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Electoral Reform and Changes in Legislative Behavior: Adoption of the Secret Ballot in Congressional Elections

There is widespread agreement that the Australian ballot fundamentally altered the American electoral system. One common approach to test the effects of ballot reform is to examine legislators elected under the party and secret ballot. An alternative research design, which we adopt here, compares changes in the behavior of legislators who were elected under *both* ballot types. We use this approach to investigate whether ballot reform directly influenced legislators' decisions to seek renomination and their behavior within the institution. Our results raise a number of important implications for understanding the effects of electoral reform on political behavior.

The American political system underwent a remarkable array of changes during the final two decades of the nineteenth century. In response to the numerous excesses associated with the Gilded Age, progressives pushed for a variety of electoral and institutional reforms in an attempt to weaken the party bosses' control over the electorate. Adoption of the Australian (or secret) ballot was one such reform, which had an immediate and lasting impact on the U.S. electoral landscape. Secret ballots first appeared in the 1888 presidential election and were used in approximately 7% of all congressional elections that year. By 1892, over 75% of all congressional races were conducted using the secret ballot, and the number quickly approached 90% in subsequent elections.

Prior to the adoption of the Australian ballot in the late nineteenth century, the political parties, rather than individual states, printed and distributed ballots for voters to use when they went to the polls. These party-controlled ballots provided the parties with considerable influence over access to the ballot (Carson and Roberts 2013). For instance, parties could regulate who participated in the elections as well as monitor who individual citizens were voting for when they showed up at the polls on

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Election Day. This arrangement made voters, candidates, and elected officials more dependent on the parties in a way not found in the contemporary electoral system.

By severing these ties, the Australian ballot has been widely viewed as fundamentally altering both the American electoral system and political institutions (see, e.g., Katz and Sala 1996; Rusk 1970; Ware 2002). In the modern era where all elections are administered by the states, it can be hard to fully appreciate the magnitude of these changes. Indeed, it is important to remember that the Australian ballot was the first in a series of reforms that eventually produced the modern U.S. electoral system. Ballot reforms predate other electoral changes like the direct primary, direct election of U.S. senators, and reporting requirements for federal campaign expenditures by a decade or more. In this way, ballot reforms were a crucial step in modernizing elections and ultimately facilitated the adoption of subsequent electoral reforms (Ware 2002).

As with any institutional change of this magnitude, ballot reforms led to a number of unanticipated outcomes that were difficult, if not impossible, to reverse once they were in place (Pierson 2000). Ware (2002) contends that while the parties were largely supportive of ballot reform, these changes would facilitate the subsequent adoption of direct primaries, an outcome the parties did not anticipate. These unexpected consequences, however, are the reason that the transition to the secret ballot is still relevant well over a century after its adoption. Untangling the relationship between electoral rules and legislative behavior can be valuable for evaluating and adjudicating between different reform proposals or types of electoral rules. Our analysis also has implications beyond the United States given political scientists' broad interest in the electoral implications of ballot design and electoral rules (Reynolds and Steenbergen 2006). By examining the adoption of the Australian ballot in America, we can draw on and speak to studies that examine similar questions in a comparative or country-specific context outside of the United States. In this way, our analysis can add to the larger body of comparative research on the effect of electoral rules.

Our article proceeds as follows. We begin by recounting the background and history of the secret ballot as well as its effects on our electoral system. Next, we discuss the theoretical linkage between ballot reform and changes in legislative behavior. From there, we outline our key hypotheses and propose a research design to test these expectations. The research design, which applies a crossover design from randomized experiments to observational data (Imai et al. 2011), is of note because it provides a more direct test of the effect of ballot reform than do prior studies. We then conduct a variety of empirical analyses and find that legislators were more responsive to the ballot reforms in areas where they had the greatest agency to act. These results can prove useful for countries, states, or interest groups that are designing or proposing new electoral reforms aimed at modifying the behavior of elected officials. We conclude with a summary of the main results and discuss the broader implications for electoral systems more generally.

The Australian Ballot and American Electoral Politics

Prior to the adoption of the Australian ballot by the states around the early 1890s, parties exercised far greater control over elections and the balloting process than they do in elections today (Burnham 1965).[1] Ballots in use at the time did not list candidates from all major parties on a single ballot; rather, each party printed its own ballot that listed its slate of candidates. These party-supplied ballots facilitated voting among illiterate citizens and provided a convenient way for the parties to monitor their own voters as well as their opposition's voters (Ware 2002).[2]

Aside from the physical differences between the party and the secret ballots, the distribution of both ballots was markedly different as well. Party ballots were given to voters either in advance of the election itself or outside of the polling place on Election Day. As such, party bosses or "henchmen" often used a variety of tactics to persuade voters to support their slate of candidates (Reynolds 2006; Summers 2004). Once states adopted the secret ballot, however, ballots were printed and distributed by the state and were "available only at the place of balloting and at the time of voting, and a ballot paper could not legally be removed from the balloting place" (Ware 2002, 31–32). In addition to a standardized ballot, these reforms instituted a set of new legally binding rules to govern the electoral process.

Although these reforms had numerous consequences for electoral politics, one of our primary interests relates to how they impacted the cost of running for office. Under the party ballot, the political parties covered much of the actual electioneering costs. One of the most significant costs shouldered by the parties was the acquisition of votes. With the party ballot, voters were not given a ballot that listed all candidates for elected office, but rather one that included only one party's slate of candidates. When voters went to the polls, they would simply submit their party ballot to the election officials, thereby supporting the party's entire slate of candidates (Ware 2002). Under this system, then, the ballot was effectively a collective good for all of the party's candidates, and they would benefit proportional to the overall quality of the party's ticket.

After the adoption of the Australian ballot, however, these costs were shifted largely to the candidates themselves (Carson and Roberts 2013).

Once the states took over the responsibility of printing electoral ballots for each election, the parties' ability to enforce straight ticket voting was curtailed. Rusk notes that by placing "both major parties on the same ballot and guaranteeing a secret vote," the Australian ballot "allowed and encouraged the expression of cross-party preferences in the polling booth" (1970, 1235). The rise of split-ticket voting highlights how the Australian ballot changed the principal-agent dynamic in congressional elections (Gailmard and Jenkins 2009). Prior to the Australian ballot, members of Congress were primarily agents of their political party. The party organizations not only controlled access to the ballot, but also marshaled the resources and voters needed to run a successful campaign. After adoption of the Australian ballot, candidates became more directly responsible for securing their own votes.

We should note that the type of secret ballot a state adopted conditioned the magnitude of these changes. States adopted one of two types of secret ballots, either the party column or the office bloc. Party-column ballots, while still printed by the state and cast in private, essentially mirrored the form of the old party ballot. As Rusk notes, the party column ballot "resembled a consolidation of the old party strips, placed side by side on the same sheet of paper" (1970, 1221). Office bloc ballots, meanwhile, organized the ballot by the elected office candidates sought. In some states, the office bloc ballots omitted the party identification of candidates altogether (Rusk 1970), but other states included a "party box" that allowed voters to still cast a straight ticket vote (Ware 2002). The two ballot types did have differential impacts within the electoral system. Not surprisingly, the office bloc ballot had a sizable impact on election outcomes, but the party column still led to significant changes (Engstrom and Kernell 2005; Rusk 1970).

Ballot Reform and Legislative Behavior

A major implication of these reforms is that voters, and not political elites, became members of Congress' primary political principals (Gailmard and Jenkins 2009). Given the lower cost of defection with the secret ballot (Rusk 1970), voters could more easily express their dissatisfaction with individual members of Congress when they went to the polls. Perhaps more importantly, the *threat* of defection, which could condition members' legislative behavior, was now a more credible and powerful tool for these newly empowered principals. Given the change in the principal-agent relationship, it is only natural to expect that legislators, particularly those interested in reelection (Carson and Jenkins 2011; Mayhew 1974), should change their behavior in predictable and quantifiable ways.

Katz and Sala (1996) were among the first to examine the relationship between adoption of the secret ballot and legislative behavior. Katz and Sala maintain that ballot reform made the cultivation of a personal reputation with voters more important for reelection-minded legislators. As a result, credit claiming and position taking became more valuable for incumbents who had to cultivate their own electoral support. Indeed, committee membership is one legislative activity that allows members to build a reputation that may yield benefits with constituents (Fenno 1973). Katz and Sala focus their theoretical argument and subsequent empirical analysis on how ballot reforms should influence committee assignment behavior. Specifically, they posit:

> The ballot changes raised the interest of members of congress in institutional arrangements that would help them build reputations. Stable committee assignments give members the leeway and confidence they need to become policy experts within their committee's jurisdiction. Policy experts are better equipped to claim credit. . . . Hence, a "norm" of reappointing incumbents to their same committees would be consistent with a widespread desire for building personal reputations. (1996, 23)

In order to test this proposition, Katz and Sala examine individual representatives' committee tenure from 1874 to 1928. They find that the adoption of the Australian ballot significantly increased the likelihood that a legislator would remain on a given committee once appointed.

Wittrock et al. (2008) both reevaluate and expand upon Katz and Sala's (1996) earlier work by examining a broader range of legislative behavior. First, they change the focus on committee assignments from tenure to the overall "value" of a member's committee portfolio. As a result, Wittrock et al. find that members elected under the office bloc, but not the party-column ballot, sought more desirable committee assignments. Second, they find that members elected under both types of secret ballot received more in pork barrel expenditures, but only those elected under the office bloc received more pork barrel projects. Lastly, Wittrock et al. conclude that only the office bloc ballot led to lower levels of party-unity voting. They note that the effect was relatively small, which they attribute to important institutional reforms during this period—such as the adoption of Reed's rules—that had countervailing effects.

In sum, prior studies suggest that House members engaged in more electorally beneficial activities during the postreform period. Although there is an intuitive appeal to these findings, it is important to consider whether these reforms had a direct and independent effect on legislative behavior. That is to say, we need to examine the extent to which reforms changed the behavior of legislators who continued to serve after their adoption. In order to do this, we employ a different measurement strategy than the one used in prior research, a point we address below.

Theoretical Expectations

Much of the prior research on the effect of electoral institutions focuses on changes in legislators' behavior within the institution.[3] Although our analysis examines changes in legislative behavior, we also wish to test whether ballot reforms directly influenced legislator's decisions about whether or not to seek another term in office. The party ballot served as a collective good for a party's entire slate of candidates, which shifted electioneering costs to the party. Under the secret ballot, however, these costs were transferred to the candidates themselves (Carson and Roberts 2013). Changes in the allocation of costs are the main theoretical reason to expect ballot reforms to condition members' electoral calculations. As more of the costs shifted to the candidates, we would expect them to update their decision calculus accordingly. Our argument draws directly from prior empirical and theoretical research (Jacobson and Kernell 1983; Maestas et al. 2006; Rohde 1979), which demonstrates that costs—or at least the perceptions of costs—are an important component of candidates' decision making.

The impact of these reforms should not be distributed uniformly among all members. We expect incumbents residing in states where the office bloc ballot would be used in the subsequent election to be less likely to seek renomination. In comparison to the party-column ballot, the office bloc ballot's organizational structure placed more emphasis on an elected office rather than party affiliation. Furthermore, in some states such as Massachusetts, candidates' partisan affiliation was not included on the ballot (Rusk 1970). These changes undermined the informational shortcuts that made the party ballot beneficial to both candidates and voters alike. We do not expect to find an effect for members whose state was to adopt the party-column ballot in the subsequent election. In these states, voters could still easily choose between parties rather than having to choose between candidates for each different office.

In addition to electoral behavior, we are also interested in two types of legislative behavior—party-unity voting and committee assignments—examined in prior studies. The relationship between party unity and changes in electoral institutions has been analyzed in the U.S. Congress (Meinke 2008; Wittrock et al. 2008), legislatures in other countries (Coman 2012; Olivella and Tavits 2014), and in comparative studies (Carey 2007). In the context of ballot reform, prior research suggests that adoption of the Australian ballot led to a decrease in party-unity voting. Wittrock et al. (2008) found that legislators elected via the office bloc ballot voted less frequently with their party than legislators elected via the party ballot, but they found no effect for the party-column ballot. We adopt these findings as our baseline expectation about the relationship between ballot reforms and party-unity voting.

Prior research on the secret ballot generally agrees that these reforms led to changes in the way committee assignments were awarded to legislators (Katz and Sala 1996; Wittrock et al. 2008). Katz and Sala (1996) argue that committee transfers should be less likely after the adoption of the secret ballot. Wittrock et al. (2008) take a slightly different approach and focus on the "value" of committee assignments. They posit that legislators elected under the office bloc, but not the party-column ballot, should seek more "valuable" committee assignments. We adopt these findings as the baseline expectations about the relationship between ballot reforms and committee-assignment politics.

Research Design

One of the most common approaches for quantifying the effects of electoral reforms is to examine legislative behavior before and after the adoption of new electoral institutions (see, e.g., Bernhard and Sala 2006; Gailmard and Jenkins 2009; Meinke 2008). Under this research design, the electoral reform is thought of as a treatment in a quasi-experiment where the goal is to "use before-and-after comparison—untreated observations compared to treated observations—to assess the treatment effect . . ." (Gailmard and Jenkins 2009, 330). Prior studies have generally operationalized the pre- and postcomparison either through interaction terms (Bernhard and Sala 2006; Gailmard and Jenkins 2009) or by estimating separate models before and after the reform of interest (Meinke 2008). In the former case, a comparison of the coefficient estimate for the constitutive and interactive terms provides evidence of whether or not the reform conditions legislative behavior. In the latter case, the conditioning effect can be discerned by comparing the coefficient estimates in the pre- and postmodels.

Our measurement strategy differs from prior studies in terms of how we operationalize the pre- and postreform measures of legislative behavior. Instead of comparing the behavior of *all* legislators before and after a specific reform, we compare the behavior of the *same* legislators across consecutive elections or congresses. Our unit of analysis is a legislator's behavior in the congress at time t and the *same* legislator's behavior in the subsequent congress at time $t + 1$. Each observation therefore measures the difference in legislative behavior across two consecutive congresses. In cases where a legislator served in two or more consecutive terms, the dataset will include multiple observations for the legislator. If, for example, a legislator served in three congresses then he would be included in the dataset twice. The first observation would measure changes in the legislator's behavior across the first and second congresses while the second observation would be for changes across the second and third congresses.[4]

We can test for the influence of ballot reform through the inclusion of measures that account for whether or not the type of ballot changed between congresses.[5] Our approach can be thought of as applying a crossover design for randomized experiments to observational data (Imai et al. 2011). In a randomized experiment, a crossover design is one where "each experimental unit is exposed to both treatment and control conditions sequentially" (Imai, Tingley, and Yamamoto 2013, 16). The observational analogue conducts sequential comparison of units who change between treatment and control (Imai et al. 2011, 782). In our case, then, such a design allows us to test whether or not electoral system change *directly* influences legislative behavior (Olivella and Tavits 2014).

The measurement strategy we propose is a significant departure from prior research on ballot reform. Previous studies use legislators elected under party ballots as controls for those elected under the Australian ballot in order to make comparisons across these two groups (Katz and Sala 1996; Wittrock et al. 2008). The estimated effect of ballot reform under this research design amounts to a comparison of the behavior of these two groups. One concern with this alternative design is the extent to which these two groups are properly comparable. If there are systematic differences between these two groups, referred to as imbalance in the causal inference literature (Rubin 2006), the resulting estimates will not fully capture the treatment's causal effect. Our research design overcomes these issues in part by comparing changes in behavior of individuals as opposed to behavior across groups that may or may not be comparable. Our empirical analysis of ballot reform therefore captures the direct effect since we measure changes in individual legislator behavior.

Data Analysis

Before moving to our empirical analyses, it is important to briefly discuss our selection of the appropriate sample of legislators. Prior empirical work employs data from a relatively broad timeframe. For instance, Wittrock et al. (2008) examine the period from 1884 to 1898, while Katz and Sala (1996) use data from 1874 to 1928. Since our interest involves comparing changes in individual legislators before and after the adoption of ballot reforms, our analysis cannot start any earlier than 1886 since the Australian ballot was first used in the 1888 election. As such, defining a start point for our analysis is relatively straightforward.

The more critical task, however, is to define the appropriate end point. Since we want to determine the unique effect of ballot reform, our decision about an end point must take into account other changes that could possibly confound our estimates. One obvious factor is the adoption of direct primaries, particularly since the establishment of direct primaries during the early part of the twentieth century is hypothesized to have an effect on legislative behavior that is directionally similar to ballot reforms. We therefore want to confine our analysis to a period of time in which ballot reforms were ongoing, but prior to the adoption of the direct primary.

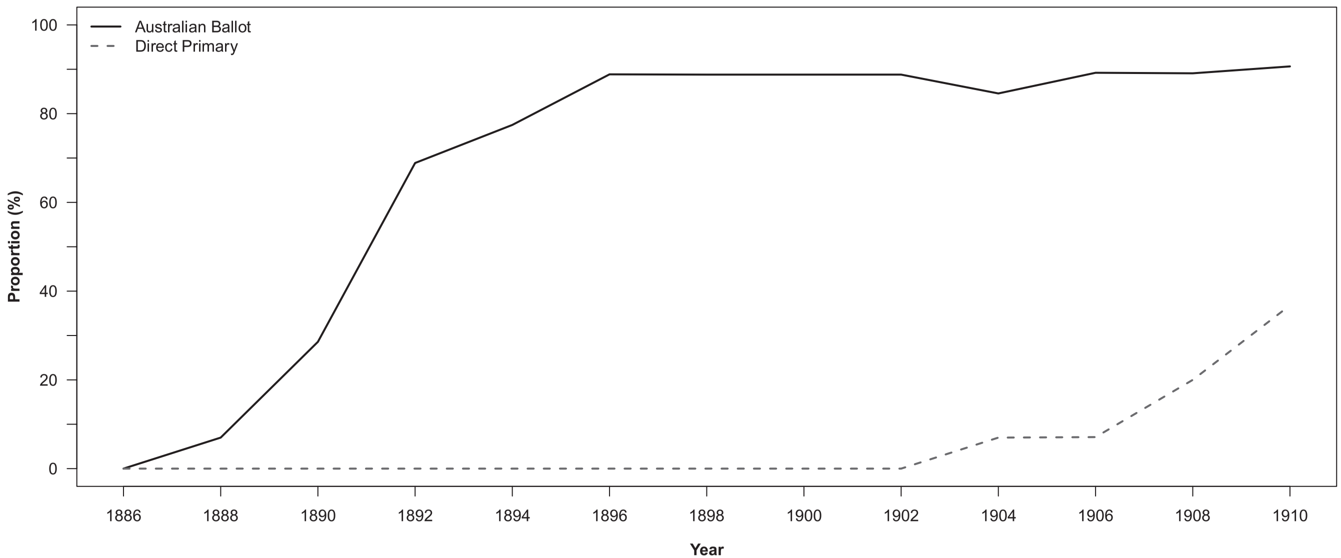
Figure 1 presents the proportion of states that have adopted either the Australian ballot or the direct primary. From 1888 to 1902, no states used a direct primary, but nearly all states had switched to some form of the Australian ballot by the mid to late 1890s. After 1896, it is important to note that there is effectively no change in the proportion of states using the secret ballot. Since our focus is on changes in individual legislator's behavior across two time points, it is imperative that there be a reasonable number of actual changes. Furthermore, after 1896, there are no states that change from the party ballot to the secret ballot until 1906, which is after states start to adopt the direct primary. We therefore use 1896 as our end point since there are only a handful of changes that occur after this time.[6]

The remainder of this section is organized as follows. We begin with an analysis of electoral behavior, specifically whether or not ballot reforms impacted legislators' decisions to seek renomination. We then focus on two aspects of legislative behavior. First, we examine the extent to which Australian ballot reform affected a legislator's proclivity to toe the party line on party-splitting votes. Second, we assess the relationship between ballot reforms and committee-assignment politics to determine if the value of committee portfolios changed following passage of the secret ballot.

Renomination

We used data from ICPSR and McKibbin (1997) and the *Biographical Directory of the United States Congress* to determine whether

FIGURE 1
Proportion of States with Secret Ballot or Direct Primary, 1888–1910



Note: The solid black line denotes the proportion of congressional districts that used a version of the Australian ballot in a given election year. The dashed line denotes the proportion of congressional districts where a direct primary was held in a given election year.

or not a legislator sought renomination to the next Congress. The population of legislators seeking renomination, which we code as a 1, includes all those individuals who won reelection as well as those who were denied renomination by their party or lost in the general election. The set of legislators not seeking renomination, which we code as 0, include those who retired, accepted another position, or sought a different office.[7] Members who died in office or resigned during the previous session, of which there were 35 and 8 cases respectively, are not included.[8]

Our measures of ballot reform are coded as follows. If an incumbent's state adopted the office bloc ballot for the election to the next Congress (e.g., the 51st Congress for all members who served in the 50th Congress), then the predictor "change to office bloc" is coded 1, and all other cases are coded as a 0. We used the same coding procedure for change to party column, which means the no-change category is our reference group.

We expect a member's prior electoral contest to condition the extent to which the impending electoral reform influences the decision to pursue another term. In particular, incumbents from districts with a partisan recruitment advantage should be less affected than those where no such advantage exists. By partisan recruitment advantage, we mean whether or not the incumbent faced a quality challenger in the previous election (Jacobson 1989). Partisan recruitment advantage is coded 1 in all cases where the incumbent did not face a quality challenger in the previous election and 0 when they did.

The logic here is relatively straightforward. Even under the party ballot, quality candidates outperformed their less experienced counterparts (Carson, Engstrom, and Roberts 2007). Although we make no claims about how ballot reform might influence these quality effects, the change does mean that incumbents could no longer rely as heavily on the party's collective reputation to fend off experienced challengers. We therefore expect that the candidates who were most likely to face a quality challenger in the next election, namely those who did so in the last election, will perceive the impending electoral changes as more costly than those who had a lower expectation of facing a quality challenger. Given the conditional nature of this expectation, we include an interaction between changes in ballot type and our indicator for recruitment advantage. As before, we expect to find this conditional effect in cases where the office bloc was to be adopted, but we expect a weaker or insignificant effect in cases where the party-column ballot was to be adopted.

Beyond our key predictors, we also include a number of important control variables. First, we include an indicator variable to account for redistricting since it has been shown to influence candidate decision making (Cox and Katz 2002; Engstrom 2013). As with the measure of ballot changes, our redistricting variable accounts for whether or not the next election would be one in which district lines were redrawn. We use data from Engstrom and Kernell (2005) and Parsons, Dubin, and Parsons (1990) to determine when redistricting occurred.[9] Second, we control for a member's age under the expectation that older members might be less likely to seek renomination. Third, we control for a legislator's tenure in office (i.e., length of service), which should be negatively, but albeit far from perfectly,[10] related to the likelihood of seeking renomination. We include the logged value of tenure since we expect there to be larger differences between those who have served relatively few terms (e.g., one or two terms) versus those who have served for long periods of time. Third, we use a member's roll-call participation rate, which is a proportion (0–100) calculated as a legislator's number of votes cast over the total number of votes for which the legislator was a member, in the current Congress to control for his or her level of legislative activity. This is necessary since less active members might be disinclined to seek a return trip to Congress. Fourth, we control for a member's margin of victory in the election to the current Congress, which should be positively related to the propensity to seek renomination. We obtained data on electoral returns and margin of victory from Dubin (1998). Lastly, we control for a member's party unity in the current Congress, which could be an important factor in a period where some congresses were characterized by relatively high levels of partisan voting (Brady and Althoff 1974).

We estimate a series of logit regression models to test out expectations about the relationship between ballot reform and legislators' decision to seek renomination. Our first model examines the unconditional effect of ballot reform, which is to say the ballot reform variables are not interacted with the recruitment advantage measure. Our second model includes these interaction terms to test our expectation of a conditional relationship. The final model adds in the series of control variables. The estimates from both models are reported in Table 1.

The coefficient estimates in Model 1 support our expectation that changing to the office bloc ballot should decrease the likelihood of seeking renomination, but estimates for the adoption of the party-column ballot have an unexpected positive and statistically discernible effect. In Model 2, the significant interaction term for Change to Office Bloc, but not for change to party column, supports our second expectation that the relationship between ballot reform and the likelihood of seeking renomination is moderated by prior electoral conditions. In this model, the

TABLE 1
Probability of Seeking Renomination, 50th–54th Congresses
(standard errors in parentheses)

Variable	Estimate	Estimate	Estimate
Change to Office Bloc$_{t+1}$	–0.402*	–0.950*	–1.005*
	(0.215)	(0.321)	(0.308)
Change to Party Column$_{t+1}$	0.544*	0.443	0.473
	(0.217)	(0.316)	(0.321)
Party Recruitment Advantage$_t$		–0.038	–0.079
		(0.129)	(0.139)
Change to Office Bloc$_{t+1}$ ×		0.962*	0.946*
Party Recruitment Advantage		(0.439)	(0.428)
Change to Party Column$_{t+1}$ ×		0.211	0.334
Party Recruitment Advantage		(0.437)	(0.438)
Redistricting$_{t+1}$			0.097
			(0.175)
Tenure (logged)			–0.139
			(0.098)
Age			–0.029*
			(0.007)
Roll-Call Participation			0.016*
			(0.003)
Margin of Victory			0.009*
			(0.003)
Party Unity$_t$			0.006
			(0.006)
Constant	1.203*	1.224*	1.175*
	(0.064)	(0.093)	(0.590)
N	1655	1655	1655
AIC	1768.4	1769.2	1728.0

Note: Cell entries are estimates from logit regression models and standard errors clustered by legislator are reported in parentheses. The outcome variable is whether or not a legislator sought renomination to the next Congress.
*$p < 0.05$ one-tailed test.

coefficient for Change to Office Bloc, which is negative and statistically significant, represents the predicted effect when there was no recruitment advantage.

The conditional relationship continues to hold in Model 3 even after including a series of covariates representing alternative explanations that could influence an incumbent's decision about whether to seek another term in office. The estimated effect for when there was a recruitment advantage is negative (−0.059) but not statistically discernible from zero.[11] Substantively this means that incumbents who faced a quality

TABLE 2
Change in Predicted Probability of Seeking Renomination

Counterfactual	Δ Pred. Prob.	IQR (25th–75th)
Change to Office Bloc	–0.100	[–0.210, –0.010]
No Recruitment Advantage	–0.211	[–0.234, –0.193]
Recruitment Advantage	–0.010	[–0.012, –0.009]
Change to Party Column	0.064	[0.051, 0.076]

Note: Cell entries in column 2 are the mean change in probability for the given counterfactual and entries in column 3 are the interquartile range (IQR) for the predicted change in probability.

challenger in their last election *and* whose state adopted the office bloc ballot for the next election were less likely to seek renomination. The change in ballot structure had no effect for incumbents who faced an inexperienced challenger in their last election. In Model 3, the control variables all perform largely as expected. The exceptions are redistricting and tenure, which appear to have no discernible effect on renomination during this time period.

Since the regression coefficients are not directly interpretable, we use the estimates from Model 3 to calculate a set of predicted probabilities to highlight the substantive implications of these model estimates. In order to accomplish this, we calculated two sets of predicted probabilities. The first predicted probability is calculated by setting all cases to their observed values, except for the change in ballot type, which is set to zero. The second predicted probability is calculated in the same manner, but we change the ballot-type variable to one. We then use the mean change in predicted probability and the interquartile range to summarize these counterfactuals (Hanmer and Ozan Kalkan 2013). Since prior electoral conditions are expected to moderate the likelihood of seeking renomination for the change to office bloc category, we subset each group further for the office bloc counterfactual depending on whether the incumbent enjoyed a recruitment advantage in the last election.

We report the predicted probabilities for each counterfactual in Table 2. If we leave party recruitment at its observed value for each legislator, the change to the office bloc ballot is estimated to decrease the probability of seeking renomination by 10 percentage points. The change to office bloc for candidates who did not have a recruitment advantage is predicted to decrease the probability of seeking renomination by 21 percentage points. For candidates who had a recruitment advantage, however, adoption of the office bloc ballot is predicted to have no effect. In contrast, the estimates for the change to party column are always positive and around 6 percentage points. In short, the subset of incumbents who could expect to see the most sizable increase in the cost of running for another term in office were the most likely to decline renomination.

Party Unity

Although our expectations about the relationship between ballot reform and party unity are the same as prior studies, our measurement strategy examines whether ballot reforms led to a direct change in member's voting behavior. To test for a direct effect, we operationalize our dependent variable as the change in a legislator's party-unity score over consecutive congresses.[12] In addition to a direct effect, we also expect the effect of the office bloc ballot to be moderated by a member's roll-call participation rate. Specifically, a higher rate of roll-call participation should lead to a larger decrease in party unity. Conversely, we expect to find little to no difference for legislators whose state adopted the party-column ballot.

The logic behind the latter expectation is based on the assumption that legislators whose state adopted the office bloc ballot will be less certain about their constituent's preferences. As a result, these legislators will find it more difficult to always identify the "correct" position on roll-call votes. One response would be to strategically abstain in order to minimize the chance of being punished for being on the "wrong side" on a particular vote (Jones 2003). Alternatively, legislators might vote against their party in order to develop a reputation independent of their party. Under the former strategy, legislators' party-unity scores should change little, if at all, because they would simply abstain rather than vote against their party. Conversely, legislators who adopt the latter strategy of defection would vote at normal rates, but they would build a record of bucking the party's position when they deemed it necessary.

Our key predictors, change in ballot type and roll-call participation, are measured in the same manner as in the renomination model. In addition to these measures, we also include a series of controls. First, we control for party recruitment advantage with the expectation that legislators who faced a quality challenger should vote less frequently with their party in the next congress. Second, we include the legislator's margin of victory, which we expect to be positively related to changes in party unity.

We test our expectations with a series of ordinary least squares (OLS) regression models. We include fixed effects by congress, with the 51st Congress treated as the reference category, in each model. The

TABLE 3
Change in Party-Unity Scores, 51st to 55th Congresses
(standard errors in parentheses)

Variable	Estimate	Estimate	Estimate
Change to Office Bloc	–2.084*	4.909	4.660
	(1.192)	(4.795)	(3.331)
Change to Party Column	0.805	–4.437	–4.156
	(1.017)	(2.540)	(2.819)
Roll-Call Participation		0.023	0.022
	(0.018)	(0.020)	
Change to Office Bloc × roll-Call Participation		–0.119*	–0.113*
		(0.080)	(0.058)
Change to Party Column × Roll-Call Participation		0.093*	0.089*
		(0.041)	(0.041)
Party Recruitment Advantage			–1.081*
			(0.649)
Margin of Victory			0.018
			(0.013)
Constant	5.604*	4.237*	4.582*
	(0.553)	(1.278)	(1.507)
N	934	934	934
R^2	0.384	0.393	0.396

Note: Cell entries are estimates of an OLS model with congressional-level fixed effects, which are not reported here, with robust standard errors in parentheses. The outcome variable is the change in a legislator's party-unity score across consecutive congresses. *$p < 0.05$ one-tailed test.

adoption of the Australian ballot occurred at the same time as the House empowered the Speaker through a series of reforms commonly known as Reed's Rules (Cox and McCubbins 2005). More important for the current discussion, Reed's Rules were not in place during the entire time period under consideration (Carson, Lynch, and Madonna 2011), and these rules changes will have a systematic effect on party unity for which we wish to control. These House reforms make it necessary to control for congress-specific differences, which if ignored, can result in biased coefficient estimates (Gujarati and Porter 2009).[13]

Our first model examines the unconditional effect of ballot reform, and the second model includes interaction terms to test our expectation of a conditional relationship. Since the interaction includes both a dichotomous and continuous variable, interpretation of the resulting coefficient estimates is somewhat more complicated, which we address more fully below. Our third model includes each of the control variables discussed above. The estimates from each model are reported in Table 3.

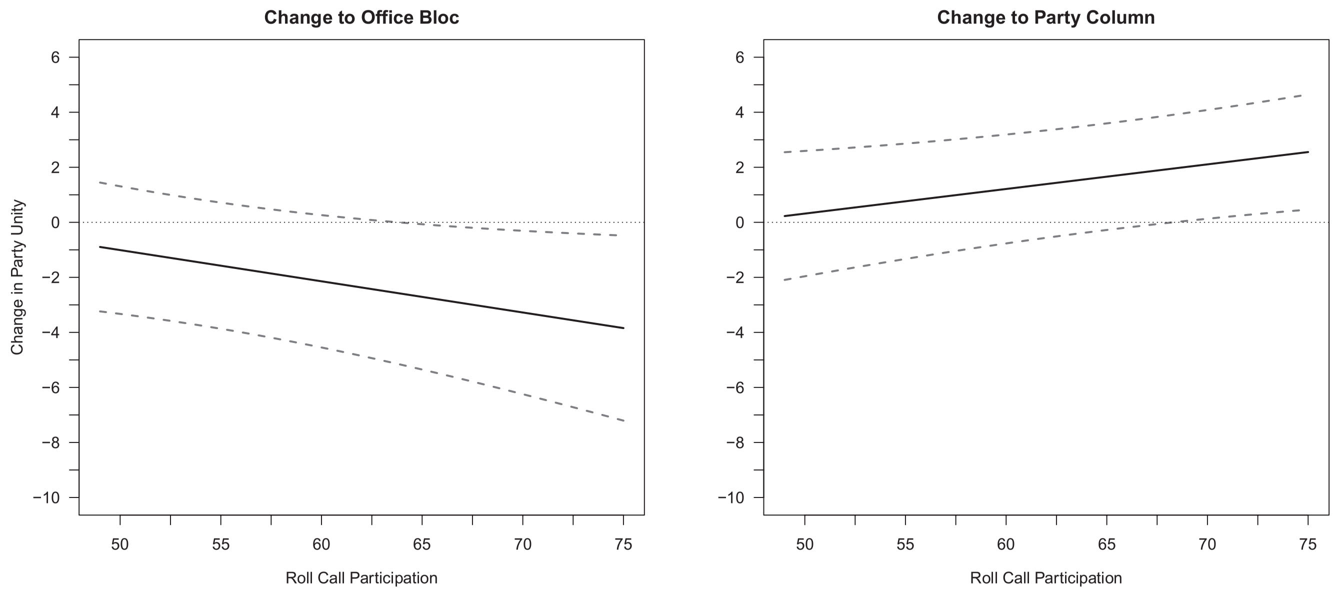
The coefficient estimates in Model 1 comport with the expectations of prior research. A legislator whose state adopted the office bloc ballot is predicted to decrease his party unity by approximately 2 percentage points. Conversely, the coefficient estimate for change to the party column is not discernible from zero. Interpreting the interaction terms from Models 2 and 3 requires some additional care since one of the covariates is a continuous measure. The coefficients for Change to Office Bloc and Change to Party Column are for a legislator with a roll-call participation rate of zero, which makes them somewhat uninformative since that cannot occur for a legislator with a party-unity score. What matters for our purposes is determining the effect of ballot change over levels of roll-call participation. Figure 2 reports the estimated conditional effect and corresponding 95% confidence interval over the interquartile range of roll-call participation based on the coefficient estimates from Model 3.

Overall, Figure 2 provides support for our theoretical expectations. For legislators whose state changed to the office bloc ballot, the effect is negative but it is not discernible from zero until roll-call participation rate reaches approximately 64%. However, as these legislators participate more, they are predicted to vote against their party more frequently than they did in the previous congress. Among legislators whose state adopted the office bloc ballot, participating in 75% of roll calls is expected to lead to a 3.8 percentage point reduction in party unity, holding all else constant. This finding is consistent with evidence from the contemporary era that suggests representatives may be punished electorally for voting too often with their parties (Carson et al. 2010). Meanwhile, the estimated effect for adoption of the party-column ballot is positive, but it is not significant until a roll-call participation rate of approximately 69%.

Committee Assignments

Given the research design issues with previous studies of ballot reform, it is useful to begin with a review of how these studies test for the effect of ballot type on committee assignments. Katz and Sala (1996) use an indicator, coded at the congress-level, to account for the effect of ballot reform. The use of a simple indicator variable is problematic because it not only ignores differences between the types of Australian ballot, but it also fails to account for when a legislator's ballot type changed. Wittrock et al. (2008) use legislators elected under the party ballot as controls for legislators elected under either form of the Australian ballot. Although this accounts for potential differences between ballot types, and thus is an improvement over the approach utilized by

FIGURE 2
Effect of Change in Ballot Type over Levels of Lagged Party Unity



Note: The left panel shows the marginal effect (solid line) for the Office Bloc and Roll Call Participation interaction term with corresponding 95% confidence intervals (dashed lines). The right panel shows the marginal effect (solid line) for the Party Column and Roll Call Participation interaction term with corresponding 95% confidence intervals (dashed lines).

Katz and Sala, it does not test for a direct effect of ballot reform on committee-assignment politics. Our measurement strategy of comparing changes in behavior of the same legislator across consecutive congresses does, however, provide a test for a direct effect of ballot reform on committee-assignment politics.[14]

We use two measures to account for changes in committee-assignment politics. Our first outcome variable is dichotomous and is coded 1 if a legislator received a new committee assignment and 0 if he did not. Our second measure, which builds on Wittrock et al. (2008), uses the committee scores developed by Groseclose and Stewart (1998) and extended by Canon and Stewart (2009) to measure changes in the value of a legislator's committee portfolio.[15] In order to construct this variable, we sum the value of each committee on which a member serves in the current and previous congress and take the differences between these two quantities.

In addition to the measures of ballot change, we include a series of control variables. First, we include the lagged committee-portfolio value in the model predicting the probability of a new committee assignment. A more valuable committee portfolio should, on average, decrease the likelihood of a new assignment since committee assignments generally followed a hierarchical structure during this time period (Stewart 1992). Second, we include lagged party unity under the expectation that loyal partisans should, on average, receive better committee assignments and retain those assignments. Third, we control for a member's tenure in office. We use the logged value of this predictor since we expect to find more transfers early in a member's term, but we expect to find little difference between those who have served for longer periods of time. Lastly, we control for a legislator's margin of victory in the previous election. Table 4 reports the estimates of the models outlined above.

The first two sets of estimates are from a logit regression model with congress-specific fixed effects. The second set of estimates comes from a pooled OLS model with congress-specific fixed effects. In the case of committee assignments, congress-specific intercepts are necessary because the changes in majority party control that occurred during this time period will influence the number of observed committee transfers. As before, we first estimate a model with only the indicators for adoption of the ballot reforms and then estimate a model that includes the control variables.

In each of the four models, the estimates for the change in ballot type are not statistically different from zero. In fact, the only predictor that explains the likelihood of receiving a new committee assignment is a legislator's tenure, which also has a negative and discernible impact on

TABLE 4
Change in Committee Assignments, 51st–55th Congress
(standard errors in parentheses)

	New Assignment		*Committee Portfolio*	
	Estimate	Estimate	Estimate	Estimate
Change to Office Bloc	–0.122	–0.165	–0.018	–0.036
	(0.366)	(0.370)	(0.101)	(0.101)
Change to Party Column	–0.078	–0.041	–0.014	–0.014
	(0.244)	(0.249)	(0.075)	(0.076)
Committee Portfolio$_{t-1}$		–0.047		
		(0.115)		
Party Unity$_{t-1}$		–0.007		0.005*
		(0.009)		(0.002)
Tenure (logged)		–0.515*		–0.082*
		(0.172)		(0.047)
Margin of Victory		0.003		–0.001
		(0.004)		(0.001)
Constant	2.209	3.370*	0.104*	–0.205
	(0.242)	(0.789)	(0.050)	(0.2095)
N	939	939	939	939
AIC	952.6	948.5		
R^2			0.004	0.013

Note: Cell entries for the Reappointment model are coefficient estimates from a logit regression model with congressional fixed effects. The outcome variable is whether or not a legislator was assigned to a new committee. Cell entries for the Committee Portfolio model are from an OLS regression model where the outcome is the change in the value of a legislator's committee portfolio across consecutive congresses. *$p < 0.05$ one-tailed test.

changes in the value of a committee portfolio. The effect of tenure on the change in the value of a legislator's committee portfolio is a bit surprising. One explanation is that legislators with longer tenure tend to have more valuable committee assignments, which means any change in their assignments will result in a larger drop in the value of their committee portfolio. Lagged party unity is the only other significant predictor and has a positive effect.

One alternative explanation our models cannot readily address is whether ballot reforms led newly elected members to adopt a different approach to committee assignments. Such a dynamic could lead legislators who were elected only under the secret ballot to use committee assignments differently than their predecessors. Indeed, Wittrock et al. (2008) claim that adoption of the office bloc, but not party column, ballot would lead legislators to "be more aggressive in securing positions on

TABLE 5
Mean Committee Portfolio Value by Ballot Experience, 51st–55th Congress

Ballot Type	Mean
Only-Party Ballot	0.897
Only Secret Ballot	0.880
Office Bloc	0.909
Party Column	0.840
Both Types	1.129
Party Ballot	0.960
Office Bloc	1.198
Party Column	1.112

Note: Cell entries are mean committee portfolio values, calculated using the committee value estimates from Canon and Stewart (2009), for all legislators who served in the 51st (1889–91) to the 55th Congress (1897–99) by ballot experience.

preferred committees" (436). Based on this argument, we would expect legislators who were only elected under the secret ballot to seek valuable committee assignments.

In order to examine this possibility, we identified whether a legislator was elected under only the party ballot, only the secret ballot, or both. We then calculated the mean committee portfolio value for each set of legislators. For legislators elected under the secret ballot, we also calculated these quantities for each type of secret ballot.

The results, which are reported in Table 5, are quite revealing. Legislators elected only under either type of secret ballot did not, on average, secure more desirable committee portfolio than legislators who were elected under only the party ballot or both types of ballots. The t statistics for a difference of means tests of the committee portfolios of legislators elected only under the party ballot and those elected only under the secret ballot, the office bloc, and the party column were −0.267, −0.705, and 0.164, respectively. What then accounts for Wittrock et al.'s findings? One possible explanation is that legislators who served on more valuable committees prior to ballot reform continued to hold these committee seats even after ballot reform. The final four rows of Table 5 confirm this as these legislators' average committee portfolios were 1.129 for the entire time period and 0.960, 1.198, and 1.112 when they were elected under the party, office bloc, and party-column ballots, respectively.[16] It appears that tenure was a more important determinant of committee-assignment politics during this period than was ballot reform.

In sum, our estimates indicate that the ballot reforms adopted near the end of the nineteenth century did not have a *direct* effect on committee-assignment politics. Although the expectations outlined in prior work have a certain intuitive appeal, we do not find evidence to corroborate them once we control for the timing of adoption by individual states. That being said, it is still possible that changes in the electoral system had a long-term or indirect effect on committee-assignment politics. These effects, however, likely occurred through either member replacement, changes in member recruitment (Swenson 1982), or later institutional reforms that followed as a consequence of electoral reforms.

Conclusion

Over the course of our nation's history, changes to electoral rules have been infrequent in nature. When they have occurred, however, their impact has been noticeable and long lasting in a number of different respects. Our main objective in this article was to test for the direct effect of ballot laws adopted at the end of the nineteenth century. Overall, we found that changes in the electoral system led to predictable modifications in both the electoral and legislative behavior of incumbent members of Congress. First, our results suggest that incumbents whose states were set to adopt the office bloc ballot were significantly less likely to seek another term in Congress if they had previously faced an experienced challenger. Changes in the ballot laws transferred more of the electioneering costs to the candidates themselves, and this particular group of legislators was the most likely to see a substantial uptick in the cost of running for office.

Second, our results suggest that changes in ballot rules impacted incumbents' propensity to support their parties on party-splitting votes. With the increase in uncertainty about the correct position to adopt, legislators tended to vote against their party at greater rates than they did in previous congresses. Much like we see in the modern era, there appeared to be a concerted effort on the part of incumbents to avoid voting too often with their party when it could alienate constituents during an upcoming election.

Lastly, we find no evidence to support the prior claim that changes in the ballot laws should influence committee-assignment politics. Contrary to earlier studies, our results provide no evidence that the ballot reforms influenced legislators' interest in, or demand for, committee property rights. This, of course, does not preclude the possibility that these electoral reforms helped to usher in a long-term change in committee-assignment politics. Alternatively, our null findings might serve to corroborate studies suggesting that committee property rights were securely in place decades before the ballot reforms were adopted (Gamm and Shepsle 1989; Jenkins 1998).

These results indicate that legislators did what they could to respond rationally to a new set of electoral rules once they were in place. For some incumbents, this meant declining to seek another term in office since the risks of being defeated were far greater under the guise of new ballots that emphasized candidate-specific attributes. For others, it meant returning to Congress but being much more selective in how often they supported the party on divisive votes. The presence of a direct effect for these types of behaviors is quite intuitive since they are ones over which members have the most direct control. For committee assignments, however, individual members had less direct control over outcomes, which likely accounts for our inability to uncover a direct relationship between ballot reforms and committee-assignment politics.

An important implication of our findings, then, is that electoral reforms are more likely to influence legislative behavior when individual members or parties have greater agency over the behavioral choice set. In states where parties determined the type of ballot adopted once the Australian ballot was widely in use, they were able to maintain a semblance of control over how individual candidates were perceived by the electorate. This observation could be of particular importance for countries or interest groups who are considering new electoral reforms. When evaluating the likelihood that a proposed reform will lead to its intended outcome, reformers should carefully consider whether the targeted political actors will be constrained in formulating a response to the new electoral rules.

Although we have found evidence of a direct effect of ballot reforms, it is important to note a few final points regarding our analysis and results. First, the substantive effects uncovered here are, at times, relatively modest. Nevertheless, one should keep in mind that reforms do not occur in a vacuum, but rather they occur in a world with potentially countervailing influences. In this particular case, electoral reforms that weakened the connection between parties and legislators were implemented at the same time as House rules changes that significantly strengthened the role of the majority-party leadership. The fact that we were able to find any effects at all speaks both to the importance and considerable influence of these reforms.

Second, our analysis only focuses on the direct effects of ballot reforms, but this is not meant to suggest that the indirect or long-term effects of ballot reform were inconsequential. By weakening the connection between legislators and the parties, the ballot reforms created an environment that made it possible for future legislators to assert even greater independence from their parties and while enhancing existing property rights among committees. Furthermore, these reforms had important consequences for the recruitment of future legislators, which in turn contributed to additional changes in the House over time (Swenson 1982). In sum, our analysis should not be taken to suggest that the ballot reforms acted like a light switch with sudden and immediate effects. Although we find evidence of a direct effect of ballot reforms, the full impact of these reforms continued to build over time.

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NOTES

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1. Burnham observes that voting in the late nineteenth century was "marked by a more complete and intensely party-oriented voting participation among the American electorate" (1965, 22) than during previous or the most subsequent periods in American history.

2. Bensel (2004) notes that the party-ticket system aided those who wished to engage in voter intimidation or suppress their opponent's vote.

3. One notable exception is Rusk's (1970) analysis of ballot reforms and split-ticket voting.

4. Since our study covers five congresses, a legislator can appear in our dataset anywhere from once if he served in two consecutive congresses or up to five times if he served throughout the entire time period.

5. It is important to note that the Seventeenth Amendment's uniform adoption date makes the proposed research design far more difficult, if not impossible, to operationalize in an analogous setting.

6. There were a few states that changed between types of party ballots in this period but even that provides little in the way of variation.

7. The latter two categories account for 32 of the 355 cases we code as not seeking renomination. Given the paucity of these observations, we opted to treat them all as a single category.

8. Our substantive conclusions remain the same if members who resigned are coded as not seeking renomination rather than being excluded from the analysis.

9. This is especially important to account for since redistricting during this era is not confined to years ending in "2" (Engstrom 2006).

10. Although members began to serve for longer during this period (Polsby 1968), we still, in general, expect retirements to be more concentrated among those who served for longer periods of time.

11. The standard error for this term is 0.300. See Fox (2008) on how to calculate this quantity.

12. One potential critique of this approach is that since ballot reforms influenced decisions about renomination, there might be systematic differences between members who sought renomination and those who retired. In order to address this point, we conducted a difference of means test to compare the party unity of legislators who did and did not seek renomination when their state was set to adopt ballot reforms. The *t* statistics for states that adopted the office bloc ballot was 0.634 and for states that adopted the party-column ballot was −1.276.

13. In an earlier version, we estimated a multilevel model with varying intercepts. One reviewer raised the possibility that our measure of ballot reforms might be correlated with the random intercepts. This could lead to biased estimates due to correlation between the error term and the first-level predictors. As Monogan (2013) notes, fixed effects are one solution to this problem. We therefore use fixed effects rather than a random intercepts model, but it is important to note that our substantive conclusions remain the same with both types of models.

14. The same critique that could be raised with our analysis of party unity votes (see Note 12) could be applicable to this analysis as well. As before, we used a difference of means test to compare the committee portfolios of legislators who did and did not seek renomination when their state was set to adopt ballot reforms. The *t* statistic for states that adopted the office bloc ballot was −0.343, and for states that adopted the party-column ballot, it was −1.258.

15. Wittrock et al.'s analysis uses committee value estimates from a 2002 version of Canon and Stewart (2009), which are no longer accessible and appear to include different committee estimates. For example, Wittrock et al. (2008, 437) reference an estimate for the Library Committee, a joint committee that is not included in Canon, Nelson, and Stewart (2009). The 2009 article does not include estimates for any joint committees, which comports with the original approach used by Groseclose and Stewart (1998).

16. Wittrock et al. (2008) use data from the 49th to 56th Congresses while we use data from the 51st to the 55th Congresses. Our substantive conclusions do not change, however, if we instead look at the 49th to the 56th Congresses. The average committee portfolio values for these congresses are 0.778 for legislators elected only under the party ballot, 0.714 for legislators elected only under the secret ballot, and 1.045 for legislators elected only under both types of ballot.

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