Does Transparency Inhibit Political Compromise?

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Abstract

Governments around the world face an apparent tension when considering whether to allow public access to the governing process. In principle, transparent institutions promote accountability and good governance. However, politicians and scholars contend that such reforms also constrain politicians' capacity to negotiate and compromise, producing inefficiency and gridlock. This argument—that transparency inhibits compromise—is widely accepted, but rarely empirically tested. We develop a theoretical framework around the claim and evaluate it in the context of American state legislatures. We leverage temporal variation in state "sunshine law" adoptions and legislative exemptions to identify the effects of transparency on several observable indicators of compromise: legislative productivity, polarization, partisanship, policy change, and budget delay. Our analyses generally do not support the argument; we mostly report precisely-estimated negligible effects. Thus, transparency may not be the hindrance to policymaking that conventional wisdom suggests. Effective governance appears possible in state legislatures even under public scrutiny.

Keywords: Transparency; Sunshine laws; Open meetings requirements; Legislative decision-making; 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, *Other People's Money and How the Bankers Use It*, 1914.

"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 included a provision requiring the legislature to end its exemption from Massachusetts' "open meetings" law. 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). These legislators contended that compromise is necessary for efficient lawmaking, but not feasible if constituents can see it. Their perspective on transparency has endured among political leaders around the world for centuries—especially those in democracies who face pressure to maintain accountable governing institutions. Tony Blair, for instance, famously regretted his role in passing Britain's 2000 Freedom of Information Act (Berliner 2014, 479). The framers of the United States Constitution claimed that their debates would have ended in failure had they been open to the public (Anderson, Butler, and Harbridge-Yong 2020, 125). Moreover, politicians are not alone in making the argument. Numerous scholars and observers of American politics, for example, contend that the 1970s transparency reforms in Congress exacerbated partisanship and gridlock, rendering negotiation and compromise more difficult (Congressional Research Institute 2019).

Despite the widespread acceptance of this claim, almost no research has directly assessed whether it holds systematic empirical support. Are politicians actually constrained in their capacity to politick and negotiate under open governance requirements? In this research, we address that question in the context of American state legislatures. Currently, 21 states exempt their legislatures from so-called "sunshine laws," citing facilitation of the legislative process as one justification (Diana 2014; La Corte 2018). We test this logic directly, examining whether public access to legislative deliberation hinders several observable indicators of political compromise: productivity, polarization, partisanship, policy change, and budget delay. Our empirical results are limited to these specific measures in the United States, but the normative implications of our investigation are potentially quite important for democratic reform efforts more broadly. 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 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 empirical predictions from this theory, then test them by leveraging temporal variation in the adoptions and legislative exemptions of states' transparency requirements for legislative meetings. We employ novel data and multiple modeling strategies to identify the effects of "exposure to sunshine" on our indicators of political compromise in state legislatures. The results suggest that legislators' concerns about the stifling consequences of transparency might be overblown; we mostly find precisely-estimated negligible effects. Thus, we conclude that politicians may not actually lose their capacity to compromise under public scrutiny. Legislative deliberation—as we are able to measure it in the American states—appears to function about the same under open or closed proceedings.

2 The Political Costs of Transparency

Transparency in government initially 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.[1] Public officials generally

[1]Research on open governance includes many topics beyond the claim that it inhibits compromise. For instance, this work evaluates the effects of transparency on citizens' trust in government (Tolbert and Mossberger 2006), political attitudes and behavior (Piotrowski and Van Ryzin 2007), governmental responsiveness (Berliner et al. 2019), state fiscal health (Harden, Kirkland, and Shea 2020), and corruption (Cordis and Warren 2014). One particularly important line of inquiry focuses on how and why states and countries adopt open information requirements (e.g., Alt, Lassen,

adopt this logic, but it clearly reflects conventional wisdom in scholarly accounts as well. In the supporting information (SI) we summarize more than 20 studies from political science and economics that state, imply, or assume that open negotiation hinders the prospects for compromise (see page 1).

A sizable portion of the scholarship on the costs of transparency comes from formal models of bargaining and negotiation. For instance, in a 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 "[a]lthough there may be benefits to 'sunshine laws' and other measures to make negotiations open... [transparency] may actually harm efficiency" (114). Patty (2016) conveys similar sentiment: "bargaining in private might be preferable from the standpoint of the voter's welfare because it eliminates any potential gain [to legislators] from [obstructing legislation]" (187). A variety of other such works make similar contentions about transparency, particularly regarding the incentives of career-minded bargainers (e.g., Stasavage 2004, 2007; Fox 2007; Fehrler and Hughes 2018; Benesch, Bütler, and Hofer 2018).

A prominent empirical example supporting this conventional wisdom is Anderson et al.'s (2020) book, *Rejecting Compromise*. The authors investigate why American state legislators and local officials hesitate to compromise, even when it would move policy closer to their preferred points. Their conclusion—drawn from surveys, interviews, and experiments—is that legislators often reject compromise because of fear that it will motivate primary election challengers. They demonstrate in survey experiments that legislators are more likely to accept compromise under closed door negotiations, because doing so offers political cover and the chance to explain the final outcome (although they also find that legislators are concerned about the reduced accountability of closed meetings). They note that "sunshine laws... give legislators less room to maneuver" (107) and ultimately conclude that "one way to increase legislators' willingness to accept compromise is to insulate the process of negotiation from the public" (146).

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and Rose 2006; Berliner 2014; Berliner and Erlich 2015).

In short, the conventional wisdom—that transparency inhibits compromise—can be found in politicians' own reactions to open governance requirements, formal theories of bargaining, and survey experiments that present hypothetical scenarios to state legislators. However, support for this claim only indicates that many politicians and observers *believe* that open negotiations harm compromise. From an empirical standpoint the question of whether transparency actually inhibits observable indicators of compromise largely remains unanswered. Addressing this question is important for the literature on transparency because research on the decision to adopt open governance reforms (see note 1) often 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 modern United States, a sizable minority of citizens actually prefers representatives who are open-minded and willing to consider the other side's perspective (see also Wolak 2020). 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 the role of transparency in legislative politics. 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 monitoring the agents' efforts and/or 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 engage in some monitoring to prevent corruption or shirking, but not so much that they 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 expertise: 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 for citizens—either directly and/or indirectly via journalists and interest groups who closely observe state politics. Specifically, transparency requirements provide citizens with information on legislative negotiations and deliberation that was previously private. Such information may reveal, for example, that a particular policy solution preferred by citizens was considered, but dropped from the discussion. Due to their own lack of expertise, citizens may not fully understand or appreciate *why* their representatives made certain choices during deliberation and sanction them for what the citizens view as capitulating to the other side. In contrast, legislators would prefer to reveal only the final outcome of deliberations, because doing so gives them control of the message and the ability to explain their decisions (see Grose, Malhotra, and Van Houweling 2015; Anderson et al. 2020). In short, lawmakers in transparent legislatures must worry more about how constituents view their behavior in policy negotiations, which constrains their ability to strike deals and develop winning coalitions. We expect that the consequences of this reduced capacity appear in several aggregated legislative outcomes.

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 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 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.

Our theory also implies that open governance leads to increased partisanship in the chamber. If constituents oppose legislative compromise, a safe choice for lawmakers is to simply fall in line with the party. 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 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 (Anderson et al. 2020).

Moreover, there is additional nuance to consider regarding the bills that receive a floor vote. 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 party polarization in roll call votes than legislatures without transparency requirements.

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

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 year-to-year spending differences. This consistent pattern in public budgets provides an additional means by which we can look for evidence of compromise 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.

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 for passing a budget 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 still requires negotiation and deal-cutting within the legislature. Accordingly, delay in the budgeting process is a clear indicator that compromise is not viable. Specifically, we posit that legislators who are constrained in their capacity to politick 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.

3.1 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 motivation 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 transparency 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 of their deliberations, then the corruption-fighting, illuminative effects of transparency may not be realized either. 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.

4 Research Design

The primary goal of our empirical analyses is to identify the effects of exposure to sunshine laws on observable indicators of compromise in state legislatures. Sunshine laws include a variety of different transparency rules for state government, including the well-known requirements of open access to government records (i.e., freedom of information). Here we focus specifically on *open meetings requirements*—provisions that guarantee public access to governmental meetings and proceedings. Since 1998, every state has had such a requirement on the books, but many states have fully or partially exempted their legislatures from them at some point in time. Consider the contrast between California's and Massachusetts' open meetings requirements as an illustrative example. The former state's law appears in California Government Code Sections 9027-9031 and states the following:

> Except as otherwise provided, all meetings of a house of the Legislature or a committee thereof shall be open and public, and all persons shall be permitted to attend the meetings. [The term] "meeting" means a gathering of a quorum of the members of a house or committee in one place for the purpose of discussing legislative or other official matters within the jurisdiction of the house or committee.

California does not have a legislative exemption to this requirement, and the "except as otherwise provided" clause refers to either personnel discussions (hiring and firing decisions) or threats to the safety and physical integrity of the legislature. Policy discussions cannot happen behind closed doors. With respect to enforcement, California grants its courts the power to provide injunctions and even invalidate decisions made in violation of the law.

In contrast, Massachusetts' analogous law (General Laws Part I, Title III, Chapter 30A, Sections 18-21), despite containing similar language, places a much different transparency expectation on the state legislature. It states that: "Except as provided in Section 21, all meetings of a public body shall be open to the public." Section 21 specifies that exemptions to this law only apply to meetings to discuss personnel. However, Section 18 of this same law defines a "public body" as follows:

> ...a multiple-member board, commission, committee or subcommittee within the executive or legislative branch or within any county, district, city, region or town...provided, further, that [a public body] shall not include the General Court [state legislature] or the committees or recess commissions thereof.

In short, the law requires openness on the part of essentially every organizational unit within state government as well as local governments, but exempts the legislature itself from this requirement. Journalists, interest groups, citizens, and other interested parties cannot witness the details of legislative proceedings in Massachusetts, whereas in California such observation is relatively easy. Based on our theory described above, we expect that this difference in the legislature's potential audience changes the bargaining environment for its members, making negotiation and compromise more difficult in a state like California compared to Massachusetts.

4.1 Coding the Treatment Variable

Our main treatment is the presence of an open meetings requirement that applies to a state's legislature in a given year. This coding decision assumes any effect of transparency occurs immediately, which we view as reasonable because these requirements are closely monitored in state politics. Journalists pay attention to public access because it helps them complete their work. Additionally, news coverage of attempts to enact or repeal legislative exemptions consistently indicates that legislators are also quite attuned to whether their proceedings are open or closed (Diana 2014). Accordingly, any change in legislative behavior is likely to occur sooner rather than later.[2]

States are considered untreated if they have not adopted open meetings *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 these laws as well as the legislative exemption dates,

[2] In the SI (pages 5 and 9) we relax this assumption and consider the possibility of effects that develop over time with 1–4 year lags of treatment and a cumulative treatment effect. The results of these analyses are consistent with what we report below.

if applicable. In some cases, only certain groups within the legislature received exemption. For our main treatment variable we coded a state-year as exempt if *any* of the following specific groups were exempt.[3] These groups reflect venues that could—to varying degrees—provide legislators with the opportunity to negotiate and/or contain information about legislative deliberation.[4]

1. Subcommittees;
2. Committees outside of Committees of the Whole;
3. Partisan caucuses;
4. Conference committees;
5. Standing political committees;
6. Ethics committees;
7. Political parties.

This binary coding is conceptually simple, but may obscure important nuance in states' transparency requirements. Accordingly, we also consider two other treatment variables: (1) a count (ranging 0–7) of the groups as categorized above that were open in a state-year and (2) a binary indicator for whether standing political committees were open. The former variable better measures the heterogeneity among states that partially open their proceedings to the public. The latter measures the transparency of the legislative group in which most bargaining and negotiation over policy occurs (Kingdon 1973).

4.2 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

[3] See SI page 1 for complete details of our coding decisions.

[4] While collecting these data we observed exemptions to parts of the legislature in a variety of combinations. For example, one law grants exemptions to "Standing political committees, conferences, or caucuses," while another exempts "political parties, groups, caucuses, rules, or sifting committees." Some states distinguish between exemptions for meetings of a party caucus (just the sitting legislative members of the party) or the political party itself (which might include members of the party organization outside the legislature). This final list represents the distillation of the groups that receive exemptions in at least one open meetings law.

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.[5]

More specifically, we consider 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. We measure all of them over time using available data as well as original collection efforts. Table 1 summarizes these variables.

[Insert Table 1 here]

First, aggregated productivity in lawmaking is a key observable implication of lawmakers' discretion to compromise. Our first empirical measure is the proportion of substantive (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).[6] This approach is common in legislative scholarship (e.g., Squire 1998; Rogers 2005; Crosson 2019). 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.[7]

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).[8] Furthermore, while we gain considerable analytic

[5]We also combined these measures in a latent measure of compromise with confirmatory factor analysis and modeled the factor scores as described below. The results are substantively similar to what we report here (see SI pages 9–11).

[6]Some state legislatures met biennially during portions of this time period. However, in practice some met in "flexible" sessions over consecutive years (National Conference of State Legislatures 2019), and thus we include all state-years in our sample. Our results are unchanged if we omit the off years of biennial states.

[7]It is important to note that aggregate productivity in legislation is distinct from the *quality* of that legislation. Politicians' and scholars' primary argument against transparency is not that it makes legislation worse in quality, but that it causes legislators to become unwilling to compromise with opponents on either "good" or "bad" legislation. Thus, bill enactment is an appropriate measure for testing our theory.

[8]Indeed, Congress scholars have attempted to create measures of the "important" set of bills passed by Congress

leverage by examining state legislatures over multiple 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. The measure is also subject to manipulation through the legislative agenda. Party leaders may steer the session away from controversial topics to maintain the appearance of productivity in an otherwise gridlocked legislature.[9]

Accordingly, we also assess ideological polarization of the parties. When legislators have discretion to 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 capturing the *means* of conducting legislative business rather than just 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.

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. We posit that an open legislative process is more likely to select legislation that induces legislators to vote with their party more often, thus generating lower 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

each year (e.g., Mayhew 1991). However, even these measures are the subject of disagreement (Howell et al. 2000; Binder 2003).

[9]Another potential drawback to the measure might be that it masks changes in the volume of individual bills if transparent legislatures combine more bills into omnibus legislation. We consider this possibility in the SI (pages 7–8) and ultimately conclude that it is likely not a major threat to our inferences.

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. 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 our own data collection efforts, stems from the distribution of year-to-year percent changes in spending on 20 public policy categories from 1980 to 2017. A legislature that is highly status quo biased will see changes from one year's spending to the next tightly clustered around 0% with a few large outliers. 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 produces normally-distributed spending changes.

Finally, we analyze Klarner et al.'s (2012) indicator for a late state budget from 1961 to 2018. 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. 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).

4.3 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, party polarization, party loyalty, and budget kurtosis). Our modeling efforts on this group of variables reflect our own original design (which we detail below) to test H1–H4. 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 (see section 5.2), then add our own treatment variables to the specification and more data to the sample to test H5.

Our primary strategy for modeling the main outcomes leverages the timing of states' open meetings law adoptions and/or legislative exemptions with a two-way fixed effects estimator—a regression model with fixed effects for cross-sectional units (states or legislators) and time (years).[10] The method provides one means of isolating the independent influence of the treatment by estimating the effect of a within-state change in legislative exposure to open meetings 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 secular temporal trends in the outcomes. Of course, the method also has limitations. We discuss these limitations in the SI and demonstrate that our results are robust

[10]This approach is essentially a difference-in-differences (DID) design. However, it is not the canonical two-group/two-period DID case. Instead, the data include multiple units and time periods, variation in treatment timing and treatment turning "on" and "off" over time. Recent work demonstrates that these issues must be addressed carefully (Goodman-Bacon 2018; Imai and Kim 2019; de Chaisemartin and D'Haultfoeuille 2019). One particular concern is bias due to heterogeneous treatment effects. With multiple groups and time periods, there is variation in the weight of each individual two-group/two-period combination in the data and the weights can even be negative (see de Chaisemartin and D'Haultfoeuille 2019). This unequal weighting can bias the coefficient on the treatment variable unless the analyst assumes that the treatment effect is constant across groups and time. In the SI (pages 6–7) we relax this assumption and report results from two estimators—Imai and Kim's (2019) weighted fixed effects (WFE) and de Chaisemartin and D'Haultfoeuille's (2019) DID_M—that are robust to heterogeneous effects. These methods reduce our statistical power, but their estimates suggest no change to our substantive conclusions.

to several alternative estimators (see pages 3–7).

We include time-varying covariates in an attempt to further mitigate confounding of the treatment effects.[11] First, we control for the number of bills vetoed by the governor in a given state-year (Council of State Governments 2018). We expect that vetoes indicate an antagonistic bargaining environment, reducing legislators' willingness to compromise.[12] Next, we control for Bowen and Greene's (2014) two dimensions of legislative professionalism. The first dimension is primarily a measure of legislative salary while the second is largely session length conditional on salary (289). We expect legislators' interest in keeping their jobs increases in salary, leading them to choose ideological consistency for their constituents over compromise. However, a shorter period of time to work on legislative business may force more compromising behavior. We also include logged state expenditures on the legislature to fully account for differences in resources. Legislators with more resources likely develop more policy knowledge and, as a result, more specific policy preferences, which may reduce their interest in compromise.

We include state citizen and state government ideology (Berry et al. 1998) under the expectation that conservatives are, on average, particularly adverse to compromise (Harden and Kirkland 2018; Anderson et al. 2020). A folded Ranney index measures the level of competition between the two major political parties for control of the legislature (Klarner 2018). We expect that competitiveness produces more compromise. However, we posit that compromise declines when term limits are in effect because legislators with a finite time horizon may be less able (or willing) to facilitate an efficient process (Olson and Rogowski 2020). Finally, we include logged state population and gross state product in current dollars (Klarner 2018). We expect that states with large populations and/or economies have more policy demanders and complexity, yielding more opportunities for bargaining and logrolling in the legislature.

[11] We utilize multiple imputation to address missing data; all results presented below reflect the necessary adjustment to measures of uncertainty (Blackwell, Honaker, and King 2017). See the SI (pages 11–13) for complete details on the imputation procedures, including diagnostics and estimation results using listwise deletion, which produce no change to our substantive conclusions.

[12] Results are substantively identical throughout the analyses if we control for these dynamics with an indicator for divided government or the distance between majority party medians across the two legislative chambers.

4.4 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 and/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 $m \approx 0.09$ for the first outcome. A legislature must enact nine percentage points more or less legislation while exposed to sunshine compared to closed access before we consider the effect substantively meaningful. For party polarization (which ranges 0.20–3.44), party loyalty scores (1–100), budget kurtosis (-0.11–1), and budget delay (15% late) our choice is associated with $m \approx 0.25$, 7, 0.10, and 18%, respectively.

Of course, any value of m is somewhat arbitrary, and thus it is important to establish empirical context for our choice (Rainey 2014, 1085).[13] 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 25$^{\text{th}}$ percentiles, medians, and 75$^{\text{th}}$ 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 panel a).

[Insert Figure 1 here]

4.5 Statistical Power

In addition to substantive significance, it is important to consider our statistical power for detecting effects. To do so, we conducted power simulations of hypothetical treatment effects with the main treatment variable on the outcomes in the main category. We defined "true" treatment effects in each data generating process (DGP), simulated many fake datasets from those DGPs using

[13] We also assess the sensitivity of our results to the choice of m in the SI (pages 8–9).

the model output, then computed the proportion of times that the estimator rejects the null hypothesis of a zero treatment effect at the 0.05 level. We varied 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 the two-way fixed effects models' statistical power at the threshold for a substantively meaningful effect, as well as the smallest effect detectable at 95% power.[14]

[Insert Table 2 here]

Table 2 shows that the power at m is over 95% in all 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 less than 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 about 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, beginning with the main outcomes.

5 Results

Table 3 reports the two-way fixed effects model estimates with standard errors multiway clustered by state and year for the main outcomes. In each case model (1) includes no covariates and model (2) includes covariates. The third and fourth rows in gray denote 95% confidence intervals for the coefficients on the treatment variable—"Sunshine." We compare these bounds to each outcome's value of m, which we list at the top of each column.

[Insert Table 3 here]

These results indicate that the effects of open meetings laws are generally minimal, though with some heterogeneity.[15] The treatment effects for the first three outcomes are quite small and not statistically significant, providing no support for H1–H3. Opening legislative proceedings

[14] See the SI (pages 13–14) for a complete description of the power simulations as well as full results with alternative estimators.

[15] In the SI we further show that the results are robust to alternative modeling and measurement choices and stable over time (pages 3–11).

corresponds with a minuscule drop of about 1 percentage point in the proportion of bills enacted (H1), a small negative effect on polarization (H2), and a decrease of less than one point in party loyalty (H3). Most importantly, the 95% confidence intervals for these estimates are completely bounded by $\pm m$, establishing that they are all substantively negligible according to our definition.

The test of H4 provides a moderate exception to this pattern. Consistent with our theory, the budget kurtosis models yield positive treatment effects that reach statistical significance ($p < 0.05$). However, these estimates are not clearly *substantively* significant. The coefficients themselves fall below our threshold of 0.10, but the confidence intervals do reach slightly past that value (upper bounds of 0.117 and 0.107). Still, substantively significant *and* nonsignificant values are both plausible, so we cannot declare support for H4 with great conviction. Moreover, it is important to note that these results are model dependent. As we show in the SI (pages 4–5), treatment effect estimates from models with one-year lags of budget kurtosis are near zero with confidence intervals bounded by $\pm m$. Accordingly, the "bracketing property" of fixed effects and lagged dependent variable models (Ding and Li 2019) would suggest that the true effect likely falls inside the $[-m, m]$ interval. Nonetheless, the estimates in Table 3 on their own provide at least a glimmer of support for legislators' claims and our theoretical perspective.

5.1 Alternative Treatment Variables

Next, we consider the effects of our alternative treatment variables—a count of open legislative groups and an indicator for open standing committees. Table 4 presents two-way fixed effects models with covariates including each version of the treatment variable. Models (1)–(4) indicate that the effects of an additional open group on the main outcomes are substantively miniscule. Across all four outcomes, the treatment effect estimates and their confidence intervals all fall entirely inside $[-m, m]$. The effects of open standing committees show a bit more heterogeneity (models 5–8). All of the estimates fall within the bounds of substantive significance, although only the confidence intervals for the bill enactment and party loyalty effects are completely bounded by $\pm m$. The party polarization treatment effect is again negative and its upper confidence bound is less than m (i.e., no support for H2). The effect of open standing committees on budget kurtosis is nearly identical to the main treatment variable's effect: a statistically significant coefficient with substantively significant and nonsignificant values both plausible.

[Insert Table 4 here]

Overall, we find a general trend of minimal support for our first four hypotheses using three different measurement strategies for the treatment variable. All of the coefficient estimates are bounded by $\pm m$. Additionally, the one case among the tests of H1–H3 in which a confidence interval falls outside of $[-m, m]$ is in the opposite direction of the hypothesis (Table 4, model 6). Our results are the most optimistic regarding H4, but even then we can never rule out substantively negligible effects; in fact, they are quite plausible with all three treatment variables. The best evidence for H4 appears where we would be most likely to see support: the transparency of standing committees, where bargaining and negotiation is most likely to occur. However, the favorable results are essentially confined to some confidence interval values that are slightly larger than our threshold for substantive significance.

5.2 A Final Test: Sunshine and Budget Delay

Our last remaining task is to test H5. To do so we depart from the modeling strategy for the main category of variables to replicate and extend Klarner et al.'s (2012) analysis of late state budgets. The original sample covers the period 1961–2006; we updated these data through 2018. Klarner et al. (2012, hereafter, KPM) develop an account of budget delay that incorporates numerous institutional, political, and electoral factors (see 998–1000). 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.[16] We adopt this strategy as a baseline, then add our three treatment variables to it in succession. Table 5 reports the coefficients and standard errors from these models, estimated on the 1961–2018 sample.

[Insert Table 5 here]

[16]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.

In contrast to H5, the coefficients on Sunshine in models (2)–(4) of Table 5 indicate that open legislative meetings correspond with a *decrease* in the likelihood of a late budget. The estimate for the main treatment variable (model 2) is statistically distinguishable from zero ($p < 0.05$) and its inclusion improves model fit over KPM's original specification.[17] However, all three estimates represent substantively small effects. Recall that our threshold for a substantively significant effect—one-half of a standard deviation in the outcome—yields $m \approx 18$ percentage points in this case. Averaging over the other variables, model (2) indicates that requiring transparency in a legislature's proceedings corresponds to a drop of 3 percentage points in late budget probability. The 95% confidence interval for this estimate of $[-5.9, -1.0]$ is well within the bounds of $\pm m$.[18]

In sum, this additional analysis provides further evidence against the theoretical framework we set out to test. Just as with our prior analyses, we again find precisely-estimated negligible effects of transparency. 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 demonstrate from six decades of data on budget negotiations that transparency requirements play a limited role in shaping the timing of budget passage. Instead of increasing the likelihood of a late budget, as our theory predicts, these results suggest that open meetings law exposure exerts almost no change in the chance of reaching a budget agreement before the new fiscal year.

6 Conclusions

American state legislators defend their exemptions from sunshine laws by asserting that too much public oversight hinders 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 little empirical support for such a tension in several observable, aggregate outcomes that are likely related to compromise. These results contribute to a well-developed

[17] This pattern is also evident in the raw data. Approximately 19% of the untreated state-years include late budgets while only about 11% of the treated state-years' budgets were late ($p < 0.05$).

[18] Similarly, a standard deviation increase in open groups (model 3) and switching from closed to open standing committees (model 4) each correspond to a drop of 1 percentage point in probability, with confidence intervals that fall inside $[-m, m]$.

literature in political science and economics on transparency, bargaining with an audience, and compromise and fit into the broader landscape of scholarship on open governance as well. Indeed, dating back to Hobbes' (1651) critique of public oversight of monarchical decisions, scholars have long been interested in understanding why governments around the world would choose to disclose information to citizens and what implications that disclosure might yield. Importantly, this scholarship often directly states, implies, or assumes that transparency produces negative consequences for the actors involved. Our findings suggest that one particular cost—the effect on politicians' discretion to negotiate—may not be so consequential in practice.

Specifically, we develop a theory of how transparency inhibits political compromise that reflects conventional wisdom from politicians, scholars, and political observers. We test our expectations on five distinct outcome variables measured at two levels of analysis. We find a general trend of negligible estimates that provide minimal evidence in favor of our hypotheses; only certain versions of the budget kurtosis analysis offer any support for our theory. 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 analyses do not simply fail to reject the null, but rather yield precisely-estimated negligible 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. One possibility is the outcome measures we employ; none of them are perfect, and their disadvantages (see Table 1) might obscure any effect of transparency. Thus, there is clear motivation for future research that employs alternative strategies for measuring compromise (e.g., Anderson et al. 2020). Additionally, citizens' general lack of political engagement may play a role (Rogers 2017). Our theoretical case for opposition to transparency requires an attentive public to monitor legislators' discussions and punish compromising behavior. Yet despite journalists' best efforts, voters may not hold enough knowledge of their legislators' decisions to provide credible electoral or political sanctions.

Of course, we do not claim that transparency is necessarily a benign institution. Critics contend that it exerts other negative effects on legislative politics, such as expanding lobbyists' influence in policymaking (Lee 2019, 4). Rather, we simply maintain that the consequences of transparency for several observable, aggregated indicators of compromise may not always be what the conventional wisdom suggests. Politicians' ability to work on behalf of their constituents—as measured by productivity, polarization, and budget negotiations in the American states—does not appear hampered by public scrutiny. This conclusion is important for normative discussions about the design of political institutions. While efficiency may initially seem like a reasonable justification for limiting public access to governmental proceedings, our research uncovers essentially no supporting evidence in several examples of lawmakers' observed behavior.

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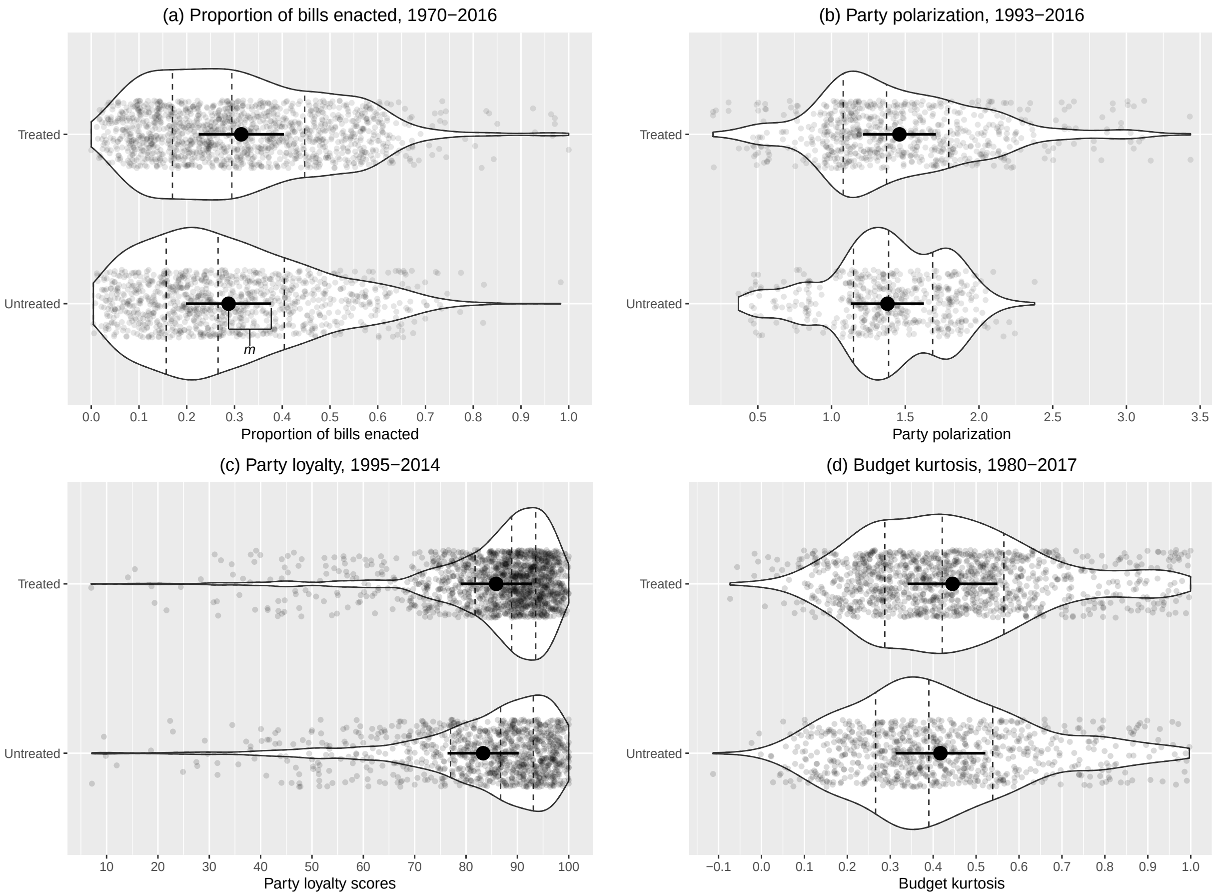
Figure 1: Main Outcome Variable Densities by Treatment Status



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 25$^{\text{th}}$ percentiles, medians, and 75$^{\text{th}}$ percentiles. The solid points are the means for each group (μ_{treated} and μ_{control}) and the solid horizontal lines represent $\{\mu_{\text{treated}}, \mu_{\text{control}}\} \pm m$.

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 values, 1980–2017.	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, 1961–2018.	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 polarization and partisanship measures omit Nebraska. Klarner et al.'s (2012) data exclude Alaska and Illinois (see 998).

Table 2: Power Simulation Results

Outcome	Covariates	Power at m	Smallest Effect with $\geq$ 95% Power
Bill enactment ($m \approx 0.09$)	No covariates	100%	0.038
	With covariates	100%	0.036
Party polarization ($m \approx 0.25$)	No covariates	99%	0.235
	With covariates	99%	0.225
Party loyalty ($m \approx 7$)	No covariates	100%	1.950
	With covariates	100%	1.950
Budget kurtosis ($m \approx 0.10$)	No covariates	96%	0.100
	With covariates	96%	0.100

Note: Cell entries report power simulations of hypothetical treatment effects in the two-way fixed effects models.

Table 3: Estimated Effects of Open Legislative Meetings on the Main Outcomes

	Bill enactment $m \approx 0.09$		Party polarization $m \approx 0.25$		Party loyalty $m \approx 7$		Budget kurtosis $m \approx 0.10$	
Variable	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Sunshine	−0.014	−0.006	−0.010	−0.028	−0.657	−0.462	0.077*	0.084*
	(0.019)	(0.015)	(0.099)	(0.111)	(2.325)	(2.112)	(0.021)	(0.011)
Sunshine 95% CI	[−0.051, 0.023]	[−0.036, 0.023]	[−0.206, 0.187]	[−0.247, 0.191]	[−5.214, 3.901]	[−4.601, 3.677]	[0.037, 0.117]	[0.062, 0.107]
Bills vetoed (100s)		0.018		0.015		−0.241		0.008
		(0.011)		(0.027)		(0.386)		(0.023)
Professionalism (1d)		0.003		0.009		0.469		−0.014
		(0.008)		(0.015)		(0.384)		(0.011)
Professionalism (2d)		0.011		0.001		0.925*		−0.009
		(0.008)		(0.019)		(0.385)		(0.011)
State ideology		−0.093		0.041		0.505		0.034
		(0.053)		(0.171)		(3.612)		(0.089)
Governmental ideology		−0.026		−0.184		−0.413		0.043
		(0.040)		(0.116)		(2.931)		(0.074)
Folded Ranney index		−0.188*		0.332*		4.633		0.019
		(0.042)		(0.160)		(2.482)		(0.053)
Term limits in effect		0.025		0.230*		1.294*		−0.031
		(0.024)		(0.086)		(0.571)		(0.026)
ln(Population)		0.002		−0.055		−3.067*		0.017
		(0.019)		(0.085)		(0.772)		(0.034)
ln(GSP)		−0.012		0.099		2.184*		−0.019
		(0.016)		(0.083)		(0.754)		(0.033)
ln(Legislative expenditures)		−0.017		−0.014		0.787		0.029
		(0.016)		(0.034)		(0.632)		(0.021)
State Fixed Effects	✓	✓	✓	✓			✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Legislator Fixed Effects					✓	✓		
Upper Chamber Indicator					✓	✓		
Adjusted R^2	0.600	0.618	0.762	0.777	0.414	0.416	0.060	0.060
N	2,350	2,350	1,176	1,176	70,081	70,081	1,900	1,900

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 and model (2) is two-way fixed effects with covariates. The third and fourth rows in gray denote 95% confidence intervals for the coefficients on Sunshine. * $p < 0.05$ (two-tailed).

Table 4: Estimated Effects with Alternative Treatment Variables

Sunshine defined as...	Count of open legislative groups				Open standing committees			
	Bill enactment $m \approx 0.09$	Party polarization $m \approx 0.25$	Party loyalty $m \approx 7$	Budget kurtosis $m \approx 0.10$	Bill enactment $m \approx 0.09$	Party polarization $m \approx 0.25$	Party loyalty $m \approx 7$	Budget kurtosis $m \approx 0.10$
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sunshine	−0.003	−0.028	0.332	0.014*	−0.026	−0.218	3.849*	0.088*
	(0.003)	(0.018)	(0.369)	(0.003)	(0.018)	(0.107)	(1.121)	(0.017)
Sunshine 95% CI	[−0.009, 0.002]	[−0.065, 0.010]	[−0.392, 1.056]	[0.008, 0.020]	[−0.062, 0.010]	[−0.446, 0.010]	[1.623, 6.075]	[0.055, 0.122]
Bills vetoed (100s)	0.018	0.016	−0.253	0.007	0.018	0.016	−0.245	0.007
	(0.011)	(0.025)	(0.481)	(0.022)	(0.011)	(0.027)	(0.651)	(0.024)
Professionalism (1d)	0.004	0.009	0.500	−0.015	0.004	0.010	0.470	−0.016
	(0.007)	(0.015)	(0.429)	(0.010)	(0.007)	(0.015)	(0.417)	(0.010)
Professionalism (2d)	0.011	0.002	0.935*	−0.009	0.011	0.003	0.895*	−0.009
	(0.008)	(0.020)	(0.398)	(0.011)	(0.008)	(0.020)	(0.398)	(0.011)
State ideology	−0.087	0.061	0.220	0.038	−0.085	0.067	0.205	0.041
	(0.053)	(0.164)	(3.586)	(0.085)	(0.053)	(0.161)	(4.291)	(0.093)
Governmental ideology	−0.028	−0.194	−0.187	0.044	−0.029	−0.196	−0.099	0.043
	(0.040)	(0.117)	(2.944)	(0.074)	(0.040)	(0.117)	(2.741)	(0.078)
Folded Ranney index	−0.184*	0.342*	4.482	0.022	−0.184*	0.347*	4.416	0.024
	(0.041)	(0.160)	(2.457)	(0.052)	(0.041)	(0.159)	(2.436)	(0.053)
Term limits in effect	0.027	0.236*	1.278*	−0.036	0.027	0.239*	1.269*	−0.035
	(0.024)	(0.086)	(0.595)	(0.026)	(0.024)	(0.086)	(0.581)	(0.026)
ln(Population)	−0.000	−0.054	−3.107*	0.020	−0.001	−0.055	−3.095*	0.022
	(0.019)	(0.084)	(0.785)	(0.033)	(0.019)	(0.084)	(0.782)	(0.033)
ln(GSP)	−0.011	0.102	2.159*	−0.019	−0.011	0.103	2.133*	−0.020
	(0.016)	(0.083)	(0.764)	(0.032)	(0.016)	(0.083)	(0.768)	(0.032)
ln(Legislative expenditures)	−0.017	−0.017	0.835	0.028	−0.016	−0.018	0.844	0.027
	(0.016)	(0.034)	(0.650)	(0.021)	(0.016)	(0.034)	(0.659)	(0.021)
State Fixed Effects	✓	✓		✓	✓	✓		✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Legislator Fixed Effects			✓				✓	
Upper Chamber Indicator			✓				✓	
Adjusted R^2	0.619	0.778	0.416	0.060	0.619	0.779	0.416	0.060
N	2,350	1,176	70,081	1,900	2,350	1,176	70,081	1,900

Note: Cell entries report regression coefficients with standard errors multiway clustered by state and year in parentheses. All models are two-way fixed effects with covariates. Models (1)–(4) use the count of open legislative groups as the treatment variable and models (5)–(8) use an indicator for open standing committees as treatment. The third and fourth rows in gray denote 95% confidence intervals for the coefficients on Sunshine. * $p < 0.05$ (two-tailed).

Table 5: Estimated Effects of Open Legislative Meetings on Budget Delay

Variable	(1) KPM's model	(2) Main variable	(3) Open groups	(4) Standing committees
Sunshine		−0.785*	−0.071	−0.179
		(0.247)	(0.039)	(0.242)
Government shutdown	−1.173*	−1.149*	−1.207*	−1.198*
	(0.545)	(0.534)	(0.541)	(0.543)
Election year	−0.066	−0.063	−0.068	−0.067
	(0.210)	(0.212)	(0.211)	(0.210)
Divided government	0.631*	0.667*	0.649*	0.635*
	(0.165)	(0.166)	(0.165)	(0.165)
Session end vs. start of FY	2.389*	2.237*	2.407*	2.414*
	(0.565)	(0.549)	(0.554)	(0.560)
Supermajority budget	−0.647	−0.814	−0.775	−0.698
	(1.012)	(0.997)	(1.009)	(1.009)
Personal income	−0.073	−0.085	−0.090	−0.081
	(0.186)	(0.187)	(0.187)	(0.186)
Biennial budget	0.955*	0.961*	0.940*	0.945*
	(0.403)	(0.397)	(0.396)	(0.399)
Budget size	0.957*	1.111*	1.062*	0.991*
	(0.248)	(0.254)	(0.255)	(0.252)
Surplus	−0.148	−0.165	−0.152	−0.150
	(0.183)	(0.185)	(0.184)	(0.183)
Start of FY	−0.737*	−0.438	−0.550	−0.664
	(0.356)	(0.355)	(0.362)	(0.365)
Legislative salary	0.337	0.382	0.274	0.302
	(0.317)	(0.316)	(0.318)	(0.319)
Government shutdown ×	−0.496	−0.521	−0.505	−0.500
Election year	(0.477)	(0.478)	(0.478)	(0.477)
Government shutdown ×	−1.843*	−1.497	−1.792*	−1.847*
Session end vs. start of FY	(0.772)	(0.766)	(0.764)	(0.767)
Intercept	−3.228*	−2.833*	−2.881*	−3.086*
	(0.431)	(0.435)	(0.460)	(0.464)
σ_{State}	1.413	1.371	1.394	1.401
σ_{Year}	0.354	0.371	0.363	0.355
BIC	1,304	1,301	1,308	1,311
N	2,193	2,193	2,193	2,193

Note: Cell entries report logistic regression coefficients and standard errors in parentheses with state and year random effects. The outcome is an indicator for a late budget. Model (1) is KPM's original specification. Model (2) adds our main binary treatment variable and models (3) and (4) add the alternative treatment variables—the count of open groups (model 3) and indicator for open standing committees (model 4). * $p < 0.05$ (two-tailed).

Does Transparency Inhibit Political Compromise?

Supporting Information

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Contents

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A Works on Transparency and Compromise

In the main text we claim that a large amount of scholarly research argues that transparency is problematic for negotiation and compromise. Table 1 briefly summarizes recent published work in political science and economics that states, implies, or assumes that argument. This work ranges from negotiations in international relations to comparative legislative politics. It covers formal theory and survey and lab experiments, but does not focus much on outside-the-lab observational behavior. We concentrate on pieces published since 1999, though critiques of transparency as a benefit to good governance date back at least to Hobbes (1651), Chapter 19.

[Insert Table 1 here]

B Coding Open Meetings Laws and Exemptions

Table 2 presents details on our coding of open meetings laws and exemptions in state legislatures, including which specific groups' meetings were exempt in states with legislative exemptions. We searched legislative records to obtain the specific statute name and adoption and exemption dates. Additionally, there are some cases in which the legislature's status was governed by a rule outside of the state's open meetings laws. We coded states as not exempt if open meetings were required by another statute, the state's constitution, legislative chamber rules, and/or a court decision.

[Insert Table 2 here]

Figure 1 reports the pattern of treatment status (main treatment variable) in all states from 1960–2018, which includes the entire time span collectively covered by our various outcomes.

[Insert Figure 1 here]

C Model Diagnostics and Alternative Estimators

C.1 Parallel Trends

The key identifying assumption for our two-way fixed effects estimator 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). As a check on this assumption, we compare the pre-treatment trends in our bill enactment and kurtosis outcome variables for every state that became treated (adopted an open meetings law that applied to the legislature).[1] Figure 2 graphs the average outcome for treated (red) and untreated (blue) states up to 1997, the year before the last 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 2 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 3 reports estimates with leads of 1–4 years in the two-way fixed effects models.

[Insert Figure 3 here]

The top panels in Figure 3 show validating evidence for the bill enactment and party polarization models. The leads of treatment are small in magnitude (near zero and within $\pm m$) and not statistically significant at the 0.05 level, suggesting no effects of treatment prior to sunshine exposure. The estimates in the bottom panels show similar evidence for the party loyalty and budget

[1] We cannot construct such a graph for the polarization measure or party loyalty scores because they do not begin until 1993 and 1995, respectively. The only treated state that adopted its open meetings law after that time was Nebraska, which has a nonpartisan legislature. Temporal variation in treatment in those data stems entirely from legislative exemptions.

kurtosis models. Some of the lead estimates are bounded away from zero and their confidence intervals suggest that substantively large values are plausible. But the estimates themselves are essentially all contained in $[-m, m]$. In short, we find reasonable evidence, but perhaps not complete evidence, favoring the key identification assumption of our main modeling strategy. These findings justify the use of a variety of alternative strategies (see below).

C.2 Alternative Estimators

The two-way fixed effects estimator we employ in the main text facilitates control of time-varying confounding via observed covariates. However, it does not preclude the possibility of bias from unmeasured time-varying confounders. An alternative is the lagged dependent variable model, which conditions on the previous year's value of the outcome for each state instead of the group and time effects. This approach identifies the average treatment effect on the treated (ATT) with an ignorability assumption conditional on the lag and covariates (Ding and Li 2019). The two-way fixed effects and lagged dependent variable approaches also 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).

Our two-way fixed effects estimator also assumes that the initial decision to adopt an open meetings law and/or exempt the legislature is unrelated to the outcome. The fact that legislatures can exempt themselves from such laws calls this assumption into question. Accordingly, we combine our main 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).[2]

Finally, we consider the possibility that our estimates are biased due to treatment timing and/or heterogeneous treatment effects. Recent work demonstrates that two-way fixed effects models can produce biased estimates when there are multiple groups and time periods and treatment turns "on" and "off" at different times (e.g., Goodman-Bacon 2018; Imai and Kim 2019). In particular, the coefficient on our treatment variable is equal to a weighted average of the treatment effect in each treated state-year or legislator-year. The weights, which sum to one, represent the various two-unit/two-period combinations in the data. Importantly, in the presence of heterogeneous treatment effects across states and/or time they can be negative because some combinations may exist in which the "control" unit is treated in both periods (for details, see de Chaisemartin and D'Haultfoeuille 2019). If the treatment effect is heterogeneous across states and/or time, these negative weights will produce bias in the overall treatment effect estimate. Accordingly, we employ two estimators designed to mitigate this type of bias: Imai and Kim's (2019) weighted fixed effects (WFE) and de Chaisemartin and D'Haultfoeuille's (2019) DID_M.[3]

C.3 Results

C.3.1 Lagged Dependent Variable

Table 3 reports results from lagged dependent variable models, with and without covariates. Across the four outcomes, the coefficients on Sunshine indicate small treatment effects. Furthermore, the confidence intervals for those effects are bounded by $\pm m$. Thus, these results uniformly indicate that the effect of transparency is negligible according to our definition.

[Insert Table 3 here]

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

[3] de Chaisemartin and D'Haultfoeuille (2019) introduce several variants of their estimator. We use the default Wald-TC version in their software package (see de Chaisemartin and D'Haultfoeuille 2019, section 3.3).

C.3.2 IPTW

Table 4 reports results from the IPTW models.[4] The top panel reports logistic regression weighting models.[5] The middle and bottom panels report treatment effects from marginal structural models (MSM) of the outcomes. The contemporaneous effects given in the middle panel represent the treatment effects in a given year averaging over all the possible treatment histories prior to that year (see Blackwell and Glynn 2018). This estimand is simple to conceptualize, but necessarily assumes that the entire effect of treatment occurs instantly. Such an assumption could be problematic because the influence of a transparency reform on legislators' behavior may take time to develop and/or accumulate. Accordingly, the second estimand is a cumulative treatment effect, in which the treatment variable in a given year is the number of years a state has been treated up to that year. Blackwell and Glynn (2018, 1076) recommend this approach as a low-dimensional means of accounting for a state's full treatment history on the outcome, which allows for the possibility of a treatment effect that builds over time.

[Insert Table 4 here]

Table 4 generally shows small treatment effects for all four of the main outcomes. The contemporaneous effect estimates are near zero and their confidence intervals generally fall within the bounds of negligible effects (party polarization and budget kurtosis show slight deviations). The cumulative effects and their standard errors are even smaller, with all of the confidence intervals contained inside $[-m, m]$. Of course, those estimates reflect a change in the outcome corresponding to just one additional year of exposure to sunshine via open meetings; a better interpretation might be the change in the outcome for a standard deviation shift in years under treatment. However, even in that case the effects are still quite small: 0.006 (bill enactment, $m \approx 0.09$), 0.052 (party polarization, $m \approx 0.25$), 1.487 (party loyalty, $m \approx 7$), and 0.012 (budget kurtosis, $m \approx 0.10$).

[4] See the replication materials for complete diagnostics as recommended by Blackwell (2013).

[5] 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, and time trends. Some of these variables are omitted from the polarization and party loyalty models due to singularities.

C.3.3 WFE and DID$_M$

Table 5 reports treatment effect estimates for several specifications with Imai and Kim's (2019) WFE and de Chaisemartin and D'Haultfoeuille's (2019) DID$_M$ estimators. Recall that these methods provide estimates that are robust to the potential biases stemming from variation in treatment timing as well as heterogeneous treatment effects. The table reports treatment effect estimates with robust standard errors in parentheses and 95% confidence intervals in brackets.

For each outcome, model (1) is WFE with no covariates. These models place high demand on the data (even with no covariates) because the method assigns weights to observations and many receive a weight of zero (see Imai and Kim 2019). When combined with the group and time fixed effects, statistical identification of the parameters is difficult. This issue inflates the standard errors for the bill enactment models. Moreover, we cannot estimate WFE with the two-way fixed effects specification on the party loyalty outcome. Variation in treatment over time is low after 1995 (when those data begin) and the weighting of cases reduces the estimation sample. To mitigate this problem, in model (2) we estimate WFE with covariates and after substituting a linear time trend for the year fixed effects. While this change produces a different specification, it does improve the method's statistical power and is estimable for the party loyalty outcome. Next, model (3) is de Chaisemartin and D'Haultfoeuille's (2019) DID$_M$ estimator without covariates and model (4) is DID$_M$ with covariates. These models suffer the same problem as WFE—not enough temporal treatment variation for estimation with the party loyalty data.

Table 5 also reports diagnostics on these estimators. The row labeled N ($w_{\text{WFE}} \neq 0$) gives the number of observations for which the WFE method assigns non-zero weight. In all cases, that number is much lower than the total sample size (N), which is consistent with reduced statistical power. Additionally, Imai and Kim (2019) develop a specification test based on a χ^2 statistic for comparing a standard two-way fixed effects model to WFE. The null hypothesis is that the standard estimator is correct. As the row of test results show, we cannot reject this null for any of our models for which the two-way fixed effects WFE model is estimable (see model 1 for each outcome). Finally, the row labeled % $w_{\text{DIDm}} < 0$ provides insight into the potential for heterogeneous treatment effect bias. It indicates the proportion of individual group/time treatment effects with negative weights as defined by de Chaisemartin and D'Haultfoeuille (2019). These values are fairly large, ranging from 26% to 44% of the combinations. While not direct evidence of heterogeneous effects, they make the case for estimating the DID_M model by showing the potential for bias from heterogeneous effects.

[Insert Table 5 here]

The results in Table 5 are substantively quite similar to those in the main text, albeit with less statistical power in some cases. The estimates themselves are generally small, falling within the bounds of $\pm m$ in all cases. A few of the confidence intervals are large, limiting our ability to make strong inferential statements with respect to statistical or substantive significance. But in many cases the 95% confidence intervals are completely or nearly bounded in $[-m, m]$. Overall, these estimators generally suggest that our results are not greatly impacted by the possible bias from treatment timing and/or heterogeneous effects.

D Robustness Checks

D.1 Omnibus Legislation

One possible concern with our bill enactment outcome is how omnibus legislation might affect the results. For instance, a legislature facing increased transparency might increase its use of omnibus bills and lower its use of single-issue bills, which in turn would drive down the total bills introduced in the chamber. Table 6 reports treatment effect estimates (main treatment variable) with our two-way fixed effects and lagged dependent variable models using the log of the total count of non-resolution bills enacted in a legislature as the outcome.[6] If open legislatures rely more heavily on omnibus legislation, we would expect a negative treatment effect. Three of the four coefficients are, in fact, negative. However, by our standards all of the estimates are substantively negligible. Their 95% confidence intervals fall entirely within $\pm m$ ($m \approx 0.43$ for this outcome). The largest effect in magnitude comes from the two-way fixed effects models and corresponds to

[6]Results are substantively unchanged without logging the outcome.

a decrease of about 9% in bills introduced. However, the confidence intervals extend above zero, so we cannot rule out the possibility of small *positive* effects. In short, these results do not indicate that transparency produces a heavy new reliance on omnibus bills.

[Insert Table 6 here]

D.2 Sensitivity to m

Our substantive interpretations of the results depend on m. Perhaps our definition—one-half of an outcome standard deviation—is too generous, declaring some values negligible that should be considered substantively meaningful. 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. See Figure 4 for an example. The graph collects the main treatment variable's effects from our various estimators along with their 95% confidence intervals. We also report the difference-in-means between treated and untreated cases to show that the patterns we report with the estimators also appear in the raw data.

[Insert Figure 4 here]

Consider H1, which posits a negative treatment effect. In panel (a) of Figure 4, 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 panels (b) and (c) the largest upper confidence bounds (H2 and H3 posit positive effects) are about 0.25 for party polarization and 4 percentage points in party loyalty. These results indicate that the best case scenarios for H2 and H3 are only about 50% and 29% of a standard deviation increase in the outcome variable, respectively. This exercise does indicate that there is more sensitivity to m in the test of H4. Panel (d) of Figure 4 shows that the ends of some confidence intervals are larger than m. Nonetheless, even the largest possible expected effect that is plausible in those data is still only 68% of a standard deviation in budget kurtosis. Moreover, because this hypothetical effect comes from the endpoint of a confidence interval, it would be very unlikely to be realized if we could repeatedly draw new samples of the data.

D.3 Effects of Treatment Lags

We present contemporaneous effects of sunshine exposure in the main text. However, the effects of transparency reforms may take time to develop. Accordingly, we re-estimated our two-way fixed effects models and random effects models with 1–4 year lags of treatment status.[7] Figure 5 presents the results for all five outcomes, with the original estimates (contemporaneous effects) as a comparison. As the graphs show, the estimates we report are generally quite stable from contemporaneous effects to four-year lags. The one exception is the two-way fixed effects model of budget kurtosis, in which the treatment effect actually moves toward zero with increasing lags.

[Insert Figure 5 here]

D.4 Latent Variable Compromise Outcome

An alternative to our strategy of modeling each outcome separately is to combine the five outcomes in a single variable measuring latent compromise. We do so here using confirmatory factor analysis (CFA) for all state-years in which we measure all five outcome variables. The sample spans 1995–2014, which is the time period of the party loyalty data (aggregated to the state-year level). The total sample size is 456. Table 7 summarizes the CFA model, with the loadings in the top panel and variances in the bottom. Note that the latent variable is scaled with the proportion of bills enacted variable, which fixes its loading at 1. Larger (smaller) values of the latent factor indicate more (less) compromise.

[Insert Table 7 here]

[7] We also re-estimated these models and the lagged dependent variable models with the cumulative treatment variable described above (see Table 4). The results mirror those with treatment lags—substantively negligible estimates with confidence intervals entirely contained inside $\pm m$.

The results indicate that late budgets and bill enactment load the strongest, with standardized estimates of 0.195 and -1.037, respectively. However, none of the estimates reach statistical significance at the 0.05 level. Budget kurtosis is the weakest variable, with the smallest loading (top panel) and largest unexplained variance (bottom panel). The overall test of the model (χ^2) is marginally statistically significant ($p < 0.10$). The Root Mean Square Error of Approximation (RMSEA) is 0.046 and the Standardized Root Mean Square Residual (SRMR) is 0.037, both of which correspond to "good" fits in the literature (MacCallum, Browne, and Sugawara 1996; Hu and Bentler 1999).

Finally, we generated factor scores from the model reported in Table 7 and included them as the outcome variable in our various estimation models. Figure 6 graphs the treatment effects along with the substantive threshold for this variable ($m \approx 0.09$).[8] The estimates for the two-way fixed effects and IPTW models are just below and just above m, respectively. Their confidence intervals extend past m, indicating that some positive and substantively significant values are plausible. The lagged dependent variable estimates and confidence intervals are entirely contained in $[-m, m]$. However, perhaps the most important pattern in these results is that all of the treatment effects are positive; if anything, transparency produces *more* compromise rather than less. Put differently, the confidence intervals do not reach below $-m$, which would suggest a substantively large negative effect. In sum, while non-negligible effects are plausible, using this latent variable outcome we find no evidence supporting our theoretical claim that exposure to open meetings laws hinders legislative compromise.

[Insert Figure 6 here]

D.5 Results by Decade

The effects of transparency may have changed over time, perhaps because state legislatures have polarized unevenly in the last several decades. Accordingly, we re-estimated the two-way fixed effects (with covariates), lagged dependent variable (with covariates), and IPTW models

[8] We again report the difference-in-means between groups in this graph to confirm that the estimates we report are similar to the pattern in the raw data.

after subsetting by decade.[9] Figure 7 presents the results for all five outcome variables.

[Insert Figure 7 here]

The results generally show no systematic pattern over time, and for the most part the estimates themselves remain inside $[-m, m]$. The reduced sample sizes yield more uncertainty, and thus the confidence intervals do expand outside of the substantive thresholds in some cases. The budget kurtosis is a slight exception to this pattern; those results include several estimates and confidence intervals greater than m. However, even in those cases the confidence intervals still show that negligible and negative estimates are plausible. In short, there is no clear indication that the effects of transparency were systematically different in an earlier decade compared to recent years.

E Multiple Imputation Diagnostics

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

E.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 8 presents overimputation results for the variables used in the proportion of bills enacted models.[10] The observed 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

[9] For the outcomes that include fixed effects models we combine all state years after 1999 into one group because the only variation in treatment after 2009 is cross-state variation. Estimating the models with sunshine $\times$ year interactions produces substantively similar results.

[10] See the replication materials for diagnostics on the other datasets.

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 8 here]

The graphs in Figure 8 generally shows good, though not perfect, coverage of the reference line. The clouds of points trend upward, 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 9 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 9 here]

E.2 Results with Listwise Deletion

Figure 10 reports the effects of sunshine exposure and their 95% confidence intervals, estimated with listwise deletion of missing cases (i.e., no imputation).[11] The results are quite similar to those with imputed data. Most of the estimates suggest negligible effects, with some support for the plausibility of positive effects on budget kurtosis in the two-way fixed effects and IPTW models (panel d).

[Insert Figure 10 here]

[11] We also report difference-in-means by treatment group to show that our substantive conclusions hold even when looking only at the raw data. The sample size change due to listwise deletion are as follows: (1) Proportion of bills enacted: 2,350 to 1,516; (2) Party polarization: 1,176 to 574; (3) Party loyalty: 70,081 to 48,508; (4) Budget kurtosis: 1,900 to 1,250.

F Power Simulations

We generated fake outcome data by defining a treatment effect in the linear predictor of each model specification, then adding random normal error with a mean of zero and standard deviation equal to the average residual standard deviation (across imputed datasets) from the model.[12] 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 11 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 11 here]

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[12]This approach produces a "well-behaved" error term that satisfies the relevant Gauss-Markov assumptions. However, standard diagnostics show that issues such as clustering and autocorrelation are present to varying degrees in our models. We repeated these simulations using wild cluster bootstrapping—an empirically-based method for generating the error term. This alternative approach captures any violations to the regression assumptions that exist in the residuals of our models. The results do not change our conclusions about the models' statistical power.

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Table 1: Transparency and Compromise Research Since 1999

Authors	Years	Journal/Press	Argument
Anderson et al.	2020	Cambridge University Press	Closed door negotiations increase the chances for compromise in American state legislatures.
Benesch et al.	2018	*Journal of Public Economics*	Transparency in legislative voting leads to more party line votes.
Fehrler and Hughes	2018	*American Economic Journal: Microeconomics*	Transparent deliberation harms information aggregation in committees.
Gradwohl	2018	*Economic Theory*	Anonymous voting can lead to higher voter welfare than either fully secret or fully open voting.
Gradwohl and Feddersen	2018	*Journal of Politics*	Transparency eliminates the ability of an advisory committee to influence a decision maker and distort committee member preferences.
Wooley and Gardner	2017	*The Social Science Journal*	Transparency might encourage poor reasoning when making decisions. Empirical evidence indicates that transparent deliberation did not affect decisionmaking on the Federal Open Market Committee.
Binder and Lee	2016	APSA Task Force Report	Public attention increases the incentive of lawmakers to adhere to party messages.
Patty	2016	*American Journal of Political Science*	Obstruction of Pareto optimal proposals because of elections is reduced by private bargaining.
Berliner and Ehrlich	2015	*American Political Science Review*	Politically competitive states adopt transparency laws to bind the hands of future political actors.
Stadelman et al.	2014	*Journal of Experimental Political Science*	Increased transparency does not increase the quality of representation in legislatures.
Fox and Van Weelden	2012	*Journal of Public Economics*	When learning costs are asymmetric, observing the consequences of an expert's actions can harm a principal's welfare.
Seidmann	2011	*Social Choice and Welfare*	Committees can only exhibit a norm of consensus if committee members vote privately.
Gavazza and Lizzeri	2009	*Review of Economic Studies*	Transparency of spending can be beneficial, but transparency of revenues can be counterproductive, because it leads to wasteful spending.
Fox	2007	*Public Choice*	Transparency causes lawmakers to emphasize policies that make voters believe they are unbiased, rather than policies than that are best for constituents.
Hood	2007	*Public Management Review*	Transparency, when paired with blame avoidance, can lead to back fire effects and poor agency performance.
Levy	2007	*American Economic Review*	Secretive committees, combined with higher voting thresholds (super majorities) lead to better committee decisions.
Stasavage	2007	*Journal of Politics*	Policy decisions made in public may polarize legislators.
Prat	2005	*American Economic Review*	Transparency makes agents behave in conformist manners, harming principals' welfare.
Stasavage	2004	*International Organization*	Open door bargaining leads to posturing in international negotiations.
Heald	2003	*Public Administration*	Descriptive discussion on the tradeoff between "sunlight" and "the danger of overexposure" in fiscal policy. "Some transparency is needed to deter fraud and corruption. . . [but] too much leads to losses in effectiveness through. . . excessive politicization" (727).
Groseclose and McCarty	2001	*American Journal of Political Science*	Sunshine laws reduce efficiency (114).
Bengt and Holstrom	1999	*Review of Economic Studies*	A lack of private action options makes agents put in minimal effort on behalf of principals.
Finel and Lord	1999	*International Studies Quarterly*	Transparency makes international conflicts worse by overwhelming diplomatic signals with domestic politics "noise."

Table 2: State Open Meetings Laws and Legislative Exemptions

State	Sunshine Law	Year Enacted	Exemption Statute	Exemption Year	Exempt Groups
Alabama	Alabama Open Meetings Act	1975	–	–	–
Alaska	Alaska's Open Meetings Act	1959	*Abood v. League of Women Voters and Anchorage Daily News*	1987	1–6
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	1–6
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	1–6
Idaho	Idaho's Open Meeting Law	1974	Statute § 67-2341(4)	1998	1–6
Illinois	Ill. Const. Art. II, § 14	1818	–	–	–
Indiana	Open Door Law	1977	–	–	–
Iowa	Open Meetings Law	1967	Iowa Code. §21.2	1967	1–6
Kansas	Kansas Open Meetings Act	1972	–	–	–
Kentucky	Open Meetings of Public Agencies Act	1974	Statute 61.810(1)(i)	1974	1, 3–6
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	1–6
Michigan	Open Meetings Act	1976	–	–	–
Minnesota	Open Meetings Law	1957	–	–	–
Mississippi	Open Meetings Act	1975	Code Ann. § 25-41-3(a)	1975	1, 4
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	3
New Jersey	Open Public Meeting Act	1975	–	–	–
New Mexico	Open Meetings Act	1959	Statute § 10-15-2(A)(B)	1978	1, 4
New York	Open Meetings Law	1976	N.Y. Pub. Off. Law § 108(2) (a)	1976	1–6
North Carolina	Open Meetings Law	1971	Statute § 143-318.18	1979	3, 4, 6
North Dakota	Open Meetings Law	1974	–	–	–
Ohio	Ohio Const. Art. II, § 13.	1851	Ohio Rev. Code § 101.15	2002	3
Oklahoma	Open Meeting Act	1959	25 O.S. § 304.1	1977	1–6
Oregon	Public Meetings Law	1973	37 Op Atty Gen 1087, 1089	1976	3, 7
Pennsylvania	Sunshine Act	1987	Statute § 712	1998	3, 6
Rhode Island	Open Meetings Law	1976	R.I. Gen. laws § 42-46-2 (3)	1976	3, 7
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	3, 7
Vermont	Public Meetings Law	1976	1 V.S.A. § 313(c).	1979	1–6
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	3
Wyoming	Public Meeting Law	1973	Statute § 16-4-402(a)(ii)	1977	1–6

Note: Exempt groups are coded as follows. 1 = Subcommittees; 2 = Committees outside of Committees of the Whole; 3 = Partisan caucuses; 4 = Conference committees; 5 = Standing political committees; 6 = Ethics committees; 7 = Political parties. Groups not listed for a given state remained open after legislative exemption.

Table 3: Estimated Effects with Lagged Dependent Variable Models

	Bill enactment $m \approx 0.09$		Party polarization $m \approx 0.25$		Party loyalty $m \approx 7$		Budget kurtosis $m \approx 0.10$	
Variable	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Sunshine	0.011	0.028*	0.018	−0.021	1.546	1.598	0.025	0.015
	(0.013)	(0.013)	(0.016)	(0.022)	(1.061)	(1.051)	(0.014)	(0.013)
Sunshine 95% CI	[−0.014, 0.037]	[0.003, 0.053]	[−0.014, 0.050]	[−0.065, 0.023]	[−0.533, 3.625]	[−0.461, 3.658]	[−0.001, 0.052]	[−0.011, 0.041]
Outcome$_{t-1}$	0.607*	0.383*	0.881*	0.809*	0.459*	0.430*	0.127*	0.118*
	(0.052)	(0.059)	(0.052)	(0.071)	(0.059)	(0.054)	(0.024)	(0.022)
Bills vetoed (100s)		0.034*		0.057		−1.014		0.002
		(0.012)		(0.034)		(0.730)		(0.021)
Professionalism (1d)		0.009		0.004		0.345		−0.011
		(0.006)		(0.010)		(0.332)		(0.008)
Professionalism (2d)		0.018*		0.002		0.244		−0.012
		(0.008)		(0.014)		(0.484)		(0.007)
State ideology		−0.178*		−0.019		0.620		−0.019
		(0.042)		(0.093)		(1.847)		(0.053)
Governmental ideology		−0.044		−0.088		0.460		0.026
		(0.046)		(0.074)		(2.154)		(0.057)
Folded Ranney index		−0.083		0.225		0.503		0.050
		(0.045)		(0.156)		(2.214)		(0.043)
Term limits in effect		0.052*		0.071*		0.124		0.021
		(0.025)		(0.034)		(0.679)		(0.021)
ln(Population)		−0.051*		−0.027		−2.999*		−0.007
		(0.011)		(0.044)		(1.285)		(0.021)
ln(GSP)		0.044*		0.084		2.547		0.025
		(0.017)		(0.049)		(1.369)		(0.022)
ln(Legislative expenditures)		−0.062*		−0.046		1.218		0.001
		(0.014)		(0.034)		(0.818)		(0.017)
Upper chamber					1.742*	1.723*		
					(0.517)	(0.482)		
Intercept	0.111*	1.632*	0.176*	0.345	44.828*	40.488*	0.362*	0.129
	(0.018)	(0.201)	(0.072)	(0.364)	(5.902)	(14.458)	(0.015)	(0.222)
Adjusted R^2	0.367	0.462	0.766	0.777	0.211	0.226	0.020	0.023
N	2,350	2,350	1,127	1,127	48,151	48,151	1,850	1,850

Note: Cell entries report regression coefficients with standard errors multiway clustered by state and year in parentheses. The third and fourth rows in gray denote 95% confidence intervals for the coefficients on Sunshine. For each outcome, model (1) is lagged dependent variable with no covariates and model (2) is lagged dependent variable with covariates. There is no sample size loss in the bill enactment models because the outcome data include the year 1969. * $p < 0.05$ (two-tailed).

Table 4: IPTW Model Results

Variable	Bill enactment $m \approx 0.09$	Party polarization $m \approx 0.25$	Party loyalty $m \approx 7$	Budget kurtosis $m \approx 0.10$
	Weighting models			
$\text{Sunshine}_{\text{t-1}}$	6.746*	24.950*		11.179*
	(0.549)	(9.777)		(1.434)
Cumulative sunshine	−0.062	0.649	0.565*	0.063
	(0.090)	(0.479)	(0.013)	(0.114)
Bills vetoed	0.001	−0.031	0.004*	0.007
	(0.004)	(0.026)	(0.001)	(0.006)
Professionalism (1d)	−0.164	0.351	−0.930*	−0.385
	(0.221)	(1.189)	(0.036)	(0.389)
Professionalism (2d)	0.171	5.079	4.844*	0.523
	(0.295)	(2.671)	(0.157)	(0.465)
State ideology	0.004	−0.051	0.018*	−0.022
	(0.014)	(0.093)	(0.003)	(0.029)
Governmental ideology	0.007	0.304	0.017*	0.049
	(0.018)	(0.187)	(0.003)	(0.037)
Folded Ranney index	4.063*	58.217*	9.982*	8.451*
	(1.824)	(24.816)	(0.405)	(4.068)
Term limits in effect	0.966	−4.169	−0.834*	0.202
	(1.155)	(2.915)	(0.098)	(1.230)
ln(Population)	1.085	19.894	3.678*	2.282
	(0.688)	(12.582)	(0.232)	(1.150)
ln(GSP)	0.018	−12.575	0.233	−0.996
	(0.813)	(9.070)	(0.212)	(1.477)
ln(Legislative expenditures)	−0.820	−9.946	−2.651*	−0.693
	(0.487)	(5.809)	(0.154)	(1.047)
Upper chamber			0.129	
			(0.067)	
Time	0.068		−0.832*	0.080
	(0.096)		(0.032)	(0.143)
Time^2	−0.002		0.015	−0.003
	(0.002)		(0.001)	(0.003)
$\text{Sunshine}_{\text{t-1}} \times$ Cumulative sunshine	0.182*	0.539		−0.012
	(0.092)	(0.611)		(0.115)
Intercept	−11.026	−65.654	−28.367*	−26.262*
	(6.839)	(42.144)	(1.488)	(13.134)
	Contemporaneous effects			
Sunshine	−0.014	0.076	−1.823	0.069*
	(0.018)	(0.090)	(2.040)	(0.034)
Sunshine 95% CI	[−0.050, 0.022]	[−0.105, 0.257]	[−5.822, 2.176]	[0.004, 0.135]
State Fixed Effects	✓	✓		✓
Year Fixed Effects	✓	✓	✓	✓
Legislator Fixed Effects			✓	
Upper Chamber Indicator			✓	
	Cumulative effects			
Cumulative sunshine	0.000	0.007	0.102	0.001
	(0.001)	(0.006)	(0.118)	(0.001)
C. sunshine 95% CI	[−0.002, 0.003]	[−0.004, 0.018]	[−0.129, 0.333]	[−0.001, 0.003]
State Fixed Effects	✓	✓		✓
Year Fixed Effects	✓	✓	✓	✓
Legislator Fixed Effects			✓	
Upper Chamber Indicator			✓	
N	2,350	1,176	70,081	1,900

Note: Cell entries report coefficients with standard errors in parentheses. The top panel reports logistic regression weighting models. The middle panel reports contemporaneous treatment effects and the bottom panel reports cumulative treatment effects from marginal structural models (MSM) of the outcomes. Weights generated from the weighting models were used in estimation of the treatment effects. Some variables are omitted from the polarization and party loyalty weighting models due to singularities. * $p < 0.05$ (two-tailed).

Table 5: Estimated Effects with Weighted Fixed Effects and DID_M Models

	Bill enactment $m \approx 0.09$				Party polarization $m \approx 0.25$			
Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Sunshine	−0.017	0.006	−0.010	−0.013	0.011	0.011	0.011	0.006
	(1.460)	(0.015)	(0.031)	(0.031)	(0.126)	(0.154)	(0.115)	(0.125)
Sunshine 95% CI	[−2.878, 2.844]	[−0.025, 0.037]	[−0.074, 0.054]	[−0.078, 0.052]	[−0.237, 0.259]	[−0.358, 0.381]	[−0.217, 0.239]	[−0.242, 0.254]
Covariates		✓		✓		✓		✓
N	2,350	2,350	2,350	2,350	1,176	1,176	1,176	1,176
N ($w_{\text{WFE}} \neq 0$)	135	1,457			218	120		
WFE test	NS				NS			
% $w_{\text{DIDm}} < 0$			33%	26%			35%	42%

	Party loyalty $m \approx 7$				Budget kurtosis $m \approx 0.10$			
Variable	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Sunshine		4.234*			0.053	0.073*	0.053	0.051
		(0.837)			(0.067)	(0.021)	(0.080)	(0.079)
Sunshine 95% CI		[2.578, 5.891]			[−0.078, 0.184]	[0.026, 0.120]	[−0.104, 0.209]	[−0.104, 0.206]
Covariates		✓				✓		✓
N		70,081			1,900	1,900	1,900	1,900
N ($w_{\text{WFE}} \neq 0$)		3,376			404	342		
WFE test					NS			
% $w_{\text{DIDm}} < 0$							44%	42%

Note: Cell entries report treatment effect estimates with robust standard errors in parentheses and 95% confidence intervals in brackets. For each outcome, model (1) is Imai and Kim's (2019) weighted two-way fixed effects (WFE) with no covariates, model (2) is WFE with covariates and a linear time trend instead of year fixed effects, model (3) is de Chaisemartin and D'Haultfoeuille's (2019) DID_M estimator without covariates and model (4) is DID_M with covariates. Models (1), (3), and (4) are not estimable for the party loyalty outcome due to low temporal variation in treatment after 1995. N ($w_{\text{WFE}} \neq 0$) refers to the number of observations with non-zero weight in the WFE estimation. The WFE test produces a χ^2 statistic; the null hypothesis is that the standard (unweighted) two-way fixed effects model is correct. The proportion of treated state-years with negative weights is reported as % $w_{\text{DIDm}} < 0$ (see de Chaisemartin and D'Haultfoeuille 2019). NS Not significant; * $p < 0.05$ (two-tailed).

Table 6: Estimated Effects on the Log of Total Bills Introduced

	Two-way FE		Lagged DV	
	(1)	(2)	(1)	(2)
Sunshine	−0.096	−0.096	0.026	−0.026
	(0.105)	(0.086)	(0.067)	(0.066)
Sunshine 95% CI	[−0.301	[−0.263	[−0.105	[−0.154
	0.110]	0.072]	0.158]	0.103]
Outcome_{t-1}			0.633*	0.314*
			(0.077)	(0.083)
Covariates		✓		✓
State Fixed Effects	✓	✓		
Year Fixed Effects	✓	✓		
Adjusted R^2	0.697	0.706	0.399	0.526
N	2,350	2,350	2,350	2,350

Note: Cell entries report regression coefficients with standard errors multiway clustered by state and year in parentheses. The third and fourth rows in gray denote 95% confidence intervals for the coefficients on Sunshine. For this outcome, $m \approx 0.43$. For each estimator, model (1) includes no covariates and model (2) includes covariates. There is no sample size loss in the lagged dependent variable models because the outcome data include the year 1969.
* $p < 0.05$ (two-tailed).

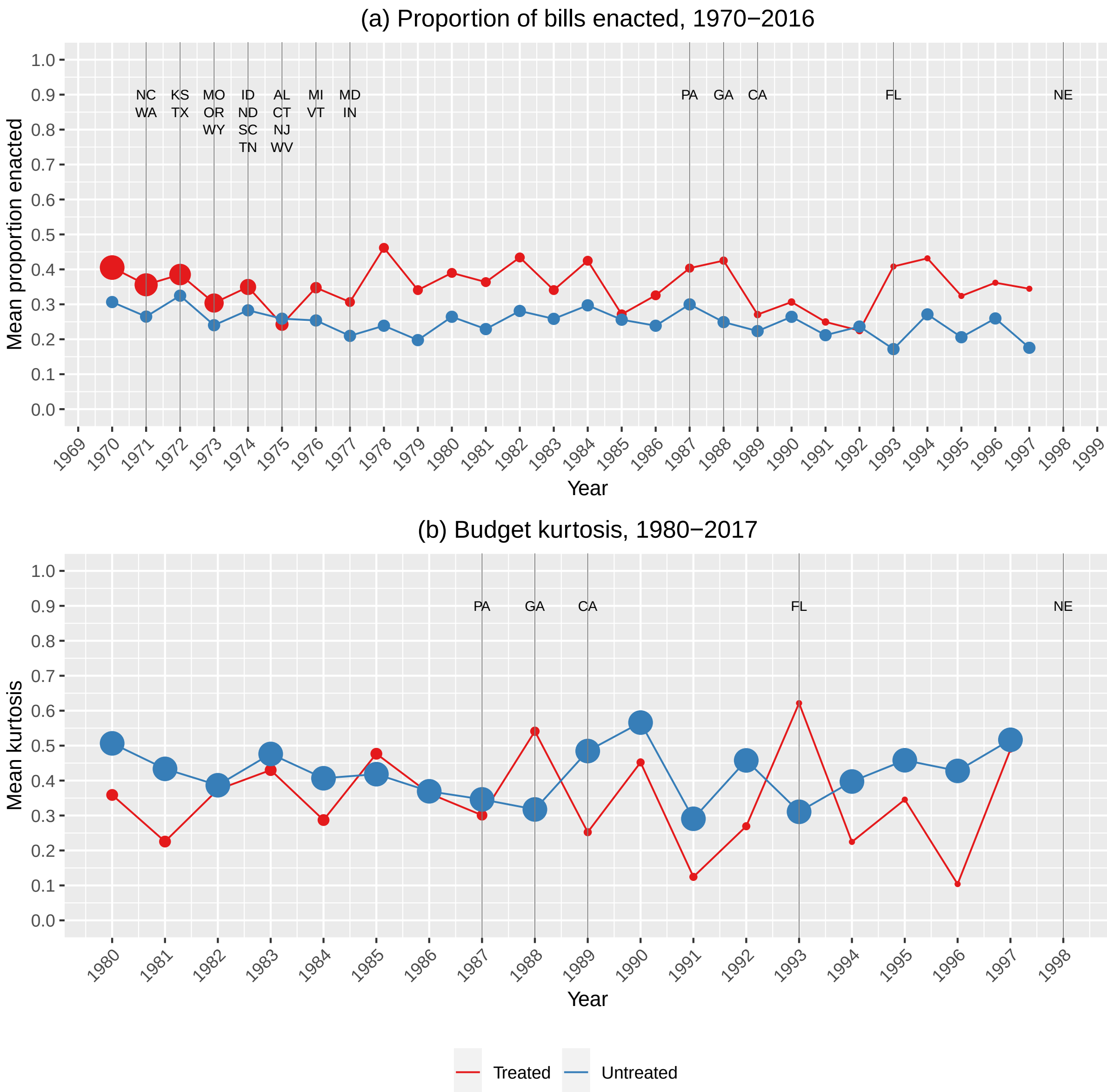
Table 7: Confirmatory Factor Analysis of the Outcome Variables

	Loadings				
Outcome	Estimate	SE	Z	p	Std. estimate
Enactment	1.000	–	–	–	0.195
Polarization	−0.234	0.238	−0.983	0.325	−0.046
Loyalty	−0.337	0.244	−1.382	0.167	−0.066
Kurtosis	0.167	0.235	0.711	0.477	0.032
Late budget	−5.325	14.207	−0.375	0.708	−1.037
	Variances				
Outcome	Estimate	SE	Z	p	Std. estimate
Enactment	0.960	0.119	8.072	0.000	0.962
Polarization	0.996	0.066	15.049	0.000	0.998
Loyalty	0.994	0.067	14.879	0.000	0.996
Kurtosis	0.997	0.066	15.087	0.000	0.999
Late budget	−0.075	2.850	−0.026	0.979	−0.075

Note: Cell entries report factor loadings and variances from confirmatory factor analysis (CFA) of the five outcome variables described in the main text. N = 456; $\chi^2(5)$ = 9.881 (p = 0.079); RMSEA = 0.046 (90% CI: [0.000, 0.089]); SRMR = 0.037.

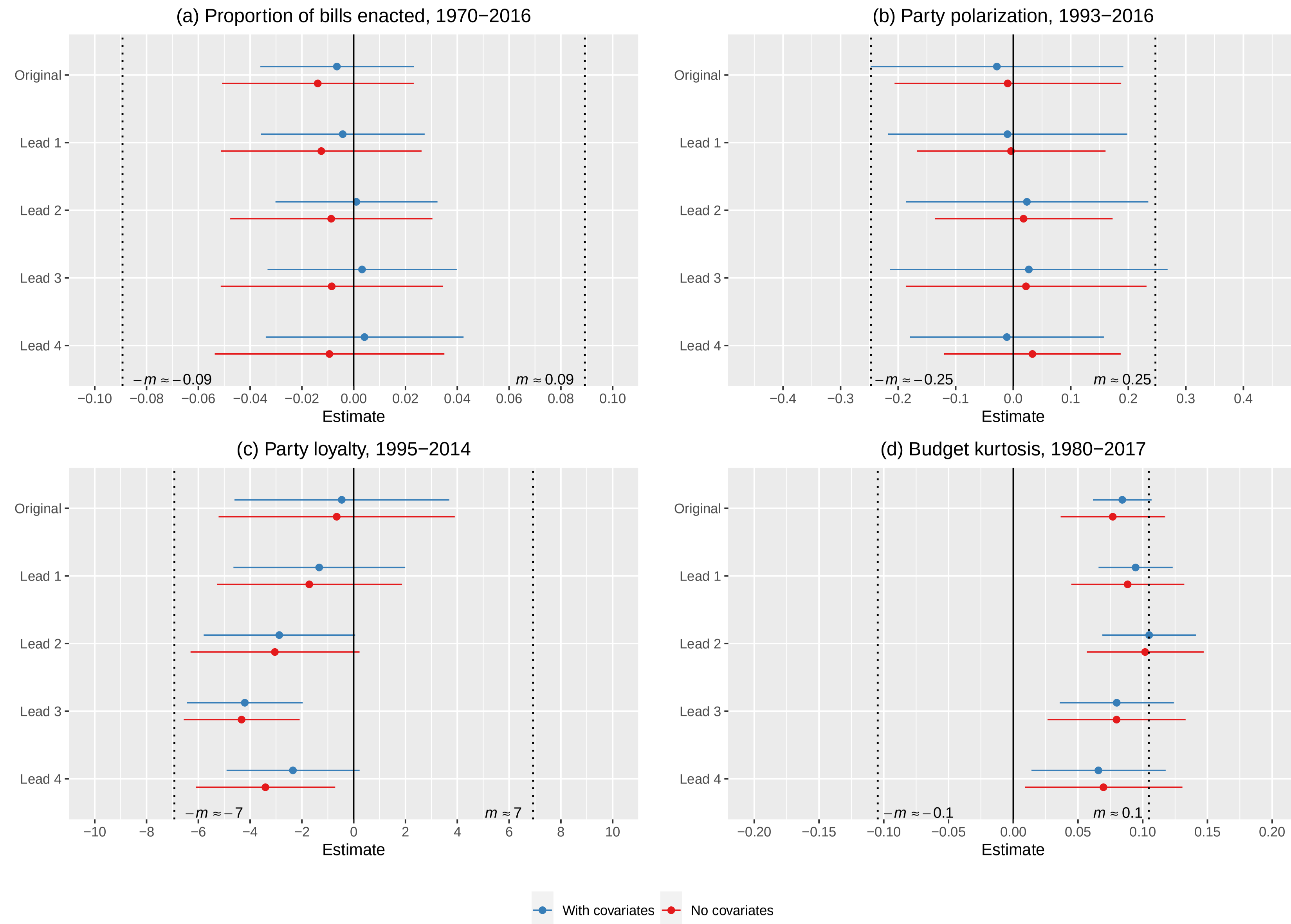
Figure 1: Variation in State Open Meetings Law Legislative Exposure, 1960–2018

Figure 2: Pretreatment Means in the Outcomes for Treated and Untreated States



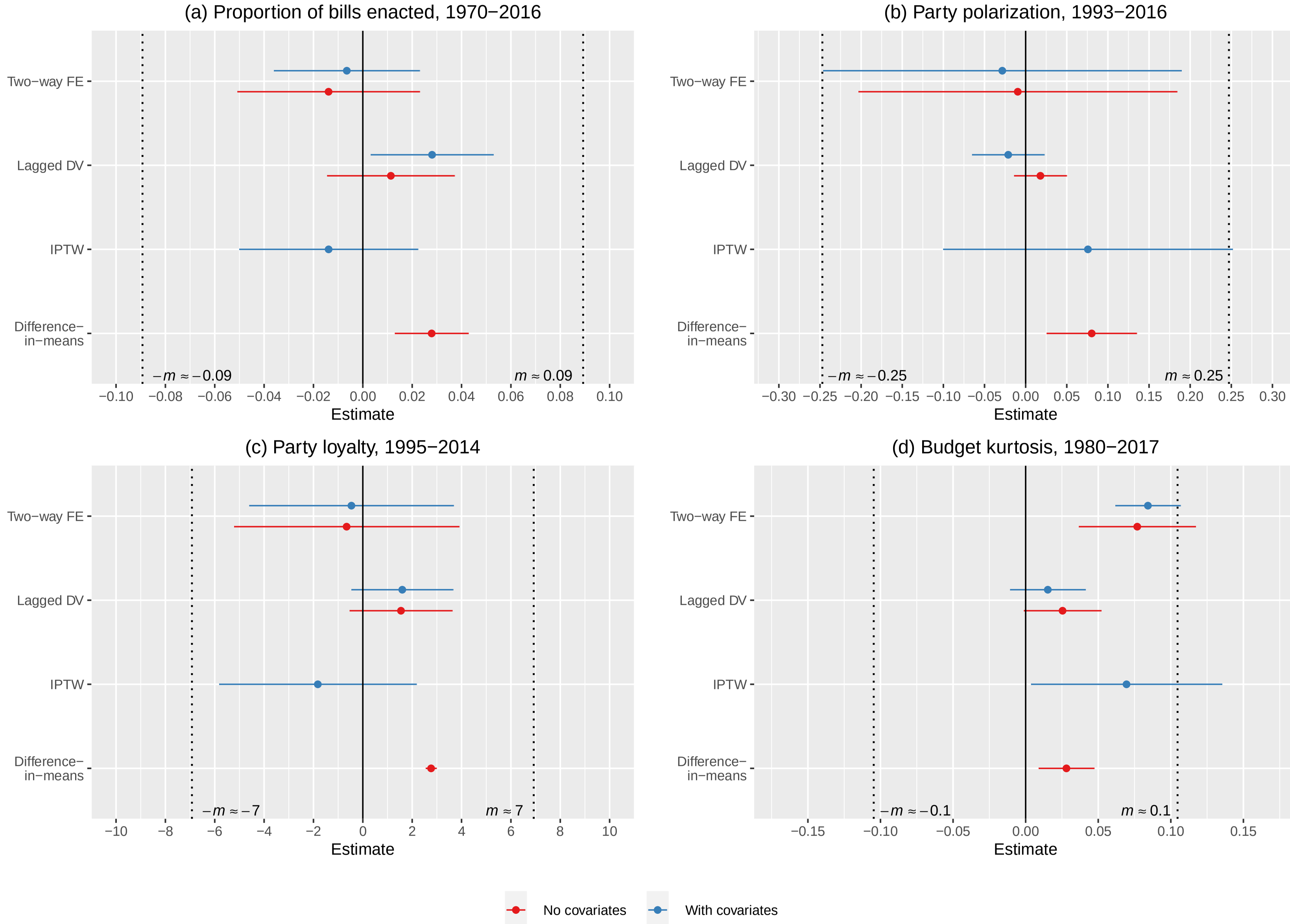
Note: The graph presents the average outcome for treated (red) and untreated (blue) states up to 1997, the year before the last open meetings law adoption (Nebraska). The vertical lines reflect adoption dates for the treated states listed on the graphs. Dot sizes are proportional to the sample sizes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

Figure 3: Estimated Effects of Treatment Leads



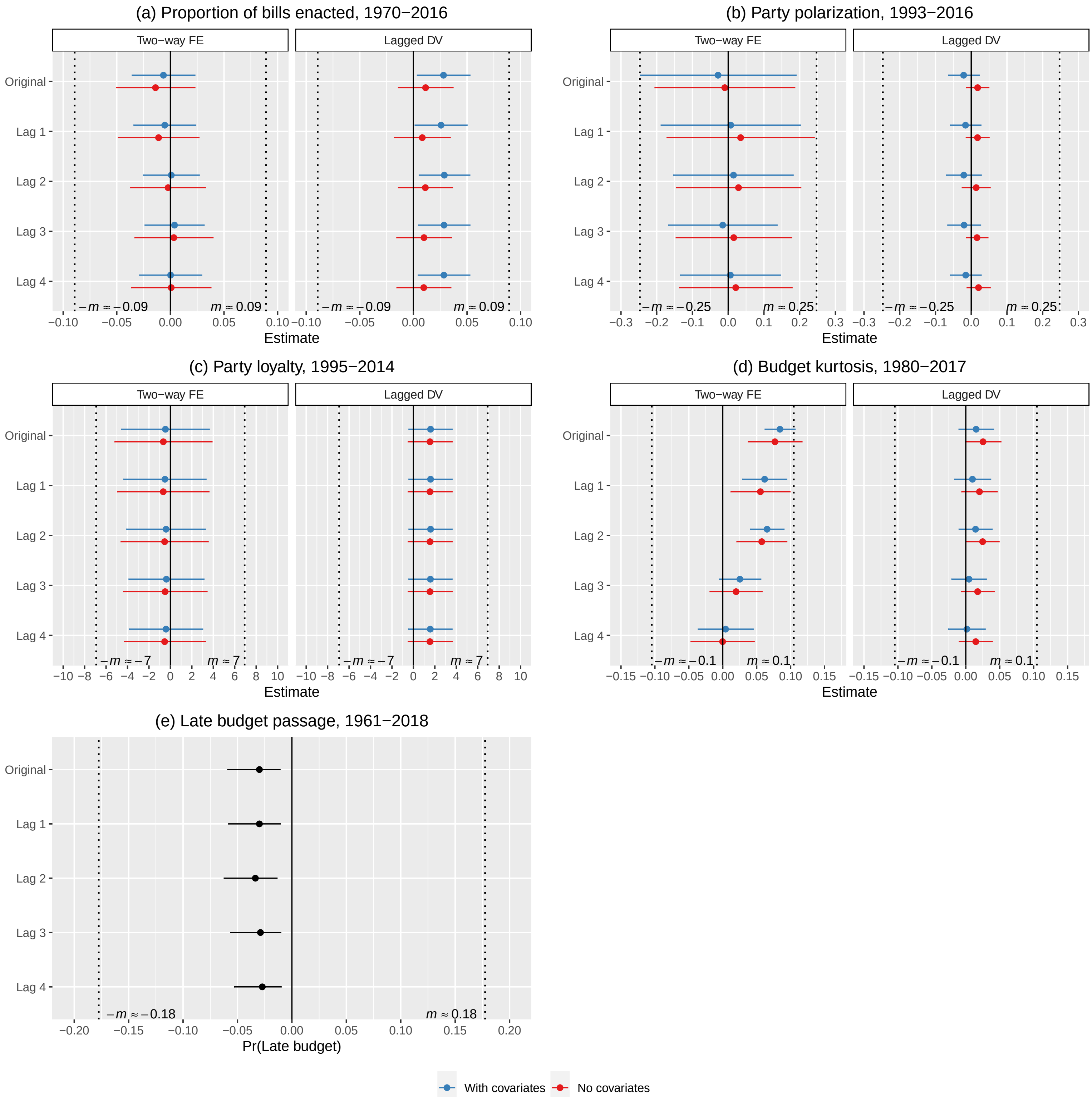
Note: The graphs present estimated treatment effects from the two-way fixed effects models for leads of treatment from 1 to 4 years. Points represent effect estimates and line segments represent 95% confidence intervals.

Figure 4: Estimated Effects of Open Legislative Meetings on the Main Outcomes



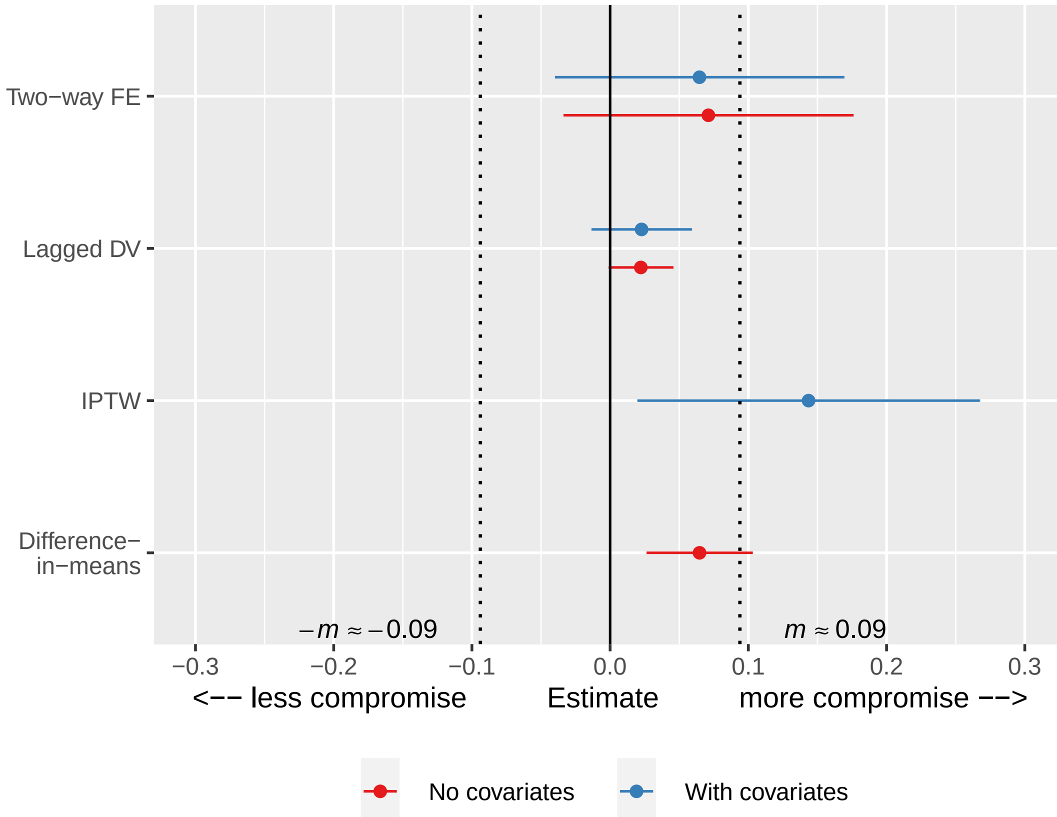
Note: The graphs present the estimated effects of exposure to open meetings laws and their 95% confidence intervals. The dotted vertical lines denote $\pm m$, our chosen threshold for a substantively meaningful effect. Only one IPTW specification is shown because that estimator requires covariates for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

Figure 5: Estimated Effects of Treatment Lags



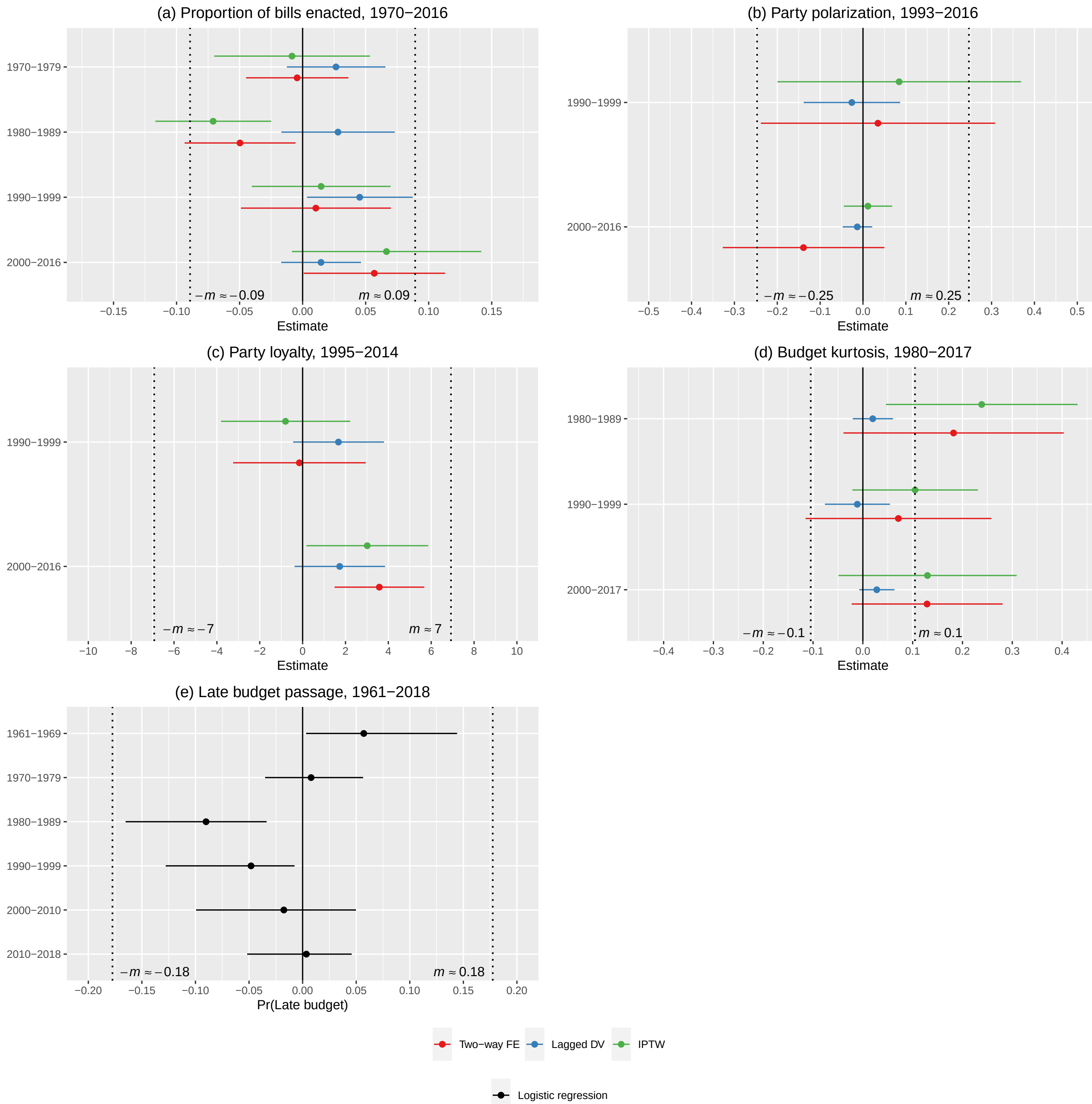
Note: The graphs present estimated treatment effects from the two-way fixed effects models (panels a–d) and random effects models (panel e) for lags of treatment from 1 to 4 years. Points represent effect estimates and line segments represent 95% confidence intervals. In panel (e) the effects are reported on the probability scale.

Figure 6: Treatment Effects on the Latent Compromise Outcome Variable



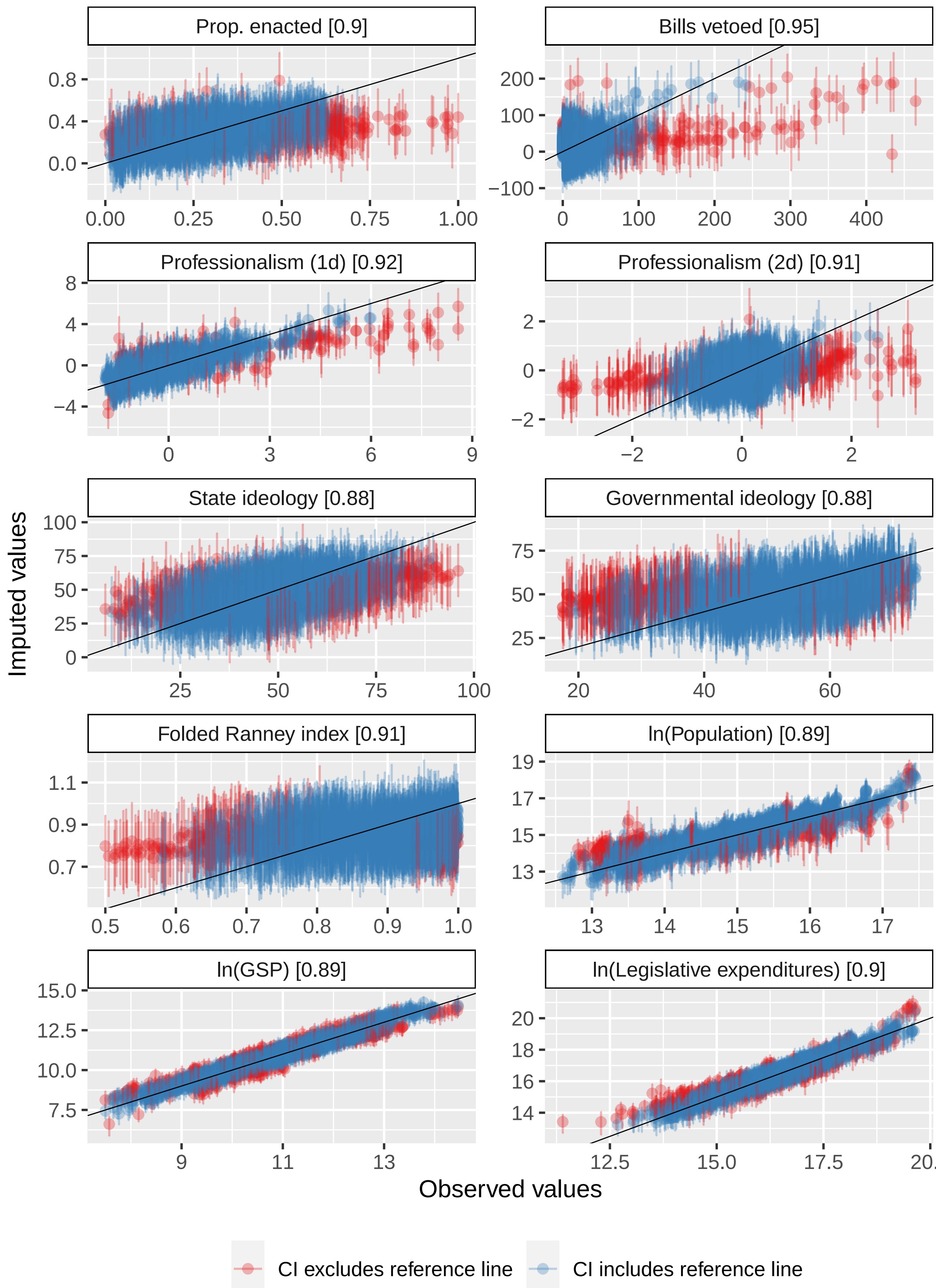
Note: The graph presents the estimated effects of exposure to open meetings laws and their 95% confidence intervals. The dotted vertical lines denote $\pm m$, our chosen threshold for a substantively meaningful effect. Only one IPTW specification is shown because that estimator requires covariates for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

Figure 7: Estimated Effects by Decade



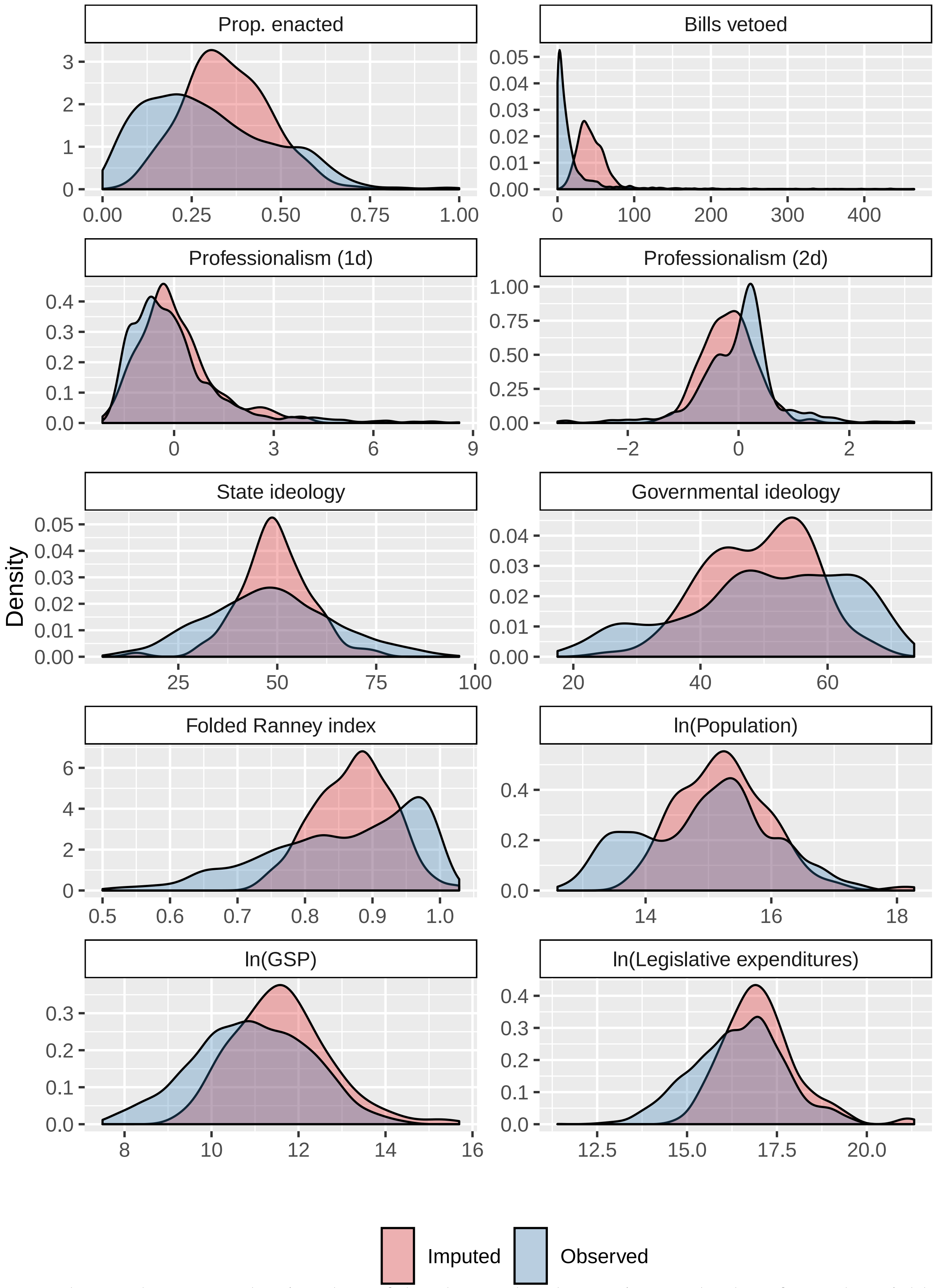
Note: The graphs present estimated treatment effects after subsetting by decade for several estimators. Points represent effect estimates and line segments represent 95% confidence intervals. In panel (e) the effects are reported on the probability scale. For the outcomes that include fixed effects models (i.e., all except the late budget outcome), we combine all state years after 1999 into one group because the only variation in treatment after 2009 is cross-state variation.

Figure 8: Overimputation Results for the Proportion of Bills Enacted Data



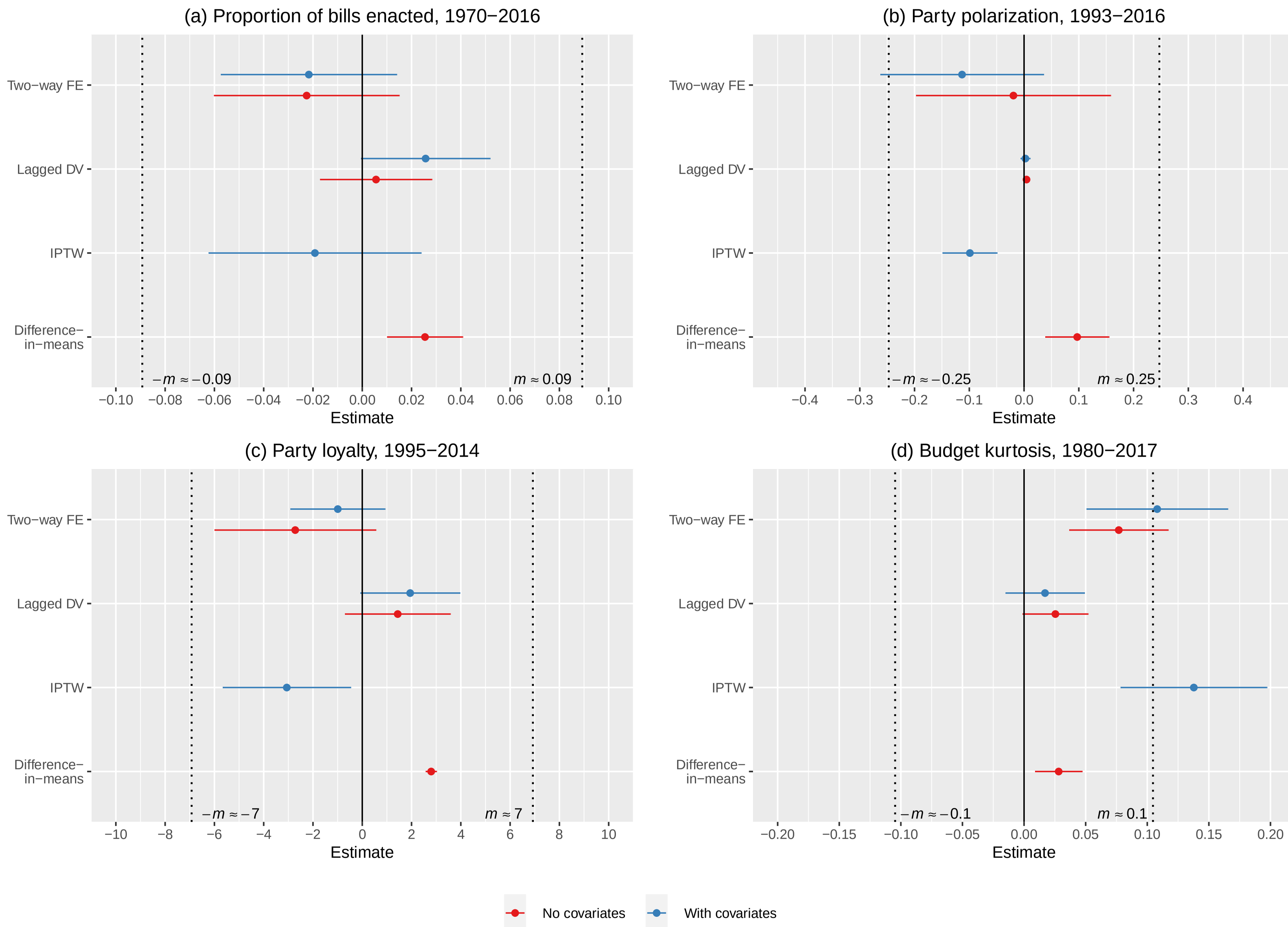
Note: The graphs present observed values of each variable on the x-axes against mean imputations of those values on the y-axes. Line segments indicate 95% confidence intervals. The solid line serves as a reference point for perfect imputation. The values in square brackets next to each variable label refer to the actual coverage level for that variable.

Figure 9: Observed and Imputed Densities for the Proportion of Bills Enacted Data



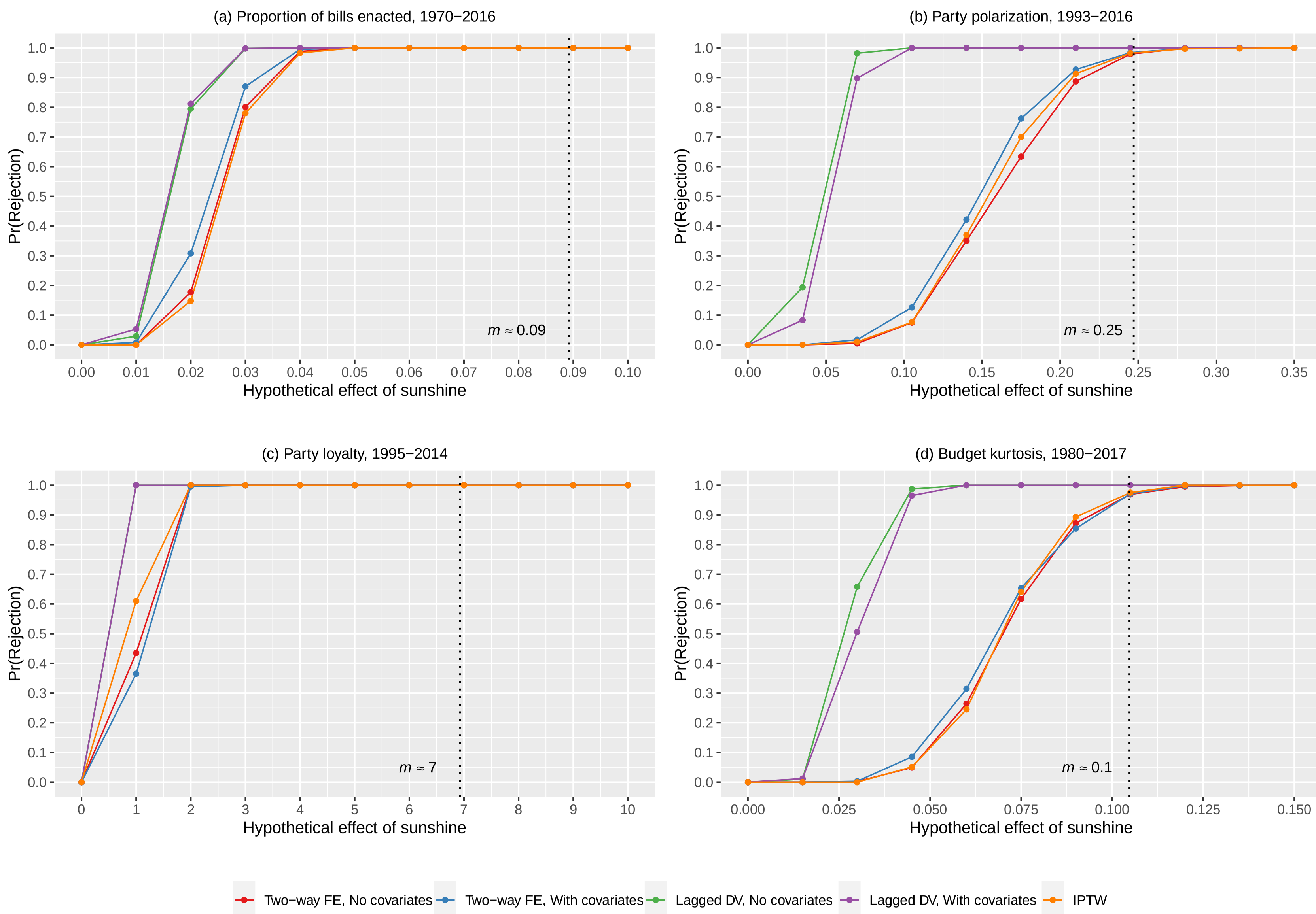
Note: The graphs present density plots of the observed and mean imputed values for each variable.

Figure 10: Estimated Effects Using Listwise Deletion for Missing Data



Note: The graphs present the estimated effects of exposure to open meetings 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 11: 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.