Does Transparency Inhibit Political Compromise?

Supporting Information

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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

[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.

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 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. Further-

[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).

more, 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]

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

[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.

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, $m \approx 0.10$).

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 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

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

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

[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 $\pm m$.

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]

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

[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.

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 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 di-

[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.

agnostics 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 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]

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

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]

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

[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.

[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.

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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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			
Sunshine$_{t-1}$	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)
Sunshine$_{t-1}$ ×	0.182*	0.539		−0.012
Cumulative sunshine	(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).

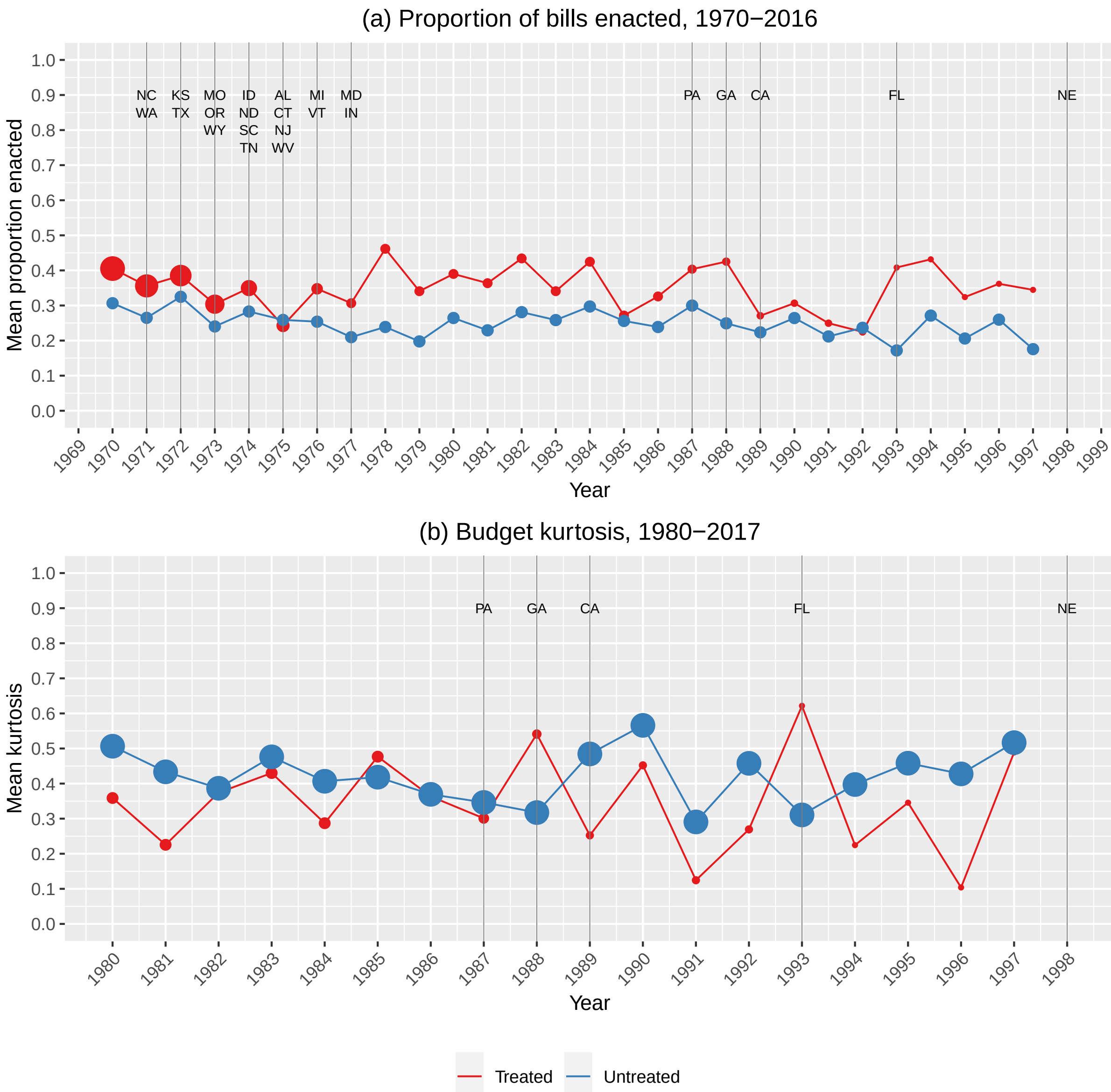
Table 7: Confirmatory Factor Analysis of the Outcome Variables

	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.

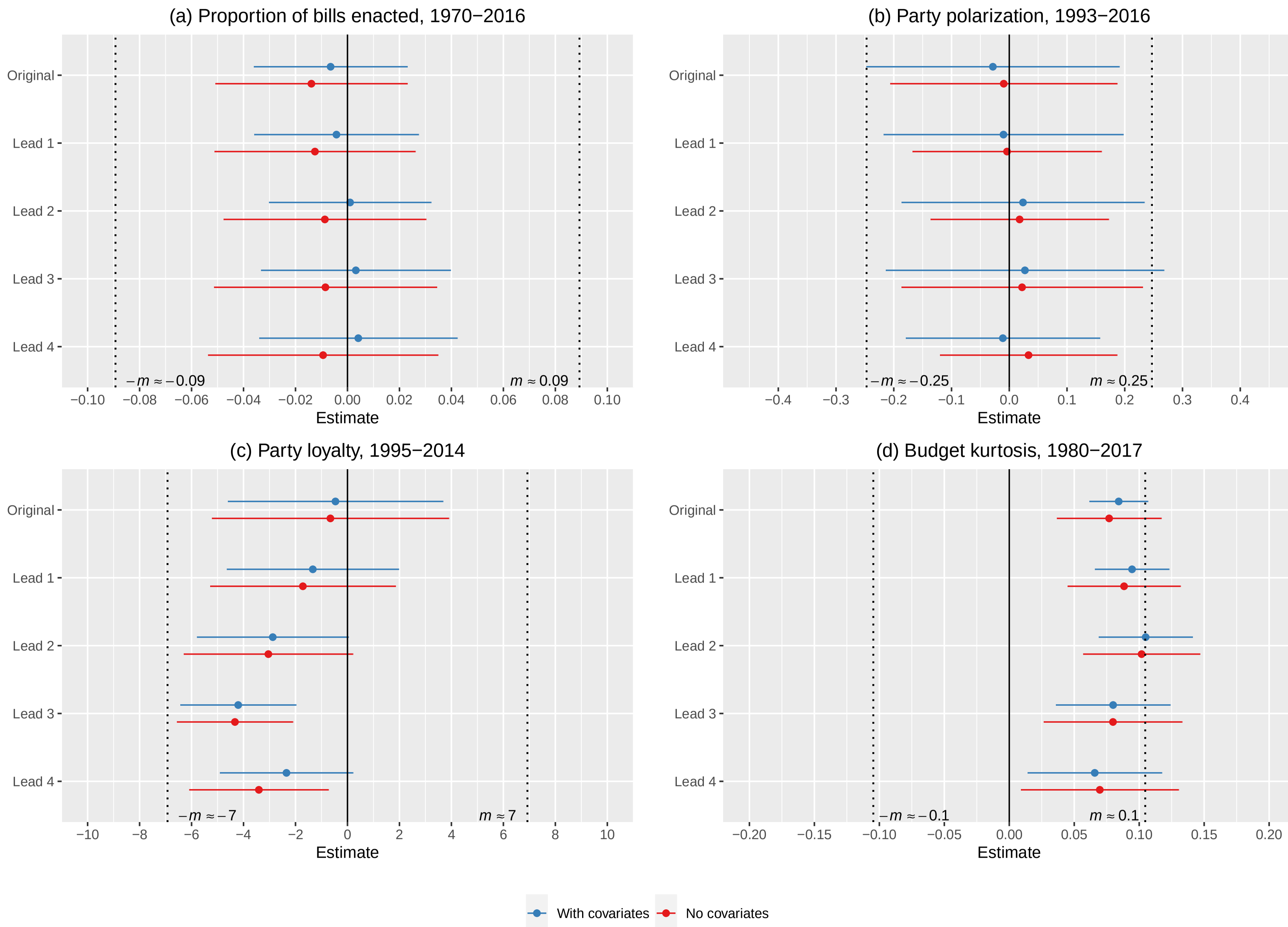
Figure 1: Variation in State Open Meetings Law Legislative Exposure, 1960–2018

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



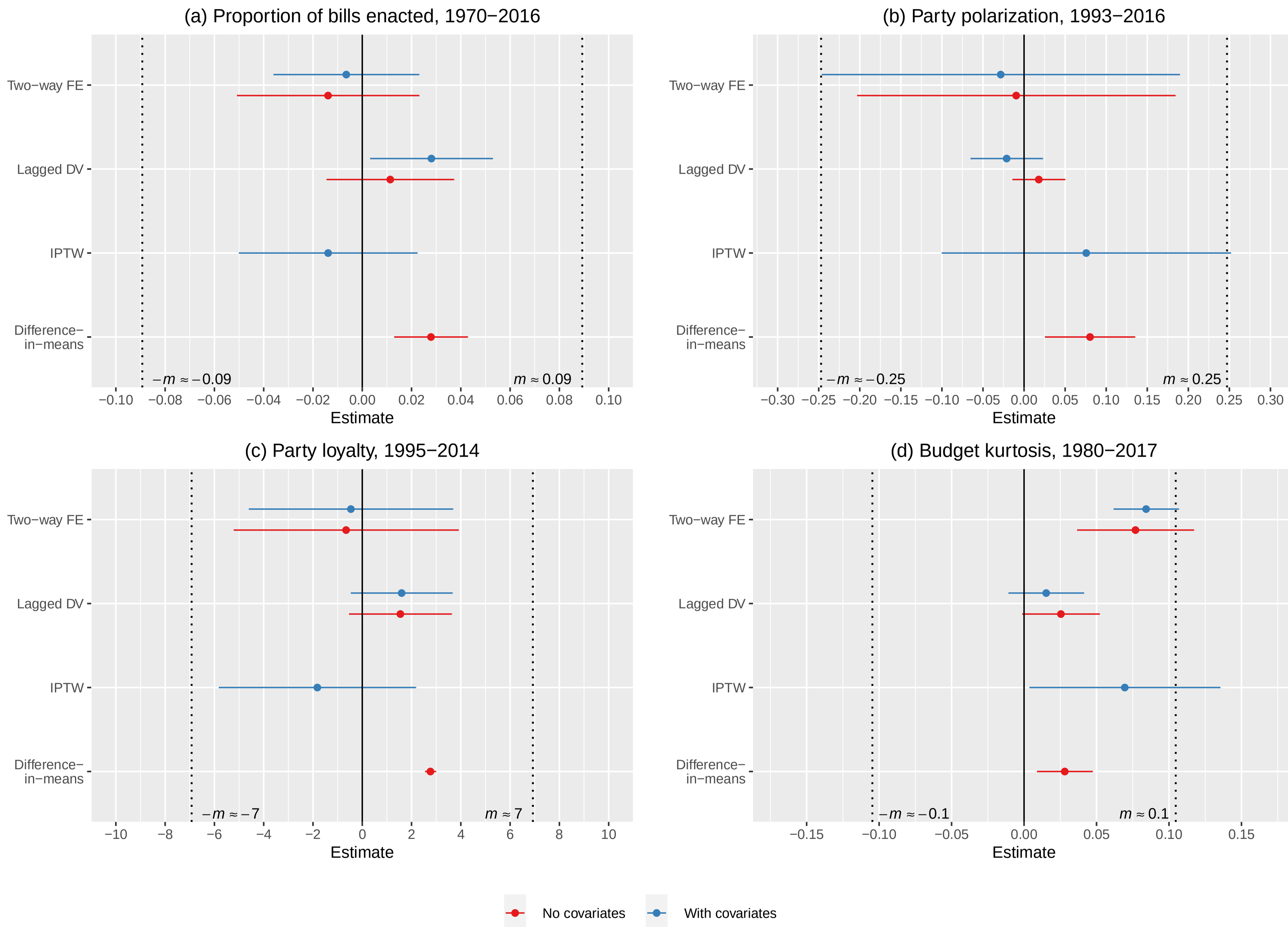
Note: The graph presents the average outcome for treated (red) and untreated (blue) states up to 1997, the year before the last open meetings law adoption (Nebraska). The vertical lines reflect adoption dates for the treated states listed on the graphs. Dot sizes are proportional to the sample sizes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

Figure 3: Estimated Effects of Treatment Leads



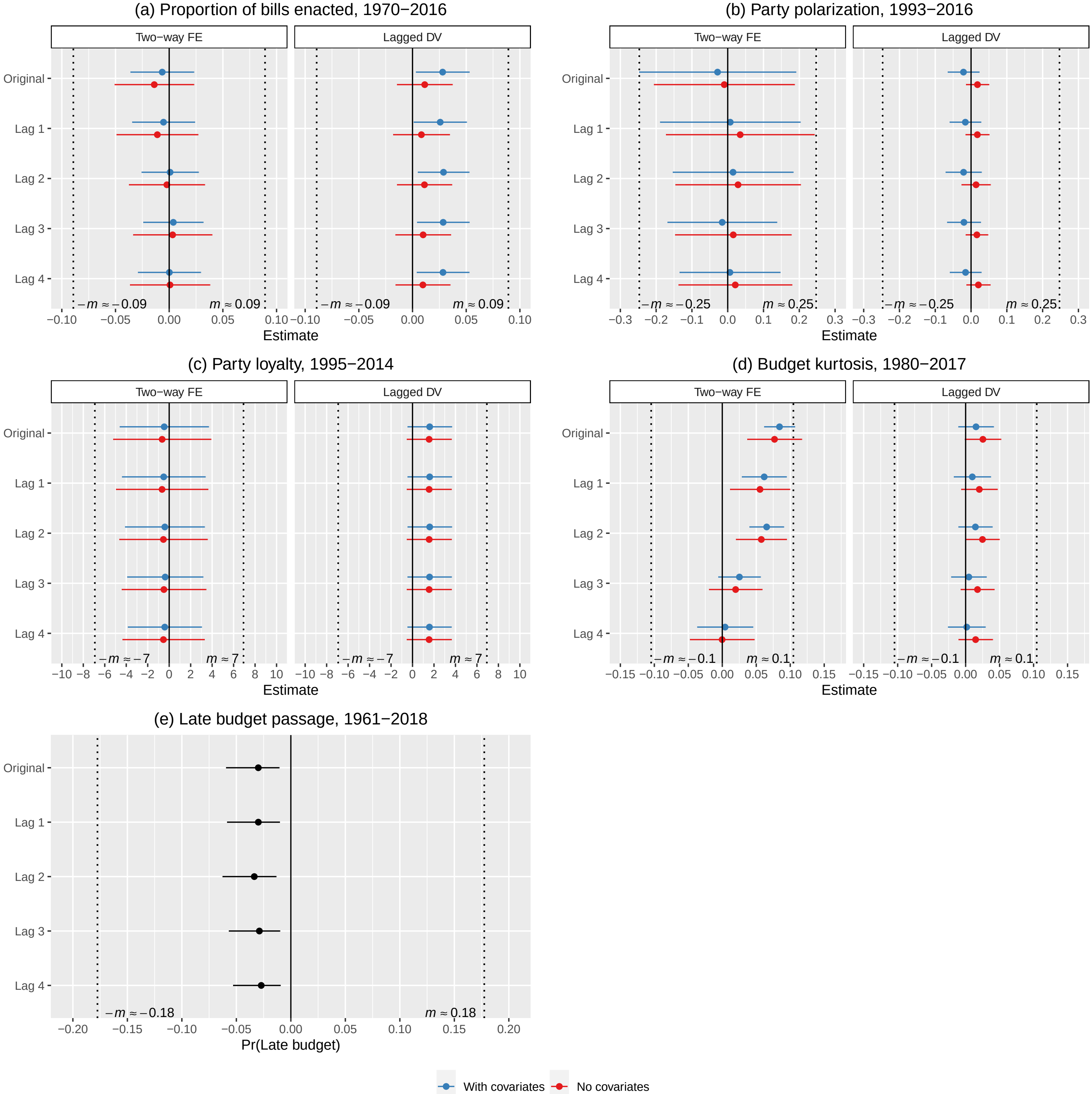
Note: The graphs present estimated treatment effects from the two-way fixed effects models for leads of treatment from 1 to 4 years. Points represent effect estimates and line segments represent 95% confidence intervals.

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



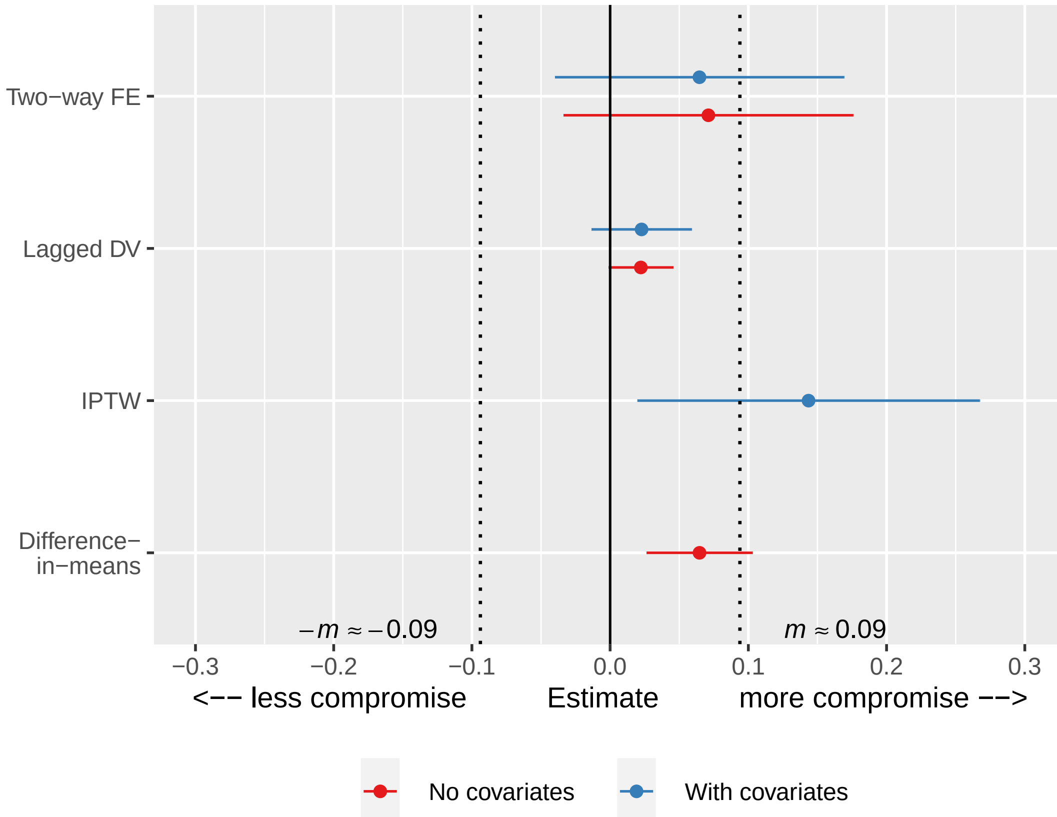
Note: The graphs present the estimated effects of exposure to open meetings laws and their 95% confidence intervals. The dotted vertical lines denote $\pm m$, our chosen threshold for a substantively meaningful effect. Only one IPTW specification is shown because that estimator requires covariates for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

Figure 5: Estimated Effects of Treatment Lags



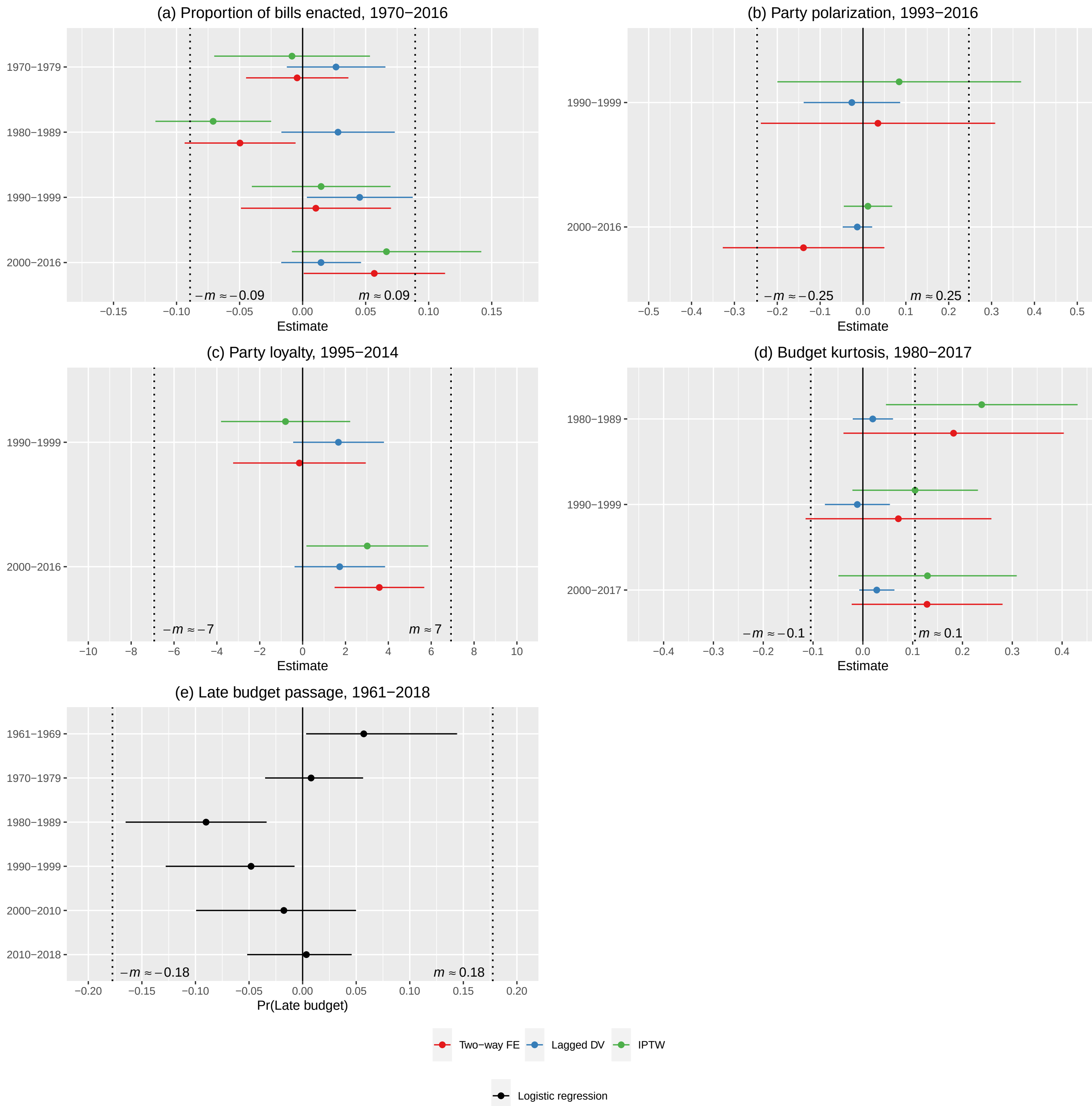
Note: The graphs present estimated treatment effects from the two-way fixed effects models (panels a–d) and random effects models (panel e) for lags of treatment from 1 to 4 years. Points represent effect estimates and line segments represent 95% confidence intervals. In panel (e) the effects are reported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

Figure 6: Treatment Effects on the Latent Compromise Outcome Variable



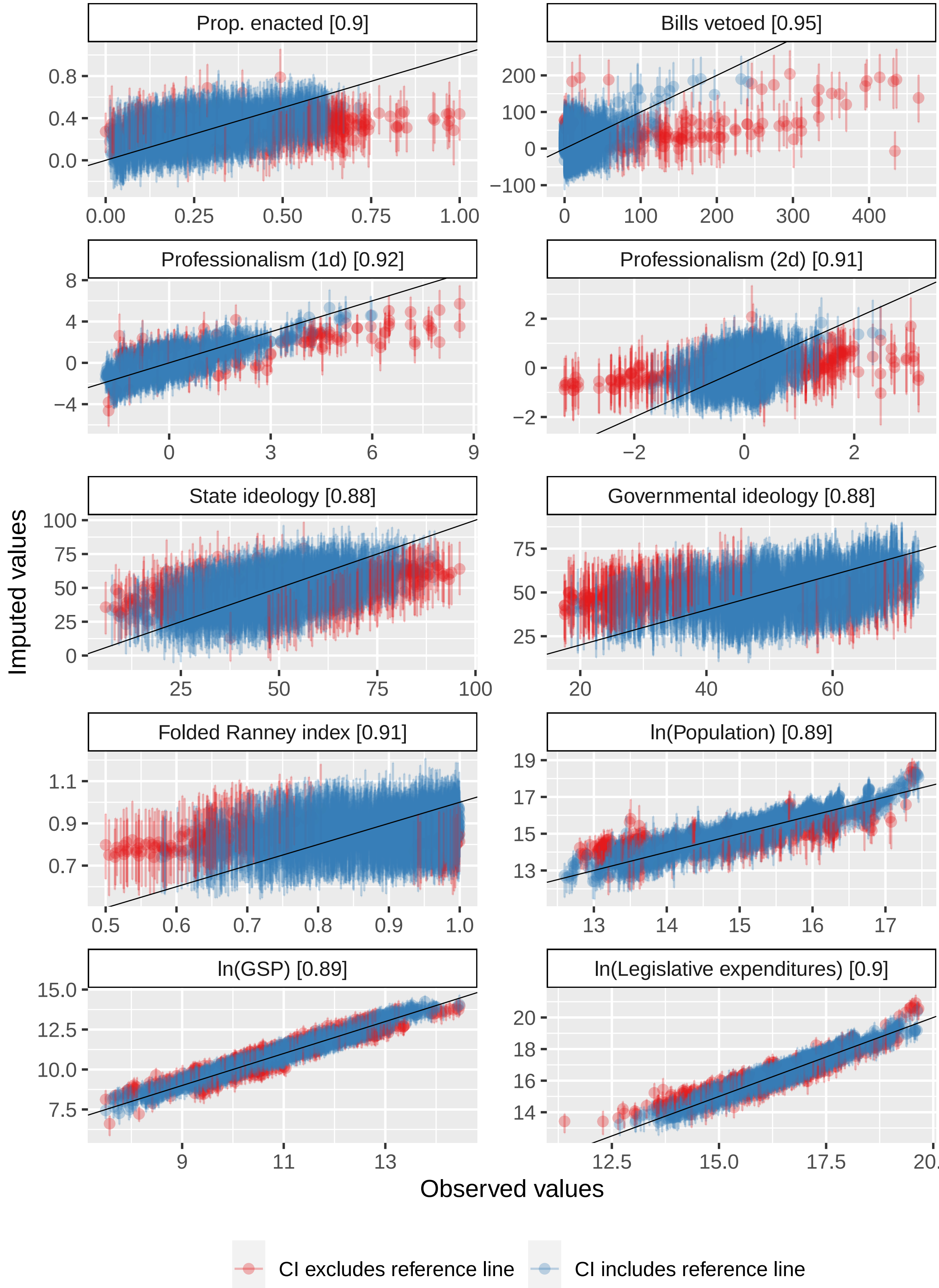
Note: The graph presents the estimated effects of exposure to open meetings laws and their 95% confidence intervals. The dotted vertical lines denote $\pm m$, our chosen threshold for a substantively meaningful effect. Only one IPTW specification is shown because that estimator requires covariates for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

Figure 7: Estimated Effects by Decade



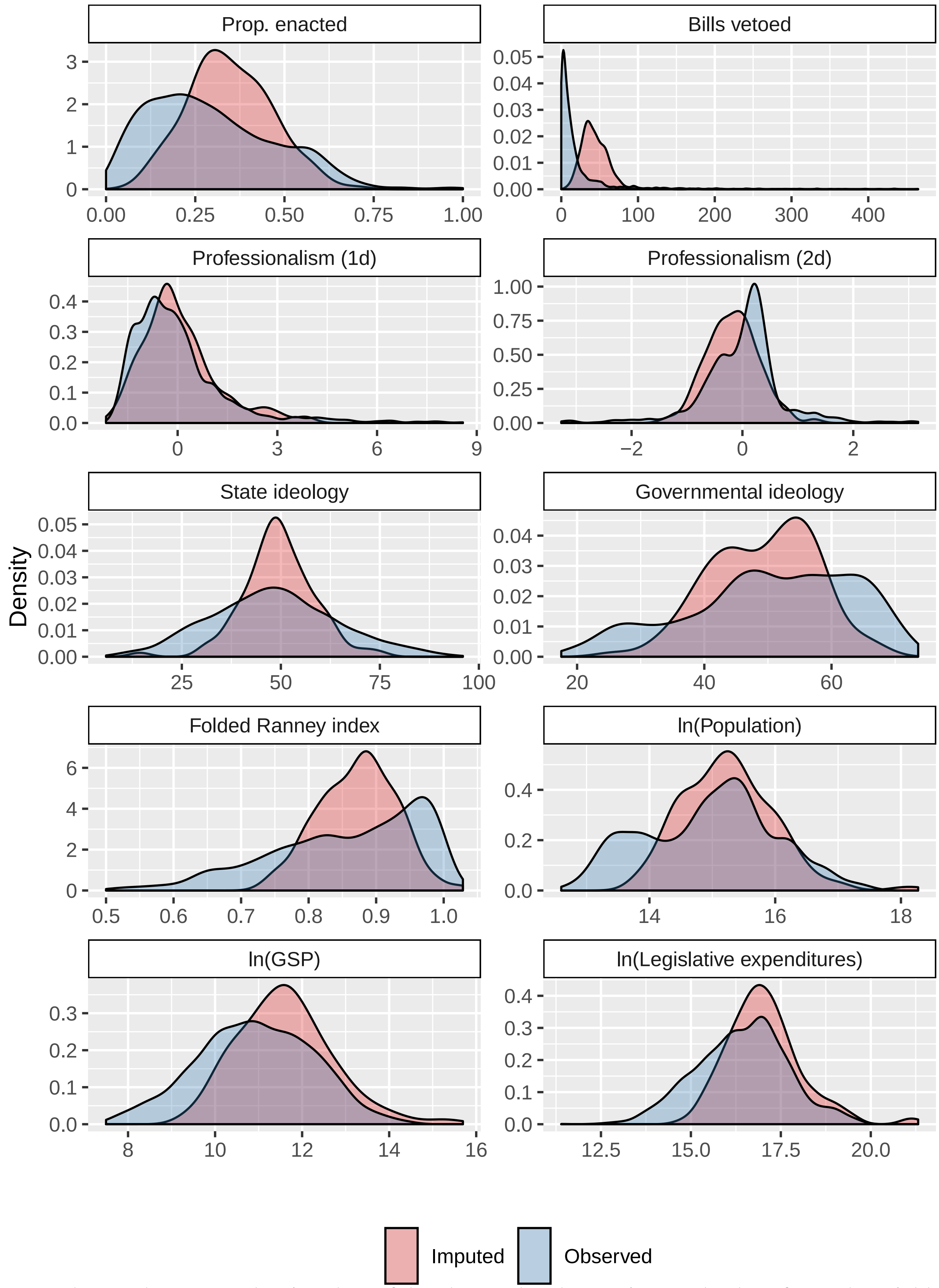
Note: The graphs present estimated treatment effects after subsetting by decade for several estimators. Points represent effect estimates and line segments represent 95% confidence intervals. In panel (e) the effects are reported on the probability scale. For the outcomes that include fixed effects models (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



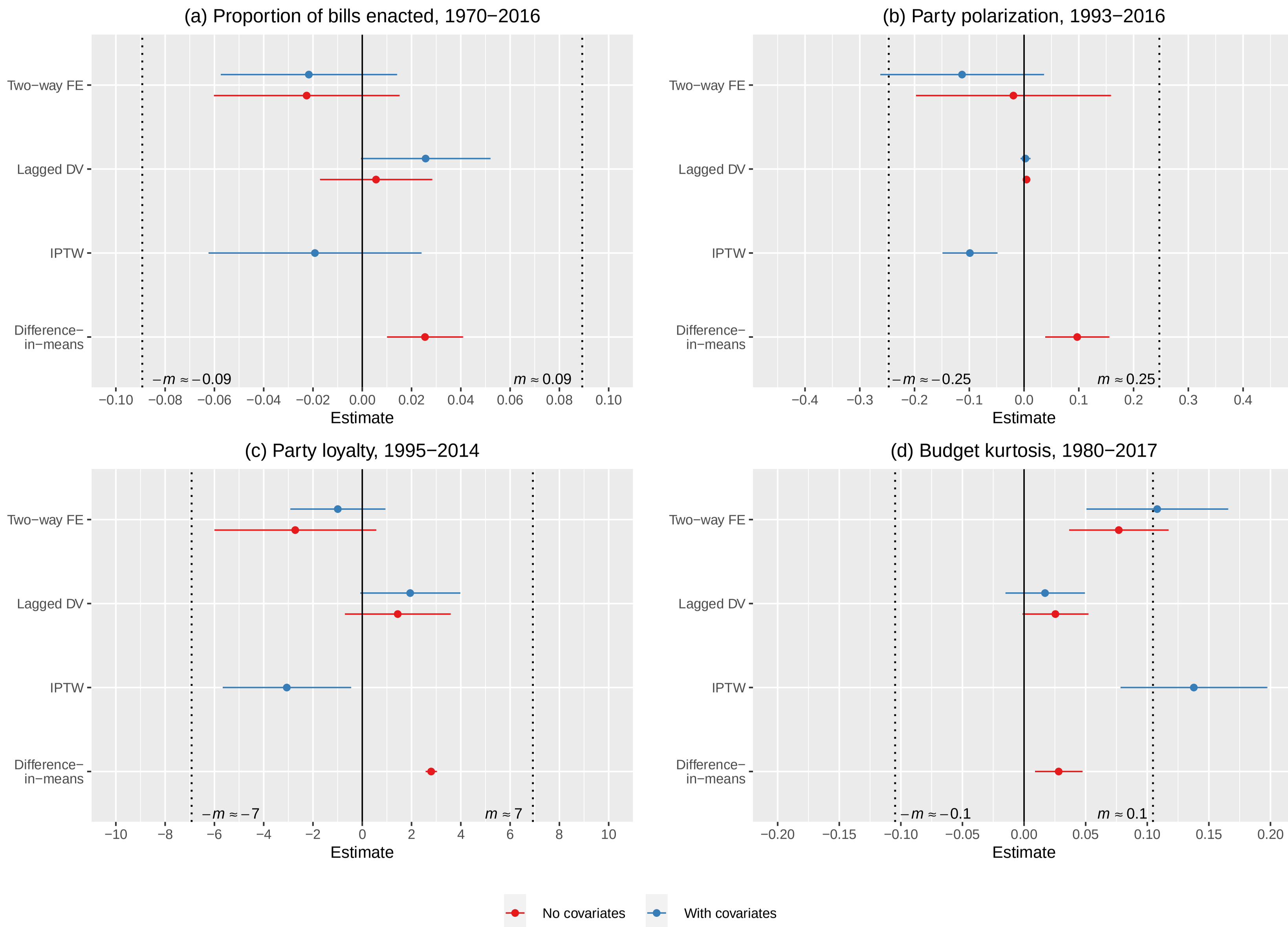
Note: The graphs present observed values of each variable on the x-axes against mean imputations of those values on the y-axes. Line segments indicate 95% confidence intervals. The solid line serves as a reference point for perfect imputation. The values in square brackets next to each variable label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

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



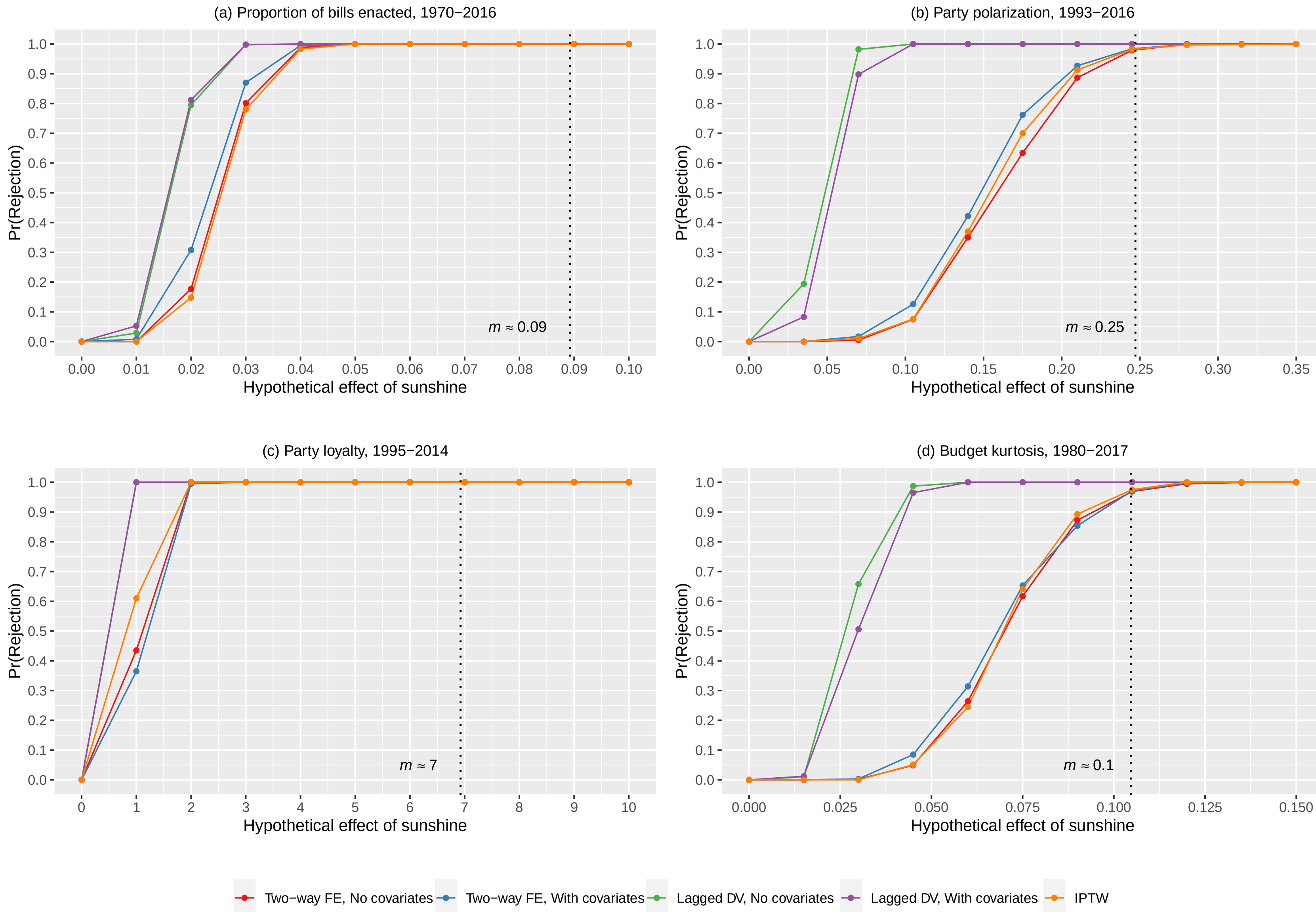
Note: The graphs present density plots of the observed and mean imputed values for each variable.

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.