British Journal of Political Science (2023), page 1 of 18
doi:10.1017/S0007123423000091

British Journal of Political Science

ARTICLE

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

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(Received 6 March 2022; revised 9 September 2022; accepted 19 February 2023)

Abstract

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

Keywords: Australian ballot; ballot reform; congress; representation; accountability

An incumbent's incentive to develop a personal vote underlies most theories of electoral accountability (Fearon 1999; Ferejohn 1986).[1] In these models, voters observe an incumbent's performance and decide to retain the incumbent or support the challenger. In contrast, incumbents anticipate the electorate's decision and respond to voters' potential sanctions (Ashworth 2012). These accounts suggest the importance of individual officeholders' behaviour for election outcomes and provide insight into constituents' control over their representatives. Accordingly, an extensive literature studies legislators' pursuit of the personal vote through constituency service, distributive outlays, and campaign appeals (Cain, Ferejohn, and Fiorina 1987; Hirano et al. 2022; Kaslovsky 2022; Mayhew 1974).

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

[1]Our use of 'personal vote' follows Cain, Ferejohn, and Fiorina (1987, 9): 'that portion of a candidate's electoral support which originates in his or her personal qualities, qualifications, activities, and record.'

We study how the incentives to develop a personal vote affect officeholder behaviour. We do so by leveraging the introduction of the Australian ballot in US states at the turn of the twentieth century.[2] In contrast with the party strip, the Australian ballot displayed all parties' candidates on a single ballot, which voters then cast privately. It was first adopted statewide in Massachusetts in 1888 and quickly spread across states, with thirty-six states implementing it within a decade.[3] While theories of electoral accountability suggest that voters' newfound ability to evaluate individual officeholders should increase legislators' efforts on behalf of their constituents in pursuit of the personal vote, we also draw from historical accounts to suggest that the effects of the Australian ballot could have been more limited than its proponents argued.

We combine original data with existing sources to provide the most comprehensive study to date on the effects of the secret ballot on legislative behavior. Our dependent variables span three components of the personal vote. First, following research that associates pork-barrel politics and constituency service with officeholders' attempts to secure the personal vote (Cain, Ferejohn, and Fiorina 1987), we examine the provision of distributive resources and social welfare benefits. Second, following studies of publicly observable indicators of legislative effort (Dal Bó and Rossi 2011; Finocchiaro and MacKenzie 2018; Fouirnaies and Hall 2022), we evaluate patterns of abstention, bill sponsorship, and floor speech. Third, following research on legislators' ideological responsiveness to constituency and party preferences (Ansolabehere, Snyder, and Stewart 2001; Carson and Sievert 2015; Gailmard and Jenkins 2009), we study several dimensions of legislators' roll-call voting patterns. Altogether, our dependent variables characterize an extensive range of representational outcomes that span 1880–1930.[4]

We use these measures with a differences-in-differences design to estimate within-constituency and within-legislator effects of the secret ballot. This strategy allows us to account for the possibility that the secret ballot could have also affected the characteristics of the legislators who sought office, that constituencies from different states or districts could have different demands for legislative representation, and that secular trends in patterns of representation could confound estimates of the secret ballot's effect. Figure 1 provides an illustrative example, comparing the average number of private bills sponsored by representatives from the states that implemented the Australian ballot in 1890 with representatives from late-adopting states across time (data is sourced from Finocchiaro and MacKenzie 2018). Based on a visual inspection of the figure, a cross-sectional comparison would indicate that legislators from Australian ballot states sponsored more private legislation than legislators from late-adopting states (the blue line is always above the red line after 1890); however, this gap existed prior to the implementation of ballot reform in many of these states. Thus, inferring that ballot reform caused such a gap would be problematic. Similarly, a before-and-after comparison between the 1890 ballot reform states indicates that legislators, on average, sponsored substantially more private legislation after ballot reform compared to the period before ballot reform. However, a similar upward trend over the same period is also present in the states that did not experience any change in their balloting institutions. Again, it would be problematic to infer that the increase during this period was due to ballot reform when a similar increase occurred in states that did not change balloting institutions.

Our approach addresses limitations in both design and inference from previous studies. This work primarily uses pooled cross-sectional research designs (Finocchiaro and MacKenzie 2018; Katz and Sala 1996; Wittrock et al. 2008) or before-and-after comparisons of legislators whose states adopted the secret ballot during their tenures (Carson and Sievert 2015) but none accounts

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

[3]States used two versions of the secret ballot. The 'office bloc' grouped candidates by office while the 'party column' ballot listed all candidates under the party label. While our primary interest is in the effect of the adoption of the Australian ballot, we distinguish the effects of different ballot formats in Appendix Section A.9.

[4]As we explain below, the start and end years of our analyses vary somewhat across measures based on data availability.

https://doi.org/10.1017/S0007123423000091 Published online by Cambridge University Press

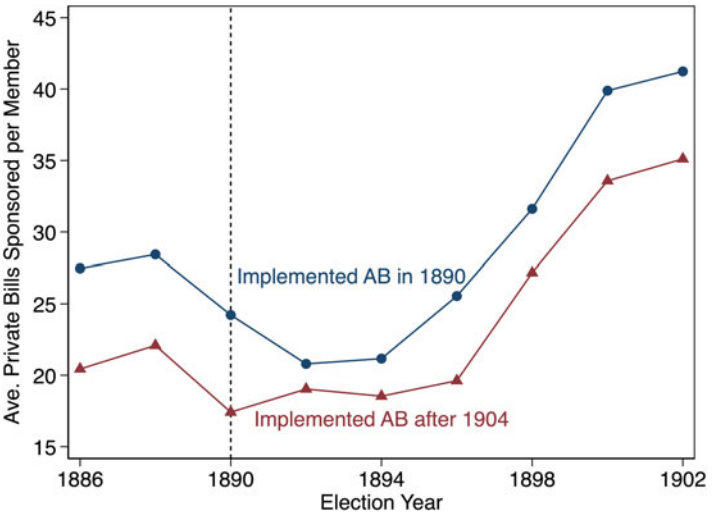

Figure 1. Trends in Private Legislation Sponsorship by Year of Adoption of the Australian Ballot.

for both unobserved characteristics of legislators or constituencies and temporal patterns in congressional behaviour.[5] Though the evidence from this scholarship is generally consistent with the hypothesis that the Australian ballot increased legislative effort and improved the quality of representation, these results are vulnerable to the potential sources of confounding noted above. Additionally, the standard errors reported in previous scholarship are likely incorrect due to the failure to cluster appropriately at the state level (Primo, Jacobsmeier, and Milyo 2007), which may bias standard errors downwards and produce invalid inferences.

We find no systematic evidence that the secret ballot directly affected the provision of legislator effort or the quality of representation. These patterns are robust across model specifications, measurement strategies, and subsets of observations. Our research contributes to several bodies of scholarship.

First, contra claims advanced by proponents of ballot reform (George 1883), our evidence suggests that its effects have been overstated by previous scholarship (see, e.g., Carson and Jenkins 2011). Instead, the results align with accounts that are sceptical about its effects (Ostrogorski 1908; Ware 2000). In reviewing several plausible explanations for our null findings, we suggest conditions under which institutional changes do not produce the behavioural changes anticipated by their proponents.

Second, our findings relate to the role of parties in political selection. Parties have electoral incentives to nominate high-quality candidates (Hirano and Snyder 2019). Previous scholarship shows that parties benefitted electorally when they nominated quality candidates (Carson, Engstrom, and Roberts 2007). Partisan control of the electoral apparatus during this period thus may have produced high-performing legislators, primarily through selection rather than pure moral hazard (see Fearon 1999). Our null results could reflect the possibility that the Australian ballot shifted the relative importance of these mechanisms rather than increasing the effect of moral hazard while holding selection constant.[6]

[5]Engstrom and Roberts (2020, chapter 3) study the effects of the Australian ballot on turnout and roll-off in a differences-in-differences framework for the period similar to the focus of this article but do not evaluate the effect on legislative behaviour. Engstrom and Roberts (2020, chapter 4) use a similar research design to study the effect of ballot design on bill sponsorship, legislative effectiveness, and presidential support for a much later period (the second half of the twentieth century) but focuses on variants of the office bloc and party column ballots rather than the switch from the party ballot to the Australian ballot.

[6]It could also be the case that partisan and constituency interests were relatively aligned during this period, which would limit efforts to empirically distinguish trade-offs between them.

https://doi.org/10.1017/S0007123423000091 Published online by Cambridge University Press

Third, we contribute to research that investigates variation in the personal vote. For the period we study, Ansolabehere, Snyder, and Stewart (2000) estimate that while the incumbency advantage was relatively small, it was accounted for almost entirely by the personal vote. Our results suggest that ballot design was probably not an important contributor to the personal vote in this period, nor did it '[usher] in a new era of candidate-centred politics' (Engstrom and Kernell 2014, 191–192), at least not in the short run.

Finally, our study complements multiple strands of research outside the US. One line of research examines how cross-national variation in electoral institutions is associated with the incentives for representatives to engage in personal vote-seeking behaviour (Carey and Shugart 1995). A second strand examines how variation in subnational electoral institutions affects political outcomes such as the number of parties, policy volatility, and voter turnout (Bordignon, Nannicini, and Tabellini 2016; Fiva and Folke 2016; Lucardi 2019). We contribute to this research by leveraging subnational variation in balloting institutions to identify their effect on personal vote-seeking behaviour.

The Australian Ballot in the United States

During the first century of American elections, political parties created and separately distributed ballots. Voters chose between the ballots produced by competing parties, each listing the party's candidates for all offices. Ballots were deposited publicly so that a voter's choice could be monitored. This balloting system made it difficult (though not impossible) for voters to split their tickets across candidates for different offices and political parties (Rusk 1970), complicating voters' efforts to sanction individual officeholders. It may have been challenging 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. As a result, progressives embraced ballot reform to reduce the influence of political parties and strengthen popular control over officeholders because party corruption had frustrated constituents and 'created a favorable attitude on the part of a majority of voters' toward reform (Evans 1917, 21). As the *New York Times* declared in an editorial advocating for ballot reform: '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.'[7] The Australian ballot was quickly adopted by most states (including New York), in which the state issued ballots with the names of all candidates for each office, which were cast in secret. Henry George (1883, 208) predicted that the Australian ballot 'would be the greatest single reform' and would 'very much lessen the importance of party nominations and party machinery' of selecting elected representatives.

The evidence suggests that the Australian ballot enabled voters to distinguish between candidates for different offices. As split-ticket voting increased following the adoption of the Australian ballot (Rusk 1970), scholars have argued that ballot reform 'made credit-claiming and other personal vote activities by members of Congress significantly more important for reelection' (Katz and Sala 1996, 21) and 'firmed up the agency relationship by increasing accountability [as] members of Congress now had a greater incentive to be responsive to voters' needs' (Carson and Jenkins 2011, 39). Consistent with these claims, previous research indicates that legislators elected under the Australian ballot exhibited less party loyalty in congressional voting patterns (Carson and Sievert 2015; Wittrock et al. 2008), had longer committee tenures and more favourable committee assignments (Carson and Sievert 2015; Katz and Sala 1996; Wittrock et al. 2008), sponsored more private bills on behalf of constituents (Finocchiaro and MacKenzie 2018), and

[7]'No Place for Partisanship', 3 May 1888, page 4.

https://doi.org/10.1017/S0007123423000091 Published online by Cambridge University Press

secured more spending for district projects (Wittrock et al. 2008). As noted above, however, the research designs used in these studies complicate their ability to isolate the effect of ballot reform from other potential confounding factors that could affect legislative behaviour.

We offer several reasons to reconsider the conclusions from previous scholarship on the effects of the Australian ballot. First, the incumbency advantage pre-dated the secret ballot (Carson, Engstrom, and Roberts 2007), suggesting that legislators were already exerting effort to cultivate a personal vote before its implementation. Second, while the Australian ballot may have made it theoretically possible for voters to distinguish individual candidates from their parties, the informational environment may not have changed in such a way to permit voters to make these distinctions in practice, as Hirano et al. (2022) show that the secret ballot did not increase the rate of candidate-centred newspaper advertisements. Third, party control may not have been weakened overall due to ballot reform. While parties could no longer as easily engage in vote buying, they had greater control over ballot access, which prevented local party organizations from engaging in so-called ballot treachery (Ware 2000). Fourth, ballot reform may have had smaller effects on voter behaviour than is commonly thought, as voters were engaged in relatively high levels of ticket splitting prior to the Australian ballot (Reynolds and McCormick 1986). Collectively, these factors suggest that the Australian ballot had limited effects, if any, on legislative behaviour and political representation, at least in the short run.

The Australian ballot would not be the only institutional reform estimated to have had little impact on voters and politicians. For example, previous research reports null results when examining the impact of primary elections (Hirano et al. 2010) and gerrymandering (McCarty, Poole, and Rosenthal 2009) on partisan polarization and the effect of franchise expansion on the backgrounds of elected officials (Berlinski, Dewan, and Van Coppenolle 2014). However, another Progressive Era reform, the direct primary, is estimated to have positively affected political competition, voters' ability to select high-quality candidates, and the provision of public goods (Hirano and Snyder 2019). Thus, we discuss the evidence for whether the Australian ballot activated the mechanisms through which it was theorized to produce behavioural change. By comparing this evidence with findings about how direct primaries increased candidate-centred politics (Hirano et al. 2022), our research provides suggestive evidence about the conditions under which institutional reforms produce behavioural effects.

Data

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

Our primary analyses use data on three kinds of government outputs. First, we assembled original data on the annual state-level distribution of war pensions from the fiscal years 1882 to 1920. These data come from the *Annual Report of the Commissioner of Pensions to the Secretary of the Interior*, describing the number of pension recipients and the total dollar amount of pension payments.[8] Pension payments for disabled veterans in the US were initiated in 1776 and later expanded in the early part of the nineteenth century as members of Congress recognized the benefits of using pension payments as particularistic goods (Finocchiaro and Jenkins 2016). During this period, both anecdotal evidence and the enormous volume of correspondence from

[8]Generally speaking, we average the measures of pension activity across the two years that comprise a given congress. However, state-level pension data on both the number of pensioners and the total dollar amount of pensions are unavailable from these reports for fiscal years 1883–6 and 1888. We aggregate county-level totals, which are available in FY1886 and FY1888, to the state level. Additionally, data are unavailable on the total dollar amount for fiscal years 1889–91. We exclude observations from FY1882 to obtain an uninterrupted balanced panel from FY1886 to FY1920 but results are robust to its inclusion (see Table A.23).

https://doi.org/10.1017/S0007123423000091 Published online by Cambridge University Press

Table 1. Summary of dependent variables

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

Note: The table provides descriptive statistics, time periods, and units of analysis for dependent variables. All outcomes are measured per Congress. All dependent variables measured as a count ('number') in this table are log-transformed prior to analysis due to the highly skewed nature of the underlying distributions (see Figure A.1). The rivers and harbours projects data are aggregated from the U.S. House-district level to the state level for analysis; we also report results from a district-level analysis in Appendix Section A.13. We derive our roll-call discretion measure using data from Lewis et al. (2022).
[a]indicates that the analysis for the outcome is reported in the Appendix.

members of Congress to the Bureau of Pensions indicate that pension assistance was an essential source of constituency service.[9] We study both the number of pensions and their dollar values.

Second, we use biennial data on the geographic distribution of post offices. We supplement data reported by Rogowski (2016) on post offices from 1876 to 1896 with original data to extend the time series through 1916. Local communities frequently mobilized around and petitioned for the expansion of postal services, and Kernell and McDonald (1999) provide evidence of the constituency-induced political incentives that affected congressional action on postal services.[10] We study the provision of post offices, which we operationalize with raw counts and per-capita values.

Third, we use data collected by Wilson (1986) on the allocation of rivers and harbours projects from 1889 to 1912. Rivers and harbours were among the most important internal improvements for which the late nineteenth-century Congress awarded line-item appropriations. These appropriations were generally awarded to dredge waterways and strengthen river banks. Because these projects were both tangible and geographically fixed, they provided natural opportunities for credit claiming. We study the secret ballot's effect on the number of rivers and harbours projects and the amount appropriated for them.

These three measures – pensions, post offices, and rivers and harbours projects – provide a comprehensive portrait of the provision of constituency service at the turn of the twentieth century. The pension system was the most important social welfare programme prior to the New Deal and, for many years, accounted for a large share of federal expenditures (Finocchiaro and Jenkins 2016). Likewise, the post office was 'one of the most important institutions of the day'

[9]Representative Robert La Follette (1911, 84) estimated that a quarter to a third of his time was spent on pension cases at the Pension Office.

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

https://doi.org/10.1017/S0007123423000091 Published online by Cambridge University Press

(John 1995, vii) as the most significant federal employer. Moreover, rivers and harbours projects were 'a quintessential part of the pork barrel' (Wilson 1986, 733). If the secret ballot increased the incentives for members of Congress to cultivate personal reputations and intervene on behalf of their constituents, we expect to observe increased provision of these outputs following its introduction.

In a second set of analyses, we study whether the secret ballot affected legislative effort. We examine the introduction of private legislation in the U.S. House, roll-call participation rates, and floor speeches. Private legislation was used to address claims for 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 use data from Finocchiaro and MacKenzie (2018) on the number of private bills introduced in the U.S. House between 1881 and 1930 to test the hypothesis that the secret ballot increased legislators' efforts to address private claims. We also examine two other measures, roll-call participation (using data from Lewis et al. 2022) and House floor speeches (using data from Gentzkow, Shapiro, and Taddy 2019), which are widely used to characterize legislative effort (Dal Bó and Rossi 2011; Fouirnaies and Hall 2022) and indicate the degree to which the representative is engaged in, rather than absent from, the legislative process.

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

Empirical Strategy

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

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

where Y is the relevant dependent variable and i and t index units and years, respectively. Our sample includes observations for all states in each year, with territories entering the panel upon statehood.[12] The main independent variable is an indicator, *Australian ballot*, which characterizes whether the secret ballot was used to elect a state's representatives in year t.[13] The coefficient β is thus the key parameter of interest. If, for example, the secret ballot increased legislator effort, we expect to observe positive estimates for β.[14] We include unit fixed effects (λ_i) to control for observed and unobserved time-invariant attributes that may affect legislative behaviour. As

[11]H.R. 267 in the 1st Session of the 62 Congress provides an illustrative example. Representative Edgar Dean Crumpacker (R-IN) sponsored the bill with the following summary: 'Granting an increase of pension to Charles W. Sexton.' The full text of this bill is available in Appendix Section A.3. Today, these matters would generally be referred to legislative staff and/or the bureaucracy (Finocchiaro and MacKenzie 2018).

[12]See Appendix A.2 for additional information about the sample.

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

[14]One underlying assumption of our research design is that legislators from non-reform states did not change their behaviour in response to the changes in reform states. While this is a standard assumption for our design as well as most other research that uses similar designs, it is worth stating explicitly.

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discussed below, we estimate models with various unit effects that account for the nature of the data and different substantive hypotheses about representation.

For data at the district level, we estimate separate models that include either legislator or district fixed effects.[15] The former accounts for within-legislator changes in behaviour with the introduction of the secret ballot, thereby controlling for fixed characteristics of legislators that might affect their behaviour. District fixed effects account for characteristics of House constituencies that might affect the demand for legislators to exhibit particular patterns of behaviour.[16] These two approaches allow us to examine distinct channels through which the secret ballot could have improved representation: reduced moral hazard via changes in effort within legislators and reduced adverse selection via voters' improved ability to select high-quality officeholders. We also include time-fixed effects (δ_t) to account for secular trends in legislative behaviour, legislative capacity, and constituent demands. Additionally, we include a matrix ($\boldsymbol{X}_{it}$) of other unit, legislator, and contextual characteristics that may also be related to legislative behaviour and productivity, which are explained below in greater detail. Finally, Ω_{it} is a vector of coefficients for these time-varying control variables and ϵ_{it} is a random error term, clustered on state.

In model (1) the coefficient for the Australian ballot is identified by comparing outcomes within states, legislators, or districts that correspond to changes in a state's ballot institutions. The key identifying assumption is that, absent the introduction of the secret ballot, trends in the outcome in states that adopted the secret ballot would have followed the same trends as those in states that experienced no change in ballot format (that is, parallel trends).[17] This estimation strategy improves upon the research designs used in previous scholarship on the secret ballot. For example, pooled, cross-sectional designs (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 behaviour.[18] While other studies compare the same legislators before and after reforms were implemented (Carson and Sievert 2015), this approach does not account for secular trends in legislative behaviour and political representation that could also be correlated with the adoption of the secret ballot.[19] Finally, because ballot laws were implemented at the state level, research that does not cluster standard errors on states (Carson and Sievert 2015; Finocchiaro and MacKenzie 2018; Wittrock et al. 2008) may produce misleadingly small standard errors and generate inappropriate statistical inferences.

Results

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

Government Outputs: Pensions, Post Offices, and Rivers and Harbours Projects

We begin by examining the effect of the secret ballot on government outputs. Table 2 shows results for the state-level distribution of pensions (Panel A), post offices (Panel B), and rivers

[15]For 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.

[16]District fixed effects are specific to the relevant redistricting cycle.

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

[18]The two-way fixed effects approach that we employ can result in problematic comparisons between treated and already-treated units. We also present results from three alternative estimators, which ensure appropriate comparisons, in Appendix Section A.8. The results from these estimators are similar.

[19]This issue also complicates inference in pooled cross-sectional designs, which generally do not 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.

https://doi.org/10.1017/S0007123423000091 Published online by Cambridge University Press

Table 2. The Australian ballot and the distribution of federal resources

	1	2	3
Panel A. Pensions			
Australian ballot	−0.061	−0.069	−0.038
	(0.052)	(0.040)	(0.038)
Panel B. Post offices			
Australian ballot	−0.007	−0.003	−0.049
	(0.046)	(0.041)	(0.034)
Panel C. Rivers and Harbours			
Australian ballot	−0.121	−0.140	0.252
	(0.372)	(0.418)	(0.242)
Congress fixed effects	✓	✓	✓
Unit fixed effects	✓	✓	✓
Controls		✓	✓
State-specific trends			✓
Panel A observations	800	800	800
Panel B observations	893	893	893
Panel C observations	532	532	532

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

and harbours projects (Panel C). The dependent variable for each panel, respectively, is the logged (plus one) count of pensioners in the state, the logged (plus one) count of post offices in the state, and the logged (plus one) appropriations to rivers and harbours projects in the state.[20] We estimate three specifications for each measure. Model (1) represents a standard, generalized difference-in-differences approach without time-varying covariates, model (2) introduces time-varying control variables, and model (3) includes time-varying covariates and unit-specific linear trends, which reduce our reliance on the parallel trends assumption and address the possibility that the secret ballot was correlated with other trends in the provision of government outputs.

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

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

We now evaluate the effects of the secret ballot on legislative behaviour. We study whether the secret ballot affected the behavioural inputs that could secure constituency benefits. We first analyse the number of private bills introduced by each legislator from 1881 to 1930. These private

[20]Appendix Section A.6 shows results for alternative measures of these three dependent variables: the dollar amount of pensions granted to state residents, the number of post offices per 1,000 in the population in each state, and the count of rivers and harbours projects. Results are similar to those in Table 2. We also find similar results when creating an index of government outputs based on the counts of pensions and post offices and appropriations to rivers and harbours project. See Table A.4.

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Table 3. The Australian ballot and legislator effort

	1	2	3	4	5	6
Panel A. Private bill sponsorship						
Australian ballot	−0.202	−0.226*	−0.039	−0.114	−0.126	0.064
	(0.107)	(0.101)	(0.091)	(0.137)	(0.115)	(0.066)
Panel B. Roll-call participation rate						
Australian ballot	−0.048*	−0.050*	−0.060*	−0.017	−0.025	−0.020
	(0.019)	(0.018)	(0.025)	(0.021)	(0.018)	(0.025)
Panel C. Floor speeches						
Australian ballot	−0.081	−0.163	−0.102	−0.079	−0.070	−0.066
	(0.149)	(0.132)	(0.124)	(0.097)	(0.101)	(0.115)
Congress fixed effects	✓	✓	✓	✓	✓	✓
District fixed effects	✓	✓	✓			
Member fixed effects				✓	✓	✓
Controls		✓	✓		✓	✓
District/member-specific trends			✓			✓
Panel A observations	8,845	8,845	8,845	8,845	8,845	8,845
Panel B observations	8,840	8,840	8,840	8,840	8,840	8,840
Panel C observations	8,845	8,845	8,845	8,845	8,845	8,845

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

bills primarily address claims made on behalf of individual constituents (often related to war pensions or appealing for relief). If the secret ballot provided electoral incentives for members of Congress to generate personal reputations, we expect to observe increased bill sponsorship following its adoption. We also examine participation in the legislative process using the roll-call participation rate and the count of House floor speeches.[21] Finally, we estimate models using district and Congress fixed effects and, separately, member and Congress fixed effects. The latter empirical strategy evaluates whether the secret ballot produced changes in bill sponsorship levels within legislators (that is, electoral incentives), while the former also examines whether the secret ballot resulted in legislators exerting greater effort due to electoral constituencies choosing legislators who invested greater effort in introducing private legislation (that is, both electoral incentives and electoral selection). We also estimate models that contain time-varying control variables and member-specific and district-specific linear trends.

Table 3 displays the results. Panel A provides no evidence that the secret ballot increased private bill sponsorship. Only one of the six coefficients is positively signed and is not statistically significant at conventional levels. In fact, one of the five negatively signed coefficients is statistically distinguishable from zero.[22] The legislative agenda in the turn-of-the-century Congress was dominated by private bills (Finocchiaro and MacKenzie 2018), which could have been a valuable way for legislators to exhibit their attentiveness to issues important to their constituencies in an

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

[22]We do not interpret these results as evidence that the Australian ballot decreased production in private legislation. However, one plausible explanation is that ballot reform could have resulted in the election of progressive legislators who were strongly suspicious of private pension legislation and its favouritism 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 between states that had adopted the Australian ballot.

https://doi.org/10.1017/S0007123423000091 Published online by Cambridge University Press

attempt to cultivate personal reputations. Across various model specifications, however, we find no evidence that the secret ballot increased private bill sponsorship.

Examining roll-call participation and floor speeches, the estimates again do not indicate that the secret ballot increased legislators' efforts on behalf of their constituents. All twelve estimated coefficients in Panels B and C have negative signs, which is the opposite direction of the hypothesized relationship. Surprisingly, three estimated coefficients for roll-call participation are statistically significant from zero (the three district fixed effects specifications). These results imply that, under the secret ballot, voters selected representatives who participated in 5–6 percentage points fewer roll call votes than representatives elected under the party ballot. One speculative explanation is that these members were less beholden to party pressure and whips and, thus, could more easily skip votes. Nevertheless, the results in Table 3 do not indicate that the secret ballot changed behaviour between legislators who were newly motivated to secure personal reputations.[23] Combined with the results in Table 2, these findings weigh strongly against claims that the secret ballot meaningfully affected congressional representation.

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

In additional analyses, we explore the implications of the above findings for legislators' responsiveness to electoral constituencies and political parties. In doing so, we study whether and how the secret ballot altered the relevant principals for members of Congress. Proponents of ballot reform argued that the secret ballot would strengthen the agency relationship between voters and officeholders and weaken state and local party control. We propose a new measure that gauges the extent to which representatives exhibited greater discretion in their roll-call voting relative to the state party organization that controlled ballot access prior to the Australian ballot. In addition, we also implement two commonly used approaches in the existing literature to assess legislator responsiveness to constituent preferences and party loyalty in Congress (as measured by party unity scores).

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

We focus on the period prior to the 17th Amendment and assume that members of the Senate during this period are agents of state party organizations. We measure each state party's preferences using Senators' Common Space NOMINATE scores.[24] For instance, if a state has two Republican senators, the average of those two scores characterizes the ideological position of the state party. If only one senator in a state represents a given party, we assume that the senator's roll-call score is the position of that state party.[25] We then calculate the squared distance between each member of a state's House party delegation and the estimated position of the state party as proxied by that state's Senate party delegation, as described above. Finally, we take the mean of these squared distances to characterize the level of discretion afforded to a state's party delegation in the House, where larger values of this variable indicate greater ideological differences between

[23]Our conclusions remain unchanged when creating an index of legislative effort using these three indicators. See Table A.5.

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

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

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Table 4. Discretion from the state party on roll-call voting

	1	2	3	4
Australian ballot	−0.002	−0.000	−0.003	−0.002
	(0.003)	(0.004)	(0.003)	(0.004)
State FEs	✓	✓		
Congress FEs	✓	✓		
State-party FEs			✓	✓
Congress-party FEs			✓	✓
Unit-specific trends		✓		✓
Observations	746	746	746	746

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

House members and state party leaders.[26] If the secret ballot decreased subnational party control over legislators, we would expect that the secret ballot is associated with larger values of the discretion variable.

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

We also examine the consequences of the secret ballot for responsiveness to constituent preferences. Using a strategy similar to Gailmard and Jenkins (2009), we study whether the secret ballot increased legislators' responsiveness to constituency preferences as measured by roll-call voting records. Details on this analysis and the results are reported in Table A.22 in Appendix Section A.14. While we find that voters select more liberal (or conservative) representatives as the district shifts in a more liberal (or conservative) direction, we uncover no evidence indicating that this relationship strengthened with the secret ballot.

Finally, we study the relationship between the Australian ballot and legislators' party unity scores. If the Australian ballot weakened legislators' connections to their parties and thus their party loyalty, we would expect legislators elected under the secret ballot to exhibit decreased party unity in their roll-call voting. Details are reported in Table A.23 in Appendix Section A.14. While all of the estimated coefficients for the main specifications are negative, the magnitude of the estimates is very small. Examining the estimates across all specifications, two of the thirty are statistically significant. Again, however, all of the estimated coefficients are small in magnitude, with even the largest implying a minimal decline in party loyalty in response to the Australian ballot.

We find little evidence that the Australian ballot transformed legislative behaviour toward the relevant principals. Upon ballot reform, legislators did not exhibit greater responsiveness to local constituent preferences, nor did they markedly reduce their commitment to national party positions or exhibit behaviour consistent with greater discretion from their state party organization.

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

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

where H_{ispt} indicates the ideal point of representative i in state s and party p in Congress t, $\bar{S}_{spt}$ represents the state party's ideal point as measured by the average of the roll-call scores of the Senate delegation in state s and party p in Congress t, and n_{spt} is the number of members in the House delegation of state s and party p in Congress t. Results for a measure of discretion based on the mean absolute distance, rather than mean squared distance, are very similar (see Appendix Table A.6).

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To be sure, each of the analyses discussed in this section has important limitations related to measurement and modelling. However, the totality of the evidence suggests that the Australian ballot had less of a transformative impact on political representation than its proponents argued it would. The results also offer an explanation for our earlier null findings: because the Australian ballot did not produce systematic changes in the principals to whom legislators respond, it is perhaps not surprising that we do not observe differences in government outputs or legislative behaviour upon the introduction of the secret ballot.

Robustness Checks

The results are robust to a wide range of additional analyses. First, we estimate models that distinguish the effects of the party column ballot from the office bloc ballot. While no state repealed the secret ballot after adopting it, states varied in how they implemented the secret ballot, and sixteen states switched at least once between the party column and office bloc formats. Because the party column ballot format resembled, in certain respects, the party slip ballots used previously, it is possible that any potential effects would be concentrated in states with the office bloc. As Appendix Section A.9 shows, however, we find no consistent evidence that either the office bloc or party column ballots affected government outputs or legislative behaviour. In fact, in some models, we find that the office bloc was associated with more negative representational consequences relative to the party column ballot (see, for example, Table A.12), which runs contrary to expectations.

Second, we examine the potential anticipatory effects of the secret ballot by regressing outcomes in Congress t on the use of the secret ballot in the *next* election. The timing of the secret ballot's enaction varied by state, in some cases providing ample time for legislators to strategically adjust their behaviour in anticipation of standing for re-election by secret ballot in the next election. The results are shown in Appendix Section A.11. These analyses continue to support our main findings, however, but we find no evidence that our estimates above are overly conservative due to anticipatory behaviour between 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 Section A.12. Across dozens of analyses, we find zero significant estimates in the hypothesized direction for our estimated coefficients of interest. We also explore whether the effect of the Australian ballot was concentrated in only one of the parties in Appendix Section A.10 but find no evidence supporting this possibility.

Finally, for dependent variables that are measured in counts (for example, the count of pensioners), we estimated models using Poisson regressions rather than using OLS with log transformations. The results from these models are very similar to the equivalent specifications reported in the main text and are shown in Appendix Section A.6.

Summary of Results

With the number of outcomes under consideration and the array of alternative specifications and measurement strategies, we report more than 300 estimated coefficients of interest in the main text and appendix. Table 5 provides a high-level summary of these estimates, indicating whether the sign of the estimate is in the hypothesized direction and whether the estimate is statistically significant from zero. The table presents a comprehensible (albeit coarse) summary of all estimates reported here and in the appendix. Overall, only about a third of the estimated coefficients are in the hypothesized direction and just over one per cent are both in the hypothesized direction and statistically significant from zero. In other words, we observe

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Table 5. Comprehensive summary of results

Outcome category	In Hypoth. direction	Significant in hypoth. direction	In opposite direction	Significant in opposite direction	Total
Government outputs	32 (31.4%)	1 (1.0%)	70 (68.6%)	2 (2.0%)	102
Leg. effort	22 (17.5%)	1 (0.8%)	104 (82.5%)	23 (18.3%)	126
Leg. ideological behaviour	39 (52.7%)	2 (2.7%)	35 (47.3%)	1 (1.4%)	74
Total	93 (30.8%)	4 (1.3%)	209 (69.2%)	26 (8.6%)	302

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

slightly fewer significant estimates in the hypothesized direction than we would expect to observe merely by chance.

Our interpretation of these results is that, across a bevy of relevant outcomes and sensible alternative specifications, we find essentially no evidence supporting the claim that the Australian ballot altered the character of political representation in the United States. As discussed above and in greater depth below, it is highly plausible that the eventual rise of candidate-centred politics in the latter half of the twentieth century would not have occurred without ballot reform. However, it is nearly impossible to infer from the analyses that ballot reform by itself had a quick, direct, and transformative effect on American democracy as many accounts claim.

Contextualizing the Non-Effects of Ballot Reform

We have found strikingly little evidence that the Australian ballot transformed American democracy. In this section, we discuss four possible explanations for our null results.

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

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

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

> When you consider the labour 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.[27]

[27]2066 House Report 466, 16 February 1882.

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Horr subsequently enumerated the typical constituent requests for legislators, which included attending to land patents, homestead claims, patent applications, military discharges, treasury claims, postal routes, mail clerks, and sources of federal employment (among other items). While the Garfield and Horr examples are but two legislators who served in the decades prior to the introduction of the secret ballot, their accounts are consistent with the possibility that legislators were already exerting considerable effort to serve their constituents. Indeed, previous scholarship documents an incumbency advantage in post-Reconstruction America (Ansolabehere, Snyder, and Stewart 2000; Carson, Engstrom, and Roberts 2007), suggesting that legislators were already invested in developing personal votes prior to ballot reform. While corruption and graft may have accompanied party ballots, it does not necessarily follow that constituents received low-effort or low-quality representation as a consequence. Therefore, the secret ballot may not have substantially improved the quality of political representation because legislators were already advocating for their constituents.

Second, even if the secret ballot enabled voters to cast votes for candidates from different parties across offices, the nature of the campaign and/or news environment may not have meaningfully provided voters with the information that would permit them to do so. We explore this possibility using data from Ban et al. (2019) and report the results in Appendix A.16. We find little evidence that the secret ballot increased the supply of news coverage about political candidates. Other research similarly documents the absence of an association between ballot reform and increased newspaper advertising by congressional candidates (Hirano et al. 2022). Therefore, the secret ballot may not have generated the effects anticipated by its proponents because the information environment did not meaningfully change with its introduction. Given that the direct primary had a more significant effect on candidate-centred campaigning (Hirano et al. 2022), this could also explain why that reform had more substantial implications for political representation (Hirano and Snyder 2019).

Third, the Australian ballot may not have meaningfully weakened party control over candidates and legislators. While initial proponents of ballot reform were anti-party in orientation, parties had less control over balloting in the previous system than they would have preferred and recognized the potential benefits of reform (Ware 2000, 12–13). Consequently, Ware (2002, 40) argues that 'party elites took the reform in a very different direction from the one [antipartisan reformers] had intended'. Ballot reform also enhanced party control over ballot access, which limited the opportunities for local party organizations to undermine the candidates preferred by party elites. In addition, while the secret ballot may have made it more difficult for parties to monitor voter behaviour explicitly, historical accounts indicate that parties devised alternative means of exchanging bribes, including through dissuading turnout and registration (Ostrogorski 1908, 346–7). Collectively, these accounts suggest that ballot reform enhanced party control rather than popular control over elected officials.

Fourth, the secret ballot may not have meaningfully affected ticket splitting as much as some accounts suggest. Reynolds and McCormick (1986) provide evidence that ticket splitting is meaningfully underestimated by most of the literature in the period prior to ballot reform. They also show that ticket splitting declined in the decade following ballot reform, at least in New Jersey and New York, due in part to rules passed that specified exacting procedures voters needed to follow to cast a split ticket. Even when ticket splitting increased by the mid-1900s, it most commonly occurred with respect to the two top executive offices on the ballot (president and governor). In all, this historical evidence suggests that the ballot reform had more minimal implications for split ticket voting in congressional elections than previous accounts have indicated.

To recapitulate, we do not deny the parties' control over political affairs in the late-nineteenth century. Our evidence suggests, however, that party control over elections may not have come at the expense of constituent representation. Primary accounts point to considerable legislative effort exerted by members prior to ballot reform. Analyses of the news environment demonstrate that candidates did not receive greater coverage following the secret ballot's enactment. Moreover,

https://doi.org/10.1017/S0007123423000091 Published online by Cambridge University Press

the design and implementation of ballot reform may not have occurred in a way that effectively undermined party control or led voters to split their tickets to support individual House candidates. These possibilities, which are not mutually exclusive, contextualize our lack of evidence about the relationship between the secret ballot and political representation.

Conclusion

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

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

Instead, our results contribute to scholarship that subjects popular claims about the effects of institutional change to empirical scrutiny. For instance, recent scholarship has found that the effects of electoral competition (Moskowitz and Schneer 2019) and legislative term limits (Olson and Rogowski 2020) on political outcomes are quite different in comparison with what their advocates commonly claim. Likewise, the secret ballot maintains a revered status among turn-of-the-twentieth-century reforms, yet previous attempts to evaluate its effects have used research designs not especially well-suited for the task. Using an appropriate research design, however, we show that earlier scholarship likely overstates its consequences.

More speculatively, we contend that the secret ballot is a necessary but not sufficient condition for candidate-centred electoral politics. While anecdotal evidence suggests that members of Congress exerted considerable effort on behalf of their constituents prior to the secret ballot, they may have lacked the institutional capacity to improve upon those efforts. Consider, for example, that the Legislative Reorganization Act of 1946 dramatically expanded the resources available to members of Congress. Prior to this, legislators were mainly limited to the work they could accomplish on their own. If the Australian ballot had appeared when legislators had greater institutional resources, it is possible that 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 necessary for its full effects to be realized.

We close by noting several limitations of our study and potential directions for future research. First, while our data measure the production of legislative proposals by individual representatives, they do not indicate legislators' success in securing them. Given the high petition rates 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 remains unanswered. Second, our

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data do not evaluate whether the secret ballot affected the production of substantive public policy. Future research could evaluate whether legislators elected through the secret ballot were more likely to develop and pass programmatic policy proposals. Third, while our data focuses on the House of Representatives, it is less clear whether our findings generalize to other officials such as governors and state legislators. As historical data on the U.S. Congress and states are more easily accessible and widely available, additional research can shed light on these and other important questions.

Supplementary material. Online appendices are available at https://doi.org/10.1017/S0007123423000091.

Data Availability Statement. Replication Data for this article can be found in Harvard Dataverse at: https://doi.org/10.7910/DVN/NIZT5R.

Acknowledgements. Previous versions of this manuscript were presented at the 2019 Midwest Political Science Association meeting, the 2019 meeting of the Australian Society for Quantitative Political Science, and the 2020 meeting of the Southern Political Science Association. We thank John Baughman, Dan Carpenter, Chuck Finocchiaro, Sam Kernell, Zhao Li, Ken Shepsle, Andrew Sinclair, Dan Smith, Jim Snyder, and Adam Zelizer for helpful discussion; Christopher Gibson for exceptional research assistance; and Jim Snyder and Rick Wilson for generously sharing some of the data used in this project.

Financial Support. None.

Conflicts of Interest. The author(s) declare none.

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Cite this article: Moskowitz DJ, Rogowski JC (2023). Ballot Reform, the Personal Vote, and Political Representation in the United States. *British Journal of Political Science* 1–18. https://doi.org/10.1017/S0007123423000091