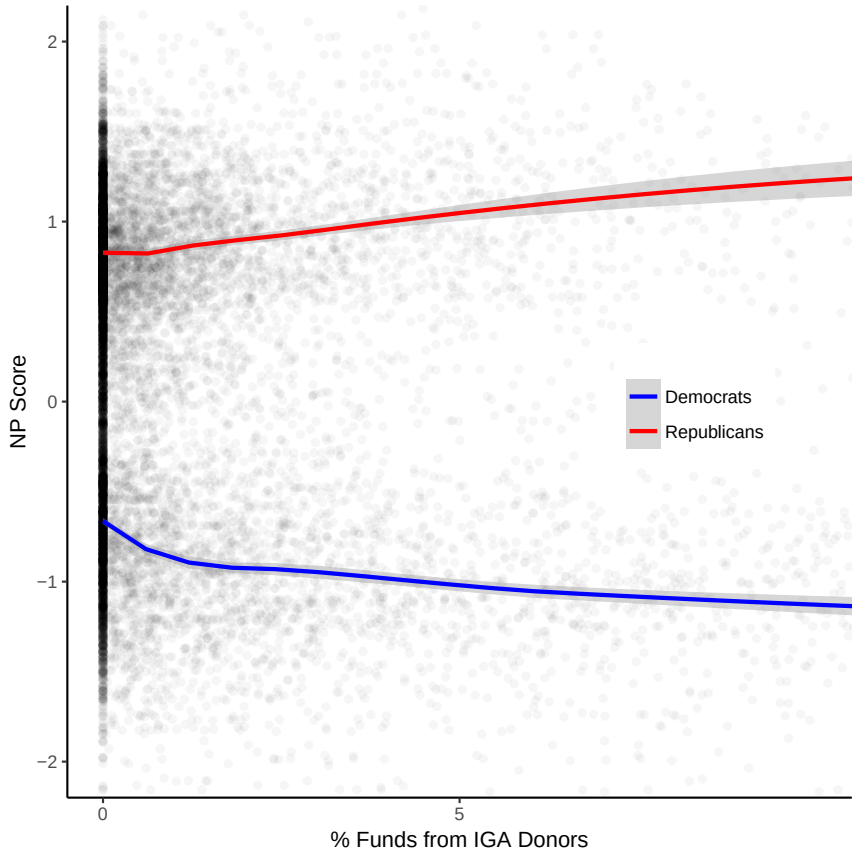
5.5 Results

I first plot the bivariate relationship between candidates' concentration of IGA donors and legislative behavior in Figure 5.5 using loess regressions. As expected, legislators with

[10] Like (Barber 2016*b*), I also construct models in which I only use Shor-McCarty scores from legislators' first term in office.

larger proportions of interest group activists in their donor networks have more extreme (or consistently partisan) roll-call voting behavior.

Figure 5.5: Donor Composition and Legislator Ideology



Both Republicans and Democrats appear more extreme in office when they rely on greater numbers of IGA donors. At lower concentrations of IGA donors (i.e., moving from 0 to 1 percent IGA contributions), the slope is steeper for Democratic legislators. Overall, the slopes of these loess curves are quite symmetric.

However, the relationship between IGA contributions and legislative behavior in Figure 5.5 may be confounded by time, geography, and other sources of fundraising that may be correlated with IGA contributions. Figures 5.2 and 5.3 show the relationship between the concentration of IGA contributions and legislative behavior for Democrats and Republicans, respectively, conditional on these potential confounders.

Tables 5.2 and 5.3 report that Democratic and Republican candidates with greater concentrations of IGA contributions have more extreme roll-call voting records once in office. As shown in Table 5.2, a one standard deviation increase in candidate funds from IGA donors is associated with more liberal NP-scores (a shift of between 0.028 and 0.041 units). Table

Table 5.2: Effect of Legislator Donor Networks (Democrats)

	(1)	(2)	(3)	(4)
% funds from IGA Donors	-0.0414***	-0.0353***	-0.0339***	-0.0277***
	(0.00728)	(0.00685)	(0.00670)	(0.00633)
% funds from Party Insiders	-0.0675*	-0.0860**	-0.0299	-0.0465
	(0.0304)	(0.0309)	(0.0256)	(0.0246)
% funds from Ideological PACs			-0.0229**	-0.0242**
			(0.00723)	(0.00729)
% funds from Party Committees			0.0261**	0.0247**
			(0.00769)	(0.00784)
% funds from PACs			0.0110	0.0116
			(0.00801)	(0.00800)
% funds from Individuals			-0.0295**	-0.0305**
			(0.0102)	(0.00994)
Total funds	0.214***	0.219***	0.153***	0.162***
	(0.0467)	(0.0479)	(0.0408)	(0.0433)
Num. unique donors	0.00546	0.00645	0.0105	0.0116
	(0.0151)	(0.0154)	(0.00842)	(0.00859)
Dem. POTUS vote share	-0.0220	0.000174	-0.0232	0.00154
	(0.0208)	(0.0212)	(0.0212)	(0.0208)
Constant	-0.452***	-0.384***	-0.446***	-0.448***
	(0.0361)	(0.0434)	(0.0408)	(0.0435)
Party Control FEs	x	x	x	x
Chamber FEs	x	x	x	x
State FEs	x	x	x	x
Year FEs		x		x
N	7443	7443	7331	7331
R^2	0.619	0.621	0.626	0.628

Robust standard errors in parentheses. Standard errors are clustered by legislator.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5.3: Effect of Legislator Donor Networks (Republicans)

	(1)	(2)	(3)	(4)
% funds from IGA Donors	0.0288***	0.0188**	0.0268**	0.0162*
	(0.00781)	(0.00674)	(0.00788)	(0.00658)
% funds from Party Insiders	-0.0165	-0.0136	-0.0289	-0.0270
	(0.0187)	(0.0194)	(0.0187)	(0.0185)
% funds from Ideological PACs			0.0149*	0.0164**
			(0.00591)	(0.00591)
% funds from Party Committees			-0.00741	-0.00338
			(0.00534)	(0.00570)
% funds from PACs			-0.0368***	-0.0380***
			(0.00881)	(0.00950)
% funds from Individuals			-0.000651	0.00206
			(0.0130)	(0.0129)
Total funds	-0.106	-0.125*	-0.112	-0.139*
	(0.0609)	(0.0573)	(0.0649)	(0.0625)
Num. unique donors	-0.0765	-0.0739	-0.0873*	-0.0850*
	(0.0415)	(0.0397)	(0.0426)	(0.0407)
Dem. POTUS vote share	0.0795*	-0.0134	0.0823**	-0.0153
	(0.0309)	(0.0409)	(0.0307)	(0.0395)
Constant	0.895***	0.659***	0.897***	0.648***
	(0.0487)	(0.0782)	(0.0510)	(0.0782)
Party Control FEs	x	x	x	x
Chamber FEs	x	x	x	x
State FEs	x	x	x	x
Year FEs		x		x
N	8788	8788	8634	8634
R^2	0.405	0.416	0.412	0.424

Robust standard errors in parentheses. Standard errors are clustered by legislator.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.3 reports that a one standard deviation increase in IGA contributions for Republicans is associated with between a 0.016 and 0.029-unit rightward shift.

The magnitudes of the effects are substantial. For context, in 2012 the average Democratic state legislator was 0.11 units more liberal, and the average Republican 0.18 units more conservative than in 2000. The aggregate increase in IGA donors shown in Panel (c) of Figure 5.1 can explain about 25% of the liberal shift for Democrats over this period, and about 9% of the conservative shift of Republicans.

Importantly, this finding for IGA donors remains robust even when controlling for contributions from the activist *organizations* with which IGA donors are affiliated (shown in Models 3 and 4). Ideological/single-issue groups and their extended networks of individual activists appear to have independent effects on legislative behavior. Contributions from these ideological and single-issue PACs such as the NRA and MoveOn.Org are associated with legislative extremism for both parties, as expected, and their effects are of similar magnitude to those of IGA donors. If groups are to be conceptualized as formal organizations and individual affiliates, then the effect of these activist groups could be as large as the sum of the effects of the ideological PACs and IGA contributions.

In both tables, Models 3 and 4 also control for candidates' share of funding from PACs and individual donors. I find effects that are consistent with those of Barber (2016*b*), in which greater overall PAC contributions relative to individual contributions is associated with moderate legislative behavior. For Democrats, the effect of percent individual contributions is significantly different from zero and significantly larger in magnitude than the effect of IGA donors. For Republicans, the effect of percent PAC contributions is significantly different from zero, but statistically indistinguishable from the effect of percent IGA contributions.

In contrast to IGA donors, contributions from party insiders have no consistent relationship to extreme roll-call voting in state legislatures. Tables 5.2 and 5.3 report that party insiders are associated with greater extremism for Democrats and moderation for Republicans, but these associations are not statistically significant (with the exceptions of Models 1 and 2 for Democrats).

Primary and General Election Contributions

Activist groups are understood to influence parties in the nomination process (Bawn et al. 2012; Hassell 2016). In this section, I present results of models that separate sources of campaign contributions in the primary and general election periods.

Table 5.4 reports the results of models of the composition of donor networks separated into the primary and general election periods. Critically, the large effects for percent ideological activist donors are concentrated in the primary election period. An increase in percent IGA contributions in the primary period of one standard deviation is associated with a 0.015- to 0.0589-unit shift leftward for Democrats, and a 0.031-unit shift rightward for Republicans.

Ideological/single-issue PAC contributions in primaries are also associated with more extreme legislative behavior, but the effect is considerably smaller than that of IGA donors. Party insiders again appear to moderate Democrats, with their greatest effect in primary

Table 5.4: Donor Networks at the Legislator Level (Primary and General)

	(1) Democrats	(2) Democrats	(3) Republicans	(4) Republicans
P: % funds from IGA Donors	-0.0151***	-0.0589**	0.0307	0.0307*
	(0.00376)	(0.0175)	(0.0168)	(0.0146)
P: % funds from Party Insiders	-0.0885***	-0.0376*	-0.00180	-0.0140
	(0.0213)	(0.0161)	(0.0103)	(0.0114)
P: % funds from Ideological PACs	-0.0230***	-0.0257***	0.0176*	0.0180*
	(0.00556)	(0.00541)	(0.00815)	(0.00791)
P: % funds from Party Committees	0.00399	0.00449	0.00299	-0.00108
	(0.00687)	(0.00677)	(0.00392)	(0.00390)
G: % IGA	-0.0182	-0.00751	0.0179*	0.0149
	(0.0114)	(0.00985)	(0.00845)	(0.00827)
G: % Party Insider	-0.00868	-0.00218	0.00138	-0.00393
	(0.0157)	(0.0140)	(0.0198)	(0.0211)
G: % Ideological PACs	-0.00969	-0.0110	0.00753	0.00788
	(0.00610)	(0.00671)	(0.00467)	(0.00483)
G: % Party Committees	0.0239**	0.0252**	0.00831	-0.00568
	(0.00697)	(0.00758)	(0.00927)	(0.0102)
P: % PAC		0.0180*		-0.0403***
		(0.00805)		(0.00955)
P: % Individual		-0.0242*		-0.00143
		(0.0105)		(0.0135)
Dem POTUS Vote Share	-0.00462	0.00136	-0.0122	-0.0155
	(0.0219)	(0.0207)	(0.0409)	(0.0401)
Constant	-0.404***	-0.454***	0.662***	0.650***
	(0.0457)	(0.0439)	(0.0816)	(0.0793)
Party Control FEs	x	x	x	x
Chamber FEs	x	x	x	x
State FEs	x	x	x	x
Year FEs	x	x	x	x
N	7468	7370	8811	8672
R^2	0.623	0.627	0.417	0.424

Robust standard errors in parentheses. Standard errors are clustered by legislator.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

elections. Again, however, party insiders have no significant relationship to Republican legislative behavior.

In general elections, IGA contributions and formal ideological/single-issue PAC contributions still have effects in the expected direction (extremism). The effect magnitudes are consistently smaller than those for primary election periods, but still apparent and, in most cases, statistically significant at the $p < 0.1$ level. Overall, the results corroborate theories that emphasize the role of activist groups in the nomination process.

5.6 Organization Matters

Organized groups are more than their staff members, their offices, and their formally incorporated 501(c) organizations. They are networks of activists and patrons engaged in a relationship. This investigation of the role of organizationally affiliated donors offers a more complete picture of the influence of groups on polarization, and provides an additional explanation for the weak relationship between public opinion and state legislative behavior (Rogers 2017). A large body of literature suggests that legislators are more responsive to elites and groups than mass attitudes (Bartels 2009; Gilens 2012; Gilens and Page 2014), but campaign contributions—a tool in which wealthy individuals and organized groups hold advantages over ordinary citizens—are often found to have minimal effects.

I find that the percentage of a legislative candidate's donors who are affiliated with interest groups may influence legislative behavior. Both Republican and Democratic state legislators with larger proportions of IGA donors are more extreme than their copartisan counterparts in their states. This relationship is robust to holding constant legislators' overall contributions from PACs and individuals, which previous research has shown to influence legislative behavior (Barber 2016*b*).

However, individual campaign donors with close ties to formal party organizations do not appear to be a moderating influence. There is evidence that these party insiders are influential in the candidate nomination process (Hassell 2016), but they may not systematically select for more moderate candidates. This finding challenges arguments that point to the decline of party insider influence as central to the polarization of state legislatures (La Raja and Schaffner 2015).

This study investigates the role of activists affiliated with ideological and single-issue groups, but such groups represent a mere fraction of organizationally mobilized money in state politics. In contrast to this study, other emerging research investigates the influence of super-elite groups like the American Legislative Exchange Council (ALEC) (Hertel-Fernandez 2014), the Koch network (Skocpol and Hertel-Fernandez 2016), and business groups (Hacker and Pierson 2010), which have few activist affiliates outside of large donors and individuals who are directly employed by the interest groups. Such super-elite groups often attempt to mobilize grassroots or "astroturf" movements in support of their causes (Walker 2014), but the ratio of resources provided by major patrons relative to activists affiliates is much higher than for the ideological and single-issue groups addressed in this

study. Ideological and single-issue activist groups are more likely than business and super-elite groups like Americans for Prosperity to prioritize social issues over economic issues. In addition, super-elite groups appear more likely to employ or partner with 501(c)4 or 501(c)6 organizations with the ability to raise, spend, and transfer unlimited sums of "dark" money from anonymous contributors toward political activities. Future research should not neglect the larger context of groups' political resources and expenditures in drawing conclusions.

Three additional areas of further research are ripe for investigation. First, the relationships between organized groups and individual activists needs theoretical and empirical development. Why do donors contribute to activist groups, and how does the affiliation with interest groups shape the political participation of individuals? What variation in coordinating strategies exists among interest groups, and what are the results of such variation? Such investigation can shed light the "origin story" of IGA donors.

Second, further research can disentangle the mechanisms of IGA influence. Although this study makes progress in uncovering mechanisms in various ways, such as disaggregating IGA contributions in primary and general elections, these IGA contributions are likely to be correlated with other forms of political participation. Additional research may discover creative ways to exploit quasi-exogenous variation in specific forms of IGA participation, such as lobbying candidates or volunteering in campaigns.

Third, a policy-oriented focus on the influence of activists is warranted. The donors in the field experiment by Kalla and Broockman (2016) were coordinated by a interest group to lobby legislators about a complex piece of legislation on chemical regulation. Organized groups provide individual activists with resources to effectively lobby on specific policy issues. While this study estimates the effect of activist donors on legislative behavior measured on a single left-right dimension, subsequent research should investigate policy-specific effects in areas such as environmental regulation, labor relations, gun control, abortion, and civil rights.

Chapter 6

Conclusion

6.1

In the 2016 Republican presidential primaries, Ohio Governor Kasich had run as the perceived "moderate." Kasich bolstered his moderate credentials with folksy Midwestern politeness and a personal story of attending a friend's same-sex wedding. Unidimensional measures of "ideology" from political science, such as that of CrowdPAC, agreed, placing Kasich to the left of Marco Rubio, Jeb Bush, Carly Fiorina, Rick Santorum, and others.

But like those of other Republican governors and state legislatures in the 2000s and 2010s, Kasich's policy agenda for Ohio was not particularly moderate. He signed laws that dramatically restricted women's right to obtain abortions, including new restrictions on abortion clinics and mandatory ultrasounds; the number of abortion clinics in Ohio declined by approximately half during Kasich's tenure in office. He signed a 2016 bill to permit concealed carry of firearms on college campuses, child care centers, and the public areas of airport terminals. He implemented a partial expansion of Medicaid under the ACA and was quicker to back down from intense battles with organized labor than his Midwestern counterparts like Rick Snyder (MI) and Scott Walker (WI)—but he has also pursued Medicaid work requirements and hardline tax and spending cuts.

Democratic state governments have also become increasingly active policymakers, but they are mostly treading water as the national policy landscape continues to move rightward due to both conservative national policies and to policy drift (Hacker and Pierson 2010).[1] Tax policy is illustrative. The federal income tax for high earners declined by more than half since its marginal rate of 71.75% in 1970. On average, Democratic state governments have raised top tax rates more than Republican states have cut them—but blue states' tax increases are many times too small to make up for the dramatic cuts to Federal taxes in recent decades.[2]

[1] Drift occurs when policy is not updated along with changing economic, technological, and social forces.

[2] Immigration is a similar story. Welfare reform in the 1990s banned the use of Federal funds for food stamps, Medicaid, and other safety net programs for legal green card-holding immigrants. Some state governments, especially those controlled by Democrats, used their own budgets to make up for this new

Unlike the new state laws in Ohio, the policy attitudes of the Ohioans had not suddenly become more conservative, as we saw in Chapter 4. Even in partisan terms the Ohio mass public had not become especially Republican: they elected Kasich by a margin of 49 to 47%, smaller than the margin that Barack Obama won the Ohio presidential vote in 2008 and 2012. But in the context of national and polarized political parties, small electoral margins that lead to unified party control of state government have profound policy consequences. As we saw in Chapter 2, these policy changes affect socioeconomic outcomes such as health insurance coverage.

In states across the country, policies shifted dramatically since 2000. If not the mass public, what explains these shifts? This dissertation argues that changes in party organization are key to understanding the resurgence and polarization of state policy over the past generation.

Important research has shed new light on elite- and organizational-level changes that helped usher in a new era of ambitious, coordinated policy agendas across many states (Hertel-Fernandez 2014; Hertel-Fernandez and Skocpol 2016; Hertel-Fernandez 2016). Chapter 5 points to an additional as yet uncovered shift in the organizational landscape: coordination of activist groups. I investigate activist networks' campaign contributions, an important form of political participation that connects activists and groups to candidates and parties in government. Since 2000, there has been a precipitous increase in the number of individual campaign donors who are connected to single-issue and ideological activist organizations such as the NRA, Americans for Tax Reform, MoveOn.org, and Planned Parenthood.

6.2 New Directions in the Study of American Politics

Just as findings about polarization in Congress upended "textbook" studies of American politics, the insights of this research—that state policy is increasingly consequential, that it is mostly not driven by public opinion, and that it may be partly the result of activist groups—point to new puzzles and routes of inquiry. In addition to the more specific questions for further investigation outlined in each chapter's conclusion, I point to four broad areas that political and economic research should address.

First, research should investigate the institutional dynamics of polarized federalism. As I touch upon in Chapter 2, polarization and divided government have led to increased gridlock at the federal level. Policy demanding actors face greater costs of influencing policy as the gridlock interval widens. The state level can serve as a "safety valve" that allows these policy demanders to venue shift their political investments to the states in response. Further theoretical and empirical research should investigate this *vertical* shift in federalism.

Polarized federalism may also systematically affect *horizontal* incentives in federalism. In the generations since Louis Brandeis described the U.S. states as "laboratories of democracy," federalism has been lauded for incentivizing policy experimentation and learning. State governments engage in policy experimentation and may "act as scientists, watching these

Federal restriction.

experiments and learning from them" to produce more effective governance (Shipan and Volden 2012, 490). This sort of institutional learning, in which governments may emulate best practices in other states, has been thoroughly investigated in empirical studies of policy diffusion.

But just as the *Federalist Papers* had done before, Brandeis' comments about federalism neglected *political parties* in describing governments' incentives. While a plethora of observational studies have separately investigated whether governments emulate successful policies (e.g., Volden 2006) or emulate the policies of copartisan governments (e.g., Grossback, Nicholson-Crotty and Peterson 2004), elite polarization may decrease the incentive to learn from other governments. Partisan-aligned organizations provide informational resources and "legislative subsidies" to governments (Hall and Deardorff 2006), so the polarization of the organizational environment is likely to affect institutional learning.

Second, research should devote greater attention to obstacles to political accountability under federalism. Hopkins (2018) suggests that voters' attention has shifted from local and state toward national politics in ways that may reduce their ability to hold state and local politicians responsible for solving local problems. de Benedictis-Kessner (2018), for instance, shows that residents may understand the quality of public goods in their communities, but because it is difficult to know which level of government is responsible, they do not update their beliefs about governmental performance accordingly. As briefly discussed in Chapter 4, media firm conglomeration has led to a steep decline in state politics journalism since the 1990s—making it more difficult for citizens to get the information they need to make informed decisions in lower level elections.[3]

Third, this dissertation highlights the need for additional research on the development of political organizations and their relationships to the political parties. Recent years have seen renewed scholarly interest in group-party coalitions (e.g., Karol 2009; Masket 2009; Bawn et al. 2012; Hacker and Pierson 2014; Achen and Bartels 2016), but there remains considerable need for the ways that organizations marshal political resources to influence politics. A study from Lacombe (2018) investigates the way that the NRA politicizes gun owner identity and helps to develop a common activist language around opposition to gun control policy, offering a helpful example of a path forward.

Finally, I hope that this project reinvigorates normative debates about the relationship between federalism, democracy, and equality. It is safe to characterize the scholarly consensus on the role of federalism in this country's authoritarian racial order in the words of William Riker (1975, 155-156): "All that federalism ever did was to facilitate the expression of racist beliefs and the perpetuation of racist acts." By amplifying the institutional authority (especially the veto powers) of powerful minority interests, slaveholders and segregationists, federalism delayed the end of slavery and Jim Crow.

Allowed greater discretion by the courts than a generation ago, conservative state governments have innovated policies and administrative procedures that harken back to Jim

[3]This is consistent with the finding that the minimal incidence of retrospective voting in local elections is conditional on cities' overlap with media markets (Hopkins and Pettingill 2017).

Crow. Voter identification laws, motivated by desires to influence the partisan and racial distribution of voters (Rocha and Matsubayashi 2014), create new barriers to the franchise that disproportionately affect nonwhite and poor citizens (Sobel and Smith 2009). Medicaid provision has long been racially unequal (Michener 2018), but the Michigan state government is poised to institute an innovative system of work requirements for Medicaid qualification that, due to the design of exemptions, would disproportionately apply to black and Latino beneficiaries (Scott 2018). These policies do not stem from changes in voter attitudes. They are the product of a new Republican Party agenda, built over many years by conservative patrons, organized activists, and party insiders, that is keenly aware of voters' inattention to state politics and to the advantages of sewing favorable political terrain through policies that constrain their political opponents.

6.3 Implications for Political Actors

The results of this project underscore the importance of influence at the level of elites, organizations, and activists rather than the broad electorate. Major state policy changes have occurred with little correspondence to overall public opinion, and evidence suggests that there is minimal accountability in state politics for politicians whose behavior is "out of step" with their voters. Anti-abortion activists, for instance, have succeeded in dramatically restricting access to abortion across many states without much change in mass opinion on abortion. Environmental groups have begun to follow suit, shifting from strategies focused on changing voter attitudes about climate change toward a more activist-based model.

The most important insight for donors, activists, and groups, however, is quite simple: *do not neglect the state level*. Given the dominance of theories that emphasize constraints facing state governments, it is understandable that some policy demanders may under-invest in state politics. It appears in particular that conservative groups have had greater recognition of the increasing consequentiality of the state level (Hertel-Fernandez and Skocpol 2016). Meanwhile, Democrats have suffered historic long term electoral losses in state legislatures and governorships, and Democratic-aligned organizations that have traditionally invested in state politics, such as labor unions, have declined.

The Trump Administration and Republican Congress are attempting to use federal rules and policy to undo liberal state policies in areas like immigration and marijuana, and the Congressional Republican tax legislation passed in late 2017 aims to constrain resources deployed by liberal states. If future elections usher in Democrats and a divided government, however, we may see either new rounds of state-level initiatives to overcome gridlock. Scholars and activists alike must keep an eye on the states, because in contemporary U.S. government, they are often where the action is.

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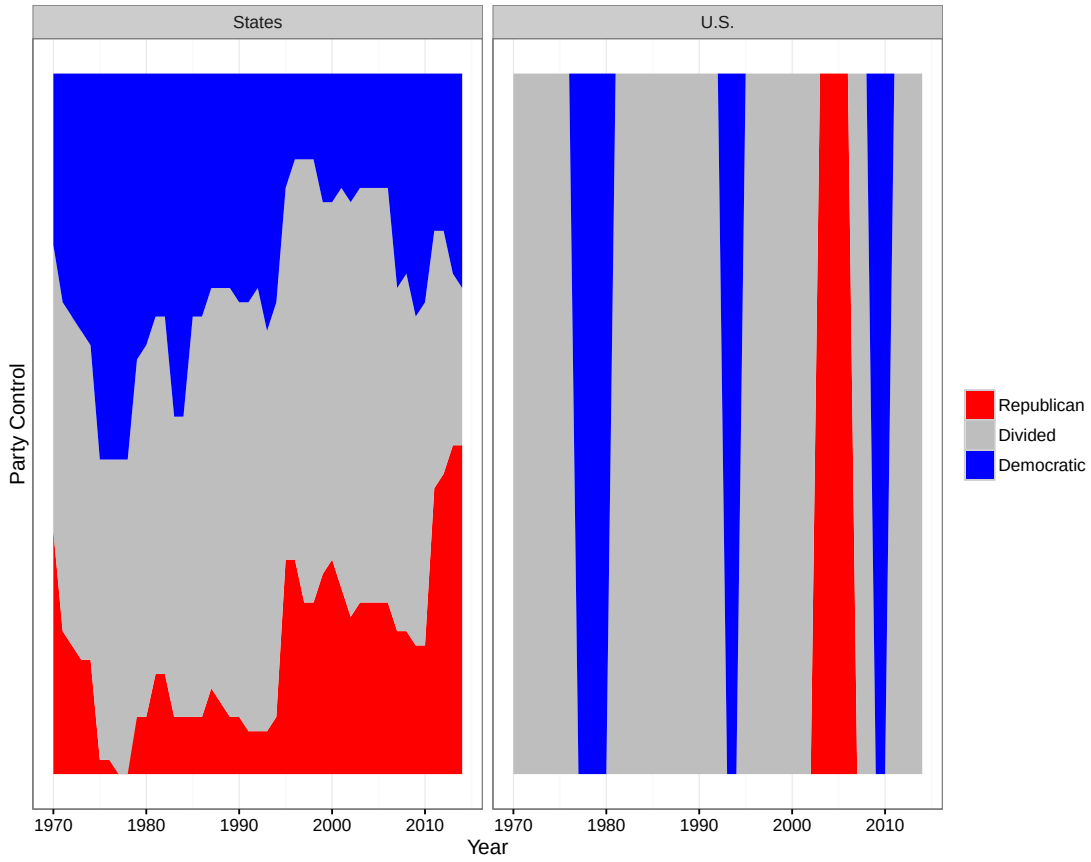
Appendix A

Chapter 2 Supplement

Party Control of Government

Figure A.1: Party Control of Government, 1970-2014

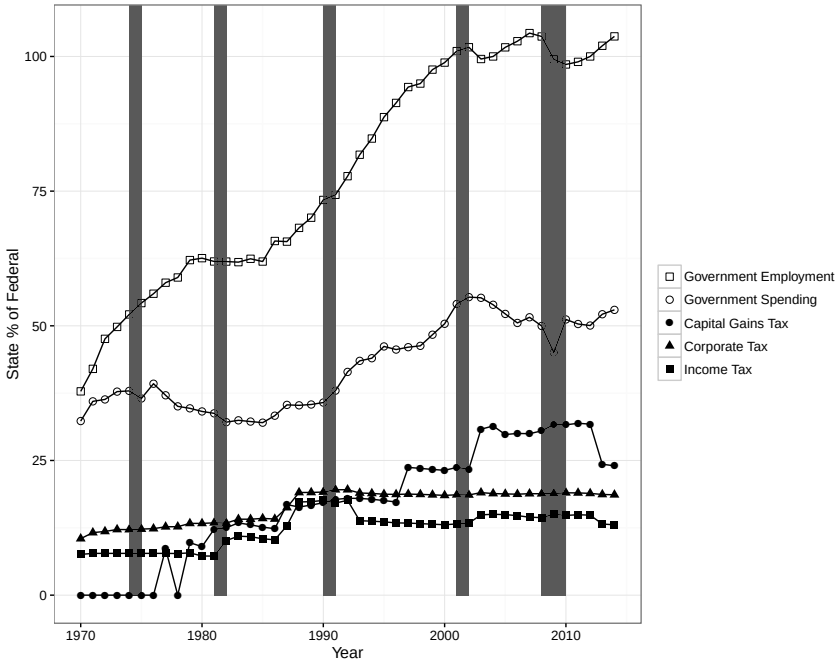


Note: **On average, states have been less frequently under divided government than has the Federal Government since 1970.** For the states, party control is measured as holding the governorship and majorities in both state legislative chambers. For the Federal Government, party control is measured as holding the presidency and majorities in both houses of the U.S. Congress.

Polarized Federalism and the Size of Government

Party polarization may increase incentives for the Federal Government to allow for greater interstate policy variation *and* the desire of partisan state governments to generate this variation in practice. What might this policy variation look like? Historically, the desire for greater decentralization and interstate variation in American federalism is associated with conservatism. Concepts like states' rights and devolution saw their most prominent historical use by factions in support of slavery and segregation or of a smaller redistributive role of government. Conversely, liberalism is associated with centralization and nationalization because, as Melnick (1996, 326) describes, "[s]ince the New Deal, centralization of authority has gone hand-in-hand with the expansion of a particular type of individual rights—positive rights guaranteeing government benefits and protections."

Figure A.2: Expanding Fiscal Role of States



Note: The plot shows total state governmental employment and spending as a percentage of federal employment and spending, respectively; tax rates are the top marginal rates for the average state as a percentage of the top federal tax rate (for each type of tax). Shaded areas represent recessions, which tend to increase the role of the Federal Government relative to the states.

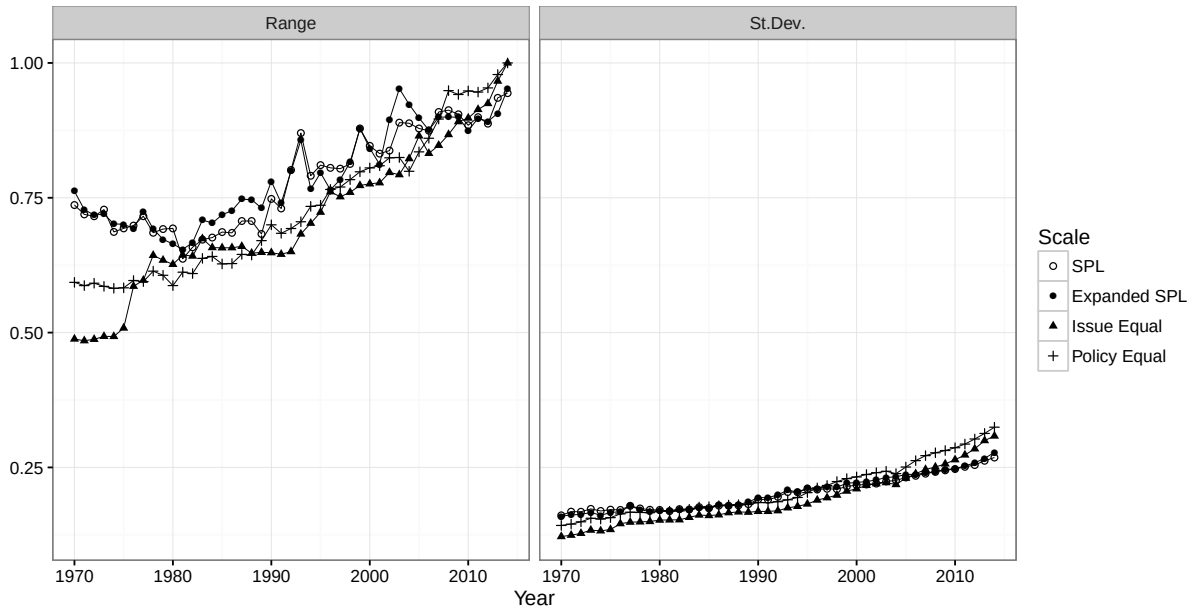
Yet this historical association does not necessarily imply that state policy change will tend to be conservative under polarized federalism. On the contrary, because federal policy has trended more conservatively since the 1970s (especially in economic policy) and policy drift has conservative effects (Hacker and Pierson 2010), *liberal* actors may be more likely to use the states as a safety valve. Democrats may be more likely to substitute state policy for declining production of federal policy. For instance, the federal income tax for high earners has declined dramatically since its marginal rate of 71.75% in 1970. In 1988 it hit a postwar nadir of 28%, which rose to 39.6% in the 1990s and then again in 2013. These federal cuts may move policy toward Republicans' ideal policy of lower taxes at all levels of government, but farther away from the Democratic ideal. This would generate no change in Republican behavior at the state level, but it is likely to lead state Democrats to raise state taxes to substitute for the federal cuts. Correspondingly, Figure A.2 shows state governmental employment, spending, and various tax rates as a percentage of the federal level (shaded areas represent recessions), and there is indeed a clear expansion of the fiscal role of states. In addition, because federal policy change tends to be liberal in general (Grossmann 2014), this dynamic could also occur with social policies as Democrats pass state laws expanding the rights of historically marginalized groups that would have otherwise been implemented

nationwide.

Unidimensional Analysis

The four unidimensional measures show expanding variation over time. In Figure A.3 I show the range of state ideal points (i.e., the difference between the most liberal and most conservative state) in each year (the left panel), as well as the standard deviation of state ideal points in each year (the right panel).

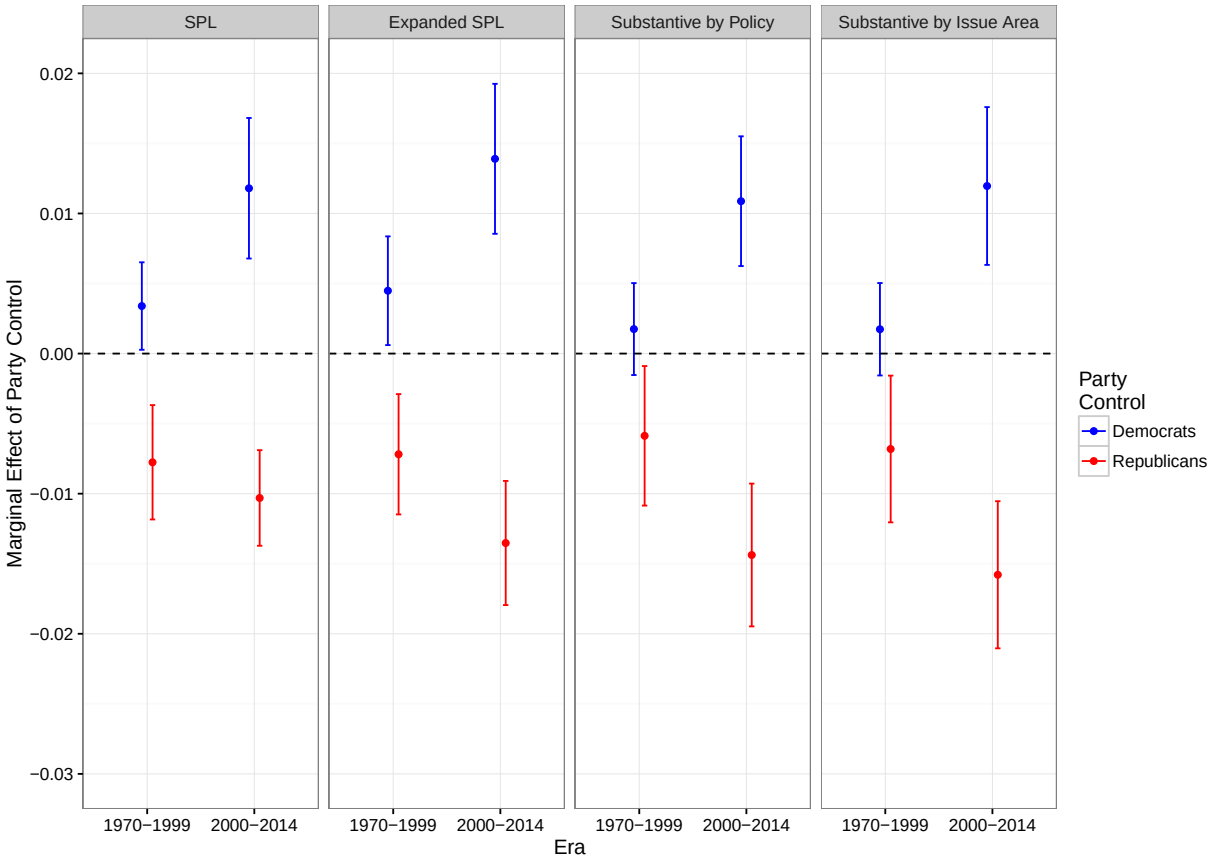
Figure A.3: Policy Variation with Unidimensional Ideal Point Measures



Note: **The range and standard deviation of state policy ideal points increase over time.**

The measures also show greater policy polarization after 2000. Figure A.4 shows the marginal effect of unified party control on each of the unidimensional measures.

Figure A.4: Effect of Party Control Pre-2000 and Post-2000



Note: **Unidimensional measures show a growing relationship between party control and policy outcomes.** Estimates are derived from models that include state and year fixed effects, state × era fixed effects, and lagged dependent variables for years $t-1$ and $t-2$. Standard errors are clustered by state.

Alternative Temporal Breaks in the Party-Policy Relationship

Analyses presented in this paper compared the party-policy relationship before and after 2000 to balance precision with the need to highlight more recent years of hyper-polarization. Chow tests show clear evidence of a structural break in that year. However, there may exist heterogeneity across policy areas and measures in the timing of *optimal* breakpoints (i.e., that minimize the model's sum of squared residuals). In this section I test empirically for the location break years when the relationship between party control and policy outcomes shifts systematically. The procedure involves estimating many segmented regression models to test which temporal break point (or points) minimizes the sum of squared residuals. Specifically, I test for temporal breaks in traditional two-way fixed effects models.

Figure A.5 shows the results of two kinds of tests of temporal breakpoints. The solid circles represent the most likely year of a single structural break using Chow tests. However, there may be multiple breakpoints in the relationship (Bai and Perron 2003; Wawro and Katznelson 2014). I thus execute Bai and Perron (2003) tests, which dynamically reuse estimated residuals to simultaneously test for the existence of multiple break points.[1]

The unidimensional measures show single breakpoints between 1992 and 1993 (SPL and Expanded SPL) and between 1996 and 1997 (Substantive measures). Models of multiple breakpoints yield at least on additional breakpoint in the 2000s for each unidimensional measure.

For the issue-specific policy measures, there is a greater, albeit still moderate, amount of variation in the timing of structural breaks in the party-policy relationship. The earliest single breakpoint is between 1983 and 1984 for public sector labor policy, while the latest is marijuana policy between 2001 and 2002. Models of multiple breaks show shifts as early as 1978 (civil rights and liberties) and as late as 2007 (immigration policy and tax policy).

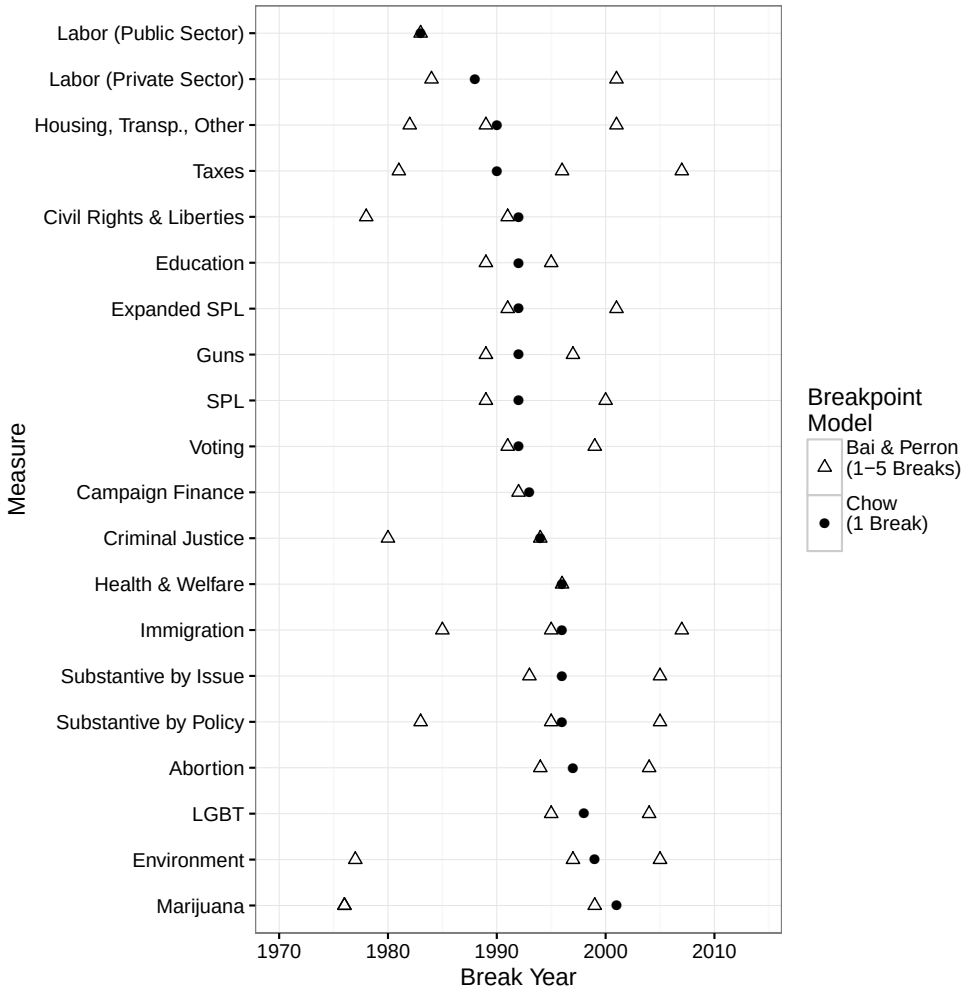
There are benefits and drawbacks to using 1993-1994 as an alternative temporal break. The time periods are more equal in length, aiding precision. Scholars also point to the Republican wave election in Congress and state governments in 1994 as a turning point in partisan polarization (e.g., Lee 2009; Mann and Ornstein 2013). However, the *policy* results of increased partisanship and polarized agendas may take time to appear as policy demander groups and legislative coalitions coalesce.

Figure A.6 provides estimates of the party-policy relationship for the 1970-1993 period and the 1994-2014 period.[2] Policy polarization across issue areas using a 1993-1994 temporal break are overwhelmingly consistent with those presented earlier. There is a slightly smaller growth of the party effect for civil rights and environmental policy than in the analyses of the pre- and post-2000 periods.

[1] I follow the convention of specifying that each broken segment of data must comprise at least 15% of the data, which yield at a maximum five inner temporal breaks. Due to the high precision of the breakpoint estimates (all with F-statistics of 12 or larger), Figure A.5 omits confidence intervals.

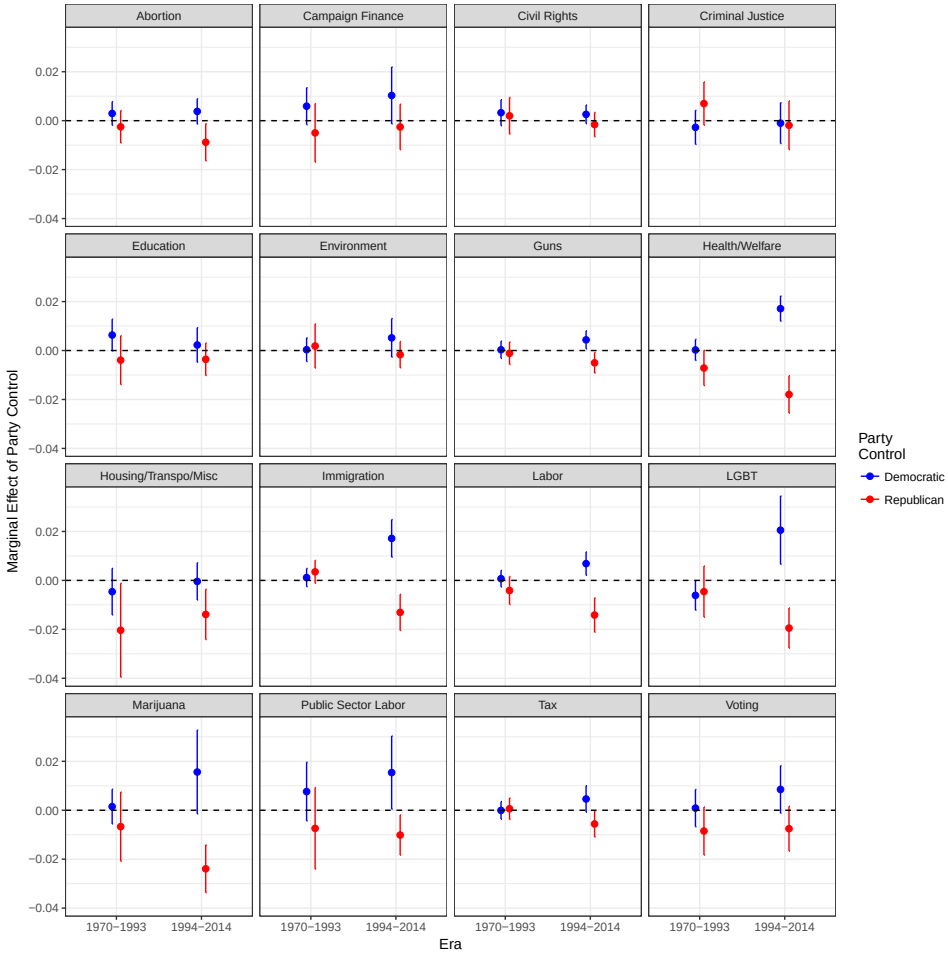
[2] Model specifications are analogous to those used throughout the paper.

Figure A.5: Temporal Breaks in the Party-Policy Relationship



Note: **Plot shows years of structural breaks in the party-policy relationship** across unidimensional and issue area policy measures (using two-way fixed effects models). Solid circles represent the most likely single break year. Open triangles represent the optimal break years in models allowing multiple structural breaks.

Figure A.6: Policy Polarization with 1993-1994 Temporal Break

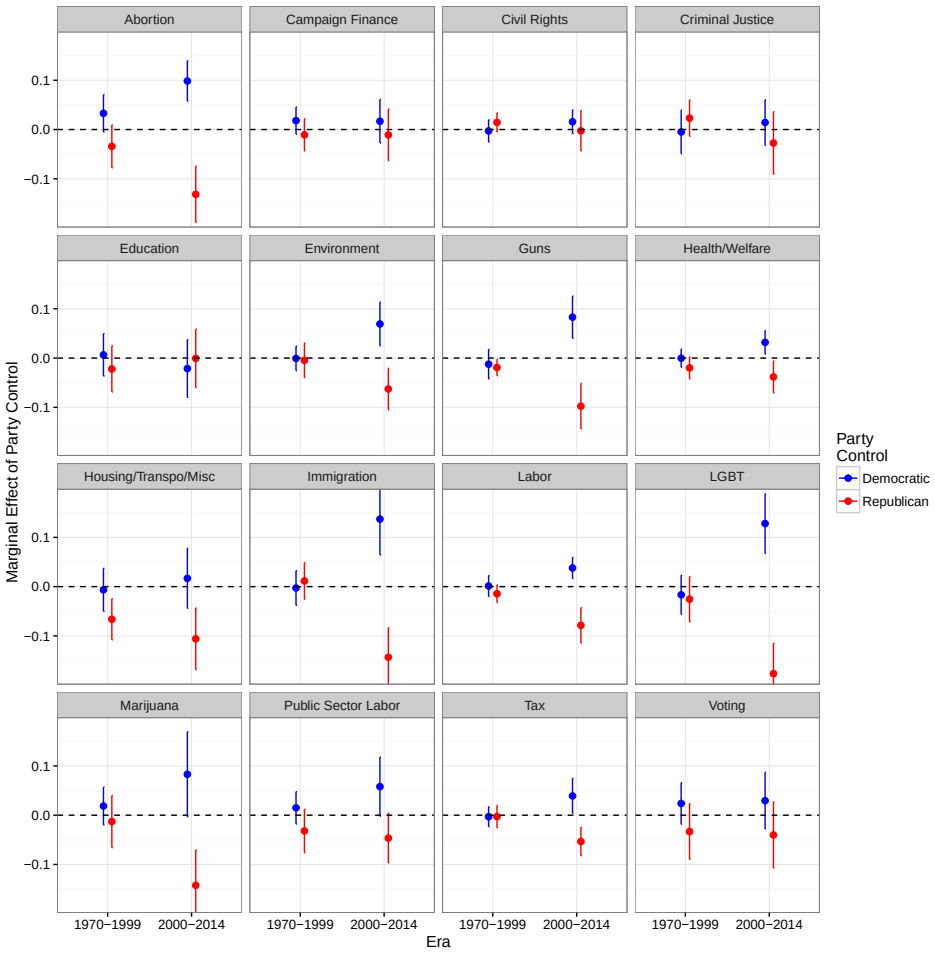


Note: Plots show the marginal effect of party control on policy outcomes across 16 policy issue areas. **Policy polarization across issue areas using a 1993-1994 temporal break are consistent with those presented earlier.** Estimates are derived from models that include state and year fixed effects and lagged dependent variables for years $t-1$ and $t-2$. Robust standard errors are clustered by state.

Alternative Regression Specifications

The dynamic panel regressions presented earlier use state and year fixed effects, and they include lagged dependent variables to control for past outcomes and improve model fit. More traditional time-series regressions do not include lagged dependent variables. As a robustness check I estimate traditional two-way fixed effects models for the issue area measures in Figure A.7.

Figure A.7: Party Effect on Issue Area Scales Using State & Year Fixed Effects



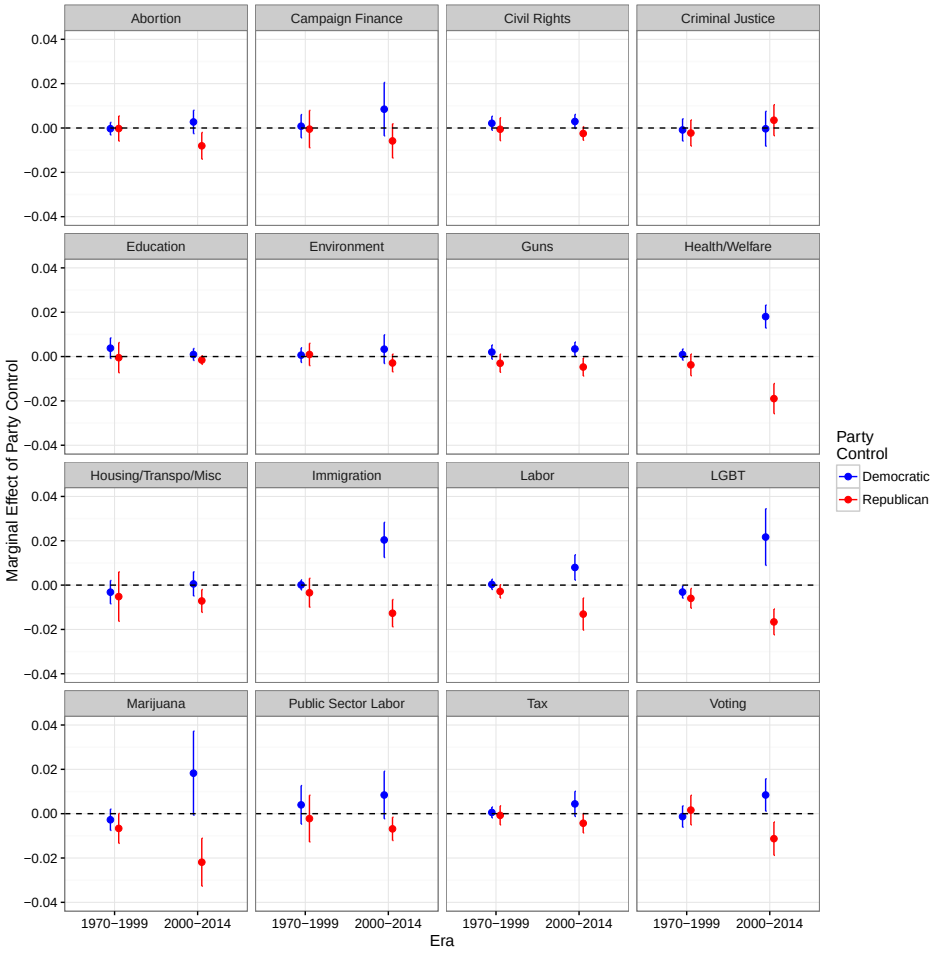
Note: **Plots show the marginal effect of party control on policy outcomes across 16 policy issue areas.** Estimates are derived from models that include state and year fixed effects. Robust standard errors are clustered by state.

The results are substantively identical to those presented earlier, but, as expected, the magnitude of the marginal effects is substantially larger in these fixed effects models—in some cases by an order of magnitude or more.

It is well known that spurious relationships may appear in regressions of non-stationary time-series data. However, taking the first-difference induces stationarity for variables of order one (e.g., Granger and Newbold 1974). In turn, ideal points for measure m, state s, and year t, θ_{mst}, are transformed to $\Delta_{mst} = \theta_{mst} - \theta_{mst-1}$, the change in the ideal point between year $t-1$ and year t.

The resulting estimates of policy polarization are substantively identical to those of the dynamic panel regressions presented earlier, and increase confidence that the relationships uncovered in this study are driven by non-stationarity.

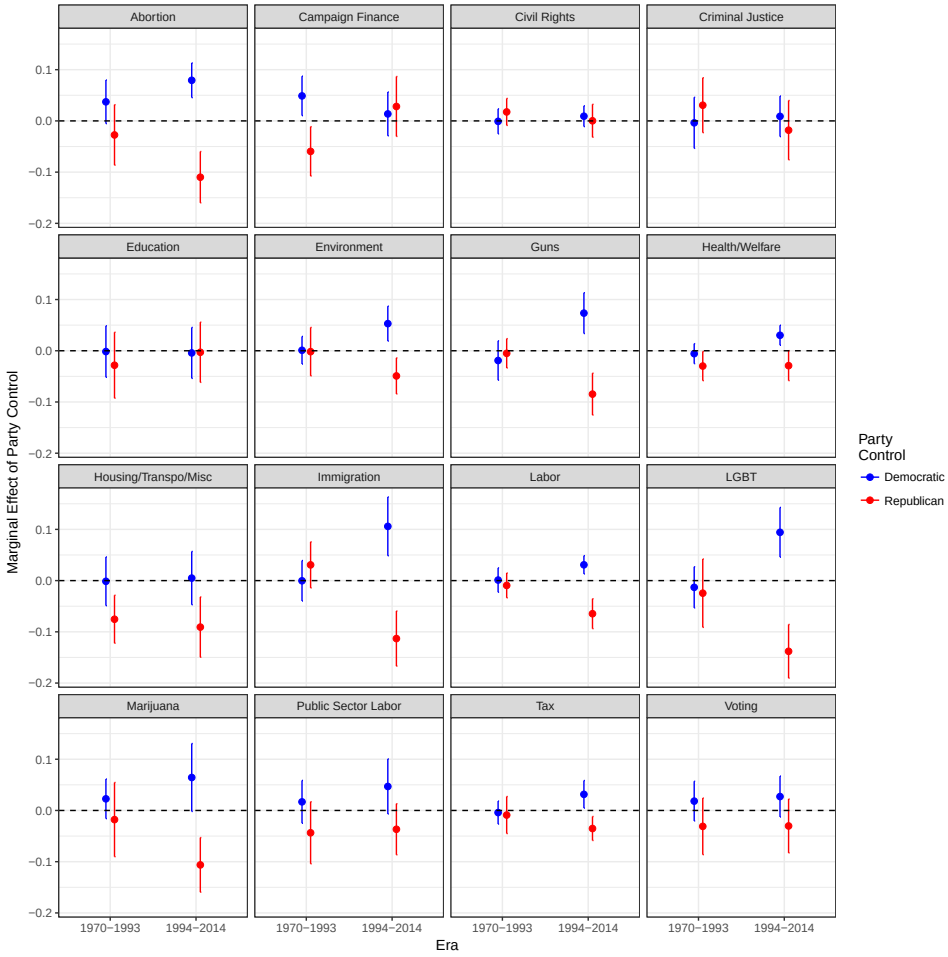
Figure A.8: Party Effect on Issue Area Scales Using First-Difference DV



Note: **Plots show the marginal effect of party control on policy outcomes across 16 policy issue areas** using first-differenced dependent variables. Results are substantively identical to those of from other model specifications. Models include year fixed effects. Robust standard errors are clustered by state.

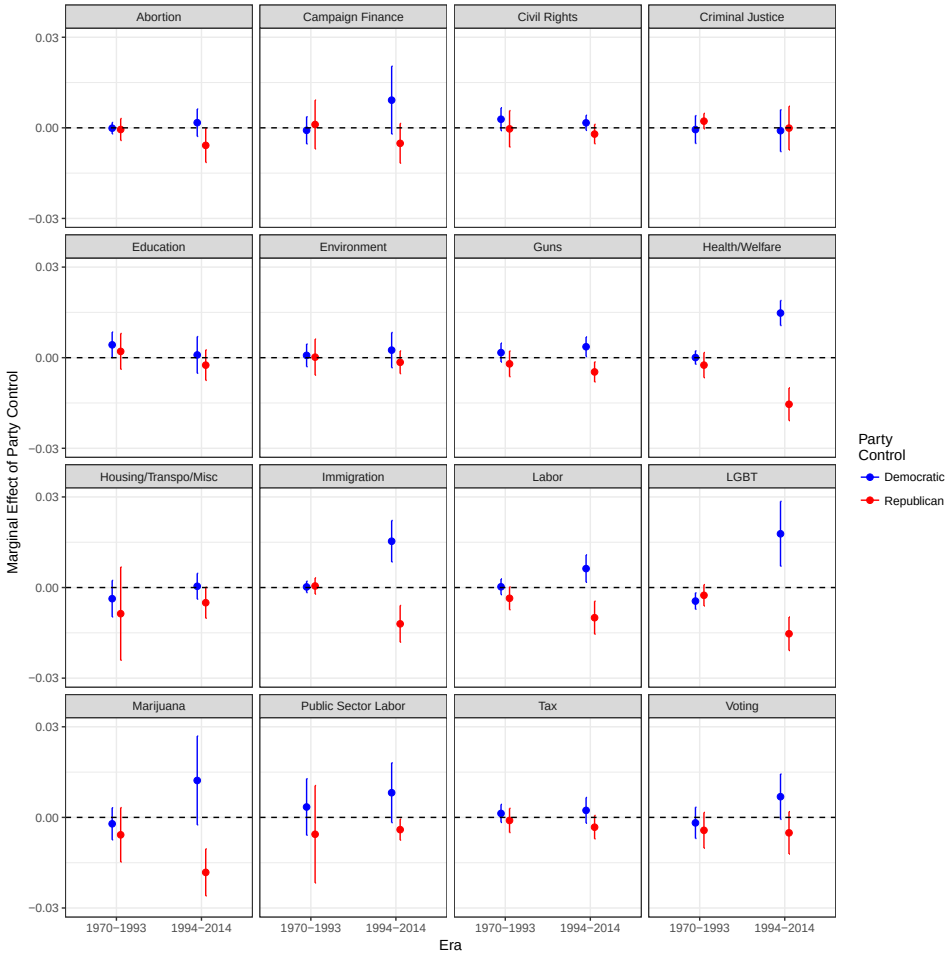
I also fit these alternative model specifications using the alternative temporal break of 1993-1994 in Figures A.9 and A.10.

Figure A.9: State & Year Fixed Effects with 1993-1994 Temporal Break



Note: **Plots show the marginal effect of party control on policy outcomes across 16 policy issue areas for the 1970-1993 and 1994-2014 periods.** Estimates are derived from models that include state and year fixed effects. Robust standard errors are clustered by state.

Figure A.10: First-Difference DV with 1993-1994 Temporal Break

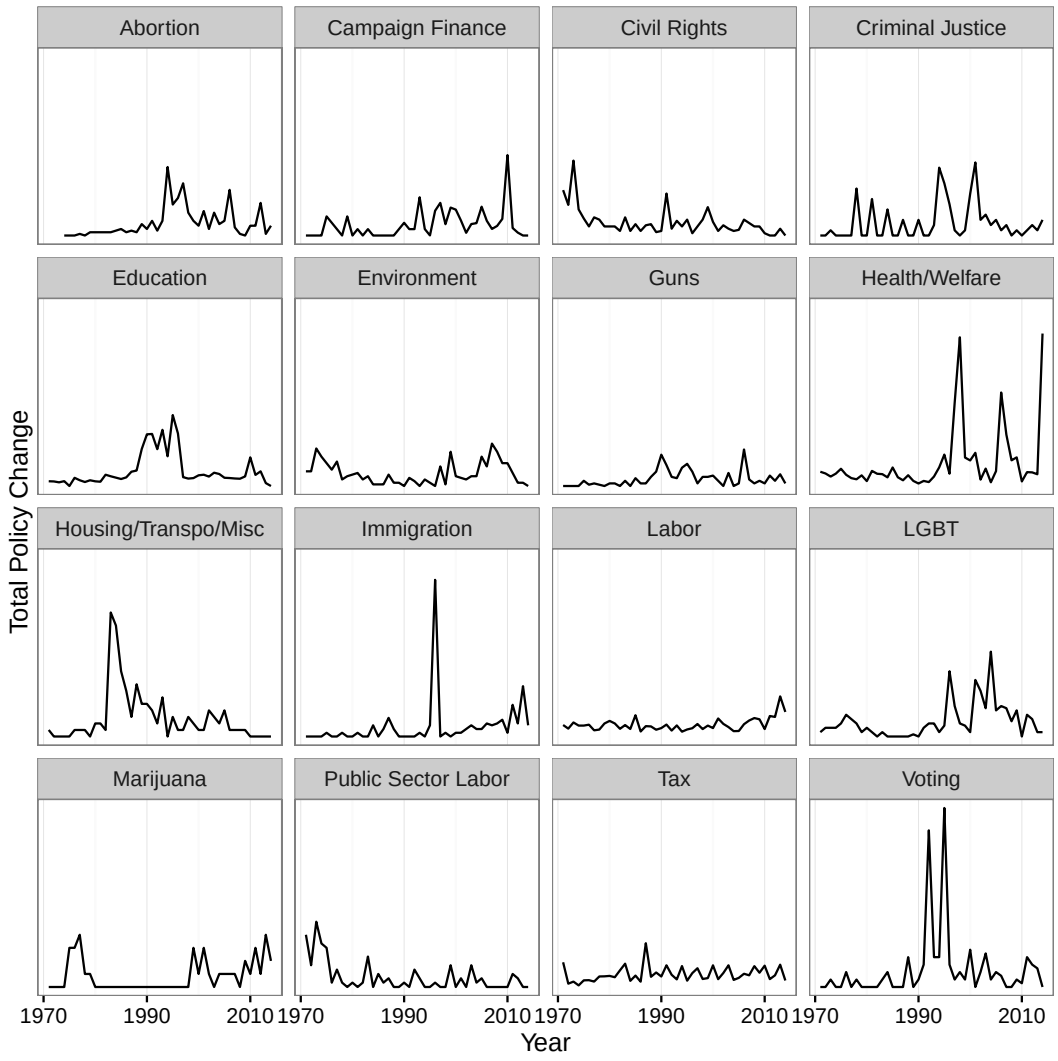


Note: **Plots show the marginal effect of party control on policy outcomes across 16 policy issue areas for the 1970-1993 and 1994-2014 periods** using first-differenced dependent variables. Models include year fixed effects. Robust standard errors are clustered by state.

Policy Productivity

Whether an issue area is polarized is also determined by overall activity in the area. Policies in some issue areas reached many state agendas in early decades, and then were rarely brought up in recent years; policies in other areas were dormant, only to show activity after 2000. Figure A.11 plots the aggregate sum of state changes in each issue scale by year.

Figure A.11: Total Policy Change by Issue



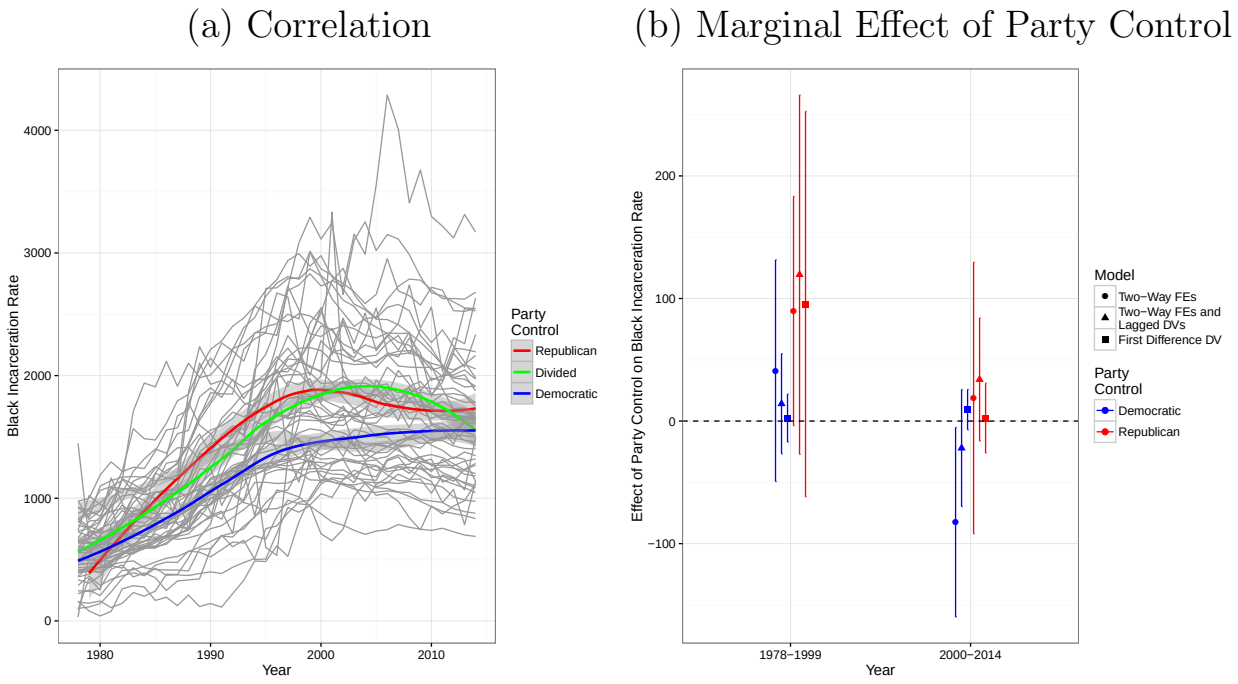
Note: Plots show the total amount (absolute value) of change on the issue area scales by year.

Some policy areas show less activity over time, such as civil rights and liberties. Other areas show increased activity, such LGBT rights and health and welfare. Importantly, there are also certain years of especially frequent policy change. This is often driven by federal

action. Policy changes in health and welfare peak in the mid-1990s upon the devolution of AFDC to state-run TANF programs and the creation of SCHIP; a second peak occurs in 2014 as states expand Medicaid and create state-run insurance exchanges under the ACA. 1996 federal welfare reform also restricted public benefits for newly arrived immigrants; many states, in turn, created their own state-funded programs for new immigrants.

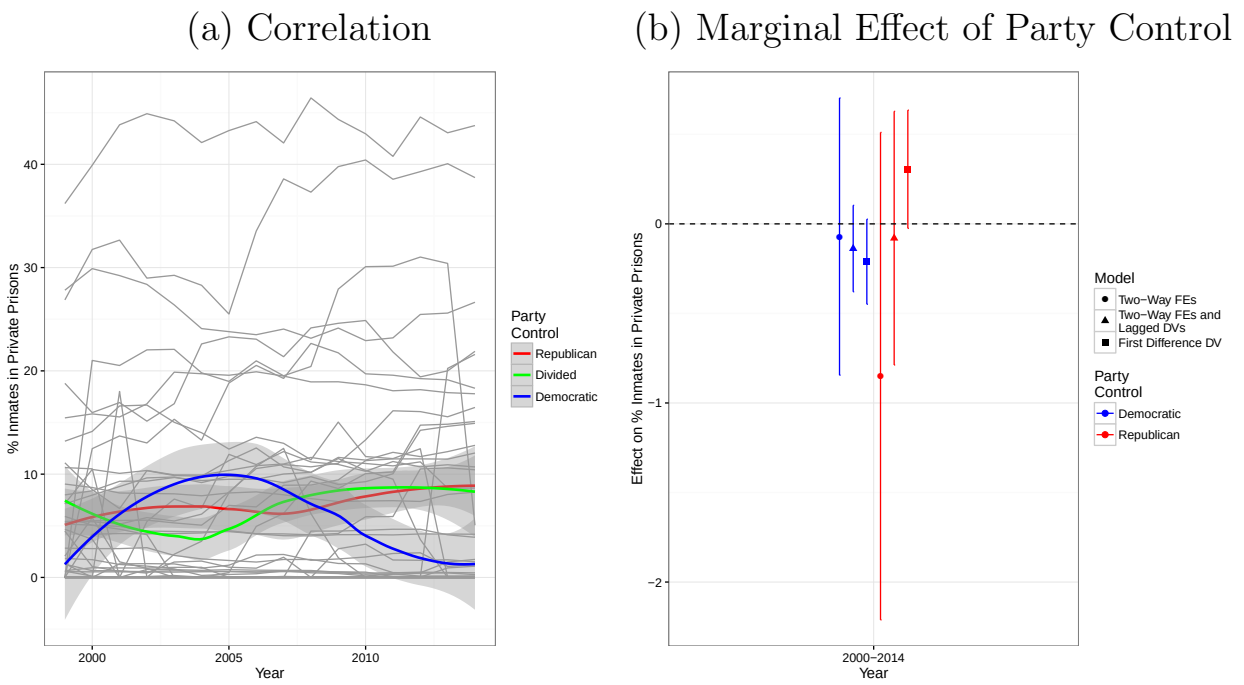
Party Control and Criminal Justice Outcomes

Figure A.12: Party Control and Black Incarceration



Note: **Incarceration of blacks *does not* polarize by party over time**. Plot (a) shows the average incarceration rate per 100,000 residents by state party control over time (using loess). Plot (b) shows the marginal effect of unified Republican control on the incarceration rate for the 1978 to 1999 period and the 2000 to 2012 period across three time-series model specifications. Models control for the crime rate at year $t-1$ (see Yates and Fording 2005).

Figure A.13: Party Control and Percent Inmates in Private Prisons

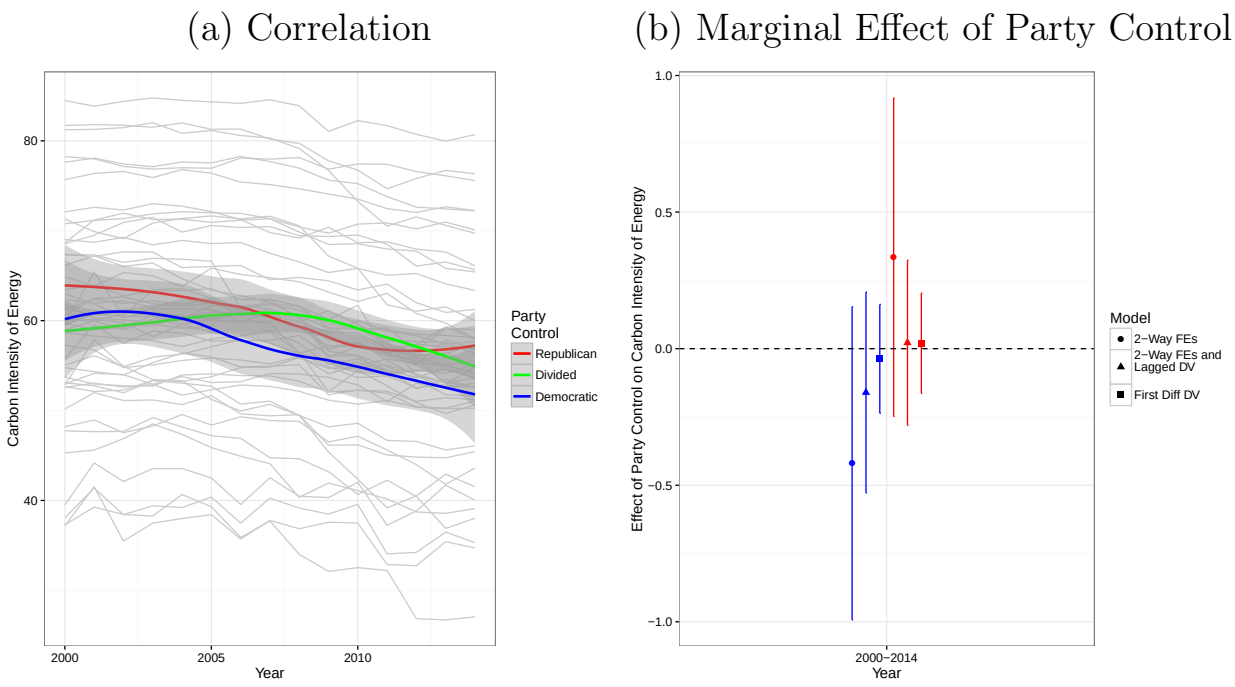


Note: **There is modest evidence of a relationship between party control and use of private prisons.** First difference models show a significant relationship between party control and change in the percentage of inmates housed in private facilities.

Party Control and Environmental Outcomes

I estimate the relationship between party control and greenhouse gas emissions using a measure of the carbon intensity of the energy supply, a rate of emissions per unit of energy utilized in the state. Because state economies vary greatly in concentration of intensive industry, there exist large differences across states in overall emissions and energy use. Using carbon intensity of energy consumption helps to avoid such confounders. The carbon data is from the U.S. Energy Information Administration, and is only available beginning in 2000.

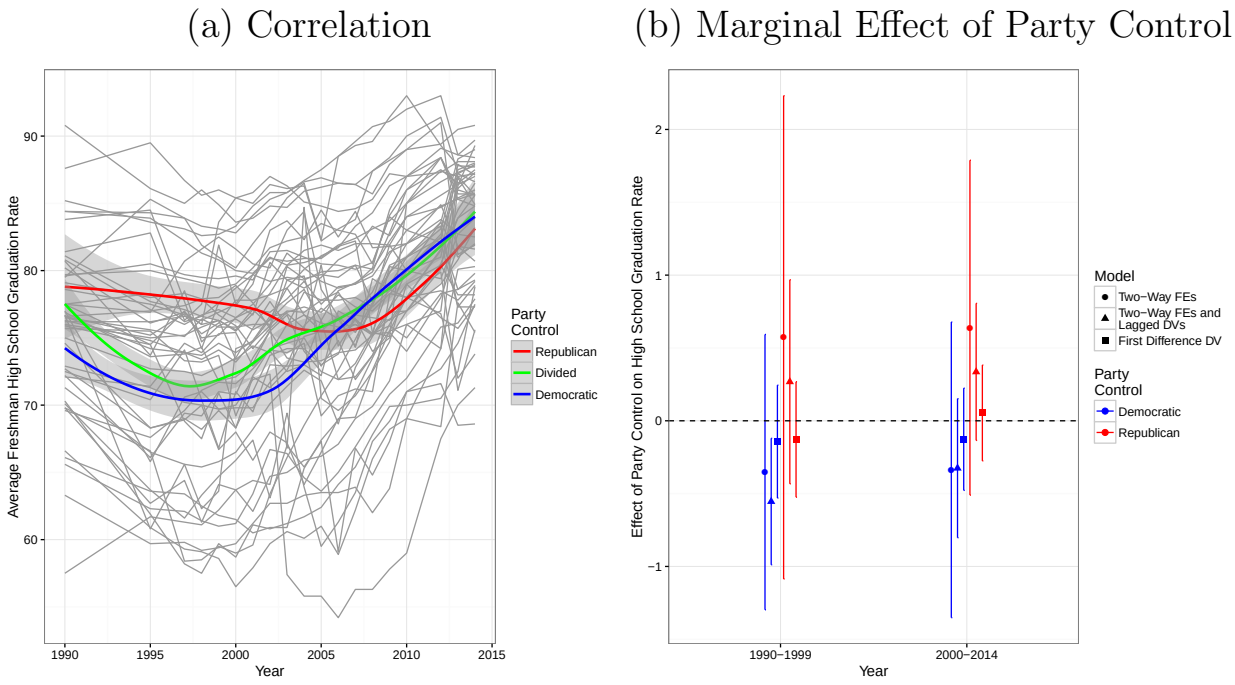
Figure A.14: Party Control and Carbon Intensity of Energy Supply



Note: **Party control modestly predicts carbon intensity of a state's energy supply**. Republican control is associated with greater carbon intensity than Democratic control in the two-way fixed effect model ($p < 0.01$) and the two-way fixed effect with lagged dependent variable model ($p < 0.15$).

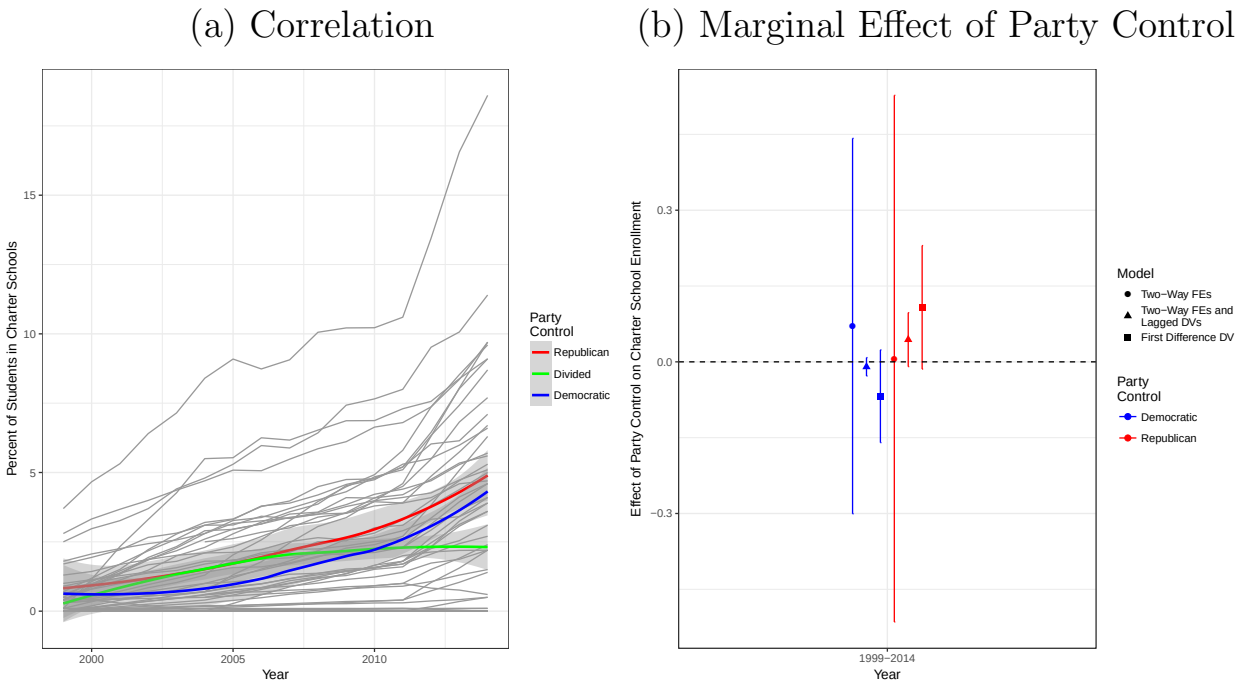
Party Control and Education Outcomes

Figure A.15: Party Control and High School Graduation Rates



Note: **The effect of party control on graduation rates does not change over time.** In both time periods, Republican control is associated with slightly higher graduation rates in the dynamic panel and first difference models.

Figure A.16: Party Control and Charter School Enrollment



Note: **Party control modestly predicts charter school enrollment.** Republican control is associated with slightly larger proportions of students in charter schools. The effect size is small (an interparty difference of only 0.175 percentage points in the first difference model).

Appendix B

Chapter 3 Supplement

Alternative Regression Specifications

Figure B.1 shows analogous estimates of the effect of party control using traditional two-way fixed effects regression specifications (state and year fixed effects, with robust standard errors clustered at the state level). The estimates are much larger than those produced from the dynamic panel and first difference models shown in Figure A.4, by as much as an order of magnitude.

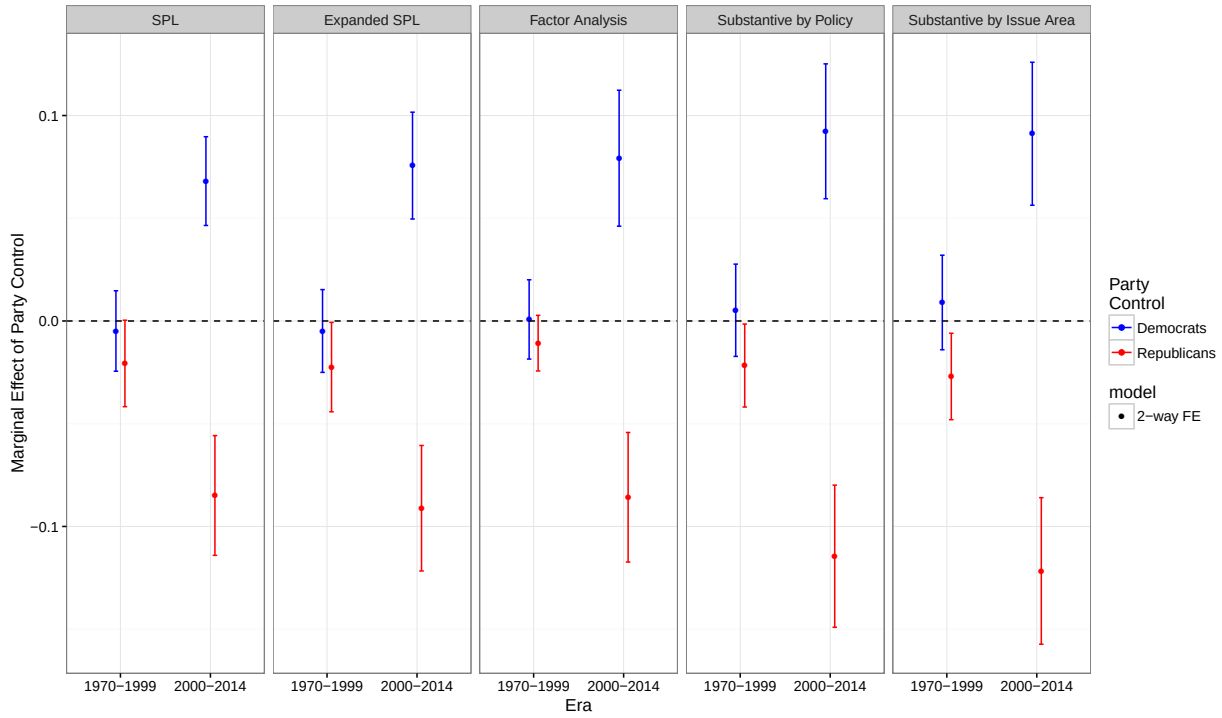
As in the models presented earlier, the Substantive measures show larger party effects for Republicans. However, the SPL and Expanded SPL estimates are more similar than those shown in Figure A.4.

Data Coverage

One concern for estimating temporal dynamics is inconsistent coverage in the data over time. As shown in Figure B.2, there are more state-policy observations in the SPL dataset as time progresses (from a nadir of 2343 observations in 1956 to a peak of 3799 observations in 2009). It is possible, in turn, that the measure is noisier in the periods with fewer policy observations, but shifts in policy coverage could also reflect real changes in American federalism. It is likely the case that the true population of major state level policies is smaller in the early postwar period, as Congress and the Supreme Court superseded many state laws, than in later years.

However, the relationship between the number of state-policy observations in the SPL data and variance in the posterior distribution of state ideal points is unexpectedly *positive* (with a correlation coefficient of 0.21). Ideal point estimates are slightly more uncertain as the number of policy items in the data increases. This may appear strange, but it is likely because there are a greater number of policies that divide states closer to the middle of the ideal point distribution in the earlier periods. Martin and Quinn (2002, 145) similarly estimate the ideal points of moderate justices with more precision in their dynamic Bayesian IRT model of judicial ideal points. Again, this could reflect historical changes: In recent

Figure B.1: Effect of Party Control Pre-2000 and Post-2000 (2-Way FEs)



Note: Marginal effect estimates are derived from models that include state and year fixed effects. Standard errors are clustered by state.

decades, states may be more likely to innovate new policies that are only adopted by a few states and thus provide less information per policy than older ones adopted by about half of the states.

Simulations with Original Policy Sample

In this section, I execute analogous Monte Carlo simulations as described earlier, but this time using only the sample of policy items provided by Caughey and Warshaw (2016) to simulate 10,000 corresponding additive indices. In Figure B.3, the vertical lines represent the marginal effect estimates from the original SPL measure.

For the 1970-1999 period, The Bayesian IRT measure (this time the original SPL measure) again produces effect estimates much greater than virtually all of the estimates produced from the simulated Substantive measures. The SPL effects for the 2000-2014 period are also of greater magnitude than those produced with the simulated indices, and quite symmetric. The same cannot be said, however, about the *difference* between the 1970-1999 and 2000-2014 estimates; compared to the simulated measures, the SPL measure appears to slightly

Figure B.2: Data Support Across Years

Note:

overstate the difference for Democratic control and understate it for Republican control.

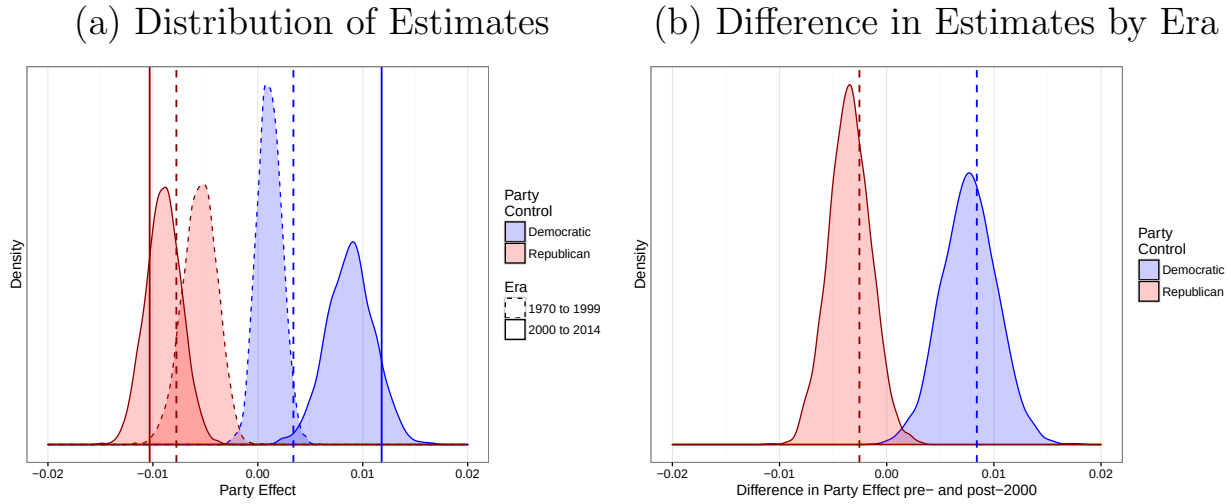
Factor Analysis Eigenvalues

Cronbach's α is estimated to be 0.924 for the matrix of policy data, suggesting quite high interitem reliability. Note, however, that Cronbach's α is an increasing function of the number of items, so the large size of the α statistic is partly due to the large number of policy items in the data.

Relationship to SPL Scale

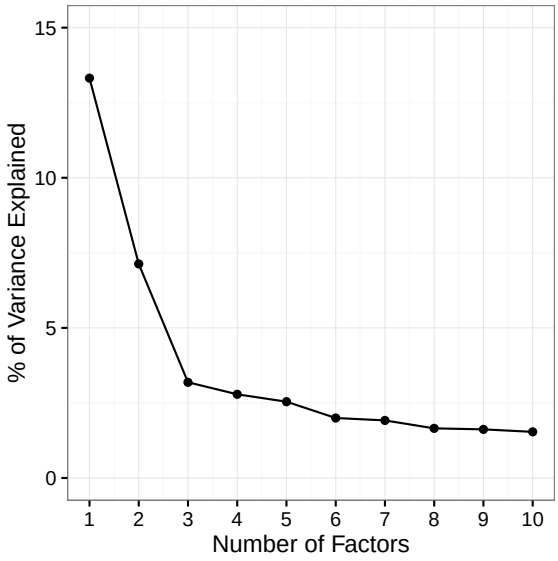
Figure B.5 plots the correlation between SPL ideal points and each of the four alternative ideal point measures. As expected, the Expanded SPL measure always receives the highest correlation. With the exception of the Substantive measures in the very early 1970s,

Figure B.3: Party Effects Across Many Indices



Note: **Plots show estimates of the change in marginal effect of party control between the pre-2000 period and the post-2000 period.** The left panel uses only data from Caughey and Warshaw (2016) and the right panel uses the expanded policy dataset to construct 10,000 policy scales generated with random weights for each policy (with a maximum ratio of weights of 10 to 1). Dashed vertical lines show estimates using the SPL scale for comparison. Estimates are derived from models that include state and year fixed effects, state × era fixed effects, and lagged dependent variables for years $t-1$ and $t-2$.

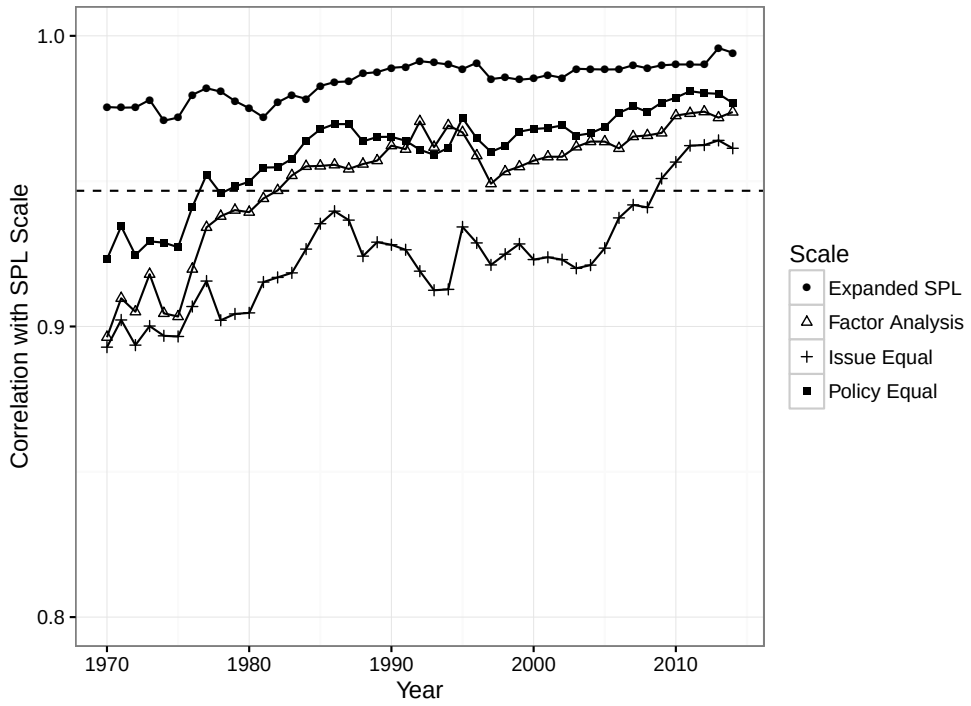
Figure B.4: Variance Explained



Note: Plot shows the percent of variance explained by the inclusion of each additional factor.

the yearly correlation is always greater than 0.9. The average yearly correlation is 0.95, represented by the dotted horizontal line.

Figure B.5: Correlation of Substantive Scales with SPL Scale

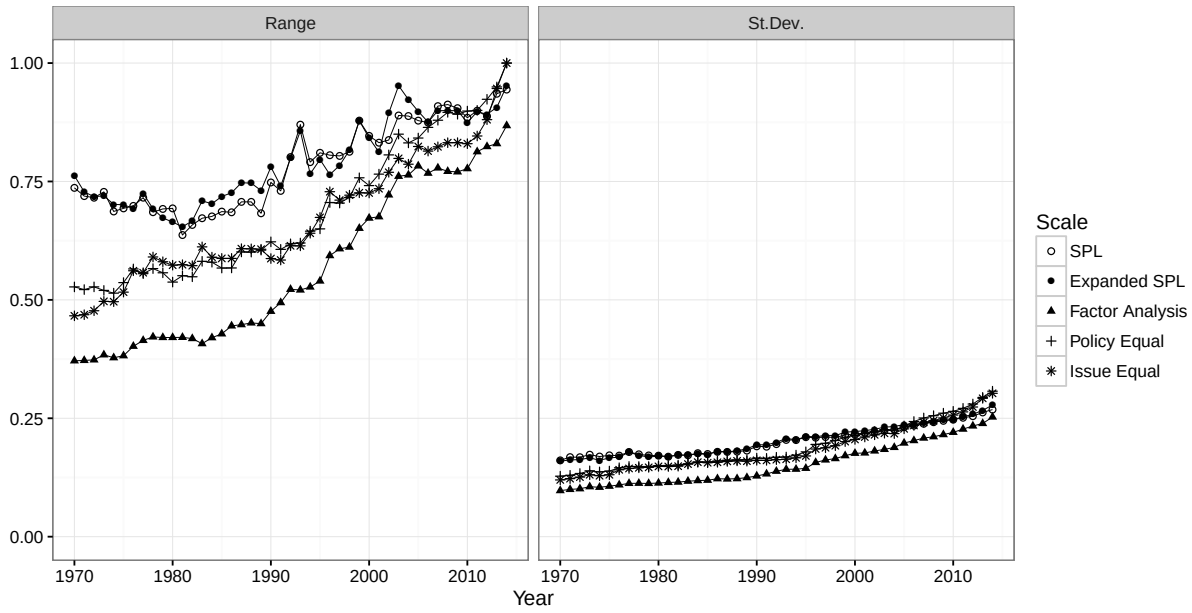


Note: Plot shows the correlation between substantive scales and the SPL scale by year. The average yearly correlation is 0.95, represented by the dashed horizontal line.

The left panel of Figure B.6 shows the range of ideal points by year for each of the five measures. Substantively, the range represents the distance between the most liberal and most conservative state in each year. This statistic varies based on the measure. The Substantive measures suggests that this distance has approximately doubled between 1970 and 2014, while the SPL and Expanded SPL measures suggest that it has increased by about a third. The Factor Analysis measure shows an increase of approximately 0.5 units, identical to the Substantive measures, but this represents greater proportional growth given the measure's smaller range in 1970.

The variance of the measures is also of interest. All else equal, increasing the spread of the distribution of y will increase the correlation between an x and y. I check to ensure that the differences in correlations between the scales that I describe in this article are not an artefact of large differences in the scales' variance estimates. The right panel of Figure B.6 suggests that they are not; the standard deviation and its time trends are similar across the scales.

Figure B.6: Range and Standard Deviation of Ideal Points



Appendix C

Chapter 4 Supplement

Table C.1: Party as Mediator for Public Opinion

	(1)	(2)	(3)	(4)	(5)
Unified Republican t-1	0.0495***	0.0121***	0.0018**	0.00089	0.00147
(Mediator for public opinion)	(0.0405, 0.0594)	(0.0088, 0.0155)	(0.0002, 0.0041)	(-0.00008, 0.0024)	(-0.00006, 0.0034)
Public Opinion t-1	0.0747***	0.0309***	-7.82e-05	0.0120	-4.21e-03
(Direct effect; 10-point shift)	(0.0478, 0.1026)	(0.0150, 0.0442)	(-0.0271, 0.0325)	(-0.0088, 0.0317)	(-0.0359, 0.0370)
Total Effect	0.1242***	0.0430***	0.0017	0.0128	-0.0028
	(0.0946, 0.1495)	(0.0254, 0.0580)	(-0.0244, 0.0333)	(-0.0080, 0.0342)	(-0.0344, 0.0378e)
Proportion Mediated	0.4035***	0.2742***	0.0363	0.0685	-0.0209
	(0.3063, 0.5294)	(0.2035, 0.4182)	(-0.999, 0.773)	(-0.194, 0.436)	(-1.02, 1.66)
Policy area FEs	x	x	x	x	x
State FEs			x	x	x
Year FEs					x
Lagged DV		x		x	
N	2497	2497	2497	2497	2448

Quasi-Bayesian 95% CIs in parentheses. Standard errors are clustered by state.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure C.1: MRP & CCES SCHIP Support

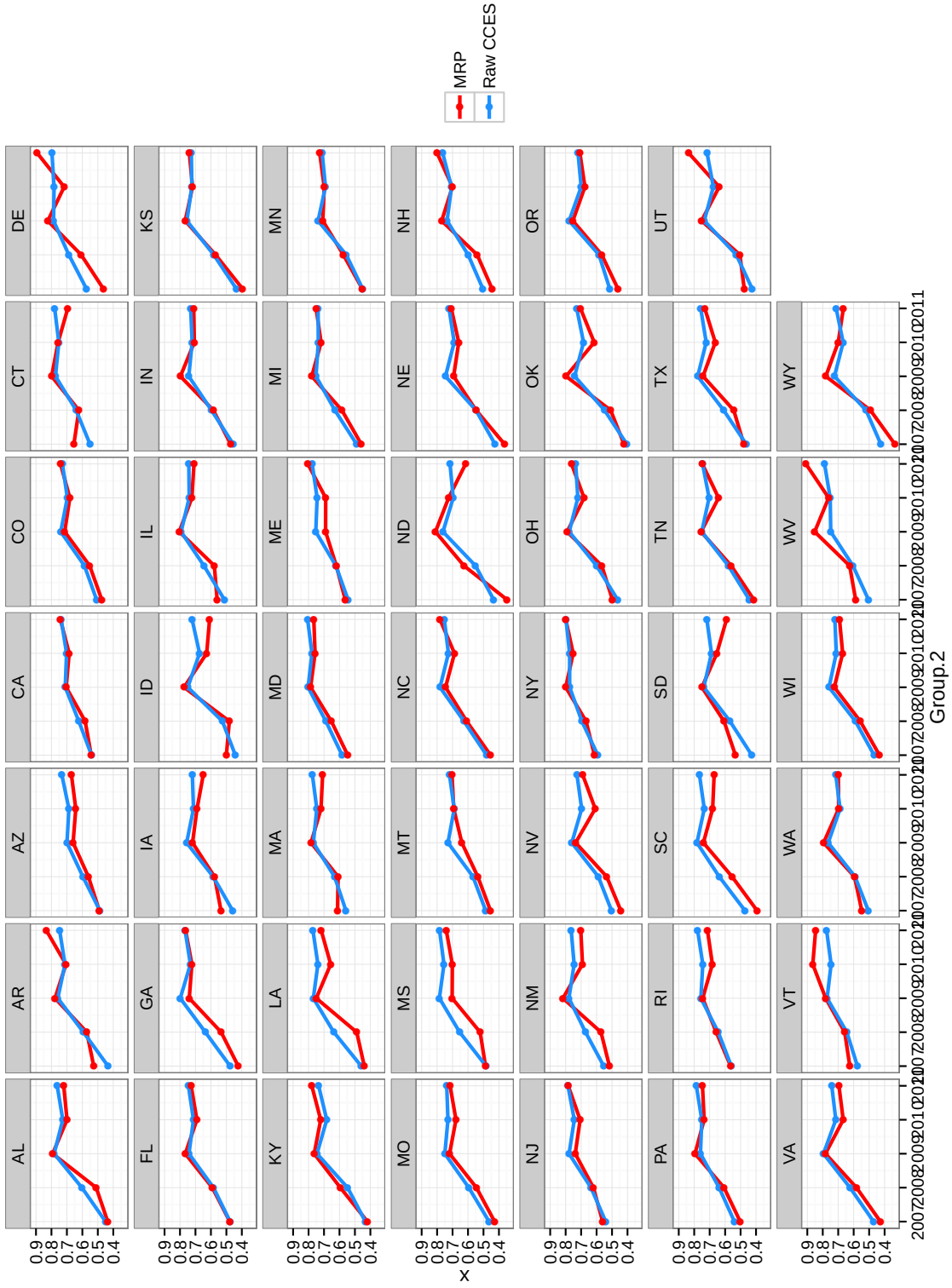


Table C.2: Logit Models: Policy Responsiveness to Party and Public Opinion

	(1)	(2)	(3)	(4)	(5)
Opinion t-1	0.0405**	0.0325***	0.00180	0.0142	-0.00456
(100-point scale)	(0.0134)	(0.00917)	(0.0142)	(0.0114)	(0.0169)
Unified Republican t-1	1.336***	0.816***	0.480	0.492*	0.240
	(0.242)	(0.162)	(0.251)	(0.214)	(0.268)
Unified Democratic t-1	-0.307	-0.238	0.0167	-0.124	0.111
	(0.174)	(0.128)	(0.164)	(0.135)	(0.181)
Policy area FEs	x	x	x	x	x
State FEs			x	x	x
Year FEs					x
Lagged DV		x		x	
N	2497	2497	2497	2497	2448
Pseudo R^2	0.1980	0.5585	0.3271	0.5807	0.3584

Robust standard errors in parentheses. Standard errors are clustered by state.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.3: Policy Responsiveness to Party and Public Mood from Enns and Koch (2013)

	(1)	(2)	(3)	(4)	(5)
Public Mood t-1	0.0917***	0.0485***	0.0328**	0.00752	0.0290
100-point scale	(0.0174)	(0.0117)	(0.0111)	(0.00772)	(0.0233)
Unified Republican t-1	1.005***	0.668***	0.370**	0.347***	0.313**
	(0.195)	(0.118)	(0.124)	(0.0996)	(0.117)
Unified Democratic t-1	-0.289	-0.196	-0.179	-0.120	-0.108
	(0.151)	(0.108)	(0.0953)	(0.0700)	(0.0948)
Policy area FEs	x	x	x	x	x
State FEs			x	x	x
Year FEs					x
Lagged DV		x		x	
N	5139	4991	5139	4991	5139
Pseudo R^2	0.2441	0.5274	0.3495	0.5545	0.3657

Robust standard errors in parentheses. Standard errors are clustered by state.

Public liberalism is recoded to correspond to liberal policies.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

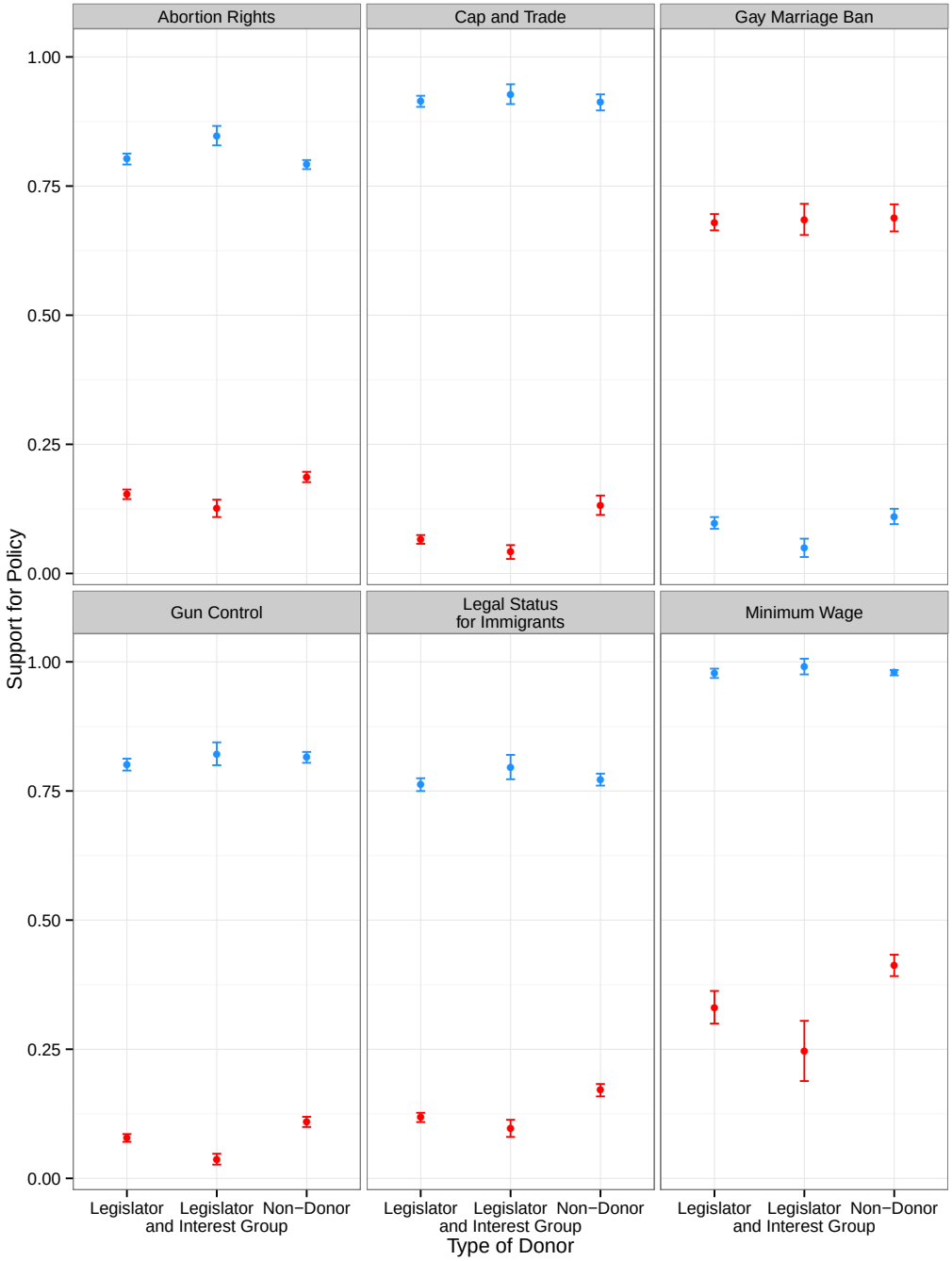
Appendix D

Chapter 5 Supplement

Donor Policy Attitudes

Figure D.1 shows the predicted probability of support for specific policies among different types of individual donors (from logit models using CCES data). Group activist donors have more consistently extreme (partisan) policy attitudes than non-donors and those who only donate to candidates. Models control for age, education, gender, income, and race. Minimum wage support is so high among Democrats that a linear probability model is necessary in order to estimate confidence intervals. The predicted probabilities from this linear probability model are virtually identical to those generated by the analogous logit model.

Figure D.1: Donor Policy Attitudes



Donor Attitudes in ANES

The ANES asks respondents whether they donated to various entities: a candidate, a party committee, or a "group that supported or opposed candidates" (an interest group). The results of analogous regressions using the ANES data are presented in Appendix Table D.1. The data covers a longer time period and the question wordings differ from those in the CCES, but results are a starker version of the pattern reported in Figure 5.3. On the 7-point ideology scale, Democrats who give to candidates and interest groups are about 0.36 units more liberal than candidate-only donors and 0.80 units more liberal than non-donors; Republicans are about 0.12 units and 0.29 units more conservative, respectively. However, in the ANES data, IGA donors' extremism is driven by the coefficient for group donor, not the interaction of group donor and candidate donor (which was significant in the CCES model). In other words, the ANES data suggest that those who donate to interest groups do not differ from those who donate to both interest groups *and* legislators.

Regression Tables

Table D.1: Donor Ideology (ANES)

	(1) Democrats	(2) Republicans
Candidate Donor	-0.460	0.168
	(0.0738)	(0.0727)
Group Donor	-0.291	0.294
	(0.0834)	(0.0723)
Candidate * Group Donor	-0.0678	-0.175
	(0.138)	(0.132)
Party Donor	-0.266	0.165
	(0.0941)	(0.0776)
Candidate * Party Donor	0.287	0.0628
	(0.130)	(0.118)
Controls		
Race	x	x
Gender	x	x
Income	x	x
Age	x	x
Education	x	x
Constant	3.635	4.521
	(0.0638)	(0.0683)
N	8363	6861
R^2	0.098	0.056

Higher values denote greater conservatism. Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.2: Donor Ideology

	(1) 7-point Ideology (Democrats)	(2) 7-point Ideology (Republicans)
Legislative Donor	-0.0624	0.135***
	(0.0338)	(0.0238)
Interest Group Donor	-0.211***	0.0931**
	(0.0484)	(0.0325)
Legislative* Group Donor	-0.0519	0.0325
	(0.0629)	(0.0428)
Party Committee Donor	-0.183***	0.0720**
	(0.0335)	(0.0259)
Legislative* Party Donor	0.0293	-0.0521
	(0.0468)	(0.0347)
Controls		
Race	x	x
Gender	x	x
Income	x	x
Age	x	x
Education	x	x
Constant	3.251***	5.790***
	(0.155)	(0.102)
N	11579	10962
R^2	0.0884	0.0223

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.3: Contacting Legislators

	(1) Contacted Leg. (Democrats)	(2) Contacted Leg. (Republicans)
Legislative Donor	0.877***	0.692***
	(0.0902)	(0.103)
Interest Group Donor	0.569***	0.389***
	(0.105)	(0.117)
Legislative* Group Donor	0.149	0.230
	(0.180)	(0.208)
Party Committee Donor	0.281***	0.298***
	(0.0848)	(0.0805)
Legislative* Party Donor	-0.121	-0.0316
	(0.146)	(0.151)
Controls		
Race	x	x
Gender	x	x
Income	x	x
Age	x	x
Education	x	x
Constant	-0.838***	-0.393
	(0.234)	(0.267)
N	5075	3821
Pseudo R^2	0.0811	0.0358

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$