Ballot Reform, the Personal Vote, and Political Representation in the United States

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Abstract

Theories of electoral accountability emphasize voters' ability to evaluate individual officeholders, which provides incentives for officials to demonstrate their quality. Before the Australian ballot was introduced in the US around the turn of the twentieth century, however, most ballot designs constrained voters' ability to distinguish individual candidates. Previous scholarship argues that ballot reform led to the rise of candidate-centered politics and the decline in party influence in the 20th century. We reassess the evidence for this claim and implement the most comprehensive analysis to date on the secret ballot's effects on outcomes related to distributive politics, legislator effort, and party influence. Using an improved research design, we find scant evidence that ballot reform directly affected legislator behavior, much less that it transformed political representation. While the Australian ballot may have been a necessary condition for the eventual rise of candidate-centered politics, ballot reform did not by itself reshape American politics.

Word count: 9,976

An incumbent's incentive to develop a personal vote underlies most theories of electoral accountability (e.g., Fearon 1999; Ferejohn 1986).¹ In these models, voters observe an individual incumbent's performance and decide whether to retain the incumbent or support the challenger, while incumbents anticipate the electorate's decision and respond to voters' potential sanction (for a review of this literature, see Ashworth 2012). These accounts suggest the importance of individual incumbents' (and candidates') performance and qualities for election outcomes and provide insight into the nature of constituents' control over their representatives. Accordingly, an extensive literature studies legislators' pursuit of the personal vote through constituency service, distributive outlays, campaign appeals, and relationship-building (e.g., Cain, Fere-john, and Fiorina 1987; Fenno 1978; Hirano et al. Forthcoming; Kaslovsky Forthcoming; Mayhew 1974).

These models of electoral accountability take as given voters' ability to sanction incumbents in each office up and down the ballot. Yet for the first century of American electoral history, most elections were conducted with the party strip, under which the vast majority of voters cast what was in essence a straight-party ticket for all offices on the ballot (Allen 1910; Evans 1917; George 1883). With this constraint on voters' ability to distinguish individual candidates on the ballot, it is unclear whether elections provide incentives for officeholders to invest effort in distinguishing themselves to their constituents.

In this paper, we study how the incentives to develop a personal vote affect officeholder behavior. We do so by leveraging the introduction of the Australian ballot in US states at the turn of the twentieth century.² In contrast with the party strip, the Australian ballot displayed all parties' candidates on a single ballot, which voters cast privately. It was first adopted statewide in Massachusetts in 1888 and

¹Our use of "personal vote" follows Cain, Ferejohn, and Fiorina (1987, 9): "that portion of a candidate's electoral support which originates in his or her personal qualities, qualifications, activities, and record." This differs from the somewhat narrower quantity estimated in other research (Ansolabehere, Snyder, and Stewart 2000).

²Party strip ballots were deposited publicly; thus, the general public, including party officials, could observe an individual voter's choices (Allen 1910). We use the terms *Australian ballot* and *secret ballot* interchangeably to describe state-level ballot reforms that required government-printed ballots and ensured secrecy in the voting booth.

quickly spread across states, with 36 states implementing it within a decade.³ While theories of electoral accountability suggest that voters' newfound ability to evaluate individual officeholders should increase legislators' efforts on behalf of their constituents in pursuit of the personal vote, some historical accounts suggest that the Australian ballot enhanced party control rather than popular control over elected officials (Ostrogorski 1908; Ware 2000).

We combine original historical data with existing sources to provide the most comprehensive study to-date on the effects of the secret ballot on legislative behavior. Our dependent variables span three components of the personal vote. First, following research that associates pork barrel politics and constituency service with officeholders' attempts to secure the personal vote (Cain, Ferejohn, and Fiorina 1987; Ferejohn 1974), we examine the provision of distributive resources and social welfare benefits. Second, following studies of publicly observable indicators of legislative effort (Dal Bó and Rossi 2011; Finocchiaro and MacKenzie 2018; Fouirnaies and Hall Forthcoming), we evaluate patterns of abstention, bill sponsorship, and floor speech. Third, following research on legislators' ideological responsiveness to constituency and party preferences (Ansolabehere, Snyder, and Stewart 2001; Carson and Sievert 2015; Gailmard and Jenkins 2009), we study several dimensions of legislators' roll call voting patterns. Altogether, our dependent variables characterize an extensive range of representational outcomes and span the years 1880-1930.⁴

In various specifications, we use these measures with a differences-in-differences design to estimate within-constituency and within-legislator effects of the secret ballot. This strategy allows us to account for the possibility that the secret ballot could have also affected the characteristics of legislators who sought office, that constituencies from different states or districts could have different demands for legislative representation, or that secular trends in patterns of representation could confound estimates of the secret

³States used two versions of the secret ballot. The *office bloc* groups candidates by office and their names may or may not be accompanied by their political party, while the *party column* ballot lists all candidates under the party label. While our primary interest is in the effect of the adoption of the Australian ballot, we report results from models that distinguish the effects of these different ballot formats in Appendix Section A.6.

⁴As we explain below, the start and end years of our analyses vary somewhat across measures based on data availability. ballot's effect. Figure 1 provides a simplified illustrative example, comparing the average number of private bills sponsored by representatives from states that implemented the Australian ballot in 1890 with representatives from late-adopting states across time (data from Finocchiaro and MacKenzie 2018). Based on a visual inspection of the figure, a cross-sectional comparison would indicate that legislators from Australian ballot states sponsored more private legislation than legislators from late-adopting states (the blue line is always above the red line in the period after 1890), but this gap existed prior to the implementation of ballot reform in any of these states. Thus, it would be problematic to infer that ballot reform caused such a gap. Similarly, a before-and-after comparison among the 1890 ballot reform states indicates that legislators, on average, sponsored substantially more private legislation after ballot reform compared to the period before ballot reform. However, a similar upward trend over the same time period is also present in the states that did not experience any change in their balloting institutions. Again, it would be problematic to infer that the increase during this time period is due to ballot reform when an increase of similar magnitude also occurred in states that did not change their balloting institutions.





Our approach addresses limitations in both design and inference from previous studies in this area. This work primarily uses pooled cross-sectional research designs (Finocchiaro and MacKenzie 2018; Katz and Sala 1996; Wittrock et al. 2008) or before-and-after comparisons of legislators whose states adopted the secret ballot during their tenures (Carson and Sievert 2015), but none accounts both for unobserved characteristics of legislators or constituencies and temporal patterns in congressional behavior.⁵ Though the evidence from this scholarship is generally consistent with the hypothesis that the Australian ballot increased legislative effort and improved the quality of representation, these results are vulnerable to the potential sources of confounding noted above. Additionally, the standard errors reported in previous scholarship are likely incorrect due to the failure to cluster appropriately at the state level (see Primo, Jacobsmeier, and Milyo 2007), which may bias standard errors downwards and produce invalid inferences.

We find no systematic evidence in support of the claim that the secret ballot directly affected the provision of legislator effort or the quality of representation. These patterns are robust across model specifications, measurement strategies, and subsets of observations. Our work contributes to several bodies of scholarship. First, contra claims advanced by proponents of ballot reform (e.g., George 1883), our evidence aligns with primary and secondary accounts that have expressed skepticism about its effects (e.g., Ostrogorski 1908; Ware 2000). Our results suggest that its effects have been overstated by previous scholarship (see, e.g., Carson and Jenkins 2011).

Second, our findings offer evidence on the role of political parties in political selection. Parties have electoral incentives to nominate high-quality candidates (e.g., Hirano and Snyder 2019), and prior to ballot reform, parties both controlled ballot access and benefited electorally when they nominated quality candidates (Carson, Engstrom, and Roberts 2007; Carson and Sievert 2018). Previous scholarship has also found that nineteenth-century legislators were career-oriented (Kernell 1977) and dedicated significant time and resources to constituency service (Bryce 1995 [1888], 197). Partisan control of the electoral apparatus during this period thus may have produced high-performing legislators primarily through the mechanism of selection rather than pure moral hazard (see Fearon 1999). Our null results could reflect the possibility

⁵Engstrom and Roberts (2020, chapter 3) study the effects of the Australian ballot on turnout and rolloff in a differences-in-differences framework for the period similar to the focus of this paper, but do not evaluate the effect on legislative behavior. Engstrom and Roberts (2020, chapter 4) use a similar research design to study the effect of ballot design on bill sponsorship, legislative effectiveness, and presidential support for a much later period (the second half of the 20th century), but this analysis focuses on variants of the office bloc and party column ballots rather than the switch from the party ballot to the Australian ballot. that the Australian ballot shifted the relative importance of these mechanisms, rather than increasing the effect of moral hazard while holding selection constant.

Third, previous research investigates sources of variation in the personal vote and incumbency across time (e.g., Ansolabehere, Snyder, and Stewart 2000; Cox and Katz 1996). For the period we study, Ansolabehere, Snyder, and Stewart (2000) estimate that while the incumbency advantage was relatively small, it was accounted for almost entirely by the personal vote. Our results suggest that ballot design was probably not an important contributor to the personal vote in this period, nor did it "[usher] in a new era of candidate-centered politics" (Engstrom and Kernell 2014, 191-2), at least not in the short run.

Finally, our research complements studies on the personal vote outside the US. This research often examines cross-national or cross-party variation in the incentives for representatives to engage in personal vote-seeking behavior (e.g., Carey and Shugart 1995; Samuels 1999). Other scholarship investigates how other institutional features, such as the candidate nomination process and interbranch relations, affect vote-seeking incentives in cross-national contexts (e.g., Crisp et al. 2004). We contribute to this research by leveraging subnational variation in balloting institutions to identify their effect on personal vote-seeking behavior.

The Australian Ballot in the United States

During the first century of American elections, ballots were created and separately distributed by political parties. Voters chose between the ballots produced by competing parties, each of which listed the party's candidates for all offices. Ballots were deposited publicly so that a voter's choice could be monitored. This balloting system made it difficult (though not impossible) for voters to split their tickets across candidates for different offices and political parties (e.g., Rusk 1970), complicating voters' efforts to sanction individual officeholders. It may have been especially difficult for down-ballot candidates, such as those running for the House of Representatives, to distinguish themselves from competing candidates, as partisanship was likely the dominant decision criterion for most voters (Katz and Sala 1996).

Due in part to the balloting procedures then in place, 19th-century legislators are often believed to have privileged their party's priorities at the expense of their constituents' interests. Progressives embraced ballot reform to reduce the influence of political parties and strengthen popular control over officeholders, as party corruption had frustrated constituents and "created a favorable attitude on the part of a majority of voters" toward reform (Evans 1917, 21). As the *New York Times* declared in an editorial advocating for ballot reform: "We can see no ground on which men who desire that elections shall be the fair and honest expression of the public will can be opposed to the purpose and principle of this bill ... This measure would do more to raise the standard of political management and purify elections than any one act of legislation that could be devised."⁶ The Australian ballot was quickly adopted by most states (including New York), in which ballots were issued by the state with the names of all candidates for each office and were cast in secret. Henry George (1883, 208) predicted that the Australian ballot "would be the greatest single reform" and would "very much lessen the importance of party nominations and party machinery" in the selection of elected representatives.

The available evidence suggests that the Australian ballot enabled voters to distinguish between candidates for different offices. Using various methods of tabulating split-ticket voting rates across different offices, split-ticket voting substantially increased following the adoption of the Australian ballot (Reynolds and McCormick 1986; Rusk 1970). As a result, scholars have argued that ballot reform "made creditclaiming and other personal vote activities by members of Congress significantly more important for reelection" (Katz and Sala 1996, 21) and "firmed up the agency relationship by increasing accountability [as] members of Congress now had a greater incentive to be responsive to voters' needs" (Carson and Jenkins 2011, 39).

Previous research provides some support for the claim that ballot reform had important consequences for legislative behavior. Studies indicate that legislators elected under the Australian ballot exhibited less party loyalty in congressional voting patterns (Carson and Sievert 2015; Wittrock et al. 2008), had longer committee tenures and more favorable committee assignments (Katz and Sala 1996; Wittrock et al. 2008; cf. Carson and Sievert 2015), sponsored more private bills on behalf of constituents (Finocchiaro and MacKenzie 2018), and secured more spending for district projects (Wittrock et al. 2008). As noted above, however, the research designs used in these studies complicate their ability to isolate the effect of ballot reform from other potential confounding factors that could affect legislative behavior. In addition, the dependent variables used in this research measure relatively few dimensions of legislative behavior, which provides

⁶"No Place for Partisanship," May 3, 1888, page 4.

a limited view of the range of ways that ballot reform could have affected political representation.

Data

We study the effect of the secret ballot using three sets of dependent variables, examining its effects on government outputs, legislator effort, and legislator ideological behavior. These data reflect a combination of original data collection as well as outcomes used in prior research; these measures are summarized in Table 1.

Our primary analyses use data on three kinds of government outputs. First, we assembled original data on the annual state-level distribution of war pensions from fiscal years 1882 to 1920. These data come from the *Annual Report of the Commissioner of Pensions to the Secretary of the Interior* and describe the number of pension recipients and the total dollar amount of pension payments.⁷ Pension payments for disabled veterans in the United States were initiated in 1776 and later expanded in the early nineteenth century as members of Congress recognized the benefits of using pension payments as particularistic goods (Finocchiaro and Jenkins 2016). The pension system expanded dramatically following the Civil War and, beyond expanding eligibility for pension payments, members of Congress were frequently ensnared in pension politics when enlisted by constituents, and often their pension agents, to intervene in the Pensions Office on their behalf. During this time period, both anecdotal evidence and the enormous amount of correspondence from members of Congress to the Bureau of Pensions indicates that pension assistance was an important source of constituency service.⁸ If the secret ballot increased the incentives for members

⁷Generally speaking, we average the measures of pension activity across the two years that comprise a given congress. However, state-level pension data on both the number of pensioners and the total dollar amount of pensions are unavailable from these reports for fiscal years 1883-1886 and 1888. We aggregate county-level totals, which are available in FY1886 and FY1888, to the state level. Additionally, data are unavailable on the total dollar amount for fiscal years 1889-1891. We exclude observations from FY1882 to obtain an uninterrupted balanced panel from FY1886 to FY1920 but results are robust to its inclusion (see Table A.17).

⁸Representative Robert La Follette (1911, 84) estimated that a quarter to a third of his time was spent on pension cases at the Pension Office. of Congress to intervene on behalf of their constituents, we expect to observe increases in pensions in states that adopted it. We study both the number of pensions and their dollar values.

Second, we use biennial data on the geographic distribution of post offices. We supplement data reported in Rogowski (2016) on post offices from 1876 to 1896 with original data to extend the time series through 1916. Local communities frequently mobilized around and petitioned for expansion of postal services (Fuller 2003), and Kernell and McDonald (1999) provide evidence of the constituency-induced political incentives that affected congressional action in relation to postal services.⁹ Therefore, we examine whether the secret ballot increased the provision of post offices, which we operationalize with raw counts and its per-capita values.

Third, we use data from Wilson (1986) on the allocation of rivers and harbors projects from 1889 to 1912.¹⁰ Rivers and harbors were among the most important internal improvements for which the late nineteenth-century Congress awarded line-item appropriations. These appropriations were generally awarded to dredge waterways and strengthen river banks. Because these projects were both tangible and geographically fixed, they provided natural opportunities for credit-claiming. If the secret ballot increased the incentives for legislators to cultivate personal reputations, we expect that it increased the allocation of river and harbors projects in states that adopted it. We study the secret ballot's effect on both the number of rivers and harbors projects and the amount appropriated for them.

These three measures—pensions, post offices, and rivers and harbors projects—provide a comprehensive portrait of the provision of constituency service at the turn of the twentieth century. The pension system was the most important social welfare program prior to the New Deal and for many years accounted for a large share of federal expenditures (Finocchiaro and Jenkins 2016). Likewise, the post office was "one of the most important institutions of the day" (John 1995, vii) as the largest federal employer. And rivers and harbors projects were "a quintessential part of the pork barrel" (Wilson 1986, 733) during a period in which "the federal government turned out little but land disposal programs, shipping subsidies, tariffs, internal improvements, and the like" (Lowi 2009, 46).

⁹While the allocation of post offices may have been more responsive to the president's partisan interests (Rogowski 2016), legislators of both parties likely had incentives to secure post offices for their districts.

¹⁰These data are also used in Wittrock et al. (2008).

Outcome	Unit	Years	Min	Max	Source
Government Outputs					
Pensioners (number)	state	1882-1920	66	105,746	original
Pensions $(\$)^{\dagger}$	state	1882-1920	\$6,840	\$20,944,580	original
Post offices (number)	state	1874-1914	90	5,342	Rogowski (2016) + original
Post offices (per capita) †	state	1874-1914	0.21	4.30	Rogowski (2016) + original
Rivers and harbors appropriations (\$)	state	1889-1912	\$0	\$3,000,000	Wilson (1986)
Rivers and harbors projects (number)^{\dagger}	state	1889-1912	0	184	Wilson (1986)
Legislator Effort					
Private bill sponsorship (number)	district/member	1881-1930	0	683	Finocchiaro and MacKenzie (2018)
Roll-call participation (rate)	district/member	1881-1930	0	1	Lewis et al. (2022)
Floor speeches (number)	district/member	1881-1930	0	873	Gentzkow, Shapiro, and Taddy (2019)
Floor speech word count (number)^{\dagger}	district/member	1881-1930	0	1,038,121	Gentzkow, Shapiro, and Taddy (2019)
Legislator Ideological Behavior					
Roll-call discretion from parties	state	1881-1914	0.00	0.39	original
Responsiveness (Nokken-Poole, 1st dim.) †	district/member	1881-1930	-1	1	Lewis et al. (2022)
Party unity score [†]	district/member	1881-1930	11.1	100	Lewis et al. (2022)

Table 1: Summary of Dependent Variables

Table provides descriptive statistics, time periods, and units of analysis for dependent variables. All outcomes are measured per Congress. All dependent variables measured as a count ("number") in this table are log-transformed prior to analysis due to the highly skewed nature of the underlying distributions (see Figure A.1). The rivers and harbors projects data are aggregated from the U.S. House-district level to the state level for analysis; we also report results from a district-level analysis in Appendix Section A.9. We derive our roll-call discretion measure using data from Lewis et al. (2022). [†] indicates that the analysis for the outcome is reported in the Appendix.

In a second set of analyses, we study whether the secret ballot affected legislative effort. We examine the introduction of private legislation in the U.S. House, roll-call participation rates, and floor speeches. Private legislation was used to address claims to federal relief from individual constituents (or groups of them).¹¹ Intervening in federal claims on behalf of constituents may have been an important component of legislators' attempts to cultivate personal reputations. We study this hypothesis using impressive data from Finocchiaro and MacKenzie (2018) on the number of private bills introduced in the U.S. House between 1881

¹¹H.R. 267 in the 1st Session of the 62 Congress provides a representative example of a private bill during this time period. Representative Edgar Dean Crumpacker (R-IN) sponsored the bill with the following summary: "Granting an increase of pension to Charles W. Sexton." The full text of this bill is available in Appendix Section A.1. Today, these matters would generally be referred to legislative staff and/or the bureaucracy (Finocchiaro and MacKenzie 2018).

and 1930 (the 47th through the 71st Congresses). Using these data, we test the hypothesis that the secret ballot increased legislators' efforts to address private claims. In addition, we examine two other measures of legislator effort based on roll-call data from Lewis et al. (2022): roll-call participation and House floor speeches. Both measures are widely used in the literature (e.g., Dal Bó and Rossi 2011; Fouirnaies and Hall Forthcoming) and indicate the degree to which the representative is engaged in, rather than absent from, the legislative process.

A third set of analyses (discussed below) studies the implications of the secret ballot for political representation. We examine whether legislators exhibit weaker partisanship and greater responsiveness to their constituents under the Australian ballot using legislative voting records, party unity scores, and a new measure of legislator discretion from party organizations as dependent variables.

Empirical Strategy

We leverage the panel nature of the data and use a difference-in-differences design to estimate the effects of the secret ballot on legislative representation. Specifically, we estimate the average differences in representational outcomes between the pre- and post-reform periods within units while controlling for common time trends and time-varying covariates. This strategy distinguishes the effects of the secret ballot from other secular trends and time-invariant characteristics of legislators, districts, and/or states that may also affect patterns of government outputs or legislator behavior. Specifically, we estimate the following general model:

$$Y_{it} = \lambda_i + \delta_t + \beta \text{Australian Ballot}_{it} + \mathbf{X}\Omega_{it} + \epsilon_{it}, \tag{1}$$

where *Y* is the relevant dependent variable and *i* and *t* index units and years, respectively. The main independent variable is an indicator, *Australian Ballot*, that characterizes whether the secret ballot was used to elect a state's representatives in year t.¹² The coefficient β thus is the key parameter of interest. If the secret ballot increased, for example, legislator effort, we expect to observe positive estimates for β . We include unit fixed effects (λ_i) to control for observed and unobserved time-invariant attributes that may

¹²The year fixed effects are Congress (biennial period) fixed effects. If a state first implemented the secret ballot in the 1892 election, that implementation would correspond to the 53rd Congress (March 1893 through March 1895).

affect legislative behavior. As we discuss below in greater detail, we estimate models with various unit effects that account for the nature of the data and different substantive hypotheses about representation.

For data at the district level, we estimate separate models which include either legislator or district fixed effects.¹³ The former accounts for within-legislator changes in behavior with the introduction of the secret ballot, thereby controlling for fixed characteristics of legislators that might affect their behavior. District fixed effects account for characteristics of House constituencies that might affect the demand for legislators to exhibit particular patterns of behavior.¹⁴ These two approaches allow us to examine distinct channels through which the secret ballot could have improved representation: reduced moral hazard via changes in effort within legislators and reduced adverse selection via voters' improved ability to select high-quality officeholders. We also include time fixed effects (δ_t) to account for secular trends in legislative behavior, legislator, and contextual characteristics that may also be related to legislative behavior and productivity, which are explained below in greater detail. Finally, Ω_{it} is a vector of coefficients for these time-varying control variables, and ϵ_{it} is a random error term, clustered on state.

In model (1), the coefficient for *Australian Ballot* is identified by comparing outcomes within states, legislators, or districts that correspond to changes in a state's ballot institutions. The key identifying assumption is that absent the introduction of the secret ballot, trends in the outcome in states that adopted the secret ballot would have followed the same trends as those in states that experienced no change in ballot format (i.e., parallel trends).¹⁵ This estimation strategy improves upon the research designs used in previous scholarship on the secret ballot. For example, pooled, cross-sectional designs (see, e.g., Finocchiaro and MacKenzie 2018; Katz and Sala 1996; Wittrock et al. 2008) risk confounding by not accounting for (potentially unobserved) differences in legislators or constituencies that produce differential patterns

¹⁴District fixed effects are specific to the relevant redistricting cycle.

¹⁵The raw data appear to satisfy the parallel trends assumption, as we observe reasonably similar trends in outcomes among states that adopted the secret ballot at different points in time prior to adoption in the early adopting states. See Figure A.2.

¹³For analyses conducted at the district and legislator levels, we include all observations in which the legislator elected in the most recent election served the full two-year term in office.

of legislator behavior. While other studies compare the same legislators before and after reforms were implemented (e.g., Carson and Sievert 2015), this approach does not account for secular trends in legislative behavior and political representation that could also be correlated with adoption of the secret ballot.¹⁶ Finally, because ballot laws were implemented at the state level, research that does not cluster standard errors on states (e.g., Carson and Sievert 2015; Finocchiaro and MacKenzie 2018; Wittrock et al. 2008) may produce misleadingly small standard errors and generate inappropriate statistical inferences.

Results

We discuss results for the three sets of dependent variables related to government outputs, legislator effort, and legislator ideological behavior.

Government Outputs: Pensions, Post Offices, and Rivers & Harbors Projects

We begin by examining the effect of the secret ballot on government outputs. Table 2 shows results for the state-level distribution of pensions (Panel A), post offices (Panel B), and rivers & harbors projects (Panel C). The dependent variable for each panel, respectively, is the logged (plus one) count of pensioners in the state, the logged (plus one) count of post offices in the state, and the logged (plus one) appropriations to rivers and harbors projects in the state.¹⁷ We estimate three specifications for each measure. Model (1) represents a standard, generalized difference-in-differences approach without time-varying covariates, model (2) introduces time-varying control variables, and model (3) includes time-varying covariates and

¹⁶This issue also complicates inference in pooled cross-sectional designs, which generally fail to account for these trends through year/Congress fixed effects or other techniques. As a result, the comparison implicit in these studies is between a party-ballot group of legislators, comprised mostly of representatives from an earlier period, with a secret-ballot group of legislators, comprised almost entirely of representatives from a later period.

¹⁷In Appendix Section A.4, we also present results based on alternative measures of each of the three dependent variables: the dollar amount of pensions granted to state residents (plus one, logged), the number of post offices per 1,000 in the population in each state, and the count of rivers and harbors projects (logged, plus one). Across all three alternative measures, results are similar to those reported in Table 2.

unit-specific linear trends, which reduce our reliance on the parallel trends assumption and address the possibility that the secret ballot was correlated with other trends in the provision of government outputs.

	(1)	(2)	(3)
Panel A. Pensions			
Australian Ballot	-0.061	-0.069	-0.038
	(0.052)	(0.040)	(0.038)
Panel B. Post Offices			
Australian Ballot	-0.007	-0.003	-0.049
	(0.046)	(0.041)	(0.034)
Panel C. Rivers & Harbor	rs		
Australian Ballot	-0.121	-0.140	0.257
	(0.372)	(0.418)	(0.245)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark
Unit Fixed Effects	\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark
State-Specific Trends			\checkmark
Panel A Observations	800	800	800
Panel B Observations	893	893	893
Panel C Observations	532	532	532

Table 2: The Australian Ballot &

 the Distribution of Federal Resources

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged count of pensioners. The dependent variable for Panel B is the logged count of post offices. The dependent variable for Panel C is the logged appropriations to rivers and harbors projects. Control variables include log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.

We find consistent results across the three specifications for all of the dependent variables. The estimated coefficients on the Australian ballot indicator are relatively small in magnitude and none are statistically distinguishable from zero. In fact, while most of the estimates in Table 2 are near to zero, the negative sign on eight of the nine estimated coefficients is the opposite of what we would expect if the Australian ballot led legislators to exert greater effort to secure these federal resources for their constituents. We therefore find almost no evidence that a state's implementation of ballot reform induced greater effort among its representatives in assisting constituents with pension-related claims or the provisioning of post offices and river and harbors appropriations.

Legislator Effort: Private Legislation, Roll-Call Participation, and Floor Speeches

We now evaluate the effects of the secret ballot on legislative behavior. Here we focus on whether the secret ballot affected the behavioral inputs that could secure constituency benefits. We first analyze the number of private bills introduced by each legislator from 1881 to 1930. These private bills primarily address claims made on behalf of individual constituents (often related to war pensions or appealing for relief). If the secret ballot provided electoral incentives for members of Congress to generate personal reputations, we expect to observe increased bill sponsorship following its adoption. We also examine two measures of legislator participation in the legislative process: the roll-call participation rate and the count of House floor speeches.¹⁸ We estimate models using district and Congress fixed effects and, separately, member and Congress fixed effects. The latter empirical strategy evaluates whether the secret ballot produced changes in bill sponsorship levels within legislators (i.e., electoral incentives), whereas the former also examines whether the secret ballot resulted in legislators exerting greater effort due to electoral constituencies choosing legislators who invested greater effort in introducing private legislation (i.e., electoral incentives and electoral selection). Additionally, we again estimate models that contain time-varying control variables and member-specific and district-specific linear trends.

Table 3 displays the results. Panel A provides no evidence that the secret ballot increased private bill sponsorship. Only one of the six coefficients is positively signed, and it is not statistically significant at conventional levels. In fact, one of the five negatively signed coefficients is statistically distinguishable from zero.¹⁹ The legislative agenda in the turn-of-the-century Congress was dominated by private bills

¹⁸The roll-call participation rate is the proportion of roll-call votes on which a representative cast a vote, and the count of floor speeches is the number of distinct statements made on the floor of the House that are at least 200 words in length.

¹⁹We do not interpret these results as evidence that the Australian ballot *decreased* production in private legislation. However, one plausible explanation is that ballot reform could have resulted in the election of Progressive legislators who were strongly suspicious of private pension legislation and its favoritism to certain individuals and groups. Another possibility is that the passage of the Dependent and Disability

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Private Bill Sponsorship						
Australian Ballot	-0.202	-0.226*	-0.039	-0.114	-0.126	0.064
	(0.107)	(0.101)	(0.091)	(0.137)	(0.115)	(0.066)
Panel B. Roll-Call Participation Rat	te					
Australian Ballot	-0.048*	-0.050*	-0.060*	-0.017	-0.025	-0.020
	(0.019)	(0.018)	(0.025)	(0.021)	(0.018)	(0.025)
Panel C. Floor Speeches						
Australian Ballot	-0.081	-0.163	-0.102	-0.079	-0.070	-0.066
	(0.149)	(0.132)	(0.124)	(0.097)	(0.101)	(0.115)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
District Fixed Effects	\checkmark	\checkmark	\checkmark			
Member Fixed Effects				\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark		\checkmark	\checkmark
District/Member-Specific Trends			\checkmark			\checkmark
Panel A Observations	8,845	8,845	8,845	8,845	8,845	8,845
Panel B Observations	8,840	8,840	8,840	8,840	8,840	8,840
Panel C Observations	8,845	8,845	8,845	8,845	8,845	8,845

Table 3: The Australian Ballot & Legislator Effort

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged count of private bills introduced by the member. The dependent variable for Panel B is the roll-call participation rate (measured as a proportion). The dependent variable for Panel C is the logged count of floor speeches given by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member of any committee, committee fixed effects, and primary election status in the state. District- or member-specific trends are district-specific linear trends for specification (3) and member-specific linear trends for specification (6). (Finocchiaro and MacKenzie 2018), and could have been a valuable way for legislators to exhibit their attentiveness to issues important to their constituencies in an attempt to cultivate personal reputations. Across a variety of model specifications, however, we find no evidence that the secret ballot increased private bill sponsorship.

In terms of roll-call participation and floor speeches, the estimates again provide no indication that the implementation of the Australian ballot increased legislators' effort on behalf of their constituents. In fact, all 12 estimated coefficients in Panels B and C have a negative sign, which is the opposite direction of the hypothesized relationship. Surprisingly, three estimated coefficients for roll-call participation are statistically significant from zero (the three district fixed effects specifications). These results imply that, under the secret ballot, voters selected representatives who were about 5-6 percentage points less likely to participate in a given roll-call vote than representatives elected under the party ballot. One speculative explanation is that these members were less beholden to party pressure and whips, and, thus, could more easily skip votes. Nevertheless, altogether the results in Table 3 do not support the claim that the secret ballot changed behavior among legislators who were newly motivated to secure personal reputations. In combination with the results in Table 2 above, these findings weigh strongly against claims that the secret ballot meaningfully affected congressional representation.

Parties, Principals, and Ballot Reform: Legislator Discretion from State Parties

In additional analyses, we explore the implications of the findings presented above for legislators' responsiveness to electoral constituencies and political parties. In doing so, we study whether and how the secret ballot meaningfully altered the relevant principals for members of Congress. Proponents of ballot reform argued that the secret ballot would strengthen the agency relationship between voters and officeholders and weaken state and local party control. We propose a new measure that gauges the extent to which representatives exerted greater discretion over their roll-call voting from the state party organization that controlled ballot access in the period prior to the Australian ballot. In addition, we also implement two commonly used approaches in the existing literature to assess legislator responsiveness to constituent Act in 1890, which greatly diminished private pension legislation in the short term, differentially affected demand for private legislation among states that had adopted the Australian ballot. preferences and party loyalty in Congress (as measured by party unity scores).

We first examine whether the secret ballot freed legislators from the control of state parties. Prior to the Australian ballot, these party organizations purportedly controlled ballot access, as they were primarily responsible for printing and distributing ballots to voters. Thus, these entities would likely be the relevant principals for House members. We construct a novel measure of the discretion afforded to members of the House of Representatives in their roll-call voting behavior. If members are less beholden to party organizations with the advent of the Australian ballot, we should likely expect them to exhibit greater discretion in how they vote on issues.

We focus on the period prior to the 17th Amendment and assume that members of the Senate during this period are agents of state party organizations. We measure each state party's preferences using Senators' Common Space NOMINATE scores.²⁰ For instance, if a state has two Republican senators, the average of those two scores characterizes the ideological position of the state party. If only one senator in a state represents a given party, we assume that senator's roll-call score is the position of that state party.²¹ We then calculate the distance (i.e., absolute value of the difference) between each member of a state's House party delegation and the estimated position of the state party as proxied by that state's Senate party delegation, as described above. Finally, we take the mean of these distances to characterize the level of discretion afforded to a state's party delegation in the House, where larger values of this variable indicate greater ideological differences between House members and state party leaders.²² If the secret ballot de-

²⁰This analysis requires Common Space NOMINATE scores so that we can make comparisons across chambers. This measure comes with the limitation, however, of being able to identify changes in House delegation ideology via replacement and not conversion, as Common Space scores are constant across legislators' careers.

²¹If a party is not represented in a state's Senate delegation, that observation is missing.

²²Our measure of discretion in roll-call voting for state *s* and party *p* in Congress *t* is calculated as follows:

Discretion_{spt} =
$$\frac{1}{n_{spt}} \sum_{i=1}^{n_{spt}} |H_{ispt} - \bar{S}_{spt}|,$$

where H_{ispt} indicates the ideal point of representative *i* in state *s* and party *p* in Congress *t*, \bar{S}_{spt} represents the state party's ideal point as measured by the average of the roll-call scores of the Senate delegation in

creased subnational party control over legislators, we would expect that the secret ballot is associated with larger values of the discretion variable.

Table 4 shows the results. We find no evidence that members of the House exhibited greater discretion from state parties upon the adoption of the secret ballot. Instead, our results are null and estimated quite precisely, suggesting that the introduction of the Australian ballot did not provide greater latitude for members of the House to deviate from the preferences of state party leaders.

	(1)	(2)	(3)	(4)
Australian Ballot	-0.002	-0.000	-0.003	-0.002
	(0.003)	(0.004)	(0.003)	(0.004)
State FEs	\checkmark	\checkmark		
Congress FEs	\checkmark	\checkmark		
State-Party FEs			\checkmark	\checkmark
Congress-Party FEs			\checkmark	\checkmark
Unit-Specific Trends		\checkmark		\checkmark
Observations	746	746	746	746

Table 4: Discretion from the State Party on Roll-Call Voting

Robust standard errors clustered by state are in parentheses. * p < 0.05. Dependent variable is the mean squared distance for the House party delegation of a state from the state's Senate party delegation. Unitspecific trends are state-specific linear trends for model (2) and state-party-specific linear trends for model (4).

We also examine the consequences of the secret ballot for responsiveness to constituent preferences. Elections enable constituents to influence the behavior of their elected representatives, both by selecting representatives whose preferences are shared by the electorate and providing electoral incentives for representatives to advance constituency preferences. Using a strategy similar to Gailmard and Jenkins (2009), we study whether the secret ballot increased legislators' responsiveness to constituency preferences as measured by roll-call voting records. Details on this analysis and the results are reported in Table A.15 in Appendix Section A.10. While we find strong evidence that voters select more liberal (or conservative) representatives as the district shifts in a more liberal (or conservative) direction, we uncover no evidence indicating that this relationship strengthened with the secret ballot.

state *s* and party *p* in Congress *t*, and n_{spt} is the number of members in the House delegation of state *s* and party *p* in Congress *t*.

Finally, we study the relationship between the Australian ballot and legislators' party unity scores. If the Australian ballot weakened legislators' connections to their parties and thus their party loyalty, we would expect legislators elected under the secret ballot to exhibit decreased party unity in their roll-call voting. Details are reported in Table A.16 in Appendix Section A.10. While all of the estimated coefficients for the main set of specifications are negative, the magnitude of the estimates is very small. Examining the estimates across all of the specifications, two of the 30 are statistically significant. However, all of the estimated coefficients are small in magnitude with even the largest implying a minimal decline in party loyalty in response to the Australian ballot.

Overall, we find little evidence that the Australian ballot fundamentally transformed the relevant principals for members of Congress. In response to ballot reform, legislators did not exhibit greater responsiveness to local constituent preferences, nor did they markedly reduce their commitment to national party positions or exhibit behavior consistent with greater discretion from their state party organization. To be sure, each of the analyses in discussed in this section has important limitations related to measurement and modeling. However, the totality of the evidence suggests that the Australian ballot had less of a transformative impact on political representation than its proponents argued it would. The results in this section also offer an explanation for our earlier null findings; because the Australian ballot did not produce systematic changes in the principals to whom legislators respond, it is perhaps not surprising that we do not observe differences in government outputs or legislative behavior upon the introduction of the secret ballot.

Robustness checks

The results are robust to a wide range of additional analyses. First, we estimate models that distinguish the effects of the party column ballot from the office bloc ballot. While no state repealed the secret ballot after adopting it, states varied in how they implemented the secret ballot and 16 states switched at least once between the party column and office bloc formats. Because the party column ballot format resembled, in certain respects, the party slip ballots that were previously used, it is possible that any potential effects would be concentrated among states with the office bloc. As Appendix Section A.6 shows, however, we find no consistent evidence that either the office bloc or party column ballots affected government outputs or legislative behavior. In fact, in some models we find that the office bloc was associated with more negative representational consequences relative to the party column ballot (for example, see Table A.6), which runs contrary to expectations. Overall, therefore, distinguishing ballot format does not change our substantive conclusions.

Second, we examine the potential anticipatory effects of the secret ballot by regressing outcomes in Congress *t* on the use of the secret ballot in the *next* election. The timing of the secret ballot's enaction varied by state, in some cases providing ample time for legislators to strategically adjust their behavior in anticipation of standing for re-election by secret ballot in the next election. These results are shown in Appendix Section A.7. These analyses continue to support our main findings, however, and we find no evidence that our estimates above are overly conservative due to anticipatory behavior among legislators.

Third, we re-estimate our models while excluding Southern states, which we define as the eleven states of the former Confederacy. For the most part, these states had one-party systems following Reconstruction, and the absence of inter-party competition could have implications for our ability to isolate the effects of the secret ballot in those states. These results are shown in Appendix A.8. Across dozens of analyses, we find zero significant estimates in the hypothesized direction for our estimated coefficients of interest.

Finally, for dependent variables that are measured in counts (e.g., the count of pensioners), we estimated models using Poisson regressions rather than using OLS with log transformations. The results from these models are very similar to the equivalent specifications reported in the paper and are shown in Appendix Section A.4.

Summary of Results

With the number of outcomes under consideration as well as the array of alternative specifications and measurement strategies, we report more than 250 estimated coefficients of interest in the paper and appendix. Table 5 provides a high-level comprehensive summary of these estimates, indicating whether the sign of the estimate is in the hypothesized direction and whether the estimate is statistically significant from zero. The table presents a comprehensible (albeit coarse) summary of all of the estimates reported in the paper and appendix. Overall, only about a third of the estimated coefficients are in the hypothesized direction, and just over one percent are both in the hypothesized direction and statistically significant from zero. In other words, we observe slightly fewer significant estimates in the hypothesized direction than we would expect to observe merely by chance.

Our interpretation of these results is that, across a bevy of relevant outcomes and sensible alternative specifications, we find essentially no evidence in support of the claim that the advent of the Australian ballot directly altered the character of political representation in the United States. As discussed above and at greater depth below, it is highly plausible that the eventual rise of candidate-centered politics in the latter half of the 20th century would not have occurred in the absence of ballot reform. However, it is nearly impossible to infer from the analyses in this paper that ballot reform by itself had a quick, direct, and transformative effect on American democracy as many accounts claim.

Outcome Category	In Hypoth. Direction	Significant in Hypoth. Direction	In Opposite Direction	Significant in Opposite Direction	Total
Government Outputs	28 (31.1%)	0 (0.0%)	62 (68.9%)	2 (2.2%)	90
Leg. Effort	16 (15.7%)	1 (1.0%)	86 (84.3%)	19 (18.6%)	102
Leg. Ideological Behavior	39 (52.7%)	2 (2.7%)	35 (47.3%)	1 (1.4%)	74
Total	83 (31.2%)	3 (1.1%)	183 (68.8%)	22 (8.3%)	266

Table 5: Comprehensive Summary of Results

Table provides a comprehensive summary of all of estimated coefficients of interest reported in the paper and Appendix. See Table 1 for the list of outcomes by category. For all outcomes, the hypothesized direction of the relationship is positive except for party unit score, which has a negative hypothesized relationship. Percentages in parentheses are row percentages.

Contextualizing the Non-Effects of Ballot Reform

In contrast with the arguments of the secret ballot's proponents, prominent historiography, and recent scholarship, we have found strikingly little evidence that the Australian ballot transformed American democracy. However, a number of other prominent accounts that discuss political representation in post-Reconstruction America provide evidence consistent with our findings.

First, one possible explanation is that members of the postbellum Congress utilized their office to attend to their constituents. Indeed, members of Congress remarked on the many hours they spent working in their official capacities, many of them dedicated to constituency service. For instance, Representative (and future President) James Garfield documented his efforts on Saturday, December 14, 1872 as follows:

Worked up correspondence. Dictated letters until about ten o'clock. Then spent four hours among the departments on other people's business. I do not know that I have ever been much more weary of this sort of vicarious suffering than I am tonight.

Garfield was not alone in describing the long hours that members of Congress worked to serve district interests. A decade later, the House was considering reducing annual salaries from \$5,000 to \$4,000. Representative Roswell Horr rose in colorful opposition to underscore the expectations and duties of the office:

When you take into account the labor at present required of a member of Congress, can you conceive how any man in his normal condition, in full possession of all his faculties, could for a moment suppose that the salaries of these officials as now fixed by law are excessive? The work of a member of this House which is expected of him by his constituents and demanded of him by the people, if properly performed, is no means light. We doubt if there is a single member of this House who will claim for a single moment that the work which he is compelled to do is not largely in excess of what really ought to be required of any man.²³

Horr subsequently enumerated the long list of constituent requests that was typical for legislators, which included attending to land patents, homestead claims, patent applications, military discharges, treasury claims, postal routes, mail clerks, and sources of federal employment (among other items). While the

²³2066 House Report 466, February 16, 1882.

Garfield and Horr examples are but two legislators who served in the decades prior to the introduction of the secret ballot, their accounts are consistent with the possibility that legislators were already exerting considerable effort to serve their constituents. While corruption and graft may have accompanied party ballots, it does not necessarily follow that constituents received low-effort or low-quality representation as a consequence. Therefore, the secret ballot may have not substantially improved the quality of political representation because legislators were already advocating for their constituents rather effectively.

Second, even if the secret ballot enabled voters to cast votes for candidates from different parties across offices, the nature of the campaign and/or news environment may not have meaningfully provided voters with the information that would permit them to do so. We explore this possibility using data from Ban et al. (2019) and report the results in Appendix A.12. We find little evidence that the secret ballot triggered changes in the news environment by increasing the supply of information about political candidates. More generally, the timing of the secret ballot does not coincide with the rise of candidate-centered politics. According to Ansolabehere, Snyder, and Stewart (2001), candidates were not especially responsive to local preferences until the 1930s, and incumbent officeholders did not acquire significant personal votes or incumbency advantages until the mid-twentieth century (Ansolabehere, Snyder, and Stewart 2000). Therefore, the secret ballot may not have generated the effects anticipated by its proponents because the information environment did not meaningfully change with its introduction.²⁴

To recapitulate, we do not deny the parties' control over political affairs in the late nineteenth century. Our evidence suggests, however, that party control over elections may not have come at the expense of constituent representation. Primary accounts point to considerable legislative effort exerted by members prior to ballot reform. Analyses of the news environment demonstrates that candidates did not receive greater coverage following the secret ballot's enactment. These possibilities, which are not mutually exclusive, help to contextualize our lack evidence about the relationship between the secret ballot and political

²⁴Alternatively, voters' attention in the pre-reform days may have been fixed on Congress, such that the secret ballot would have changed the incentives for candidates for other (down-ballot) offices. Our analyses using data from Ban et al. (2019) do not allow us to study this hypothesis; however, additional research using data on lower offices may be able to distinguish whether the secret ballot affected officeholder behavior for offices that otherwise were less publicly salient. representation.

Conclusion

The institutions governing the selection of legislators in the US have reflected the logic that electoral incentives would motivate their behavior. By requiring members of the House to stand for re-election every two years, delegates to the Constitutional Convention seemed to believe that legislators would faithfully represent their constituencies to win voters' approval and continue in office. A century later, Progressive reformers argued that by severing party control over balloting procedures, the secret ballot would improve political representation and initiate candidate-centered rather than party-centered elections. As a result, the secret ballot is a central component in the historiography of elections and in the periodization of Congress.

Our evidence casts considerable doubt on this received wisdom. Across a number of dependent variables and empirical strategies, we find no evidence that the secret ballot systematically affected legislative behavior or political representation. Our analyses suggest that the ballot reform movement does not register as a detectable—let alone transformative—moment in congressional history. Despite increasing the ease with which voters can distinguish candidates for office based on the candidates' personal characteristics rather than their party affiliation, we find no evidence that the Australian ballot significantly modified legislators' electoral incentives or strategic calculations. We do not infer, however, that electoral incentives do not affect legislative behavior. Instead, we have argued and presented evidence to suggest that legislators exhibited higher-quality behavior prior to the secret ballot than is commonly appreciated. In addition, the results presented here should not be interpreted as more general evidence of the failure of Progressive era electoral reforms. To the contrary, the direct primary had wide-ranging effects on political representation by, for instance, increasing the provision of public goods and allowing parties and voters to better distinguish candidates on quality within parties (e.g., Hirano and Snyder 2019).

Our results contribute to scholarship that subjects popular claims about the effects of institutional change to empirical scrutiny. For instance, recent scholarship has found that the effects of electoral competition (Moskowitz and Schneer 2019) and legislative term limits (Olson and Rogowski Forthcoming) on political outcomes are quite different in comparison with what their advocates commonly claim. The secret

ballot maintains a revered status among turn-of-the-twentieth-century reforms, yet previous attempts to evaluate its effects have used research designs not especially well-suited for the task. In using an appropriate research design, however, we show that its consequences are likely overstated in earlier scholarship.

More speculatively, we contend that the secret ballot is a necessary but not sufficient condition for candidate-centered electoral politics. While anecdotal evidence suggests that members of Congress exerted considerable effort on behalf of their constituents prior to the secret ballot, they may have lacked the institutional capacity to improve upon those efforts. For example, the Legislative Reorganization Act of 1946 dramatically expanded the resources available to members of Congress; prior to this, legislators were limited mostly to the work they could accomplish on their own. If the Australian ballot and its incentives for the development of personal reputations had appeared at a time when legislators had greater institutional resources, it is possible ballot reform could have had more substantial effects. Further, the effects of the secret ballot may have accumulated slowly over time, possibly due to the development of other institutional characteristics that were necessary for its full effects to be realized. Based on our evidence, however, the effects of the secret ballot are less transformational than existing literature claims.

We close by noting several limitations of our study and potential directions for future research. First, while our data measure the *production* of legislative proposals by individual representatives, they do not allow us to evaluate the *success* of legislators in securing them. Given the high rates of petitioning during this period, it may have been relatively costless for legislators to file bills with the House clerk; whether legislators put in the time and effort to secure their passage, however, remains unanswered. Second, our data do not evaluate whether the secret ballot affected the production of substantive public policy. Future research could evaluate whether legislators elected through the secret ballot were more likely to develop and pass programmatic policy proposals. Third, our data focus on the House of Representatives, but it is less clear whether our findings generalize to other officials, such as governors and state legislators. As historical data on the U.S. Congress and states is more easily accessible and widely available, additional research can shed light on these and other important questions.

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ONLINE APPENDIX

Robustness Checks and Supplementary Analyses for

Ballot Reform, the Personal Vote, and Political Representation in the United States

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A.1 Example of Private Bill

The following is a representative private bill from this time period, which demonstrates that private legislation was used a means of constituency service. The bill, 62 H.R. 267, was introduced in the House on April 4, 1911 by Representative Edgar Dean Crumpacker (R-IN) and was referred to the Committee on Invalid Pensions.



iN THE HOUSE OF REPRESENTATIVES

April 4, 1911.

Mr. CRUMPACKER introduced the following bill; which was referred to the Committee on Invalid Pensions and ordered to be printed.

A BILL

Granting an increase of pension to Charles W. Sexton.

Be it enacted by the Senate and House of Representa-Ì tives of the United States of America in Congress assembled, $\mathbf{2}$ That the Secretary of the Interior be, and he is hereby, au- $\mathbf{3}$ thorized and directed to place on the pension roll, subject to $\overline{4}$ the provisions and limitations of the pension laws, the name $\mathbf{5}$ of Charles W. Sexton, late of Company I, Second Regiment $\mathbf{6}$ New York Volunteer Cavalry, and pay him a pension at the 7 8 rate of seventy-two dollars per month in lieu of that he is now receiving. 9

A.2 Distribution of Outcome Measures



Figure A.1: Distribution of Log-Transformed Outcomes, Original & Log-Transformed



Figure A.1: Distribution of Log-Transformed Outcomes, Original & Log-Transformed, Continued

A.3 Trends and Pre-Trends

Figure A.2: (Pre-)Trends by Election Year of Australian Ballot Implementation



A.4 Alternative Measures of Dependent Variables

Tables A.1 displays results based on alternative measurement strategies for the three dependent variables that capture federal resource allocation. In the body of the paper, we employ the count of pensioners (logged, plus one), the count of post offices (logged, plus one), and appropriations (\$) to rivers and harbors projects (logged, plus one). In this section, we also report results from alternative measures of each: the dollar value of the pension rolls (logged, plus one), the count of post offices per capita, and the count of rivers and harbors projects (logged, plus one). Across all specifications, the results provide no indication that the arrival of the Australian ballot altered the allocation of federal resources.

	(1)	(2)	(3)
Panel A. Pensions			
Australian Ballot	-0.046	-0.055	-0.015
	(0.057)	(0.045)	(0.046)
Panel B. Post Offices			
Australian Ballot	0.013	0.012	-0.029
	(0.051)	(0.044)	(0.056)
Panel C. Rivers & Harbo			
Australian Ballot	0.125	0.157	0.143
	(0.095)	(0.089)	(0.093)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark
Unit Fixed Effects	\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark
State-Specific Trends			\checkmark
Panel A Observations	762	762	762
Panel B Observations	893	893	893
Panel C Observations	532	532	532

Table A.1: The Australian Ballot &the Distribution of Federal Resources,Alternative Outcome Measures

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged value of the pension roll. The dependent variable for Panel B is the count of post offices per 1,000 in the population. The dependent variable for Panel C is the logged count of rivers and harbors projects. Control variables include log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends. We also present results using an alternative distance metric for the legislator discretion from state party leaders measure based on the mean squared difference rather than the mean absolute difference. More formally:

Discretion_{spt} =
$$\frac{1}{n_{spt}} \sum_{i=1}^{n_{spt}} (H_{ispt} - \bar{S}_{spt})^2$$
,

where H_{ispt} indicates the ideal point of representative *i* in state *s* and party *p* in Congress *t*, \bar{S}_{spt} represents the state party's ideal point as measured by the average of the roll-call scores of the Senate delegation in state *s* and party *p* in Congress *t*, and n_{spt} is the number of members in the House delegation of state *s* and party *p* in Congress *t*. Similar to the results from Table 4 in the paper, all four estimated coefficients are near to zero and not statistically significant in Table A.2.

	(1)	(2)	(3)	(4)
Australian Ballot	-0.006	-0.003	-0.011	-0.009
	(0.011)	(0.013)	(0.011)	(0.011)
State FEs	\checkmark	\checkmark		
Congress FEs	\checkmark	\checkmark		
State-Party FEs			\checkmark	\checkmark
Congress-Party FEs			\checkmark	\checkmark
Unit-Specific Trends		\checkmark		\checkmark
Observations	746	746	746	746

 Table A.2: Discretion from the State Party on Roll-Call Voting,

 Alternative Outcome Measure

Robust standard errors clustered by state are in parentheses. * p < 0.05. Dependent variable is the mean absolute distance for the House party delegation of a state from the state's Senate party delegation. Unitspecific trends are state-specific linear trends for model (2) and state-party-specific linear trends for model (4).

A.5 Poisson Models

Several of the outcome measures used in the paper are count variables. Due to their highly skewed distributions, we log-transform these measures prior to analysis (see Section A.2). In this section, we present estimated coefficients all count outcome measures based on Poisson models. Tables A.3-A.4 contain the results from these models for all of main outcome measures that are counts. Across both tables, only six out of 21 estimates are signed in the hypothesized direction, and the single estimate statistically distinguishable from zero is not signed in the hypothesized direction.

	(1)	(2)	(3)
Panel A. Pensions			
Australian Ballot	-0.009	-0.032	-0.002
	(0.023)	(0.020)	(0.013)
Panel B. Post Offices			
Australian Ballot	-0.021	-0.024	-0.049
	(0.035)	(0.034)	(0.025)
Panel C. Rivers & Harbo	rs		
Australian Ballot	0.070	0.131	0.094
	(0.126)	(0.100)	(0.108)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark
Unit Fixed Effects	\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark
State-Specific Trends			\checkmark
Panel A Observations	800	800	800
Panel B Observations	893	893	893
Panel C Observations	489	489	487

 Table A.3: Poisson Results:

 the Distribution of Federal Resources

Robust standard errors clustered by state are in parentheses. * p < 0.05. Reported coefficients are estimated from Poisson models. The dependent variable for Panel A is the count of pensioners. The dependent variable for Panel B is the count of post offices. The dependent variable for Panel C is the count of dollars appropriated to rivers and harbors projects. Control variables include log log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Private Bill Sponsorship						
Australian Ballot	-0.049	-0.040	0.003	-0.044	-0.031	0.137
	(0.102)	(0.100)	(0.090)	(0.110)	(0.099)	(0.075)
Panel B. Floor Speeches						
Australian Ballot	-0.284	-0.255	-0.157	-0.202*	-0.187	0.039
	(0.168)	(0.155)	(0.187)	(0.093)	(0.095)	(0.082)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
District Fixed Effects	\checkmark	\checkmark	\checkmark			
Member Fixed Effects				\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark		\checkmark	\checkmark
District/Member-Specific Trends			\checkmark			\checkmark
Panel A Observations	8,612	8,612	8,600	7,822	7,822	7,791
Panel B Observations	8,553	8,553	8,401	7,588	7,588	7,232

Table A.4: Poisson Results:Legislator Effort

Robust standard errors clustered by state are in parentheses. * p < 0.05. Reported coefficients are estimated from Poisson models. The dependent variable for Panel A is the count of private bills introduced by the member. The dependent variable for Panel B is the count of floor speeches given by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member of any committee, committee fixed effects, and primary election status in the state. District- or member-specific trends are district-specific linear trends for specification (3) and member-specific linear trends for specification (6). The South is defined as the 11 states of the former Confederacy.

A.6 Decomposing the Type of Ballot

In the paper, we focus on the effect of the advent of the Australian ballot, which entails both the government printing and supplying the ballot to voters, and the voter being ensured secrecy in casting her vote. However, states adopted different versions of ballot reform and shifted between types of the Australian ballot across time. In particular, while the party column format allowed a voter to quickly and conveniently cast a straight-party ticket (by voting for all candidates in that party's column), the office bloc format made casting a straight-party ballot somewhat less convenient. Because of the relative ease of casting a straight-party ballot with the party column format, it could be that the effect of the Australian ballot is more concentrated in electoral settings with such a format. We examine this hypothesis in this section by decomposing our results by ballot type. Examining outcomes related to government outputs (Table A.5), legislator effort (Table A.6), and legislator discretion from state parties (Table A.7), we do not uncover evidence indicating that ballot reform's effects were concentrated in places with a particular ballot format. For government outputs, five of the nine estimated coefficients for the office bloc format are in the hypothesized direction and none are statistically significant. In the case of legislator effort, there are several statistically significant estimates for both the party column and office bloc coefficients. However, all of these significant coefficients are not signed in the hypothesized direction. And, finally, for the roll-call discretion measure, all estimated coefficients are near to zero and non-significant.²⁵

²⁵See also results within Tables A.14-A.16. Again, none of the reported estimates provide evidence in support of the claim that office bloc (or party column) format in particular affected government outputs or legislator behavior as hypothesized.

	(1)	(2)	(3)
Panel A. Pensions			
Party Column	-0.086	-0.085	-0.031
	(0.055)	(0.047)	(0.037)
Office Bloc	-0.017	-0.023	-0.013
	(0.067)	(0.041)	(0.041)
Panel B. Post Offices			
Party Column	-0.000	0.035	-0.023
	(0.058)	(0.055)	(0.036)
Office Bloc	0.062	0.036	-0.043
	(0.063)	(0.055)	(0.043)
Panel C. Rivers & Harbo	rs		
Party Column	0.031	0.045	0.275
	(0.437)	(0.417)	(0.268)
Office Bloc	0.600	0.605	0.612
	(0.425)	(0.473)	(0.307)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark
Unit Fixed Effects	\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark
State-Specific Trends			\checkmark
Panel A Observations	800	800	800
Panel B Observations	893	893	893
Panel C Observations	532	532	532

Table A.5: The Australian Ballot & theDistribution of Federal Resources by Ballot Type

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged count of pensioners. The dependent variable for Panel B is the logged count of post offices. The dependent variable for Panel C is the logged appropriations to rivers and harbors projects. Control variables include log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Private Bill Sponsorship						
Party Column	-0.244*	-0.273*	-0.056	-0.167	-0.177	0.055
	(0.090)	(0.093)	(0.100)	(0.145)	(0.122)	(0.078)
Office Bloc	-0.082	-0.088	0.039	-0.002	0.001	0.105
	(0.150)	(0.138)	(0.114)	(0.170)	(0.151)	(0.093)
Panel B. Roll-Call Participation Rat	te					
Party Column	-0.039*	-0.040*	-0.053*	-0.008	-0.017	-0.020
	(0.016)	(0.017)	(0.024)	(0.018)	(0.017)	(0.025)
Office Bloc	-0.048	-0.047	-0.062	-0.038	-0.045	-0.026
	(0.030)	(0.025)	(0.034)	(0.029)	(0.024)	(0.030)
Panel C. Floor Speeches						
Party Column	-0.100	-0.184	-0.118	0.020	0.018	0.012
	(0.184)	(0.158)	(0.132)	(0.089)	(0.094)	(0.118)
Office Bloc	-0.009	-0.077	-0.130	-0.258*	-0.237*	-0.158
	(0.141)	(0.128)	(0.144)	(0.118)	(0.116)	(0.111)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
District Fixed Effects	\checkmark	\checkmark	\checkmark			
Member Fixed Effects				\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark		\checkmark	\checkmark
District/Member-Specific Trends			\checkmark			\checkmark
Panel A Observations	8,845	8,845	8,845	8,845	8,845	8,845
Panel B Observations	8,840	8,840	8,840	8,840	8,840	8,840
Panel C Observations	8,845	8,845	8,845	8,845	8,845	8,845

Table A.6: The Australian Ballot & Legislator Effort by Ballot Type

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged count of private bills introduced by the member. The dependent variable for Panel B is the roll-call participation rate (measured as a proportion). The dependent variable for Panel C is the logged count of floor speeches given by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member of any committee, committee fixed effects, and primary election status in the state. District- or member-specific trends are district-specific linear trends for specification (3) and member-specific linear trends for specification (6).

	(1)	(2)	(2)	(4)
	(1)	(2)	(3)	(4)
Party Column	-0.002	0.001	-0.004	-0.002
	(0.004)	(0.004)	(0.003)	(0.004)
Office Bloc	0.001	-0.002	-0.000	-0.003
	(0.004)	(0.005)	(0.004)	(0.005)
State FEs	\checkmark	\checkmark		
Congress FEs	\checkmark	\checkmark		
State-Party FEs			\checkmark	\checkmark
Congress-Party FEs			\checkmark	\checkmark
Unit-Specific Trends		\checkmark		\checkmark
Observations	746	746	746	746

Table A.7: Discretion from the State Party on Roll-Call Voting

Robust standard errors clustered by state are in parentheses. * p < 0.05. Dependent variable is the mean squared distance for the House party delegation of a state from the state's Senate party delegation. Unitspecific trends are state-specific linear trends for model (2) and state-party-specific linear trends for model (4).

A.7 Do Members Engage in Anticipatory Behavior?

For legislators from many states it is plausible that they were aware of ballot reform in advance of the subsequent election and, thus, altered their behavior in anticipation of reform. We test this hypothesis by using a leading indicator for ballot reform. Overall, we find scant evidence that legislators secured distributive goods, increased effort, or became more independent of their parties in anticipation. We report a single statistically significant estimated coefficient in the hypothesized direction (see column 6 of Panel C in Table A.9) versus five significant coefficients in the opposite direction (two of which are also in Panel C of Table A.9).

	(1)	(2)	(3)
Panel A. Pensions			
Australian Ballot $_{t+1}$	-0.024	-0.060	-0.016
	(0.055)	(0.044)	(0.036)
Panel B. Post Offices			
Australian Ballot $_{t+1}$	-0.005	-0.013	-0.056
	(0.042)	(0.039)	(0.042)
Panel C. Rivers & Harbo	rs		
Australian Ballot $_{t+1}$	0.366	0.301	0.199
	(0.333)	(0.360)	(0.221)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark
Unit Fixed Effects	\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark
State-Specific Trends			\checkmark
Panel A Observations	800	800	800
Panel B Observations	893	893	893
Panel C Observations	532	532	532

Table A.8: The Australian Ballot (in Forthcoming Election) &the Distribution of Federal Resources

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged count of pensioners. The dependent variable for Panel B is the logged count of post offices. The dependent variable for Panel C is the logged appropriations to rivers and harbors projects. Control variables include log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Private Bill Sponsorship						
Australian Ballot $_{t+1}$	-0.264*	-0.272*	-0.150	-0.160	-0.157	-0.069
	(0.109)	(0.104)	(0.089)	(0.132)	(0.115)	(0.084)
Panel B. Roll-Call Participation Rat	e					
Australian Ballot $_{t+1}$	-0.037	-0.034	-0.043*	-0.020	-0.019	0.001
	(0.025)	(0.023)	(0.020)	(0.026)	(0.022)	(0.024)
Panel C. Floor Speeches						
Australian Ballot $_{t+1}$	-0.215	-0.252*	-0.271*	-0.046	-0.033	0.242^{*}
	(0.135)	(0.108)	(0.123)	(0.126)	(0.120)	(0.113)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
District Fixed Effects	\checkmark	\checkmark	\checkmark			
Member Fixed Effects				\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark		\checkmark	\checkmark
District/Member-Specific Trends			\checkmark			\checkmark
Panel A Observations	8,845	8,845	8,845	8,845	8,845	8,845
Panel B Observations	8,840	8,840	8,840	8,840	8,840	8,840
Panel C Observations	8,845	8,845	8,845	8,845	8,845	8,845

Table A.9: The Australian Ballot & Legislator Effort

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged count of private bills introduced by the member. The dependent variable for Panel B is the roll-call participation rate (measured as a proportion). The dependent variable for Panel C is the logged count of floor speeches given by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member of any committee, committee fixed effects, and primary election status in the state. District- or member-specific trends are district-specific linear trends for specification (3) and member-specific linear trends for specification (6).

	(1)	(2)	(3)	(4)
Australian Ballot $_{t+1}$	-0.002	-0.001	-0.002	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)
State FEs	\checkmark	\checkmark		
Congress FEs	\checkmark	\checkmark		
State-Party FEs			\checkmark	\checkmark
Congress-Party FEs			\checkmark	\checkmark
Unit-Specific Trends		\checkmark		\checkmark
Observations	746	746	746	746

Table A.10: Discretion from the State Party on Roll-Call Voting

Robust standard errors clustered by state are in parentheses. * p < 0.05. Dependent variable is the mean squared distance for the House party delegation of a state from the state's Senate party delegation. Unit-specific trends are state-specific linear trends for model (2) and state-party-specific linear trends for model (4).

A.8 Excluding the South

Because southern States were so dominated by a single party, there is some concern that the inclusion of the South could dilute much of the effect of ballot reform. In this section, we exclude the 11 states of the former Confederacy. Again, these estimates excluding the South do not meaningfully change the substantive interpretation of any relationships.

Excluding the bound					
	(1)	(2)	(3)		
Panel A. Pensions					
Australian Ballot	-0.065	-0.001	-0.011		
	(0.053)	(0.024)	(0.021)		
Panel B. Post Offices					
Australian Ballot	-0.001	0.059	0.010		
	(0.051)	(0.038)	(0.019)		
Panel C. Rivers & Harbo	rs				
Australian Ballot	-0.946	-0.957	-0.087		
	(0.578)	(0.593)	(0.293)		
Congress Fixed Effects	\checkmark	\checkmark	\checkmark		
State Fixed Effects	\checkmark	\checkmark	\checkmark		
Controls		\checkmark	\checkmark		
State-Specific Trends			\checkmark		
Panel A Observations	602	602	602		
Panel B Observations	662	662	662		
Panel C Observations	400	400	400		

Table A.11: The Australian Ballot &the Distribution of Federal Resources,Excluding the South

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged count of pensioners. The dependent variable for Panel B is the logged count of post offices. The dependent variable for Panel C is the logged appropriations to rivers and harbors projects. Control variables include log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends. The South is defined as the 11 states of the former Confederacy.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Private Bill Sponsorship						
Australian Ballot	-0.049	-0.040	0.003	-0.044	-0.031	0.137
	(0.102)	(0.100)	(0.090)	(0.110)	(0.099)	(0.075)
Panel B. Roll-Call Participation Ra	te					
Australian Ballot	-0.008	-0.012	-0.015	0.002	-0.003	0.024
	(0.022)	(0.022)	(0.036)	(0.025)	(0.022)	(0.020)
Panel C. Floor Speeches						
Australian Ballot	-0.284	-0.255	-0.157	-0.202*	-0.187	0.039
	(0.168)	(0.155)	(0.187)	(0.093)	(0.095)	(0.082)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
District Fixed Effects	\checkmark	\checkmark	\checkmark			
Member Fixed Effects				\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark		\checkmark	\checkmark
District/Member-Specific Trends			\checkmark			\checkmark
Panel A Observations	6,637	6,637	6,637	6,637	6,637	6,637
Panel B Observations	6,632	6,632	6,632	6,632	6,632	6,632
Panel C Observations	6,637	6,637	6,637	6,637	6,637	6,637

Table A.12: The Australian Ballot & Legislator Effort, Excluding the South

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panel A is the logged count of private bills introduced by the member. The dependent variable for Panel B is the roll-call participation rate (measured as a proportion). The dependent variable for Panel C is the logged count of floor speeches given by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member of any committee, committee fixed effects, and primary election status in the state. District- or member-specific trends are district-specific linear trends for specification (3) and member-specific linear trends for specification (6). The South is defined as the 11 states of the former Confederacy.

	(1)	(2)	(3)	(4)
Australian Ballot	-0.000	0.004	-0.002	0.003
	(0.006)	(0.007)	(0.005)	(0.006)
State FEs	\checkmark	\checkmark		
Congress FEs	\checkmark	\checkmark		
State-Party FEs			\checkmark	\checkmark
Congress-Party FEs			\checkmark	\checkmark
Unit-Specific Trends		\checkmark		\checkmark
Observations	555	555	555	555

Table A.13: Discretion from the State Party on Roll-Call Voting,

 Excluding the South

Robust standard errors clustered by state are in parentheses. * p < 0.05. Dependent variable is the mean squared distance for the House party delegation of a state from the state's Senate party delegation. Unitspecific trends are state-specific linear trends for model (2) and state-party-specific linear trends for model (4). The South is defined as the 11 states of the former Confederacy.

A.9 District-Level Analysis of Rivers and Harbors

The data from Wilson (1986) measuring rivers and harbors projects and appropriations were collected at the U.S. House district level. For the sake of simplicity and concision, we aggregate the data to the state level given that the other government output outcome measures used in the paper (i.e., pensions and post offices) are all measured at the state level, and the treatment (implementation of the Australian ballot) is measured at the state level. While unsurprising, it is worth noting that results from a district-level analysis are very similar to the results from the state-level analysis in the paper. In Table A.14, we report results from a U.S. House district level analysis of the rivers and harbors data. Examining all 21 of the relevant estimated coefficients in Table A.14, none are statistically significant, and 11 out of 21 are positive. In other words, the district-level analysis does not provide any evidence that members elected via the Australian ballot were able to deliver more rivers and harbors appropriations/projects for their districts.

	(1)	(2)	(3)
Panel A. Main Specificat	ion: Logge	d Appropr	riations
Australian Ballot	-0.390	-0.236	-0.300
	(0.246)	(0.296)	(0.376)
Panel B Alternative Out	ved Proiec	t Count	
Australian Ballot	-0.017	-0.001	-0.019
	(0.043)	(0.045)	(0.051)
Panel C Decomposing h	v Ballot Tr	ine ,	· · ·
Party Column	-0 246	-0.095	-0 320
Tarty Column	(0.240)	(0.350)	(0.320)
Office Bloc	-0.801	-0 649	-0.835
Office Dioc	(0.481)	(0.578)	(0.544)
	(0.101)	(0.570)	(0.511)
Panel D. Anticipatory Le	gislator Be	ehavior	
Australian Ballot $_{t+1}$	0.151	0.188	-0.322
	(0.398)	(0.391)	(0.463)
Panel E. Excluding State	s in the So	uth	
Australian Ballot	-0.786*	-0.802*	0.069
	(0.319)	(0.313)	(0.433)
Panel F. Member Fixed E	ffects		
Australian Ballot	0.212	0.358	-0.682
	(0.304)	(0.433)	(0.465)
Congress Fixed Effects	\checkmark	\checkmark	\checkmark
District Fixed Effects	\checkmark	\checkmark	
Member Fixed Effects			\checkmark
Controls		\checkmark	\checkmark
Unit-Specific Trends			\checkmark
Panel A Observations	4,040	4,039	4,039
Panel B Observations	4,040	4,039	4,039
Panel C Observations	4,040	4,039	4,039
Panel D Observations	4,040	4,039	4,039
Panel E Observations	3,006	3,006	3,006
Panel F Observations	4,040	4,039	4,039

Table A.14: Rivers & Harbors Projects,District-Level Analysis

Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for Panels A, C, D, E, and F is the logged count of rivers and harbors projects. The dependent variable for Panel B is the logged rivers and harbors appropriations. Unit-specific trends are district-specific linear trends for Panels A-E and member-specific linear trends for Panel F.

A.10 Ideological Adaptation

To examine legislator responsiveness to constituent preferences, we utilize a similar approach to Gailmard and Jenkins (2009). We use the first dimension of Nokken-Poole (2004) scores from Lewis et al. (2022) to characterize the ideological nature of congressional voting behavior from 1876 to 1924. Following common practices in the literature, we characterize district preferences using district-level presidential vote shares. We then interact this term with the indicator for whether the secret ballot was in place. If the secret ballot strengthened the agency relationship between representatives and their constituents, we expect to observe a stronger relationship between constituent preferences and the roll-call behavior of their elected representatives.

The results are shown in Table A.15. Our within-member estimates—columns (3) and (4)—provide no evidence that, in the absence of the secret ballot, individual legislators exhibit changes in legislative behavior as the preferences of their constituents change. The estimated coefficient on Republican presidential vote share is small in magnitude and not statistically distinguishable from zero. This is the case for the main set of specifications reported in Panel A as well as all of the alternative sets of specifications reported in Panels B-D. The results also indicate that the secret ballot did not significantly change the nature of this relationship, as the interaction term is also small in magnitude and not statistically significant (again, this is the case across all sets of specifications reported in this table). These results suggest that members of Congress exhibit relatively consistent patterns of voting behavior over the courses of their career such that ballot reforms are unlikely to meaningfully shift their voting patterns in Congress.

Models (1) and (2) provide within-district estimates of legislative behavior. The coefficients for Republican vote share are positive and statistically significant (except for model (1) in Panel D), indicating that districts are more likely to select more conservative [liberal] representatives as the district becomes increasingly conservative [liberal]. However, the interaction between the Australian ballot and presidential vote share is relatively small in magnitude and statistically indistinguishable from zero, indicating that the secret ballot did not meaningfully change how legislators' voting records respond to district preferences. While we acknowledge the limitations of the data used in this analysis—for instance, the secret ballot may have also changed the degree to which presidential vote shares reasonably characterized constituent preferences—the results in Table A.15 do not support the hypothesis that the secret ballot caused

	(1)	(2)	(3)	(4)
	(1)	(2)	(3)	(4)
Panel A. Main Specification				
Australian Ballot × GOP Pres. Share	-0.006	-0.139	0.036	-0.010
	(0.106)	(0.117)	(0.051)	(0.048)
Australian Ballot	0.050	0.048	0.027	0.018
	(0.048)	(0.047)	(0.024)	(0.025)
GOP Pres. Share	0.479^{*}	0.472^{*}	0.023	0.044
	(0.120)	(0.143)	(0.053)	(0.056)
Panel B. Decomposing by Ballot Type				
Party Column \times GOP Pres. Share	0.026	-0.114	0.037	-0.009
	(0.123)	(0.139)	(0.053)	(0.052)
Office Bloc x GOP Pres Share	-0.156	-0.228	0.056	0.012
	(0.130)	(0.135)	(0.059)	(0.053)
Party Column	0.042	0.062	0.026	0.020
	(0.065)	(0.060)	(0.026)	(0.028)
Office Bloc	0.080	0.044	0.020	0.007
	(0.058)	(0.057)	(0.027)	(0.028)
GOP Pres Share	0 531*	0 496*	0.014	0.033
	(0.129)	(0.148)	(0.050)	(0.055)
	(0.12))	(0.110)	(0.000)	(0.000)
Panel C. Anticipatory Legislator Behavior				
Australian Ballot $_{t+1}$ × GOP Pres. Share	-0.177	-0.206*	0.021	-0.021
	(0.120)	(0.091)	(0.049)	(0.044)
Australian Ballot $_{t+1}$	0.076	0.036	0.033	0.022
	(0.056)	(0.046)	(0.021)	(0.020)
GOP Pres. Share	0.638^{*}	0.534^{*}	0.037	0.055
	(0.137)	(0.130)	(0.050)	(0.057)
Panel D Excluding States in the South				
Australian Ballot \times GOP Pres. Share	-0.674	-0.879	0.156	0.133
	(0.659)	(0.472)	(0.130)	(0.120)
Australian Ballot	0 418	0 413	-0.042	-0.062
	(0.383)	(0.262)	(0.070)	(0.069)
GOP Pres Share	1 197	1 215*	-0 100	-0.091
	(0.667)	(0.440)	(0.123)	(0.125)
	(0.007)	(0.110)	(0.120)	(0.120)
District Fixed Effects	\checkmark	\checkmark	/	/
Member Fixed Effects		/	\checkmark	V
Congress Fixed Effects	7 7 4 0	√ 	7 7 40	√
Panel A Observations	/,/12	/,/12	/,/12	/,/12
Panel B Observations	7,712	7,712	7,712	7,712
Panel C Observations	7,/12	7,/12	7,/12	7,712
Panel D Observations	5,648	5,648	5,648	5,648

Table A.15: The Australian Ballot & Responsiveness

Robust standard errors clustered by state are in parentheses. * p < 0.05.

The dependent variable for all panels is the first dimension of Nokken-Poole scores.

legislators to become *more* responsive to district preferences.

Additionally, we examine the degree to which the secret ballot may have dampened party loyalty in the House using party unity scores. We use legislators' party unity scores from 1880 to 1930.²⁶ The results from several specifications are shown in Table A.16. Across various specifications of our withindistrict and within-legislator models, we find consistently negative coefficient estimates for the effect of the secret ballot, suggesting that the secret ballot reduced legislators' fidelity to their political parties. For our main set of specifications in Panel A, none of the estimates is statistically significant. Of the 30 estimated coefficients, we only observe two statistically significant estimates. Both of these significant estimated coefficients are in Panel B when the results are decomposed by ballot type, indicating that the office bloc ballot format may have resulted in a decreased party loyalty. However, the magnitude of all the estimates are extremely small. The dependent variable is measured on a scale ranging from zero to 100, indicating the percentage of roll calls in which a legislator votes with a majority of his party against a majority of the opposite party. The largest coefficient is -2.6, which indicates that, at most, the secret ballot reduced a legislators' level of party unity by about 2.6 percentage points. The small magnitudes of these estimates provide little evidence that the secret ballot severed legislators' commitments to their political parties.

²⁶These data were obtained from https://legacy.voteview.com/k7ftp/House_Party_Unity_35-113.xls (accessed March 25, 2019).

	(1)	(2)	(3)	(4)	(5)	(6)	
Panel A. Main Specification							
Australian Ballot	-0.954	-0.738	-1.522	-0.653	-0.456	-0.995	
	(1.069)	(0.934)	(1.153)	(0.938)	(0.969)	(1.306)	
Panel B. Decomposing by Ballot Ty	ре						
Party Column	-0.086	-0.138	-0.758	-0.127	-0.025	-0.842	
	(0.929)	(0.822)	(1.184)	(1.052)	(1.029)	(1.387)	
Office Bloc	-1.545	-0.817	-1.989	-1.861*	-1.651	-2.627*	
	(1.292)	(1.210)	(1.489)	(0.911)	(0.970)	(1.255)	
Panel C. Anticipatory Legislator Be	ehavior						
Australian Ballot $_{t+1}$	-0.440	0.159	0.142	-0.264	-0.130	-1.476	
	(1.220)	(1.173)	(1.224)	(0.927)	(0.923)	(0.864)	
Panel D. Excluding States in the So	uth						
Australian Ballot	-1.428	-0.535	-2.244	-0.001	0.055	-0.611	
	(1.614)	(1.219)	(1.791)	(1.315)	(1.172)	(1.669)	
Congress Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	
District Fixed Effects	\checkmark	\checkmark	\checkmark				
Member Fixed Effects				\checkmark	\checkmark	\checkmark	
Controls		\checkmark	\checkmark		\checkmark	\checkmark	
District/Member-Specific Trends			\checkmark			\checkmark	
Panel A Observations	8,826	8,826	8,826	8,826	8,826	8,826	
Panel B Observations	8,826	8,826	8,826	8,826	8,826	8,826	
Panel C Observations	8,826	8,826	8,826	8,826	8,826	8,826	
Panel D Observations	6,620	6,620	6,620	6,620	6,620	6,620	
							1

Table A.16: The Australian	Ballot & Party Unit	y
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Robust standard errors clustered by state are in parentheses. * p < 0.05. The dependent variable for all panels is the party unity score. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member of any committee, committee fixed effects, and primary election status in the state. District- or member-specific trends are district-specific linear trends for specification (3) and member-specific linear trends for specification (6).

A.11 Inclusion of FY 1882

As noted in the data section of the paper, due to a gap in our panel between FY1882 and FY1886, we exclude observations from FY1882 for the main analyses in the paper. In Table A.17, we report results including the FY1882 observations in our panel. As with the primary results reported in Panel A of Table 2, the estimated coefficients across all three specifications are of substantively small magnitude and not statistically significant.

	ln(Pens	ln(Pensioner Count + 1)			ion Roll V	alue + 1)
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	-0.043	-0.050	-0.036	-0.027	-0.035	-0.001
	(0.054)	(0.041)	(0.041)	(0.057)	(0.047)	(0.052)
Year Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
State Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark		\checkmark	\checkmark
State-Specific Trends			\checkmark			\checkmark
Observations	838	838	838	800	800	800

Table A.17: Pensions Rolls and the Australian Ballot, Including Observations FY1882

Robust standard errors clustered by state are in parentheses. * p < 0.05.

Dependent variable for models (1)-(3) is the log of the count of pensionsers on the roll in the state, and dependent variable for models (4)-(6) is the log of the annual value (\$) of the pension roll in the state. Control variables include log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.

A.12 The Secret Ballot and the Media Environment

We explore whether the secret ballot meaningfully changed the media and information environment in which voters decided between candidates. If the secret ballot improved representation by strengthening the accountability mechanism, voters would have needed to have sufficient information about the candidates between which they could choose. We explore whether the secret ballot was accompanied by a shift in the media environment that reflected a more candidate-centered informational context. To do so, we use data from Ban et al. (2019) on the newspaper coverage in each state provided to political candidates. These data are the most comprehensive data available on the nature of media coverage of politics during the period in which the secret ballot was adopted, reflecting "nearly 50 million historical newspaper pages from 2,700 local US newspapers over the years 1877-1977" (Ban et al. 2019, 1). The measure of candidate coverage reflects the number of news stories dedicated where candidates for major offices, including governor, US senator, and US House, are mentioned. Ban et al. (2019) identify newspaper stories that use the word-stem "candidate" in close proximity to a mention of one of these political offices. To be clear, our approach does not allow us to distinguish as to whether candidates *sought* greater coverage or whether the media opted to provide more coverage of candidates in response to the new electoral environment (post-ballot reform).

We follow the empirical approach from the text and estimate models for the period from 1880 to 1920 and include state and year fixed effects. In Table A.18, we examine whether the secret ballot was associated with an increased number of news stories about political candidates. The dependent variable in models (1)-(3) is the raw number of news stories about political candidates in the state in a given year; because this quantity is positively skewed, models (4)-(6) report results using the logged (+1) value of this variable. Models (1) and (4) are bivariate regressions with state and year fixed effects; models (2) and (5) include control variables to account for population (logged), whether the state used the direct primary, and past Republican share of the two-party presidential vote; and models (3) and (6) include state-specific linear trends. Table A.19 reports results from similar models; here, however, we index for the general level of political coverage newspapers provided by creating a measure used in Ban et al. (2019) on the amount of political coverage dedicated to candidates *relative* to political party committees. The dependent variable in models (1)-(3) is the percentage of stories about candidates and ranges from zero to 1, and the dependent variable in models (4)-(6) is the logged value of this quantity plus one. As before, we cluster standard errors on state in all models.

Neither table provides evidence that the secret ballot systematically increased the supply of information about political candidates. A third of the coefficients are negatively signed, opposite what we would expect if the secret ballot was accompanied by a growth in political coverage that could help voters distinguish between competing candidates. Moreover, all of the coefficients are imprecisely estimated, with none of them larger in magnitude than their standard errors. The results from these tables suggest the absence of a potential mechanism that could have

helped improve the quality of voter decision making and thereby strengthen the incentives for officeholders to represent their constituents. If voters did not have more access to candidate-specific political information—either because candidates did not perceive the incentives to secure it or because newspaper editors did not perceive the incentives to print it—it is unlikely that ballot reform would have meaningfully affected their choices. These results suggest some of the scope conditions of our findings and raise the possibility that the secret ballot could have had greater representational consequences in a more information-rich and candidate-centered environment.

	Candidate Mentions			ln(Candidate Mentions + 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	-127.442	-75.609	543.047	0.082	0.080	0.136
	(835.548)	(818.515)	(585.684)	(0.187)	(0.191)	(0.186)
Year Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
State Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark		\checkmark	\checkmark
State-Specific Trends			\checkmark			\checkmark
Observations	1,374	1,374	1,374	1,374	1,374	1,374

Table A.18: Candidate Newspaper Coverage and the Australian Ballot

Robust standard errors clustered by state are in parentheses. * p < 0.05.

Dependent variable for models (1)-(3) is the number of news stories in the state, and dependent variable for models (4)-(6) is the logged number of news stories in the state. Control variables include log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.

	Percent Mentions			ln(Percent Mentions + 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	0.010	0.007	-0.001	0.007	0.005	-0.000
	(0.013)	(0.014)	(0.007)	(0.009)	(0.010)	(0.004)
Year Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
State Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
Controls		\checkmark	\checkmark		\checkmark	\checkmark
State-Specific Trends			\checkmark			\checkmark
Observations	1,374	1,374	1,374	1,374	1,374	1,374

Table A.19: Relative Candidate Newspaper Coverage and the Australian Ballot

Robust standard errors clustered by state are in parentheses. * p < 0.05.

Dependent variable for models (1)-(3) is the percentage of news stories on political candidates out of total stories on candidates and parties. The dependent variable for models (4)-(6) is the logged value of this quantity plus one. Control variables include log population, primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.