The Polarization Will Not Be Televised: The Effects of Gavel-to-Gavel Floor Coverage on U.S. State Legislatures

Abstract

As elected officials and citizens struggle to understand the increasingly polarized and dysfunctional political landscape in the United States, some have pointed to the introduction of cameras in legislative bodies as driving the downward trajectory of these institutions. Increased transparency may make legislators more willing to engage in attention-seeking behavior, or may empower voters or partisan actors to more effectively monitor behavior. Previous research offers mixed conclusions, in part because of the focus on national legislatures where the introduction of cameras occurs only once. Using an original dataset of the adoption of gavel-to-gavel coverage in state legislative chambers, we examine whether cameras are associated with a range of chamber- and individual-level outcomes. The findings suggest that there are virtually no impacts of gavel-to-gavel coverage. Normative concerns about greater transparency seem overstated, an important finding given the proliferation of cameras in public proceedings that has occurred since the COVID-19 pandemic.

Word Count: 10,471

"It's probably the worst thing that happened to the Congress"—Representative Don Young (R-AK) on the introduction of C-SPAN in 1979.[1]

Representative Young is not alone in his assessment that when the cameras are rolling, policymaking processes deteriorate and outcomes are altered. Representative Jeff Jackson (D-NC) said in 2023, "I've been in committee meetings that are open to the press and committee meetings that are closed. The same people who act like maniacs during the open meetings are suddenly calm and rational during the closed ones. Why? Because there aren't any cameras in the closed meetings."[2] Outside observers share similar perspectives: Supreme Court Justice Sonia Sotomayor said on *The Daily Show* "…the partisanship in the Senate started to grow when cameras went into the Senate room."[3]

What are the effects on legislator behavior when "gavel-to-gavel" coverage is introduced in a legislature? Live coverage of the legislative process increases transparency, but may reward attention-seeking behavior, intransigence, or emotional appeals, which in turn, has a deleterious effect on institutional processes and outcomes. House member Matt Gaetz (R-FL) writes in his book that former Speaker Paul Ryan "knocked me for going on TV too much,"[4] while former member Max Rose (D-NY) said at a public forum in 2023 that, "every member of Congress...[is] a product to be branded and marketed...you don't get on TV for passing legislation. You get on TV for dividing, for saying things that are incendiary."[5] Gavel-to-gavel coverage may also empower special interests and other outlying constituency or donor groups to more effectively monitor legislators, encouraging policy outcomes that disproportionately favor the preferences of these groups.

Most research on the effect of live, gavel-to-gavel coverage has focused on national legislatures such as the U.S. Congress (Cook 1986; Mixon Jr, Hobson and Upadhyaya 2001; Mixon Jr and Upadhyaya 2002; Mixon Jr, Gibson and Upadhyaya 2003), the Canadian House of Commons (Soroka, Redko and Albaugh 2015), and the Turkish Parliament (Yildirim 2020). Findings are mixed with respect to the normative balance between increased transparency and altering legislator incentives and behaviors. Introducing cameras may increase media coverage of individual U.S. Representatives (Cook

[1]USAToday.com "Not everyone is a fan of C-SPAN cameras in Congress", https://www.usatoday.com/story/news/politics/2014/03/19/cspan-anniversary/6577593/

[2]Rep. Jeff Jackson's Twitter https://twitter.com/JeffJacksonNC/status/1647955875317833729

[3]The Daily Show, November 14, 2021, https://www.youtube.com/watch?v=HcMhgKywE1c

[4]CNN.com, "'He really jumped on the Trump train': How a brash Matt Gaetz climbed the ranks in Trump's Washington. https://www.cnn.com/2021/04/08/politics/matt-gaetz-florida-republican/index.html.

[5]"Former congressmen offer strategies for overcoming polarization and performative politics." *Cornell Chronicle*, March 30, 2023. https://news.cornell.edu/stories/2023/03/former-congressmen-offer-strategies-overcoming-polarization-and-performative.

1986), the length of legislative sessions (Mixon Jr, Hobson and Upadhyaya 2001), the number of filibusters in the U.S. Senate (Mixon Jr, Gibson and Upadhyaya 2003), and help to bolster senator incumbent reelection rates (Mixon Jr and Upadhyaya 2002). Other research finds that cameras have not altered the content or nature of debates (Soroka, Redko and Albaugh 2015).

While these findings are suggestive, their focus on national legislatures—which already receive far more media attention than other institutions—makes it difficult to draw inferences about possible effects in the vast majority of legislative or policymaking institutions about which voters have far less knowledge and receive far less information. Further, because all legislators in the national legislature only receive the treatment once, and at a single point in time, a host of possible time-based factors unrelated to camera introduction (e.g., increasing wealth inequality) could explain pre- and post-treatment differences in outcomes.

As an alternative approach, we introduce a new dataset on the adoption of broadcast or streaming gavel-to-gavel coverage of state legislative chambers. With data from 91 state-chambers on the implementation of television or streaming coverage occurring from 1987 to 2022,[6] we are able to use a difference-in-difference design with a significant number of observations, all of which occur within the same national context. These data allow us to answer substantively important questions about possible effects on individual- and chamber-level outcomes, while also clarifying findings from other institutional settings. At the chamber-level, we explore polarization, legislative productivity, and budget passage, and at the individual-level we assess legislator ideology, party loyalty, and legislator effectiveness.

Despite compelling reasons to suspect that greater visibility into the lawmaking process should cause outcomes and processes to deteriorate (e.g., Anderson, Butler, and Harbridge 2020), our findings suggest that there are few systematic consequences of the adoption of live gavel-to-gavel coverage in state legislatures. State legislative chambers do not become more or less polarized, dysfunctional or productive. Looking at individual legislators, the results are generally similar; there is no clear evidence that the adoption of gavel-to-gavel coverage alters their ideology, party loyalty, or legislative effectiveness.

The results suggest that a normative trade-off between transparency and dysfunction does not ex-

[6] We use the terms television or broadcast coverage, meaning live coverage consumed by viewers through their television set, noting that most early coverage was delivered via cable (technically not broadcast television). We use the term "streaming" to mean consumption by viewers through the internet via a dedicated website or third-party platform like `Youtube.com`. We recognize the distinction between television and streaming is increasingly blurred, but we do not separate, either theoretically or empirically, the type of coverage implemented in the states.

ist, echoing findings from Harden and Kirkland (2021) who employ a similar approach to explore the effects of sunshine laws. While the timeline of gavel-to-gavel coverage adoption coincides with significant changes in American politics, our evidence suggests live floor coverage is not a cause of these changes, and increased transparency need not carry deleterious democratic consequences. This finding is especially welcome as the use of cameras and televised/streamed proceedings exploded in popularity following the COVID-19 pandemic,[7] and as many advocate for increased transparency in institutions, such as the American federal court system, that have long resisted them.

Two Theoretical Perspectives on Why Cameras Might Matter

While anecdotes from current and former legislators are colorful and plentiful, existing social science research offers mixed theoretical guidance on how behavior might be affected. We draw on two distinct literatures that describe mechanisms on how cameras might matter (or not). We first survey the social psychology literature, which suggests that legislators may engage in emotional appeals to conform to social norms among in-group members. Second, we draw on the representation literature to discuss how cameras might decrease monitoring costs for voters or partisan actors, thus incentivizing legislators to behave differently.

Psychological Responses and Social Conformity

Psychologists have demonstrated that observing oneself or being observed by others heightens one's sense of self-awareness (Morin 2004). Forms of self- or social-observation include standing in front of an audience (Buss 1980; Carver and Scheier 1978), listening to a tape recording of one's voice (Wicklund and Duval 1971), or even looking in a mirror (Carver and Scheier 1978; Davis and Brock 1975; Wicklund and Duval 1971). A considerable body of research demonstrates that being in front of a camera can also induce self-awareness (Davis and Brock 1975; Li et al. 2022; Morin 2011; Silvia and Phillips 2004). When confronted with these self-focusing events and the ensuing self-awareness, individuals go through a process of self-evaluation that identifies mismatches between themselves and the expectations or norms around them (Wicklund 1975). In response, they may attempt to escape or avoid the stimuli that focuses attention on them, but if they are unable to do so, they will modify their

[7] Streaming proceedings on platforms such as `Youtube.gov` is now exceedingly common for state and local governmental bodies. School districts (e.g., Seattle Public Schools), city councils (e.g., New Orleans, LA and Mobile, AL) and state bureaucracies (e.g., Iowa Department of Corrections, Utah Department of Natural Resources) all live-stream their proceedings, policymaking processes, and votes, as of 2024.

behavior leading to more pro-social actions that align with group norms (Bateson, Nettle and Roberts 2006; Munger and Harris 1989). For example, the presence of a security camera may make people more likely to help others in observable ways (Van Rompay, Vonk and Fransen 2009) and reduce the "bystander effect" in online settings (Van Bommel et al. 2012).

If legislators are psychologically induced to behave in a more pro-social manner, to which group do they appeal? The claim among opponents of increased transparency is that legislators signal shared in-group characteristics to donors, activists, or co-partisans; they act poorly to attract attention, help build a brand, sell books, or secure future financial returns through their notoriety. Additionally, opponents of cameras argue there is not an equally compelling case that observer attention should make legislators more likely to conform to "average" or "typical" voters.[8] The same returns to the legislator for moderate or reasonable behavior do not exist. Legislators do not receive attention for moderation, and financial benefits are more difficult to secure when engaging in behavior that does not directly appeal to in-group members. To summarize, legislators who know they are being observed feel psychological pressure to demonstrate shared in-group characteristics with observers who are likely to reward them for their behavior.

Recent evidence, however, casts doubt on claims that heightened self-awareness necessarily makes individuals more responsive to social norms (Dear, Dutton and Fox 2019; Northover et al. 2017). Explanations of these non-effects rely on heterogeneous responses to self-focusing stimuli (in this case, cameras). Individual traits can predispose people to reject any perceived discrepancy between themselves and the norms that the broader group expects or they may re-characterize expectations and norms to align with their own actions and behaviors (Duval and Lalwani 1999; Duval and Silvia 2002). A legislator under the watchful eye of a camera may not perceive of a disconnect between their actions and the expectations of the monitoring entity, or they may blame voters, activists, or other observers for any discrepancy.

Empirical findings in the policy space call observer effects into question. For example, research in criminal justice finds scant evidence of behavioral changes in police officers and those whom they interact with when the officers wear body cameras despite the widespread implementation of this policy (Lum et al. 2020, 2019). Similarly, video recordings of physician-patient interactions show that camera-related behaviors are fairly infrequent (Penner et al. 2007; Pringle and Stewart-Evans 1990).

[8]E.g., Mark Strand and Timothy Lang, "The Super Secret Committee," *The Congressional Institute*, November 21, 2011, https://www.congressionalinstitute.org/2011/11/21/the-super-secret-committee/.

In the case of legislators, cameras may show appeals to extremists or polarizing behavior that reflect their existing preferences, or allow for more efficient signaling to their in-group members, rather than inducing a social or emotional change in a legislator who would otherwise behave reasonably.

Monitoring and Electoral Incentives

A second strain of research relies not on psychological conformity and emotional appeals to in-group members, but instead suggests rational legislators respond to incentives that result from increased monitoring of their behavior. Gavel-to-gavel live streaming/broadcast coverage of legislatures should lower monitoring costs for interested observers, which in turn, reduces legislator agency and makes them more responsive to observers' preferences.

The normative motivation behind the adoption of gavel-to-gavel coverage is to increase the ability of voters to gather information and monitor their legislator and the institution as a whole. A substantial literature demonstrates that even one "bad" or incorrect vote results in electoral punishment for legislators at the congressional level (Ansolabehere and Kuriwaki 2020; Canes-Wrone, Brady and Cogan 2002; Carson et al. 2010; Nyhan et al. 2012) and that legislators are exceedingly risk averse when casting roll call votes (Kingdon 1973; Matthews and Stimson 1975; Sullivan et al. 1993). Electoral punishment relies on voters *knowing* their legislator's behavior, however (Canes-Wrone and Shotts 2004; Kalt and Zupan 1990; Snyder and Stromberg 2010), and the adoption of gavel-to-gavel coverage should increase the quantity and quality of relevant information transmitted to voters. Citizens are largely unaware of who their legislators are, or what takes place in their state legislature (Carpini, Keeter and Kennamer 1994; Rogers 2023), so increases in access to information ought to produce legislative behavior and policy outcomes more in-line with citizen preferences. The result, according to advocates of gavel-to-gavel coverage, is likely to be normatively positive outcomes as a result of lower information acquisition costs for voters and thus, better monitoring.

If voters exercise greater control over legislators as a result of gavel-to-gavel coverage, legislators should become more moderate, willing to compromise, and less partisan. In single-member districts, the legislator must appeal to the median voter, who by definition, lies in the middle of the preference distribution of all voters within the district (Downs 1957). Survey data show elites are more polarized than the general public (Enders 2021; Hill and Tausanovitch 2015; Levendusky 2009), citizens value compromise and wish to see elected officials set aside partisan differences (Hibbing and Theiss-Morse 2002; Wolak 2020), and even partisans broadly agree upon and support democratic norms (Holliday et al. 2024).[9]

Importantly, gavel-to-gavel coverage does not require all voters to pay constant, close attention, to legislator behavior. Even if voters are lax in their oversight of elected officials, behavior can be revealed by other actors. Campaign advertisements, for example, can show undesirable behavior rather than describing it through verbal accounts. Local media outlets can facilitate citizen learning by pulling video clips of events that occur and showing them to viewers who may not have otherwise been exposed to them. Cameras allow monitoring to occur at any time, when a salient issue reaches the legislative agenda. For example, in 2013 Texas state senator Wendy Davis filibustered a bill which sought to restrict abortion in the state.[10] She received national attention, as over 180,000 people tuned in to watch her filibuster via livestream, clips were posted to Twitter, and President Obama tweeted, "Something special is happening in Austin tonight. #StandwithWendy."[11]

Opponents of increased transparency fear that the decrease in information acquisition costs may not be sufficient to engage the vast majority of voters, and may actually increase non-representative behavior by exacerbating information differences between voters and party actors such as interest groups, leaders, or activists (Bawn et al. 2012). Political elites and campaign donors are more polarized than the mass public (Enders 2021; La Raja and Schaffner 2015), and activists have been central in driving conflict extension across issue domains (Layman et al. 2010). If recorded behaviors are more likely to reach these groups than the average voter, legislators will be aware of how the most partisan actors perceive their behavior on the floor, and seek to appeal to them.

Effects of monitoring from party activists are amplified by electoral considerations. One of the principle fears of state legislators is facing a challenger in a primary election, and elected officials perceive that compromise and bipartisanship are a liability in these contests (Anderson, Butler and Harbridge 2020). Knowing that activists and interest groups are heavily involved in primary contests (Grumbach 2020), legislators might adjust their behavior to become more extreme when cameras are introduced.

[9]Observing democratic institutions at work also has positive effects on voters. Evidence on the courts suggests that citizens perceive the institution to be more legitimate if they observe it in action (Black et al. 2023; Cann and Goelzhauser 2024).

[10]The bill failed in the session in which Davis filibustered, later passed in a special session, and was then invalidated by a 2016 Supreme Court ruling.

[11]Alana Rocha, Justin Dehn, Todd Wiseman and Tenoch Aztecatl, "Running out the clock: The Wendy Davis abortion filibuster, 5 years later," *Texas Tribune*, June 25, 2018; Tom Dart, "Wendy Davis's remarkable filibuster to deny passage of abortion bill," *The Guardian*, June 26, 2013.

Empirical Expectations

Combined, both the social psychology and representation literature offer different perspectives about how the introduction of cameras may affect legislator behavior. They may feel pressure to make emotional appeals to in-group members or engage in more pro-social behavior, but the empirical evidence that this occurs in other policy settings is weak. Increased monitoring may make them more responsive to voters, or to ideologically extreme party actors. Recent research examining a different form of transparency—sunshine laws—finds that their effects on outcomes in state legislatures are minimal (Harden and Kirkland 2021). The authors posit that this is likely a function of low citizen knowledge and engagement with state government. With an apathetic and disengaged public, legislators may not be concerned about changes in monitoring from any group enough to alter their behaviors. And, even if they are concerned about monitoring from the public, activists or interest groups, legislators can respond by simply shifting the venues where important business is conducted. If the chamber floor is now on display to the public, conversations and bargaining move to private offices or other locations where cameras are absent (Anderson, Butler and Harbridge 2020).

We develop two empirical expectations that describe the different possible outcomes as a result of camera adoption. The first is that cameras produce more dysfunction, partisanship, extremity, and discourage compromise, either because they encourage in-group emotional appeals or because legislators are incentivized to represent outlying preferences. The second describes the opposite, driven by theoretical claims that cameras incentivize individual legislators to engage in "good" behavior that satisfies ordinary voters.

Dysfunction/Extremity Hypothesis: Legislatures that adopt gavel-to-gavel coverage see more partisanship and less productivity at both the chamber- and individual-level, compared to those that do not adopt gavel-to-gavel coverage.

Moderation Hypothesis: Legislatures that adopt gavel-to-gavel coverage see less partisanship and more productivity at both the chamber- and individual-level, compared to those that do not adopt gavel-to-gavel coverage.

There are also theoretical reasons to expect that the effects of cameras will be negligible. The corresponding null hypothesis is that the introduction of gavel-to-gavel coverage does not produce different observable outcomes for partisanship or productivity at the chamber- or individual-level compared to legislatures which do not adopt gavel-to-gavel coverage.

Data Collection Process

A major inferential advantage of using state legislatures to identify possible effects of live floor coverage is that there exists significant variation in adoption timing, both within and across states.[12] This allows us to control for possible confounding state- and time-level factors. As our descriptive data show, some state-chambers adopted live television coverage of their floor proceedings in the early 1990s, while a number of state-chambers began web streaming or television broadcasting only as recently as 2020, in response to limitations on public access to the chambers during the COVID-19 pandemic.

To our knowledge, there is no existing, comprehensive dataset of state adoption of floor coverage. Our initial attempts to use online sources or other available references provided data on only a very limited subset of data. While the National Conference of State Legislatures lists states which currently televise or stream floor proceedings, their data do not list the year of implementation.[13] The strategy we used was to call and email state legislative offices and request the information. We identified a likely source within each legislature or chamber using web resources (e.g., the chamber reference librarian, the clerk, etc.), then requested contact information for other officials if our first contact was unavailable or did not know when coverage adoption occurred. We maintained notes on the phone calls, including the name of the person who provided us with the information and their title. We found for the vast majority of state-chambers that emails did not generate responses from state legislative offices. If we were not able to make contact with a state employee who could provide us with the requested information, we followed up with repeated emails and phone calls, necessary in the vast majority of states. Even after multiple attempts across two years, we were not able to collect data for nine state-chambers, out of 99 total.[14] In a few cases, we could find information about adoption on the webpage of the broadcast networking, typically under a FAQ or "About Us" section. Figure 1 shows yearly adoption of gavel-to-gavel coverage by state-chamber, ordered by date of coverage adoption. Table A1 contains additional details about the data, including the sources who provided the

[12]For example, the Louisiana House adopted live streaming in 1999, while the Senate did not start streaming until three years later, in 2002.
[13]See: https://www.ncsl.org/resources/details/legislative-broadcasts-and-webcasts.
[14]Data collection occurred from February 2022 through April 2024.

chamber-level information on adoption.

Figure 1: Distribution of Treatment Across State-Chamber-Years

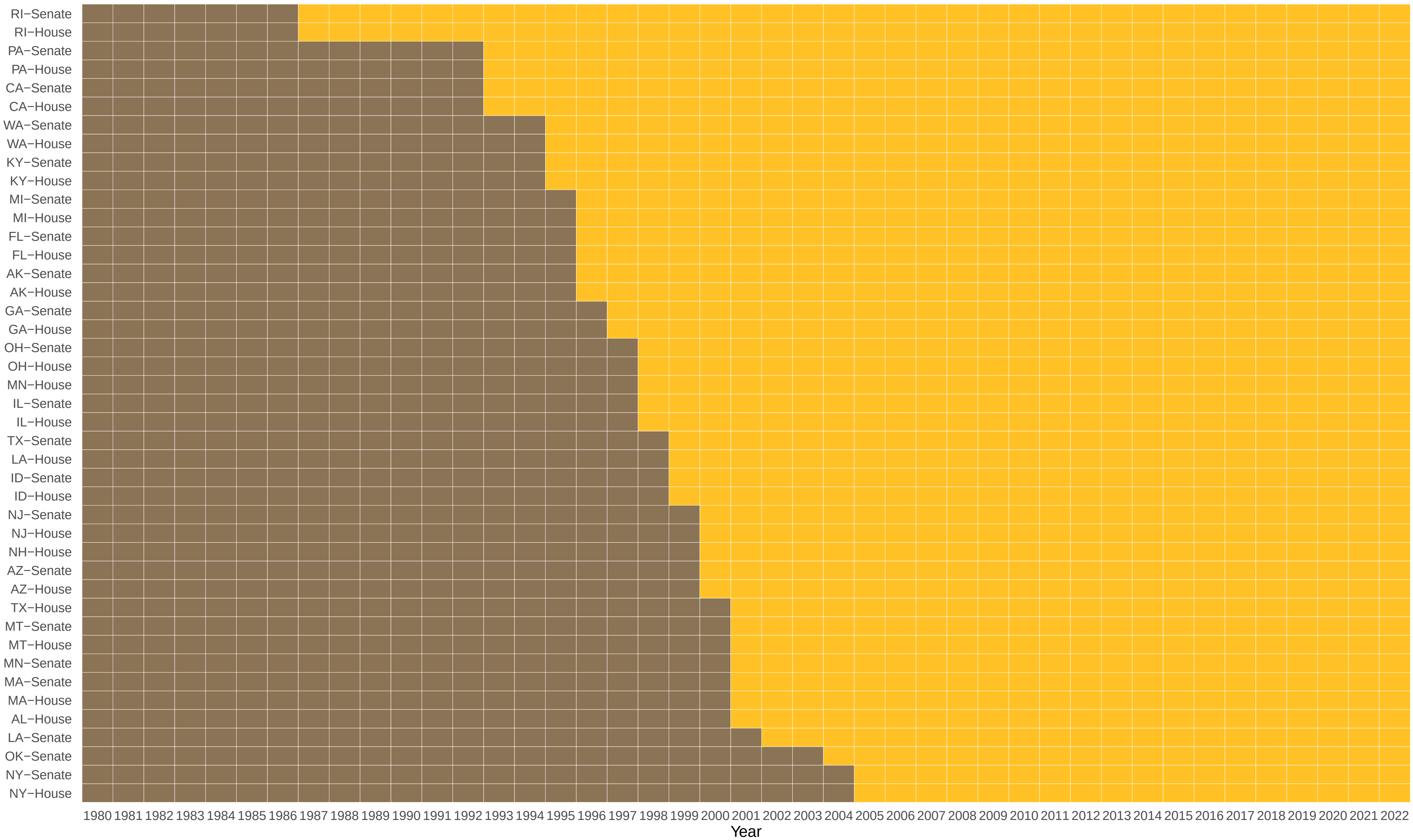
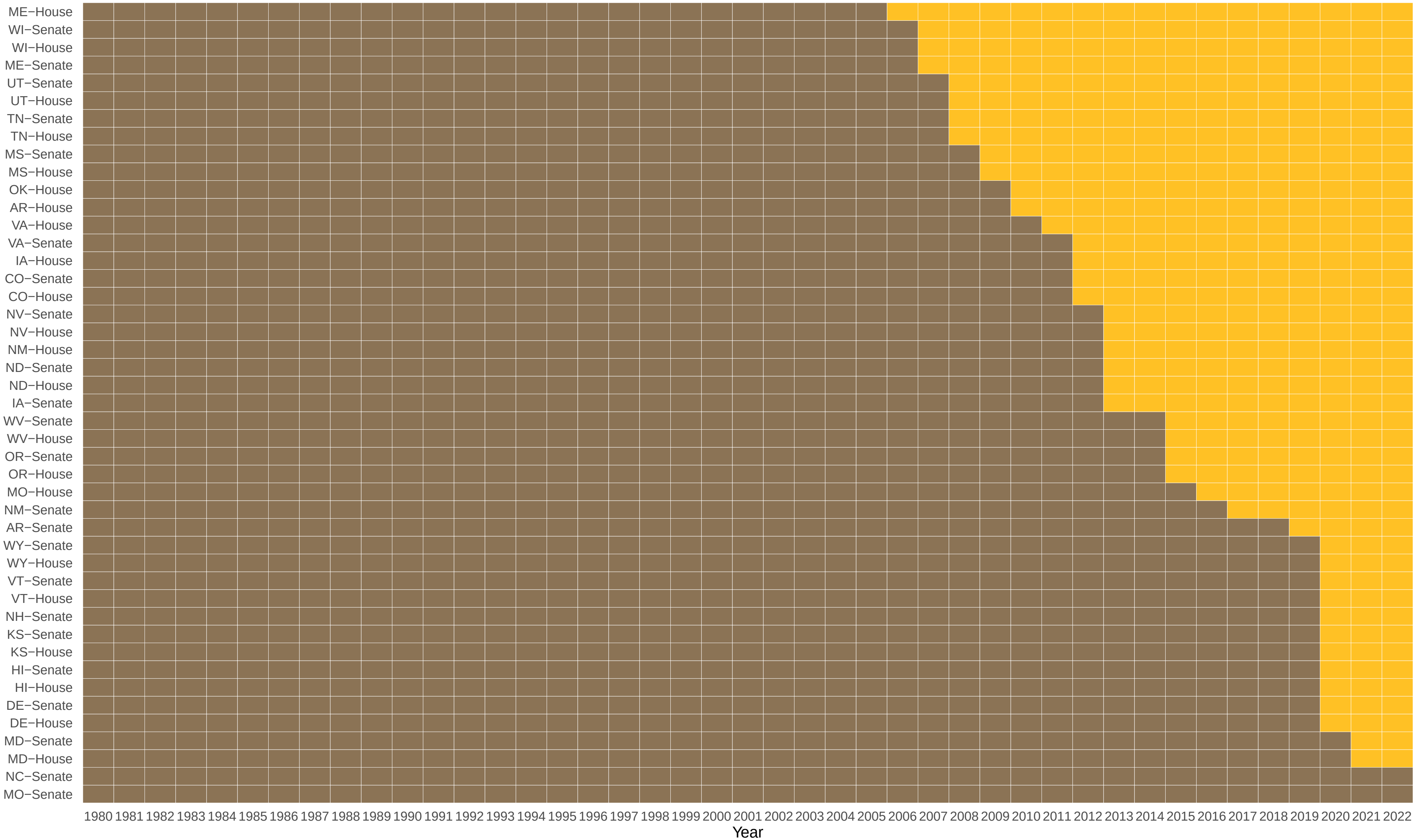


Figure 1 (cont:): Distribution of Treatment Across State-Chamber-Years



Note: The figures display treatment assignment variation by state-chamber-year, ordered by adoption of broadcast or streaming (the treatment). Brown panels represent state-chamber-years without broadcast/streaming coverage, yellow panels represent state-chamber-years with broadcast/streaming coverage. Nebraska and missing state-chambers excluded.

In most cases for which we were not able to obtain the date of adoption, it was due to an inability to make contact with an appropriate staffer at the state legislature (i.e., emails and phone calls went unanswered). In a few cases, we made contact with staffers, but they could not identify when adoption occurred. For three chambers, the Alabama Senate and South Carolina House and Senate, we were unable to speak to any staffer or receive a response after repeated emails and phone calls.

Additional Details and Assumptions

We assume the nine state-chambers for which we do not have data are missing at random. There is no correlation between state legislative professionalism and missingness as, for example, we were unable to gather data for both Connecticut and South Carolina chambers, which have professionalism scores higher than average, and both South Dakota and Indiana chambers, which have lower than average professionalism scores (Squire 2024). Though one might assume that legislatures with lower professionalism were less likely to respond, in our contacts with staffers, it was often more difficult to speak with someone in higher professionalism states, presumably because these staffers are busier.

The time units used in the analysis are years. In most states, staffers in the state legislative offices could not identify a specific month in which coverage began. Further, most covariates used in the analysis are measured at the year-level. Thus, the empirical models assume that effects occur in the year of adoption. However, for those states in which we could identify a month, it was almost always at the beginning of a year or term, suggesting that this empirical assumption is valid. We also empirically examine lagged effects to account for possible delays in behavioral changes by legislators or parties.

Some states began broadcasting or streaming committee activity before or after floor coverage, and in some cases, states archived audio prior to video broadcasting or streaming. Because of inconsistencies in data availability across states, we consider treatment to occur when streaming or broadcasting of gavel-to-gavel floor coverage began, ignoring the implementation of audio-only or committee action. We are only interested in the first implementation of gavel-to-gavel coverage. Some states initially adopted broadcasting and subsequently moved to streaming, or to both methods (e.g., Kentucky in 2015), but staffers' knowledge about these changes is limited. Further, we expect moving from television broadcasting to web streaming has minimal effects on behavior given that our theory focuses on the presence of cameras rather than the medium of delivery. Thus, our treatment variable is equal to one beginning with the state-chamber-year in which gavel-to-gavel floor proceedings were either first broadcast or web streamed to the public.[15]

Chamber- and Legislator-level Outcomes

Our dependent variables of interest are largely drawn from Harden and Kirkland (2021) and measure chamber- and individual-level legislative outcomes.To these, we also add chamber-level within-party cohesion, a commonly used measure of chamber polarization for both Democrats and Republicans (Kirkland 2014), and newly available individual-level legislator effectiveness data (Bucchianeri, Volden and Wiseman 2024). Table 1 shows these measures and the implications of a positive effect of coverage.

Normative Implications of Outcomes

Most obviously, late passage of a budget has important fiscal and policy effects on state government, while also creating uncertainty for the public and financial institutions. For example, if a budget is passed late, public employees may receive IOUs rather than paychecks (as California most recently issued in 2009), financial lenders may not receive payments, and the state may have its credit rating downgraded, all of which may harm the state's economy while also imposing significant costs on individual state employees or other members of the public who depend on state spending (Andersen, Lassen and Nielsen 2012). A positive effect of state-chamber adoption of televised/streamed floor proceedings on the probability of late budget adoption indicates that polarization and unwillingness to compromise are a product of increased public attention and visibility.

The difference in party medians and a decrease of within-party heterogeneity are two related components of increased polarization within a chamber (Aldrich and Rohde 1998; Aldrich and Battista 2002), and polarization is almost universally seen as normatively bad for the legislative process and policy outcomes. Polarization between Republicans and Democrats has been blamed for the decline in institutional comity, more extreme policy outcomes, a rise in negative partisanship and political violence, and mounting distrust of political institutions (Abramowitz and McCoy 2019; Jones 2015; Mason 2015; Thomsen 2014). A positive effect of live floor coverage via broadcast/streaming on party median difference (i.e., parties grow more ideologically distinct), and a negative effect on within party heterogeneity (i.e., party member ideology becomes more cohesive) indicate that gavel-to-gavel coverage helps accelerate polarization within the state's legislature. As described previously, this claim is

[15] There are no instances in which a state-chamber began broadcasting/streaming then subsequently stopped.

Table 1: Outcomes Predicted by Adoption of Gavel-to-Gavel Coverage

Concept	Measure	Source	Empirical Implications	Sample Years
State-Chamber-Year Level Measures				
Late Budget	Budget enacted after state statutory requirement; dichotomous indicator, 1=yes	Harden and Kirkland (2021) from Klarner, Phillips and Muckler (2012)	Positive coefficient indicates adoption increased probability of late budget adoption in states; suggests more polarization/position-taking	1980-2018
Budget Kurtosis	Distribution of percentage spending changes in 20 public policy categories; continuous measure, increasing values indicate more kurtosis	Harden and Kirkland (2021) from Epp (2018)	Positive coefficient indicates adoption produced greater budgetary changes in states; suggests more uneven policy change	1980-2017
Inter-party Polarization	Absolute difference in state-chamber party median NPAT scores; continuous measure, increasing values indicate greater difference	Shor and McCarty (2011)	Positive coefficient indicates adoption produced greater party differences; suggests more polarization	1993-2018
Democratic Intra-party Polarization	Standard deviation in Democratic Party within state-chamber NPAT scores, continuous measure, increasing values indicate more cohesiveness	Shor and McCarty (2011)	Positive effect indicates adoption produced greater Democratic Party cohesiveness; suggests more polarization	1993-2018
Republican Intra-party Polarization	Standard deviation in Republican Party within state-chamber NPAT scores; continuous measure, increasing values indicate more cohesiveness	Shor and McCarty (2011)	Positive effect indicates adoption produced greater Republican Party cohesiveness; suggests more polarization	1993-2018
Legislative Productivity	Proportion of bills introduced passed into law within state-chamber; continuous measure, higher values indicate more productivity	Harden and Kirkland (2021)	Positive coefficient indicates adoption produced more bills passing; suggests more active legislators, greater compromise within the state-chamber	1981-2016
Legislator-Year Level Measures				
Legislator Ideology Score	Legislator-year NPAT score; continuous measure, decreasing values for Democrats and increasing values for Republicans indicate more extremism	Shor and McCarty (2011)	Conditional on party, positive/negative coefficient indicates adoption produced more ideological legislators; suggests more polarization	1993-2018
Party Loyalty	Percentage of legislator-year votes in agreement with party on party votes; continuous measure, higher values indicate more loyalty	Shor and McCarty (2011)	Positive coefficients indicate adoption produced more loyal partisans; suggests more polarization, less compromise	1995-2014
Legislative Effectiveness Score	Proportion of legislator-year bills advancing to different stages of process; continuous measure, higher values indicate more productivity	Bucchianeri, Volden and Wiseman (2024)	Positive coefficient indicates adoption produced more effective legislators; suggests more responsive representation	1987-2018

Note: Sample years indicate minimum and maximum years with at least one observation; not all state-years are available within the years noted.

common in popular accounts of American legislatures.

The policy literature treats budget kurtosis as symptomatic of a dysfunctional legislature (Harden and Kirkland 2021). If the distribution of budgetary changes has fat tails, there is a greater proportion of small values, indicating budgetary stasis, and a greater proportion of high values, indicating dramatic budgetary changes. These patterns are indicative of a legislature that does not engage in regular or routine budgetary maintenance, which may produce poorer policy outcomes (Epp 2018; Jones and Baumgartner 2005), such as lower long-term growth rates (Breunig and Koski 2012).

Our three legislator-level outcomes are ideology, party loyalty, and effectiveness. Legislator ideology allows us to capture changes in the relative extremity of individual Democrats and Republicans. These ideology scores are the individual data points used to construct the chamber-level party measures (difference in party medians and party standard deviation) and are taken from voting records within each state legislative chamber (Shor and McCarty 2011). More extreme Democratic legislators (i.e., more liberal voting records) have decreasingly negative scores, while more extreme Republicans have increasingly positive scores. Greater extremity promotes gridlock and vitriol within the legislature.

High levels of party loyalty is largely seen as normatively bad in modern American legislatures, despite the organizational and collective benefits cohesive parties provide within the legislature (Aldrich 1995; Cox and McCubbins 1994, 1993). Instead, party loyalty is seen as a symptom of polarization, unwillingness to compromise, position-taking in lieu of substantive lawmaking, and hardball procedural tactics (Jessee and Theriault 2014). There will be a positive effect of streaming/broadcasting on party loyalty if increased public scrutiny gives rise to more polarization, and a greater ability of party actors, interest groups, or extremists to monitor individual legislators.

Finally, legislative effectiveness is a widely used measure of the extent to which individual legislators engage in substantive representation. The measure captures the weighted average of the number of bills advancing to various stages of the legislative process, which accounts for the total size of the agenda within a chamber-year and the number of total legislators (Volden and Wiseman 2014). Bucchianeri, Volden and Wiseman (2024) demonstrate that at the state-level, individual effectiveness is a combination of personal traits and institutional position. Members of the majority, committee leaders, and more senior members are more effective, but certain legislators seem to have innate characteristics that make them more willing to engage in the difficult and time-consuming activities necessary to push their bills through the lawmaking process, consistent with long-held notions of "workhorses," and "showhorses" (Langbein and Sigelman 1989). We are interested in whether the addition of streamed/televised proceedings resulted in legislators substituting the hard work of legislating for grandstanding. If so, we expect a negative effect of the treatment on individual legislator effectiveness.

Estimation Strategy for Predicting Coverage Effects

Because we observe both state-chambers and districts over time, with treatment adoption occurring in staggered years, our primary estimation technique is a difference-in-difference estimation through a two-way fixed effects model:

$$\hat{y}_{it} = \alpha_{it} + \beta_1 D_{it} + \gamma_t + \sigma_i + \beta_j x_{it} + \varepsilon_{it} \tag{1}$$

where $\hat{y}_{it}$ is the outcome of interest, D_{it} is the treatment (adoption of streaming or broadcast coverage of state legislative floor sessions), γ_t is a set of dummy variables for years, σ_i is a set of dummy variables for state legislative chamber or state-chamber-district, depending on whether the outcome is at the state-level or the legislator-level, and ε_{it} are chamber- or district-year clustered standard errors. This model specification controls for factors that vary across years but not states, such as increasing polarization within the country, and controls for factors which vary across states but not time, such as the size of the legislature or other institutional rules. This model does not control for factors that vary within states across time, so we also include a set of covariates x_{it} to control for these factors.

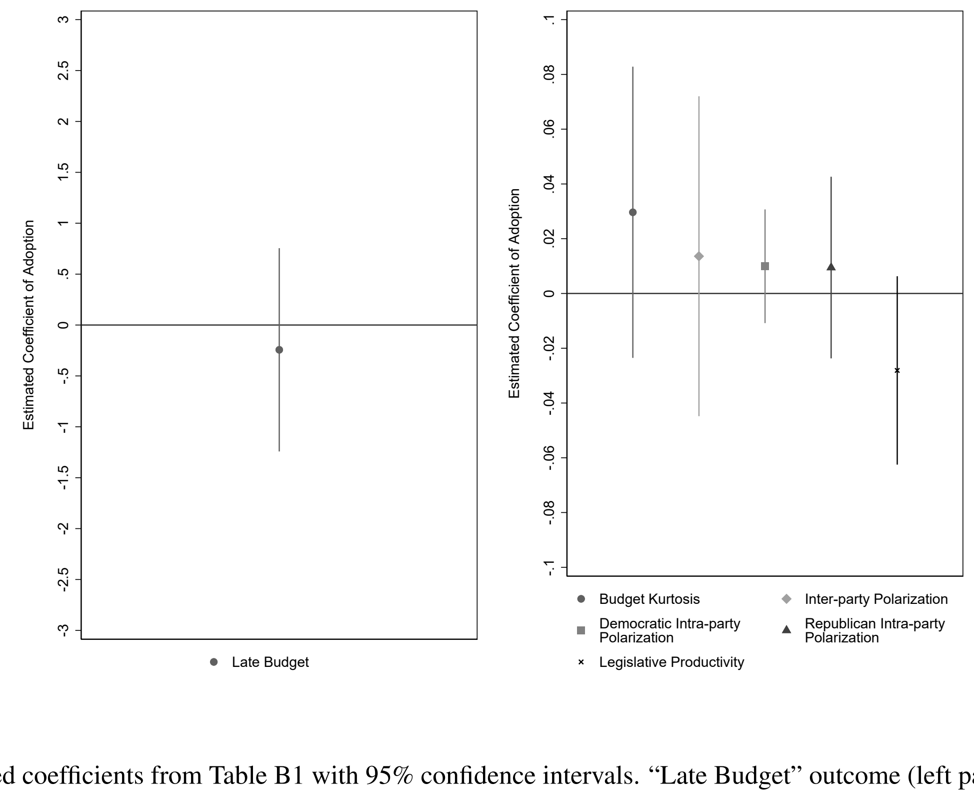
The covariates are the same as those in Harden and Kirkland (2021) and include the total number of bills and resolutions vetoed in a state-year, first and second dimension legislative professionalism in state-year, the Berry et al. (1998) measures of state citizen and government ideology, the Ranney political competition measure as compiled by Klarner (2013), the logged gross state product, logged legislative expenditures, an indicator for whether the state has term limits, and the logged state population. To these measures, we also add the logged number of bills introduced in the legislature as a measure of agenda size within each state-chamber (Volden and Wiseman 2009). For models estimating the effect on individual legislators, we also control for party identification and whether the legislator was in the majority party, as the majority is more likely to enforce party-line voting (Carson et al. 2010). Table A2 in the Appendix shows descriptive statistics for all variables used in the analyses.

Recent literature on difference-in-difference estimation shows that the treatment estimate may be biased if the effect varies across time or units, or uninterpretable in the absence of strong assumptions concerning different treatment effects across time or units. Difference-in-difference estimation also requires that the treated units would not have differed in the absence of the treatment (parallel trends assumption). We address each of these complications through a series of robustness tests and assumption checks after each set of analyses.

Estimated Effects on Chamber-Level Outcomes

Table B1 in the Appendix shows our preferred models predicting each chamber-level dependent variable using the treatment (adoption of gavel-to-gavel coverage via streaming or broadcast) alone and with a full set of controls, including state-chamber and year fixed effects with clustered standard errors for state-chamber and year.[16] The first model is a logit estimate predicting a late state budget, while the other five columns are OLS estimates. Figure 2 displays the estimated coefficients with 95% confidence intervals, with the left panel showing the logit model estimate, while the right panel shows regression coefficients, which can directly be interpreted.

Figure 2: Estimated Chamber-Level Coefficients From Diff.-in-Diff. Analysis



Note: Estimated coefficients from Table B1 with 95% confidence intervals. "Late Budget" outcome (left panel) is predicted using fixed effects logit, all other outcomes predicted using fixed effects regression. Scales differ between the two graphs.

[16]The logit models use bootstrapped standard errors rather than clustered standard errors.

Gavel-to-gavel coverage has no effect on the late adoption of state budgets. As the left panel shows, the estimate is negative, though statistically insignificant. The substantive effect is equal to a decrease in the odds ratio of a late budget by about 4% (95% CI: 78% decrease to 273% increase). In the right panel, none of the other five outcomes, budget kurtosis, the difference between the Democratic and Republican party medians, the standard deviation of both parties, and legislative productivity reach standard levels of statistical significance. Legislative productivity is the closest, with a negative coefficient and a p-value of .11 (95% CI: -.06 to .006), suggesting that the adoption of coverage reduced the total number of bills passed. The effect is equal to a reduction of about 2.8% in the proportion of total bills passed, a very small substantive effect. The standard deviation of legislative productivity is .18, thus the overall substantive effect is equal to .155 of a standard deviation. For context, the average number of bills passed in a state-chamber is about 804, and the point estimate suggests that adoption decreased enacted bills by approximately 22. Overall, we find no evidence that introducing gavel-to-gavel coverage of state legislative sessions had any effect on our six chamber-level variables at conventional standards of statistical significance; further, even if we accept the result for legislative productivity as statistically significant, the size of the effect is exceedingly small.

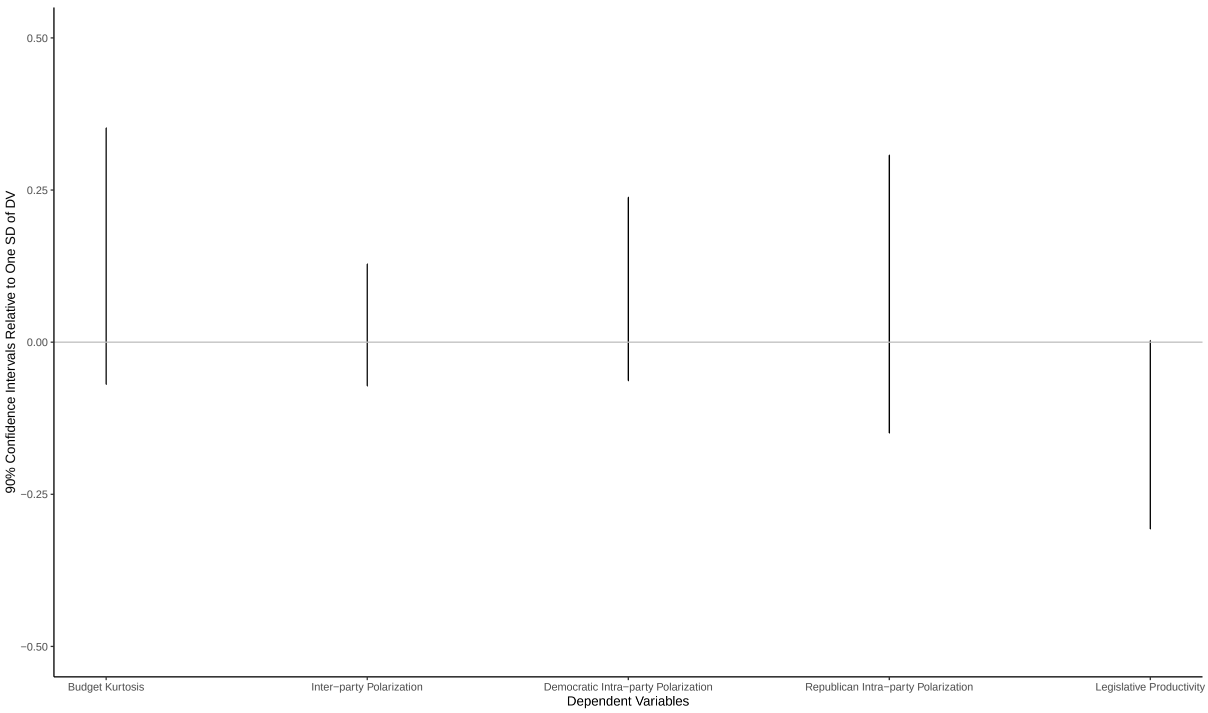
Precision of Null Effects

Because our estimates overwhelmingly point to insignificant effects for chamber-level outcomes, we wish to estimate the precision of these null results (Rainey 2014). To do so, we must identify a substantively informative effect for each of the outcomes, develop a hypothesis which specifies the null relationship between the treatment and the outcome, and then estimate 90% confidence intervals around these substantive effects. Our hypothesis for each outcome is that adoption by a state-chamber of gavel-to-gavel broadcast/streaming coverage will not lead to a substantively meaningful change in our six chamber-level outcomes. 90% confidence intervals are equivalent to conducting two, one-tailed tests for the null that the estimated effect is greater (or smaller) than the substantive effect of interest.

We first need to define substantive quantities of interest for each of our outcomes, motivated by subject knowledge and theoretical considerations. In the case of late budget outcome, the estimate is sufficiently imprecise that nearly any substantive value will lie within the 90% confidence interval. As a result, the results for late budget must be treated with caution as its effects are imprecisely estimated.

For all other outcomes, we define a substantive effect of the treatment as producing at least a one-half standard deviation change in the outcome. To display the results and allow for comparisons

Figure 3: Estimated Precision of Chamber-Level Null Effects



Note: Results from Table B1. 90% confidence intervals scaled to standard deviations of each dependent variable. "Late Budget" outcome excluded because confidence interval encompasses all plausible values.

between different effect sizes, we scale the 90% confidence intervals to standard deviations. Figure 3 displays the relative effects sizes compared to a one-half standard deviation change in the dependent variable. As the figure shows, all estimated effects lie outside of our substantive effect criteria. Importantly, this demonstrates that the null effects are precisely estimated (with the exception of late budget), are substantively small, and do not encompass meaningful changes in the outcomes. This analysis gives additional confidence to the claim that the actual treatment effect is zero and Type II errors are unlikely.

Parallel Trends Assumption

The key assumption of the difference-in-difference estimator is that states which were treated with adoption of coverage would have had the same trends, over time, in each outcome as states that were not treated. Alternatively, this assumption requires that non-treated state-chambers are comparable or effective controls for treated states. There is no direct way to test this assumption, but we conduct two standard empirical tests, both of which show no evidence that the parallel trends assumption is violated for our chamber-level outcomes (see Appendix C for more details).

First, we predict whether change in treatment status is predicted by one of the outcomes. For example, do differences in state budget kurtosis predict treatment adoption? If so, this would indicate that states which are treated in year t systematically differ from those without the treatment in any given year, implying that adoption was not exogenous to the outcomes. Our second test uses treatment in the current year to predict each of the outcomes in the previous year, a common way of testing for violations of the parallel trends assumption (Barber and Holbein 2020). These combined results give confidence that the parallel trends assumption is satisfied.

Chamber-Level Heterogeneous Treatment Effects and Lagged Treatment Effects

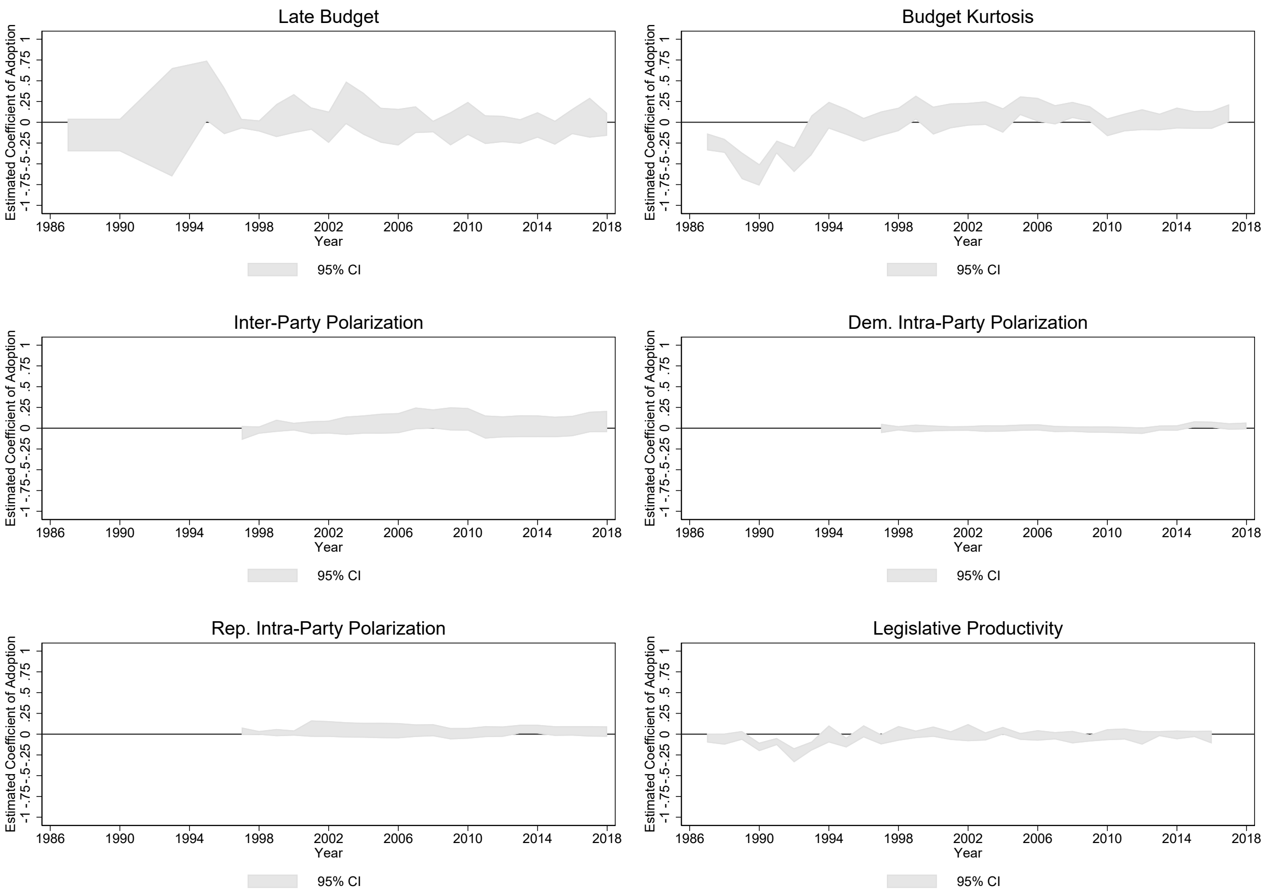
Recent literature emphasizes how difference-in-difference estimates weight observations differently when treatment assignment is staggered, rather than in the canonical difference-in-difference setup in which all units receive the treatment at the same time (Baker, Larcker and Wang 2022; Goodman-Bacon 2021; Imai and Kim 2021). This problem can bias the coefficients and, "staggered DiD treatment effect estimates can actually obtain the opposite sign of the true ATT. (Baker, Larcker and Wang 2022, 371)." This problem cannot be corrected by simply adding unit and time fixed effects, which can produce uninterpretable results (Kropko and Kubinec 2020).

It may be the case, for example, that adoption of gavel-to-gavel coverage in the 1990s had greater effects than adoption in the 2000s. Perhaps the novelty and limited channel selection drove greater viewership or greater engagement with legislative activity than in the 2000s after the rise of widespread broadband internet access. By examining heterogeneous treatment effects across time, we can discern whether different times of adoption affected the outcomes of interest.

Our first test uses Stata's difference-in-difference command which allows for separate estimation of treatment effects across different cohorts, allowing us to examine whether the treatment effect differs across time. The command estimates average treatment effects across different years and groups (state-chambers), then aggregates by year to determine whether the treatment effect differs for earlier or later treated states.[17] If it does differ, it is evidence that the average treatment effect depends on when states were treated with streaming or broadcast coverage, and suggests we need to take additional steps to account for these differing effects. The results are displayed in Figure 4 and shown in Table D1. Across all outcomes and years, there are virtually no statistically significant treatment effects, nor are there any obvious trends in treatment effects.

[17]We use the regression adjustment technique, though results are substantively similar to those calculated using the two-way fixed effects technique, available in replication file. Control variables are excluded.

Figure 4: Estimated Chamber-Level Treatment Effects Over Time



Note: Results from Table D1. Estimated average treatment effects aggregated by time period, with 95% confidence intervals.

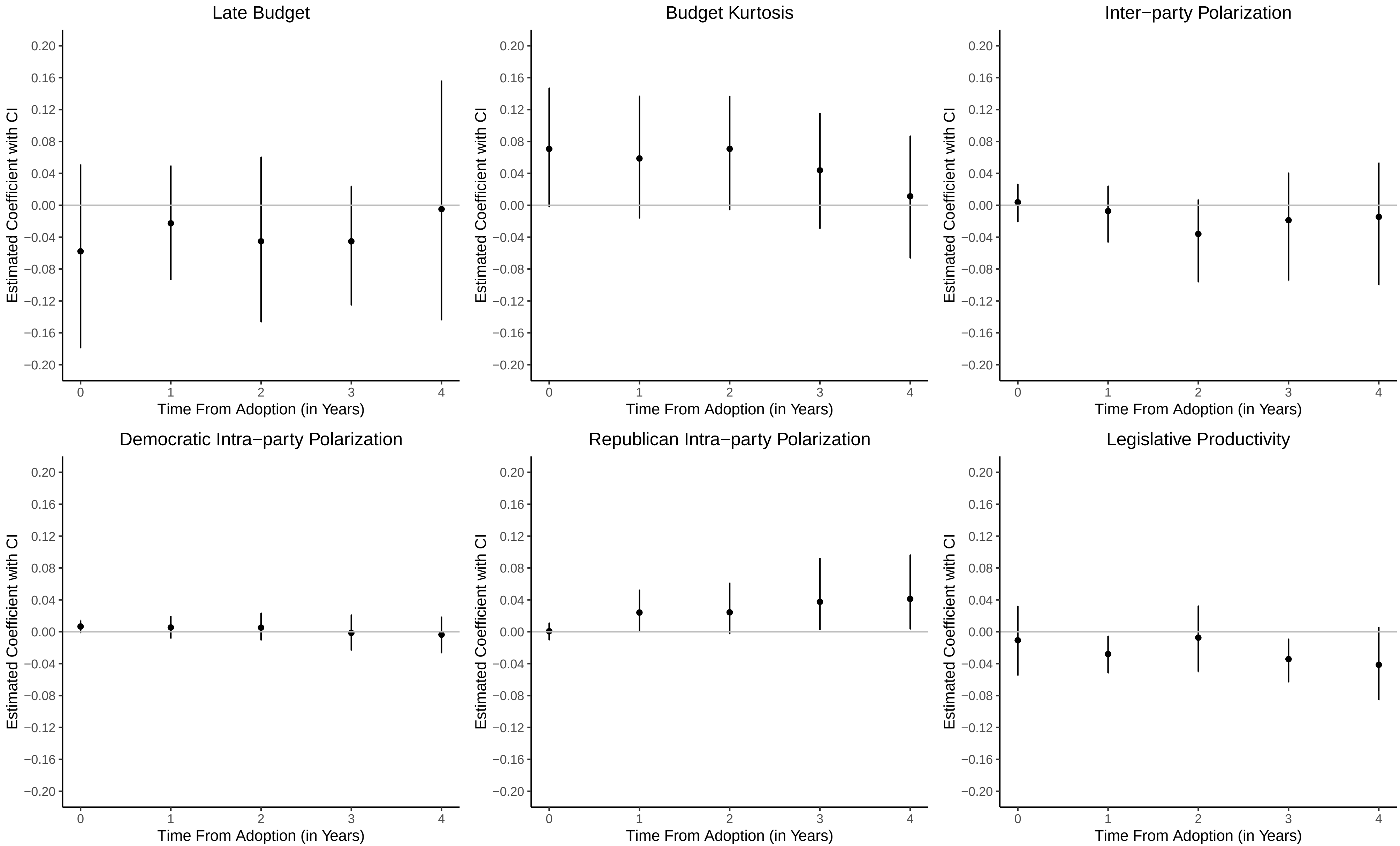
We also use an implementation of the Goodman-Bacon estimator (Goodman-Bacon 2021) to determine the extent to which the treatment effects differ across different treatment cohorts, and the extent to which each different cohort is weighted when calculating the overall average treatment effect. Additional discussion and results are shown in Appendix D. There is very little evidence effects differ across cohorts and we conclude that heterogeneous treatment effects are not a concern for our analysis.

Chamber-Level Lagged Effects

Finally, we seek to determine whether the effects of broadcast or streaming adoption on our chamber-level outcomes are lagged. Perhaps live coverage of floor activity produces more polarization, but only after a few years as legislators adjust their behavior. Examining lagged effects is also important in understanding whether legislators become responsive after a particularly salient agenda item.

We use the PanelMatch estimator developed by (Imai, Kim and Wang 2023) which matches each treated observation with a control observation using observables, then estimates both short- and long-term treatment effects using difference-in-difference estimates averaged across all treated observations. We estimate possible current and future effects up to four years, for a total of five years per state-chamber. See Table E1 for the results and Appendix E for additional details on the PanelMatch process including covariate balance tests and the number of matched control units for each outcome.

Figure 5: Estimated Chamber-Level Treatment Effects From PanelMatch



Note: Estimated average treatment effects from Table E1 with 95% confidence intervals.

Figure 5 shows the estimated point estimate and 95% confidence interval of broadcast or streaming adoption on late budget for the adoption year (time zero) and subsequent four years for each of the outcomes. Across 30 individual estimates, we find five statistically significant results. The average effect for Republican intra-party polarization is positive and significant in the year after adoption, and in years three and four. The point estimate in year one is 0.02 (95% CI: .013 to .05), equal to .2 of a standard deviation, similar to the substantive effect found in the two-way fixed effects difference-in-difference models. The point estimates are larger at three and four years, equal to .31 and .34 of a standard deviation. However, this effect is substantively small, and disappears when we extend the time series past four years.

The only other statistically significant effect is found for legislative productivity, at years one and three. The substantive effect at year one is -.028 (95% CI: -0.05 to -0.006), or about .08 of a standard deviation, and at year three it is .1 of a standard deviation, both very small substantive effects. In the subsequent section, we examine legislator-level outcomes, including Republican ideology and legislative effectiveness, or the ability of individual state lawmakers to advance their bills through the legislative process. If floor coverage affects Republican ideology or reduces effectiveness, it would offer additional verification of the effects shown here.

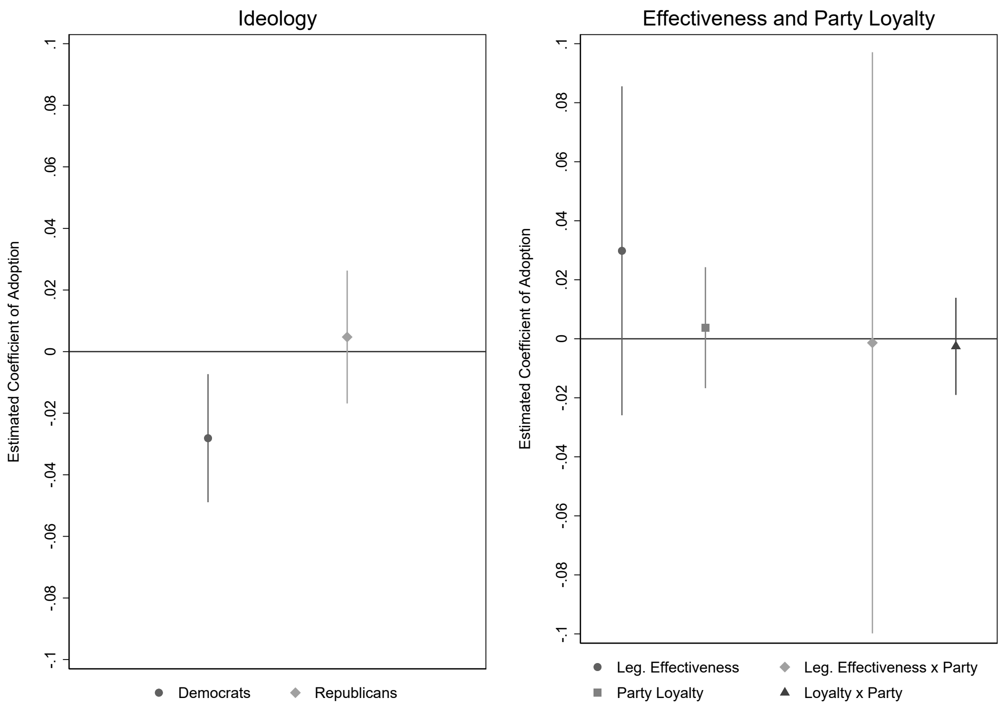
Overall, the chamber-level results offer almost no support for the claim that the adoption of broadcast or streaming coverage of state legislative floors affected any of the six outcomes we have examined. Even in those cases in which a significant or nearly significant result is found, the effect sizes are minimal. These results lead us to conclude that at the chamber-level, the effects of broadcasting or streaming floor proceedings is virtually non-existent, and is certainly not to blame for high levels of polarization or dysfunction in state legislatures.

Estimated Effects on Legislator Outcomes

We now turn toward estimating effects on individual legislator outcomes: ideology (as measured by their NPAT score), party loyalty (as measured by the percentage of legislator-year votes in agreement with their party, only on party votes), and legislative effectiveness (as measured by their yearly state legislative effectiveness score). As with chamber-level outcomes, we take the year of adoption of gavel-to-gavel coverage as the treatment, and compare differences across legislators and across time.

Our analyses use the same model specifications as for the chamber-level models, with additional legislator-level controls for majority party status and party identification.[18] We use fixed effects and clustered standard errors for year and district. Because we expect ideology scores to become more extreme due to floor coverage, the NPAT scores moves in different directions for each party. That is, if adoption increases extremity, Republicans should have increasingly high scores and Democrats should have decreasingly low scores. Thus, when estimating ideology we split the sample by party.[19] For the other two outcomes, we estimate one model with the treatment variable, and an additional model with treatment interacted with party to examine different possible effects across the two parties. Figure 6 shows the two-way fixed effects difference-in-difference point estimates and associated confidence intervals, while Table F1 in the Appendix shows the table of regression results.

Figure 6: Estimated Legislator-Level Coefficients From Diff.-in-Diff. Analysis



Note: Estimated coefficients from Table F1 with 95% confidence intervals. Left panel shows coefficient of treatment separated by party; right panels shows treatment coefficients for both non-conditional effects when interacted with party. All models predicted using fixed effects regression.

We find a statistically significant and negative effect for Democratic ideology (left panel), indicating that Democrats became .028 points more ideologically extreme after the adoption of floor coverage

[18] There are 28 legislator observations that belong to a third party and are dropped from the analyses.

[19] A common approach is to "fold" these scores by taking the absolute value such that zero is moderation and increasing values for both parties indicates greater extremity. This is not an appropriate approach however, because zero has no intrinsic meaning and either interacting the scores with party or estimating them separately is preferred. We do not interact by party because all variables in the model are conditional on party.

(95% CI: -.05 to -.01). The estimated effect is quite small, however, equal to about .05 of a standard deviation.[20] We find no similar effect for Republicans. The right panel shows no effects for legislative effectiveness or party loyalty, either as unconditional effects or interacted with party.

We also estimate regressions examining whether these effects are conditional on majority party status (Appendix Table F2). Perhaps it is the case that gavel-to-gavel coverage makes the majority more willing to enforce party loyalty, or allows the majority to exercise greater control over its members through better vote monitoring. In the first column, the Democratic Party component term is negative and statistically significant, indicating that adoption of floor coverage makes Democrats more extreme when they are in the minority. Predicted probabilities also demonstrate that minority Democrats in states with floor coverage are distinct from minority Democrats in states without coverage. The effect is about .07 of a standard deviation (95% CI: .12 SDs to .02 SDs). None of the other floor coverage component terms or interaction terms are statistically significant in any of these models and there is no similar effect for Democrats in the majority, nor for Republicans under any condition.

Precision of Legislator-Level Null Effects

As we did with the chamber-level results, we explore how precise our null estimates are in Figure 7. We also display the significant result for Democratic Party ideology to contextualize its size. As with the chamber-level estimates, the substantive effects are exceedingly small—less than .5 of a standard deviation—and precisely estimated, giving us confidence that the true effect is not statistically different from zero (with the exception of the Democratic ideology score).

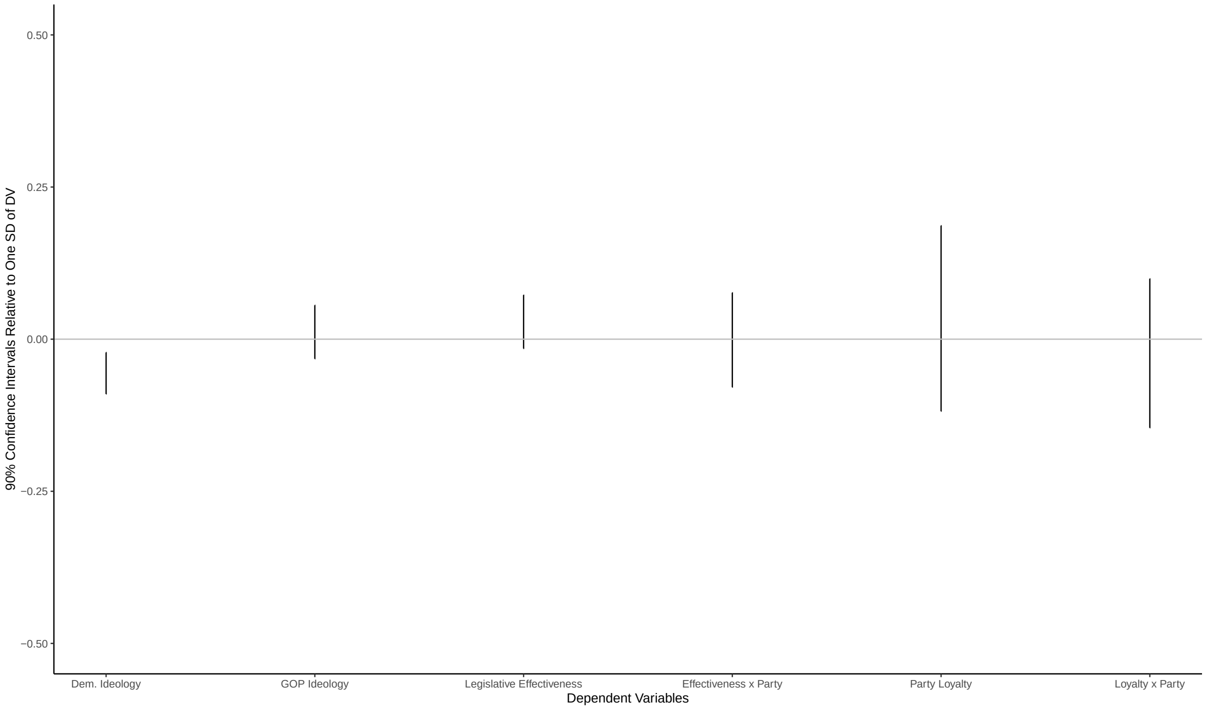
Parallel Trends Assumption for Legislators

We conduct two tests at the legislator-level to examine the parallel trends assumption (see Appendix G). The first predicts change in treatment status using legislator ideology, legislative effectiveness and party loyalty, and the second predicts lagged outcomes using floor coverage adoption.

In both cases, there is some weak evidence that party loyalty predicts adoption. Specifically, greater party loyalty predicts change in treatment status in both the bivariate model and with controls. The component term is also significant when included in an interaction with party, but not when additional controls are included. Lagged treatment status also predicts party loyalty when controls are included, but not in the bivariate model.

[20]For all states across all Democratic members, the average NPAT score decreased from -.57 in 1996 to -.953 in 2018

Figure 7: Estimated Precision of Legislator-Level Null Effects



Note: Results from Table F1. 90% confidence intervals scaled to standard deviations of each dependent variable. "Late Budget" outcome excluded because confidence interval encompasses all plausible values.

These mixed results suggest that legislators with greater party loyalty are more likely to be exposed to the treatment, perhaps because state-chambers with stronger parties are more likely to adopt these rules as a voting enforcement or monitoring mechanism. We interpret these results with caution, however, because there are no significant effects for party loyalty or related variables at the chamber-level (e.g., intra-party homogeneity). It is possible that adoption caused a reduction in these factors, but this is contrary to theoretical expectations; party leaders who adopt coverage presumably do so in order to strengthen their hand with legislators, not reduce it. Further, at the chamber-level, the parallel trends tests show no evidence that state-chambers which adopted gavel-to-gavel coverage are meaningful different than those that did not. Besides party loyalty, other variables predict change in treatment status, but the results are not consistent across models nor when predicting lagged treatment.

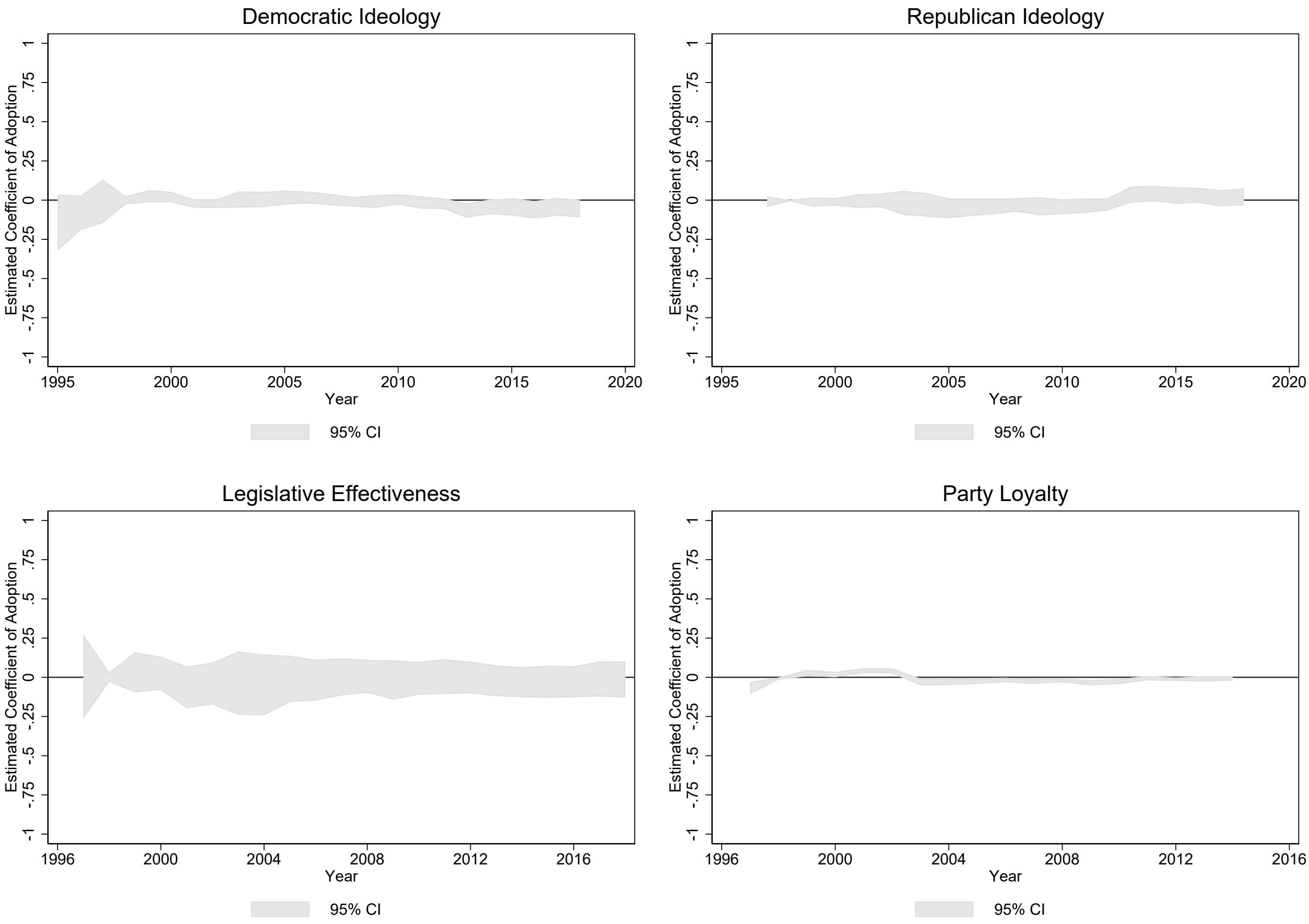
Legislator-Level Heterogeneous Treatment Effects and Lagged Effects

As with the chamber-level data, we also test for heterogeneous treatment effects based on time of adoption for individual legislators. We do so through a cohort analysis to determine whether the treatment effect differs by year of adoption of coverage (see Appendix H for details on the Bacon-

Goodman estimator).

Figure 8 shows the estimates using average treatment effects across different years and groups (districts), then aggregated by year to determine whether the treatment effect differs for earlier or later treated districts (results shown in Table H1).[21]

Figure 8: Estimated Legislator-Level Treatment Effects Over Time



Note: Results from Table H1. Estimated average treatment effects aggregated by time period, with 95% confidence intervals. Calculated using regression adjustment technique in Stata.

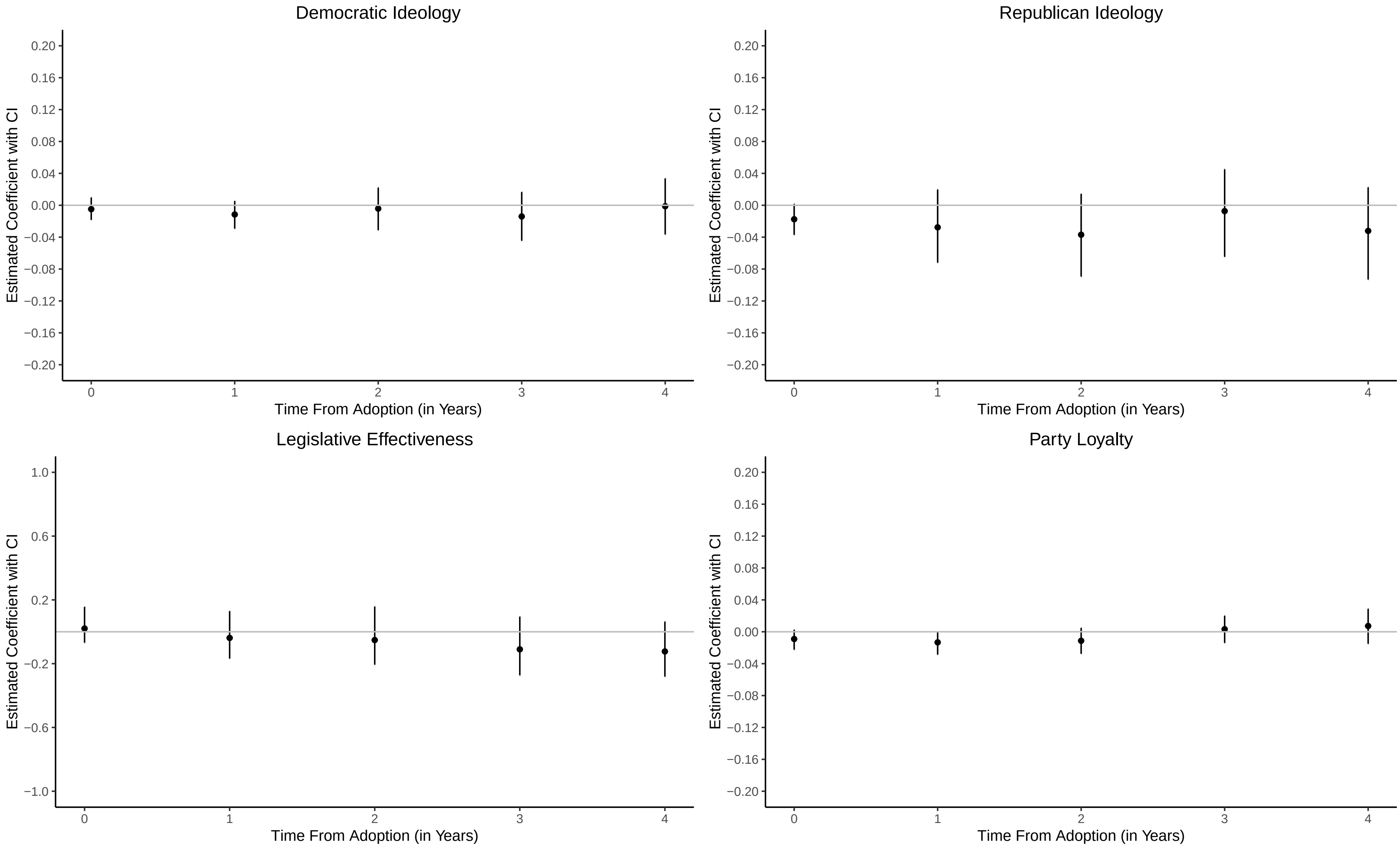
There are no consistent results across cohorts for each of the four outcomes. Party loyalty has a statistically significant and positive result for the years 1999 through 2002, but a negative and statistically significant result for the years 1997, 1998, and 2003 through 2010, and in 2012. As discussed previously, this negative result is inconsistent with theoretical expectations, and inconsistent with the occasional positive result. Thus, we conclude that there is little systematic effect of adoption on party loyalty.

[21]We also use the two-way fixed effects technique and find substantively similar results, though there is no statistical significance for party loyalty.

Legislator-Level Lagged Effects

Our last analysis examines lagged or delayed effects on individual legislator behavior using PanelMatch. The unit is the district so the analysis matches on treatment history and the other covariates specified, then finds treatment effects for the year of adoption of coverage, plus four years into the future (Table I1). Unlike the chamber-level results which all used propensity score matching, the balancing technique differs for each outcome because the samples are slightly different (see Appendix I).

Figure 9: Estimated Chamber-Level Treatment Effects From PanelMatch



Note: Estimated average treatment effects from Table I1 with 95% confidence intervals. Matching method used is Mahalanobis distance for Democratic ideology, covariate balance propensity score matching for Republican ideology, and propensity score matching for both legislative effectiveness and party loyalty.

Figure 9 shows the results for each of the four outcomes, with ideology separated by party. Consistent with the other legislator-level results, there are no statistically significant results across any of the four variables for each of the five years estimated. These results are also largely consistent with the estimates from the difference-in-difference models with respect to significance and substantive effect size. In particular, there was some evidence from those models that adoption of gavel-to-gavel coverage had a negative effect (i.e., more ideologically extremity) on Democratic ideology. We do not find evidence in this analysis for that finding. There is a negative and statistically significant effect of adoption on party loyalty in year two, but not in any other year.

Discussion

Based on the totality of results, we conclude there is virtually no evidence that the adoption of broadcast coverage or streaming has any substantive, sustained effect on our outcomes of interest. While we find substantively small and significant effects occasionally, these findings mostly appear to be idiosyncratic to a model specific or estimation technique, and there is little systematic evidence that any of our outcomes of interest are dramatically affected the treatment. We certainly do not find evidence that live coverage of state legislatures is to blame for the rapid rise in polarization or dysfunction in the chambers.

Concerns about the effect of televising or streaming government proceedings are more pronounced recently. The COVID-19 pandemic and the increasing ease with which government entities may stream their deliberations on the internet via platforms like Youtube.com encourages gavel-to-gavel coverage. As a result, recent years have seen a dramatic increase in the prevalence of cameras in deliberative bodies. School districts, city councils and bureaucratic entities now provide streaming access to their meetings. All of these government entities feature government officials, either elected or appointed, making public policy decisions. And, all of these entities make policy decisions that are mostly obscure and unknown to voters. If officials' behavior changes *because* of their awareness that their conversations, deliberations, and voting decisions are more easily accessible to the public, then it is important to understand the conditions and size of those effects. Similarly, if there is no negative effect of the adoption of public broadcast/streaming on government officials' behavior, then increased access to meetings, hearings, and legislative deliberations may produce only positive benefits for voters in the form of greater adherence to constituent preferences (though we do not find evidence for that, either).

There are limitations to this study, and to the extent to which we can generalize these findings. We have focused on gavel-to-gavel coverage of legislative proceedings as the intervention most likely to alter outcomes and behaviors, but cameras were present in some legislatures on an ad hoc basis prior to gavel-to-gavel coverage. This ad hoc camera usage was the focus of the only other research we are aware of that explores these effects in state legislatures (Crain and Goff 1986). It is possible that this early ad hoc usage is what altered behaviors and outcomes such that our measures of gavel-to-gavel adoption has missed the shift that occurred. While we cannot dismiss this possibility, comparative research on the Turkish parliament suggests that part-time camera usage (in that case, on certain days of the week) simply changes when legislators use one type of behavior over another (Yildirim 2020). With that in mind, we would expect temporary camera interventions to result in temporary behavioral modifications, but should not have the enduring effects we examine here.

We also cannot discount the possibility that behavior remains unchanged, while rhetoric, tone, and the tenor of deliberations grew more contentious because of cameras. Perhaps legislators speak differently than they did before cameras, even if their voting behavior has not changed. While we cannot capture tone using our data, and acknowledge that how a message is conveyed is important, measuring actual deliberative outcomes is the appropriate first step in understanding the effect of increased transparency through gavel-to-gavel coverage.

When it comes to broadcasting state legislative proceedings, the commonly repeated phrase, "Laws are like sausages, it's better not to see them being made" does not appear to apply. By looking at the dynamic adoption of cameras in state legislative chambers, we are able to leverage a much more powerful research design than existing empirical examinations that look only at national legislatures such as the U.S. Congress, where intervention occurs only once. Demonstrating that cameras do not have deleterious effects as many have worried suggests that a trade-off may not exist between transparency and effective governance. While we cannot be certain that this finding extends to other levels of government or types of public proceedings, they cast doubt on the claims of those who wish to restrict coverage of governing institutions, a position Supreme Court Justice Sotomayor endorsed on *The Daily Show*.

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Appendix For: The Polarization Will Not Be Televised: The Effects of Gavel-to-Gavel Floor Coverage on U.S. State Legislatures

Contents

Appendix A Adoption of Floor Coverage in State-Chambers and Descriptive Statistics

A.1 Details of State Floor Coverage Data

Table A1: Details of State Floor Coverage Data

State-Chamber	Coverage Type	Coverage Began	Source
Alabama House	Broadcast	2001	State and Local Government Records Archivist, Alabama Department of Archives and History
Alabama Senate	missing		Unable to contact
Alaska House	Broadcast	1996	”About Gavel Alaska”, Gavel Alaska KTOO
Alaska Senate	Broadcast	1996	”About Gavel Alaska”, Gavel Alaska KTOO
Arkansas House	Streaming	2010	House Chief Information Director
Arkansas Senate	Streaming	2019	Senate Information Officer
Arizona House	Broadcast	2000	Assistant Administrator for Broadcast Department
Arizona Senate	Broadcast	2000	Assistant Administrator for Broadcast Department
California House	Broadcast	1993	Television Specialist, Senate Television
California Senate	Broadcast	1993	Television Specialist, Senate Television
Connecticut House	missing		Contact made—could not answer
Connecticut Senate	missing		Contact made—could not answer
Colorado House	Streaming	2012	Online Legislative Archives
Colorado Senate	Streaming	2012	Online Legislative Archives
Delaware House	Streaming	2020	Legislative Librarian
Delaware Senate	Streaming	2020	Legislative Librarian
Florida House	Broadcast	1996	TheFloridaChannel.org, “Who We Are”
Florida Senate	Broadcast	1996	TheFloridaChannel.org, “Who We Are”
Georgia House	Broadcast	1997	Technology Coordinator (House)
Georgia Senate	Broadcast	1997	Technology Coordinator (House)
Hawaii House	Streaming	2020	Administrative Services Manager, House Chief Clerk’s Office
Hawaii Senate	Streaming	2020	Administrative Services Manager, House Chief Clerk’s Office
Iowa House	Broadcast	2012	Computer Services Director
Iowa Senate	Broadcast	2013	Computer Services Director
Idaho House	Broadcast	1999	Idaho Public TV Director
Idaho Senate	Broadcast	1999	Idaho Public TV Director
Illinois House	Streaming	1998	Information Services Specialist
Illinois Senate	Streaming	1998	Information Services Specialist
Indiana House	missing		Contact made—could not answer
Indiana Senate	missing		Contact made—could not answer
Kansas House	Streaming	2020	Broadcast Director
Kansas Senate	Streaming	2020	Broadcast Director
Kentucky House	Broadcast	1995	Video Communications Specialist
Kentucky Senate	Broadcast	1995	Video Communications Specialist
Louisiana House	Streaming	1999	Law Librarian
Louisiana Senate	Streaming	2002	Law Librarian
Maryland House	Streaming	2021	Legislative Librarian
Maryland Senate	Streaming	2021	Legislative Librarian
Maine House	Streaming	2006	Reference Librarian
Maine Senate	Streaming	2007	Reference Librarian
Massachusetts House	Streaming	2001	Senate Clerk’s Office
Massachusetts Senate	Streaming	2001	Senate Clerk’s Office
Michigan House	Broadcast	1996	Director of House TV
Michigan Senate	Broadcast	1996	Director of House TV
Minnesota House	Broadcast	1998	Minnesota Legislative Reference Library
Minnesota Senate	Broadcast	2001	Minnesota Legislative Reference Library

Table A1 (continued): Details of State Floor Coverage Data

State-Chamber	Coverage Type	Coverage Began	Source Title
Missouri House	Broadcast	2016	Public Information Specialist, Legislative Library Director
Missouri Senate	NA	Never Adopted	Public Information Specialist, Legislative Library Director
Mississippi House	Streaming	2009	IT Support
Mississippi Senate	Streaming	2009	IT Support
Montana House	Broadcast	2001	Montana Legislature, "Introducing the Montana Public Affairs Network (MPAN)"
Montana Senate	Broadcast	2001	Montana Legislature, "Introducing the Montana Public Affairs Network (MPAN)"
New Jersey House	Broadcast	2000	Geneology Librarian, State Library
New Jersey Senate	Broadcst	2000	Geneology Librarian, State Library
North Carolina House	Streaming	2022	Legislative Library Director
North Carolina Senate	NA	Never Adopted	Legislative Library Director
North Dakota House	Streaming	2013	Legislative Analyst
North Dakota Senate	Streaming	2013	Legislative Analyst
New Hampshire House	Streaming	2000	House Clerk
New Hampshire Senate	Streaming	2020	House Clerk
New Mexico House	Streaming	2013	Assistant (House Clerk Office)
New Mexico Senate	Streaming	2017	Senior Legislative Librarian (Senate)
Nevada House	Streaming	2013	Broadcast Services Unit Manager
Nevada Senate	Streaming	2013	Broadcast Services Unit Manager
New York House	Both	2005	Public Information Officer, General Assembly
New York Senate	Both	2005	Public Information Officer, General Assembly
Ohio House	Streaming	1998	Producer, Ohio Channel
Ohio Senate	Streaming	1998	Producer, Ohio Channel
Oklahoma House	Streaming	2010	IT Director
Oklahoma Senate	Streaming	2004	Communications Director
Oregon House	Streaming	2015	Media Department Director
Oregon Senate	Streaming	2015	Media Department Director
Pennsylvania House	Broadcast	1993	PCNTV, "History"
Pennsylvania Senate	Broadcast	1993	PCNTV, "History"
Rhode Island House	Broadcast	1987	Director of Capitol TV
Rhode Island Senate	Broadcast	1987	Director of Capitol TV
South Carolina House	missing		Unable to contact
South Carolina Senate	missing		Unable to contact
South Dakota House	missing		No response
South Dakota Senate	missing		No response
Tennessee House	Streaming	2008	Tennessee Secretary of State, Legislative History & Recording
Tennessee Senate	Streaming	2008	Tennessee Secretary of State, Legislative History & Recording
Texas House	Streaming	2001	House Video/Audio Services Director
Texas Senate	Streaming	1999	House Video/Audio Services Director

Table A1 (continued): Details of State Floor Coverage Data

State-Chamber	Coverage Type	Coverage Began	Source Title
Utah House	Broadcast	2008	Legislative Histories, House Floor Debate
Utah Senate	Broadcast	2008	Legislative Histories, Senate Floor Debate
Virginia House	Streaming	2011	Deputy Clerk of the House
Virginia Senate	Streaming	2012	Senior Systems Analyst
Vermont House	Streaming	2020	House Clerk
Vermont Senate	Streaming	2020	House Clerk
Washington House	Broadcast	1995	Vice President of Programming
Washington Senate	Broadcast	1995	Vice President of Programming
Wisconsin House	Broadcast	2007	Operations Manager and Senior Technical Director (WisconsinEye)
Wisconsin Senate	Broadcast	2007	Operations Manager and Senior Technical Director (WisconsinEye)
West Virginia House	Streaming	2015	House Clerk
West Virginia Senate	Streaming	2015	Senate Clerk, Executive Assistant
Wyoming House	Streaming	2020	Senior Research Analyst
Wyoming Senate	Streaming	2020	Senior Research Analyst

Note: "Broadcast" indicates first gavel-to-gavel coverage available via television broadcast, "Streaming," indicates first gavel-to-gavel coverage available via internet stream or feed. NA indicates chamber never adopted broadcast or streaming. "Unable to contact" indicates we were unable to contact state legislative offices, "contact made-could not answer," indicates no one in the state legislature knew when coverage began, and "no response," indicates repeated phone calls and emails were unanswered. Due to Nebraska's unicameral and non-partisan legislature, it is excluded from our data collection.

A.2 Descriptive Statistics of Variables Used in Models

Table A2: Descriptive Statistics of Variables Used in Models

Variable	Mean	Std. Dev.	Max	Min
Outcomes				
Late Budget	0.142	0.349	0	1
Budget Kurtosis	0.434	0.210	-0.113	1.0
Abs. Difference in Party Medians	1.47	0.48	0.09	3.04
Democratic Ideological Std. Dev.	0.304	0.114	0.071	0.861
Republican Ideological Std. Dev.	0.267	0.120	0.015	1.34
Proportion of Bills Enacted	0.325	0.185	0.011	0.985
Democratic Ideology Score	-0.783	0.502	-2.88	2.975
Republican Ideology Score	0.780	.404	-1.42	3.09
Legislative Effectiveness	1.02	1.05	0	37.74
Party Loyalty	0.871	0.111	0.06	1
Controls				
Total Bills Vetoed	25.69	50.83	1	465
Leg. Prof. 1st Dimension Scaling	0.046	1.53	-1.88	8.58
Leg. Prof. 2nd Dimension Scaling	0.032	0.760	-3.27	3.17
Berry State Govt. Ideology	49.48	13.49	17.51	73.62
Berry State Citizen Ideology	49.72	15.35	8.45	95.97
Ranney/Klarner Competitiveness Measure	0.867	0.102	0.566	1.0
Logged Gross State Product	11.35	1.15	8.49	14.46
Term Limits	0.141	0.348	0	1
Logged Legislative Expenditures	16.86	1.02	14.16	19.66
Logged State Population	15.03	1.02	12.91	17.45
Logged Bills Introduced	7.45	0.840	3.93	10.14
Party ID (1=Republican)	0.516	0.50	0	1
Majority Party (1=Majority)	0.614	.487	0	1

Late budget, term limits, party identification, and majority party are dichotomous variables, thus the mean shows the proportion of observations with a late budget term limits, Republican party identification, or majority party affiliation. All chamber-level variables measured within state-chamber-year; all legislator-level variables measured within district-state-chamber-year.

Appendix B State-Chamber Level Empirical Models and Robustness Checks

B.1 The Effect of Broadcast/Streamed Floor Coverage on State Legislative Outcomes

Table B1: The Effect of Broadcast/Streamed Floor Coverage on State Legislative Outcomes

	Late Budget		Budget Kurtosis		Inter-Party Polar.		Dem. Intra-party Polar.		GOP Intra-Party Polar.		Leg. Productivity	
Adoption of Floor Coverage	-0.18	-0.24	0.02	0.03	0.04	0.01	0.01	0.01	0.01	0.01	-0.02	-0.03
	(0.33)	(0.51)	(0.02)	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Total No. of Bills/Resolutions Vetoed in State-Year		0.07		<0.01		-0.01		>-0.01		<0.01		0.05*
		(0.35)		(0.01)		(0.01)		(<0.01)		(0.01)		(0.01)
Leg. Prof. 1st Dimensional Scaling		1.17*		-0.01		0.01		0.03		<0.01		-0.04*
		(0.45)		(0.02)		(0.04)		(0.02)		(0.03)		(0.01)
Leg. Prof. 2nd Dimensional Scaling		0.77		>-0.01		<0.01		0.02		0.01		-0.02
		(0.49)		(0.02)		(0.03)		(0.02)		(0.03)		(0.01)
State Ideology		-4.49*		0.05		-0.13		-0.03		0.07		>-0.01
		(1.80)		(0.09)		(0.08)		(0.04)		(0.05)		(0.04)
Political Competition		1.96		0.07		0.24$^{\#}$		0.03		-0.05		-0.09
		(2.20)		(0.07)		(0.13)		(0.05)		(0.10)		(0.06)
Gross State Product (Logged)		3.26		0.05		-0.18		-0.02		0.02		0.03
		(3.07)		(0.09)		(0.17)		(0.06)		(0.06)		(0.06)
State Term Limits (1=Yes)		-1.35		>-0.01		0.07		-0.03		-0.03		>-0.01
		(0.86)		(0.04)		(0.04)		(0.02)		(0.02)		(0.02)
Logged Legislative Expenditures		-1.64		0.07		0.02		-0.07$^{\#}$		-0.02		0.03
		(1.34)		(0.05)		(0.07)		(0.03)		(0.07)		(0.03)
Citizen Ideology		-1.63		0.04		-0.04		0.02		0.08		0.05
		(2.72)		(0.10)		(0.12)		(0.07)		(0.06)		(0.06)
State Population (Logged)		-1.77		0.03		1.36*		-0.19		0.03		0.01
		(5.37)		(0.13)		(0.43)		(0.18)		(0.14)		(0.09)
Logged No. of Bills Introduced		1.34*		0.02		>-0.01		0.01$^{\#}$		<0.01		-0.08*
		(0.37)		(0.02)		(0.01)		(0.01)		(0.01)		(0.02)
Constant			0.43*	-1.97	1.47*	-17.64*	0.30*	4.51$^{\#}$	0.27*	-0.15	0.33*	0.06
			(0.01)	(1.60)	(0.01)	(6.15)	(0.01)	(2.44)	(0.01)	(2.41)	(<0.01)	(1.20)
AIC	1114.10	558.52	-1320.89	-778.49	-2142.18	-1643.21	-5844.51	-3146.77	-5134.95	-2481.87	-4812.74	-3725.39
R-Squared			0.10	0.14	0.92	0.96	0.74	0.82	0.67	0.75	0.66	0.71
N	1470	816	3382	2054	2036	959	2036	959	2031	958	2962	2048

Note: $^{\#}$p<0.1; *p<0.05. Late budget models are logit with random effects for state-chamber nested within years with robust standard errors. All other results columns are fixed effects OLS with multiway standard errors clustered by state-chamber and year. Vetoes, state ideology, and citizen ideology are scaled down by 100. Number of observations changes based on differences in sample years across variables.

Appendix C Empirical Tests of Parallel Trends Assumption for State-Chamber Level Analysis

The first set of models use change in treatment status as the dependent variable and each variable previously used as an outcome as the independent variable. The model is identified by the year in which each state legislature moved from not having broadcast/streaming coverage to the year it began. Separate linear probability models are estimated for the bivariate case and with the full set of controls (Appendix Table C1). As the table shows, there is no evidence adoption was driven by differences in states. All the outcome variables used to predict change in treatment are statistically insignificant, though the Democratic Party's internal polarization, measured by Shor-McCarty NPAT score standard deviation of party members, is significant at the .1 level. However, the direction of the effect is opposite that would be expected, meaning an increase in Democratic Party ideological heterogeneity increases the chances of adoption, a theoretically ambiguous result. Further, after controlling for other relevant variables, this predictor is no longer significant at the .1 level.

C.1 Predicting Change in Treatment Status Using Outcomes

Table C1: Predicting Change in Treatment Status Using Outcomes

	Change in State-Chamber Treatment Status											
Late Budget	-0.01	>-0.01										
	(0.01)	(0.02)										
Budget Kurtosis			0.02	0.02								
			(0.01)	(0.01)								
Inter-party Polarization					-0.01	0.01						
					(0.03)	(0.05)						
Democratic Intra-party Polarization							0.16$^{\#}$	0.19				
							(0.09)	(0.12)				
Republican Intra-party Polarization									-0.06	-0.05		
									(0.04)	(0.04)		
Legislative Productivity											0.01	-0.01
											(0.02)	(0.03)
Total No. of Bills/Resolutions Vetoed in State-Year		-0.01$^{\#}$		-0.01$^{\#}$		¿-0.01		>-0.01		¿-0.01		-0.01$^{\#}$
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)		(<0.01)
Leg. Prof. 1st Dimensional Scaling		-0.01		-0.01*		¡0.01		>-0.01		<0.01		-0.01*
		(0.01)		(0.01)		(0.02)		(0.02)		(0.02)		(0.01)
Leg. Prof. 2nd Dimensional Scaling		-0.03*		-0.02*		-0.01		-0.02		-0.01		-0.02*
		(0.01)		(0.01)		(0.02)		(0.02)		(0.02)		(0.01)
State Ideology		-0.01		0.01		-0.01		>-0.01		>-0.01		0.01
		(0.04)		(0.04)		(0.08)		(0.08)		(0.08)		(0.04)
Political Competition		0.05		0.04		-0.02		-0.03		-0.03		0.04
		(0.05)		(0.04)		(0.10)		(0.10)		(0.10)		(0.04)
Gross State Product (Logged)		0.03		0.02		0.14		0.15		0.14		0.02
		(0.03)		(0.02)		(0.15)		(0.14)		(0.15)		(0.02)
State Term Limits (1=Yes)		0.03		0.03$^{\#}$		0.10*		0.11*		0.10*		0.03$^{\#}$
		(0.02)		(0.02)		(0.03)		(0.03)		(0.03)		(0.02)
Logged Legislative Expenditures		-0.01		>-0.01		0.04		0.05		0.04		>-0.01
		(0.01)		(0.01)		(0.04)		(0.04)		(0.04)		(0.01)
Citizen Ideology		-0.11$^{\#}$		-0.09$^{\#}$		-0.06		-0.06		-0.06		-0.09$^{\#}$
		(0.06)		(0.05)		(0.15)		(0.15)		(0.15)		(0.05)
State Population (Logged)		0.01		0.01		-0.13		-0.08		-0.11		0.01
		(0.05)		(0.04)		(0.27)		(0.25)		(0.25)		(0.04)
Logged No. of Bills Introduced		-0.01		-0.01		-0.06*		-0.07*		-0.06*		-0.01
		(0.01)		(0.01)		(0.03)		(0.03)		(0.03)		(0.01)
Constant	<0.01	-0.24	-0.01	-0.15	0.15	0.20	0.10	-0.86	0.16$^{\#}$	-0.04	-0.01	-0.19
	(¡0.01)	(0.62)	(0.01)	(0.53)	(0.11)	(3.64)	(0.10)	(3.42)	(0.09)	(3.37)	(0.01)	(0.53)
AIC	-2970.08	-2339.79	-3348.37	-2718.23	-1286.91	-714.98	-1292.69	-717.78	-1279.77	-713.49	-2791.50	-2707.11
R-squared	0.03	0.04	0.03	0.04	0.02	0.04	0.02	0.05	0.02	0.04	0.03	0.04
N	2854	1716	3293	1988	2036	959	2036.00	959	2031	958	2893	1984

Note: $^{\#}p<0.1$; $^{*}p<0.05$. Models are at state-chamber-year level, with fixed effects for state-chamber. Results are fixed effects OLS with multiway standard errors clustered by state and year. Vetoes, state ideology, and citizen ideology are scaled down by 100.

C.2 Predicting Lagged Treatment Status Using Outcomes

In the second set of models, we examine whether treatment predicts a lagged outcome. If so, it suggests that states which adopted coverage were systematically different in some way from those that did not, prior to receiving the treatment. There are no significant relationships in the bivariate models or with a full set of controls (Appendix Table C2).

Table C2: Predicting Lagged Treatment Status Using Outcomes

	Late Budget		Budget Kurtosis		Inter-Party Polar.		Dem. Intra-party Polar.		GOP Intra-Party Polar.		Leg. Productivity	
Adoption of Floor Coverage	-0.32	0.41	0.01	0.03	0.04	0.01	0.01	0.02	<0.01	0.01	-0.02	-0.04
	(0.33)	(0.56)	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Total No. of Bills/Resolutions Vetoed in State-Year		0.01		0.03		0.01		-0.01		>-0.01		-0.01
		(0.31)		(0.02)		(0.01)		(0.01)		(<0.01)		(0.02)
Leg. Prof. 1st Dimensional Scaling		1.46*		<0.01		0.01		0.02		-0.03		-0.03$^{\#}$
		(0.55)		(0.02)		(0.04)		(0.02)		(0.03)		(0.02)
Leg. Prof. 2nd Dimensional Scaling		0.99*		0.02		0.01		0.02		-0.02		-0.02
		(0.51)		(0.02)		(0.03)		(0.02)		(0.03)		(0.01)
State Ideology		-1.31		0.04		-0.15$^{\#}$		-0.04		0.10		-0.07
		(2.26)		(0.10)		(0.08)		(0.03)		(0.06)		(0.05)
Political Competition		2.25		0.17$^{\#}$		0.30*		>0.01		-0.04		-0.10
		(2.69)		(0.08)		(0.12)		(0.06)		(0.11)		(0.08)
Gross State Product (Logged)		-2.37		0.03		-0.29		-0.11		-0.10		0.08
		(2.74)		(0.11)		(0.18)		(0.07)		(0.09)		(0.07)
State Term Limits (1=Yes)		-2.34		-0.01		0.05		-0.04		>-0.01		>-0.01
		(10.47)		(0.04)		(0.04)		(0.03)		(0.02)		(0.03)
Logged Legislative Expenditures		-1.54		0.03		0.03		-0.02		0.05		-0.04
		(1.81)		(0.06)		(0.08)		(0.03)		(0.07)		(0.04)
Citizen Ideology		-1.01		0.05		-0.04		-0.01		0.09		0.01
		(4.65)		(0.12)		(0.12)		(0.07)		(0.08)		(0.09)
State Population (Logged)		2.48		0.09		1.61*		-0.18		0.17		0.02
		(6.51)		(0.12)		(0.44)		(0.19)		(0.15)		(0.13)
Logged No. of Bills Introduced		-0.24		>-0.01		0.01		0.01		>-0.01		0.01
		(0.41)		(0.03)		(0.01)		(0.01)		(0.01)		(0.02)
Constant			0.43*	-2.06	1.47*	-20.34*	0.30*	4.73	0.27*	-1.89	0.33*	-0.25
			(0.01)	(1.62)	(0.02)	(6.59)	(0.01)	(2.80)	(0.01)	(2.17)	(0.01)	(1.82)
AIC	1113.03	489.03	-1318.47	-753.65	-2140.97	-1567.17	-5844.73	-2954.64	-5134.28	-2371.43	-4812.03	-2975.85
R-squared			0.10	0.14	0.92	0.96	0.74	0.82	0.67	0.75	0.66	0.66
N	1470	670	3382	1988	2036	912	2036	912	2031	911	2962	1759.00

Note: $^{\#}$p<0.1; *p<0.05. All outcomes are lagged by one year. Models are at state-chamber-year level, with fixed effects for state-chamber. Results for late budget are fixed effects logit with boot strapped standard errors. All other results columns are fixed effects OLS with multiway standard errors clustered by state and year. Vetoes, state ideology, and citizen ideology are scaled down by 100.

Appendix D Checks for Chamber-Level Heterogeneous Treatment Effects

D.1 Chamber-Level Estimates Over Time, Averaged Across Cohorts

Evidence that there is significant heterogeneity in the treatment effect is differing estimates across cohorts that are heavily weighted in the average treatment effect. The drawback of the Bacon decomposition approach is that the test requires strongly balanced panel data, so some observations are dropped as the estimator can only be calculated for state-chamber years in which all states are non-missing for the outcomes. These analyses do not account for both time and state-chamber covariates.

As Table D2 shows, there is very little evidence of heterogeneous treatment effects. For the late budget, intra-party polarization, and legislative productivity outcomes, there is no overall effect and only small differences in effects across time. For inter-party polarization, measured as the distance between each party's ideological median, there is a positive effect of adoption, indicating that gavel-to-gavel coverage increased party distance. This result should be interpreted with caution, however. First, this analysis begins at 1996, rather than for our full time-series which begins in 1980, because of the perfectly balanced panel requirement. Second one of the largest positive effects, and also the most heavily weighted, is from the "never treated" versus "treated" state-chambers, though only two chambers lie in the "never treated" category. Further, there is a negative effect on party distance for earlier treated state-chambers—those which adopted coverage in the late 1990s—meaning that for those states party distance actually decreased as compared to those treated later. This result is inconsistent with claims that initial adoption (mostly broadcast) in the 1990s, when polarization in the United States accelerated, resulted in greater polarization. Instead, this result suggests that adoption in the 2000s increased polarization relative to adoption in the 1980s (as shown by the "treated later" vs. "always treated" categories) and 1990s. It is difficult to reconcile late adoption with increased polarization as the phenomenon was already well-established and well-documented by the 2000s, nor is it clear why adoption in that decade would uniquely contribute to polarization differently than adoption in the 1980s or 1990s. We examine whether there is additional support for this significant effect in the legislator-level analysis.

D.2 Bacon Decomposition of Difference-in-Difference Estimator

Table D1: Chamber-Level Estimates Over Time, Averaged Across Cohorts

	Late Budget	Budget Kurtosis	Inter-Party Polar.	Dem. Intra-party Polar.	GOP Intra-Party Polar.	Leg. Productivity
1987	-.154*	-.236				
	(.100)	(.052)				
1988		-.283*				
		(.043)				
1989		-.523*				
		(.082)				
1990	-.154	-.634*				
	(.100)	(.066)				
1991		-.298*				
		(.040)				
1992		-.451*				
		(.077)				
1993	-6e-17	-.157				
	(.333)	(.122)				
1994		.085				
		(.083)				
1995	.380*	.005				
	(.185)	(.080)				
1996	.138	-.090				
	(.144)	(.073)				
1997	-.017	-.016	-.085	.007	.041	-.099*
	(.030)	(.075)	(.057)	(.010)	(.036)	(.020)
1998	-.044	.035	-.020	.002	.011	-.049
	(.034)	(.072)	(.024)	(.005)	(.012)	(.080)
1999	.021	.177*	.035	.024	.007	.019
	(.102)	(.074)	(.042)	(.018)	(.019)	(.034)
2000	.105	.020	.022	.017	.005	.052
	(.120)	(.086)	(.025)	(.014)	(.013)	(.043)
2001	.044	.077	.010	.007	.063	.014
	(.069)	(.077)	(.040)	(.013)	(.054)	(.028)
2002	-.060	.097	.017	.008	.059	-.033
	(.097)	(.070)	(.041)	(.013)	(.052)	(.064)
2003	.235	.110	.037	.008	.049	-.045
	(.131)	(.072)	(.059)	(.019)	(.050)	(.044)
2004	.103	.021	.051	.008	.045	.041
	(.129)	(.075)	(.059)	(.018)	(.049)	(.026)
2005	-.035	.199*	.073	.017	.039	-.016
	(.107)	(.058)	(.063)	(.018)	(.049)	(.027)
2006	-.059	.151*	.079	.021	.037	-.021
	(.112)	(.073)	(.063)	(.019)	(.048)	(.031)
2007	.032	.091	.125	-.000	.036	-.001
	(.082)	(.059)	(.068)	(.018)	(.039)	(.025)
2008	-.051	.14*8	.118*	-.002	.042	-.052
	(.036)	(.050)	(.059)	(.016)	(.037)	(.036)
2009	-.079	.101*	.109	-.012	.001	-.028
	(.102)	(.047)	(.072)	(.019)	(.034)	(.023)
2010	.047	-.061	.105	-.010	.005	.004
	(.102)	(.055)	(.070)	(.019)	(.032)	(.038)
2011	-.089	-.001	.026	-.015	.021	.018
	(.088)	(.055)	(.072)	(.020)	(.032)	(.036)
2012	-.081	.032	.027	-.024	.022	-.038
	(.081)	(.064)	(.065)	(.020)	(.030)	(.047)
2013	-.110	.004	.029	.007	.051*	.018
	(.075)	(.051)	(.069)	(.016)	(.025)	(.016)
2014	-.032	.051	.030	.009	.050	-.012
	(.078)	(.064)	(.068)	(.017)	(.026)	(.028)
2015	-.125	.029	.024	.054*	.026	.013
	(.065)	(.049)	(.069)	(.016)	(.025)	(.019)
2016						-.028
						(.039)
2017	.056	.111*	.085*	.027	.023	
	(.123)	(.054)	(.063)	(.019)	(.031)	
2018	-.026		.090	.034		.020
	.070		(.066)	(.020)		(.031)
N	2,841	3,332	1,772	1,772	1,000	1,767

Note: #p<0.1; *p<0.05. Estimated ATET is calculated using regression adjustment technique.

Table D2: Bacon Decomposition of Difference-in-Difference Estimator

	Late Budget	Budget Kurtosis	Inter-Party Polar.	Dem. Intra-party Polar.	GOP Intra-Party Polar.	Leg. Productivity
Estimated ATET	-.002	.020	.118*	.009	.028	-.019
	(.038)	(.013)	(.052)	(.019)	(.022)	(.015)
Treated vs. Never Treated	.007; .331	.020; .390	.149; .319	-.008; .319	.060; .319	-.025; .40
Treated Earlier vs Later	.020; .135	.017; .416	-.109; .133	.125; .133	.018; .133	-.021; .415
Treated Later vs Earlier	.046; .283	.025; .194	.088; .274	-.016: .274	.029; .274	-.008; .18
Treated Later vs Always Treated	-.033; .285		.224; .273	.052; .274	-.006; .274	
N	748	3,382	782	782	782	2,962

Note: #p<0.1; *p<0.05. Estimated ATET is bivariate average treatment effect on the treated calculated using Stata's "xtdidregress" command. Treated estimates are ATET decomposition summaries and associated weights (after semi-colon). Estimates are calculated using bacondecomp in Stata and are weighted average of all possible two-group/two period DD estimators. Controls excluded, standard errors clustered by state-chamber. Imbalanced panels excluded.

Appendix E Additional Details on PanelMatch

The advantage of this technique is that treatment and control observations are matched on their treatment history using a set number of lags, such that each treated observation is as similar as possible to a control observation on its treatment history and other covariates, allowing for estimated effects to account for different time exposures to the treatment. This approach also accounts for staggered treatment assignment.

The matching process here uses the same covariates as those used in the regression results. We estimate lagged effects for the year of adoption and four additional years. This choice encompasses more than one full term for a four-year legislature or two terms for a two-year legislature, during which one would expect any results from adoption to manifest. We also use a lag of four years to match on treatment history and found propensity score matching best minimized distance between the treatment and control groups.

Figures E1 through E6 show visualizations of how different matching methods improve balance for each of the outcomes. We explore results using mahalanobis distance, propensity score, and propensity score weighting. The x-axis shows balance before refinement and the y-axis shows balance after refinement. Improvement in balance is shown by observations moving below the 45-degree line. As the figure shows, propensity score matching most improves balance between the treated and control observations. These results are confirmed by examining the covariate balance across matched sets across all independent variables (other than treatment) used in the analysis. All variables estimated in the panel match models are included, along with four year lags. Because we have a large number of observations, we match using up to ten observations.

Figure E7 shows the number of matched control units that share the same treatment history with a treated observation with four year lags for each outcome. As the histograms show, for most treated observations there are a substantial number of matched observations with the same treatment, giving confidence to the results.

E.1 Chamber-Level Estimates from Panelmatch By Time

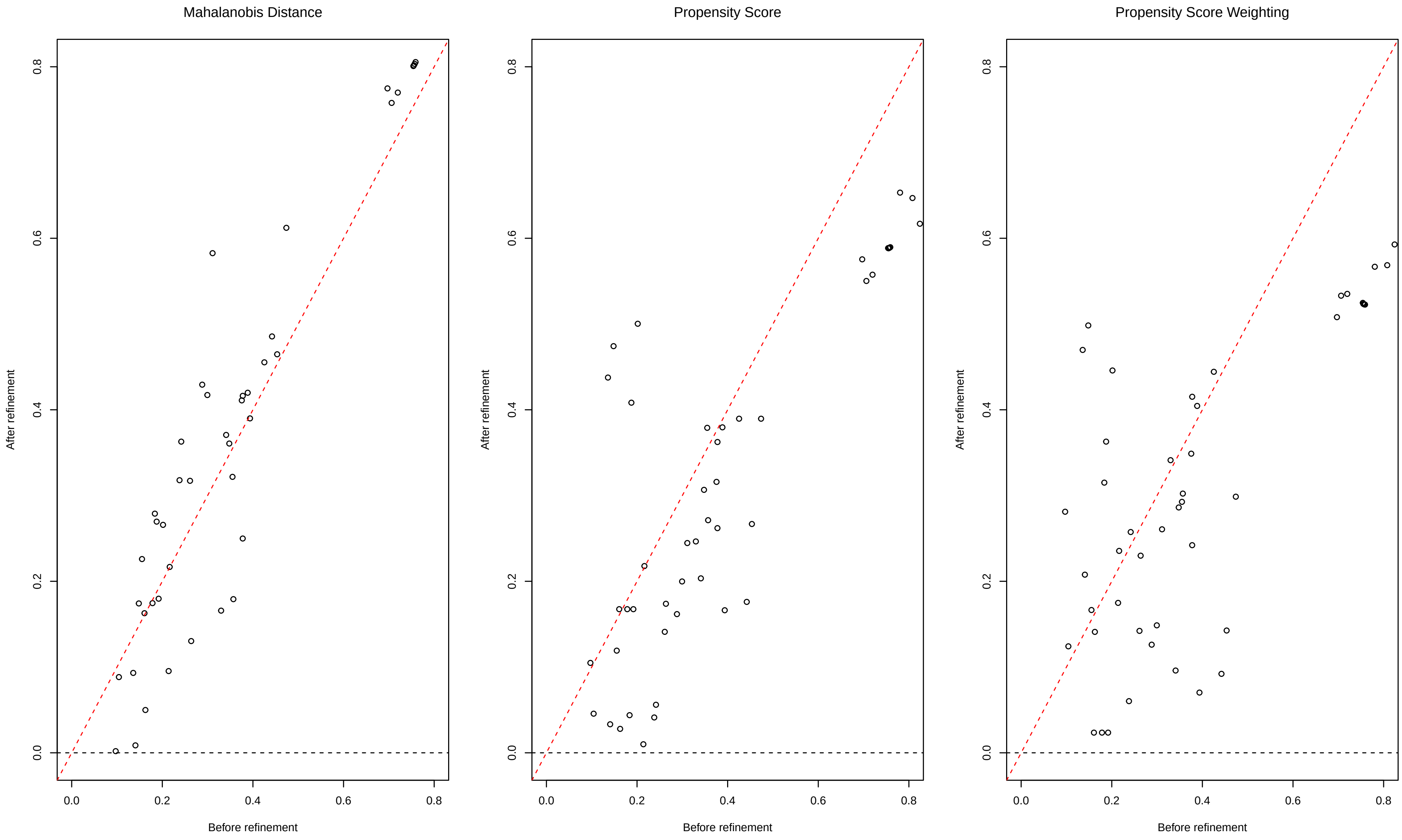
Table E1: Chamber-Level Estimates from Panelmatch By Time

	Late Budget	Budget Kurtosis	Inter-Party Polar.	Dem. Intra-party Polar.	GOP Intra-Party Polar.	Leg. Productivity
T0	-.058	.071	.004	.007	.001	-.011
	(.062)	(.039)	(.012)	(.004)	(.01)	(.021)
T1	-.023	.059	-.007	.005	.024*	-.028*
	(.036)	(.040)	(.018)	(.008)	(.013)	(.012)
T2	-.045	.071	-.036	.005	.024	-.007
	(.052)	(.037)	(.026)	(.008)	(.017)	(.020)
T3	-.045	.044	-.019	-.001	.038	-.034*
	(.037)	(.037)	(.032)	(.011)	(.025)	(.014)
T4	-.005	.011	-.015	-.004	.041*	.041
	(.075)	(.040)	(.038)	(.012)	(.001)	(.023)
Iterations	1,000	1,000	1,000	1,000	1,000	1,000

Note: $^{\#}p<0.1$; $^{*}p<0.05$. Estimated ATET is calculated using propensity score matching with four lags and all control variables shown in Table B1.

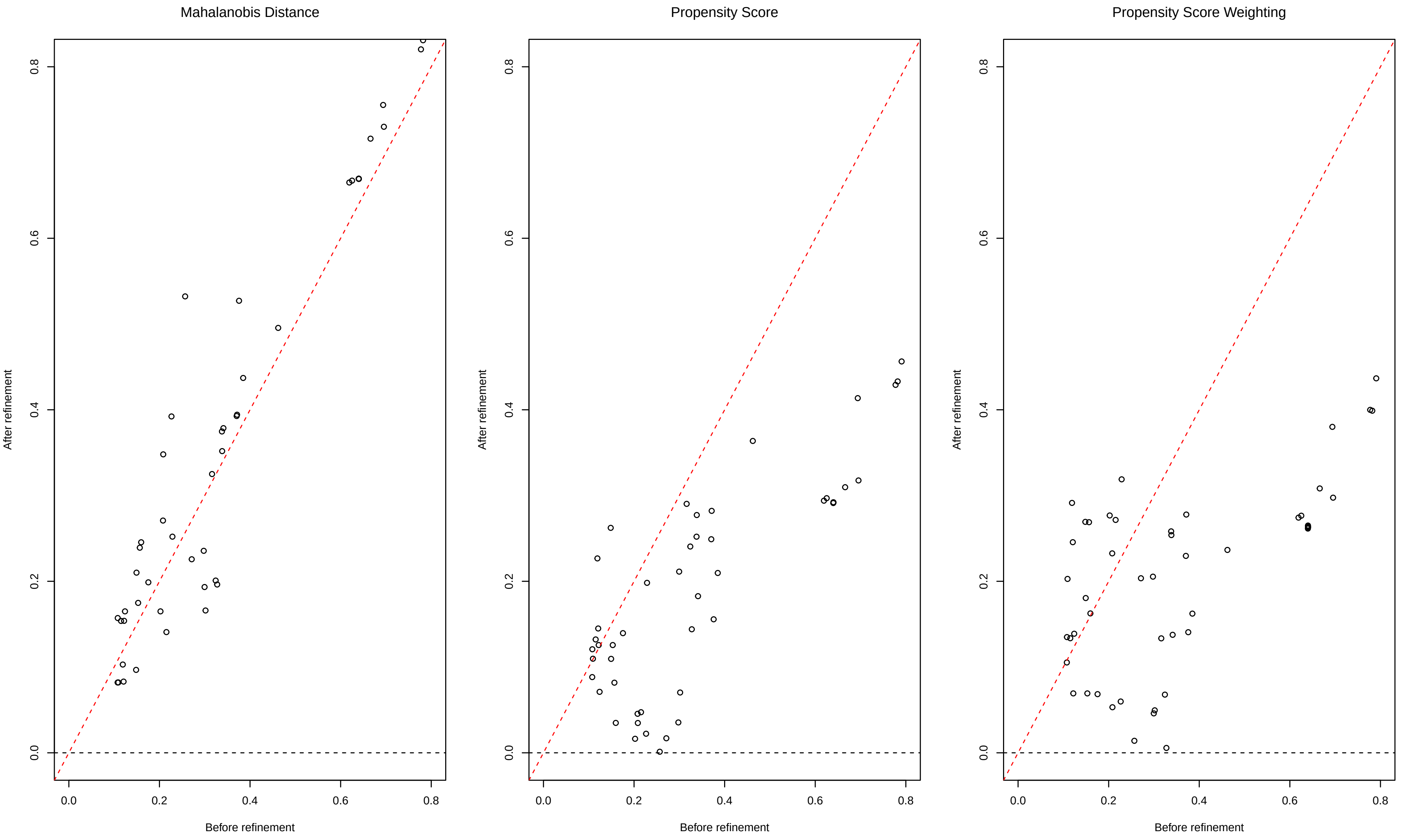
E.2 Covariate Balance From Different Matching Methods: Late Budget

Figure E1: Covariate Balance From Different Matching Methods: Late Budget



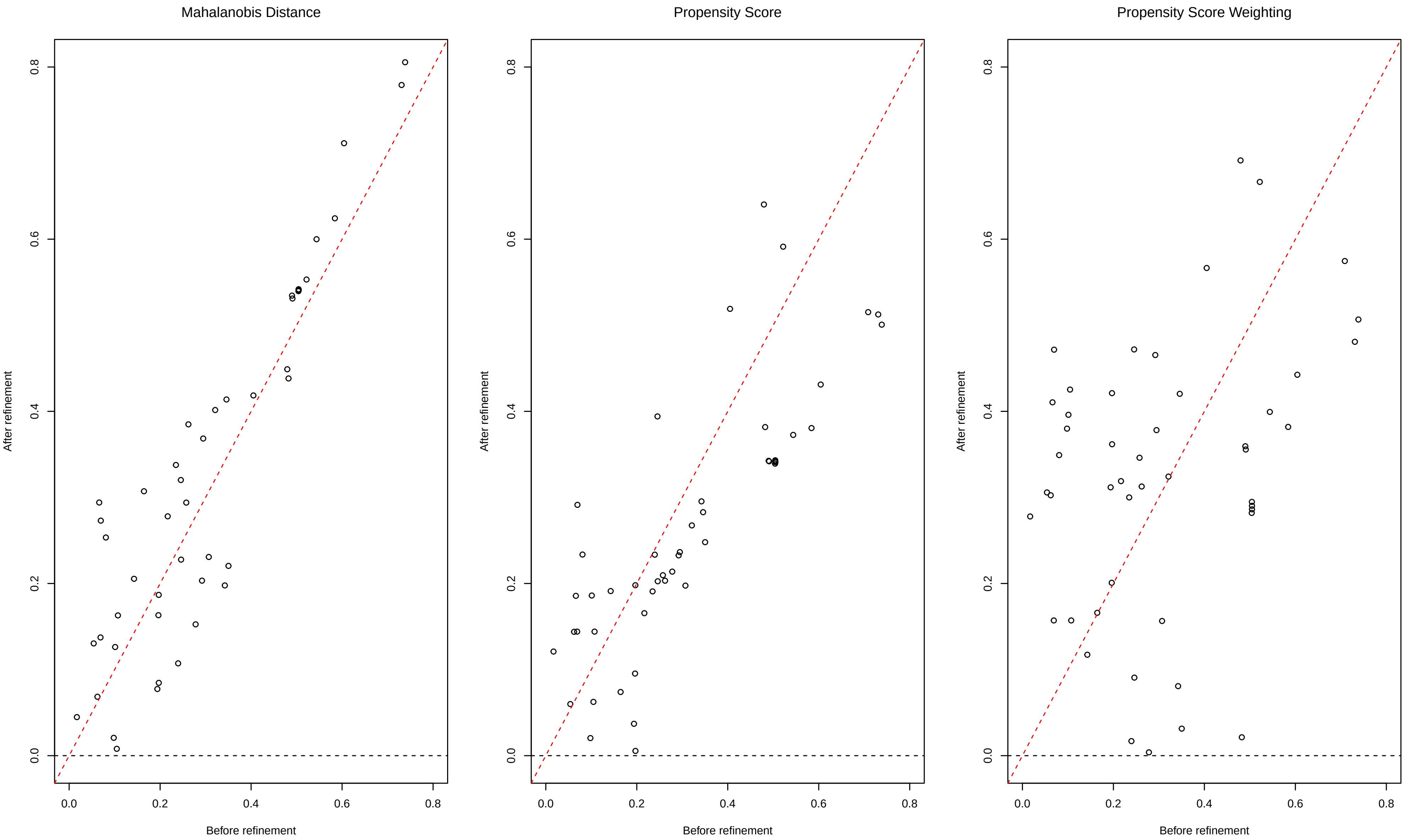
E.3 Covariate Balance From Different Matching Methods: Budget Kurtosis

Figure E2: Covariate Balance From Different Matching Methods: Budget Kurtosis



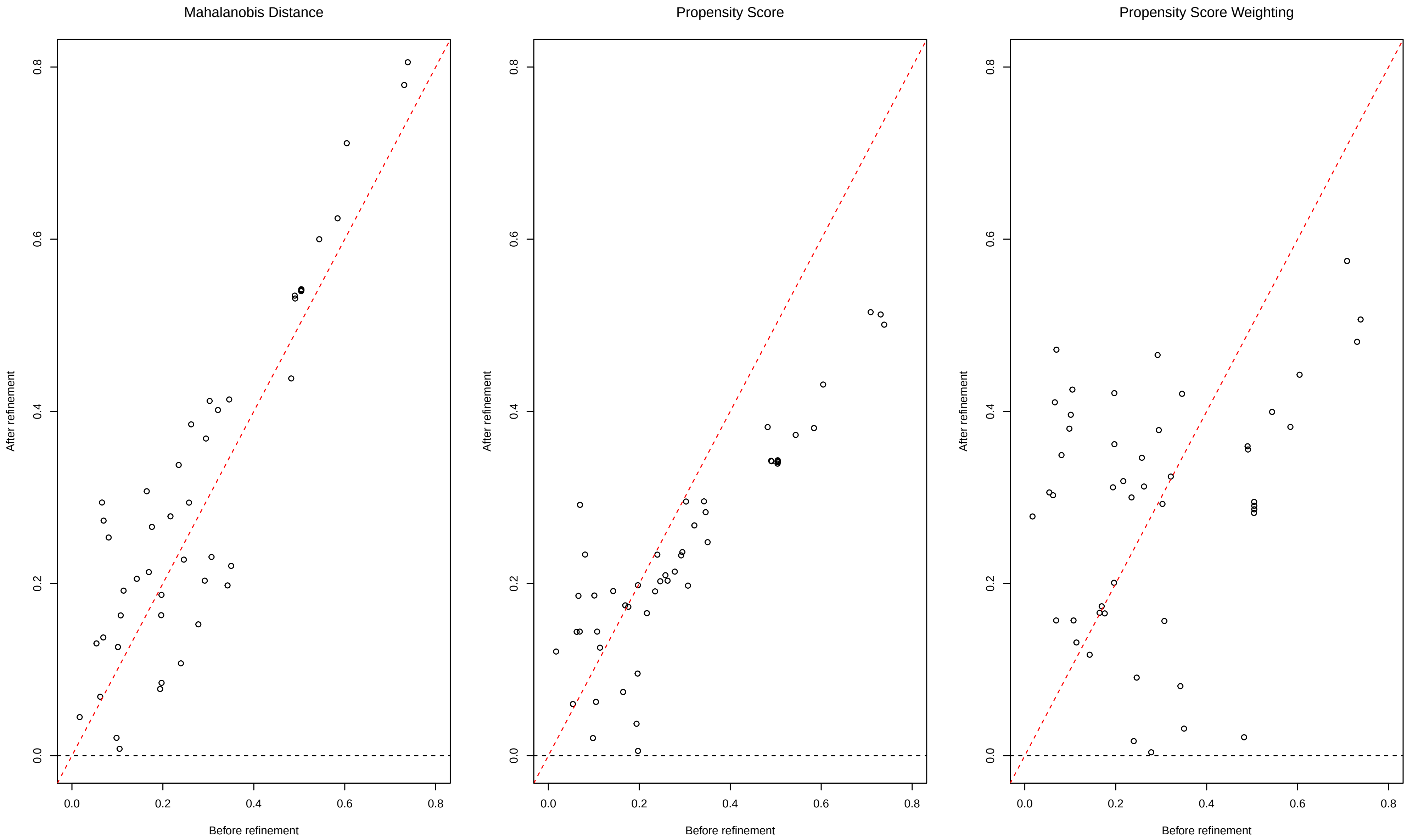
E.4 Covariate Balance From Different Matching Methods: Inter-Party Polarization

Figure E3: Covariate Balance From Different Matching Methods: Inter-Party Polarization



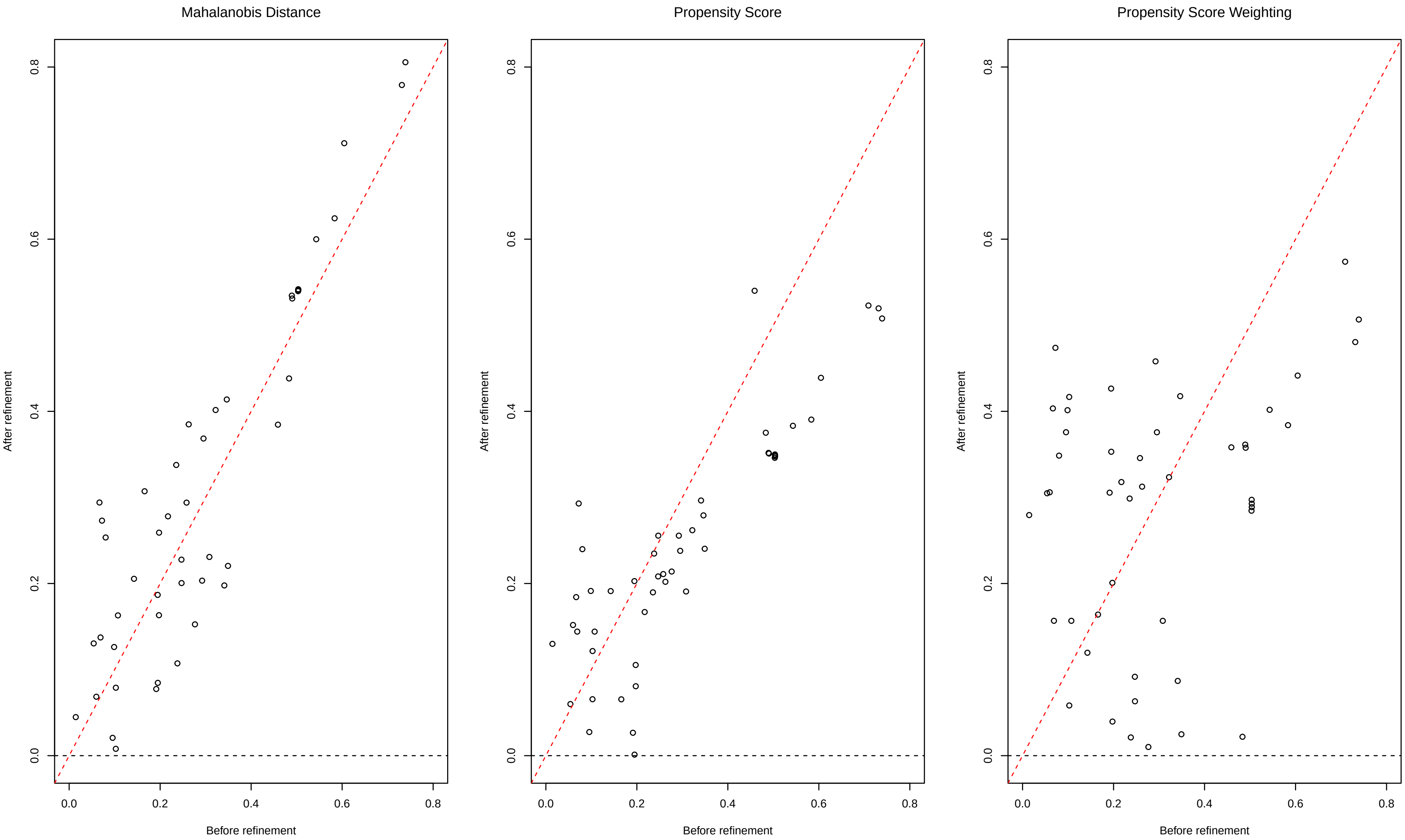
E.5 Covariate Balance From Different Matching Methods: Democratic Intra-Party Polarization

Figure E4: Covariate Balance From Different Matching Methods: Democratic Intra-Party Polarization



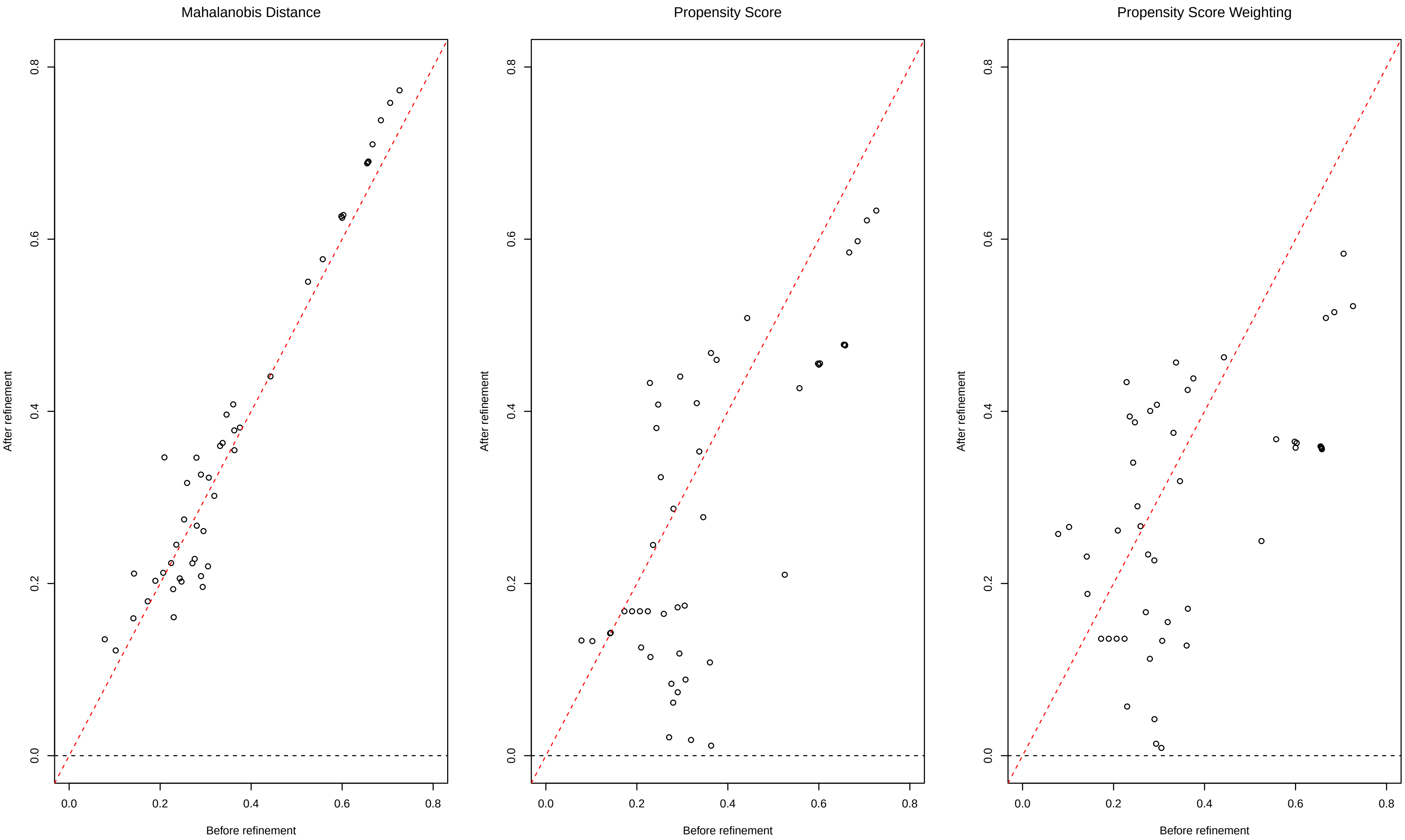
E.6 Covariate Balance From Different Matching Methods: Republican Intra-Party Polarization

Figure E5: Covariate Balance From Different Matching Methods: Republican Intra-Party Polarization



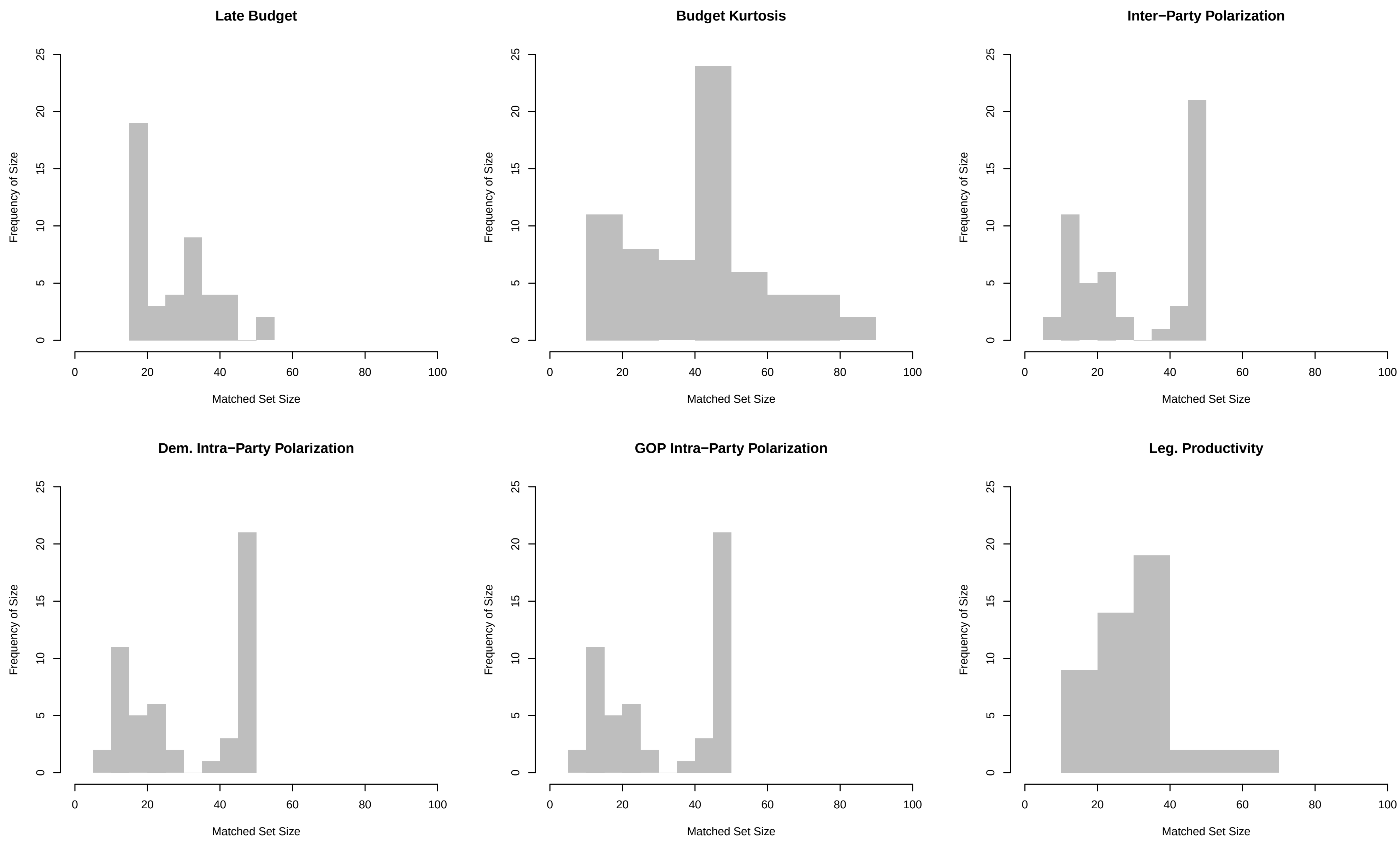
E.7 Covariate Balance From Different Matching Methods: Legislative Productivity

Figure E6: Covariate Balance From Different Matching Methods: Legislative Productivity



E.8 Frequency Distribution of Matched Control Units for Chamber-Level Outcomes

Figure E7: Frequency Distribution of Matched Control Units for Chamber-Level Outcomes



Histograms show the number of matched control units that share the same treatment history as a treated for four years prior to the treatment year for each outcome.

Appendix F Legislator-Level Empirical Models and Robustness Checks

F.1 The Effect of Broadcast/Streamed Floor Coverage on Individual Legislator Outcomes

Table F1: The Effect of Broadcast/Streamed Floor Coverage on Individual Legislator Outcomes

	Ideology		Leg. Effectiveness		Party Loyalty	
	Democrats	**Republicans**				
Adoption of Floor Coverage	-0.03*	<0.01	0.03	0.03	<0.01	0.01
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)
Majority Party Legislator	0.01	<0.01	0.67*	0.67*	0.03*	0.03*
	(0.01)	(0.01)	(0.03)	(0.03)	(<0.01)	(<0.01)
State Term Limits (1=Yes)	-0.01	0.04*	0.02	0.02	0.01	0.01
	(0.02)	(0.01)	(0.05)	(0.05)	(0.01)	(0.01)
Total No. of Bills/Resolutions Vetoed in State-Year	0.01$^{\#}$	<0.01	>-0.01	>-0.01	>-0.01	>-0.01
	(0.01)	(<0.01)	(0.01)	(0.01)	(<0.01)	(<0.01)
Leg. Prof. 1st Dimensional Scaling	-0.07*	-0.02	0.02	0.02	0.03*	0.03*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Leg. Prof. 2nd Dimensional Scaling	-0.06*	-0.02	0.01	0.01	0.02*	0.02*
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
State Ideology	0.06	0.02	0.03	0.03	0.04*	0.04*
	(0.04)	(0.04)	(0.09)	(0.09)	(0.02)	(0.02)
Political Competition	-0.06	0.06	0.23	0.23	0.11*	0.11*
	(0.06)	(0.05)	(0.14)	(0.14)	(0.02)	(0.02)
Gross State Product (Logged)	-0.07	-0.20*	0.04	0.04	-0.08*	-0.08*
	(0.08)	(0.08)	(0.17)	(0.17)	(0.03)	(0.03)
Logged Legislative Expenditures	0.08*	0.03	-0.01	-0.01	-0.02	-0.02
	(0.03)	(0.03)	(0.09)	(0.09)	(0.02)	(0.02)
Citizen Ideology	-0.05	-0.05	0.23	0.23	0.04	0.04
	(0.05)	(0.05)	(0.14)	(0.14)	(0.04)	(0.04)
State Population (Logged)	-0.51*	0.55*	-0.02	-0.01	0.04	0.04
	(0.16)	(0.16)	(0.29)	(0.29)	(0.07)	(0.07)
Logged No. of Bills Introduced	0.01	>-0.01	0.01	0.01	<0.01	<0.01
	(0.01)	(<0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Legislator Party, 1=GOP			0.01	0.01	>-0.01	<0.01
			(0.04)	(0.05)	(<0.01)	(0.01)
Adoption x Party				>-0.01		>-0.01
				(0.05)		(0.01)
Constant	6.54*	-5.90*	0.24	0.23	1.51	1.49
	(2.23)	(2.08)	(4.02)	(4.05)	(1.05)	(1.07)
AIC	-22054.58	-19504.74	95657.68	95659.68	-82584.59	-82583.37
R-Squared	0.92	0.87	0.49	0.49	0.41	0.41
N	20,414	19,699	40,458	40,458	39,434	39,434

Note: $^{\#}$p<0.1; *p<0.05. Results columns are fixed effects OLS with multiway standard errors clustered by district and year. Vetoes, state ideology, and citizen ideology are scaled down by 100.

F.2 The Effect of Broadcast/Streamed Floor Coverage on Individual Legislator Outcomes Conditional on Majority Party Status

Table F2: The Effect of Broadcast/Streamed Floor Coverage on Individual Legislator Outcomes Conditional on Majority Party Status

	Ideology		Leg. Effectiveness	Party Loyalty
	Democrats	**Republicans**		
Adoption of Floor Coverage	-0.04*	0.01	-0.02	0.01
	(0.01)	(0.01)	(0.04)	(0.01)
Majority Party Legislator	<0.01	<0.01	0.63*	0.04*
	(0.01)	(0.01)	(0.04)	(<0.01)
Adoption x Majority	0.01	>-0.01	0.08	>-0.01
	(0.01)	(0.01)	(0.06)	(0.01)
State Term Limits (1=Yes)	-0.01	0.04*	0.02	0.01
	(0.02)	(0.01)	(0.05)	(0.01)
Total No. of Bills/Resolutions Vetoed in State-Year	0.01#	<0.01	>-0.01	>-0.01
	(<0.01)	(<0.01)	(0.01)	(<0.01)
Leg. Prof. 1st Dimensional Scaling	-0.07*	-0.02	0.02	0.03*
	(0.02)	(0.02)	(0.02)	(0.01)
Leg. Prof. 2nd Dimensional Scaling	-0.06*	-0.02	0.01	0.02*
	(0.02)	(0.01)	(0.02)	(0.01)
State Ideology	0.06	0.02	0.04	0.04*
	(0.04)	(0.04)	(0.09)	(0.02)
Political Competition	-0.06	0.06	0.24	0.11*
	(0.06)	(0.05)	(0.14)	(0.02)
Gross State Product (Logged)	-0.08	-0.20*	0.04	-0.08*
	(0.08)	(0.08)	(0.17)	(0.03)
Logged Legislative Expenditures	0.08*	0.03	-0.02	-0.02
	(0.03)	(0.03)	(0.09)	(0.02)
Citizen Ideology	-0.06	-0.05	0.24	0.04
	(0.05)	(0.05)	(0.14)	(0.04)
State Population (Logged)	-0.47*	0.56*	>-0.01	0.04
	(0.16)	(0.16)	(0.29)	(0.07)
Logged No. of Bills Introduced	0.01	>-0.01	0.01	<0.01
	(<0.01)	(<0.01)	(0.02)	(0.01)
Legislator Party, 1=GOP			0.01	>-0.01
			(0.04)	(<0.01)
Constant	6.16*	-6.00*	0.05	1.52
	(2.22)	(2.10)	(4.02)	(1.05)
AIC	-22057.54	-19503.15	95647.91	-82584.77
R-Squared	0.92	0.87	0.49	0.41
N	20,414	19,699	40,458	39,434

Note: #p<0.1; *p<0.05. Results columns are fixed effects OLS with multiway standard errors clustered by district and year. Vetoes, state ideology, and citizen ideology are scaled down by 100.

Appendix G Empirical Tests of Parallel Trends Assumption for Legislator-Level Analysis

G.1 Predicting Change in Treatment Status Using Outcomes

Table G1: Predicting Change in Treatment Status Using Outcomes

	Change in Legislator Treatment Status											
	Democrats	**Republicans**	**Democrats**	**Republicans**								
Ideology	>-0.01	<0.01	-0.01	<0.01								
	(0.01)	(<0.01)	(0.01)	(0.01)								
Legislative Effectiveness					<0.01	<0.01	>-0.01	<0.01#				
					(<0.01)	(<0.01)	(<0.01)	(<0.01)				
Leg. Effect. x Party							<0.01#	>-0.01				
							(<0.01)	(<0.01)				
Party Loyalty									0.04*	0.04*	0.03*	0.02
									(0.01)	(0.01)	(0.01)	(0.02)
Party Loyalty x Party											0.01	0.04#
											(0.02)	(0.02)
Legislator Party, 1=GOP						0.01*	<0.01	0.01*		0.01*	>-0.01	-0.03
						(<0.01)	(<0.01)	(<0.01)		(<0.01)	(0.02)	(0.02)
Majority Party Legislator			<0.01	-0.01		-0.01*		-0.01*		-0.01*		-0.01*
			(<0.01)	(0.01)		(<0.01)		(<0.01)		(<0.01)		(<0.01)
State Term Limits (1=Yes)			0.12*	0.11*		0.12*		0.12*		0.11*		0.11*
			(0.01)	(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Total No. of Bills/Resolutions Vetoed in State-Year			<0.01*	0.01*		0.01*		0.01*		0.01*		0.01*
			(<0.01)	(<0.01)		(<0.01)		(<0.01)		(<0.01)		(<0.01)
Leg. Prof. 1st Dimensional Scaling			0.01	0.01*		0.01*		0.01*		0.01*		0.01*
			(<0.01)	(0.01)		(<0.01)		(<0.01)		(<0.01)		(<0.01)
Leg. Prof. 2nd Dimensional Scaling			<0.01	0.02*		0.02*		0.02*		0.02*		0.02*
			(0.01)	(0.01)		(<0.01)		(<0.01)		(<0.01)		(<0.01)
State Ideology			0.16*	0.21*		0.15*		0.15*		0.16*		0.16*
			(0.02)	(0.02)		(0.02)		(0.02)		(0.02)		(0.02)
Political Competition			-0.03	-0.14*		-0.18*		-0.18*		-0.15*		-0.15*
			(0.03)	(0.04)		(0.03)		(0.03)		(0.02)		(0.02)
Gross State Product (Logged)			-0.24*	-0.27*		-0.27*		-0.27*		-0.25*		-0.25*
			(0.06)	(0.05)		(0.03)		(0.03)		(0.03)		(0.03)
Logged Legislative Expenditures			0.04*	0.04*		0.05*		0.05*		0.07*		0.07*
			(0.01)	(0.02)		(0.01)		(0.01)		(0.01)		(0.01)
Citizen Ideology			-0.48*	-0.62*		-0.55*		-0.55*		-0.55*		-0.55*
			(0.05)	(0.06)		(0.04)		(0.04)		(0.04)		(0.04)
State Population (Logged)			1.33*	1.75*		1.33*		1.33*		1.31*		1.31*
			(0.10)	(0.11)		(0.07)		(0.07)		(0.07)		(0.07)
Logged No. of Bills Introduced			-0.08*	-0.09*		-0.09*		-0.09*		-0.09*		-0.09*
			(0.01)	(0.01)		(<0.01)		(<0.01)		(<0.01)		(<0.01)
Constant	>-0.01	0.01#	-17.77*	-23.59*	0.01*	-17.34*	0.01*	-17.38*	0.13*	-17.42*	0.13*	-17.43*
	(0.01)	(<0.01)	(1.42)	(1.58)	(<0.01)	(0.95)	(<0.01)	(0.96)	(0.02)	(0.97)	(0.02)	(0.97)
AIC	-29667.42	-31611.64	-14767.68	-14444.66	-58793.18	-27360.03	-58800.16	-27360.59	-41851.94	-26689.00	-41854.00	-26691.29
R-squared	0.03	0.03	0.12	0.14	0.03	0.13	0.03	0.13	0.03	0.13	0.03	0.13
N	38,742	41,928	17,767	17,393	80,670	35,160	80,670	35,160	62,634	34,565	62,634	34,565

Note: $^{\#}$p<0.1; *p<0.05. Models are at legislator-year level, with fixed effects for district. Results are fixed effects OLS with multiway standard errors clustered by district and year. Vetoes, state ideology, and citizen ideology are scaled down by 100.

G.2 Predicting Lagged Treatment Status Using Outcomes

Table G2: Predicting Lagged Treatment Status Using Outcomes

	Ideology				Legislative Effectiveness		Party Loyalty	
	Democrats	**Republicans**	**Democrats**	**Republicans**				
Adoption of Floor Coverage	-0.02	0.04#	-0.03	0.06	0.01	0.04	<0.01	0.02*
	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)	(<0.01)	(0.01)
Majority Party Legislator			0.02	0.02#		0.46*		0.03*
			(0.02)	(0.01)		(0.09)		(<0.01)
State Term Limits (1=Yes)			0.02	0.02		0.04		<0.01
			(0.03)	(0.04)		(0.07)		(0.01)
Total No. of Bills/Resolutions Vetoed in State-Year			<0.01	0.01		<0.01		>-0.01
			(0.01)	(0.01)		(0.01)		(<0.01)
Leg. Prof. 1st Dimensional Scaling			-0.07	>-0.01		0.01		0.04*
			(0.04)	(0.02)		(0.02)		(0.01)
Leg. Prof. 2nd Dimensional Scaling			-0.07#	<0.01		<0.01		0.02*
			(0.04)	(0.02)		(0.02)		(0.01)
State Ideology			0.01	0.03		0.01		<0.01
			(0.07)	(0.06)		(0.07)		(0.02)
Political Competition			-0.15	-0.02		0.15		0.09*
			(0.12)	(0.12)		(0.17)		(0.04)
Gross State Product (Logged)			0.13	-0.68*		0.13		-0.06
			(0.16)	(0.26)		(0.19)		(0.04)
Logged Legislative Expenditures			0.04	-0.04		-0.03		-0.03
			(0.06)	(0.08)		(0.10)		(0.02)
Citizen Ideology			0.08	-0.32		0.14		0.09#
			(0.18)	(0.23)		(0.14)		(0.05)
State Population (Logged)			-1.24*	1.22*		-0.17		0.07
			(0.52)	(0.55)		(0.34)		(0.07)
Logged No. of Bills Introduced			0.03*	-0.01		<0.01		<0.01
			(0.01)	(0.01)		(0.02)		(<0.01)
Constant	-0.74*	0.70*	16.18*	-8.99	1.02*	1.99	0.88*	0.91
	(0.01)	(0.01)	(6.32)	(6.04)	(0.01)	(5.35)	(0.00)	(1.02)
AIC	19695.60	24971.13	3925.04	3861.66	204661.69	84217.43	-144469.56	-76169.20
R-squared	0.70	0.56	0.77	0.68	0.34	0.47	0.35	0.44
N	38,528	41,737	17,302	17,060	80,572	34,886	66,759	34,423

Note: #p<0.1; *p<0.05. Models are at legislator-year level, with fixed effects for district. Results are fixed effects OLS with multiway standard errors clustered by district and year. Vetoes, state ideology, and citizen ideology are scaled down by 100.

Appendix H Check for Legislator-Level Heterogeneous Treatment Effects

H.1 Legislator-Level Estimates Over Time, Averaged Across Cohorts

Table H1: Legislator-Level Estimates Over Time, Averaged Across Cohorts

	Democratic Ideology	Republican Ideology	Legislative Effectiveness	Party Loyalty
1995	-.144			
	(.092)			
1996	-.082			
	(.056)			
1997	-.008	-.010	.004	-.069*
	(.070)	(.018)	(.137)	(.020)
1998	-.001	-.002	.001	-.009*
	(.013)	(.002)	(.016)	(.003)
1999	.024	-.013	.031	.027*
	(.020)	(.015)	(.065)	(.010)
2000	.020	-.012	.025	.017*
	(.017)	(.012)	(.054)	(.009)
2001	-.022	-.006	-.064	.041*
	(.014)	(.022)	(.068)	(.008)
2002	-.023	-.002	-.040	.041*
	(.014)	(.022)	(.068)	(.008)
2003	.004	-.018	-.037	-.030*
	(.026)	(.039)	(.103)	(.011)
2004	.004	-.030	-.049	-.029*
	(.025)	(.038)	(.099)	(.011)
2005	.015	-.052	-.011	-.024*
	(.023)	(.033)	(.076)	(.010)
2006	.017	-.045	-.018	-.018*
	(.019)	(.028)	(.066)	(.008)
2007	.003	-.039	.003	-.025*
	(.018)	(.026)	(.061)	(.008)
2008	-.011	-.031	.006	-.021*
	(.016)	(.022)	(.054)	(.006)
2009	-.009	-.038	-.017	-.034*
	(.020)	(.029)	(.064)	(.009)
2010	.004	-.043	-.007	-.027*
	(.017)	(.024)	(.054)	(.009)
2011	-.015	-.036	.004	-.006
	(.020)	(.023)	(.057)	(.007)
2012	-.022	-.027	-.001	-.013*
	(.017)	(.020)	(.052)	(.005)
2013	-.067*	.035	-.022	-.010
	(.023)	(.026)	(.050)	(.008)
2014	-.044	.041	-.032	-.008
	(.024)	(.026)	(.049)	(.007)
2015	-.044	.029	-.029	
	(.028)	(.027)	(.052)	
2016	-.061*	.030	-.028	
	(.028)	(.024)	(.050)	
2017	-.043	.011	-.011	
	(.029)	(.026)	(.057)	
2018	-.054	.022	-.014	
	(.029)	(.028)	(.059)	
N	31,112	31,781	69.095	24,574

Note: *p<0.05. Estimated ATET is calculated using regression adjustment technique.

H.2 Bacon Decomposition of Difference-in-Difference Estimator at the Legislator Level

Because the Goodman-Bacon estimator requires perfectly balanced data, we cannot use that here due to different time periods for which we have legislator data based on the state. For example, the NP scores only encompass certain states for certain years, as do the legislative effectiveness scores. Using the Goodman-Bacon estimator would require dropping all but those observations which exist for a given, overlapping time period, which would significantly reduce the number of observations.[22]

[22]For example, we could estimate heterogeneous treatment effects for all observations which have complete data from 1996-2018, or for those that have complete data from 1997-2018, or 1998-2018, or 1996-2015, etc.

Appendix I Additional Details on PanelMatch at the Legislator-Level

As with the chamber-level analysis, the matching process here uses the same covariates as those used in the regression results and with the year of adoption plus four additional years. We also use a lag of four years to match on treatment history though the best matching method differs by outcome because the samples differ slightly due to missing observations on some outcomes, we examine refinement for mahalanobis distance, propensity score matching, propensity score weighting, covariate balance propensity score matching, and covariate balance propensity score weighting. The appropriate technique differs by outcomes.

For Democratic ideology mahalanobis distance performs the best, for Republican ideology covariate balance propensity score peforms the best, and for legislative effectiveness and party loyalty propensity score matching performs the best. These results are confirmed by examining the covariate balance across matched sets across all independent variables (other than treatment) used in the analysis. All variables estimated in the panel match models are included, along with four year lags. Because we have a large number of observations, we match using up to ten observations.

I.1 Legislator-Level Estimates from Panelmatch By Time

Table I1: Legislator-Level Estimates from Panelmatch By Time

	Democratic Ideology	Republican Party Ideology	Legislative Effectiveness	Party Loyalty	GOP Intra-Party Polar.	Leg. Productivity
T0	-.005	-.018	.020	-.01		
	(.008)	(.01)	(.059)	(.006)		
T1	-.012	-.028	-.038	-.013		
	(.009)	(.024)	(.079)	(.008)		
T2	-.004	-.037	-.052	-.011		
	(.014)	(.027)	(.099)	(.009)		
T3	-.014	-.007	-.11	.003		
	(.016)	(.029)	(.099)	(.009)		
T4	-.001	-.032	-.123	.007		
	(.018)	(.031)	(.092)	(.011)		
Iterations	1,000	1,000	1,000	1,000	1,000	1,000

Note: $^{\#}$p<0.1; *p<0.05. Estimated ATET is calculated using propensity score matching with four lags and all control variables shown in Table F1.

I.2 Covariate Balance From Different Matching Methods: Democratic Party Ideology

Figure I1: Covariate Balance From Different Matching Methods: Democratic Party Ideology

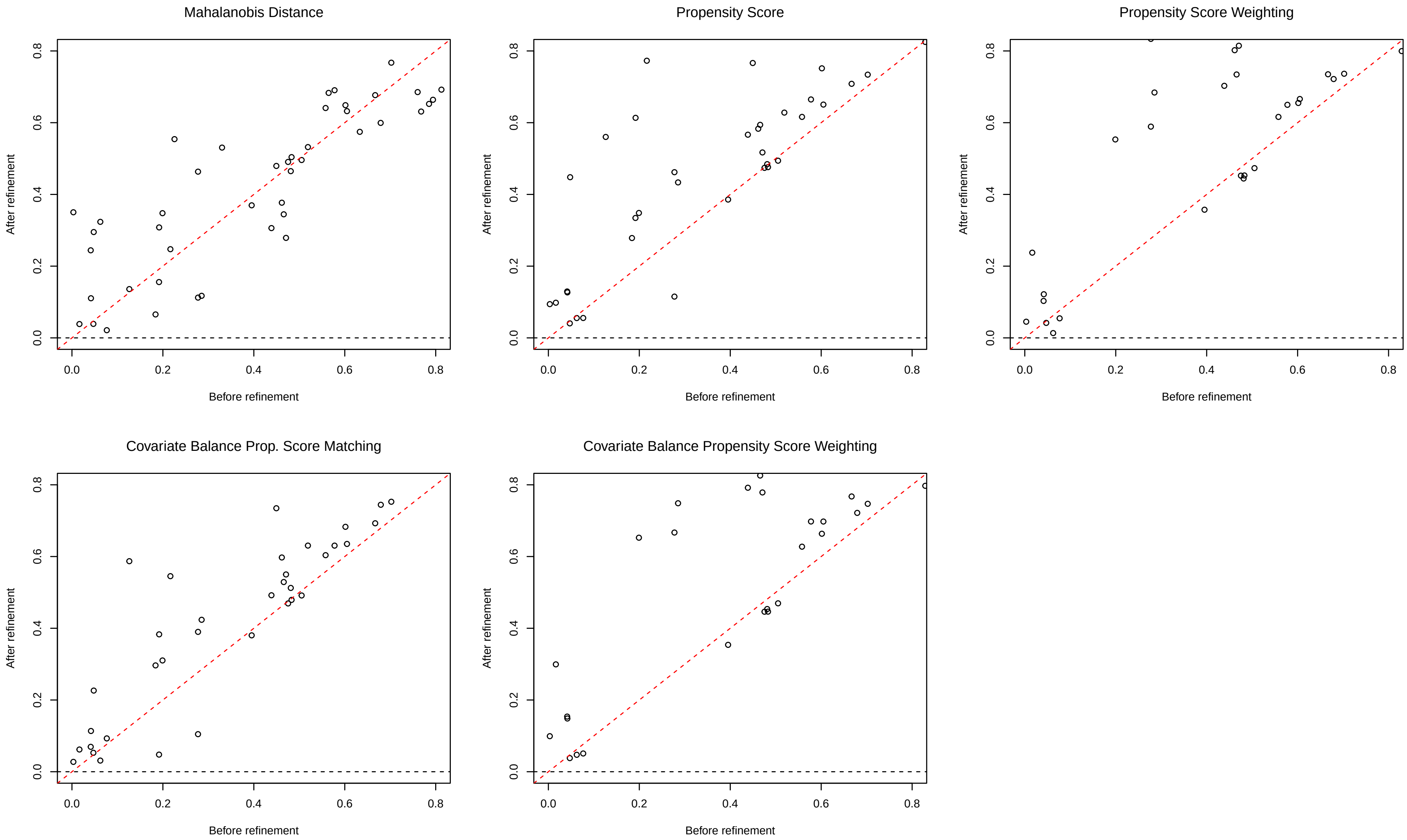


Figure I2: Covariate Balance From Different Matching Methods: Republican Party Ideology

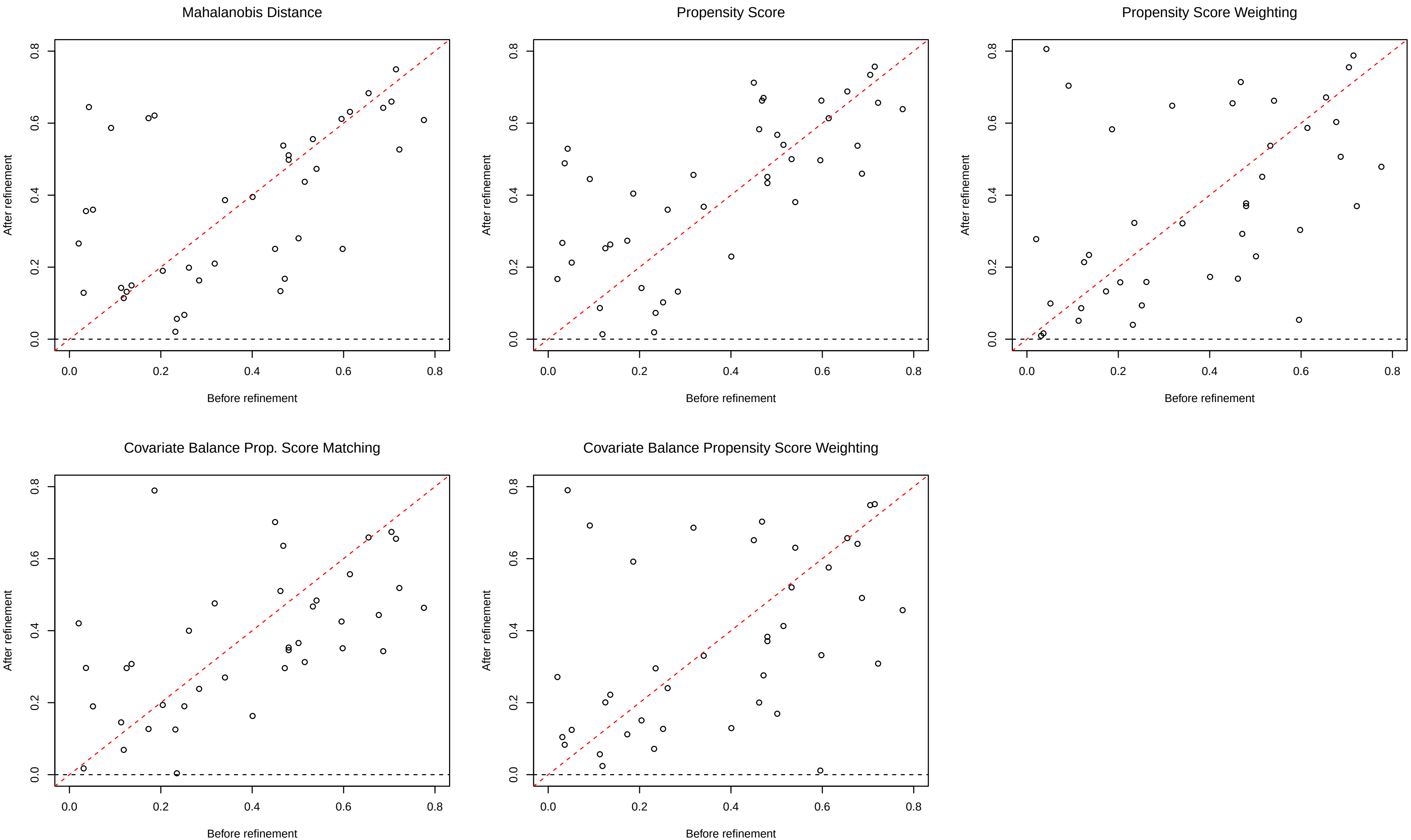


Figure I3: Covariate Balance From Different Matching Methods: Legislative Effectiveness

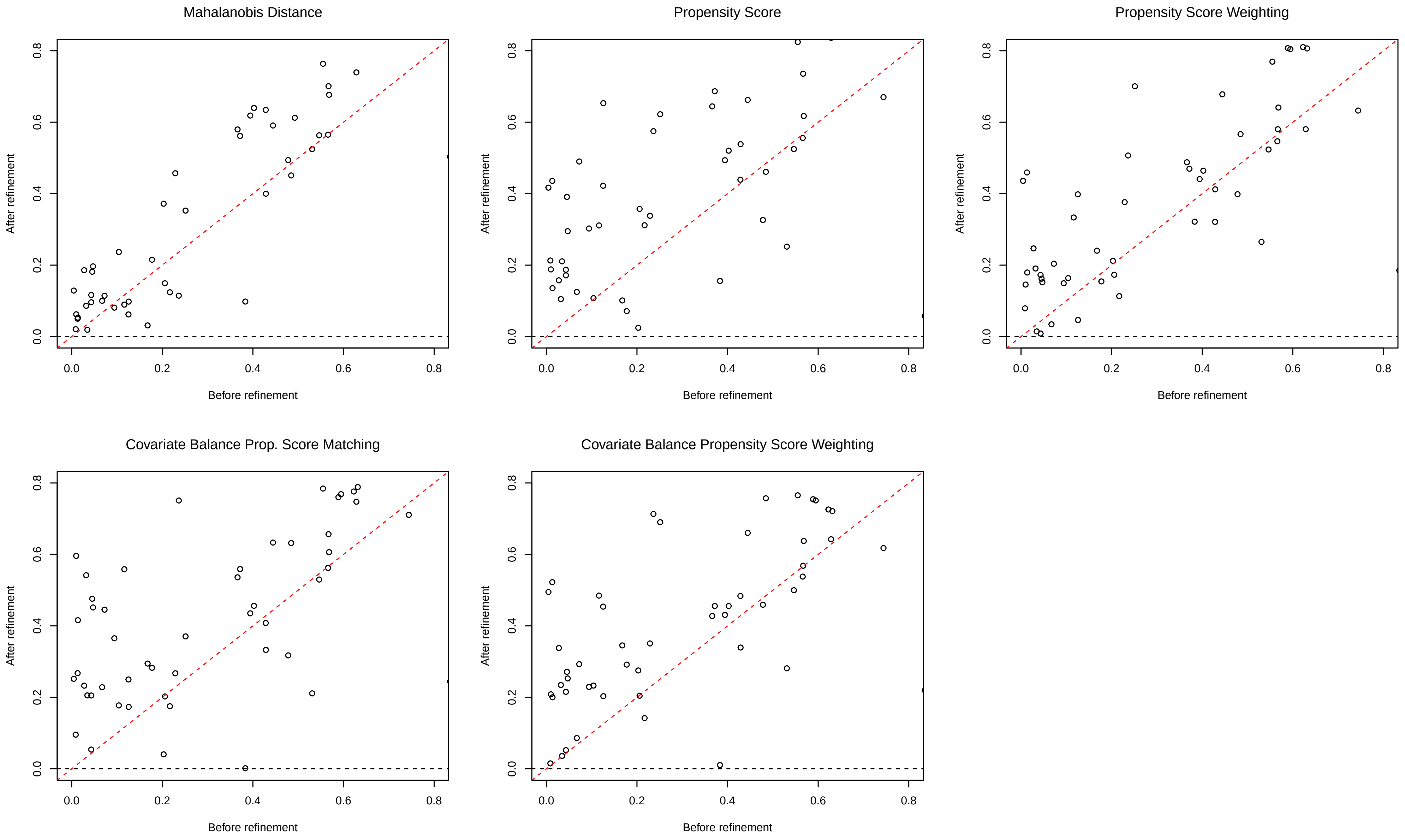
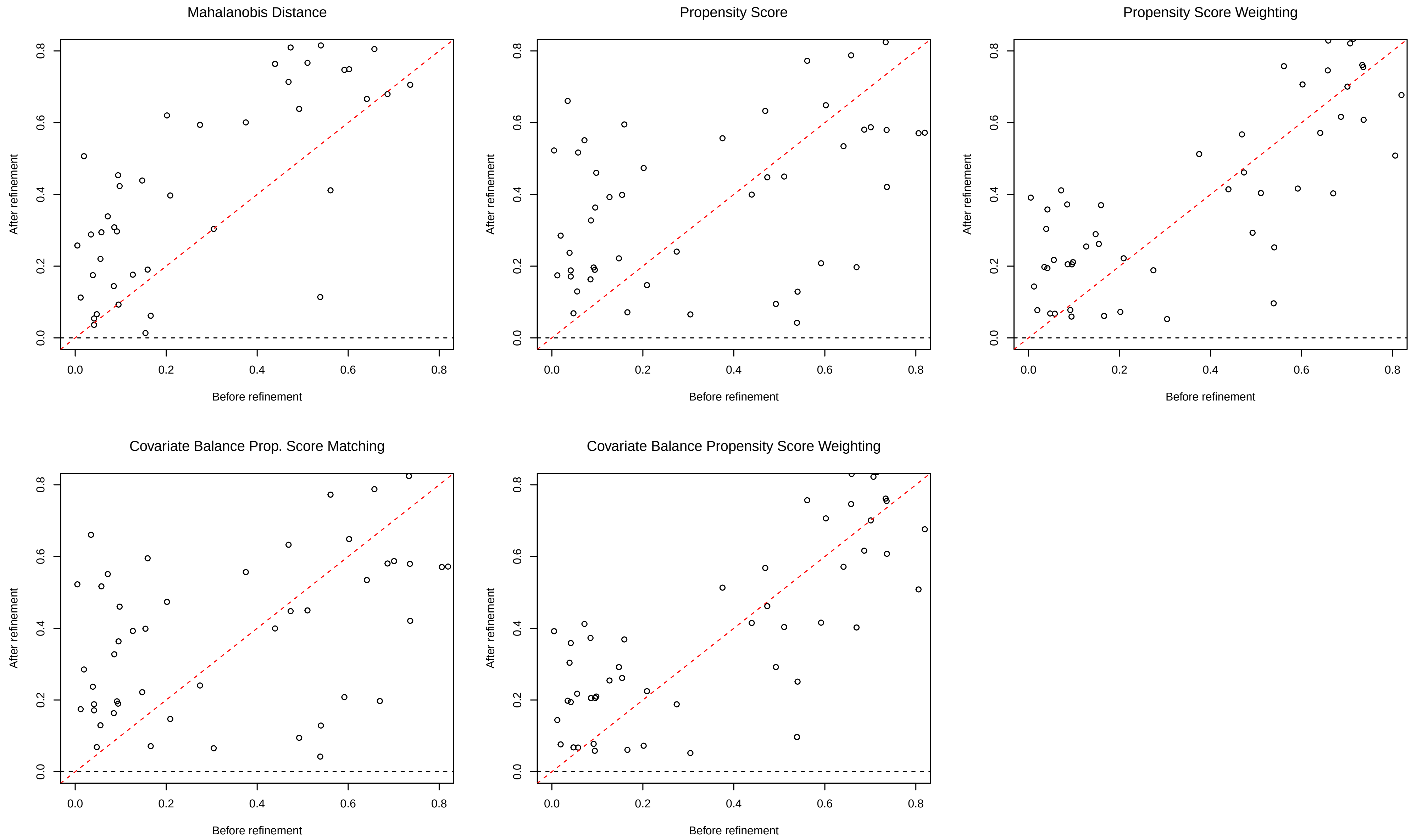
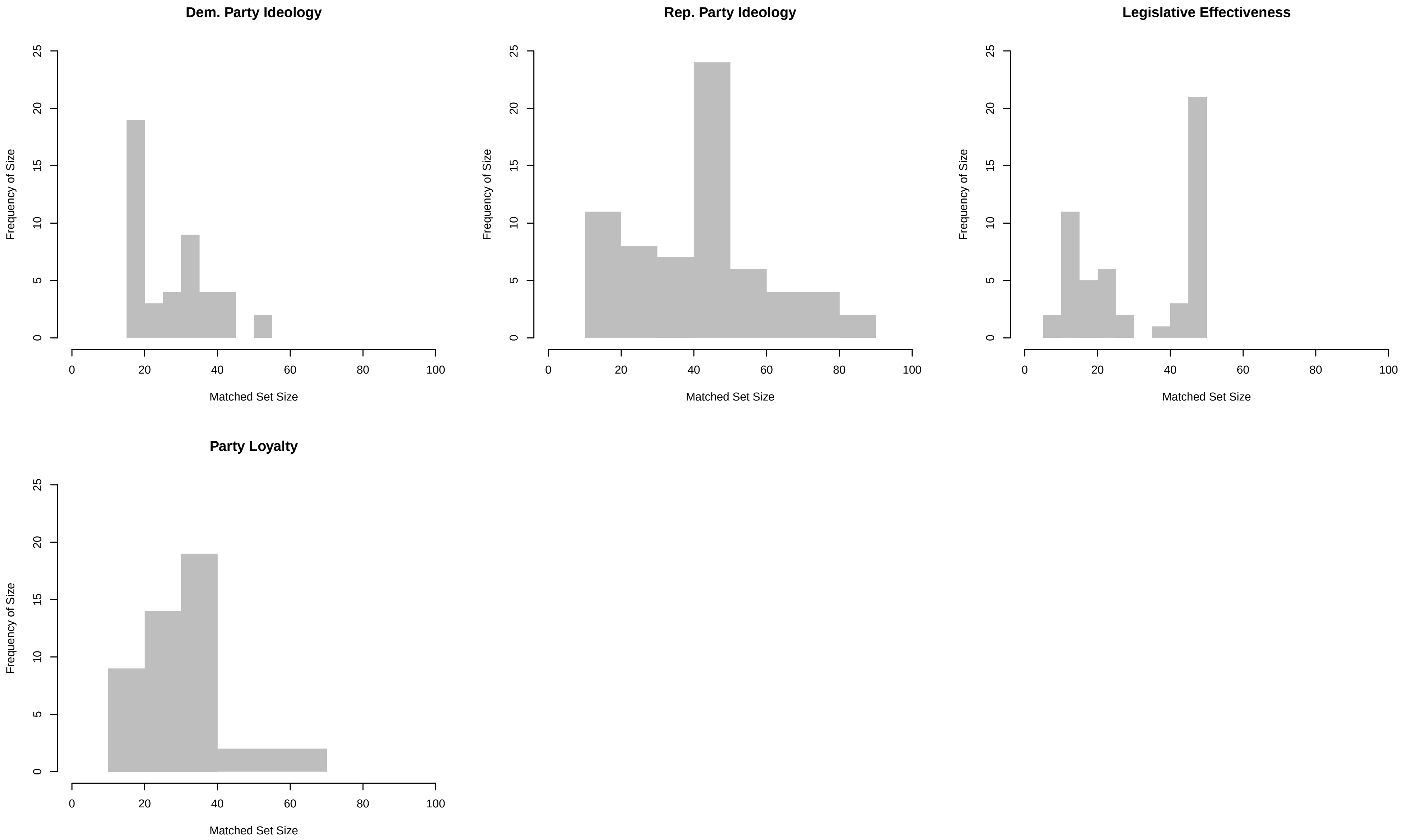


Figure I4: Covariate Balance From Different Matching Methods: Party Loyalty



I.3 Frequency Distribution of Matched Control Units for Legislator- Level Outcomes

Figure I5: Frequency Distribution of Matched Control Units for Legislator-Level Outcomes



Histograms show the number of matched control units that share the same treatment history as a treated for four years prior to the treatment year for each outcome.