

Democracy Thrives in Secret? Ballot Reform and Representation in the United States*

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Abstract

The secret ballot is widely considered to be an essential feature of fair and free democratic elections. Despite normative claims about its importance for democracy and theoretical claims from proponents about its effects on democratic performance, empirical scholarship on the secret ballot is limited by the range of dependent variables it considers and the research designs it employs. In this paper, we study how the introduction of the secret ballot in the United States affected officeholder performance and political representation using both original and existing data from 1880 to 1930. Across a host of measures of legislative effort, our results are consistently null. These patterns are robust across model specifications, characterizations of the dependent variables, and subsets of observations. Our findings revise conventional understandings about the contributions of the Australian ballot to American democracy and have important implications for understanding the link between electoral institutions and political representation.

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The turn of the twentieth century was arguably the most important period in the development of the United States Congress. Not only did a series of internal reforms reshape the legislative process within the U.S. House of Representatives, but state-level reforms fundamentally changed how legislators were elected. Following accounts of widespread corruption and voter intimidation during the closely contested 1888 presidential election, Progressives embraced ballot reform as a means of reducing the influence of political parties and strengthening the agency relationship between voters and officeholders. 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.”¹ The secret ballot was quickly adopted by most states (including New York) and is commonly credited with “[ushering] in a new era of candidate-centered politics” (Engstrom and Kernell 2014, 191-192).² Ballot reform occupies a central role in normative debates over the performance and meaning of democratic citizenship (e.g., Brennan and Pettit 1990; Mill 1991 [1861]) and scholarship on democratization argues that “modern representative government is unthinkable” (Teorell, Ziblatt, and Lehoucq 2017, 2) absent the secret ballot.

In this paper, we study the effect of the secret ballot on legislative behavior and congressional representation. Existing scholarship offers competing accounts about its potential effects. According to one perspective, the secret ballot “profoundly altered the electoral environment faced by members of Congress” by enabling voters to cast votes for individual candidates, rather than parties (Katz and Sala 1996, 22). This account suggests that voters’ newfound ability to distinguish candidates by their performance in office produced incentives for legislators to invest in activities that would generate a personal vote (e.g., Ansolabehere, Snyder, and Stewart 2000; Katz and Sala

¹“No Place for Partisanship,” May 3, 1888, page 4.

²We use the terms *secret ballot* and *Australian ballot* interchangeably to describe state-level ballot reforms that required the government printing of ballots and ensured secrecy in the voting booth.

1996; Rusk 1970). An alternative perspective notes that the incentives for election-seeking behavior among legislators were already in place prior to the secret ballot such that it was unlikely to have had substantial effects on legislative behavior. This perspective emphasizes that members of Congress serving prior to ballot reform already were sophisticated and career-oriented (Kernell 1977; Stewart 1989), dedicated significant time and resources to constituency service (e.g., Bryce 1995 [1888]), and recognized the potential for constituents to vote them out of office for poor performance (e.g. Bianco, Spence, and Wilkerson 1996; Carson and Engstrom 2005; Carson and Sievert 2018; Jenkins and Nokken 2008; Jenkins and Weidenmier 1999). Related accounts suggest that voters' inability to distinguish candidates across offices prior to the secret ballot has been overstated (e.g., Bense 2004; Cox and Kousser 1981; Reynolds and McCormick 1986; Rusk 1970).

Motivated by related scholarship on electoral institutions and legislative behavior (e.g., Fourinaies and Hall 2018; Gailmard and Jenkins 2009; Hirano 2006; Hirano et al. 2010), we provide the most comprehensive study to date on the effects of the secret ballot. To do so, we build upon the few existing studies on the secret ballot (e.g., Carson and Sievert 2015; Finocchiaro and MacKenzie 2018; Katz and Sala 1996; Squire et al. 2004; Wittrock et al. 2008), employing a research design well-suited for identifying the effects of institutional change. We examine how the secret ballot affected legislators' attentiveness to district interests, which we measure with data on government outputs and legislators' direct efforts to secure federal benefits on behalf of their constituents.³

We test our hypotheses using original and existing data sources on legislative behavior from approximately 1880 to 1930 and a panel design.⁴ Our results are consistently null. Across a number of measures of government outputs and legislative behavior, we find no evidence that the secret ballot affected the provision of legislator effort. These patterns are robust across model specifications, characterizations of the dependent variables, and subsets of observations. We dis-

³A related body of research finds that the secret ballot reduced voter turnout (Cox and Kousser 1981; Engstrom 2012; Engstrom and Kernell 2014; Heckelman 1995).

⁴As we explain below, the start and end years of our analyses vary to some degree across measures based on data availability.

cuss and present evidence for two plausible explanations: first, that the Australian ballot did not meaningfully sever legislators from the control of state and local political parties and, second, that legislators devoted substantial attention to constituency concerns even before the introduction of the secret ballot. In combination, these factors suggest that the secret ballot had more limited consequences on political representation than its proponents had promised. The findings revise conventional understandings about the contributions of the Australian ballot to American democracy and have important implications for understanding the link between electoral institutions and political representation.

Electoral Institutions and Legislative Behavior

Representative government turns on the capacity of citizens to sanction and reward elected officials for their performance in office. The authors of the U.S. Constitution sought to construct electoral institutions that provide adequate incentives for legislators to represent their constituents. Reflecting these concerns, in *Federalist 52* Madison argued that members of the U.S. House “should have an immediate dependence on, and an intimate sympathy with, the people. Frequent elections are unquestionably the only policy by which this dependence and sympathy can be effectually secured.” Contemporary theories of the U.S. Congress emphasize how legislators’ electoral incentives shape their behavior (Mayhew 1974), and an important body of empirical scholarship documents how electoral incentives affect officials’ policy choices (e.g., Alt, Bueno de Mesquita, and Rose 2011), voting records (e.g., Gailmard and Jenkins 2009), provision of government resources (e.g., Kriner and Reeves 2015), and allocation of effort (e.g., Fournaies and Hall 2018).

During the first century of American elections, though, balloting procedures did not seem to maximize the agency relationship between voters and elected representatives. Election ballots were created and separately distributed by political parties rather than by state or local governments. These party-printed ballots listed all of the party’s candidates, such that a vote for any one

particular candidate was essentially a vote for all of the party's candidates. Furthermore, ballots were deposited publicly, so the general public, including party officials, could see an individual voter's choices (Allen 1910). 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 reward or sanction particular officeholders based on their performance. This system may have provided little incentive 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, nineteenth-century legislators are often believed to have privileged their party's priorities at the expense of their constituents' interests. The net result, as Progressive reformers seemingly believed, "was bad for democracy and competition overall" (Carson and Roberts 2013, 8). In an early publication, political observer Henry George (1883, 211) lamented that "democratic government is with us becoming a failure," in large part because "parties only operate as to the heads of tickets. It is only as to a few of their nominees that party managers need take public opinion into account; the fortunes of the rest, save in very rare instances, depend upon the fate of the head of the ticket" (207).

After a series of hotly contested presidential elections and increased partisan conflict within Congress, Progressives identified ballot reform as a way to address the stranglehold they perceived parties to have over American politics in the late nineteenth century. They argued that the party-printed ballot contributed to corruption and limited voters' control over elected officials (Engstrom and Kernell 2014; Katz and Sala 1996). According to Evans (1917, 21), party corruption and inattentiveness had thoroughly frustrated constituents and thus "created a favorable attitude on the part of a majority of voters" toward reform. George (1883, 208) argued 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.

With the Australian ballot, voters viewed all parties' candidates on a single ballot, which they cast privately. It was first adopted statewide in Massachusetts in 1888 and quickly spread around the country, with 36 states implementing it within a decade.⁵

The Consequences of Ballot Reform for Political Representation

We test the hypothesis that the secret ballot affected legislative behavior by strengthening the agency relationship between legislators and constituents. Its introduction is widely credited with marking a turning point for political representation and electoral accountability in American politics. These reforms enabled voters to split their votes across party lines and select candidates on the basis of their personal characteristics (Katz and Sala 1996), creating incentives for candidates and officeholders to distinguish themselves from their opponents (Engstrom and Kernell 2014). These changes, Carson and Jenkins (2011, 39) argue, “firmed up the agency relationship by increasing accountability; members of Congress now had a greater incentive to be responsive to voters’ needs.” As Reynolds and McCormick (1986, 836) succinctly summarize the accepted wisdom, “The Australian ballot thus weakened the party machine and encouraged electoral independence.”

Other stylized facts about the nineteenth-century Congress, however, suggest that the secret ballot may have had more limited effects than its proponents argued. First, as Carson and Sievert (2018, 7-8) argue, parties had incentives to field strong candidates for office prior to the secret ballot, suggesting that potential officeholders needed to exhibit characteristics that distinguished them from would-be competitors. Even though parties may have dominated the election machinery, legislative responsiveness to constituent interests may have been high even absent the secret ballot. Second, patterns of careerism in the U.S. House predate the secret ballot by several decades

⁵States used two versions of the secret ballot. In the *office bloc*, candidates are grouped by office and their names may or may not be accompanied by their political party. In contrast, the *party column* ballot lists all candidates under the party label, making it easier to vote a straight-party ticket. The office bloc ballot therefore marked a stronger contrast to the party-printed ballot. While our primary interest is in estimating the effect of the adoption of the secret ballot, we report results from models that distinguish the effects of these different ballot formats.

(Kernell 1977). These patterns suggest that members were able to fashion increasingly long careers prior to the introduction of the secret ballot, perhaps by cultivating personal votes. Third, even though turnover rates among legislators were higher in the late nineteenth-century than in the modern Congress, the legislators who served during this time period seem to have been closely attuned to the electoral consequences of their behavior (Bianco, Spence, and Wilkerson 1996; Stewart 1989). For instance, Cooper and Young (1989) argue that House members changed the rules of bill introductions as early as the 1830s to enhance their ability to secure legislative accomplishments; following the Civil War, moreover, legislators recognized the importance of pensions to their future electoral success. Observing legislators' efforts to satisfy constituency requests, James Bryce (1995 [1888], 197) observed that "[a]n ambitious congressman is therefore forced to think day and night of his re-nomination," and attempts to secure it with "grants from the Federal treasury for local purposes." Analyses of legislators' votes on salient roll calls in the nineteenth century further indicate that legislators behaved in ways that reflected their electoral concerns (e.g., Bianco, Spence, and Wilkerson 1996; Carson and Engstrom 2005; Jenkins and Weidenmier 1999; Jenkins and Nokken 2008). Finally, anecdotal evidence suggests that it was not altogether uncommon for voters to split their tickets before the secret ballot (Bensel 2004; Reynolds 2006; Reynolds and McCormick 1986). Overall, these accounts suggest that there may have been greater continuity across the premodern and modern Congresses in the nature of legislators' electoral incentives and their efforts to represent constituent demands, perhaps limiting the magnitude of the secret ballot's effects on patterns of representation.

Despite the importance attributed to the Australian ballot for congressional behavior, relatively few studies have focused on the possible link between ballot reform and legislative behavior. Those that do, meanwhile, reach somewhat varying conclusions across dependent variables and empirical strategies. Much of this research focuses on committee assignments. For instance, one line of research finds that the secret ballot decreased turnover rates in committee membership among members of Congress and, similarly, that members from states with office-bloc formatted

ballots had more favorable committee assignments (Katz and Sala 1996; Wittrock et al. 2008). On the other hand, other studies using alternative research designs or examining legislative behavior in other settings (such as state legislatures) do not find a link between ballot type and committee assignments or the value of a committee portfolio (Carson and Sievert 2015; Squire et al. 2004).

Additional research examines the association between the secret ballot and other dimensions of legislative behavior. These findings indicate that the secret ballot led members of Congress to exhibit greater independence in their roll call voting behavior, as levels of party unity decreased following its introduction (Carson and Sievert 2015; Wittrock et al. 2008).⁶ The secret ballot also seemingly increased legislators' attentiveness to the provision of constituency service and pork projects. Using the most extensive dataset assembled to date on bill sponsorships during this time period, Finocchiaro and MacKenzie (2018) show that the secret ballot was associated with the introduction of more private bills on behalf of constituent requests. Similarly, the secret ballot was also associated with greater spending for rivers and harbors projects (Wittrock et al. 2008). Finally, the introduction of the secret ballot decreased the probability that vulnerable incumbents pursued re-election, though the results are somewhat inconsistent across model specification (Carson and Sievert 2015).

The research described above, however, is limited in several key ways. Substantively, scholars have focused on a relatively small number of dependent variables, many of which provide only indirect measures of legislative behavior.⁷ Methodologically, previous studies employ research designs that do not distinguish the potential effects of the secret ballot from other possible confounding variables. Instead, credible estimates of the effects of the secret ballot must account both for secular trends in patterns of congressional activity and district- or representative-level sources of variation in legislative effort.

⁶Wittrock et al. (2008) again show that these findings are limited to states that used the office bloc ballot.

⁷For instance, Katz and Sala (1996) and Wittrock et al. (2008) study the relationship between the secret ballot and committee assignments but do not provide evidence that legislators' committee assignments were a valuable contributor to their electoral goals.

We study the effects of the secret ballot on two main aspects of representation. First, we test whether the secret ballot increased the supply of government outputs. If the secret ballot increased legislators' incentives to exert costly effort on behalf of their constituents, we expect to observe increased resource flows to constituents in relevant communities. Second, we examine whether the secret ballot increased legislators' personal efforts to secure benefits for constituents. During this era, constituents frequently sought assistance from legislators in securing pensions, claims, and other forms of relief from the federal government, for which members of Congress often introduced private legislation. If the secret ballot increased the incentives for legislators to develop personal reputations with their constituents, we expect that legislators would initiate these efforts more frequently upon its adoption. We now describe the data and methods we use to test our expectations.

Data

We test the effects of the secret ballot using two sets of dependent variables and examine its effects on government outputs and legislative behavior. These data are summarized in Table 1.

We use data on three kinds of federal outputs. First, we assembled original annual data on the 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.⁸ 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

⁸Generally speaking, we averaged 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, 1884, 1885, 1886, and 1888. We are able to aggregate up county-level totals, which are available in a very limited number of years, to the state level for FY1886 and FY1888. Additionally, data are unavailable on the total dollar amount for fiscal years 1889-1891. For the analyses we report in the text, we exclude observations from fiscal year 1882 so as to have a balanced panel for the number of pensioners on the roll from FY1886 to FY1920. Results are insensitive to the inclusion (or exclusion) of FY1882 (see Table A.1 in the Appendix).

particularistic goods (Finocchiaro and Jenkins 2016; Jensen 2003). The pension system expanded dramatically following the Civil War and by the turn of the twentieth century had evolved into a “de facto system of old age and disability protection” (Orloff and Skocpol 1984, 728). Beyond voting to expand 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 (Finocchiaro 2008; Skocpol 1992). If the secret ballot increased the incentives for members 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 distribution of post offices from 1876 to 1896. We supplement the data reported in Rogowski (2016) on post offices in each county from 1876 to 1896 with original data collection that extends the time series through 1916. According to Fuller (2003, xi), “No arm of the national government . . . was as closely connected to American politics or subject to more pressures from various interest groups.” Local communities frequently mobilized around and petitioned for expansion of postal services,⁹ and Kernell and McDonald (1999) provide evidence of the constituency-induced political incentives that affected congressional action in relation to postal services. Therefore, we test the hypothesis that 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 at the district level from 1889 to 1913.¹⁰ Rivers and harbors were among the most important set of 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

⁹As President Hayes (1881, 337) wrote in his final message to Congress: “All the inhabitants of the country are directly and personally interested in having proper mail facilities, and naturally watch the post office very closely. This careful oversight on the part of the people has proved a constant stimulus to improvement.”

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

natural opportunities for credit-claiming by legislators. 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.

Table 1: Summary of Dependent Variables

Outcome	Unit	Start year	End year	Min	Max	Source
<i>Government outputs</i>						
Pensions (number)	state	1882	1920	66	105,746	original
Pensions (amount, \$)	state	1882	1920	\$6,840	\$20,944,580	original
Post offices (number)	state	1876	1916	90	5,342	Rogowski (2016) + original
Post offices (per capita)	state	1876	1916	0.21	4.30	Rogowski (2016) + original
Rivers and harbors (number)	district	1889	1913	0	48	Wilson (1986)
Rivers and harbors (amount, \$)	district	1889	1913	\$0	\$3,000,000	Wilson (1986)
<i>Legislative behavior</i>						
Private pension bills (House)	district	1881	1931	0	661	Finocchiaro and MacKenzie (2018)
Private non-pension bills (House)	district	1881	1931	0	268	Finocchiaro and MacKenzie (2018)

Table provides descriptive statistics, time periods, and units of analysis for dependent variables. All outcomes are measured per Congress. All dependent variables in this table except post offices per capita are log-transformed prior to analysis due to the highly skewed nature of the underlying distributions (see Figure A.1).

These three measures—pensions, post offices, and rivers and harbors projects—provide a comprehensive portrait of the provision of constituency service around the turn of the twentieth century. During this period, the post office was “one of the most important institutions of the day” (John 1995, vii) as the largest federal employer and “in the closeness of its relations to the great mass of people” (White 1958, 258). The pension system was the most important social welfare program prior to the New Deal era and for many years was the largest source of federal expenditures (Finocchiaro 2008). 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). Altogether, then, these measures serve as good indicators of legislators’ effort to provide constituency service (Ashworth and Bueno de Mesquita 2006).

In a second set of analyses, we study whether the secret ballot affected legislative behavior in more directly observable ways. We focus specifically on the introduction of private legislation in the U.S. House. Private legislation was generally 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 the impressive data collection from Finocchiaro and MacKenzie (2018) on the number of private bills introduced in the U.S. House between 1881 and 1931 (the 47th through the 71st Congresses). Using these data, we test the hypothesis that the secret ballot increased legislators’ efforts to address private claims. We separately examine both pension-related private legislation and other private bills.

Empirical Strategy

We leverage the panel nature of the data described in Table 1 and use a differences-in-differences design to estimate the effects of the secret ballot on legislative behavior. 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:

$$Y_{it} = \beta_0 + \alpha_i + \delta_t + \beta_1 \text{Australian Ballot}_{it} + \mathbf{X}\Omega_{it} + \epsilon_{it}, \quad (1)$$

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 in year t by the relevant state.¹² The coefficient for β_1 thus is the key parameter of interest. If the secret ballot increased legislative effort, we expect to observe positive coefficient estimates for β_1 . We include unit fixed effects (α_i) to control for observed and unobserved

¹¹Today, these matters would generally be referred to legislative staff and/or the bureaucracy (Finocchiaro and MacKenzie 2018).

¹²Figure A.5 displays the timing of states’ adoption of the secret ballot.

time-invariant attributes that may 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 the nature of representation. For data that are available at the district level, for instance, we estimate separate models which include legislator and district fixed effects.¹³ The former allows us to account 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 We also include time fixed effects (δ_t) to account for secular trends in legislative behavior, legislative capacity, and constituent demands. Additionally, we include a matrix (\mathbf{X}_{it}) of other unit, legislator, and contextual characteristics that may also be related to legislative behavior and productivity, which are explained below in greater detail. Finally, β_0 is a constant term, Ω_{it} is a vector of coefficients for the control variables, and ϵ_{it} is a random error term, clustered on state.

Using 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 our outcomes 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, 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 of legislator behavior.

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.

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

¹⁵The raw data appear to satisfy the parallel trends assumption for most of our outcomes, as we observe similar trends in outcomes among states that adopted the secret ballot at different points in time prior to its adoption in the early adopting states. See Figures A.2 and A.4.

While other studies have used comparisons of the same legislators before and after reforms were implemented (e.g., Carson and Sievert 2015), this approach does not account for time trends in legislative behavior 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 (Primo, Jacobsmeier, and Milyo 2007).

Results

We begin by examining the effect of the secret ballot on government outputs. Table 2 displays the results for our two pension outcomes across three specifications.¹⁷ The count of pensioners on the rolls in the state and the total annual value of pensions in the state both gauge the ability of constituents to gain access to the pension rolls and to receive increases in their pensions. During this time period, both anecdotal evidence and the enormous amount of correspondence from members of Congress that the Bureau of Pensions reported receiving points to pension assistance as an extremely important source of constituency service.¹⁸ Did the arrival of the Australian ballot strengthen the connection between officeholders and constituents, encouraging legislators to work harder at helping their constituents gain access to the pension rolls? The first specification in Table 2—models (1) and (4)—is a standard difference-in-differences approach without any time-varying covariates. In the second specification—models (2) and (5)—we include time-

¹⁶This issue also presents a threat to inference for the pooled cross-sectional studies, 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.

¹⁷As is clear in the tables containing our results, nearly all of our outcome measures are log-transformed because their underlying distributions are highly skewed. See Figure A.1 in the Appendix for the distributions of these variables in their original form and log-transformed. We do not log-transform the post offices per capita measure, as its underlying distribution is less severely skewed.

¹⁸During his time in the House, Robert La Follette (1911, 84) estimated that a quarter to a third of his time was spent looking up pension cases at the Pension Office, not including time he dedicated to private pension legislation.

varying control variables. Finally, in the third specification—models (3) and (6)—in addition to time-varying covariates, we also include state-specific linear trends.¹⁹ Across all three specifications, for both the logged pensioner count and the logged value of the pension roll outcomes, the estimated coefficient on the Australian ballot indicator is stable, is near to zero in magnitude, and is not statistically significant. In other words, we find no evidence linking the Australian ballot to increases in the pension rolls in terms of the count of pensioners or the dollar value of the pensions. This finding suggests that when a state implemented ballot reform it did not induce greater effort among the state’s representatives in terms of assisting constituents with access to the pension rolls and increases in their pensions.

Table 2: Pension Rolls and the Australian Ballot

	ln(Pensioner Count + 1)			ln(Pension Roll Value + 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	-0.061 (0.052)	-0.069 (0.040)	-0.038 (0.038)	-0.046 (0.057)	-0.055 (0.045)	-0.015 (0.046)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
State-Specific Trends			✓			✓
Observations	800	800	800	762	762	762

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 pensioners 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.

In Table 3, we examine two outcomes measuring the provisioning of post offices in each state: the logged count of post offices and the count of post offices per capita. As noted above, the post office was the primary means through which citizens interacted with the federal government during this time period and was the one of few services provided by the federal government. Again,

¹⁹The inclusion of state-specific linear trends weakens our reliance on the parallel trends assumption and helps address the possibility that the introduction of the secret ballot is correlated with other trends in the state-level provision of government outputs.

Table 3: Post Offices and the Australian Ballot

	ln(Post Office Count + 1)			Post Offices Per Capita		
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	-0.007 (0.046)	-0.003 (0.041)	-0.049 (0.034)	0.013 (0.051)	0.012 (0.044)	-0.030 (0.056)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
State-Specific Trends			✓			✓
Observations	893	893	893	893	893	893

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 post offices in the state, and dependent variable for models (4)-(6) is the count of post offices per 1,000 in the population in the state. Control variables include log population (excluded from models 4-6), primary election status, and past Republican share of the two-party presidential vote.

State-specific trends are state-specific linear trends.

across our three specifications for both outcome measures, the estimated coefficient on the Australian ballot variable is highly stable and very near to zero. As in our pension analyses, we find no evidence of a link between a state's implementation of the secret ballot and the provisioning of post offices within the state.

Finally, we examine patterns in the allocation of rivers and harbors projects. In the period after the civil war through the late nineteenth and into the early twentieth century, funding for these projects grew enormously. The line items in these bills allowed enterprising representatives to direct substantial funding for these projects to their districts. Unlike the outcomes related to pensions and post offices, the data for the rivers and harbors outcomes from Wilson (1986) are measured at the House district level rather than the state level. As such, we employ six specifications across our two outcomes (logged rivers and harbors appropriations and the logged count of rivers and harbors projects). The first three specifications use member fixed effects, while the latter three specifications include district fixed effects (all six specifications include Congress fixed effects). Specifications (2) and (4) then incorporate a set of time-vary controls,

Table 4: Rivers & Harbors and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dep. Variable: $\ln(\text{Appropriations} + 1)$</i>						
Australian Ballot	0.198 (0.314)	0.331 (0.438)	-0.631 (0.432)	-0.383 (0.239)	-0.231 (0.292)	-0.305 (0.380)
<i>Panel B. Dep. Variable: $\ln(\text{Project Count} + 1)$</i>						
Australian Ballot	0.028 (0.042)	0.050 (0.047)	-0.073 (0.061)	-0.017 (0.042)	-0.001 (0.045)	-0.022 (0.052)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Panel A Observations	4,090	4,090	4,090	4,090	4,090	4,090
Panel B Observations	4,090	4,090	4,090	4,090	4,090	4,090

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

Dependent variable for Panel A is the log of rivers & harbors appropriations (\$) in the district, and dependent variable for Panel B is the log of the count of rivers & harbors projects in the district. 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. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

and specifications (3) and (6), respectively, supplement the models with member-specific and district-specific linear trends.²⁰ These models allow us to test whether members (or districts) that experienced ballot reform increased the provisioning of rivers and harbors projects in comparison to members (or districts) during the same years that had not experienced ballot reform.

Across all six specifications for both outcomes, we fail to reject the null hypothesis that the estimated coefficient on the Australian ballot indicator is equal to zero. In the case of the six specifications for the logged count of projects (Panel B), the estimated coefficient of interest is stable and near to zero. However, the estimated coefficient is considerably less stable across the six specifications for logged appropriations (Panel A). In four of the six specifications in Panel A, the estimated coefficient is negative. Thus, while these results offer less definitive evidence on the effect of ballot reform, they do not provide convincing evidence in support of a link between the Australian ballot and rivers and harbors appropriations.

Effects of the Secret Ballot on Legislative Behavior

We now evaluate the effects of the secret ballot on legislative behavior. While our analyses above studied the provision of government goods and services to individuals and constituencies, here we focus on whether the secret ballot affected the behavioral inputs that could secure constituency benefits. We study the number of private bills introduced by each legislator from 1881 to 1931 and distinguish pension-related private bills from legislation that addresses other claims made on behalf of individual constituents. If the secret ballot provided electoral incentives for members of Congress to generate personal reputations, we expect to observe increased rates of bill sponsorship following its adoption. The unit of analysis is the number of private bills introduced on behalf of constituents in a given congress. As with our analysis of rivers and harbors

²⁰The set of control variables include member of the majority party, Republican (not included for the member fixed effects specifications, as it does not vary within member), seniority, past electoral margin, status as a chair or ranking member of any committee, committee membership fixed effects, and primary election status in the state. The inclusion of member-specific and district-specific linear trends remedy concerns about violations of the parallel trends assumption to an important degree.

projects, we estimate models using member and congress fixed effects and, separately, district and congress fixed effects. The former empirical strategy evaluates whether the secret ballot produced changes in the rate of bill sponsorships within legislators, while the latter evaluates whether the secret ballot led congressional districts to select legislators who invested greater effort in introducing private legislation. As with our analyses above, we estimate models that also contain the same set of time-varying control variables and member-specific and district-specific linear trends.

Table 5 displays the results. As is apparent in Panel A, we find no evidence that the secret ballot increased the rate at which legislators sponsored private pension bills. Only one of the six coefficients is positively signed, and it is not statistically significant at conventional levels. In fact, four of the negatively signed coefficients are statistically distinguishable from zero. The coefficients are comparable in magnitude across both member and district fixed effects strategies.²¹ The results in Panel B provide similarly meager evidence to support the hypothesis that the secret ballot caused legislators to exert more effort to cultivate personal reputations by sponsoring non-pension private bills. Here, the coefficient estimates are generally small in magnitude and two of them are negative. None of the six is statistically significant. Altogether, the results in Table 5 provide no evidence that the secret ballot led to a shift in behavior among legislators who were newly motivated to secure personal reputations.

The legislative agenda in the turn-of-the-century Congress was dominated by private bills (Finocchiaro and MacKenzie 2018), and private legislation consumed representatives' time and energy and provoked persistent interbranch conflict during much of this period (McMurry 1922). Sponsoring private bills could be one of the more valuable ways through which legislators could

²¹We are hesitant to interpret these results as evidence that the Australian ballot *decreased* production in private legislation given that the estimated coefficient of interest is relatively near to zero for the specifications that include member-specific and district-specific linear trends. 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 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.

Table 5: Private Bill Sponsorship and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dep. Variable: ln(Count Private Pension Bills Sponsored + 1)</i>						
Australian Ballot	-0.217*	-0.236*	0.030	-0.245*	-0.270*	-0.135
	(0.083)	(0.082)	(0.102)	(0.100)	(0.107)	(0.099)
<i>Panel B. Dep. Variable: ln(Count Non-Pension Private Bills Sponsored + 1)</i>						
Australian Ballot	0.048	0.047	0.156	-0.027	-0.036	0.153
	(0.180)	(0.160)	(0.101)	(0.141)	(0.129)	(0.091)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Panel A Observations	8,939	8,939	8,939	8,939	8,939	8,939
Panel B Observations	8,939	8,939	8,939	8,939	8,939	8,939

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

Dependent variable for Panel A is the log of the count of private pension bills introduced by the member, and dependent variable for Panel B is the log of the count of non-pension private bills introduced by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member any committee, committee fixed effects, and primary election status in the state. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

exhibit their attentiveness to issues important to their constituencies and attempt to cultivate personal reputations. Across a variety of model specifications and two measures of private legislation, however, we find no evidence that the secret ballot increased the rate at which legislators sponsored private bills. The results from these models suggest that the introduction of the secret ballot in the United States had no effect on the effort legislators invested on their constituents' behalf; moreover, the results in the section above demonstrate that the secret ballot did not affect the provision of government outputs. In combination, these findings weigh strongly against claims that the secret ballot meaningfully affected patterns of congressional representation.

Robustness checks

The results presented above 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 its use of 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.²² Because the party column ballot format resembled the party slip ballots that were previously used, it is possible that any potential effects of the Australian ballot would be concentrated among states with the office bloc. As Appendix A.4 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, which runs contrary to expectations. Overall, therefore, distinguishing ballot format in no way changes 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 enactment varied by state, in some cases providing ample time for legislators to strategi-

²²See Figure A.6.

cally adjust their behavior in anticipation of standing for re-election by secret ballot in the next election. These results are shown in Appendix A.5. 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.6. Across all analyses, we find a positive and statistically significant coefficient estimate for *Australian ballot* in a single specification, while the rest of the estimates are statistically indistinguishable from zero and/or negative.

Extensions: Parties, Principals, and Ballot Reform

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. As indicated in our discussion above, proponents of ballot reform argued that the secret ballot would strengthen the agency relationship between voters and officeholders and would weaken state and local party control. We conduct three analyses to study this claim.

First, we examine the consequences of the secret ballot for responsiveness to constituent preferences. Elections are the primary means through which constituents can 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 act in ways that reflect constituency preferences (e.g. Banks and Sundaram 1998; Barro 1973; Miller and Stokes 1963). Using a strategy similar to Gailmard and Jenkins (2009), we study whether the secret ballot caused legislators' roll call voting records to exhibit increased responsiveness to constituency

preferences. We use the first dimension of Nokken-Poole scores from Lewis et al. (2019) to characterize the ideological nature of congressional voting behavior from 1876 to 1924.²³ 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 would expect to observe a stronger relationship between constituent preferences and the roll-call voting behavior of their elected representatives.

The results are shown in Table 6. Our within-member estimates—models (1) and (2)—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 coefficient for Republican presidential vote share is small in magnitude and not statistically distinguishable from zero. 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. 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 (3) and (4) provide within-district estimates of legislative behavior. The coefficients for Republican vote share are positive and statistically significant, 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 small in magnitude and statistically indistinguishable from zero, indicating that the presence of the secret ballot did not meaningfully change the slope that characterizes how a legislator’s voting records respond to district preferences. While we acknowledge the limitations of the data used in this analysis—after all, for instance, the secret ballot may have also changed the degree to which presidential vote shares reasonably characterized constituent preferences—

²³See Nokken and Poole (2004) for more on how these roll-call scores are calculated.

the results in Table 6 do not support the hypothesis that legislators became *more* responsive to district preferences after the implementation of the secret ballot.

Table 6: Responsiveness and the Australian Ballot

	(1)	(2)	(3)	(4)
Australian Ballot	0.030 (0.024)	0.019 (0.026)	0.046 (0.051)	0.041 (0.046)
GOP Pres. Share	0.027 (0.051)	0.055 (0.052)	0.463* (0.123)	0.461* (0.138)
Australian Ballot × GOP Pres. Share	0.043 (0.050)	-0.007 (0.047)	0.008 (0.110)	-0.127 (0.115)
Member Fixed Effects	✓	✓		
District Fixed Effects			✓	✓
Congress Fixed Effects		✓		✓
Observations	8,353	8,353	8,353	8,353

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

Dependent variable is the first dimension of Nokken-Poole scores.

Second, 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, as was and has been claimed, we would expect legislators elected under the secret ballot to exhibit decreased levels of party unity in their roll-call voting. We use legislators' party unity scores from 1875 to 1931.²⁴ The results of our models are shown in Table 7. Across various specifications of our within-district and within-legislator models, we find consistently negative coefficient estimates for the effect of the secret ballot, suggesting that the secret ballot likely reduced legislators' fidelity to their political parties. None of the estimates is statistically significant, however. More importantly, even if they were, the magnitudes are extremely small. The dependent variable is measured on a scale ranging from zero to 100, where the numbers correspond to the percentage of the roll calls in which a legislator votes with a majority of his party against a majority of the opposite party. The largest coefficient is -1.557, which indicates

²⁴These data were obtained from https://legacy.voteview.com/k7ftp/House_Party_Unity_35-113.xls (accessed March 25, 2019).

that, at most, the secret ballot reduced a legislators’ level of party unity by about 1.5 percentage points. The small magnitudes of these estimates provide little evidence that the secret ballot severed legislators’ commitments to their political parties.

Table 7: Party Unity and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	-0.652 (0.919)	-0.458 (0.963)	-0.724 (1.373)	-0.949 (1.055)	-0.750 (0.922)	-1.557 (1.160)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Observations	8,902	8,902	8,902	8,902	8,902	8,902

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

Dependent variable is the party unity score [0-100]. 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. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

The analyses presented in this section provide 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 they did not reduce their commitment to national party positions. To be sure, each of the analyses in this section has important limitations related to measurement and modeling. However, the totality of the evidence suggests that the Australian ballot had much less of a transformative impact on the nature of political representation than its proponents argued it would. The results in this section 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.

Discussion: Ballot Reform and the Political History of Congressional Representation

Across a number of analyses, outcome variables, and models, we have found strikingly little evidence that the Australian ballot revolutionized American democracy. These results contrast with the arguments of the secret ballot's proponents, prominent historiography, and recent scholarship. We suggest, however, that these findings are not as surprising as they may seem. In fact, they are quite consistent with a number of other prominent accounts that discuss matters of political representation in post-Reconstruction America.

First, members of Congress in the postbellum period seem to have been strongly attuned to their responsibility for constituency service. Indeed, a number of members of Congress remarked on the many hours they spent working in their official capacities, with many of those hours dedicated to serving their constituents. For instance, Representative (and subsequent 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. The great crowd of people that come upon me for one thing or another draw heavily upon my vital force and go far towards exhausting the large measure of strength which I possess.

Garfield was not alone in describing the long hours that members of Congress worked on behalf of 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:

What really could the author of such a measure have been dreaming about? ... 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.²⁵

Horr went on to enumerate the long list of constituent requests a typical legislator received, which included attending to, among other items, land patents, homestead claims, patent applications, military discharges, treasury claims, postal routes, mail clerks, and sources of federal employment. While the 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 work on behalf of their constituents. While corruption and graft quite possibly accompanied party ballots, we contend that it does not necessarily follow that constituents received low-quality representation, especially in terms of attending to their requests, as a consequence. Therefore, the secret ballot may have not substantially improved the quality of political representation because legislators were already doing a rather effective job of advocating for their constituents.

Second, it would be somewhat unusual to find evidence of large, positive effects of the secret ballot because its timing does not coincide with trends in other measures commonly thought to characterize 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 and Snyder 2000). Therefore, the secret ballot seems unlikely to have ushered in a new era of candidate-centered politics because few other relevant indicators

²⁵2066 House Report 466, February 16, 1882.

suggest that the nature of campaigning or legislative behavior meaningfully changed until several decades later.

Third, it is not clear that ballot reforms were the intensely anti-party initiatives that reformers made them out to be or that many scholars have subsequently interpreted them to be. Because ballot reforms were implemented at the state level, they likely could not have been passed without at least tacit party support, and it is unlikely that the parties would have been eager to cede control over ballot access. Instead, Ware (2000, 7) argues that ballot and other Progressive-era reforms “[were] embraced by party elites and used effectively in ways intended to strengthen their parties.” According to this account, the Australian ballot not only failed to undercut the influence of state party organization, but it actually may have augmented it.²⁶ Support for this argument is found from contemporaries of ballot reform, including Ostrogorski (1908, 502), who wrote that the secret ballot “proved an almost complete failure” at “[depriving] the Machine of its electoral monopoly” but that in “reality it even aggravated the situation ... [b]y conceding the status of party to the great parties only, the law has placed them in a privileged position.” On the basis of these accounts, it is hardly clear that the effects of the secret ballot would be expected to be decidedly anti-party.²⁷

To recapitulate, we do not deny the claims of Progressive reformers and others who lamented the excessive control of parties over political affairs in the late nineteenth century. Instead, we argue that party control over elections does not preclude the possibility of effective representation. The accounts we describe above provide suggestive evidence that legislators expended considerable effort to represent their constituents prior to the introduction of the secret ballot.

²⁶Prior to the Australian ballot, state party organizations had to contend with rogue local party organizations who refused to support all of the party’s official nominees as well as individuals circulating fabricated party ballots that appeared similar to the party ballot but did not list all of the party’s officials nominees. In this sense, the Australian ballot allowed parties to assert greater control over ballot access.

²⁷At the same time, Ware (2000, 17) notes that many state parties that adopted the secret ballot did so through the party column format, which “protected party control much more” than office bloc formats. This suggests that the office bloc format had the greater potential to weaken party control. Our largely null results when distinguishing the effects of the office bloc from the party column ballot, however, cast doubt on this explanation.

Moreover, parties generally offered candidates whose political views and personal qualities rendered them up to the task of offering relatively effective patterns of representation. These factors suggest that revisionist accounts generally overstate the contributions of the secret ballot to political representation—but only because political representation was already of greater quality than those accounts acknowledge.

Conclusion

The design of the American political system, particularly the institutions governing the selection and tenure of legislators and the president, reflected the core logic that electoral incentives would provide powerful motivation for behavior. By requiring members of the House of Representatives to stand for re-election every two years, Madison and other delegates to the Constitutional Convention seem to have believed that legislators would be inclined to faithfully represent their constituencies so they may win the approval of voters and continue their service in office. A century later, Progressive reformers argued that political parties controlled the electoral apparatus and that parties, rather than voters, were legislators' principals. By severing party control over balloting procedures, the secret ballot is widely credited with improving the quality of political representation and ushering in a new era of political competition based on candidate-centered rather than party-centered elections. As a result, the secret ballot is a central component in the historiography of elections and representation and in the periodization of the United States Congress.

Our evidence casts considerable doubt on this received wisdom. Across a number of dependent variables and empirical strategies, we find no systematic evidence that the secret ballot meaningfully affected legislative behavior or the quality of political representation. Our exhaustive analyses suggest that the ballot reform movement does not register as a detectable moment in U.S. congressional history let alone constitute a transformative moment. Despite increasing the ease with which voters can distinguish candidates for office based on the candidates' personal

characteristics rather than their political party affiliation, we find no evidence suggesting that the secret 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 suggestive evidence to indicate that legislators exhibited higher-quality behavior prior to the secret ballot than is commonly appreciated. Were the secret ballot to have been implemented in another setting, its effects might have been more pronounced. 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 introduction of the direct primary had more 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 (see, 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 political science scholarship has found that the effects of partisan gerrymandering (McCarty, Poole, and Rosenthal 2009), political competition (Moskowitz and Schneer Forthcoming), primary types (Hirano et al. 2010; McGhee et al. 2014; Rogowski and Langella 2015), 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 generally 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 by historiographical accounts.

It is possible, however, that the secret ballot is a necessary but not sufficient condition for the rise of candidate-centered electoral politics. While we provide anecdotal evidence that members of Congress exerted considerable effort on behalf of their constituents prior to the secret ballot, they may simply have lacked the institutional capacity to improve upon those efforts. For example, the Legislative Reorganization Act of 1946 dramatically expanded the staffing resources

available to members of Congress; prior to this, legislators were limited mostly to the work they could feasibly 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, we can rule out the short-term, direct effects of the Australian ballot documented in other studies.

We close by noting several limitations of our study and potential directions for future research. First, while our data allow us to study 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, and related, our data do not allow us to study whether the secret ballot affected the production of high-quality substantive public policy. Future research could evaluate whether legislators elected through the secret ballot were more likely to develop programmatic policy proposals. Third, our models do not directly account for constituent demand for various forms of representation. We cannot say, for instance, whether representatives chosen under the secret ballot exhibited behavior that was more or less congruent with what constituents desired of them. Fourth, our data focus on the House of Representatives, but it is not clear whether our findings generalize to other officials, such as governors and state legislators.²⁸ 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.

²⁸To our knowledge, Squire et al. (2004) is the only published work to study the effects of the secret ballot at the subnational level.

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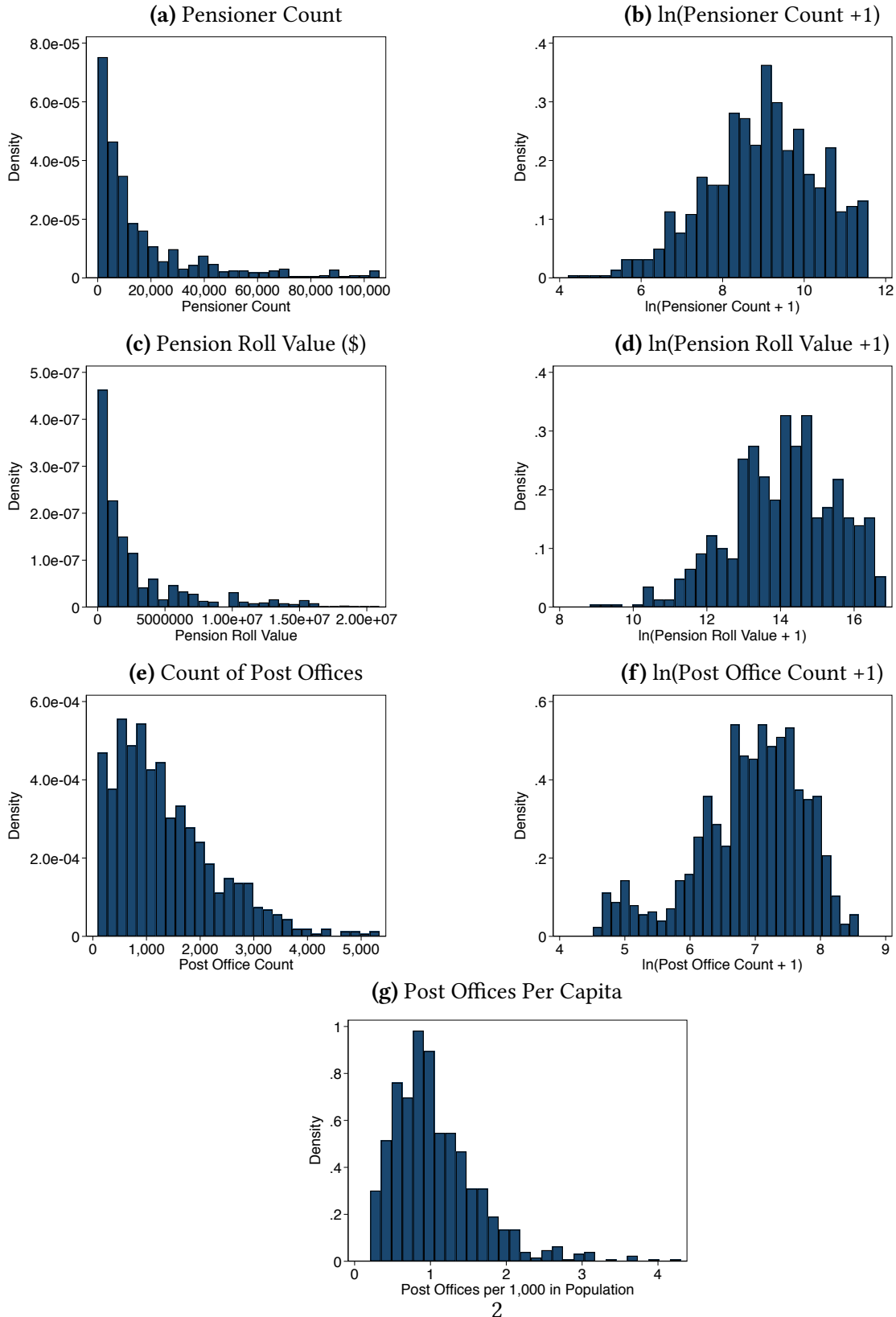
Robustness Checks and Supplementary Analyses for
Democracy Thrives in Secret?
Ballot Reform and Representation in the United States

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A.1 Distribution of Outcome Measures

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



A.2 Trends and Pre-Trends

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

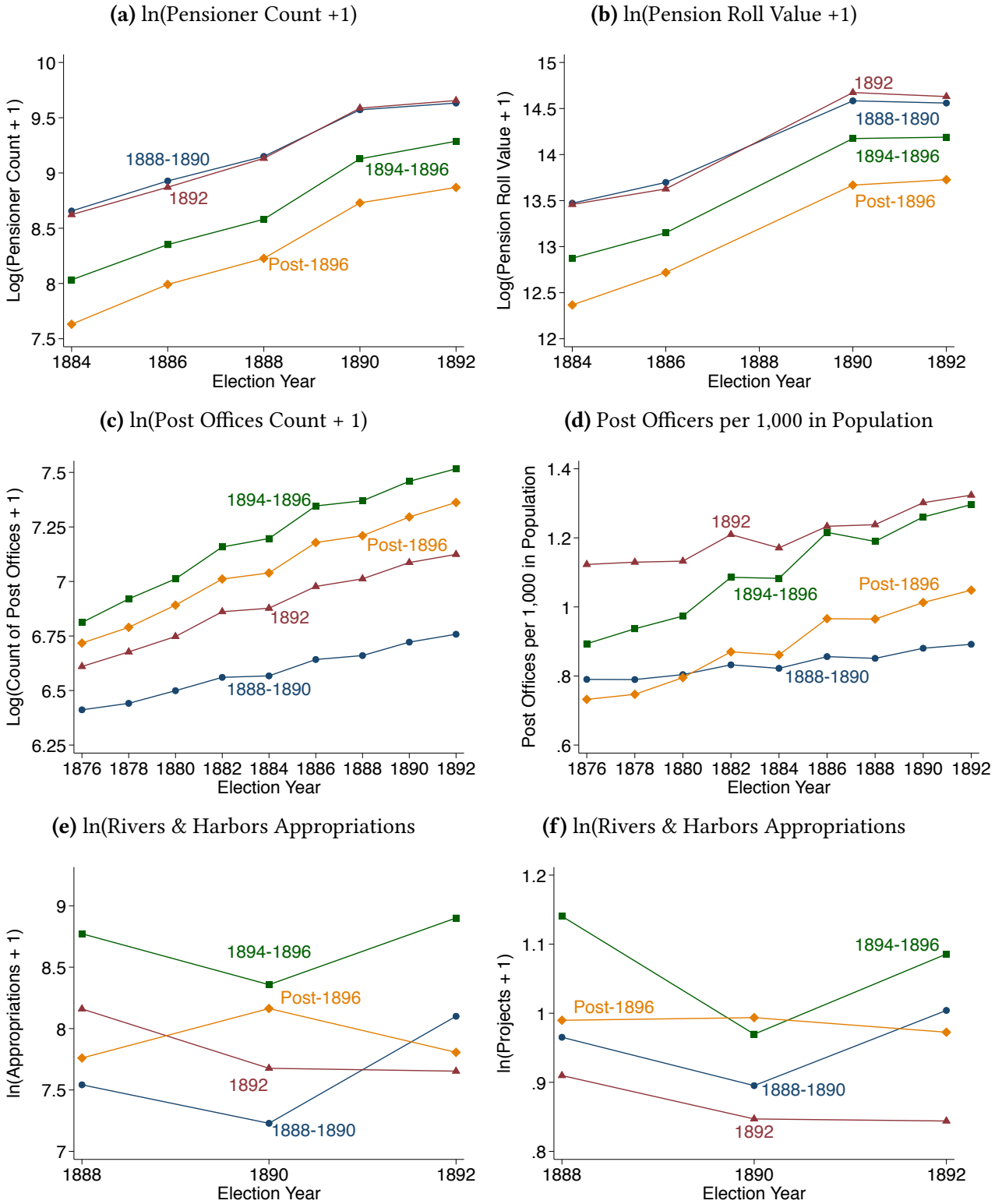


Figure A.3: Trends in Pension Private Bill Sponsorship by Year Australian Ballot Implementation

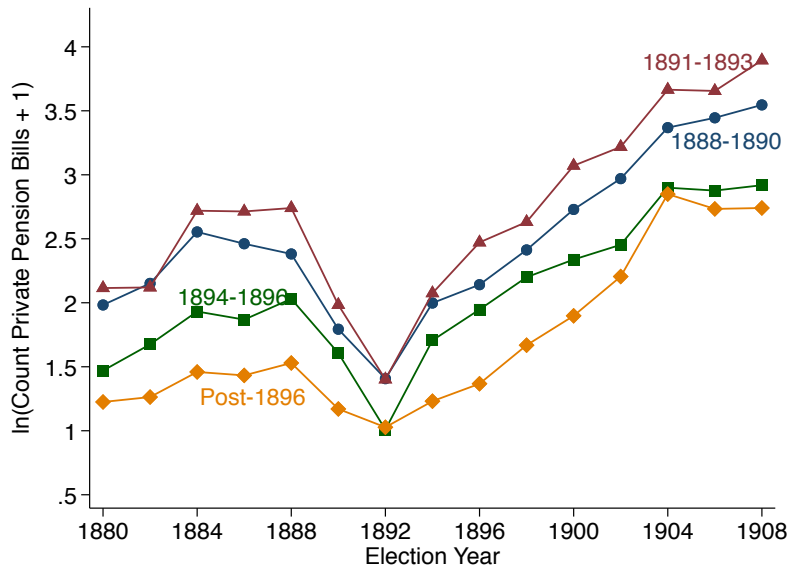
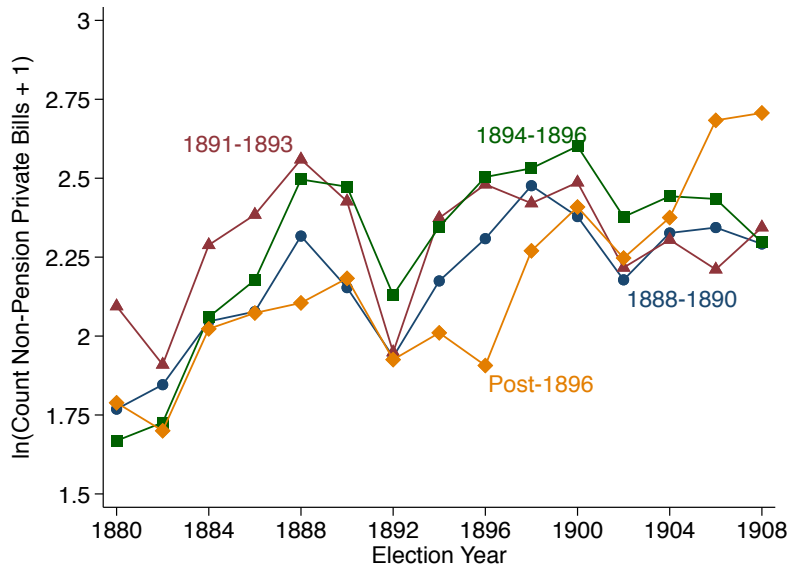


Figure A.4: Trends in Non-Pension Private Bill Sponsorship by Year of Australian Ballot Implementation



A.3 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.1, we report results including the FY1882 observations in our panel. As is the case with the primary results reported in Table 2, the estimated coefficients across all six specifications are of substantively small magnitude and not statistically significant.

Table A.1: Pensions Rolls and the Australian Ballot, Including Observations FY1882

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

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 pensioners 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.4 Decomposing the Type of Ballot

Table A.2: Pension Rolls and the Australian Ballot

	ln(Pensioner Count + 1)			ln(Pension Roll Value + 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Party Column	-0.086 (0.055)	-0.085 (0.047)	-0.031 (0.037)	-0.047 (0.052)	-0.056 (0.046)	0.001 (0.039)
Office Bloc	-0.017 (0.067)	-0.023 (0.041)	-0.013 (0.041)	-0.043 (0.069)	-0.039 (0.047)	-0.015 (0.052)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
State-Specific Trends			✓			✓
Observations	800	800	800	762	762	762

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 pensioners 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.

Table A.3: Post Offices and the Australian Ballot

	ln(Post Office Count + 1)			Post Offices Per Capita		
	(1)	(2)	(3)	(4)	(5)	(6)
Party Column	-0.000 (0.058)	0.035 (0.055)	-0.023 (0.036)	0.034 (0.056)	0.034 (0.057)	-0.002 (0.055)
Office Bloc	0.062 (0.063)	0.034 (0.055)	-0.043 (0.043)	0.073 (0.079)	0.087 (0.067)	-0.013 (0.067)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
State-Specific Trends			✓			✓
Observations	893	893	893	893	893	893

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 post offices in the state, and dependent variable for models (4)-(6) is the count of post offices per 1,000 in the population in the state. Control variables include log population (excluded from models 4-6), primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.

Table A.4: Rivers & Harbors and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dep. Variable: $\ln(\text{Appropriations} + 1)$</i>						
Party Column	0.340 (0.320)	0.500 (0.465)	-0.791 (0.456)	-0.238 (0.280)	-0.093 (0.345)	-0.315 (0.367)
Office Bloc	-0.456 (0.554)	-0.331 (0.705)	-0.671 (0.637)	-0.794 (0.477)	-0.645 (0.575)	-0.871 (0.540)
<i>Panel B. Dep. Variable: $\ln(\text{Project Count} + 1)$</i>						
Party Column	0.046 (0.046)	0.070 (0.053)	-0.098 (0.055)	0.001 (0.053)	0.014 (0.057)	-0.015 (0.055)
Office Bloc	-0.032 (0.070)	-0.004 (0.080)	-0.022 (0.098)	-0.081 (0.064)	-0.059 (0.073)	-0.058 (0.078)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Panel A Observations	4,090	4,090	4,090	4,090	4,090	4,090
Panel B Observations	4,090	4,090	4,090	4,090	4,090	4,090

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

Dependent variable for Panel A is the log of rivers & harbors appropriations (\$) in the district, and dependent variable for Panel B is the log of the count of rivers & harbors projects in the district. 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. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

Table A.5: Private Bill Sponsorship and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dep. Variable: ln(Count Private Pension Bills Sponsored + 1)</i>						
Party Column	-0.246*	-0.260*	-0.011	-0.327*	-0.349*	-0.205*
	(0.074)	(0.077)	(0.111)	(0.100)	(0.114)	(0.102)
Office Bloc	-0.161	-0.174	0.086	-0.109	-0.114	-0.043
	(0.132)	(0.129)	(0.112)	(0.119)	(0.113)	(0.125)
<i>Panel B. Dep. Variable: ln(Count Non-Pension Private Bills Sponsored + 1)</i>						
Party Column	-0.005	-0.007	0.187	-0.009	-0.031	0.171
	(0.199)	(0.179)	(0.093)	(0.127)	(0.113)	(0.102)
Office Bloc	0.174	0.188	0.172	0.114	0.111	0.256*
	(0.179)	(0.157)	(0.157)	(0.180)	(0.164)	(0.124)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Panel A Observations	8,939	8,939	8,939	8,939	8,939	8,939
Panel B Observations	8,939	8,939	8,939	8,939	8,939	8,939

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

Dependent variable for Panel A is the log of the count of private pension bills introduced by the member, and dependent variable for Panel B is the log of the count of non-pension private bills introduced by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member any committee, committee fixed effects, and primary election status in the state. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

Table A.6: Responsiveness and the Australian Ballot

	(1)	(2)	(3)	(4)
Party Column	0.029 (0.026)	0.021 (0.029)	0.037 (0.067)	0.056 (0.060)
Office Bloc	0.025 (0.028)	0.010 (0.029)	0.074 (0.058)	0.035 (0.056)
GOP Pres. Share	0.021 (0.049)	0.047 (0.051)	0.512* (0.133)	0.481* (0.143)
Party Column × GOP Pres. Share	0.044 (0.052)	-0.007 (0.053)	0.047 (0.125)	-0.095 (0.137)
Office Bloc × GOP Pres. Share	0.055 (0.060)	0.010 (0.053)	-0.145 (0.133)	-0.221 (0.133)
Member Fixed Effects	✓	✓		
District Fixed Effects			✓	✓
Congress Fixed Effects		✓		✓
Observations	8,353	8,353	8,353	8,353

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

Dependent variable is the first dimension of Nokken-Poole scores.

Table A.7: Party Unity and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
Party Column	-0.138 (1.002)	-0.037 (1.000)	-0.513 (1.448)	-0.093 (0.915)	-0.166 (0.807)	-0.758 (1.188)
Office Bloc	-1.891* (0.902)	-1.684 (0.976)	-2.523 (1.266)	-1.540 (1.281)	-0.826 (1.201)	-2.047 (1.501)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Observations	8,902	8,902	8,902	8,902	8,902	8,902

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

Dependent variable is the party unity score [0-100]. 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. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

A.5 Do Members Engage in Anticipatory Behavior?

Table A.8: Pension Rolls and the Australian Ballot

	ln(Pensioner Count + 1)			ln(Pension Roll Value + 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot $t+1$	-0.024 (0.055)	-0.060 (0.044)	-0.016 (0.036)	-0.000 (0.062)	-0.035 (0.054)	0.022 (0.056)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
State-Specific Trends			✓			✓
Observations	800	800	800	762	762	762

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 pensioners 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.

Table A.9: Post Offices and the Australian Ballot

	ln(Post Office Count + 1)			Post Offices Per Capita		
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot $t+1$	-0.005 (0.042)	-0.013 (0.039)	-0.056 (0.042)	0.017 (0.055)	0.023 (0.048)	-0.019 (0.064)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
State-Specific Trends			✓			✓
Observations	893	893	893	893	893	893

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 post offices in the state, and dependent variable for models (4)-(6) is the count of post offices per 1,000 in the population in the state. Control variables include log population (excluded from models 4-6), primary election status, and past Republican share of the two-party presidential vote. State-specific trends are state-specific linear trends.

Table A.10: Rivers & Harbors and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dep. Variable: ln(Appropriations + 1)</i>						
Australian Ballot $t+1$	0.789 (0.506)	0.866 (0.614)	0.296 (0.460)	0.137 (0.404)	0.174 (0.396)	-0.321 (0.463)
<i>Panel B. Dep. Variable: ln(Project Count + 1)</i>						
Australian Ballot $t+1$	0.141 (0.091)	0.153 (0.104)	0.207 (0.119)	0.086 (0.081)	0.094 (0.083)	0.063 (0.101)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Panel A Observations	4,090	4,090	4,090	4,090	4,090	4,090
Panel B Observations	4,090	4,090	4,090	4,090	4,090	4,090

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

Dependent variable for Panel A is the log of rivers & harbors appropriations (\$) in the district, and dependent variable for Panel B is the log of the count of rivers & harbors projects in the district. 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. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

Table A.11: Private Bill Sponsorship and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dep. Variable: ln(Count Private Pension Bills Sponsored + 1)</i>						
Australian Ballot $t+1$	-0.206*	-0.213*	-0.071	-0.248*	-0.260*	-0.175*
	(0.090)	(0.085)	(0.077)	(0.078)	(0.081)	(0.081)
<i>Panel B. Dep. Variable: ln(Count Non-Pension Private Bills Sponsored + 1)</i>						
Australian Ballot $t+1$	-0.049	-0.036	0.001	-0.175	-0.164	-0.062
	(0.163)	(0.142)	(0.094)	(0.149)	(0.144)	(0.074)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Panel A Observations	8,939	8,939	8,939	8,939	8,939	8,939
Panel B Observations	8,939	8,939	8,939	8,939	8,939	8,939

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

Dependent variable for Panel A is the log of the count of private pension bills introduced by the member, and dependent variable for Panel B is the log of the count of non-pension private bills introduced by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member any committee, committee fixed effects, and primary election status in the state. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

Table A.12: Responsiveness and the Australian Ballot

	(1)	(2)	(3)	(4)
Australian Ballot $t+1$	0.037	0.023	0.068	0.026
	(0.022)	(0.021)	(0.057)	(0.046)
GOP Pres. Share	0.039	0.065	0.606*	0.515*
	(0.049)	(0.050)	(0.140)	(0.125)
Australian Ballot $t+1$ × GOP Pres. Share	0.028	-0.017	-0.149	-0.188*
	(0.050)	(0.042)	(0.122)	(0.090)
Member Fixed Effects		✓	✓	
District Fixed Effects				✓
Congress Fixed Effects			✓	✓
Observations	8,353	8,353	8,353	8,353

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

Dependent variable is the first dimension of Nokken-Poole scores.

Table A.13: Party Unity and the Australian Ballot

	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot $t+1$	-0.250 (0.904)	-0.105 (0.911)	-1.554 (0.867)	-0.453 (1.206)	0.149 (1.166)	0.170 (1.241)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Observations	8,902	8,902	8,902	8,902	8,902	8,902

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

Dependent variable is the party unity score [0-100]. 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. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

A.6 Excluding the South

Table A.14: Pension Rolls and the Australian Ballot, Excluding the South

	ln(Pensioner Count + 1)			ln(Pension Roll Value + 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	-0.065 (0.053)	-0.001 (0.024)	-0.011 (0.021)	-0.029 (0.075)	0.028 (0.041)	0.029 (0.045)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
State-Specific Trends			✓			✓
Observations	602	602	602	575	575	575

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 pensioners 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. The South is defined as the 11 states of the former Confederacy.

Table A.15: Post Offices and the Australian Ballot, Excluding the South

	ln(Pensioner Count + 1)			ln(Pension Roll Value + 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	-0.005 (0.049)	0.054 (0.037)	0.011 (0.019)	0.091 (0.047)	0.041 (0.035)	0.057 (0.047)
Year Fixed Effects	✓	✓	✓	✓	✓	✓
State Fixed Effects	✓	✓	✓	✓	✓	✓
Controls		✓	✓		✓	✓
State-Specific Trends			✓			✓
Observations	635	635	635	635	635	635

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 post offices in the state, and dependent variable for models (4)-(6) is the count of post offices per 1,000 in the population in the state. Control variables include log population (excluded from models 4-6), 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.

Table A.16: Rivers & Harbors and the Australian Ballot, Excluding the South

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dep. Variable: $\ln(\text{Appropriations} + 1)$</i>						
Australian Ballot	-0.054 (0.455)	-0.166 (0.374)	-0.397 (0.440)	-0.758* (0.308)	-0.771* (0.306)	0.150 (0.402)
<i>Panel B. Dep. Variable: $\ln(\text{Project Count} + 1)$</i>						
Australian Ballot	-0.011 (0.059)	-0.026 (0.051)	-0.109* (0.044)	-0.067 (0.060)	-0.070 (0.059)	0.028 (0.059)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Panel A Observations	3,044	3,044	3,044	3,044	3,044	3,044
Panel B Observations	3,044	3,044	3,044	3,044	3,044	3,044

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

Dependent variable for Panel A is the log of rivers & harbors appropriations (\$) in the district, and dependent variable for Panel B is the log of the count of rivers & harbors projects in the district. 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. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6).

The South is defined as the 11 states of the former Confederacy.

Table A.17: Private Bill Sponsorship and the Australian Ballot, Excluding the South

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Dep. Variable: ln(Count Private Pension Bills Sponsored + 1)</i>						
Australian Ballot	-0.048 (0.114)	-0.045 (0.105)	0.313* (0.108)	-0.089 (0.100)	-0.084 (0.102)	-0.032 (0.117)
<i>Panel B. Dep. Variable: ln(Count Non-Pension Private Bills Sponsored + 1)</i>						
Australian Ballot	-0.048 (0.102)	-0.031 (0.093)	-0.049 (0.082)	0.043 (0.094)	0.062 (0.088)	0.027 (0.109)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Panel A Observations	6,714	6,714	6,714	6,714	6,714	6,714
Panel B Observations	6,714	6,714	6,714	6,714	6,714	6,714

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

Dependent variable for Panel A is the log of the count of private pension bills introduced by the member, and dependent variable for Panel B is the log of the count of non-pension private bills introduced by the member. Control variables include member of majority party, Republican, seniority, past electoral margin, chair or ranking member any committee, committee fixed effects, and primary election status in the state. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6). The South is defined as the 11 states of the former Confederacy.

Table A.18: Responsiveness and the Australian Ballot, Excluding the South

	(1)	(2)	(3)	(4)
Australian Ballot	-0.053 (0.069)	-0.076 (0.067)	0.400 (0.387)	0.386 (0.268)
GOP Pres. Share	-0.124 (0.120)	-0.114 (0.123)	1.165 (0.672)	1.179* (0.448)
Australian Ballot × GOP Pres. Share	0.179 (0.128)	0.162 (0.117)	-0.638 (0.665)	-0.835 (0.482)
Member Fixed Effects		✓	✓	
District Fixed Effects			✓	✓
Congress Fixed Effects			✓	✓
Observations	5,713	5,713	5,713	5,713

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

Dependent variable is the first dimension of Nokken-Poole scores. The South is defined as the 11 states of the former Confederacy.

Table A.19: Party Unity and the Australian Ballot, Excluding the South

	(1)	(2)	(3)	(4)	(5)	(6)
Australian Ballot	-0.233 (1.350)	-0.165 (1.208)	-0.478 (1.663)	-1.493 (1.589)	-0.607 (1.204)	-2.421 (1.792)
Congress Fixed Effects	✓	✓	✓	✓	✓	✓
Member Fixed Effects	✓	✓	✓			
District Fixed Effects				✓	✓	✓
Controls		✓	✓		✓	✓
Unit-Specific Trends			✓			✓
Observations	6,684	6,684	6,684	6,684	6,684	6,684

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

Dependent variable is the party unity score [0-100]. 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. Unit-specific trends are member-specific linear trends for model (3) and district-specific linear trends for model (6). The South is defined as the 11 states of the former Confederacy.

A.7 Analysis of the 1890 Pension Law

Table A.20: Analysis of the Pension Rolls Post-1890 Pension Bill

	<u>% Change Pensioners</u>	<u>Change Share Pensioned</u>	<u>% Change Value</u>
	(1)	(2)	(3)
	1892 – 1890	1892 – 1890	1892 – 1888
Australian Ballot	-0.138	-0.038	-0.386
	(0.128)	(0.033)	(0.253)
Constant	0.731*	0.271*	1.651*
	(0.051)	(0.013)	(0.101)
Observations	38	38	38

Standard errors are in parentheses. * $p < 0.05$.

Dependent variable for model (1) is the percent change in the count of pensioners in the state between June 30, 1890 and June 30, 1892; the dependent variable for model (2) is the difference in the proportion of veterans and widows on the pension rolls in the state between 1892 and 1890; the dependent variable for model (5) is the percent change in the annual value (\$) of the pension roll between between 1892 and 1888 (this measure is not available between 1889 and 1891). The Australian ballot variable is coded = 1 if the state had implemented ballot reform by the 1890 election.

Table A.21: Analysis of the Pension Rolls Post-1890 Pension Bill, Excluding the South

	% Change Pensioners	Change Share Pensioned	% Change Value
	(1)	(2)	(3)
	1892 – 1890	1892 – 1890	1892 – 1888
Australian Ballot	-0.220 (0.130)	-0.038 (0.040)	-0.553 (0.282)
Constant	0.702* (0.056)	0.279* (0.017)	1.651* (0.122)
Observations	27	27	27

Standard errors are in parentheses. * $p < 0.05$.

Dependent variable for model (1) is the percent change in the count of pensioners in the state between June 30, 1890 and June 30, 1892; the dependent variable for model (2) is the difference in the proportion of veterans and widows on the pension rolls in the state between 1892 and 1890; the dependent variable for model (5) is the percent change in the annual value (\$) of the pension roll between between 1892 and 1888 (this measure is not available between 1889 and 1891). The Australian ballot variable is coded = 1 if the state had implemented ballot reform by the 1890 election. The South is defined as the 11 states of the former Confederacy.

