Does Transparency Inhibit Political Compromise?*

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Abstract

Politicians and scholars contend that governmental transparency reforms constrain politicians' capacity to negotiate and compromise in the pursuit of policy goals. However, existing research primarily emphasizes only that governments are strategic in adopting these reforms; whether lawmakers actually incur the alleged costs of transparency remains an open question. We investigate this issue in the context of American state legislatures, many of which have become exempt from "sunshine laws" in recent decades. Legislators justify these exemptions by claiming that transparency impedes deal-making and coalition-building, producing gridlock. We leverage variation in the timing of sunshine law adoptions and exemptions to identify their effect on legislative productivity, polarization, partisanship, policy change, and budget delay. Our analyses refute legislators' argument for opacity; we report precisely-estimated negligible and contradictory effects of sunshine law exposure. We conclude that transparency does not inhibit political compromise. Legislative deliberation is equally or perhaps more effective under open governance requirements.

Keywords: Transparency; Sunshine laws; Legislative decisionmaking; Legislative performance; Representation

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"Publicity is justly commended as a remedy for social and industrial disease. Sunlight is said to be the best of disinfectants..."

—Louis Brandeis, United States Supreme Court Justice, 1933.

"Just as important as transparency is the ability of lawmakers to effectively work on behalf of those who sent us here."

—Mark Schoesler, Washington State Senate Minority Leader, 2018.

1 Introduction

In March 2014 the Joint Committee on State Administration and Regulatory Oversight introduced House Bill 3945 to the Massachusetts General Court (state legislature). The bill sought to improve transparency in the operation of state government, including the requirement that the legislature end its own exemption from the state's "open meetings" laws. It ultimately failed to pass, and a key argument made by its opponents was that closed-door meetings allow for dialogue and negotiation that might not occur under public scrutiny (Diana 2014). This perspective is quite common among political leaders around the world (see Berliner 2014). Political elites are generally reticent to accept transparency, in part, due to concern that it might limit their discretion to occasionally move away from their ideal positions on issues to facilitate their policymaking obligation. However, to date no research has directly assessed whether that concern holds empirical support. Are politicians actually constrained in their capacity to politick, negotiate, and compromise with one another under transparency requirements? In this research, we answer that question in the context of American state legislatures.

More specifically, we consider whether public access to the legislative process hinders several observable indicators of political compromise: productivity, polarization, partisanship, policy change, and budget delay. Efficient lawmaking typically requires compromise, and yet legislators operating under transparency may feel constrained from doing so if they believe constituents will react negatively to such behavior. Indeed, 21 American states currently exempt their legislatures from so-called "sunshine laws," citing facilitation of the legislative process (Diana 2014;

La Corte 2018) or "broader discretion [to claim] legislative privilege" (Marfin 2019) as justifications. We test this logic directly, examining whether governmental transparency and efficiency are mutually exclusive. The implications of this question for democratic politics are potentially quite dire. Transparency is supposed to foster accountability and reduce corruption, but if it cannot coexist with efficiency, governments may be forced to choose between productive deliberation via compromise out of the public eye and open, responsible governance.

We begin by developing a theoretical foundation for legislators' claims about the consequences of transparency that is rooted in a principal-agent dynamic. We derive several empirical predictions from this theory, then test them by leveraging temporal variation in the adoptions and legislative exemptions of states' sunshine laws. We employ novel data and multiple modeling strategies to identify the effects of sunshine law exposure on our myriad indicators of political compromise in state legislatures. Our results suggest that legislators' concern about the stifling consequences of transparency are largely overblown; we find support for precisely-estimated negligible or contradictory effects. Thus, we conclude that politicians do *not* lose their capacity to compromise under public scrutiny. Legislative deliberation functions about the same—or better—under open proceedings.

2   The Causes and Consequences of Open Government

Perhaps the most prominent line of inquiry on open governance focuses on how and why states and countries adopt public information provision institutions.[1] For example, political competition and power-sharing arrangements often lead governments to implement increased fiscal transparency (Alt, Lassen, and Rose 2006). Others suggest that uncertainty over future control of government is responsible for reform efforts, as current political actors strategically implement institutions that hold their future electoral competitors more accountable (Berliner 2014; Berliner and Erlich 2015). Indeed, transparency reforms are similar to other types of institutional change

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[1]Other important parts of this literature consider open government information (especially online) as a policy innovation (Tolbert, Mossberger, and McNeal 2008) as well as its effects on citizens' trust (Tolbert and Mossberger 2006), political attitudes and behavior (Piotrowski and Van Ryzin 2007; Berliner et al. 2018), and governmental responsiveness (Berliner et al. 2019).

in which those in power incur costs in the short-term with the prospect of potential long-term gains (Geddes 1994; Grzymała-Busse 2006). Elections matter for decisions on transparency as well; politicians' re-election motivation is at odds with requirements to provide complete information (Snider 2009). As such, electoral pressures often prevent governments from instituting transparency requirements. We directly address this process of selection into transparency in our empirical analyses described below.

Cross-national research has engaged directly with questions about the relationship between transparency and corruption. While increased transparency can lead to better *detection* of corruption, it may also help outsiders identify key decisionmakers with whom they can build stronger duplicitous *connections* (Bac 2001). This contention implies that the effects of transparency on combatting corruption may be countervailing (e.g., Costa 2012; Cordis and Warren 2014; Vadlamannati and Cooray 2017). In these studies the authors observe that freedom of information requirements actually drive up public perceptions of corruption. Importantly, this pattern emerges despite strong evidence that such requirements drastically reduce the *amount* of governmental corruption (Adsera, Boix, and Payne 2003; Lederman, Loayza, and Soares 2005; Lindstedt and Naurin 2010).

On its face, transparency in governmental proceedings appears normatively positive (Florini 2007). Yet it may also impose costs on elected officials by limiting their flexibility to pursue actions not easily explained to the public, such as compromising with political opponents. Public officials generally adopt this logic (see above), but it reflects conventional wisdom in scholarly accounts as well. For instance, Heald (2003) notes that "some transparency is needed to deter fraud and corruption…[but] too much leads to losses in effectiveness through…excessive politicization" (727). In a formal model of political blame, Groseclose and McCarty (2001) demonstrate that elites bargaining before an audience (the electorate) have incentives to send signals to that audience via their offers, which ultimately produces Pareto inefficient outcomes. They conclude that their results "make a mild case" in favor of closed-door negotiations (114). Gilmour (1995) echoes this sentiment: "negotiations conducted in private can be very useful in overcoming [stalemates], for

they allow negotiators to obscure the origins of necessary compromises" (12).

However, from an empirical standpoint the question of whether transparency actually inhibits political compromise largely remains unanswered. Addressing this gap is important because much of the research on the decision to adopt transparency reforms assumes that they exact costs on governments and policymakers. Yet the alternative—that such costs are not imposed—is at least reasonably plausible. Harden and Kirkland (2018) demonstrate that, even in the polarized political environment of the United States, a sizable minority of citizens actually prefers representatives who are open-minded and willing to consider the other side's perspective. Of course, there is also ample evidence that most citizens punish their legislators for compromising with the opposition (Harbridge and Malhotra 2011; Harden and Kirkland 2018). Accordingly, we adopt that latter perspective in developing our theoretical framework.

3   A Theoretical Case for Opacity

Our theory draws on insights from political economic models of decisionmaking to explain how politicians view transparency. Specifically, we consider the relationship between citizens and representatives as a principal-agent dynamic, where citizens function as principals guiding the actions of their elected agents (representatives). A key feature of any principal-agent relationship is the amount of discretion principals grant to agents. Agents wield unique expertise, which principals seek to leverage to their own benefit. They can discern whether they are benefiting from the relationship by (1) monitoring the agents' efforts and/or (2) evaluating the final outcomes. The first option is particularly informative, but also costly, which reduces its efficiency (Miller 2005). Thus, to take advantage of an agent's expertise, principals must avoid excessive monitoring. Of course, principals must engage in some monitoring (to prevent corruption or shirking), but not too much (to stifle otherwise useful discretion). Research on principal-agent problems has focused a great deal on this balance, assessing how institutions can (1) minimize the incentives of agents toward corruption and/or principals toward over-monitoring and (2) maximize the use of agents' discretion and expertise (see Gailmard 2012).

For our purposes, legislators serving as agents to their constituents wield two types of exper-

tise: policy expertise on specific issues (Krehbiel 1991) and political expertise in the development of legislative coalitions (Kirkland 2011). Transparency reforms alter the principal-agent relationship by lowering the cost of monitoring, granting citizens the chance to observe this expertise in action. However, due to their own lack of expertise, these citizens may not fully understand or appreciate *why* their representatives make certain choices during deliberation. They may sanction representatives for negotiating with the other side even though such negotiation is necessary for successful policymaking. In short, excessive monitoring constrains legislators' ability to strike deals and develop winning coalitions. We expect that the consequences of this reduced capacity appear in several aggregated legislative outcomes.

3.1 Legislative Productivity

First, a likely consequence of this reduction in legislators' flexibility is gridlock and declines in productivity. State legislative scholarship consistently demonstrates that institutions and circumstances that should theoretically make policymaking more challenging do, in fact, reduce the number of bills that legislatures introduce and pass (Squire 1998; Rogers 2005; Crosson 2019). Further, research on the U.S. Congress has consistently suggested that as the proportion of status quo policies within the gridlock interval increases, legislative productivity should decline (Chiou and Rothenberg 2006; Woon and Cook 2015). Increased oversight by the public—when combined with its general distaste for conciliatory behavior—limits legislators' discretion to move off their ideal points from time to time in order to strike a deal. This inflexibility widens the gridlock interval, increasing the number of status quo policies that cannot be beaten by a new proposal.

Thus, we expect that transparency causes a decrease in legislative productivity. Public scrutiny of deliberation increases the difficulty of crafting quality policy solutions to public problems and generating coalitions of support to enact them. This logic is a key element of American state legislators' justification for exemption from sunshine laws. We formalize it in the following hypothesis.

H1 Legislatures operating under transparency requirements enact less proposed legislation than legislatures without transparency requirements.

3.2 Polarization and Partisanship

Legislators' claims about the consequences of transparency also imply that open governance initiatives result in increased partisanship in the chamber. As noted above, in a polarized political environment constituents generally oppose giving in to the other side, and thus lawmakers may feel reticent to compromise in a publicly visible forum. Of course, the roll call votes they cast are always public, so even closed-door legislatures cannot completely hide evidence of compromise. Principals can always monitor their agents by observing final outcomes. However, concealing the details of negotiation and compromise from the public affords legislators better opportunities to "spin" their decisions in a positive light. Observing in broad terms that a compromise occurred is more palatable to a principal than learning the specifics about how his or her agent "caved in" to the other side (Gilmour 1995).

Moreover, we contend that there is additional nuance to consider that concerns which bills receive a floor vote. As is implied by H1, identifying legislation that generates a large coalition of support in a chamber is difficult, as legislators' preferences are not always immediately clear. Given this difficulty, a small group of legislators might require the political cover afforded by closed-door proceedings to successfully choose which bills among many potential proposals are most likely to receive broad, bipartisan support. A "winnowing" process to select such bills would result in more bipartisan voting, and thus, lower average levels of partisan voting. This process would also generate lower levels of observed polarization in ideal points for the chamber, as more bills with broader support would be selected for advancement in the legislative process. Accordingly, we expect that opening the legislative process increases partisan ideal point polarization and observed levels of party loyalty in roll call voting.

H2 Legislatures operating under transparency requirements exhibit higher levels of polarization in ideal point estimates than legislatures without transparency requirements.

H3 Legislators operating under transparency requirements exhibit higher levels of party loyalty on roll call votes than legislatures without transparency requirements.

3.3 Policy Change

Next, a lack of flexibility to engage in legislative deliberation likely creates a political environment that is more status quo biased. By this logic, transparency laws are a source of what Jones and Baumgartner (2005) call "institutional friction." A long line of policy agenda research suggests that factors that slow down or stop the creation of new policies (maintaining the status quo) yield a distribution of year-to-year budgetary changes marked by excessive kurtosis, or relatively more extreme values (e.g., Jones and Baumgartner 2005; Epp 2018). According to this perspective, consistent failure to create new policies results in (1) frequent minimal budgetary changes from one year to the next—that is, no change most of the time, (2) occasional enormous budgetary changes, and (3) almost no moderate amounts of budgetary change.

The implication of such a status quo biased policymaking environment is that the distribution of budget changes has a relatively large spike at no budgetary change and heavy tails (i.e., more kurtosis). A more "smoothly" updating legislature that is effective at deliberating over policy would instead have a normally distributed set of budgetary changes with lighter tails and more moderate amounts of budget change. This "general empirical law" of public budgets (Jones et al. 2009) provides an additional means by which we can look for evidence of compromise (or lack thereof) in state legislatures. We formalize this expectation in our fourth hypothesis.

H4 Legislatures operating under transparency requirements exhibit greater kurtosis in the distribution of year-to-year state budget changes than legislatures without transparency requirements.

3.4 Budget Delay

Finally, if transparency inhibits political compromise it should be evident in the execution of what is most likely any state legislature's most important task: passing a budget. A state's budget impacts numerous policy areas, provides critical services to citizens, and allows key state agencies to function (Kirkland and Phillips 2018). It is not surprising, then, that all states impose a consistent deadline on when a budget must be passed and enforce that deadline with extreme measures, such

as mandatory shutdowns for a late budget in 22 states (Klarner, Phillips, and Muckler 2012). The budget process reflects a high stakes situation in which the ability to compromise is crucial to a legislature's success. Of course, the governor plays a major role as well (Kousser and Phillips 2012). But getting a proposal to the governor's desk may still require negotiation and deal-cutting within the legislature.

Accordingly, delay in the budgeting process is a clear indicator that compromise is not viable in a legislature. Specifically, we posit that legislators who are constrained in their capacity to politick and trade horses are more likely to go past the preset deadline for a new budget (the start of the new fiscal year). We formalize this logic in our final hypothesis.

H5 Legislatures operating under transparency requirements are more likely to pass late budgets than legislatures without transparency requirements.

3.5 Falsifiability of the Theory

One critical assumption underlying our theoretical framework is that constituents, if given access to information, would actually hold legislators accountable for their policymaking choices. That is, we assume transparency reforms lower the cost of monitoring enough to be useful. Recent research on American elections calls such an assumption into question (e.g., Achen and Bartels 2017). Particularly in state legislative elections, the public may lack the necessary knowledge or skills to hold legislators accountable for their policy compromises (Rogers 2017). If legislators are aware of these constituent limitations, open access to legislative business may have very little effect on legislative behavior, and thus, policymaking. If legislators know that, despite the opportunity to do so, constituents simply will not hold them accountable for their choices, those legislators will not fear the appearance of compromise irrespective of transparency laws.

Thus, in spite of legislators' stated apprehension of transparency, the null hypothesis—that sunshine law exposure does not influence compromise—is certainly plausible. Moreover, if the public is so inattentive as to not hold legislators accountable for their behavior, and thus legislators do not respond to the opening up of their deliberations, then the corruption-fighting, illuminative effects of transparency may not be realized either. If constituents cannot hold legislators accountable with

open access to legislative decisions and records, there is less reason to suspect these institutions will uncover or prevent legislative malfeasance. As such, it is not only possible that the null hypothesis is true, it is normatively critical that we empirically adjudicate between the null and our alternative theory described above.

Indeed, it is possible that legislators' contentions against transparency are arguments of convenience. That is, legislators may wish to avoid transparency for any number of other reasons, but that the institution itself exerts little impact on policymaking. Legislators' justifications for avoiding transparency may amount to "straw man" arguments, with little substantive logic underlying them. In the supporting information (SI) we present a computational simulation of legislative decisionmaking that demonstrates the plausibility of our theory (section SI-1). This simulation is, of course, a simplification of the legislative process. We present it to demonstrate that legislators' arguments (and our theory) about how changes in legislators' discretion to compromise might influence policymaking are not facile. The simulation results suggest that it is reasonable to suspect that decreased flexibility creates a more gridlocked legislature. Thus, examining these dynamics empirically is a valuable enterprise for both social science and for democratic reform efforts. We turn to that endeavor next.

4   Research Design

The primary goal of our empirical analyses is to identify the effect of exposure to sunshine laws on observable indicators of compromise in state legislatures. In particular, we focus on open governance requirements for state legislative meetings and proceedings. Since 1998, every state has a sunshine law on the books, but many states have exempted their legislatures from them at some point in time. Our treatment is the presence of a transparency requirement that applies to a state's legislature in a given year. States are considered untreated if they have not adopted a sunshine law *or* if the legislature is exempt from an existing law in a given year. We searched legislative records to obtain the specific name, statute, and adoption dates for transparency laws as well as the legislative exemption dates, if applicable. In some cases, only certain groups within the legislature received exemption. We coded a state-year as exempt if *any* of the following specific

legislative groups were exempt. These choices reflect circumstances in which legislators might alter a bill, deliberate over a bill, or engage in coalition building.[2]

- Subcommittees;
- Committees outside of Committees of the Whole;
- Partisan caucuses;
- Conference committees, personnel committees, and/or committees considering legislation not yet proposed before the entire chamber;
- Political committees, conferences, and caucuses;
- Ethics caucuses;
- Political parties, groups, caucuses, rules or sifting committees.

Additionally, there are some cases in which the state legislature was not exempt due to a rule outside of the state's sunshine law. We coded states as not exempt for the following reasons. See the section SI-2 of the SI for complete details of our coding decisions.

- The presence of another statute that requires open meetings and proceedings;
- The State Constitution;
- Chamber rules;
- Court decisions.

4.1 Measuring Compromise

Conceptually, flexibility to compromise is an internal characteristic of individual legislators, which makes measurement a difficult task. Indeed, only a legislator knows how much discretion he or she has when making a decision and in most cases he or she will have no reason or prefer not to divulge that information. Accordingly, we focus our measurement strategy on aggregated

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[2]One possible alternative would be to code the treatment variable as a count of the individual legislative groups that were exempt. We opt against such an approach for two reasons. First, a count would actually produce marginal gains in treatment variation because exemption tends to be "all or nothing." State governments that decide to exempt their legislatures rarely do so one group at at time, instead choosing to cover sets of them (e.g., all committees, caucuses, and parties) at once. Furthermore, from a theoretical standpoint we maintain that even granting exemption to a single group is enough for legislators to realize their perceived benefits of secrecy. In such a case, we would expect strategic politicians to focus any deliberation that they wanted hidden from public view exclusively in that forum.

observable consequences of compromise. Although these measures are not direct indicators of the concept, we contend that they are at least as useful because they capture consequential outcomes associated with compromise that bear directly on a state government's policymaking and representative functions. In short, even if they do not measure it directly, our outcomes reflect important political and policy ramifications of compromise.

More specifically, we focus our attention on five different outcome variables implied by our hypotheses: (1) bills enacted, (2) party polarization, (3) party loyalty scores, (4) budget kurtosis, and (5) budget delay. None are perfect measures, but in analyzing all five we seek to balance the disadvantages of each one with the strengths of the others. Table 1 summarizes these variables.

[Insert Table 1 here]

First, as our theory makes clear, we contend that aggregated productivity in lawmaking is a key observable implication of lawmakers' discretion. Our first empirical measure is the proportion of substantive (i.e., non-resolution) bills introduced in a state's legislature in a given year enacted into law over the period 1970–2016 (Council of State Governments 2018). This approach is common in legislative scholarship (e.g., Rosenthal and Forth 1978; Holbrook and Tidmarch 1991; Squire 1998; Rogers 2005; Squire 2007). It is useful in that it captures a core element of a legislature's primary function—passing legislation—and relates directly to legislators' main concerns with transparency (see Diana 2014).

Of course, this measure also has some limitations. For example, although we are able to separate substantive bills from symbolic resolutions, it still gives every enacted bill equal weight, which may be problematic (Grant and Kelly 2008).[3] Furthermore, while we gain considerable analytic leverage by examining state legislatures over many decades, the data are simply not detailed enough to construct a more sophisticated indicator reflecting the many stages of the legislative process, such as Volden and Wiseman's (2014) measure of lawmakers' effectiveness. Finally, our measure is also subject to manipulation through the legislative agenda. Party leaders may steer the

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[3]Indeed, Congress scholars have attempted to create measures of the "important" set of bills passed by Congress each year (e.g., Mayhew 1991). However, even these measures are the subject of disagreement (Howell et al. 2000; Binder 2003).

session away from controversial topics to maintain the perception of productivity in an otherwise gridlocked legislature.

Accordingly, we also assess ideological polarization of the parties. When legislators have discretion to politick and work across the aisle, legislation will reflect a broader consensus and observed voting behavior will place the parties relatively closer to one another, on average. In contrast, if compromise and consensus-building are uncommon features of deliberation, observed voting will reflect the party line more often and the average ideal points of the parties will appear far apart. We employ Shor and McCarty's (2011) measure of party polarization in state legislatures over the period 1993–2016 as our second outcome variable. This approach, which is computed as the difference in ideal points between the major party medians, complements the productivity measure by focusing on a characteristic of the *means* of conducting legislative business rather than purely on the ends. This change helps alleviate (though may not eliminate) the potential concern over variation in bill importance and/or the agenda setting power of the majority party. The measure allows us to assess how transparency affects the process of lawmaking, regardless of the specific content of its output.

Our first two outcome measures aggregate data to the legislature level. However, the capacity to compromise is actually an individual legislator characteristic. While we do not observe this capacity directly, as an alternative we can we measure bipartisanship at the legislator level. This variable extends the logic of the polarization measure down to what is perhaps the most appropriate unit of analysis. We posit that a closed door legislative process is more likely to select legislation that induces legislators to vote against their party more often, thus generating higher levels of bipartisanship on roll call votes. Our empirical indicator of individual bipartisanship is party loyalty scores generated from the roll call data in Shor and McCarty (2011) over the period 1995–2014. This variable is the percentage of votes cast by a legislator in agreement with his or her party among all roll calls in which a majority of one party voted against a majority of the other party. It is the strongest of our outcomes in capturing legislators' individual willingness to compromise with the other side—a key element in normative assessments of the effects of governmental transparency.

As we discuss above, we also consider policy change as a relevant signal of compromise. Indeed, the status quo is highly privileged in a gridlocked legislature, irrespective of the relative importance of bills, the majority party's agenda, or even ideological differences between the parties. Accordingly, a measure that considers only how often the status quo changes is useful as an observable indicator that mitigates the other political incentives that may hamper our other measures. To that end, we employ yearly state budget kurtosis to examine how "smoothly" or "efficiently" governments solve public policy problems. This measure, which comes from Epp (2018) and covers the period 1980–2008, stems from the distribution of year-to-year percent changes in spending on 20 public policy categories. A legislature that is highly status quo biased will see percent changes from one year's spending to the next tightly clustered around 0% change, and have a few large outlier changes in spending. The result of this type of policy environment is a distribution of budgetary changes that exhibits high kurtosis. A legislature that smoothly responds to public demands and policy problems will have normally distributed changes in public spending.

Finally, we analyze Klarner et al.'s (2012) indicator for a late state budget from 1961–2006. Budget delay is useful because it is not directly related to policy outcomes or legislative voting decisions. Instead, it captures information about the *timing* of the legislature's (and governor's) collective decisionmaking. Importantly, the measure avoids essentially all of the problems that accompany our other four measures. Budgets are undisputedly significant pieces of legislation that are not subject to typical agenda control pressures. They must be passed every legislative session, and thus long-term trends in societal problems—or other factors that impact demand for legislation—do not influence the decision to produce a budget. Moreover, budget deadlines are exogenously set, which minimizes concerns such as reverse causality.[4] Budget bargaining represents a key arena in which legislators' ability to strike deals is substantively valuable and clearly observable. As Klarner et al. (2012) note, "on-time budgeting...tells us a great deal about the capacity of elected officials to reach policy compromises and make needed decisions" (994).

[4]See Klarner et al. (2012) and Kirkland and Phillips (2018) for further details on these and other advantages to measuring legislative performance with budget delay.

4.2 Modeling

We identify the effects of transparency on these outcome measures with a multifaceted modeling strategy. At this point it is helpful to organize our five variables into two categories. The first, or "main" category is comprised of the first four outcomes (bill enactment, polarization, party loyalty, and budget kurtosis). Our modeling efforts on this group of variables reflect our own original design and analysis (which we detail below) to test H1–H4.[5] The budget delay variable represents the second category. In that case our analysis is a replication and extension of Klarner et al.'s (2012) research design. We start with their modeling strategy (described in section 5.2), then add our own treatment variable to the specification as a test of H5.

The core of our efforts for the main group of variables leverages the timing of states' sunshine law adoptions and/or legislative exemptions with a two-way fixed effects (FE) estimator in what is essentially a difference-in-differences (DID) design.[6] This approach provides one means of isolating the independent influence of the treatment by estimating the effect of a within-state change in legislative exposure to sunshine laws. It removes the confounding role of any time-invariant characteristics of states (and legislators in the party loyalty models) that are correlated with treatment status. Moreover, the design compares the change in a state that becomes treated to the same change among states that did not become treated, which controls for the possibility of a secular temporal trend in the outcome.

The two-way fixed effects estimator also facilitates control of time-varying confounding via observed covariates (see below). However, it does not preclude the possibility of bias from unmea-

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[5]See the SI (section SI-3) for complete definitions of the approaches we employ as well as an assessment of modeling assumptions.

[6]The design is not a straightforward DID case because the data include variation in treatment timing, treatment turning "on" and "off" over time, as well as some states that were treated in all years. The implication of the former issue relates to the specific quantity of interest that we can estimate. Rather than the typical average treatment effect on the treated (ATT) identified by such designs, our two-way fixed effects estimator gives a variance-weighted average of ATTs from all possible two-group (treated versus untreated units), two-period (before versus after) comparisons in the data (Goodman-Bacon 2018). Furthermore, as we show in the SI (section SI-2), our sample contains several states that were treated prior to the start of our data and remained under treatment throughout the entire time. While these units do not pose problems for statistical identification of our models, they are not necessarily useful for causal identification in the context of a DID design. Results without these states included in the estimation samples yield substantively identical results.

sured time-varying confounders. An alternative is the lagged outcome model, which conditions on the previous year's value of the outcome for each state instead of the unit and time effects. This approach identifies the ATT with an ignorability assumption conditional on the lag and covariates (Ding and Li 2019). The two-way fixed effects and lagged outcome approaches complement one another in a well-known "bracketing property" of the treatment effect (e.g., Angrist and Pischke 2008). Specifically, with their assumptions in place, estimates from the two modeling strategies can be "[treated] as the upper and lower bounds of the true effect" (Ding and Li 2019, 2).

A DID design also assumes that the initial decision to adopt a sunshine law and/or exempt the legislature is unrelated to the outcome. The fact that legislatures routinely exempt themselves from such laws calls this assumption into question. Accordingly, we also combine our two-way fixed effects estimator with an approach that models selection into treatment directly: Inverse Probability of Treatment Weighting (IPTW, see Blackwell 2013). The basis for this approach is that the longitudinal nature of our data structure creates two competing threats to causal inference: omitted variable bias and posttreatment bias. A variable may be correlated with both treatment status and the outcome, supporting the need to include it as a control. But if part of the causal effect of treatment travels through that variable, controlling for it will block that part of the effect (see Blackwell 2013, 507–508). IPTW estimators give the analyst a way out of this problem. The logic is to address the omitted variable bias by *reweighting* the data. We first model treatment status with time-varying covariates in a logistic regression model and generate weights from its output. Then we include those weights in a marginal structural model (MSM) of the outcome that excludes the time-varying covariates (but includes the fixed effects).[7]

Finally, the DID framework carries the key parallel trends identifying assumption, which we address in two ways. First, in the SI we report standard diagnostics (section SI-3). Second, we employ two methods that relax the assumption: the lagged outcome models discussed above (which substitute ignorability for parallel trends, see Ding and Li 2019), and Xu's (2017) generalized synthetic control method (GSC). This latter approach is designed to "estimate the average treatment

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[7]We maintain the two-way fixed effects specification for consistency with our first strategy. Results are unchanged if we remove the year fixed effects, which are technically time-varying.

effect on the treated using time-series cross-sectional (TSCS) data when the 'parallel trends' assumption is not likely to hold" (Xu 2017, 57). It does so by estimating the ATT in each of the post-treatment time periods.[8] However, it also reduces statistical power because the states with treated legislatures during the entire time period drop out of the estimation.[9]

We employ all of these strategies with several time-varying covariates. In the models of proportion of bills enacted, party polarization, and budget kurtosis, we control for the number of bills vetoed by the governor in a given state-year to account for potential inter-branch negotiating dynamics (Council of State Governments 2018).[10] In all models we control for Bowen and Greene's (2014) two dimensions of legislative professionalism. We include state citizen and state government ideology (Berry et al. 1998), a folded Ranney index, which measures the level of competition between the two major political parties for control of the legislature (Klarner 2018), and an indicator for whether legislative term limits were in effect in a state-year. Finally, we include the natural logs of total state population, gross state product (current dollars), and state expenditures on the legislature in current dollars (Klarner 2018). We also utilize multiple imputation to address missing data; all results presented below reflect the necessary adjustment to measures of uncertainty (see Blackwell, Honaker, and King 2017). See section SI-5 the SI for complete details on the imputation procedures, including diagnostic results on the imputations and estimation results using listwise deletion, which produce no change to our substantive conclusions.

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[8]The typical implementation of GSC employs an interactive fixed effects (IFE) model to estimate counterfactual outcomes for treated units. One disadvantage to this approach is that it requires more pretreatment data than a standard DID estimator (Xu 2017, 59). In some of our outcome data we only have a few years of pretreatment data for some states. Thus, we estimate GSC with Athey et al.'s (2017) matrix completion method, which may be preferred in such a case (Xu 2017). Utilizing the IFE estimator does not change our substantive conclusions.

[9]Additionally, we are unable to estimate a GSC model for the party loyalty outcome due to insufficient treatment variation in the time period covered by those data. The only state that transitioned from control to treatment after 1995 was Nebraska (1998), which does not produce party loyalty scores due to its nonpartisan legislature.

[10]We do not include this measure in our party loyalty models because that outcome variable focuses on internal party dynamics within the legislature and thus is less relevant to lawmaking compared to our other outcomes (although results are not dependent on this choice). Inter-branch bargaining could also be operationalized via an indicator for divided government (e.g., Crosson 2019). Results are substantively identical throughout the analyses if we include that variable.

4.3 Substantive Significance

We employ a framework for assessing the substantive importance of our estimated effects that involves statistical reasoning with confidence intervals (Rainey 2014). In brief, the analyst chooses a value *m* representing the smallest substantively meaningful effect magnitude, then determines whether an estimate's confidence interval includes *m* or $-m$. We consider a substantively meaningful effect to be one that is greater than or equal in magnitude to half of a standard deviation of the observed outcome. For instance, this decision yields $m \approx \pm \, 0.09$ for the first outcome. A legislature must enact nine percentage points more or less legislation while exposed to a sunshine law compared to closed access before we consider the effect substantively meaningful. For party party polarization (which ranges 0.20–3.44), party loyalty scores (1–100), and budget kurtosis (-0.11–1) our choice is associated with $m \approx \pm \, 0.25$, 7, and 0.11, respectively.

Of course, any value of *m* is somewhat arbitrary, and thus it is important to establish substantive context for our choice (Rainey 2014, 1085). Figure 1 presents violin plots graphing the densities of each outcome variable in the main category by treatment status. The transparent points are the raw data and the vertical dashed lines represent the status-wise 25th percentiles, medians, and 75th percentiles. The solid points are the means for each status (μ_{treated} and μ_{control}) and the solid horizontal lines represent $\{\mu_{\text{treated}}, \mu_{\text{control}}\} \pm m$. Thus, our research design denotes a treatment effect estimate as substantively significant if it is larger in magnitude than the half-width of the total vertical line (see the example in the control density of panel a).

[Insert Figure 1 here]

4.4 Statistical Power

In addition to substantive significance, it is important to consider our statistical power for detecting effects. To do so, we conduct power simulations of hypothetical treatment effects for each of the outcomes in the main category and with several model specifications. We define "true" treatment effects in each data generating process (DGP), simulate many fake datasets from those DGPs using the model output, then compute the proportion of times that the estimator rejects

the null hypothesis of a zero treatment effect at the 0.05 level. We vary the hypothetical effect magnitude from 0 (no effect) to an effect that is larger than m for each of the outcome variables. Table 2 reports our statistical power at the threshold for a substantively meaningful effect in each outcome-specification combination, as well as the smallest effect detectable at 95% power.[11]

[Insert Table 2 here]

Table 2 shows that the power at m is over 95% in 16 of the 20 cases. Additionally, the smallest detectable effects at 95% power are quite small in substantive context. For example, at 95% power we can detect effects of about 3-4 percentage points in proposed bills enacted, which are smaller than one-fourth of a standard deviation of that outcome. Similarly, our models can detect shifts in party loyalty scores of just 1-2 percentage points 95% of the time. In short, our research design is well-powered for detecting substantively important effects, if they exist in the data. Accordingly, we next turn to the estimated effects of exposure to sunshine on these first four outcomes.

5 Results

We report the estimated treatment effects and their 95% confidence intervals from the two-way fixed effects, lagged outcome, and IPTW models in Figure 2.[12] Additionally, we report the difference in means between treated and control units for each outcome, also with confidence intervals. We do *not* take a causal interpretation from this latter statistic. Instead, we report it to carry over the context from the raw data depicted in Figure 1 to our assessments of the estimated effects. The dotted vertical lines, which denote $\pm m$ for each outcome, also provide this context.

[Insert Figure 2 here]

Our results indicate that the role of exposure to sunshine laws is minimal. Across the various specifications and outcomes, the treatment effects are near zero. Some of the estimates are bounded away from zero, indicating statistical significance ($p < 0.05$). But in general the entire confidence

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[11]See the SI for complete power simulation results (section SI-6).

[12]The confidence intervals come from standard errors multiway clustered by state and year. See the SI for complete model results (section SI-4).

intervals are completely or nearly contained within the bounds of $[-m, m]$. In the cases of H1 (bill enactment), H2, (polarization), and H3 (party loyalty), the two-way fixed effects and lagged outcome models bracket the effect almost exactly at zero. Moreover, the difference-in-means statistics confirm that these findings are not artifacts of the modeling procedures, but rather patterns observed in the raw data as well (see also Figure 1).

The only slight exception to this general pattern is in the test of H4 (panel d). The lagged outcome models indicate that the treatment effect on budget kurtosis is negligible, but the two-way fixed effects and IPTW results yield positive and statistically significant estimates ($p < 0.05$). This result is consistent with our theory, although the estimates themselves are right at m and the confidence intervals clearly appear inside $[-m, m]$, so we cannot quite declare them substantively significant with great conviction. Moreover, the bracketing property of fixed effects and lagged outcome models would suggest that the effect likely falls well inside the interval anyways. Nonetheless, these estimates provide at least a glimmer of support for legislators' claims and our theoretical perspective discussed above.

We graph the GSC estimates in Figure 3. These results provide further tests of H1, H2, and H4. The left panels present the average outcomes over time for treated states along with the average counterfactuals—estimated by the GSC models from the control states—over time. These graphs provide a key diagnostic on the method: the correspondence between the two lines in the pretreatment (nonshaded) period. In all three cases, the two lines track one another quite closely, indicating that the counterfactual estimated by the model provides a good comparison for the treated states in the posttreatment period (shaded portions).

[Insert Figure 3 here]

The right panels of Figure 3 present the actual effect estimates, computed as the difference between the lines in the shaded portion of the left graphs. The shading in the right graphs indicates 95% confidence intervals and the dotted lines denote m. We also report the treatment effect averaged over time and its 95% confidence interval. In general, the point estimates remain inside $[-m, m]$, consistent with the findings reported in Figure 2. The key difference is a loss of statistical

power, as shown by the fairly wide confidence intervals that sometimes expand outside the bounds of substantive significance. In short, the GSC method yields negligible findings, but they are not as precisely-estimated as with the other modeling strategies.

5.1 Sensitivity to m

It is important to note that our interpretations depend on m. Perhaps our choice is too generous, declaring some values that should be considered substantively meaningful to be negligible. One way to assess sensitivity to m is to use the confidence intervals to identify the largest (in magnitude) plausible values of the effects in the hypothesized directions and reconsider whether those estimates, if they were realized, are meaningful. For instance, H1 posits a negative treatment effect. In panel (a) of Figure 2, the smallest lower confidence bound is a decrease of about 0.05 in the proportion of bills enacted (two-way fixed effects, no covariates). The average count of bills introduced in our data is 2,147 (with a standard deviation of 2,537). For an average legislature the largest plausible effect in the expected direction from H1 is just 107 ($0.05 \times 2,147$) fewer bills enacted over one year. Similarly, in panel (c) the largest upper confidence bound also comes from the two-way fixed effects specification with no covariates (≈ 5 percentage points in party loyalty). This result indicates that the best case scenario for H2 in our data is only about 36% of a standard deviation increase in the outcome variable.

This exercise in reasoning with confidence intervals does indicate that there is more sensitivity to m in the tests of H2 and H4. Panels (b) and (d) of Figure 2 show that the ends of the confidence intervals fall right on m or surpass it. Nonetheless, even the largest possible expected effects that are plausible in our data are still only 50% of a standard deviation in party polarization and 76% of a budget kurtosis standard deviation. Moreover, because these hypothetical effects come from the endpoints of confidence intervals, they would be very unlikely to be realized if we could repeatedly draw new samples of the data.

5.2 A Final Test: Sunshine and Budget Delay

Our last remaining task is to test H5. Recall that the budget delay variable from Klarner et al. (2012, hereafter, KPM) is notably different from any of the outcomes examined to this point. We

step outside of the modeling strategy for the main category of variables discussed above to replicate and extend KPM's analysis of the determinants of late budgets in the states from 1961–2006. KPM develop an empirical account of budget delay that incorporates numerous institutional, political, and electoral factors. As we note previously, the outcome variable is a binary indicator for a late budget, which is defined as a budget passed after the first day of the new fiscal year (FY). They model this variable with a logistic regression that includes state and year random effects.[13] We adopt this modeling strategy as a baseline, then add our exposure to sunshine treatment variable described above to it. Table 3 reports the coefficients and standard errors from these models.

[Insert Table 3 here]

In contrast to H5, the coefficient on sunshine in Table 3 indicates that legislative exposure to sunshine laws corresponds with a decrease in late budgets. This estimate is statistically distinguishable from zero at the 0.05 level and its inclusion improves model fit over KPM's original specification.[14] Importantly, it represents a substantively large effect. Averaging over the other variables in model (2), a switch from a state without legislative transparency requirements to one with such requirements corresponds to a drop of 4.7 percentage points in the probability of a late budget (95% confidence interval: $[-8.4, -1.8]$). Given that only about 16% of the state-years in the data include a late budget, that difference is noteworthy. Indeed, it is nearly equal to the 5.4 percentage point change in probability between states with and without government shutdown requirements, which KPM discuss at length as a major political deterrent to failed budget negotiations.[15]

In sum, this additional analysis provides perhaps the strongest case against the theoretical framework we set out to test. Rather than merely finding precisely-estimated negligible effects of transparency as with the main group of outcomes, here we demonstrate a precisely-estimated

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[13]This approach requires a standard selection-on-observables assumption for causal identification. We employ the specification in KPM's model (6) from Table 2 (1001) throughout this analysis because it is the most comprehensive model estimated on their full sample of data. However, results are not dependent on this choice.

[14]This pattern is also evident in the raw data. Approximately 20% of the untreated state-years include late budgets while only about 12% of the treated state-years' budgets were late (a significant difference at the 0.05 level).

[15]Due to the interaction terms, this estimate assumes a non-election year at the mean difference between the end of the legislative session and beginning of the new fiscal year.

and substantively large effect *in the opposite direction*. This result is a decisive capstone to our efforts to answer our research question. Using what some scholars would argue is the very best observable indicator of lawmakers' political discretion to compromise (e.g., Kirkland and Phillips 2018), we find clear evidence from decades of data on budget negotiations that the actual role of legislative transparency is antithetical to what many of those lawmakers believe.

6   Conclusions

American state legislators defend their exemptions from sunshine laws by asserting that too much public oversight adversely affects the policymaking environment, inhibiting their capacity to negotiate and engage in bipartisan compromise. This logic suggests an implicit choice between a transparent, open legislature and one which efficiently produces policy. However, despite this rhetoric from lawmakers (as well parallel claims from scholars) we find no empirical support for such a tension. In fact, our results show some clear evidence supporting the opposite perspective. These findings represent a critical contribution to a broad literature in political science and economics on open governance reforms, which has generally focused on how costs and benefits affect why some governments choose transparency while others resist it. Our results suggest that one particular cost—the effect on politicians' discretion to strike deals with each other—is not particularly consequential in practice.

Specifically, we develop a theory of how transparency affects political compromise that reflects state legislators' own concerns with sunshine laws. We test our expectations on five distinct outcome variables measured at two levels of analysis and with numerous complementary modeling strategies. We find a general trend of very small or contrary estimates that provide little evidence in favor of our hypotheses. Importantly, power simulations derived from our empirical models demonstrate that they *could* detect effects of similar size if nonzero effects existed in the data. In short, we contend that our analysis does not simply fail to reject the null, but rather yields precisely-estimated negligible and contradictory effects that are useful for substantive discussions on the role of transparency in legislative politics.

A natural question arising from these results centers on why we see so little support for our

theory, which we maintain represents a plausible set of expectations *a priori*.[16] One possible explanation for the negligible effects reported in the first set of analyses may stem from citizens' political engagement. The American public generally knows very little about and pays minimal attention to the details of state legislative politics (Rogers 2017). Yet our theoretical case for opposition to open governance is predicated on the notion that an attentive public monitors legislators' discussions and punishes compromising behavior. In practice, voters may not be aware of their legislators' decisions enough to provide credible electoral or political sanctions. However, citizen engagement does not necessarily explain our finding that transparency improves budget negotiations. In that case, perhaps openness in the process combines with the extreme need to reach a solution to force even the most stalwart ideologues to consider compromise.

Of course, we do not claim that transparency lacks any costs; policymakers undoubtedly feel political pressure when their actions are observable. We simply maintain that the aggregate consequences for compromise are not what legislators' and scholars' conventional wisdom suggests. Ultimately, we conclude that politicians' ability to effectively work on behalf of their constituents is certainly not hampered—and in some circumstances is even improved—under public scrutiny of their behavior. Representative government can function about as well or better if the represented are able to see the process unfold. Public access to the business of policymaking is a prerequisite for democratic accountability, and while the facilitation of compromise and coalition-building may initially seem to be reasonable justifications for limiting accountability, we see no supporting evidence of that motivation in lawmakers' observed behavior. Transparency may have a variety of effects on policymaking, but it does *not* inhibit political compromise.

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[16]See our computational simulation in the SI (section SI-1).

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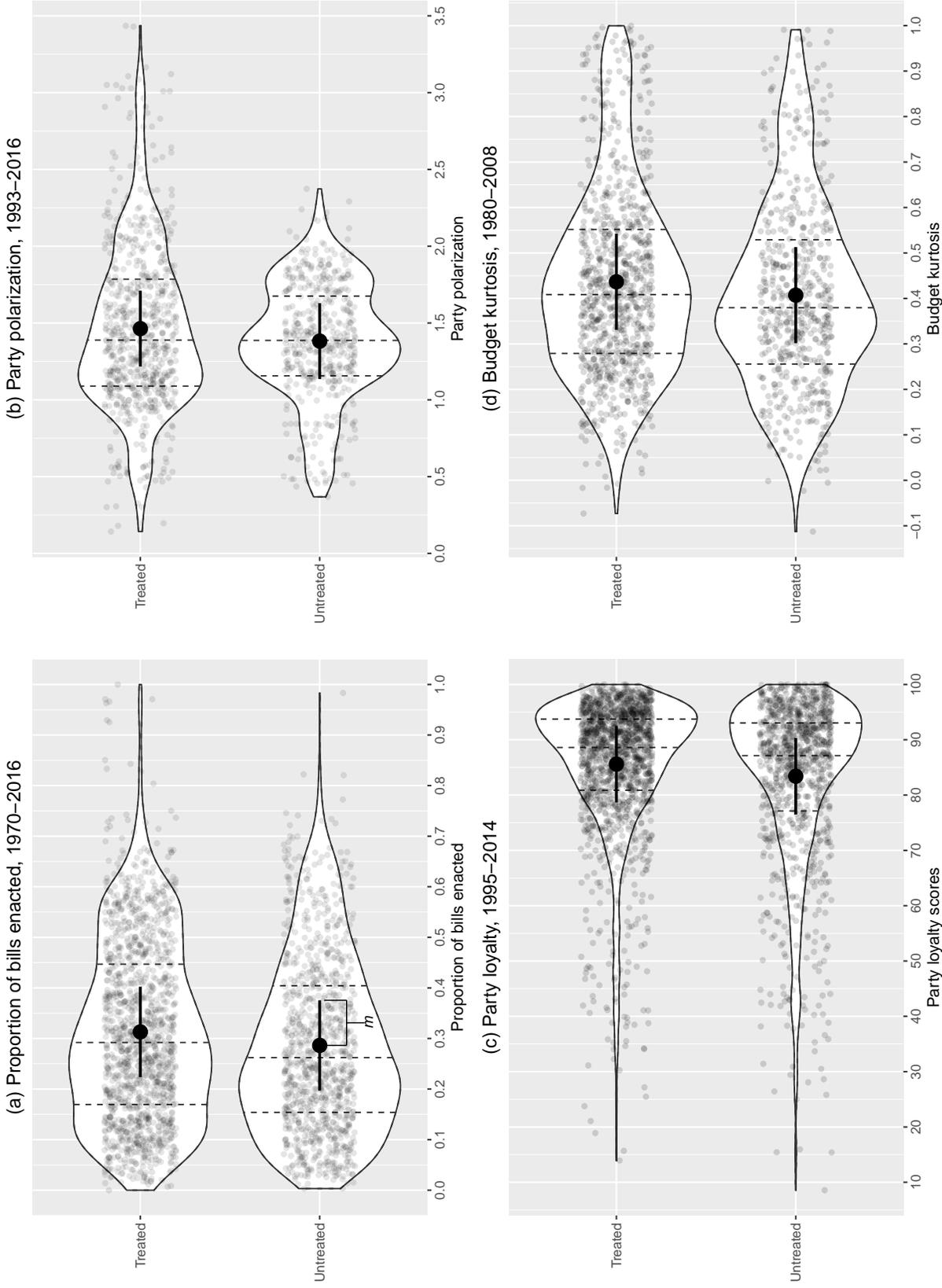
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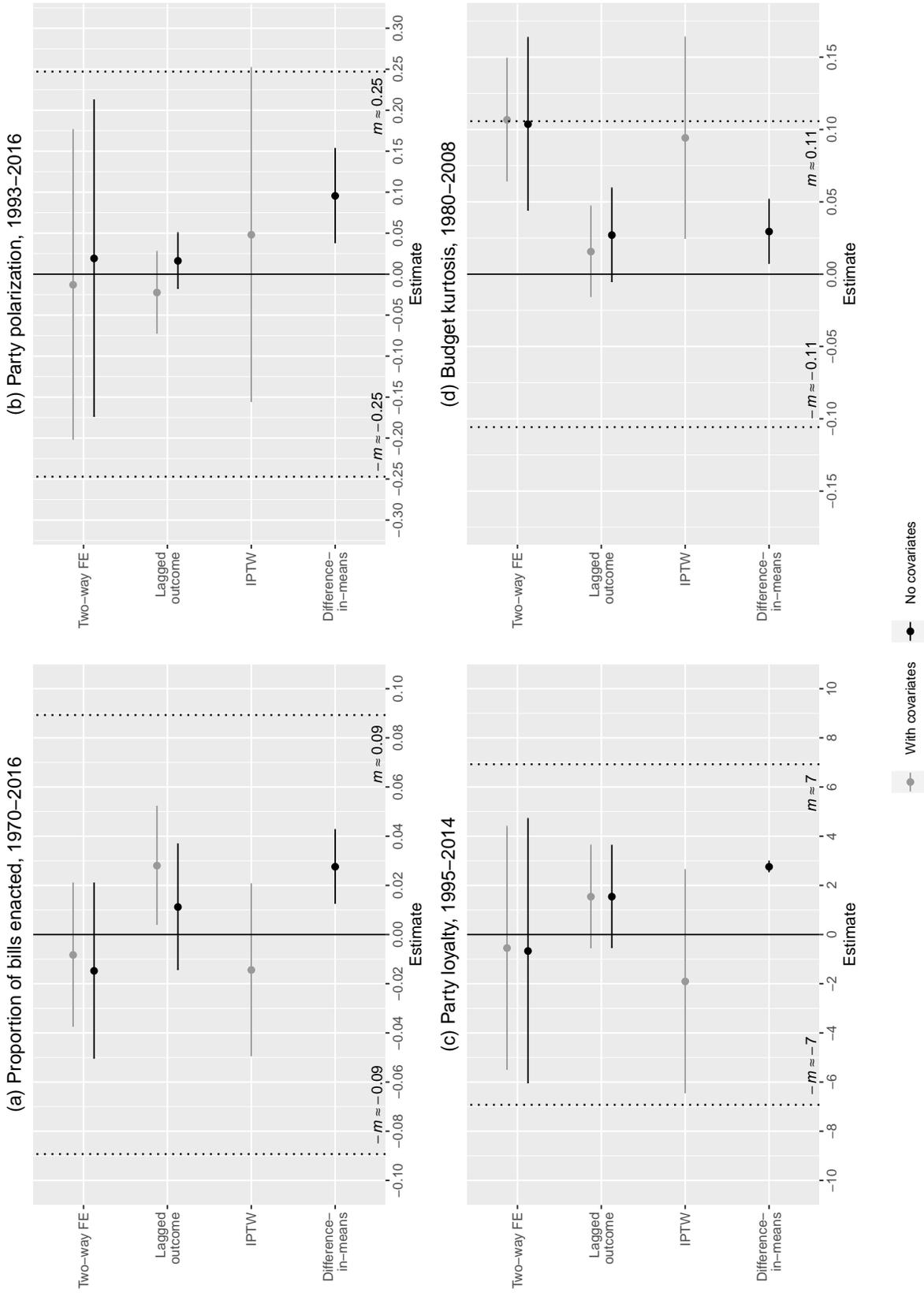Squire, Peverill. 2007. "Measuring Legislative Professionalism: The Squire Index Revisited." *State*

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Figure 1: Main Outcome Variable Densities by Treatment Status

(a) Proportion of bills enacted, 1970–2016

(b) Party polarization, 1993–2016

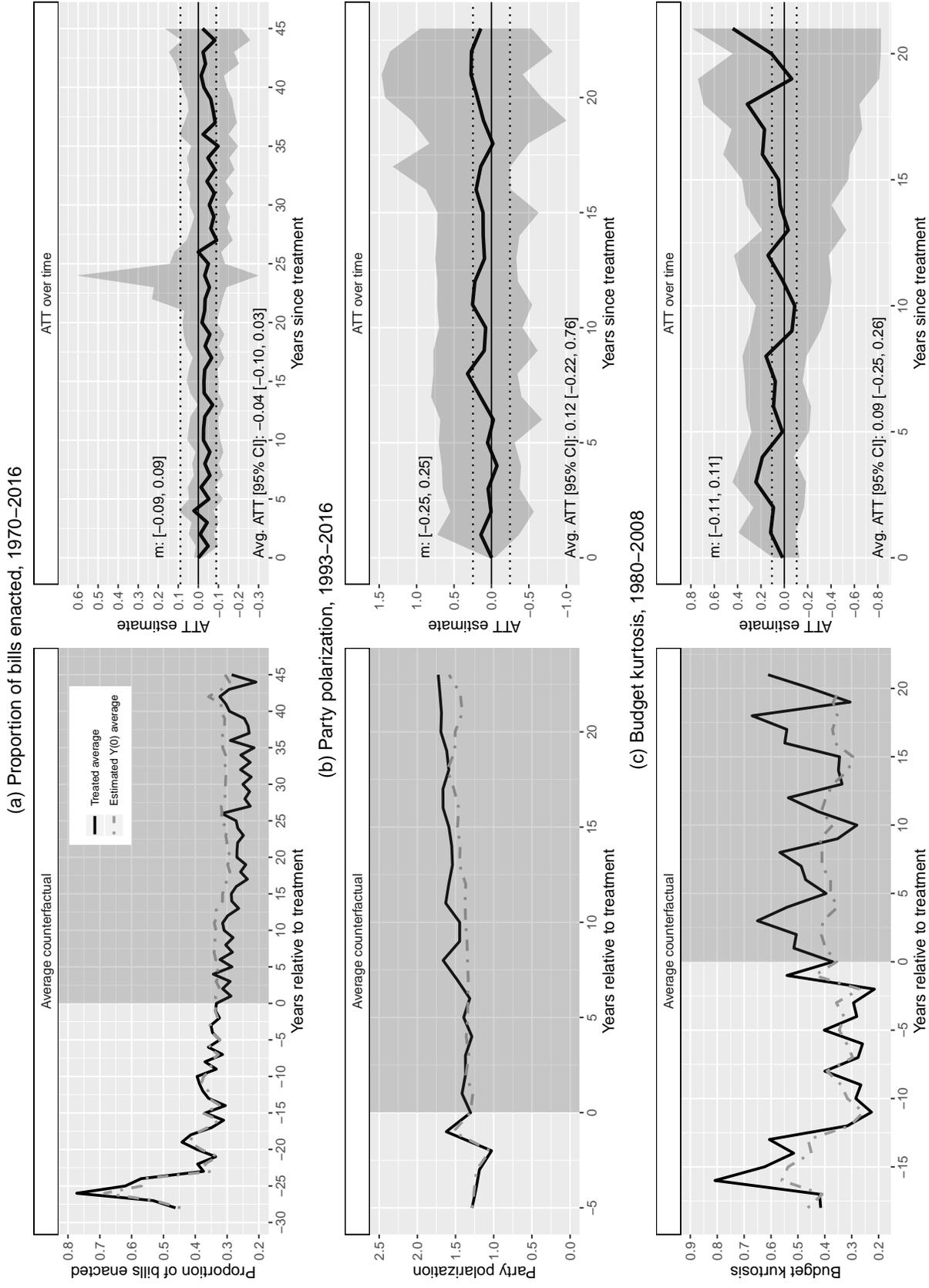(c) Party loyalty, 1995–2014

(d) Budget kurtosis, 1980–2008

Note: The graphs present violin plots of the densities of each outcome variable by treatment status. The transparent points are the raw data and the vertical dashed lines represent the group-wise 25th percentiles, medians, and 75th percentiles. The solid points are the means for each group (μ_{treated} and μ_{control}) and the solid horizontal lines represent $\{\mu_{\text{treated}}, \mu_{\text{control}}\} \pm m$.

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Figure 2: Estimated Effects of Legislative Exposure to Sunshine Laws on the Main Outcomes

(a) Proportion of bills enacted, 1970–2016

(b) Party polarization, 1993–2016

(c) Party loyalty, 1995–2014

(d) Budget kurtosis, 1980–2008

With covariates   No covariates

Note: The graphs present the estimated effects of exposure to sunshine laws and their 95% confidence intervals. The dotted vertical lines denote $\pm m$, our chosen threshold for a substantively meaningful effect. Only one IPTW specification is shown because that estimator requires covariates for weight estimation.

Figure 3: Estimated Effects with Generalized Synthetic Control

(a) Proportion of bills enacted, 1970–2016

(b) Party polarization, 1993–2016

(c) Budget kurtosis, 1980–2008

Note: The graphs on the left present the average outcomes for treated states along with the average counterfactuals from control states over time. The graphs on the right present the estimated ATTs and their 95% confidence intervals in the post-treatment time periods. Those estimates are computed as the difference between the two lines in the shaded regions of the left graphs.

Table 1: Outcome Variable Summaries

Concept	Measure	Advantages	Disadvantages
Productivity	Proportion of proposed bills enacted, 1970–2016.	A primary legislative responsibility; Central to lawmakers' concern over transparency.	A simple/blunt indicator; Gives all bills equal weight; Manipulable by the legislative agenda.
Polarization	Shor and McCarty's (2011) difference in party medians, 1993–2016.	A clear connection to compromise; Less influence from bill importance or agenda.	Based on electoral replacement (no within-legislator changes); Mostly trending upward over time.
Partisanship	Shor and McCarty's (2011) party loyalty scores, 1995–2014.	Measured at the legislator level.	May not reflect private bargaining environment; May change as bill importance changes.
Policy change	Epp's (2018) budget kurtosis scores, 1980–2008.	Minimal influence from bill importance or agenda.	Based only on policy spending; Individual legislators may have limited influence.
Budget delay	Klarner et al.'s (2012) late budget indicator, 1961–2006.	Clearly significant; Not subject to agenda control; Passage date is set exogenously.	Also impacted by the governor's preferences and actions.

Note: Cell entries report summaries and justifications for each outcome measure. The data are measured for all 50 states except for polarization and party loyalty, which exclude the non-partisan Nebraska legislature.

Table 2: Power Simulation Results

Outcome	Estimator	Power at m	Smallest Effect with $\geq 95\%$ Power
Bill enactment ($m \approx 0.09$)	Two-way FE, No covariates	100%	0.038
	Two-way FE, With covariates	100%	0.036
	Lagged outcome, No covariates	100%	0.028
	Lagged outcome, With covariates	100%	0.028
	IPTW	100%	0.038
Party polarization ($m \approx 0.25$)	Two-way FE, No covariates	99%	0.204
	Two-way FE, With covariates	100%	0.200
	Lagged outcome, No covariates	100%	0.068
	Lagged outcome, With covariates	100%	0.070
	IPTW	91%	0.264
Party loyalty ($m \approx 7$)	Two-way FE, No covariates	100%	1.950
	Two-way FE, With covariates	100%	1.950
	Lagged outcome, No covariates	100%	0.990
	Lagged outcome, With covariates	100%	0.990
	IPTW	100%	1.950
Budget kurtosis ($m \approx 0.11$)	Two-way FE, No covariates	84%	0.123
	Two-way FE, With covariates	83%	0.125
	Lagged outcome, No covariates	100%	0.049
	Lagged outcome, With covariates	100%	0.054
	IPTW	85%	0.120

Note: Cell entries report power simulations of hypothetical treatment effects in each outcome-model combination. We define "true" treatment effects, simulate many fake datasets from the models' DGPs, then compute the proportion of times for which the estimator would reject the null hypothesis of a zero treatment effect at the 0.05 level.

Table 3: Estimated Effects of Legislative Exposure to Sunshine Laws on Budget Delay, 1961–2006

	(1) KPM original	(2) With sunshine
Sunshine		−1.041*
		(0.294)
Government shutdown	−1.349*	−1.353*
	(0.540)	(0.539)
Election year	−0.061	−0.058
	(0.227)	(0.230)
Divided government	0.573*	0.633*
	(0.187)	(0.190)
Session end vs. start of FY	2.455*	2.262*
	(0.552)	(0.547)
Supermajority budget	−0.648	−0.997
	(0.972)	(0.986)
Personal income	−0.028	−0.056
	(0.204)	(0.205)
Biennial budget	0.718	0.725
	(0.400)	(0.399)
Budget size	0.863*	1.140*
	(0.260)	(0.275)
Surplus	−0.335	−0.350
	(0.199)	(0.204)
Start of FY	−0.630	−0.267
	(0.349)	(0.352)
Legislative salary	0.123	0.174
	(0.318)	(0.316)
Government shutdown × Election year	−0.296	−0.346
	(0.532)	(0.534)
Government shutdown × Session end vs. start of FY	−1.571	−1.075
	(0.813)	(0.820)
Intercept	−2.903*	−2.403*
	(0.418)	(0.431)
σ_{State}	1.333	1.322
σ_{Year}	0.249	0.268
BIC	1,066	1,060
N	1,719	1,719
States	48	48

Note: Cell entries report logistic regression coefficients and standard errors (in parentheses) with state and year random effects. The outcome variable is an indicator for a late budget (defined as a budget passed after the first day of the new fiscal year [FY]). Model (1) reproduces KPM's original specification (Klarner et al. 2012, Table 2, model [6], 1001). Model (2) adds the exposure to sunshine treatment variable. * $p < 0.05$ (two-tailed).

Does Transparency Inhibit Political Compromise?

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Contents

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SI-1 Theory Simulation Details

In this simulation, we generate 101 legislators whose ideal preferences for policy are normally distributed with a mean of 0 and a standard deviation of 1. Each of these legislators is granted some uniform amount of discretion that is plus or minus some numeric value from their ideal points. For instance, a legislator whose ideal point is -1.25 in a chamber whose discretion is ± 1 would find proposals between -2.25 and -0.25 equivalently preferable. Conceptually, the size of this interval reflects the variation in flexibility that might be induced by open or closed legislative proceedings. We then generate 5,000 normally distributed status quo government policies in the same space and randomly select a simulated legislator and one of these 5,000 status quos. The chosen legislator proposes an alternative to the status quo at her ideal point. Every legislator in the chamber then decides to vote for or against the proposal by the following decision rule:

(a) If the proposal is in the legislator's discretionary window and the status quo is not, vote for the proposal;

(b) In all other circumstances, vote for the status quo.

We consider 500 proposals to change the status quo following this same procedure. At the end of this simulation (or "legislative session"), we record how many proposals were enacted, the polarization of ideal point estimates on the roll call votes of the chamber, and the kurtosis of the policy changes.[1] We repeat this single simulation/session 200 times (generating 101 legislators, 5,000 status quos, 500 proposals, and voting to change policy 500 times), then decrease the size of the discretion window for the chamber. Doing so mimics a more open process that presents more information about legislators' policymaking activity. We use discretion windows of ± 1, ± 0.9, ± 0.8, and ± 0.7. We should emphasize that the precise values are less important than the comparative statics: that legislators are operating with increasingly lower levels of discretion when voting on bills to change the status quo. We present the results in Figure SI-1. The x-axes plot variation in legislator discretion (decreasing from left to right) against the outcomes on the y-axes.

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[1]For the ideal point estimates, we use the alpha-NOMINATE program (Carroll et al. 2013) and calculate polarization as the average distance between any pair of legislators.

Each point represents a single iteration of the simulation and the gray lines denote a lowess curve summarizing the trend in the points.

[Insert Figure SI-1 here]

The graphs demonstrate that the relationship between changing legislative discretion and the three outcomes we examine mirrors our expectations. As discretion goes from high to low (i.e., as legislative deliberation becomes more publicly visible), the percentage of proposals enacted decreases (panel a) and policy kurtosis increases (panel c). For polarization in panel (b), the relationship is more complex. The average amount of polarization increases as discretion moves from high to medium, and then declines again as discretion moves from medium to low. However, the maximum observed level of polarization increases at each value of discretion. That is, we observe the highest levels of polarization when there is the least legislative discretion. Thus, low levels of discretion seem to create the opportunity for increased polarization, but do not guarantee it.

SI-2   Coding Sunshine Laws and Exemptions

Table SI-1 presents details for our coding of sunshine laws and exemptions in state legislatures. We searched legislative records to obtain the specific name, statute, and adoption and exemption dates listed. Figure SI-2 reports the pattern of treatment status in all states from 1960–2016, which includes the entire time span collectively covered by our various outcomes. As we discuss in the main text, there is variation in treatment timing, treatment turning "on" and "off" over time, as well as some states that were treated (legislature under transparency requirements) in all years.

[Insert Table SI-1 here]

[Insert Figure SI-2 here]

SI-3   Model Definitions, Assumptions, and Diagnostics

We rely on several empirical strategies to estimate the effect of sunshine law exposure in our main analyses: a two-way fixed effects estimator (difference-in-differences design, or DID), lagged

outcome models, inverse probability of treatment weighting (IPTW), and generalized synthetic control (GSC).[2] Here we discuss several details and assumptions regarding these methods.

The two-way fixed effects model involves estimating regression equations such as the following, which describes the model for the first outcome variable (proportion of bills enacted).[3]

$$\text{Prop. enacted}_{it} = \beta_1 \mathbb{1}\{\text{Sunshine}_{it}\} + \gamma_i + \delta_t + \mathbf{X}\beta + \varepsilon_{it}, \tag{1}$$

where i indexes states, t indexes years, and the sunshine indicator denotes that a sunshine law applies to state i's legislature in year t. The variables γ_i and δ_t represent state and year fixed effects and $\mathbf{X}\beta$ represents a matrix of time-varying covariates ($\mathbf{X}$) and their coefficients (β). The term ε_{it} represents random error. The causal quantity of interest is β_1, the effect of exposure to sunshine.

In a DID, we assume that all confounding variables have been measured and included as covariates (if they vary over time) or are time-invariant (Angrist and Pischke 2008). If this assumption is invalid—that is, there exists an unmeasured time-varying confounder—conditioning on a lagged value of the outcome instead of the state and time effects may be a better modeling choice (although we must still be mindful of including appropriate covariates). Continuing the example of the first outcome variable, the estimated equation is

$$\text{Prop. enacted}_{it} = \beta_1 \mathbb{1}\{\text{Sunshine}_{it}\} + \tau\{\text{Prop. enacted}_{i(t-1)}\} + \mathbf{X}\beta + \varepsilon_{it}, \tag{2}$$

where τ is a coefficient on a one-year lag of the outcome. These models may also be preferable to DID because, while they assume ignorability conditional on the lag and covariates, they do not assume parallel trends, which we discuss below (Ding and Li 2019). When estimated together, the two modeling approaches provide a "useful bracketing property" that yields bounds on the estimated treatment effect (Angrist and Pischke 2008, 245; see also Ding and Li 2019). Thus, observing similar results with both—as we generally do here—is a helpful signal that the conclusions

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[2]We employ multiway clustered standard errors by state and year (Cameron and Trivedi 2005), combined over the imputed datasets (Blackwell, Honaker, and King 2017).

[3]The equation for the party loyalty scores is similar, but also includes an indicator for the upper chamber and legislator fixed effects.

we draw are robust to time-varying confounders and identification assumptions.

SI-1   Parallel Trends

The key identifying assumption for DID designs is parallel trends; we assume that the difference between treated and control units is constant over time in the absence of treatment (Angrist and Pischke 2008). In cases where treatment timing varies, this counterfactual assumption becomes a more complex variance-weighted common trend assumption, in which comparisons of treated and untreated groups must be weighted by each group's sample size and variation in treatment status (Goodman-Bacon 2018).[4] One means in which we address this assumption is to estimate treatment effects using methods that relax the assumption: the lagged outcome model described above (Ding and Li 2019) and Xu's (2017) generalized synthetic control (GSC) method.

As a check on the assumption itself, we compare the pretreatment trends in our bill enactment, polarization, and kurtosis outcome variables for every state that became treated (adopted a sunshine law that applied to the legislature).[5] Figure SI-3 graphs the average outcome for treated (red) and untreated (blue) states up to 1997, the year before the last sunshine law adoption (Nebraska). The vertical lines reflect adoption dates for the treated states listed on the graphs. Dot sizes are proportional to the number of states in a group.

[Insert Figure SI-3 here]

The graphs suggest that the parallel trends assumption is generally reasonable for each outcome, although there are some points where the two lines may appear to diverge. Accordingly, it is important to further investigate the assumption. Another means of doing so is the estimation of treatment leads—the effect of treatment in a given year on the outcome in *prior* years. Strong validation of the design appears if the lead effects are near zero, indicating pretreatment similarity between treated and untreated states. Figure SI-4 reports estimates with leads of 1–4 years.

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[4]Furthermore, identifying the average treatment effect on the treated (ATT) with variation in treatment timing also necessitates the assumption of constant effects across time and across units (Goodman-Bacon 2018).

[5]We cannot construct such a graph for the party loyalty scores data because they do not begin until 1995. The only treated state that adopted its sunshine law after that time was Nebraska, which has a nonpartisan legislature. Variation in treatment in the party loyalty models stems entirely from legislative exemptions.

[Insert Figure SI-4 here]

The top panels in Figure SI-4 show validating evidence for the bill enactment and party polarization models. The leads of treatment are small in magnitude (near zero) and not statistically significant at the 0.05 level, suggesting no effects of treatment prior to the adoption of sunshine laws. The estimates in the bottom panels show weaker evidence with respect to the party loyalty and budget kurtosis models. Those results show some leads with estimates bounded away from zero, casting doubt on the parallel trends assumption several years before treatment in these data. In short, we find some evidence, but not complete evidence, favoring the key identification assumption of DID. These findings justify our choice of reporting estimates from a variety of modeling strategies including those that relax the assumption.

SI-2 IPTW Balance

Finally, DID assumes that the treatment is unrelated to the outcome at baseline. It is possible that this assumption is suspect because state legislatures themselves have considerable control over whether they are subject to sunshine laws. Accordingly, we combine the DID design with an approach that explicitly models selection into treatment. The starting point for the IPTW method is the fact that the longitudinal nature of our data structure creates two competing threats to causal inference: omitted variable bias (unmeasured confounders) and posttreatment bias. If the treatment in one time period influences the confounding variable in the next, then part of the effect of the treatment will be blocked when controlling for that confounder (see Blackwell 2013, 507–508). IPTW estimators give the analyst a way out of this problem. The logic is to address the omitted variable bias by *reweighting* the data to break the correlations between treatment and confounders. We estimate the weights with our set of time-varying covariates in a first-stage logistic regression model of treatment status, then include those weights in the outcome model, which contains only the fixed effects and treatment indicator.[6]

The IPTW method assumes sequential ignorability to identify causal effects: treatment at time

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[6]We maintain the two-way fixed effects specification for consistency with our first strategy. Results are unchanged if we remove the year fixed effects, which are technically time-varying.

t is independent of the potential outcomes, conditional on the covariate and treatment histories up to time *t* (Blackwell 2013, 510). This assumption would be violated if we were unable to account for one or more covariates that affect treatment status and the outcome in our estimation of the weights. Here we have attempted to collect a comprehensive set of covariates to improve balance, or similarity, between treated and untreated cases. Second, we assume positivity, which refers to the extent to which there is "overlap" between treated and untreated units on the covariates.

We can use the weights to assess these assumptions. The left graphs in Figure SI-5 plot standardized mean differences between the treated and control units for each covariate. The red (blue) dots and lines indicate the raw unweighted data (weighted data). We use a standardized difference of 0.2 as our threshold for balance, which is shown by the vertical dashed lines. Although there is some variation, generally the standardized mean differences decrease in the weighted data (blue), indicating that the weights improve balance over the raw data (red).

[Insert Figure SI-5 here]

To check the positivity assumption, Blackwell (2013) notes that the weights should, on average, be close to 1. An average weight well below 1 indicates that there are very few "surprises" with respect to treatment status. The right graphs in Figure SI-5 give the distributions of the weights in each year. The black lines indicate the mean, the gray boxes measures the interquartile range, and the lines graph the full range. Note that the means and interquartile ranges are near 1, and even the extreme values are not appreciably larger than the other weights. We take this as evidence that the positivity assumption is reasonable for these data.

SI-4   Main Results Tables

Table SI-2 reports complete results from the two-way fixed effects and lagged outcome models. The cell entries report regression coefficients with standard errors multiway clustered by state and year (in parentheses). For each outcome, model (1) is two-way fixed effects with no covariates, model (2) is two-way fixed effects with covariates, model (3) is lagged outcome with no covariates, and model (4) is lagged outcome with covariates. Note that there is no sample size loss in the bill

enactment lagged outcome models because the outcome data include the year 1969 (we do lose observations in the models with the other outcomes). Estimates for state ideology and government ideology are multiplied by 100 throughout the table to enhance readability.

[Insert Table SI-2 here]

Table SI-5 reports complete results from the IPTW models; cell entries report coefficients with standard errors (in parentheses). The top panel (stage 1) reports logistic regression weighting models. The bottom panel (stage 2) reports treatment effects from a marginal structural model (MSM). The weighting models include the time-varying covariates discussed in the main text as well as several other baseline variables common in the literature on IPTW (e.g., Blackwell 2013): a one-year lag of treatment status, the cumulative total of years under treatment, and their interaction.[7] These covariates help stabilize the estimated weights.

[Insert Table SI-3 here]

SI-5   Multiple Imputation Diagnostics

Our main analysis data (i.e., the first four outcomes) include some missingness. We used multiple imputation with the Amelia II software (Honaker, King, and Blackwell 2011) to fill in missing values, producing five complete datasets for each outcome. Of course, imputation procedures have their own problems, which may even make listwise deletion preferable (Arel-Bundock and Pelc 2018; Pepinsky 2018). As such, we report diagnostics on the imputations of our data below. We also repeated our main models using listwise deletion and found substantively identical results to what we report in the main text (see section SI-2).

SI-1   Overimputation and Density Plots

Overimputation is a diagnostic tool that conducts imputation of the observed (i.e., non-missing) data, then compares the imputed to the actual values of those data. Figure SI-6 presents overimputation results for the variables used in the proportion of bills enacted models.[8] The observed

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[7]Sunshine$_{t-1}$ and Sunshine$_{t-1}$ $\times$ Cumulative sunshine are omitted from the party loyalty model due to singularities.

[8]To conserve space, we only present diagnostics from the dataset of that outcome, which covers the entire time periods of the other outcomes. Results are similar with the other datasets.

values of the non-missing data are plotted on the x-axes and imputed values (averaged over the five datasets) are plotted on the y-axes. The vertical line segments indicate 95% confidence intervals for the imputations and the solid line serves as a reference point for "perfect" imputation. In an ideal scenario the points would fall along the reference line. More realistically, favorable evidence for the imputation procedure would exist if (approximately) 95% of the confidence intervals include the reference line. The colors classify each point based on this criterion: blue indicates points for which the confidence interval includes the reference line and red indicates points that do not. The values in square brackets next to each label refer to the actual coverage level for that variable.

[Insert Figure SI-6 here]

The graphs in Figure SI-6 generally shows good, though not perfect, coverage of the reference line. The clouds of points trend upward with the lines, and most of the points are blue. The actual coverage rates are slightly less than, but close to, the target of 0.95. Thus, the imputation results fall short of ideal, but are nonetheless reasonable.

Figure SI-7 presents density plots of the observed (blue) and imputed (red) values (averaged across the five datasets) of each variable. These graphs indicate considerable overlap between the two groups. This finding provides further evidence that the imputation procedure produced reasonable values for the missing data.

[Insert Figure SI-7 here]

SI-2   Results with Listwise Deletion

Figure SI-8 reports the estimated effects of sunshine exposure and their 95% confidence intervals from our main specifications, estimated with listwise deletion of missing cases (i.e., no imputation).[9] The results are quite similar to our main results. The most notable difference is the IPTW estimate in the party polarization graph, which is negative, statistically significant, and passes the threshold for substantive significance (although its confidence interval falls inside $[-m,$

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[9]The sample size change due to listwise deletion for the two-way fixed effects specifications with covariates are as follows. (1) Proportion of bills enacted: 2,350 to 1,516; (2) Party polarization: 1,200 to 574; (3) Party loyalty: 70,196 to 48,508; (4) Budget kurtosis: 1,450 to 1,162.

m]). However, the other estimates in that graph are not substantively significant so the weight of the evidence still points to a negligible treatment effect on party polarization.

[Insert Figure SI-8 here]

SI-6   Power Simulations

We generated fake outcome data by defining a treatment effect in the linear predictor of each model specification, then added random normal error with a mean of zero and standard deviation equal to the average residual standard deviation (across imputed datasets) from the model. We then estimated the model again on the fake data and determined whether the 95% confidence interval was bounded away from zero. We varied the known value of the treatment effect such that it crossed *m* (the threshold for a substantively meaningful effect) for each outcome.

The graphs in Figure SI-9 plot the hypothetical treatment effect range on the x-axes and the probability of rejecting the null hypothesis of no effect on the y-axes. The dotted vertical lines denote *m*. Overall, the results indicate that our research design is generally well-powered, even with the added variation that is inherent in estimating models with imputed data.
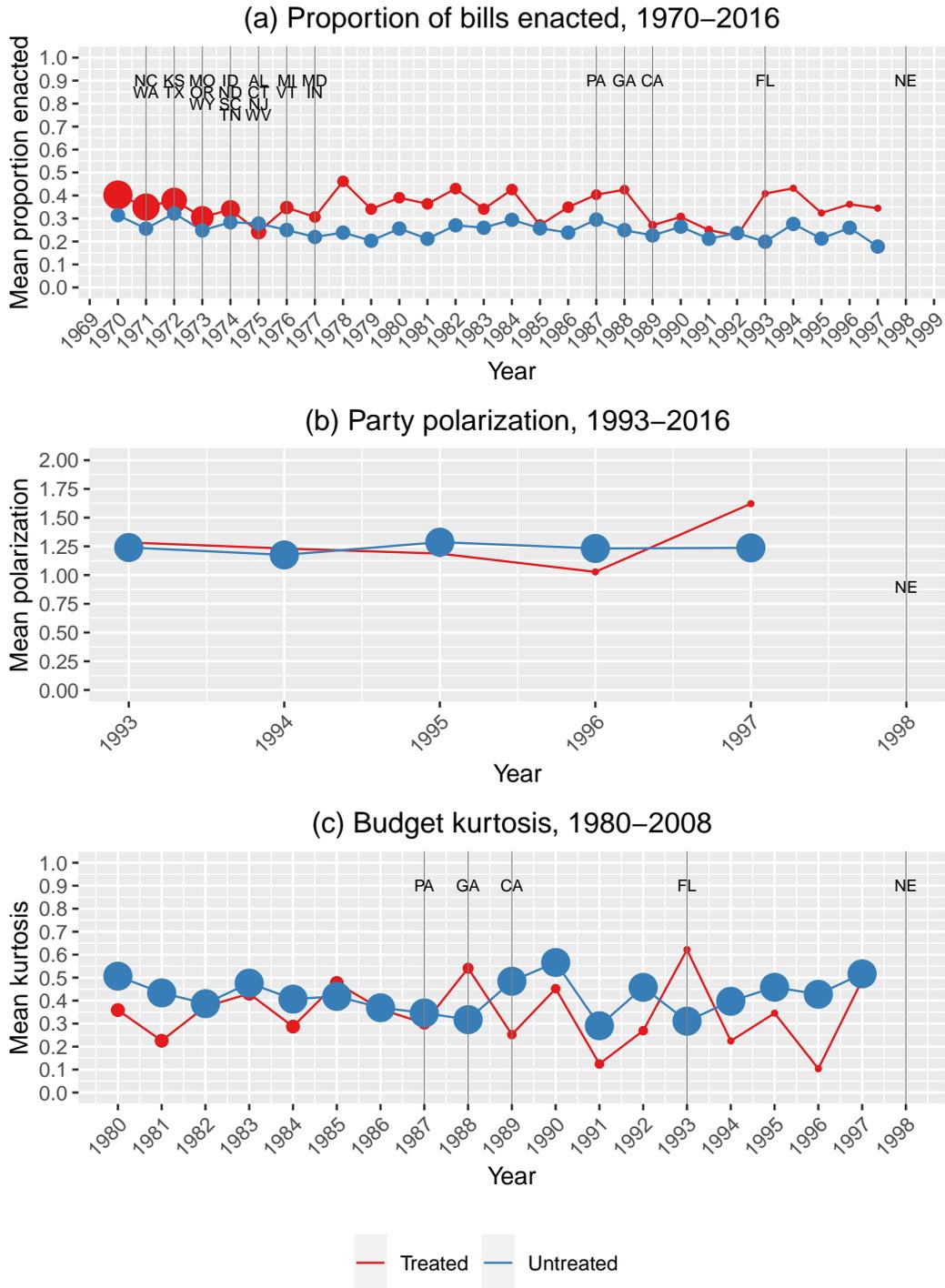
[Insert Figure SI-9 here]

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Xu, Yiqing. 2017. "Generalized Synthetic Control Method: Causal Inference with Interactive Fixed Effects Models." *Political Analysis* 25(1): 557–76.

Figure SI-1: Simulated Effects of Variation in Legislative Discretion

(a) Bill enactment

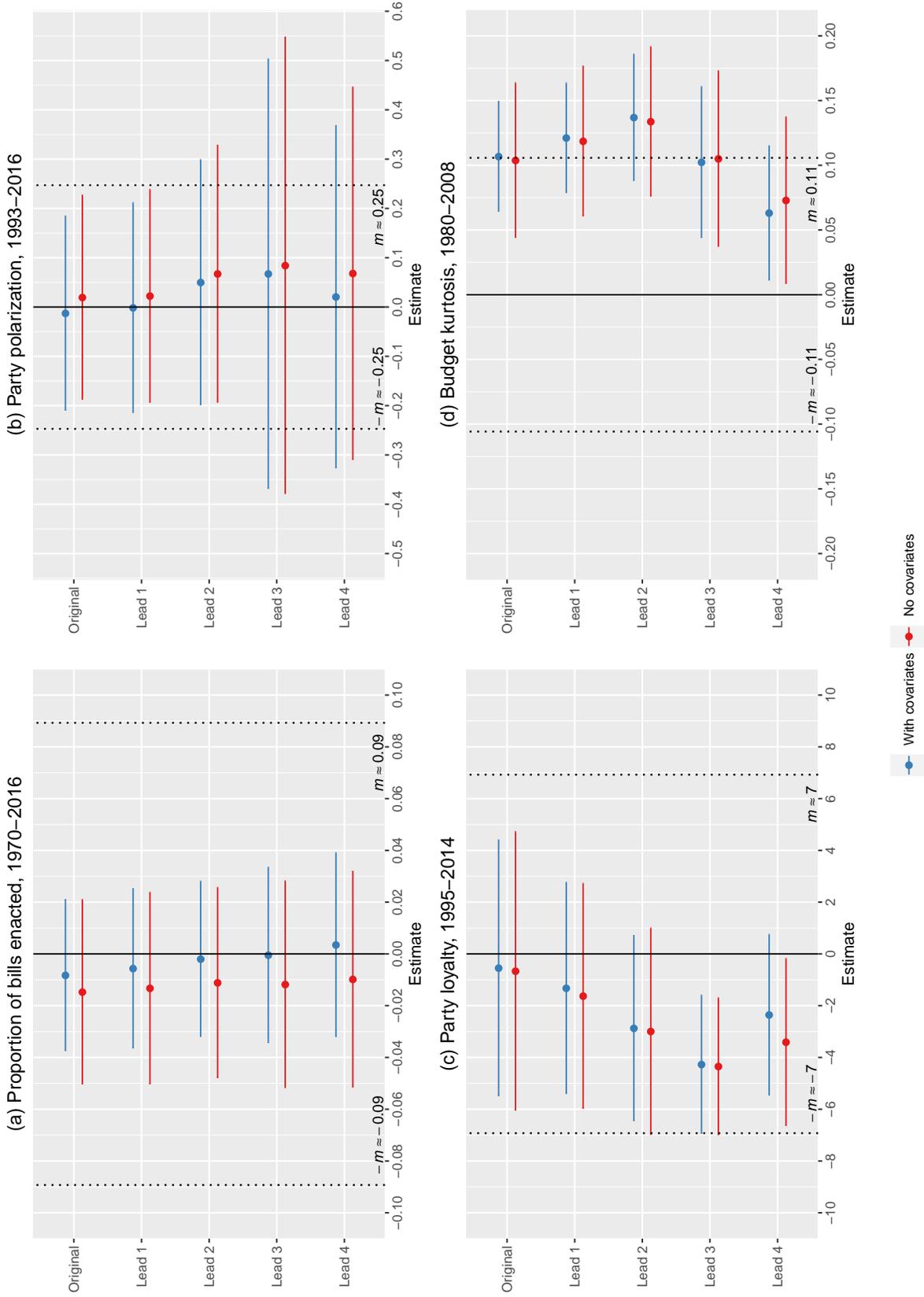## (b) Polarization

(c) Policy kurtosis
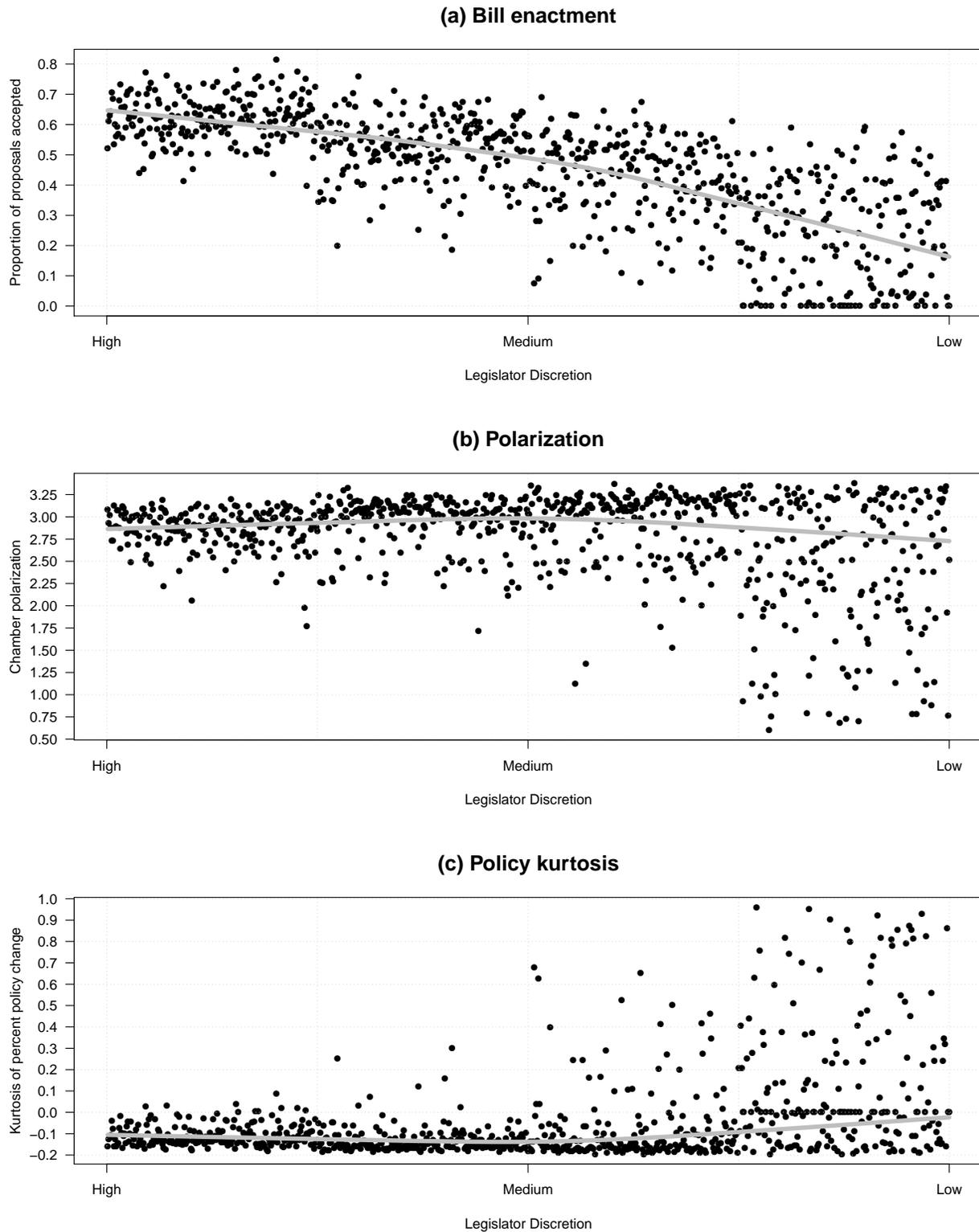
Note: The graphs present simulated legislative outcomes with each point representing a single iteration of the simulation. The gray lines are lowess curves. Simulated legislative discretion decreases from left to right in each plot.

Figure SI-2: Variation in State Sunshine Law Legislative Exposure, 1960–2016

Figure SI-3: Pretreatment Means in the Outcomes for Treated and Untreated States

(a) Proportion of bills enacted, 1970–2016

(b) Party polarization, 1993–2016

(c) Budget kurtosis, 1980–2008

Treated — Untreated

Note: The graph presents the average outcome for treated (red) and untreated (blue) states up to 1997, the year before the last sunshine law adoption (Nebraska). The vertical lines reflect adoption dates for the treated states listed on the graphs. Dot sizes are proportional to the sample sizes of states.

Figure SI-4: Estimated Effects of Treatment Leads

(a) Proportion of bills enacted, 1970–2016

(b) Party polarization, 1993–2016

(c) Party loyalty, 1995–2014

(d) Budget kurtosis, 1980–2008

With covariates   No covariates

Note: The graphs present estimated treatment effects from the two-way fixed effects estimator for leads of treatment from 1–4 years. Points represent effect estimates and line segments represent 95% confidence intervals.

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Figure SI-5: IPTW Balance and Weight Distributions

(a) Proportion of bills enacted, 1970–2016
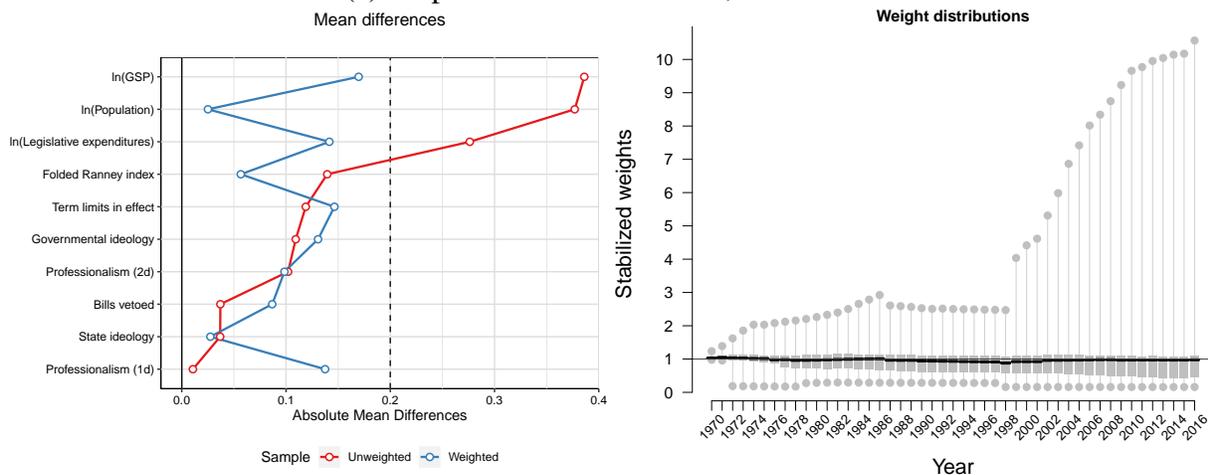
(b) Party polarization, 1993–2016
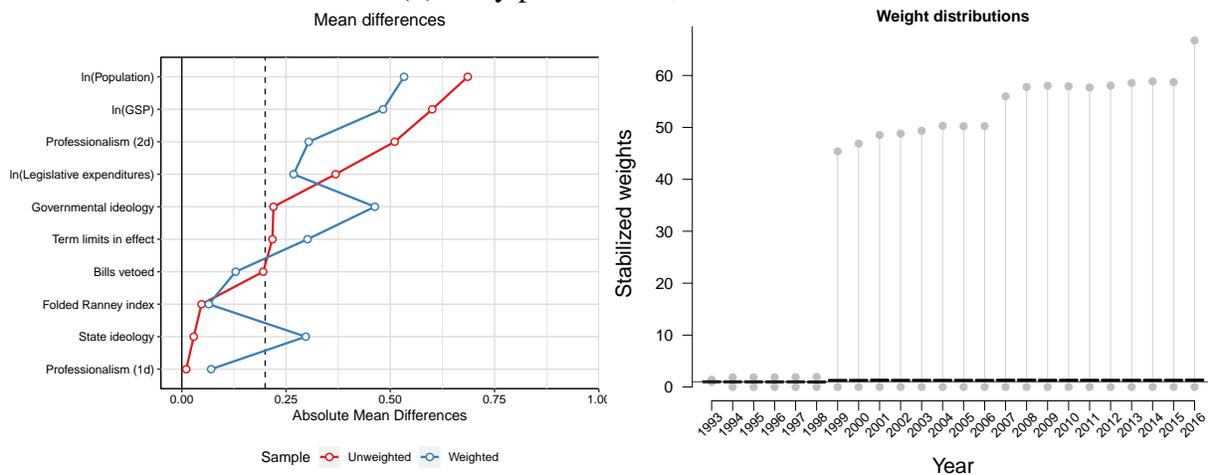
(c) Party loyalty, 1995–2014
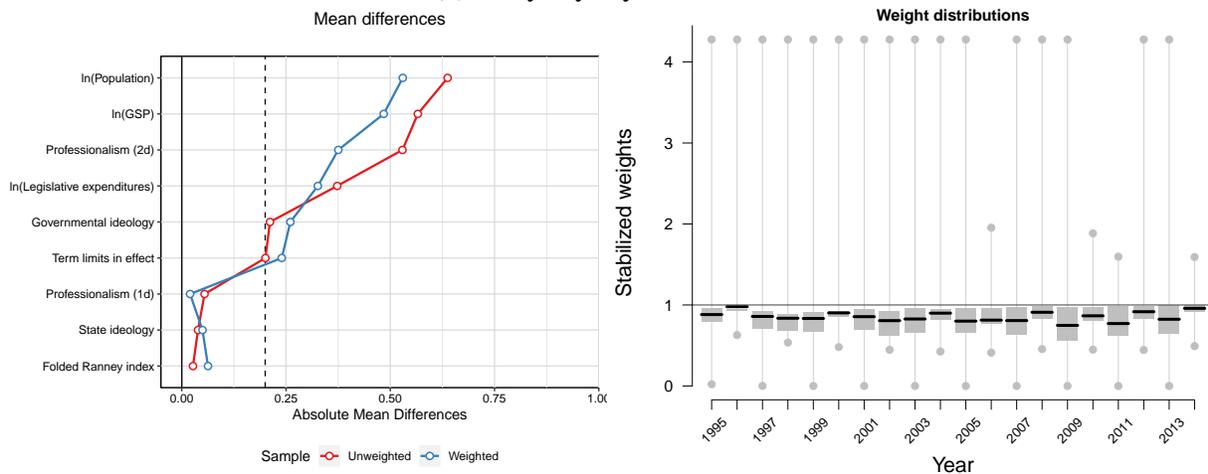
(d) Budget kurtosis, 1980–2008
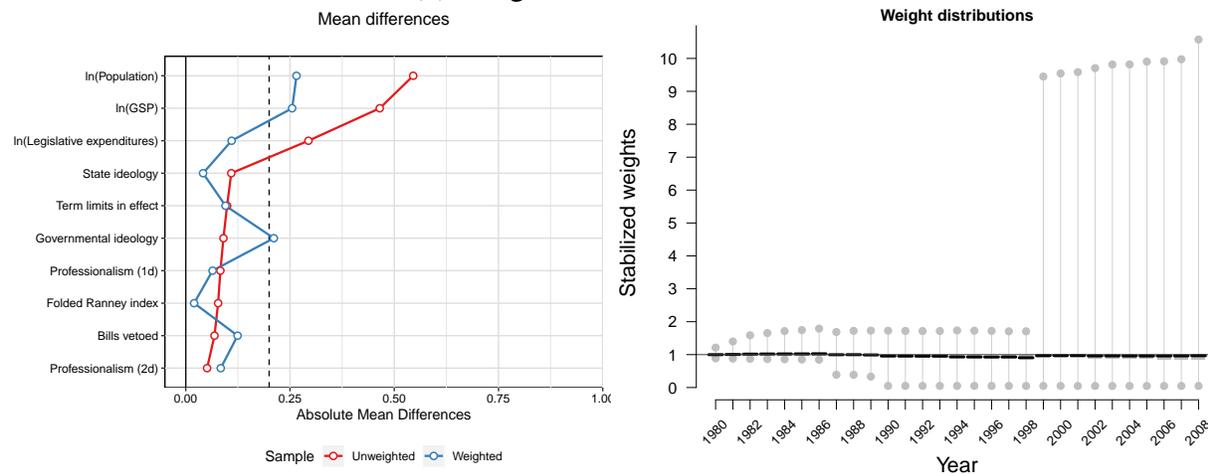
Figure SI-6: Overimputation Results for the Proportion of Bills Enacted Data
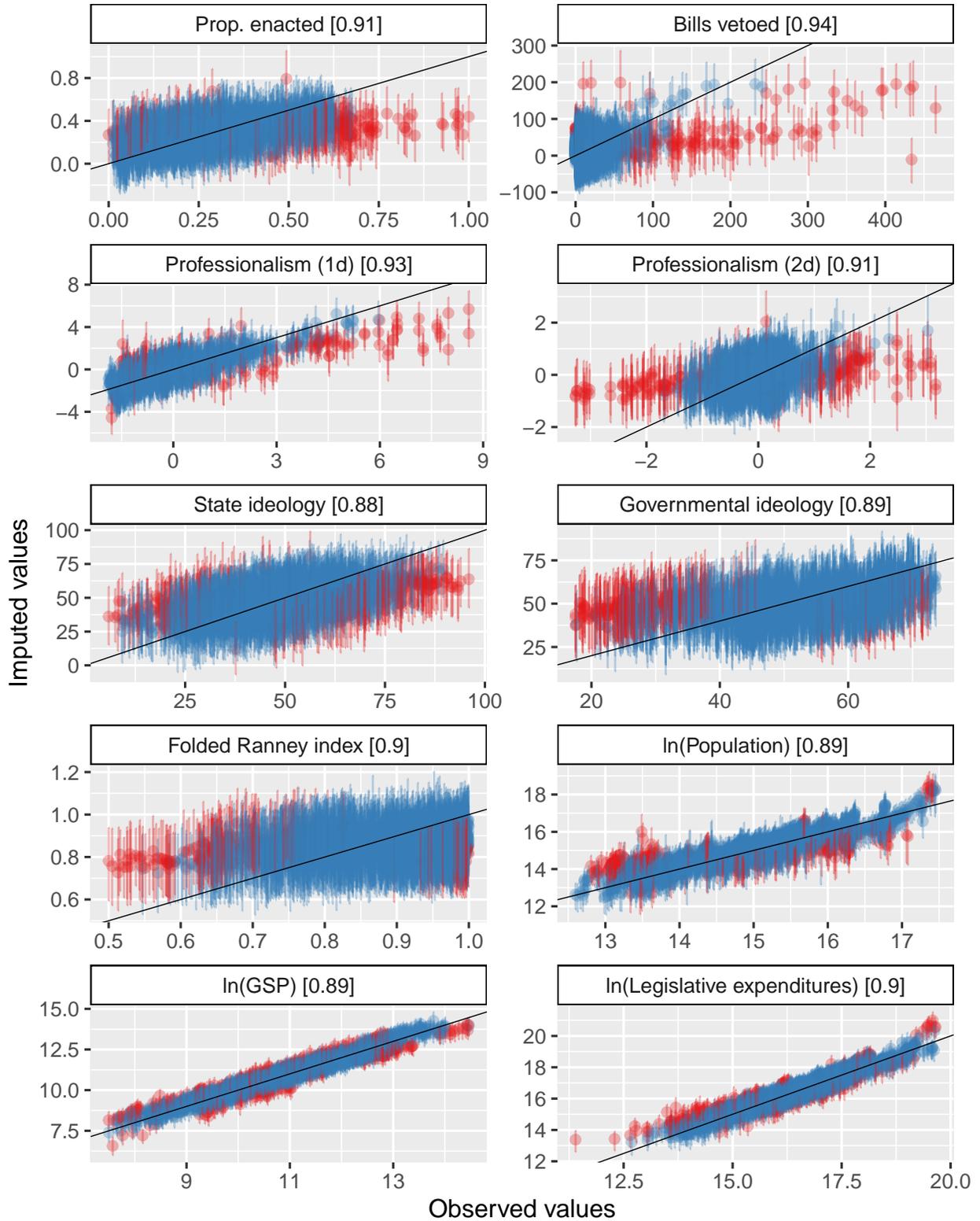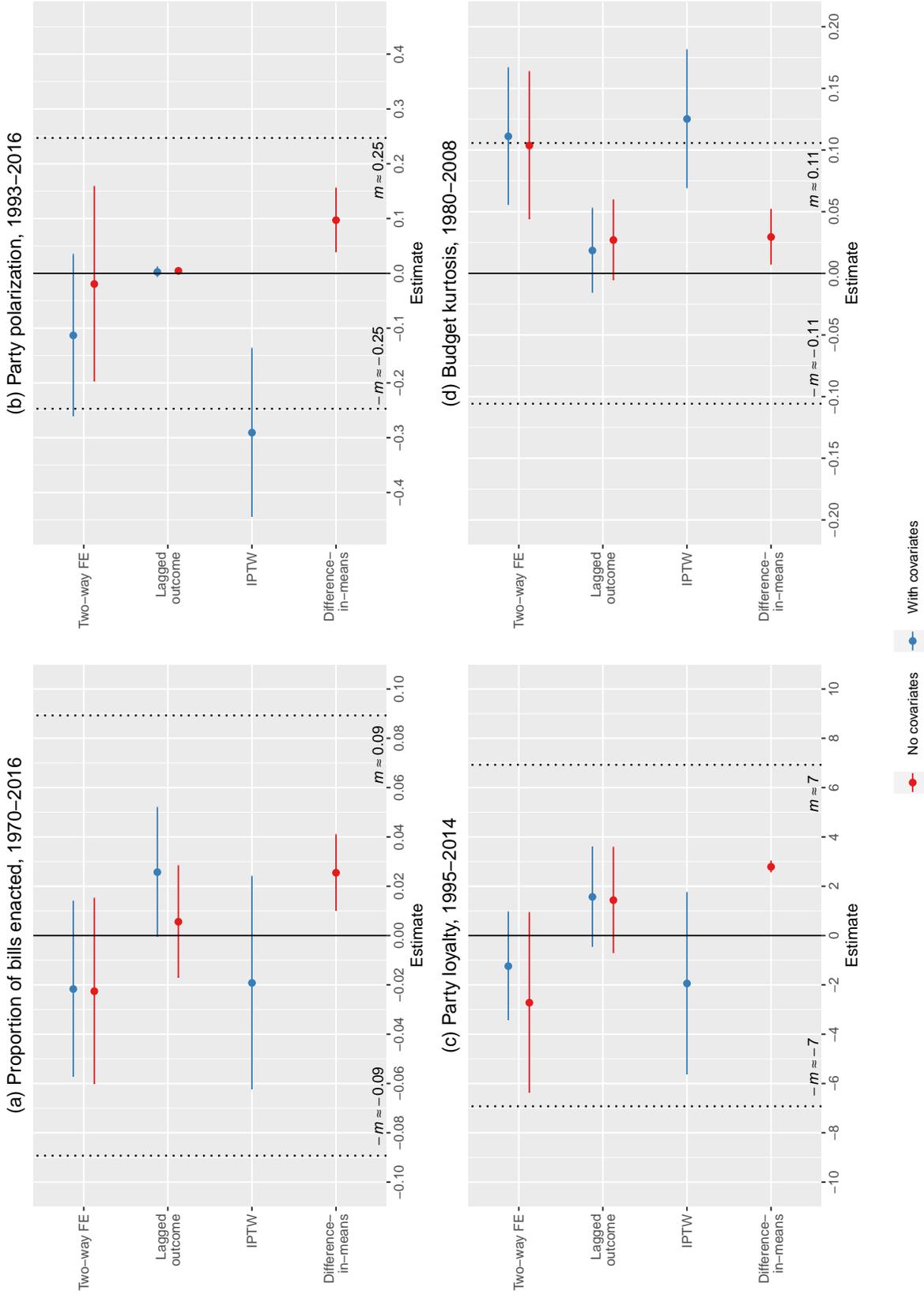
Note: The graphs present observed values of each variable on the x-axes against mean imputations of those values on the y-axes. Line segments indicate 95% confidence intervals. The solid line serves as a reference point for perfect imputation. The values in square brackets next to each variable label refer to the actual coverage level for that variable.

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Figure SI-7: Observed and Imputed Densities for the Proportion of Bills Enacted Data
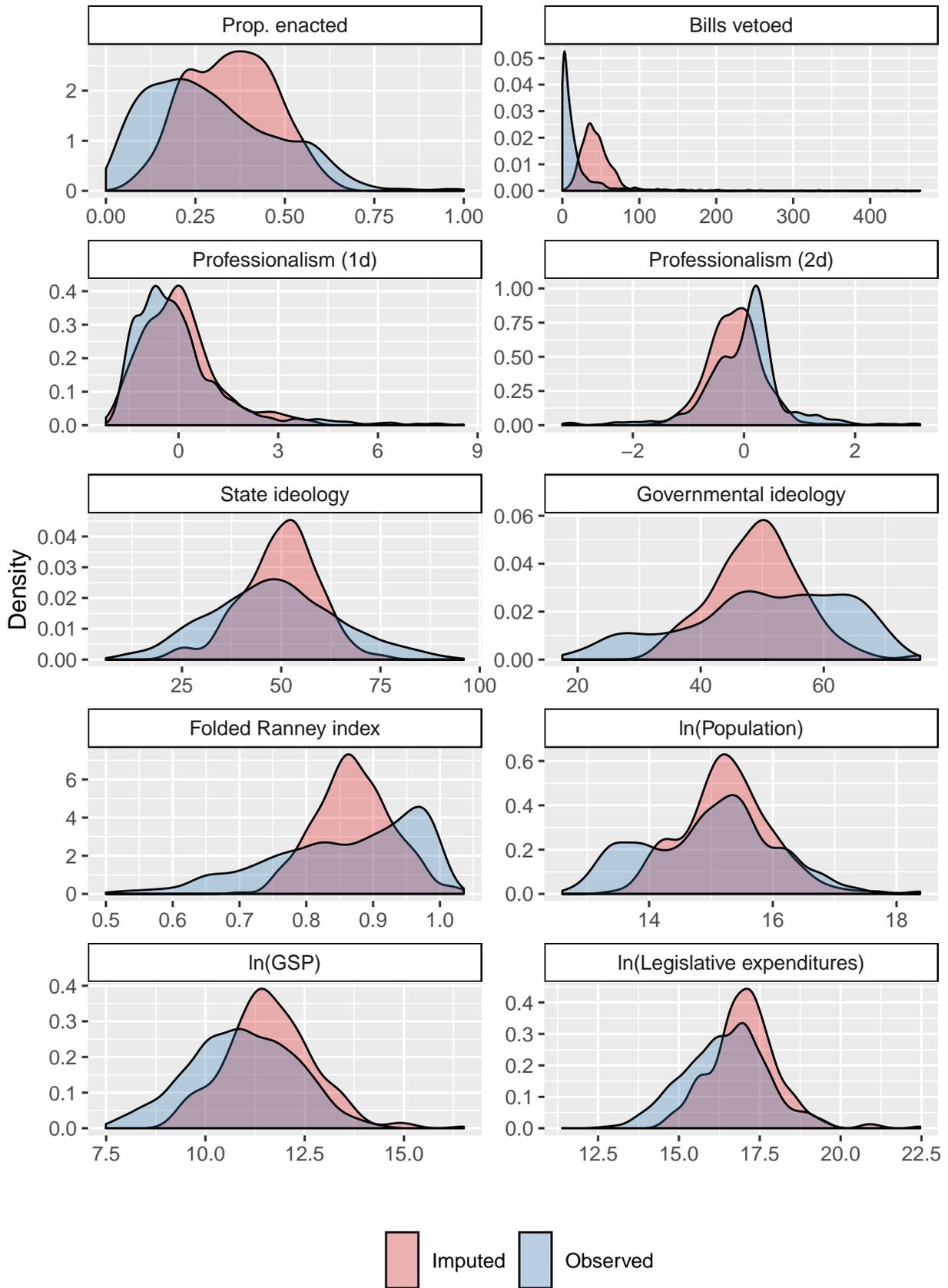

Note: The graphs present density plots of the observed and mean imputed values for each variable.

Figure SI-8: Estimated Effects Using Listwise Deletion for Missing Data

(a) Proportion of bills enacted, 1970–2016

(b) Party polarization, 1993–2016

(c) Party loyalty, 1995–2014

(d) Budget kurtosis, 1980–2008

With covariates

No covariates

Note: The graphs present the estimated effects of exposure to sunshine laws and their 95% confidence intervals. The dotted vertical lines denote $\pm m$, our chosen threshold for a substantively meaningful effect. Only one IPTW specification is shown because that estimator requires covariates for weight estimation.

Figure SI-9: Power Simulations of Hypothetical Treatment Effects
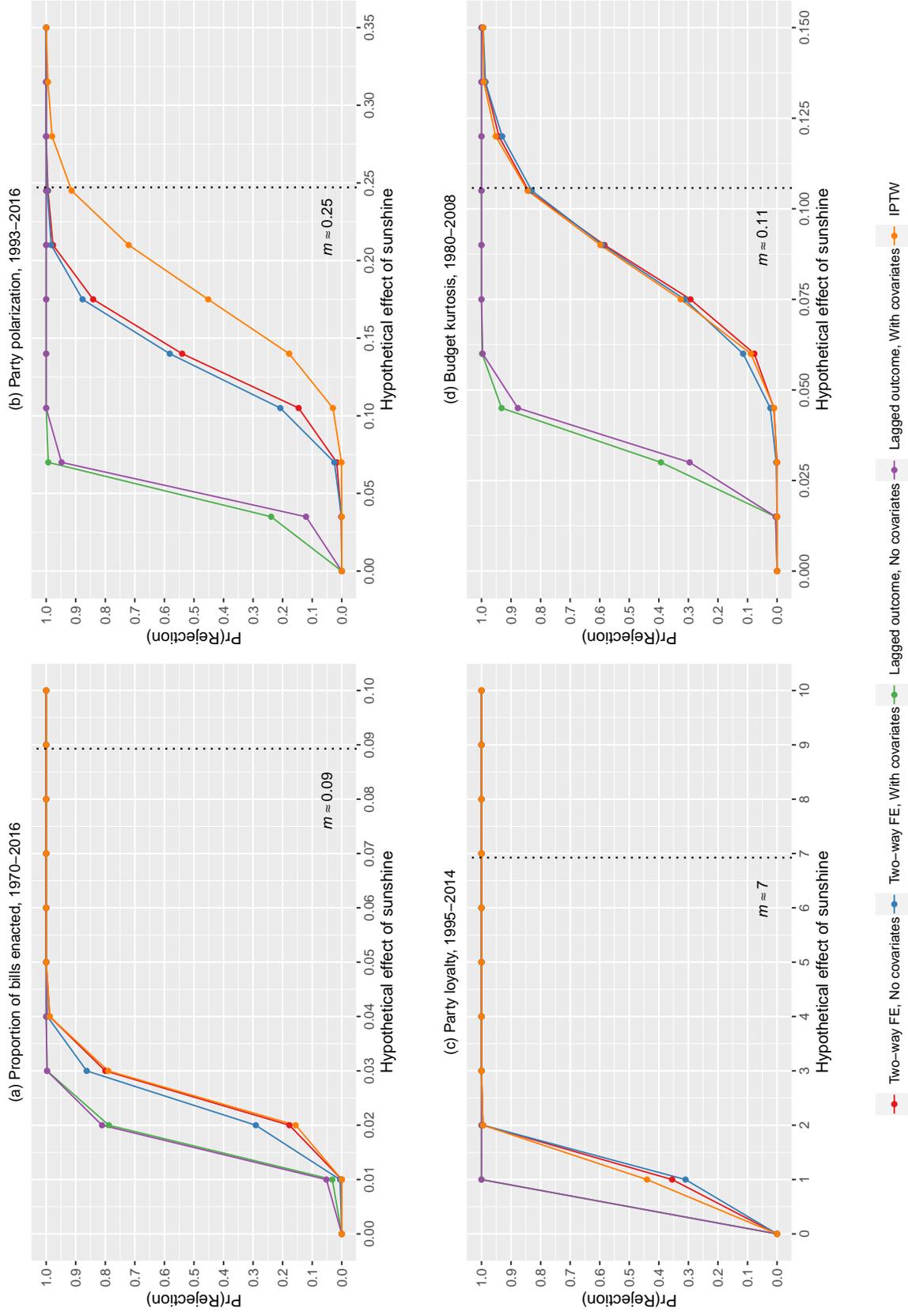
Note: The graphs present the hypothetical treatment effect range on the x-axes and the probability of rejecting the null hypothesis of no effect ($p < 0.05$) on the y-axes using the DGPs from each model and for each dataset. The dotted vertical lines denote m, our chosen threshold for a substantively meaningful effect.

Table SI-1: State Sunshine Laws and Legislative Exemptions

State	Sunshine Law	Year Enacted	Exemption Statute	Exemption Year
Alabama	Alabama Open Meetings Act	1975	–	–
Alaska	Alaska's Open Meetings Act	1959	*Abood v. League of Women Voters and Anchorage Daily News*	1987
Arizona	The Open Meetings Law	1962	–	–
Arkansas	Arkansas Freedom of Information Act	1967	–	–
California	Government Code Sections 9027-9031	1989	–	–
Colorado	Colorado Constitution Article V, 14	1876	–	–
Connecticut	Connecticut Freedom of Information Act	1975	–	–
Delaware	Freedom of Information Act	1977	29 Del. C. 10002(c)	1977
Florida	The Sunshine Amendment: Art. I, sec. 24(b), Fla. Const	1993	–	–
Georgia	Open Meetings Act	1988	–	–
Hawaii	Hawaii Sunshine Law	1975	Statute 92-10	1975
Idaho	Idaho's Open Meeting Law	1974	Statute 67-2341(4)	1998
Illinois	Ill. Const. Art. II, 14	1818	–	–
Indiana	Open Door Law	1977	–	–
Iowa	Open Meetings Law	1967	Iowa Code. 21.2	1967
Kansas	Kansas Open Meetings Act	1972	–	–
Kentucky	Open Meetings of Public Agencies Act	1974	Statute 61.810(1)(i)	1974
Louisiana	Open Meeting Law	1952	–	–
Maine	Freedom of Access Act	1959	–	–
Maryland	Open Meetings Act	1977	–	–
Massachusetts	Open Meetings Act	1958	G.L. c. 30A, 18(e)	2009
Michigan	Open Meetings Act	1976	–	–
Minnesota	Open Meetings Law	1957	–	–
Mississippi	Open Meetings Act	1975	Code Ann. 25-41-3(a)	1975
Missouri	Sunshine Law	1973	–	–
Montana	Const. Article V, 13	1889	–	–
Nebraska	Nebraska Open Legislative Committees Law	1998	–	–
Nevada	Const. Article 4, Section 15	1864	–	–
New Hampshire	Right to Know Law	1967	RSA 91-A2	1967
New Jersey	Open Public Meeting Act	1975	–	–
New Mexico	Open Meetings Act	1959	Statute 10-15-2(A)(B)	1978
New York	Open Meetings Law	1976	N.Y. Pub. Off. Law 108(2) (a)	1976
North Carolina	Open Meetings Law	1971	Statute 143-318.18	1979
North Dakota	Open Meetings Law	1974	–	–
Ohio	Ohio Const. Art. II, 13.	1851	Ohio Rev. Code 101.15	2002
Oklahoma	Open Meeting Act	1959	25 O.S. 304.1	1977
Oregon	Public Meetings Law	1973	37 Op Atty Gen 1087, 1089	1976
Pennsylvania	Sunshine Act	1987	Statute 712	1998
Rhode Island	Open Meetings Law	1976	R.I. Gen. laws 42-46-2 (3)	1976
South Carolina	Open Meetings Law	1974	–	–
South Dakota	Const. Art. III, 15	1889	–	–
Tennessee	Open Meetings Law	1974	–	–
Texas	Open Meetings Act	1972	–	–
Utah	Open Meetings Act	1955	Utah Code Ann. 52-4-103(7)(a)-(b).	1977
Vermont	Public Meetings Law	1976	1 V.S.A. 313(c).	1979
Virginia	Freedom of Information Act	1968	–	–
Washington	Open Public Meetings Act	1971	–	–
West Virginia	Open Meetings Act	1975	–	–
Wisconsin	Open Meetings Law	1959	Statute 19.87	1975
Wyoming	Public Meeting Law	1973	Statute 16-4-402(a)(ii)	1977

Table SI-2: Two-way Fixed Effects and Lagged Outcome Model Results

Variable	Proportion of bills enacted				Party polarization				Party loyalty				Budget kurtosis			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Sunshine	−0.015 (0.018)	−0.008 (0.015)	0.011 (0.013)	0.028* (0.012)	0.019 (0.099)	−0.013 (0.096)	0.016 (0.017)	−0.022 (0.025)	−0.668 (2.745)	−0.550 (2.523)	1.541 (1.062)	1.539 (1.066)	0.104* (0.030)	0.107* (0.022)	0.027 (0.017)	0.016 (0.016)
Outcome$_{t-1}$			0.607* (0.053)	0.376* (0.062)			0.875* (0.049)	0.796* (0.064)			0.457* (0.059)	0.431* (0.054)			0.131* (0.029)	0.122* (0.029)
Bills vetoed (100s)		0.019 (0.012)		0.032* (0.012)		0.006 (0.032)		0.044 (0.031)						−0.005 (0.021)		−0.003 (0.021)
Professionalism (1d)		0.006 (0.007)		0.011 (0.006)		0.024 (0.017)		0.012 (0.012)		0.371 (0.369)		0.206 (0.341)		−0.018 (0.014)		−0.010 (0.009)
Professionalism (2d)		0.012 (0.008)		0.021* (0.008)		0.011 (0.028)		0.004 (0.019)		0.848* (0.409)		0.143 (0.494)		−0.012 (0.016)		−0.011 (0.008)
State ideology		−0.093 (0.060)		−0.177* (0.042)		−0.001 (0.224)		−0.038 (0.108)		0.494 (4.281)		0.272 (1.790)		−0.007 (0.126)		−0.026 (0.055)
Governmental ideology		−0.025 (0.031)		−0.048 (0.040)		−0.155 (0.124)		−0.080 (0.099)		−0.419 (3.340)		0.318 (2.158)		0.007 (0.079)		−0.002 (0.073)
Folded Ranney index		−0.184* (0.044)		−0.081 (0.047)		0.314 (0.211)		0.244 (0.156)		4.744 (2.930)		0.538 (2.189)		0.056 (0.055)		0.078* (0.039)
Term limits in effect		0.025 (0.022)		0.051* (0.024)		0.184* (0.081)		0.071 (0.036)		1.288 (0.674)		0.131 (0.715)		−0.037 (0.036)		0.020 (0.030)
ln(Population)		−0.004 (0.023)		−0.053* (0.011)		−0.072 (0.086)		−0.028 (0.052)		−3.105* (0.797)		−3.128* (1.270)		0.072 (0.118)		0.007 (0.020)
ln(GSP)		−0.007 (0.019)		0.046* (0.016)		0.118 (0.079)		0.094 (0.054)		2.349* (0.707)		2.575 (1.395)		0.002 (0.102)		0.014 (0.023)
ln(Legislative expenditures)		−0.018 (0.019)		−0.064* (0.014)		−0.023 (0.060)		−0.060 (0.031)		0.792 (0.679)		1.428 (0.795)		0.074 (0.040)		0.001 (0.019)
State Fixed Effects	✓	✓			✓	✓			✓	✓			✓	✓		
Year Fixed Effects	✓	✓			✓	✓			✓	✓			✓	✓		
Legislator Fixed Effects	–	–	–	–	–	–	–	–	✓	✓	–	–	–	–	–	–
Upper Chamber Indicator	–	–	–	–	–	–	–	–	✓	✓	–	–	–	–	–	–
Adjusted R^2	0.621	0.368	0.466	0.757	0.770	0.755	0.767	0.414	0.416	0.211	0.224	0.055	0.057	0.021	0.023	0.603
N	2,350	2,350	2,350	2,350	1,200	1,200	1,150	1,150	70,196	70,196	48,266	48,266	1,450	1,450	1,400	1,400

Note: Cell entries report regression coefficients with standard errors multiway clustered by state and year (in parentheses). For each outcome, model (1) is two-way fixed effects with no covariates, model (2) is two-way fixed effects with covariates, model (3) is lagged outcome with no covariates, and model (4) is lagged outcome with covariates. There is no sample size loss in the bill enactment lagged outcome models because the outcome data include the year 1969. Estimates for state ideology and government ideology are multiplied by 100 to enhance readability. * $p < 0.05$ (two-tailed).

Table SI-3: IPTW Model Results

Variable	Bill enactment	Party polarization	Party loyalty	Budget kurtosis
	Stage 1: Weighting Models			
Intercept	−11.374	−17.609	−26.501*	−20.979
	(6.870)	(23.122)	(1.494)	(13.489)
Sunshine$_{t-1}$	6.740*	12.515*		11.777*
	(0.548)	(4.152)		(1.678)
Cumulative sunshine	−0.062	0.629	0.559*	0.212
	(0.090)	(0.504)	(0.010)	(0.142)
Bills vetoed	0.002	−0.011		0.007
	(0.004)	(0.015)		(0.006)
Professionalism (1d)	−0.209	−0.118	−0.899*	−0.419
	(0.217)	(0.740)	(0.041)	(0.441)
Professionalism (2d)	0.147	2.142	4.848*	0.627
	(0.291)	(1.179)	(0.115)	(0.514)
State ideology	0.005	−0.071	0.021*	−0.013
	(0.014)	(0.067)	(0.004)	(0.033)
Governmental ideology	0.007	0.122	0.017*	0.089
	(0.018)	(0.094)	(0.003)	(0.047)
Folded Ranney index	3.912*	28.721	10.158*	8.206
	(1.800)	(18.003)	(0.399)	(4.788)
Term limits in effect	1.034	−1.668	−0.816*	−0.839
	(1.158)	(1.952)	(0.083)	(1.321)
ln(Population)	1.034	3.199	3.497*	1.582
	(0.676)	(4.564)	(0.205)	(1.556)
ln(GSP)	0.050	−1.075	0.418*	0.354
	(0.807)	(3.783)	(0.200)	(1.957)
ln(Legislative expenditures)	−0.758	−3.072	−2.727*	−1.459
	(0.492)	(2.525)	(0.113)	(1.070)
Upper chamber			0.137*	
			(0.061)	
Time	0.059	−0.268	−0.844*	0.176
	(0.095)	(0.494)	(0.032)	(0.183)
Time^2	−0.002	−0.005	0.16*	−0.006
	(0.002)	(0.020)	(0.001)	(0.006)
Sunshine$_{t-1}$ ×	0.182*	0.122		−0.163
Cumulative sunshine	(0.092)	(0.369)		(0.147)
	Stage 2: Treatment Effects			
Sunshine	−0.014	0.048	−1.907	0.094*
	(0.018)	(0.104)	(2.316)	(0.036)
State Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Legislator Fixed Effects	–	–	✓	–
Upper Chamber Indicator	–	–	✓	–
N	2,350	1,200	70,196	1,450

Note: Cell entries report coefficients with standard errors (in parentheses). The top panel (stage 1) reports logistic regression weighting models. The bottom panel (stage 2) reports treatment effects from a marginal structural model (MSM). Weights generated from the stage 1 models were used in estimation of the stage 2 treatment effects. Sunshine$_{t-1}$ and Sunshine$_{t-1}$ × Cumulative sunshine are omitted from the party loyalty model due to singularities. * $p < 0.05$ (two-tailed).