Effects on Reelection Rates of the Introduction of Merit Civil Service Appointments in US States*

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Abstract

Prior scholarship contends that control over patronage appointments confers the incumbent an electoral advantage. We study the introduction of state-level legislation that abolished patronage appointments to the civil services of the 50 US states between 1900 and 2016. Using recently-developed statistical methods appropriate to reform's staggered introduction, we show that legislators were much less likely to be reelected during the patronage era than after the introduction of civil service reform. Reelection rates for legislators significantly and substantially increase following reform, when political careers also lengthen. We explore both selection and performance explanations for this surprising result. [118 words]

10638 words (excluding abstract and appendices)

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Introduction

Control over patronage appointments is commonly believed to electorally advantage political parties and politicians already in office. Under a patronage regime, incumbents can stack the bureaucracy with their cronies, thereby indirectly controlling votes in multiple ways. Thus, the abolition of patronage appointments is typically studied as a puzzling example of the sacrifice of immediate political gain in favor of long-term collective responsibility (Geddes 1994). In this paper, we analyze how the adoption of merit civil service regulations over more than a hundred years across the 50 states in the United States affected reelection rates of individuals serving in state legislatures. Contrary to expectations, we show that the abolition of patronage appointments *improved* individual reelection rates by somewhere between five and as much as 10 percentage points, making this arguably the single most consequential institutional reform in US history to affect reelection rates. This surprising outcome suggests that civil service reform may carry previously unexplored political consequences; these in turn may reshape our theoretical understanding of it and introduce new avenues of research.

The main contribution of this research is to use a range of statistical techniques to document the causal impact of reform on reelection rates. As a secondary matter, we probe evidence for two different channels that might account for the surprising outcome that we uncover. The first is post-reform entry into office by more politically ambitious and professional individuals, those who then more often sought and won reelection (a selection effect). The second is their delivery of more visible and consequential legislation once in office, thanks to their own greater legislative experience as well the more professional state bureaucracy assisting them (a performance effect). Data limitations prevent us from providing more than suggestive evidence regarding either mechanism, although analysis of available data corroborates the importance of selection but not performance. These provisional results suggest the value of future data collection and analysis.

Why might civil service reform improve reelection rates? The secondary literature provides hypotheses. Civil service reform made entry into the public bureaucracy a function of competitive examinations rather than one of the spoils of office. With the passage of reform, political parties could no longer rotate their loyalists between elected and appointed public offices, as had occurred during the era when partisan politics was dominated by division of the spoils (Engstrom and Kernell 2014). Once rotation into the state bureaucracy was blocked, the incentives of those holding elected positions to retain them increased. Elected officials then invested greater efforts in building careers as legislators. As a result, the adoption of civil service reform meant that party operatives began to be replaced by more professionally-oriented politicians, those characterized by long-term policy and legislative commitments and expertise. Substantial data limitations prevent systematic exploration of many aspects of this hypothesized scenario. In particular, very little state level data is available for the first half of the 20th century, thereby preventing analysis of the characteristics of those serving as state legislators, their overall career paths, and their legislative efforts. Nonetheless, our analysis of incomplete data suggests that post-reform elected legislators were more eager to retain office than their pre-reform counterparts.

At the same time, civil service reform also transformed the civil service into a more professional body, one that could be better entrusted to implement legislative decisions (Moreira and Pérez Forthcoming). A professional bureaucracy helped politicians fulfill programmatic commitments to voters. Thus, the new class of politicians that gradually emerged after civil service reform interacted more effectively with more qualified civil servants to deliver state services.

The dual-sided transformation we describe is still incomplete for state houses. Many part-timers hold office in legislative bodies that themselves operate only part time (Squire 2017) and where reelection rates lag those of the national Congress. But state-level developments parallel those characterizing the national Congress, with the growth of legislative professionalism (Berry, Berkman, and Schneiderman 2000). The development of internal institutions that protect incumbents has contributed to reelection rates in Congress that are higher than any other electoral democracy in the world (Golden and Nazrullaeva 2023).

Our paper proceeds as follows. We first briefly review the idea that control over patronage offers electoral advantages, an idea that largely prevails in the literature. We also explain our theoretical motivations for hypothesizing the reverse. In section two, we present the data and measures that we use. Section three discusses the statistical methods of analysis employed. We then present our main results. Following that, we provide preliminary explorations of two possible mechanisms that could explain why civil service reform led to higher reelection rates: selection and performance effects. A final section concludes and discusses the way forward.

Patronage as an electoral advantage

Meritocratic appointment of civil servants has long been known to improve the performance of public bureaucracies, and very possibly to encourage economic development more broadly (Rauch and Evans 2000; Dahlström and Lapuente 2022; Besley et al. 2022). The general view is that competitive examinations result in higher quality bureaucrats than patronage appointments, since the latter allow politicians to staff the civil service with their loyal followers — followers of lower quality than those who would be appointed on the basis of merit (Lewis 2007; Colonnelli, Prem, and Teso 2020). Accordingly, research finds that a meritocratic civil service outperforms one staffed with patronage appointments (Johnson and Libecap 1994; Ornaghi 2019), which helps explain why middle class progressives, including civil servants themselves, historically push for the abolition of patronage in the US (Reid and Kurth 1988; Reid and Kurth 1989; Theriault 2003; Anzia and Trounstine 2024). The impact on the careers of politicians has been less studied but it is generally assumed that control over patronage appointments confers political advantages to incumbents. One reason is that a patronage-based bureaucracy provides them campaign resources and votes. Not surprisingly, then, incumbents tend to resist the move from patronage to meritocratic appointment technologies.

Historical studies of the introduction of competitive examinations for civil service jobs center on the Pendleton Act, adopted by the United States federal government in 1883. This remains the best-studied instance of the passage of civil service legislation anywhere in the world. The adoption of the Act has been historically been interpreted as the work of progressive reformers who represented a growing middle class, which was in turn determined to put an end to the spoils system awarding patronage appointments to whichever political party controlled executive office (Hoogenboom 1959). Important motivations of reformers included the desire to reduce corruption and cronyism and to improve the delivery of public services. Recent relevant studies in line with this view include Folke, Hirano, and Snyder (2011) and Ting et al. (2013), which analyze US data to argue that patronage systems allowed political parties to retain control of state legislatures, and also Aneja and G. Xu (2024), which documents efficiency gains as a result of the Act.

Given that incumbents benefit electorally from control of patronage, analysts have struggled to articulate theories of successful reform. One well-known interpretation is that reformers succeed only when they are joined by governing political parties who foresee loss of office, or who find themselves in newly-competitive political environments (Dresang 1982; Geddes 1994; Kernell and McDonald 1999; Reid and Kurth 1988; Reid and Kurth 1989; Mueller 2015; Ash, Morelli, and Vannoni 2022). This nuanced view interprets the transition to meritocracy as part of a changing political landscape — perhaps one that accompanies fundamental underlyingunderly processes of economic modernization — to which incumbent political parties respond strategically to retain an ability to win elections. As far as we are aware, however, no one has used systematic historical data to investigate the impact of civil service reform on incumbent legislative careers. It has perhaps been assumed that reform is detrimental to incumbent electoral fortunes at the levels of both governing parties and individual legislators.

The outcome we study — reelection rates — has been studied as a function of institutional and political variables. Among institutional variables, the literature has identified ballot type (Katz and Sala 1996; Engstrom and Roberts 2020; Moskowitz and Rogowski 2024), electoral system (Carey and Shugart 1995), congressional seniority (McKelvey and Riezman 1992), and gerrymandering and redistricting (Cox and Katz 2002); political variables that have been highlighted include challenger quality and accompanying scare-off effects (Cox and Katz 1996; Levitt and Wolfram 1997; Eggers 2017) and fund-raising capacity (Fouirnaies and Hall 2014). These literatures were in part motivated by the increasing reelection rates of US congressional representatives that started in the 1960s (Fiorina 1977; Jacobson 1987; Gelman and King 1990; Lee 2008).

Decades of prior American politics scholarship had already revealed turnover rates of lower house state legislators substantially in excess of 60 percent in the late 19th and early 20th centuries, gradually declining until stabilizing at about 30 percent in the 1980s (Hyneman 1938; Shin and Jackson 1979; Niemi and Winsky 1987).[1] The new and complete dataset that we have assembled of individuals serving in statehouses across the United States covers the period from 1900 to 2016 (Golden and Nazrullaeva 2024). Our analysis of it reinforces earlier literature that shows decreasing turnover and increasing individual reelection rates over the course of the 20th century. Our statistical analyses establish a causal relationship between the introduction of civil service reform and this already well-documented fact. On average, reelection rates rose by between five and 10 percentage points over the first four elections after reform legislation was adopted, stabilizing thereafter. Although reelection rates were increasing prior to reform, its adoption caused a large, sharp, and permanent additional increase. To the best of our knowledge, prior literature has not identified this consequence of reform.

Work that is theoretically close to ours concerns the impact of the adoption of the Australian ballot. The Australian ballot consolidated party tickets onto a single, standardized and officially-printed ballot, making partisan choice secret. As others have noted, "As with any institutional change of this magnitude, ballot reforms led to a number of unanticipated outcomes" (Carson and Sievert 2015, p. 84), including the adoption of direct primaries. The Australian ballot shifted onto individual candidates the burden of obtaining voter recognition; candidates had to secure votes independently of their party with the abolition of the party ticket. Perhaps surprisingly, this individualization of the need to credit-claim resulted in more stable committee assignments in Congress (Katz and Sala 1996; Wittrock et al. 2008); as far as we are aware, no one has studied the impact of the adoption of the Australian ballot on incumbency reelection rates, although research shows an immediate reduction in members of Congress seeking reelection (Carson and Sievert 2015). Like the adoption of the Australian ballot, the passage of civil service reform reduced the role of political parties in candidate selection and provided yet another incentive for candidates to seek individual

[1] The literature defines turnover as the percentage of new members in a legislative chamber. The concept, while not the inverse of reelection rates, is obviously closely related. High rates of turnover imply low reelection rates.

name recognition. By studying its effects over multiple subsequent electoral periods, we are able to assess whether reform created a new and more stable equilibrium, one marked by greater rates of rerunning among incumbents.

In advance of collecting and analyzing the data we report here, we pre-registered the hypothesis that abolition of patronage appointments to the civil services of the US states would improve the reelection rates of individual legislators (the pre-analysis plan is reproduced as Appendix M). Pre-registration increases scientific transparency, and in particular has the virtue of preventing analysts from engaging in p-hacking or after-the-fact hypothesizing (Humphreys, Sanchez de la Sierra, and van der Windt 2013). However, it is unusual for analysts of observational data to have the luxury of pre-registration, since the results of the analysis are often likely to be known in advance thanks to prior research. In this case, because we were collecting data that had never been subjected to the hypothesis that motivated us, we had the good fortune that we did not know in advance if our hypothesis was correct.

Our initial reasoning was that a patronage regime in which powerful politicians and governing political parties controlled selection encouraged ”bad” types to enter the political arena whereas the imposition of a more law-abiding regime would encourage ”good” types to enter. Due to data scarcity, we remain agnostic about this theory; we have no measures to capture politician types. However, our results verify a dramatic shift in reelection rates following civil service reform, and we present some preliminary evidence that corroborates that selection effects may be at work. For a discussion of deviations from our pre-analysis plan, see Appendix F.

Data and measures

Civil service reform across the US states first emerged in the post-Civil War era as part of a political movement that supported a wave of measures aimed at improving governance. For most states, passage was long delayed until well into the 20th century. Other “good government” reform measures that were adopted in the late 19th and early 20th centuries included the establishment of city managers in the place of mayors and the adoption of the commissioner form of government, the Australian (non-partisan) ballot, and at-large elections (Carreri, Payson, and Thompson 2023). Civil service reform became detached from this larger progressive agenda and was adopted at the state level more than 50 years later. Because it was not passed alongside other reform measures, we can empirically isolate its effects.

The core of the 1883 Pendleton Civil Service Reform Act was to require aspiring civil servants to sit competitive examinations in order to be appointed. Prior to reform, politicians exercised discretion in appointments to government posts, to which they normally named their personal followers. Government posts were transitory, as appointees were dismissed once their patron lost office. While in office, patronage appointees were expected to make donations to the political campaigns of their patrons (Hoogenboom 1959), binding politicians financially to their appointees.

Following the passage of federal civil service reform, state legislatures passed similar legislation over the next 106 years. New York and Massachusetts passed civil service reform legislation in 1883 and 1884, respectively (Hoogenboom 1961, ch. 14); the remaining 46 (later 48) states adopted similar legislation over the course of the 20th century, with the exception of Texas, which never passed reform legislation. A flurry of adoptions took place after 1939, when Congress amended the Social Security Act to require that state level departments administering federal social security funds be staffed with merit appointees (Ash, Morelli, and Vannoni 2022). Nonetheless, diffusion is generally depicted as endogenous to each state (Ruhil and Camões 2003; Ujhelyi 2014b). The heart of civil service reform legislation consisted of the requirement that aspiring entrants sit competitive examinations (Ujhelyi 2014b).

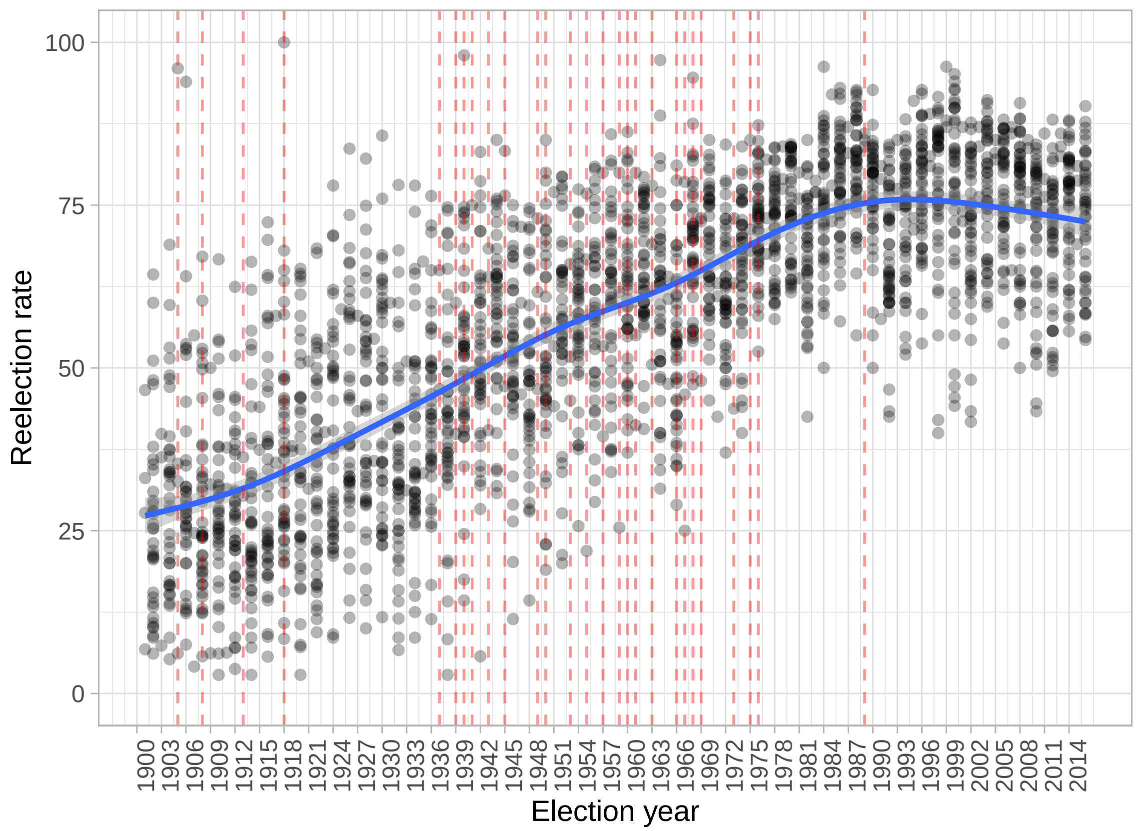
Civil service protections and merit-based competitive examinations have remained in force across most states since then, although Arizona, Colorado, and Tennessee — among others — adopted major modifications in the 2010s and there has been increasing political pressure elsewhere to roll back civil service examinations and protections.

In Figure 1, we depict state-level average reelection rates from 1900 to 2016 for 45 US states,[2]

[2]As discussed in the next section on data, we exclude Mississippi, Maryland, Louisiana, Alabama, and Nebraska.

with civil service reform adoption indicated by red vertical lines. As the data demonstrates, state level civil service reform is concentrated in the 40-year period between 1936 and 1976. Pre-reform reelection rates remained stubbornly below 50 percent, and the average US statehouse experienced reelection rates over 50 percent only after World War II. The high and increasing reelection rates of state legislators as of the 1960s have received considerable scholarly attention, in a literature that parallels that studying the incumbency advantage of members of the Congress (Jewell and Breaux 1988; Breaux 1990).

Figure 1: Reelection rates for US state legislatures, 1900–2016



Note: Each dot represents a state legislature.
Line with confidence intervals produced by LOESS smoothing.
Red vertical lines indicate civil service reform adoption in at least one state.
Alaska and Hawaii included post−1959.

In the next sections, we briefly describe the data we use.

Civil service reform

We use already-assembled data on the dates of civil service reform adoption (Ash, Morelli, and Vannoni 2022, table A1, col 4, p. 33); details appear in Appendix A. No state abolished civil service reform once enacted (although some states weakened aspects of it).[3] Only New York and Massachusetts adopted civil service legislation prior to 1900. We have a complete data matrix for all states for the post-1900 period, and thus all other reforms fall within the scope of our statistical analysis. In Appendix F (Figure F-3), we consider alternative dates of reform adoption on the basis of Ujhelyi (2014a) and Ting et al. (2013).

Reelection rates

We combine data on civil service reform adoption dates with candidate-level information about state legislative election results. We digitize archival data to assemble a complete individual-level dataset on all elected officials serving in the state legislatures since 1900 (Golden and Nazrullaeva 2024). The dataset we create complements already-existing historical datasets (Ansolabehere, Ban, and Snyder 2017; Klarner 2018) which, however, were missing most observations for the first half of the 20th century. For details, see Appendix B.

The data we assemble includes legislator name, election year, and state but no other substantive information; for instance, we do not have consistent and complete information on partisan affiliation, occupation, age, gender, or any other observable characteristic of legislators. A wealth of this kind of data on state legislators and legislatures exists but only for subperiods during the latter half of the 20th century (e.g. Bucchianeri, Volden, and Wiseman (2024)). The absence of complete timeseries data prevents us from analyzing many potentially interesting aspects of reelection, such as partisanship. As much as possible, we work with all elections for all 50 US states for the period from 1900 to 2016.[4] The total number of elections included in the analysis below is 2535, and our

[3] A few states experienced problems with initial implementation (Ting et al. 2013), but the coding we use reflects this.

[4] Alaska and Hawaii joined the Union in 1959. Alaska adopted civil service reform in 1960 and Hawaii in 1955; reelection data for both states begins in 1958. In effect, therefore, both states are always coded as reformed for the period for which we have reelection data. Four states (Mississippi, Maryland, Louisiana, and Alabama) hold elections

dataset includes 127313 individual legislators, some of whom served multiple terms.[5]

We also collect data on term limits in state legislatures because these directly alter the pool of politicians and the maximum possible reelection rate. Term limits emerged in the 1990s; they do not coincide temporally with civil service reform but reelection rates in the post-reform period could be influenced non-randomly by term limits. We use the presence or absence of term limits as a control in some specifications. Of the 50 states, 17 adopted term limits at some point during the years we study. See Appendix C for details.

We measure the reelection rate at time t as the proportion of legislators serving in the $t-1$ legislature who are reelected into the legislature at time t. For details about the construction of the reelection dataset, see the codebook, available as Appendix N.

Description of the data

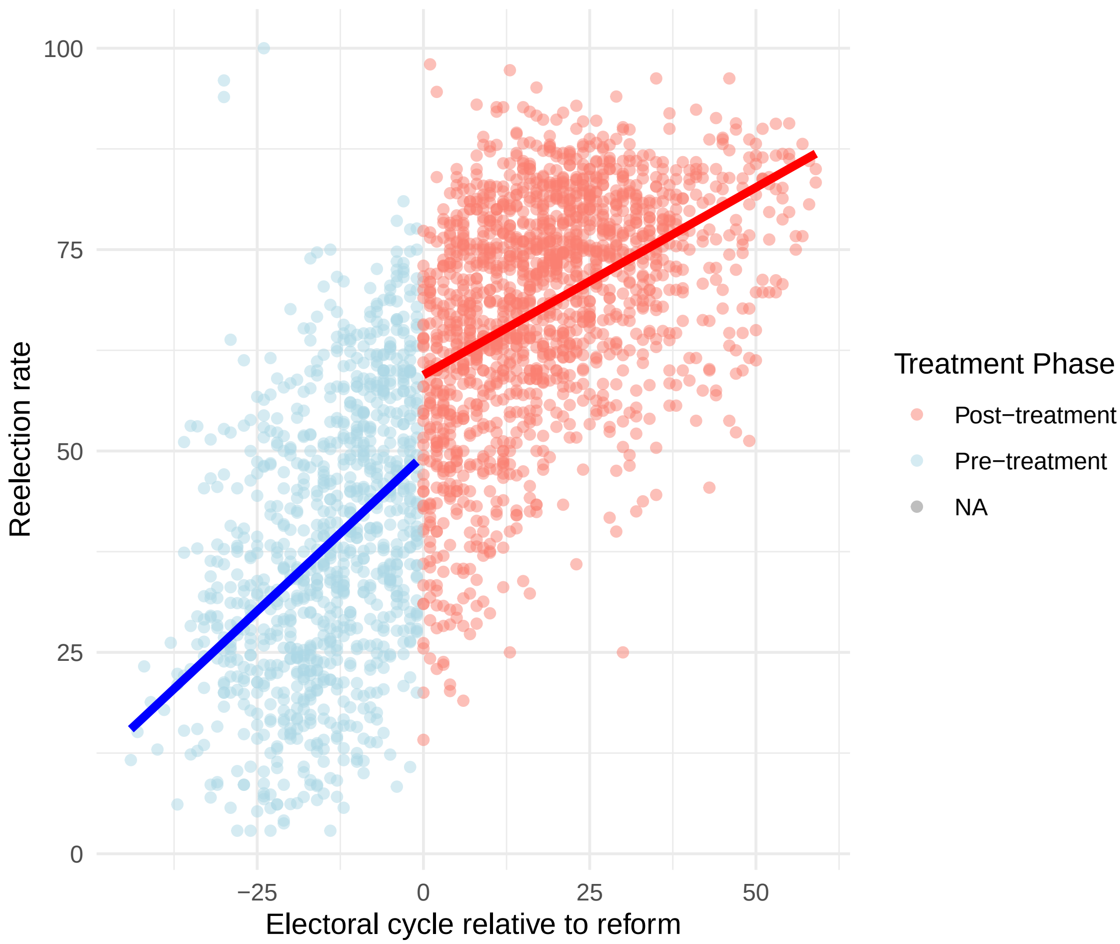
As we have documented in Figure 1, state legislatures in the Unites States see increasing reelection rates during the 20th century. The average reelection rate across all state legislatures exhibits a distinct upward slope until the late 1980s, when the line flattens and then declines slightly. Reelection rates stabilize in the final (post-1985) decades we study at around 75 percent of state legislators. The mid-century period when civil service reforms are concentrated sees a consistent and very substantial improvement in politicians' reelection rates. This fits our argument, although it does not provide causal evidence for why the steep increase in reelection rates took place. The magnitude of the increase in reelection rates over the 20th century is substantively very large. They increase from just over 29 percent at the start of the 20th century to 74 percent by 1980.

every four years instead of the standard biannual pattern. We permanently exclude them from analysis rather than impute data on the dependent variable. We also exclude Nebraska because it is unicameral and in addition half of the Senate's seats are up for election every four years. Our dataset thus covers 45 out of 50 states after 1959, when Alaska and Hawaii joined the Union, and 43 out of 48 before 1959. Although states use a variety of electoral systems (see the discussion in Carey, Niemi, and Powell 2000, p. 675n5), we do not take these into account.

[5] Most states hold elections every two years, generally in even-ended years. The few that hold elections in odd-ending years are treated as if their elections were held the following year in order to standardize election dates; the same is done by Carsey et al. (2008). A few states hold elections annually for some years (New Jersey until 1947, New York until 1940, and Massachusetts until 1921), and for these, we create biannual averages out of annual observations. We use the biannual averages as if they were single observations.

In Figure 2, we show the relationship between reform and reelection rates without considering the timing of reform adoption. The figure plots the difference between reelection rates before and after reform, after pooling the data from all the states and all elections. There is a distinct and very large discontinuity between reelection rates for elections held before and after reform. We find a roughly ten point difference in reelection rates at the discontinuity. This provides prima facie evidence of the importance of reform on reelection rates. The figure also shows that rates increase both pre- and post-treatment. In the next section, we explain our strategy for identifying whether the discontinuity is causally related to reform.

Figure 2: Pre- and post-treatment reelection rates



Identification strategies

Identification issues

Since states adopt reform legislation at different times, treatment roll-out is staggered, making estimation more complex than the classic two-period and two-group setup used in two-way fixed effects (TWFE) estimations. We utilize Goodman-Bacon's approach to diagnose the extent of the bias arising from the use of TWFE (Goodman-Bacon 2021) and report results in Figure G-4 (see the discussion in Appendix G). They show that TWFE estimation produces considerable bias; nonetheless, the estimator provides a positive, statistically significant effect of civil service reform on reelection rates, one that increases in magnitude when we control for term limits. In Table H-2, we also present the TWFE estimation results, which account for state-specific trends, and show that the positive effect of reform remains robust.

To sidestep the identification issues raised by TWFE, we turn to two alternative estimation strategies that are more appropriate to the phenomenon we study: imputation methods (Gardner 2022; Borusyak, Jaravel, and Spiess 2024) and PanelMatch (Imai, Kim, and Wang 2023). We class both as newly-developed difference-in-differences (DiD) techniques, developed to handle the kind of staggered treatment timing that is encountered in our dataset. These methods compare treated units with contemporaneous control units to derive valid causal estimates.

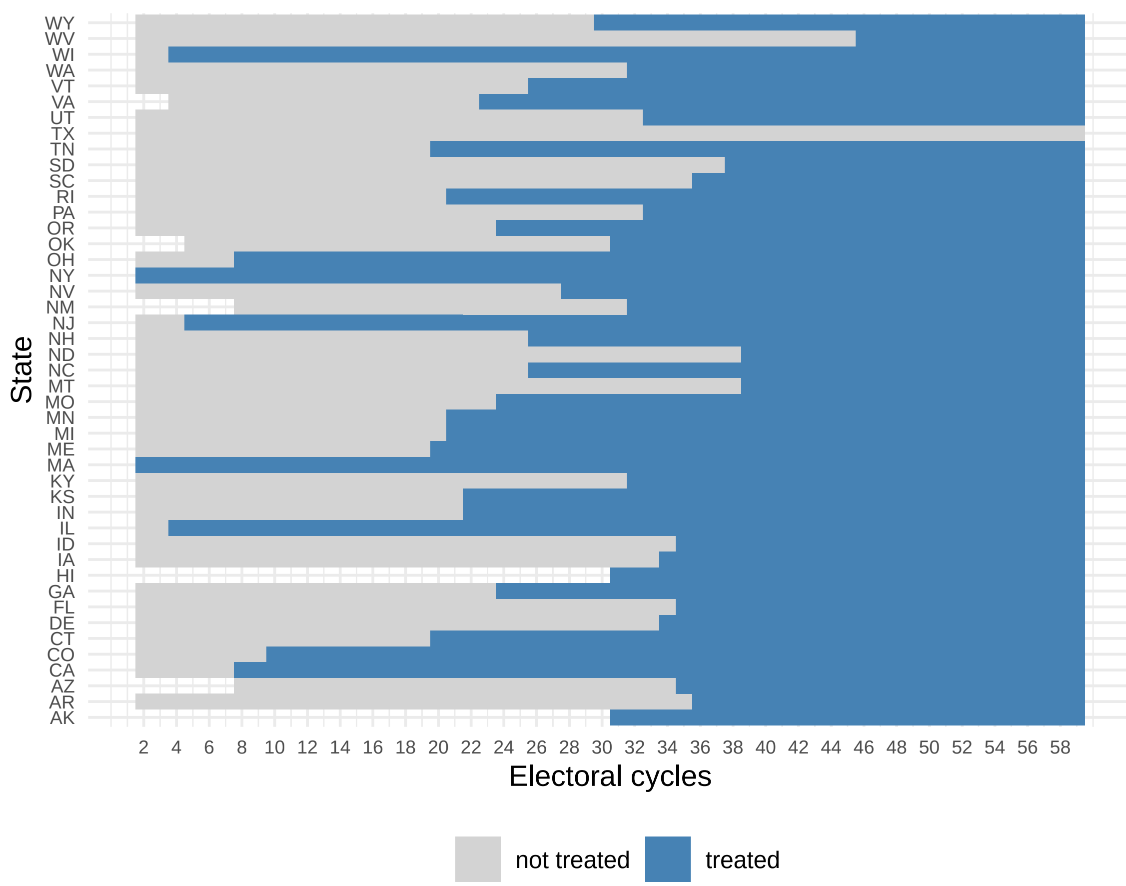
Staggered difference-in-differences

Figure 3 displays the information that allows us to see how states that adopt treatment would ideally be compared to other units that are yet to adopt civil service reform legislation.[6] Interpreting the figure is straightforward: each row corresponds to a US state and each column references an election. Because states hold elections on a staggered schedule — most in even years but some in odd years — the state-year panel is relatively sparse. To address this, we aggregate elections into

[6]Since our dataset starts in 1900, New York and Massachusetts — which passed civil service reform legislation in 1883 and 1884, respectively — are coded as always-treated states.

electoral cycles. Every cell in Figure 3 indicates a distinct state-cycle. States that have passed civil service reform legislation are indicated by blue whereas untreated states are depicted in gray. States that joined the union after 1900 are evident by their missing electoral cycles in the start of the century. Texas is visible as blue (untreated) throughout the panel. Adoption is gradual over time across states, corroborating the standard view of processes endogenous to each state's political machinations rather than spillovers. Figure 3 also highlights that there are potential empirical issues with multiple comparisons and a diminishing control group over the panel, the latter illustrated by the dominance of treated (blue) units by the second half.

Figure 3: Treatment status across states and electoral cycles



We denote y_{it} the reelection rate to the lower house in state i in electoral cycle t, D a state fixed effect, and T a year fixed effect, and we run the following regression:

$$Y_{st} = \beta_0 + \sum_{h=-5;h\neq-1}^{5} \beta_h \mathbb{1}\left[K_{st} = h\right] + D_s + T_t + \varepsilon_{st}, \tag{1}$$

where $\mathbb{1}\left[k_{st} = h\right]$ is an indicator variable that equals 1 if a civil service reform has been implemented h electoral cycles ago (e.g., $\mathbb{1}\left[\textit{Civil Service Reform}_{st} = 3\right]$ takes value 1 if it is the *third* electoral cycle post-reform of the civil service). Following Borusyak, Jaravel, and Spiess (2024), we exclude the first lead $\mathbb{1}\left[k_{st} = -1\right]$ from estimation ($h \neq -1$) as normalization. K_{st} is the "relative time" variable, i.e. the number of electoral cycles since civil service reform was implemented.

To avoid fully saturating the model and have some indicator variables estimated on very few observations, we impose $H = 5$ and group all reforms that are more than five electoral cycles old. Thus, $\mathbb{1}\left[\textit{Civil Service Reform}_{st} = 5\right]$ takes the value 1 if it is at least the fifth electoral cycle occurring after civil service reform.

From this generalized setup we estimate the coefficients of interest in equation A.3 through imputation and using matched sets. Both approaches resolve the problems found in TWFE.

Estimation results

To ensure credible estimation of treatment effects given the staggered adoption of civil service reform, we employ two imputation-based difference-in-differences estimators (Gardner 2022; Borusyak, Jaravel, and Spiess 2024) and PanelMatch (Imai, Kim, and Wang 2023; Rauh, Kim, and Imai 2025). The imputation estimators efficiently leverage the full pre-treatment outcome history; however, these methods rely on a strict parallel trends assumption (Roth et al. 2023). We therefore complement this method with PanelMatch, which replaces the parallel trends assumption with sequential ignorability by matching treated states to contemporaneous controls based on their treatment and outcome histories (Roth et al. 2023; Y. Xu 2023). This approach conditions on pre-treatment trends in reelection rates and mitigates concerns that the treatment may be confounded by differential pre-trends (Imai, Kim, and Wang 2023).

Imputation

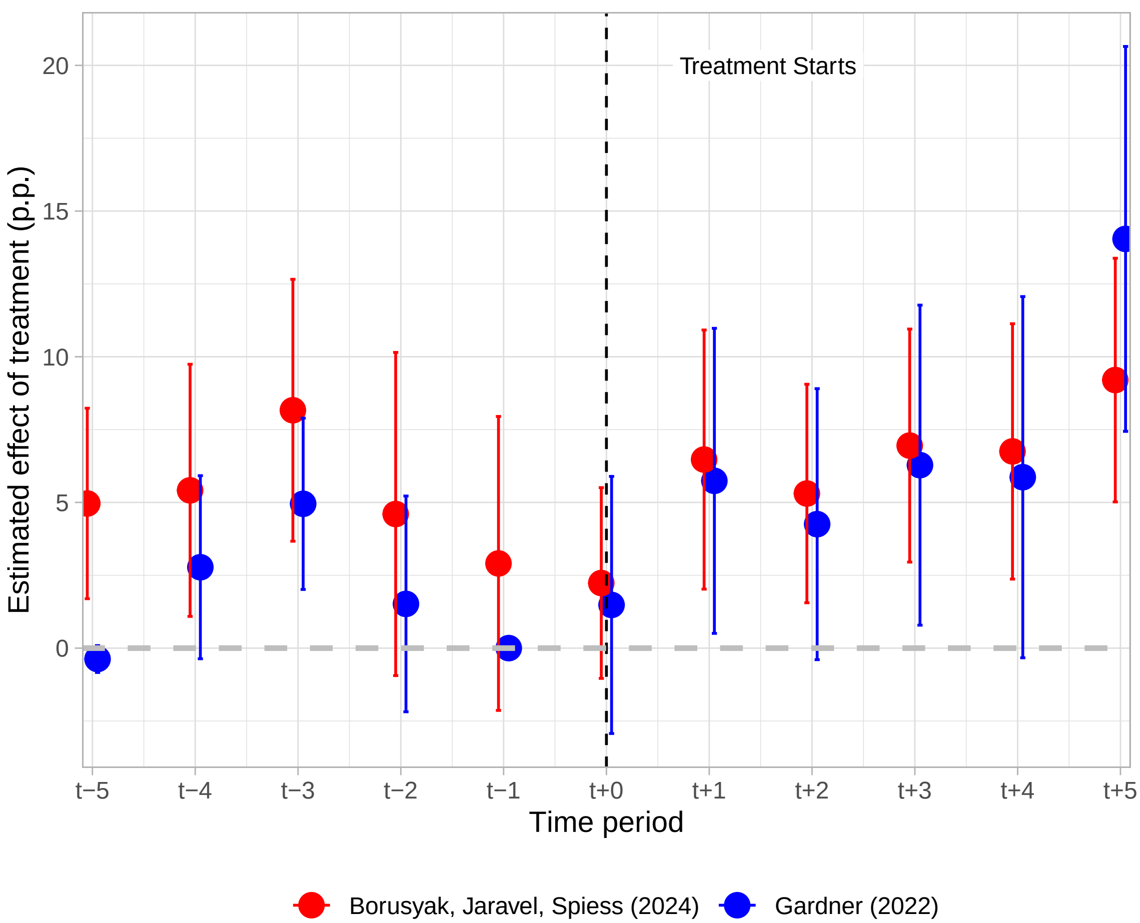
One broad distinction in the new difference-in-differences literature lies in the definition of the control group. Early work from Callaway and Sant'Anna (2021) uses the last pre-treatment period for comparison whereas imputation estimators use the full range of pre-treatment observations. This produces a trade-off; we gain more efficiency from imputation estimators because they use a larger set of control observations but using them requires stronger parallel trends assumptions. Given that our state-level sample has relatively few units, we opt for the imputation approach suggested by Gardner (2022), relying mostly on not-yet-treated observations as the control group (again, only one state, Texas, falls into the never-treated category). We also present results from the alternative imputation estimator due to Borusyak, Jaravel, and Spiess (2024), which yields similar point estimates for the treatment coefficients but has a different estimation approach for the pre-treatment placebo coefficients as well as confidence intervals (Gardner et al. 2024). We discuss the implied stronger parallel trends assumption in Appendix I, where we use randomization inference and Monte-Carlo methods to explore the assumption. Our main effort, however, is to mitigate the strong parallel trends assumption by implementing a matching strategy (Imai, Kim, and Wang 2023; Rauh, Kim, and Imai 2025), the results of which are discussed in the PanelMatch section below.

We present results of both Gardner (2022) and Borusyak, Jaravel, and Spiess (2024) imputations in Figure 4. We see a perceptible increase in reelection rates after reforms are passed. Beginning in the second post-reform electoral cycle, we find an estimated increase of approximately five percentage points, which then stabilizes with a point estimate of around six percentage points across the following four cycles. Throughout the period, we see a statistically significant effect. In the pre-treatment period, we observe statistically significant estimates comparable to the treatment effects, potentially indicating a violation of the parallel-trends assumption (Roth 2024). To further assess this, we conduct a randomization inference test, reported in Appendix I, where we randomize the timing of reform to generate placebo treatments. Results suggest that the Gardner (2022) average treatment effect on the treated (ATT) estimate falls above the 95th percentile, which mitigates concerns about lack of parallel trends, although the same is not the case for the Borusyak, Jaravel, and

Spiess (2024) ATT estimate.

To address concerns about pre-trends, we turn to the matching strategy proposed by Imai, Kim, and Wang (2023). The parallel trends assumption in Imai, Kim, and Wang (2023) holds conditionally on "the treatment, outcome, and covariate histories" (sequential ignorability), although it does not entirely rule out the possibility of unobserved confounders. We present placebo treatment effects with 95% confidence intervals from pre-reform periods. Unlike the imputation-based results, the PanelMatch estimates show no apparent violations of the sequential ignorability assumption — the placebo coefficients are statistically indistinguishable from zero — conditional on the matching strategy. From this, we conclude that there is sufficient evidence supporting conditional parallel trends to proceed with the analysis.

Figure 4: Treatment effects over time using imputation



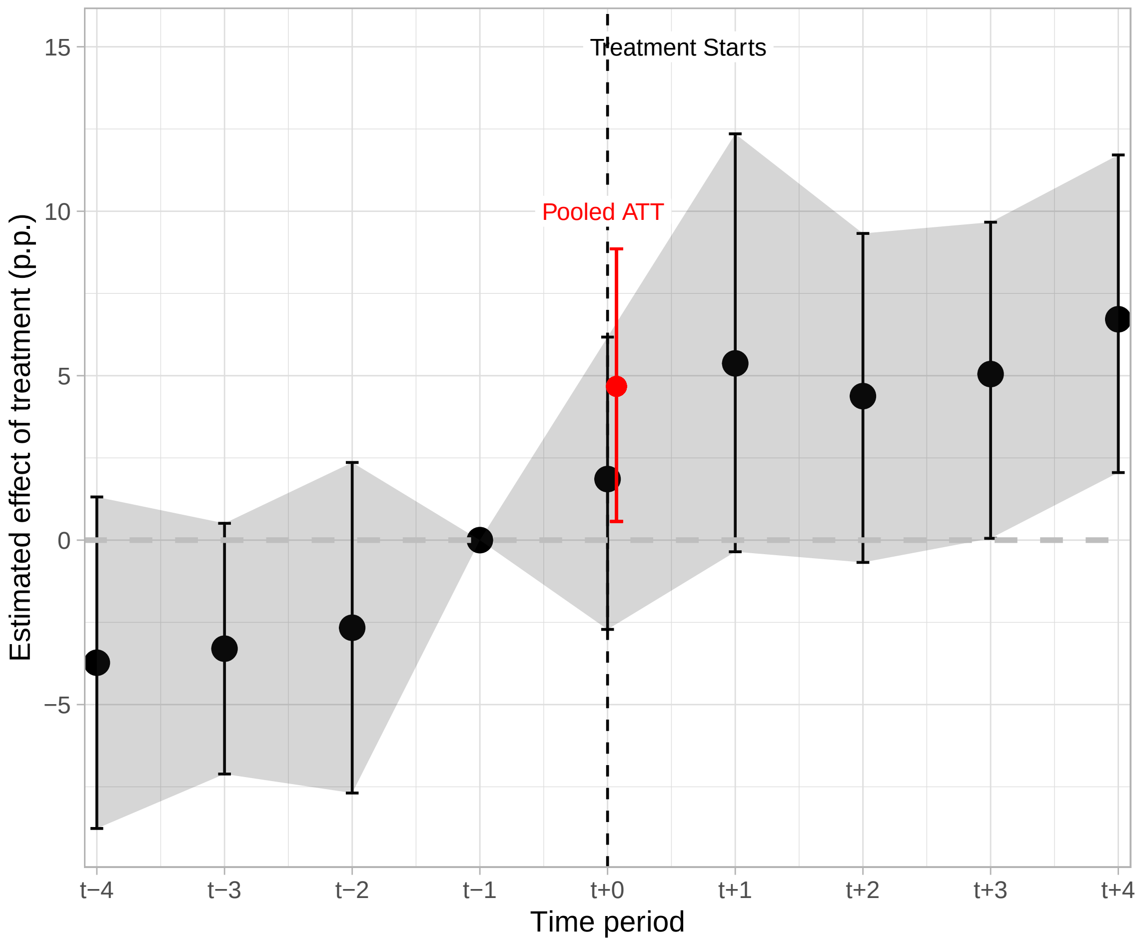
PanelMatch

PanelMatch (Imai, Kim, and Wang 2023; Rauh, Kim, and Imai 2025) makes comparisons between units with the same treatment history; that is, it compares a state that implements reform to other states that are unreformed. Comparisons are made only between contemporary states. For instance, a state which reforms in 1955 is compared to a set of unreformed states in 1955. This guards against generic time-trends contributing to results. We can additionally refine comparison states by matching them on pretreatment outcomes — their reelection rates before reform. This reduces pre-trend differences in reelection rates. Throughout, we implement PanelMatch using lags of two periods with homogeneous treatment status to generate matched sets, following the suggestion of Imai, Kim, and Wang (2023) that in analyses with fewer units, a shorter lag time is appropriate. We specify that the matched set of control units include five other states, such that estimates derive from the sum of comparisons between one treated and five control states. Intuitively, this process can be considered similar to a series of synthetic control estimations; for each treated unit, a sample of five not-yet-treated units are used as counterfactuals. The overall estimate of a treatment effect is the average across the set of one-to-five comparisons.

PanelMatch also offers a way to improve the quality of our matches and obtain a more representative control group. It first uses an algorithm to match treated and control states on pre-treatment outcomes. Then it compares matched groups of treated and control observations to estimate the average treatment effect on the treated. Above, we reported results based on comparisons of treatment status alone, where coefficients estimate the overall difference between reformed and unreformed states. However, the latter states may have been trending towards reform if underlying trends in national politics affect unreformed states. As we saw using other approaches, there are some pre-treatment differences between treated and control units in some cycles (see Figure 4) that could suggest this. Therefore, we turn to PanelMatch to address pre-treatment differences.

In Figure 5, we show the estimated positive effect of meritocratic civil service reform on reelection rates using Malhanobis matching. Pooled ATT results show an increase in reelection rates in reformed states of approximately 4.7 percentage points. The estimation strategy results in a similar pattern as the unrefined estimation, albeit slightly steadier in the rate of increase and final effect size (Figure J-7). The effect appears cumulative and grows during electoral cycles after reform.

Figure 5: Mahalanobis PanelMatch estimates



As the results reported in Figure 5 document, reelection rates in reformed states steadily improve compared to their unreformed counterparts in the post-reform electoral cycles. In the first electoral cycle after reform legislation is adopted, there is a null effect. In the subsequent period (time $t+1$), reelection rates are noticeably higher, and the same is true for the second and third periods after treatment, with point estimates of around five percentage points. By the fifth cycle after reform, reelection rates are significantly higher in the treated group compared to counterfactual states that did not adopt civil service reform. We extend the duration of our analysis (up to 10 electoral cycles after reform) and show results in Figure J-12, which sees a generally consistent magnitude of treatment effects.

Possible explanations and mechanisms

We have documented a plausibly causal relationship between the adoption of civil service reform legislation and a substantial improvement in reelection rates. What could explain this relationship?

The theoretical literature that seeks to explain reelection rates points to two broad mechanisms: selection and performance. The selection mechanism suggests that reelection rates increase as individuals entering the pool of potential politicians become more ambitious; adopting the goal of retaining elected office encourages politicians to modify their institutional environment and modes of interaction with voters in ways that improve the probability of reelection. In the case of the United States, many features of the internal organization of Congress are interpreted as specifically useful for reelection (Polsby 1968), and it is standard to contend that national level politicians (and many state level politicians as well) are motivated by political ambition (Schlesinger 1966; Mayhew 1974).

The performance mechanism by contrast suggests that reelection rates increase as politicians deliver more programmatic goods and services to broader groups of voters. This view is rooted in the theory of retrospective voting (Ferejohn 1986), which holds that voters decide whether to reelect an incumbent on the basis of performance in office. For holders of executive office, this has usually been interpreted as macroeconomic policy performance (Tufte 1978). The performance standards used by voters for holders of legislative office, including at the subnational level, have been less well studied but the main intuition seems to be that they will be principally evaluated for their spending decisions and accompanying output (Treul et al. 2022).

Selection effects

We investigate political selection using three methods of inquiry. First, we investigate whether reform ushered in a new class of legislators, comprising individuals who were likely to be more professional and politically ambitious than their pre-reform counterparts. Second, we study whether patterns of rerunning are consistent with a change in the nature of individuals seeking legislative seats in US statehouses after the passage of civil service reform. Third, we assess whether reform appears correlated with changes in the occupational backgrounds of those serving as state legislators.

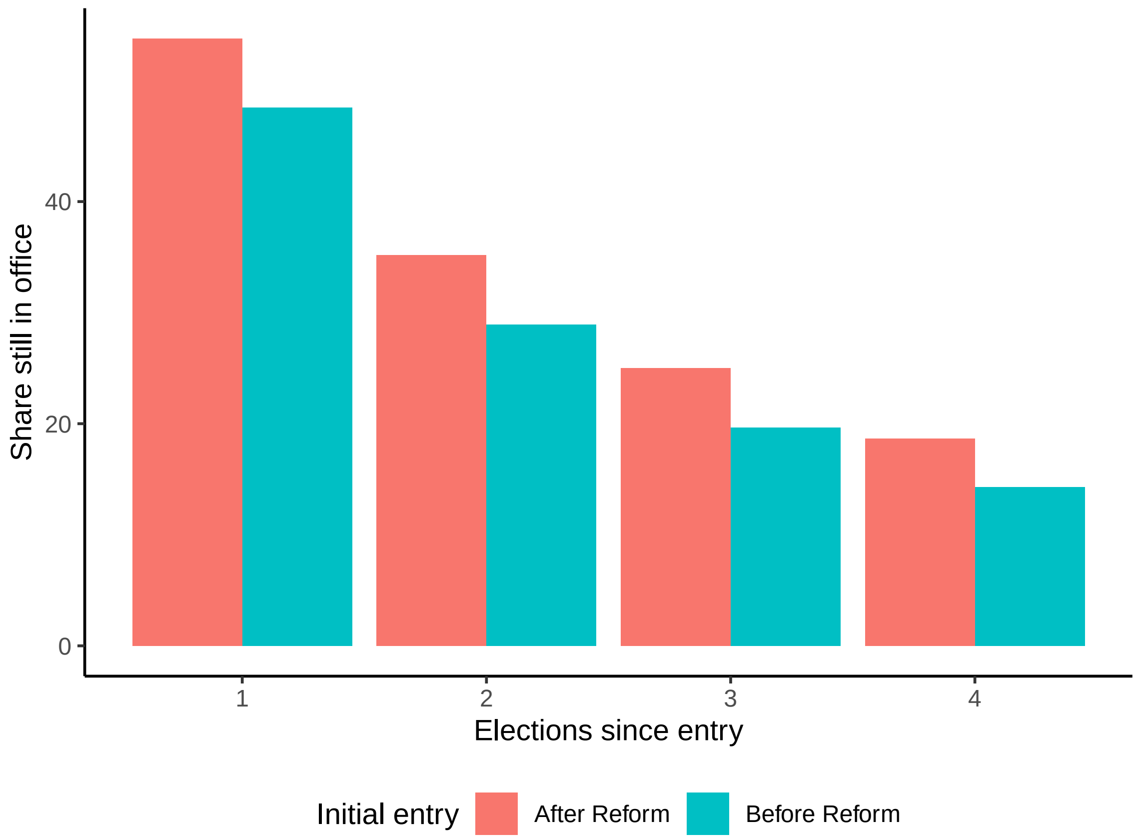
Changes in the pool of politicians

Our data allow us to examine whether changes in reelection rates stem from changes of who selects into the pool of politicians. Hypothetically, larger post-reform reelection rates could be explained by the adaptation of the legislative old-guard to civil service reform or instead by its replacement with a new class of presumably more ambitious and more professional politicians. Selection theory hypothesizes that pre-reform legislators be replaced by a new class of state-level legislative specialists.

We study the degree to which our main finding showing the impact of civil service reform on reelection rates is driven by compositional changes. We map the rate of decay in retention of politicians across electoral cycles. We compare politicians elected in the three elections before a reform (the pre-reform generation) to those elected in the three cycles after reform (the post-reform generation). If a new type of more ambitious post-reform politician enters, we anticipate that retention rates increase specifically among post-reform entrants. On the other hand, if extant politicians are adapting to changing circumstances, we expect little difference in retention rates between pre- and post-reform politicians.

Figure 6 plots reelection rates of politicians in the four legislative sessions after entry for different politician-cohorts. We find a consistent increase in the longevity of careers for post-reform entrants compared to pre-reform entrants, and longer legislative careers stretching across many elections become more common after adoption of civil service reform. We see an approximately six percentage point difference in reelection to a second term between cohorts who enter pre-reform and those entering post-reform and a continued difference between the two groups in the rates at which they achieve a third, fourth, and fifth term. Legislators who first enter after civil service reform has been adopted are more likely to have long careers and to serve as many as five cycles (ten years), for instance. Legislators who enter under a patronage regime have shorter careers overall and are less likely to serve long durations in elected office.

Figure 6: Longevity in office of pre- and post-reform politicians



Notes: The bars represent shares of legislators first elected before civil service reform (blue) and those first elected after reform was adopted (red). The sets show shares reelected for a first, second, third, and fourth term.

Overall, we interpret this descriptive evidence as consistent with the view that reform alters the pool of politicians: a new-guard emerges which experiences higher reelection rates. We do not find evidence in favor of changing behavior among the old-guard that allows them to improve career longevity. Our estimates are that 12 percent of politicians who enter just before reform serve 10 years whereas 17 percent of politicians who enter just after reform serve for that long. This is a large increase. It underscores the interpretation that post-reform reelection rates are functions of newly-ambitious legislators.

Rerunning of incumbents

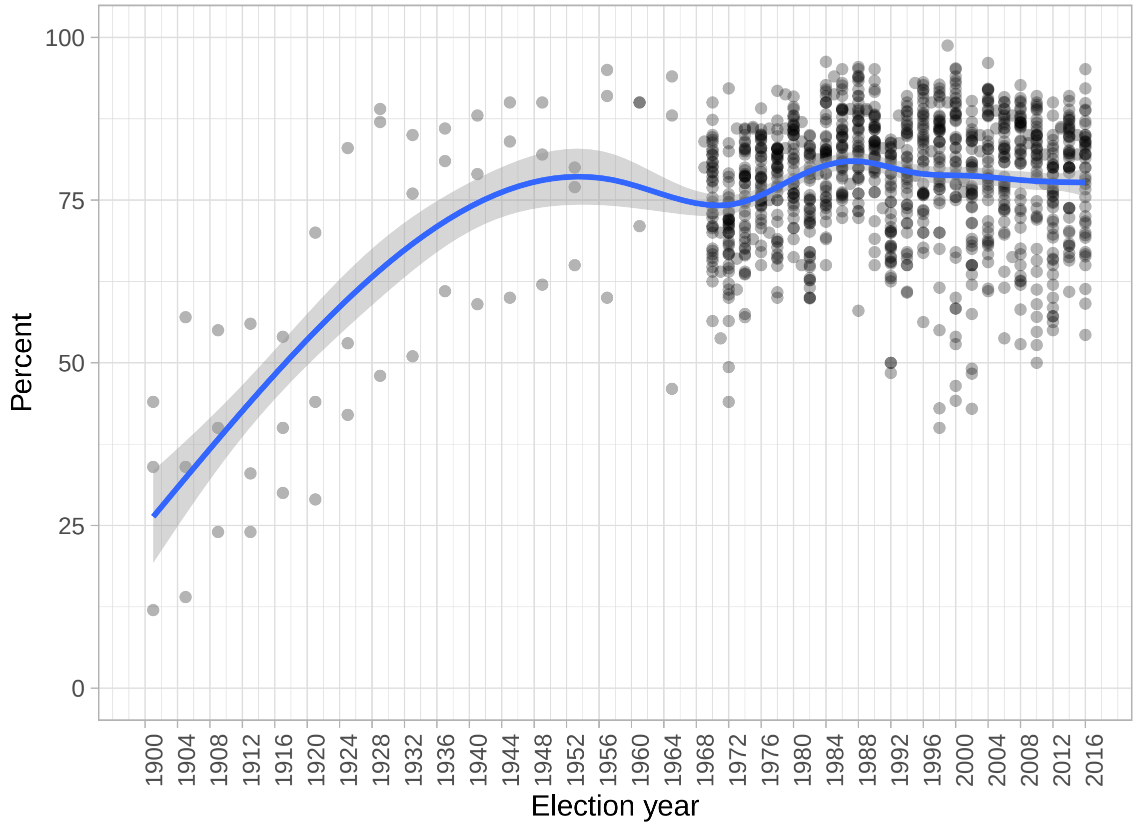
If a more professionally-oriented group of legislators emerges post-reform, we should see rerunning rates also increase: more professional legislators should more often seek reelection. We cannot test statistically whether reform affects rerunning using methods similar to those already used to study reelection due to data limitations. We have data on rerunning from only a limited number of states. However, we can examine the data we have and see whether it appears consistent with this hypothesis.

We have two sets of data on whether incumbents rerun, both incomplete but in different ways. The first combines data from two existing datasets. In Figure 7, we plot the proportion of incumbents at $t-1$ who run again at time t using candidate-level data from Klarner 2018 for all states from 1968 to 2016 and from the state-level aggregate rerunning rates reported in Ray (1974) for Connecticut, Michigan, and Wisconsin for 1900 through 1969. The latter is the only dataset we have been able to locate that systematically compiles rerunning rates for any state legislatures in the first half of the 20th century.

The data depicted in Figure 7 show that more than 75 percent of state legislators run again by 1948, and average rerunning rates remain consistently high until the dataset ends in 2016. The data for the available three states from the early period shows that rerunning rates were much lower in the first half of the 20th century. The broad although incomplete picture thus confirms the emergence of an increasingly professionalized and ambitious state legislative class over the course of the 20th century.

For reelection rates to increase, rerunning rates necessarily increase as well; thus, we are interested in whether civil service reform appears to trigger a sudden, abrupt change in rerunning rates. This would be consistent with the post-reform entry of a more professionally ambitious political class. To assess this, we manually collect rerunning data from state archives covering the first half of the 20th century for as many states as possible. Locating accessible archival data is difficult, and we have been successful for only nine states for the entire period. We display rerunning rates for 1900 through as late as 1984 for these nine states in Figure 8. (Only one overlaps with the pre-1968

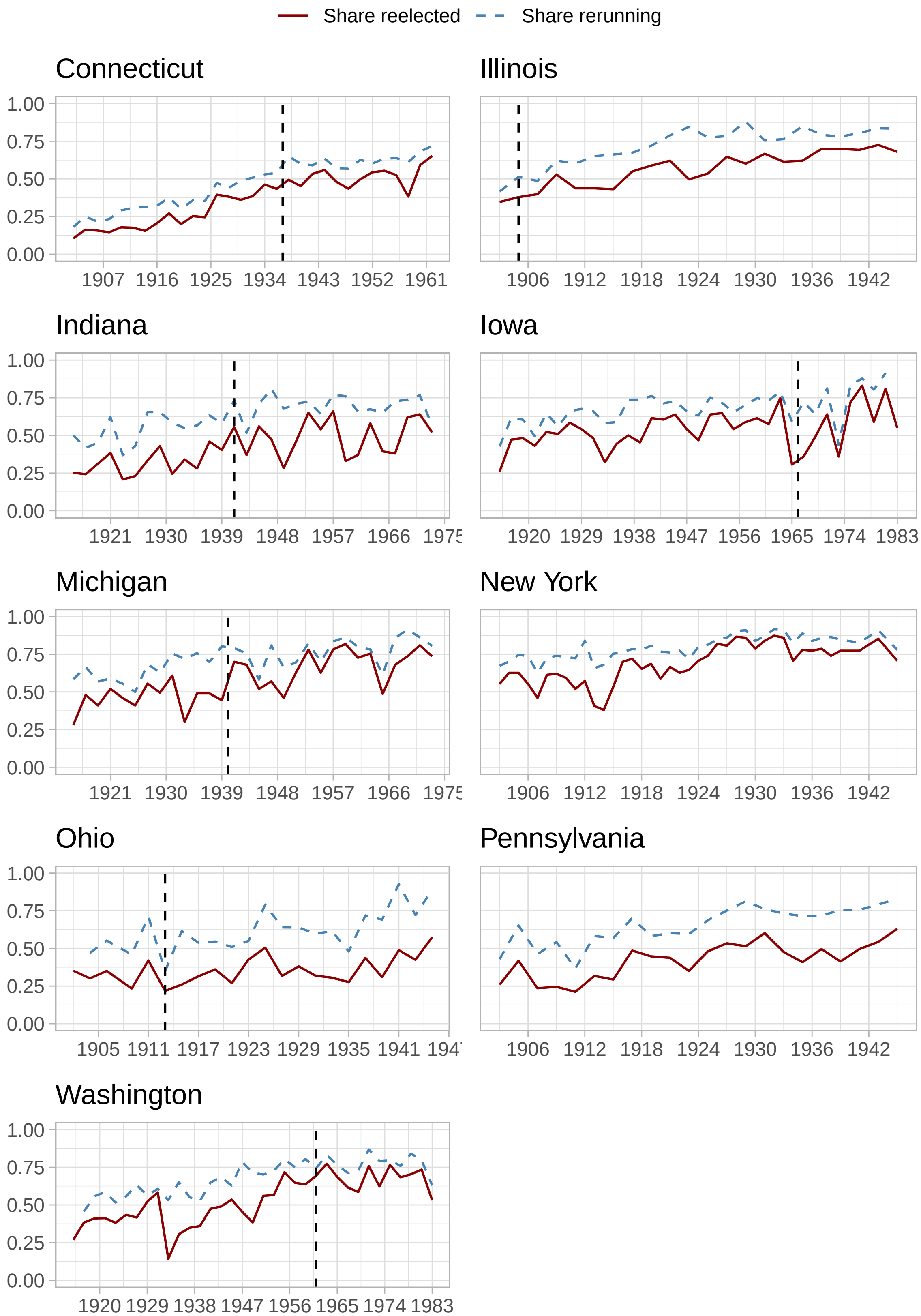
Figure 7: Proportions of incumbents running again in the next election, incomplete data, 1900–2016



Notes: Each dot represents a state legislature.
Line with confidence intervals produced by LOESS smoothing.
Data from 1968 to 2016 for all states from Klarner 2018.
Pre−1968 data for Connecticut, Michigan, and Wisconsin from Ray 1974.

data collected by Ray 1974 that is depicted in Figure 7.) In every state for which we have data, we find gradual increases in both rerunning and reelection over time. For the seven states that adopt civil service reform in years that coincide with available rerunning and reelection data, we do not observe reform associated with a visible shock to rerunning rates. To corroborate this, in Appendix K, we present results of a formal test of the statistical differences in pre- and post-reform periods between rerunning and reelection rates and document null effects of treatment.

Figure 8: Proportions of incumbents rerunning and reelected in nine states, various years, 20th century



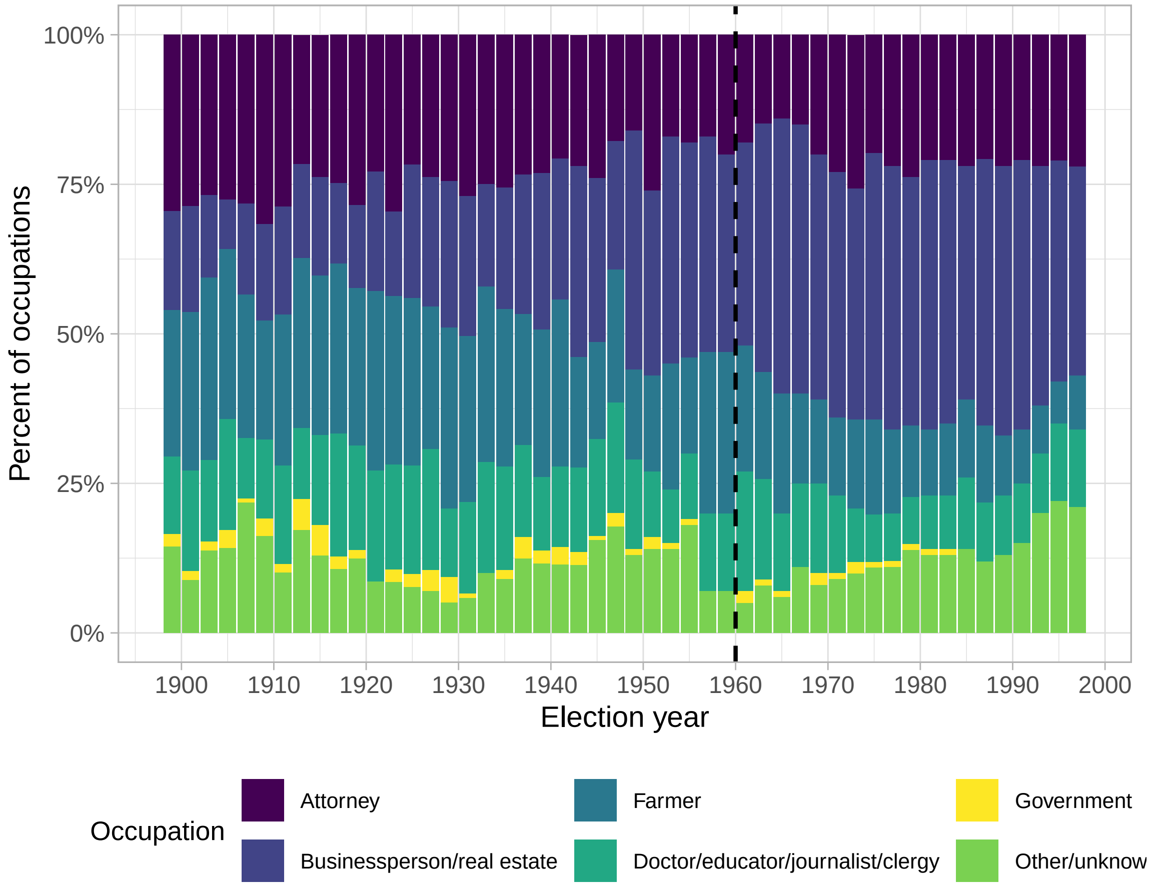
Note: Black dashed vertical line represents year civil service reform adopted. Reform adopted in 1883 in New York and in 1963 in Pennsylvania.

Prior to reform, high rates of turnover arose principally from decisions by incumbents not to run again. The 1938 study that initiated quantitative investigations into state legislative turnover reports that between 1925 and 1935, more than 60 percent of legislators retired rather than run again whereas only 30 percent were defeated at the polls or in a primary contest. As the author remarks, "people who yield their togas after only one or a very few terms usually do so of their own volition" (Hyneman 1938, p. 28). This finding is corroborated by Ray 1974, who reports (on the basis of data from 1893 to 1969 from three states) that 21 percent of incumbents sought reelection in 1893, 71 percent in 1929, and 78 percent in 1957. The increase in state-level reelection rates appears to parallel, with a lag, the transformation of congressional careers that took place nationally in the late 19th and early 20th centuries as representatives gradually exhibited higher rates of rerunning (Kernell 1977). Our data show similar trends but, in the small number of states on which we have relevant information, we do not observe that civil service reform triggers an abrupt increase in rerunning rates.

Occupational backgrounds of legislators

One final way to gain additional insight into the possibly changing characteristics of those who enter office is to examine the occupational backgrounds of legislators. Did the occupational backgrounds of legislators change noticeably after the adoption of reform? A selection theory might suggest that we would see more attorneys — the background *par excellance* of the professional politician and the dominant occupation of national congressional representatives (Bonica 2020) — and fewer government officials, businesspeople, and individuals from other occupations enter state legislatures after civil service reform. Locating complete occupational data for state legislators between 1900 and 2000 is difficult, and we have been successful only for the state of Kentucky. We display occupational backgrounds, grouped into six large buckets to simplify interpretation, in Figure 9.

Figure 9: Proportion of incumbents by occupation in Kentucky, 1900–1998



Notes: Black dashed vertical line indicates year civil service reform adopted.

The data depicted in Figure 9 is not consistent with reform-induced changes in the occupational composition of the state legislature, except possibly insofar as reform pushed out farmers and swapped in businesspeople in their place. However, this may also reflect the process of industrialization and an overall decline in the number of farmers in the state. The proportion of attorneys in Kentucky's state legislature remains almost stable over the the 20th century, if anything, shrinking somewhat. There is no evidence that attorneys, despite their legal training, dominate the process of state lawmaking after reform, although we note that the proportion of lawyers increases in the fifth post-reform election and then remains consistent. The share of former government employees is always very small and virtually disappears once civil service reform is adopted, highlighting the fact that bureaucratic positions became professional rather than patronage-based.

The occupational data from Kentucky is non-dispositive. Although it does not show significant occupational changes among state legislators following civil service reform, we do not know how representative data is from a single state. We can only say that in Kentucky, observable occupational characteristics of legislators did not change even with the lengthening of political careers except that farmers were replaced by businesspeople — which likely would have occurred regardless. Thus, the occupational backgrounds of more professional, career-oriented post-reform legislators does not obviously differ from their pre-reform counterparts.

Performance effects

Thus far, we have found that increased post-reform reelection rates are also associated with longer political careers for newly-entering state law-makers. This is consistent with the literature on state legislatures that reports that they have gradually become more professionalized over time. We now turn to the question of whether post-reform legislators seem to have delivered more or better public services to inhabitants. We can investigate this question using a proxy measure for service delivery. Lack of data over most of the period prevents us to from studying the correspondence between career longevity and better representation of public opinion, as has been done at a single point in time in Maestas (2000).

To investigate performance effects, we assemble and analyze data on state-level expenditure disbursements. In the absence of more exact measures, such as the number or types of bills passed, legislative votes, or sector-specific expenditure data, total state expenditures (including federal transfers) broadly proxy the delivery of public goods by states. It is well known that until the New Deal, states had few fiscal resources (Fitch 1953; Wallis 2018). Local governments controlled most US fiscal resources until the 1930s; after the 1930s, and especially after World War II, the federal government did so. Control over limited fiscal resources very likely hampered the performance of state legislators in efforts to gain reelection. Nonetheless, as two well-informed scholars have noted: "Between the Progressive Era and World War II, state legislatures pursued an array of initiatives, developing highway systems, adopting income taxes and sales taxes as major new sources of revenue, introducing regulatory frameworks, and establishing statewide programs of public assistance and aid to education" (Gamm and Kousser 2010, p. 152). Others have stressed the extent to which state government specialize in the delivery of geographically-specific goods and services, in contrast to the casework burden assumed by members of the national Congress (Bagashka and Clark 2016) — goods and services that require funding. We study whether civil service reform boosted state spending, which would have permitted legislators more credit-claiming opportunities with which to gain reelection. Expenditures are also easily observable to voters, especially if they take the form of highways and improvements in education and public health.

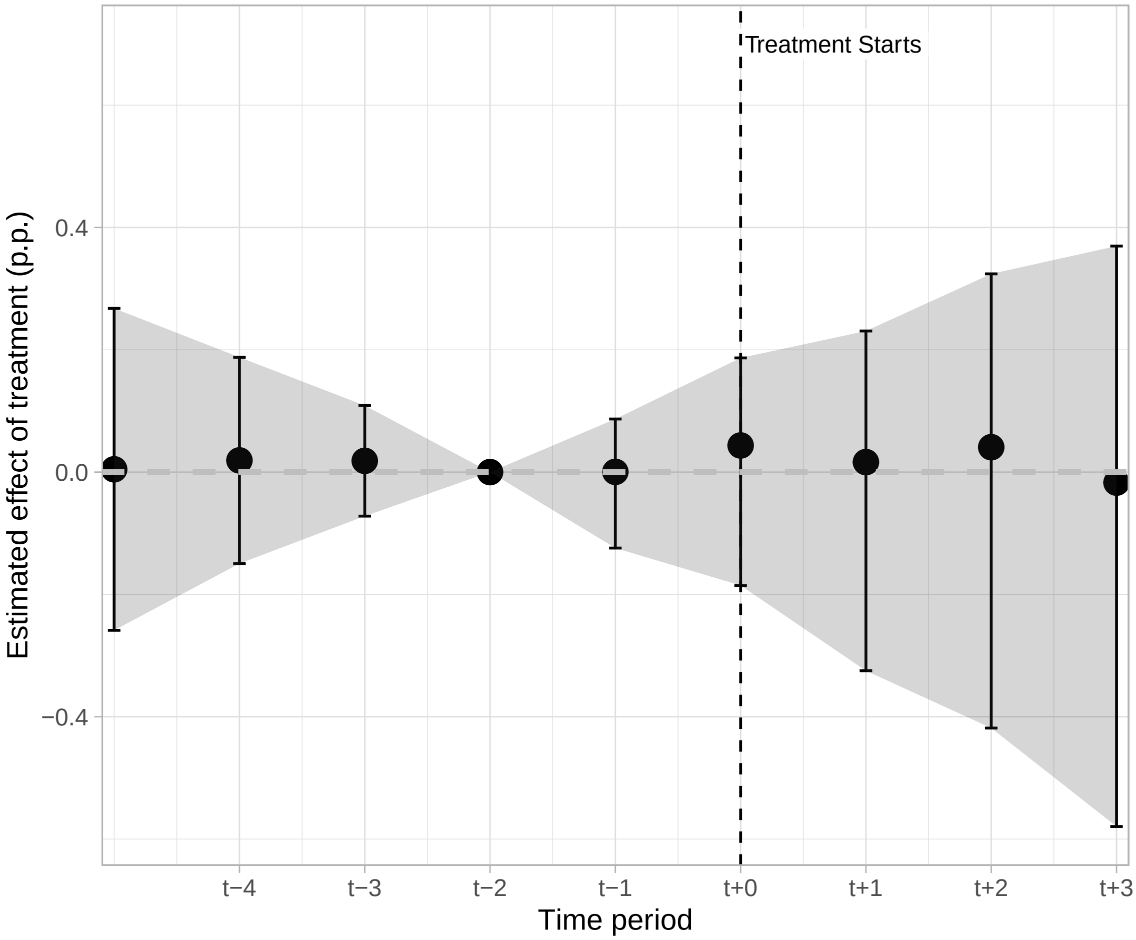
For this analysis, we collect and harmonize annual data on per capita expenditures of states across different years in the 20th century. Details are presented in Appendix L.

Figure 10 displays the results of PanelMatch estimates studying whether civil service reform increases state-level per capita spending. The effect of reform on the dependent variable of log per capita expenditure is computed for four subsequent electoral cycles. We find precisely estimated null results.[7] In the elections following a reform, the new legislators in state governments do not significantly change the volume of spending compared to state legislators in unreformed systems.[8] We find no observable difference in performance or activity; post-reform politicians behave similarly on this dimension to their pre-reform counterparts.

[7]Our results are not inconsistent with those for 1942 to 1963 reported in Ujhelyi (2014b), which finds increases in post-reform intergovernmental transfers from states to lower levels of government. The state-level expenditure data we study includes transfers to lower levels.

[8]In Appendix L, we show that expenditures and reelection rates rose in tandem over the 20th century. We also document that spending positively influences subsequent reelection rates. Thus, visible state-level legislative effort improves longevity in office — as we might expect — but civil service reform is not a directly-relevant mechanism.

Figure 10: Effects of civil service reform on per capita state expenditures



Interpretations and conclusions

Our results show that civil service reform during the 20th century across the US states contributed to the creation of a more professional class of legislators, one that more often achieved reelection and remained in office for more legislative terms. From the limited evidence we have available, this group does not appear distinctive in its pre-election occupational characteristics. Instead, based on the secondary literature, it seems more likely that this group became more independent of state and local political party organizations, in contrast to the patronage era, when party machines controlled candidate selection (Carson and Roberts 2005). Its members created long-lasting legislative careers instead of rotating through various appointed and party positions. Post-reform legislators gradually ran more often for reelection and then achieved it. Examining per capita state expenditures, we do not find an increase in this measure of legislator performance after reform, suggesting that their efforts at reelection may hinge on less visible and more incremental improvements in performance.

The analyses we report document that institutional changes may produce unexpected side effects over time that probably go beyond those anticipated by their proponents. Proponents of civil service reform were known to wish to professionalize the civil service but not to also lengthen the careers of elected public officials.

How do our findings square with prior literature that argues that reform solidified the hold of entrenched incumbents on government? Folke, Hirano, and Snyder (2011) analyzes data from US states between 1885 and 1995 to assess possible changes in the probability that the political party that controls a majority of legislative seats remains in power under patronage compared to civil service conditions. In that study, the unit of analysis is the political party, whereas this research note uses as the unit of analysis the individual legislator. Folke, Hirano, and Snyder (2011) shows that political parties, especially entrenched parties, are less likely to retain majority control of the state legislature once civil service reform is adopted than had been the case under patronage conditions. Their reasoning is simple: ”Patronage jobs constitute a valuable resource for the party in power” (Folke, Hirano, and Snyder 2011, p. 567).

We do not disagree, and our findings about individual political careers are not incompatible with the argument advanced in Folke, Hirano, and Snyder (2011). Indeed, the findings reported there help shed light on how individuals interested in serving in elected office reoptimize strategically following the passage of reform legislation. We have shown that civil service reform makes it more difficult for pre-reform *legislators* to retain office, which is consistent with the increased likelihood of the *party* in power losing its majority. The increasing partisan competitiveness that civil service reform introduces weakens the ability of party organizations to control candidate selection and election campaigns, pushing the work of creating a personal vote onto the individual. New post-reform politicians enter office, whose subsequent electoral successes demonstrate better capacities at individual vote-getting. Our data corroborates that new post-reform entrants become more successful at reelection their pre-reform counterparts; the theory of the personal vote displacing weaker political parties is consistent with our finding of increased post-reform individual reelection rates. Civil service reform weakens political parties, and individuals interested in political careers respond by building longer careers in the legislature.

The world we uncover in US history parallels what we observe in contemporary less developed countries, where the vast majority of legislators fail to gain reelection (Golden and Nazrullaeva 2023). In these settings, governments mainly use patronage rather than meritocratic criteria to staff the bureaucracy (Dahlberg et al. 2013). Whether the impact of civil service reform in today's world would parallel that of mid-20th century US states is an empirical question; it is possible that other unobserved factors need to be in play for the same effects to transpire. Nonetheless, if our results travel from 20th century US states to less developed countries around the world today, we suspect that creating a more professional and ultimately more responsive political class will require institutional changes that encourage new entrants into the political realm.

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Appendices

A Data on adoption dates of civil service reform in US states

We use data already assembled on the dates of civil service reform adoption for US states. Folke, Hirano, and Snyder (2011) reports data on civil service reform dates but the underlying data is available only in graphical format. Ting et al. (2013) presents what appear to be identical data but in numerical (tabular) format. Subsequently, Ujhelyi (2014) released a dataset that reports different years than Ting et al. (2013) for civil service reform dates for some states. Finally, Ash, Morelli, and Vannoni (2022) reviews and adjudicates coding discrepancies between the two prior sources, identifying the year in which legislation was formally adopted. We use the adoption dates reported by Ash, Morelli, and Vannoni (2022, table A1, col 4, p. 33).

B Data on reelection of individual legislators

Reelection rates by state-election are calculated using data on the reentry of individual legislators into each lower house between 1900 and 2016. A legislator is reelected if she was elected at time t in constituency i, conditional on her election in the same or a different constituency in the same state at time $t-1$. If a legislator skipped a legislative period, we do not consider her reelected.

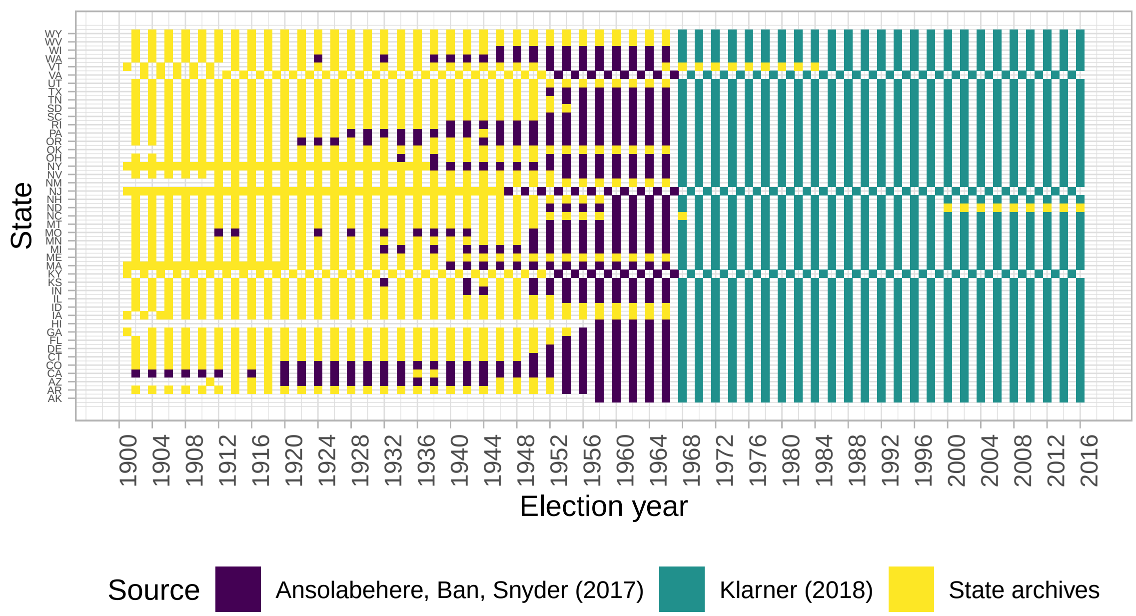
We match individual legislators using their full last name and first name initials. Because we have only the first name initial for 9.9 percent of individuals, we match on first name initials for all legislators. This provides a consistent matching procedure but probably produces some false positives.

Sources of reelection data

We combine various data sources to obtain the names of individual legislators. We begin with a dataset that provides candidate-level state legislative election information for many states between 1890 and 1978 (Ansolabehere, Ban, and Snyder 2017). However, it has limited data prior to 1952 and after 1966. We combine the Ansolabehere, Ban, and Snyder (2017) dataset with one assembled and made publicly available in Klarner (2018); the latter extends Ansolabehere, Ban, and Snyder (2017) by providing candidate-level state legislative returns from 1968 to 2016. In the final dataset used in this paper, data taken from Ansolabehere, Ban, and Snyder 2017 provide 15.6 percent of individual-level observations. Observations from Klarner 2018 constitute 40.2 percent of observations in our final dataset.[9]

After combining data from Ansolabehere, Ban, and Snyder (2017) and Klarner (2018), we are still left with 46 percent missingness for the period 1900 to 2016. To complete the dataset, we collect information directly from state legislative offices and archives on who served in each legislative period.

Figure B-1: Data source for each state-election year



C Data on term limits

We collect information on term limits, affecting 17 states, from the National Conference of State Legislatures (National Conference of State Legislatures 2024).

[9]Carsey et al. (2008) provides an alternate dataset that covers the period from 1967 to 2003 and contains a variable for incumbent status (without providing legislator IDs). We use Klarner (2018) instead of Carsey et al. (2008) because the former has longer data coverage.

D Data on legislators' occupations from Kentucky

We digitize the two volumes of the "Kentucky General Assembly Membership in 1900–2005" (Kentucky Legislature n.d.[a]; Kentucky Legislature n.d.[b]). Occupations are recorded as primary occupation at the time of election. The following occupation codes were assigned (vol. 1, p. 320; vol. 2, p. 405):

1. Attorney;
2. Businessman/Businesswoman;
3. Clergy;
4. Doctor/Druggist;
5. Educator;
6. Farmer;
7. Government;
8. Housewife/Homemaker;
9. Insurance/Real Estate;
10. Journalist/Newspaper Pub.;
11. Other;
12. Railroad;
13. Unknown.

For ease of interpretation, we collapse the 13 categories into six. We retain attorneys, farmers, and government workers as standalone categories. We merge clergy/doctor/educator/journalist into a single category covering professional occupations. We merge businessperson/insurance into a single category. Finally, we combine other/unknown/railroad/housewife into a single category.

E Data on US state expenditures

We measure state expenditures as total state government expenditures, including both operational expenses for running government offices and capital outlays for permanent or semi-permanent investments in infrastructure, such as roads, bridges, and property acquisition. The terminology used to describe these expenditures varies over the years, but it it most often labeled "All Governmental Costs," "All Governmental Costs Payments," "Total Expenditures," or "General Expenditures" (the latter should not be confused with "Expenses of General Departments," which is a narrower term).

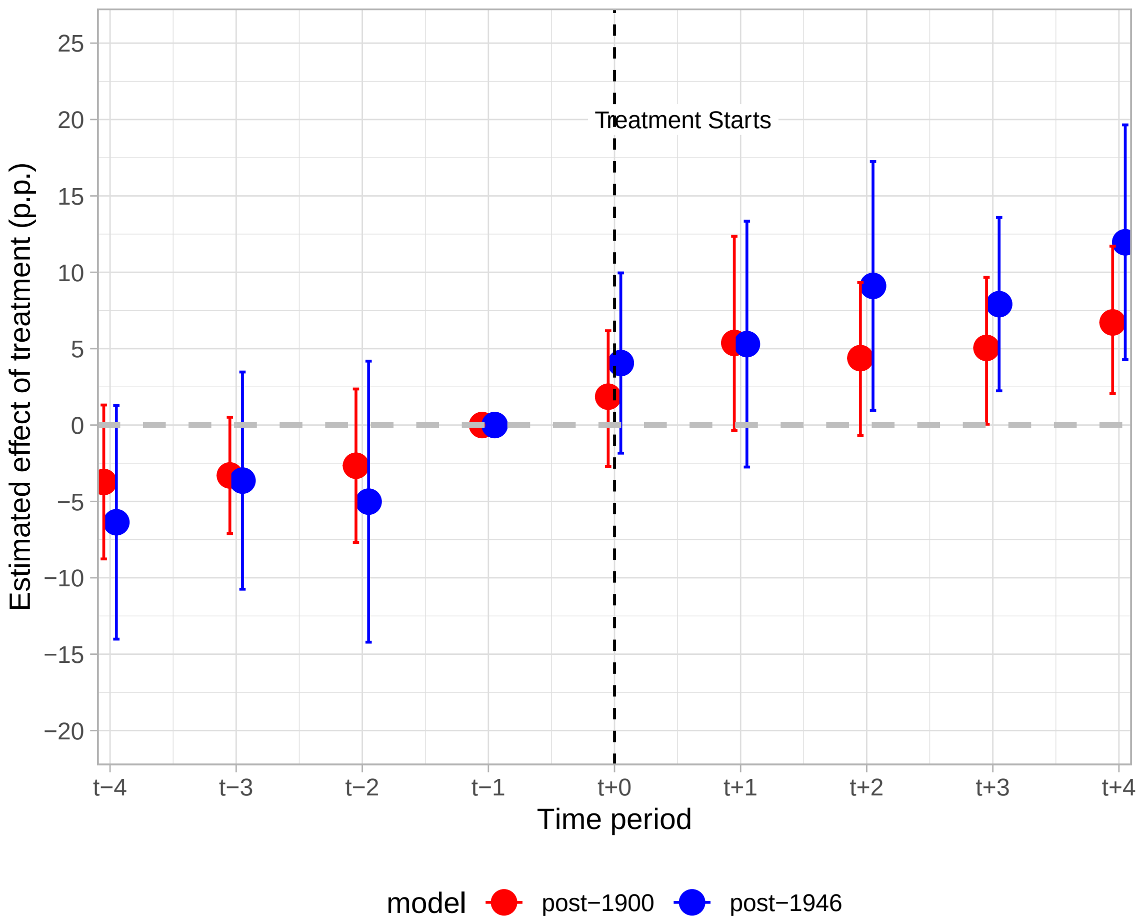
Data was assembled from reports on state government finance published by the U.S. Department of Commerce covering the periods 1915–1931 and 1937–1992 (United States Government n.d.). From these reports, we extracted total governmental expenditures per capita when this was already calculated. In cases where this information was not directly provided, we calculated it ourselves by dividing the total state expenditures by the population size, using the population statistics available in the reports.

F Deviations from the Pre-analysis Plan and Additional Robustness Checks

On the data collection and main results:

- **Deviation 1**: In our pre-analysis plan (PAP), we specified that "our analysis covers the period from 1946 to 2016," based on the justification that "much greater election data availability exists for the entire matrix of reformed and unreformed states in the postwar period." We also noted in the PAP that "over 60 percent of reform legislation was adopted in the years following the end of World War II," which implies that focusing on the post-WWII period would assume roughly 40 percent of states as already treated by the start of 1946. During data collection, we obtained additional funding to digitize pre-1945 House journals, enabling us to expand our sample period back to 1900. We believe that assuming nearly half the sample (out of 50 states) as already treated by 1946 weakens the power of our main analysis to detect the effects of civil service reforms. In addition, by starting in 1946, we miss the implications of the 1939 Congressional amendment of the Social Security Act to encourage merit civil service appointments at the state level. In Figure F-2, we provide a side-by-side comparison of the original 1946–2016 sample with the extended 1900–2016 sample used in this paper. Results are largely consistent across both the full (post-1900) and truncated (post-1946) samples.

Figure F-2: Mahalanobis PanelMatch estimates for the full dataset compared with post-1946 data



- **Deviation 2**: Our PAP proposed that we "employ both a staggered difference-in-differences model and an event study approach," estimating the main equation of interest "using OLS," specifically referring to the standard two-way fixed effects approach. While we proposed using the Goodman-Bacon decomposition approach for sensitivity analysis, we did not pre-register alternative estimation methods at that time. Given the rapidly evolving literature on staggered difference-in-differences estimation when we filed the PAP (June 2020), we were uncertain about which method would be most suitable for our analysis. We present the pre-registered TWFE results (in Appendix H) along with the Goodman-Bacon decomposition (in Appendix G). Our main findings, however, rely on staggered difference-in-differences estimators subsequently developed by Imai, Kim, and Wang (2023), Gardner (2022) and

Borusyak, Jaravel, and Spiess (2024), and made publicly available only after we wrote the PAP. We use the *PanelMatch* approach as our main estimation strategy for two primary reasons. Firstly, the estimator allows for matching on pre-trends, which is salient in our case since we find some degree of increased reelection rates prior to reform. Secondly, the estimator handles comparison units in an understandable way, which is important since we have a decreasing share of never-treated states over time. We also show results using Gardner (2022) and Borusyak, Jaravel, and Spiess (2024).

- **Deviation 3**: Our pre-analysis plan specified that the event study would examine 10 election cycles before and after reform (including a sensitivity check where our period varies from three cycles before the reform to ten cycles after). However, we do not have enough observations to identify the coefficients for 10 lags and leads when estimating the main regression specification (Equation 1). Therefore, our main event-study model specifications test five periods before and after reform. In addition, our pre-analysis plan specified a prediction (consistent with a selection mechanism) that effects on reelection rates would weakly increase over time. Our results indicate that the estimated coefficients remain stable at around five percent and show no increase over time following treatment, findings that are only partially consistent with our pre-registered prediction.

On the sensitivity analyses:

- **Deviation 4**: In our PAP, we wrote: "To check that our results are not sensitive to the inclusion of any particular state, we will reestimate our specification dropping each state one at a time. This will also allow us to verify that results hold even without the inclusion of open-primary Louisiana and unicameral Nebraska."

  In the paper, we do not drop states one at a time but instead permanently exclude four states (Mississippi, Maryland, Louisiana, and Alabama) that hold elections every four years. We also exclude Nebraska because it is unicameral and in addition half of the Senate's seats are up for election every four years.

- **Deviation 5**: The timing of reform may not be as-if random. To check that our results are not driven by legislators trying to pass civil service reform to win the upcoming election, we initially thought (following Folke, Hirano, and Snyder (2011)) that we might reestimate our preferred specification dropping the electoral cycle just before the reform and the electoral cycle immediately after reform (conditional on data availability). We decided against this approach since we now use the *PanelMatch* estimator, which corrects for pre-trends. We believe this is a superior approach to dropping data and relying on the problematic TWFE estimation strategy because we retain all the data and can correct for anticipation effects using the matching procedure.

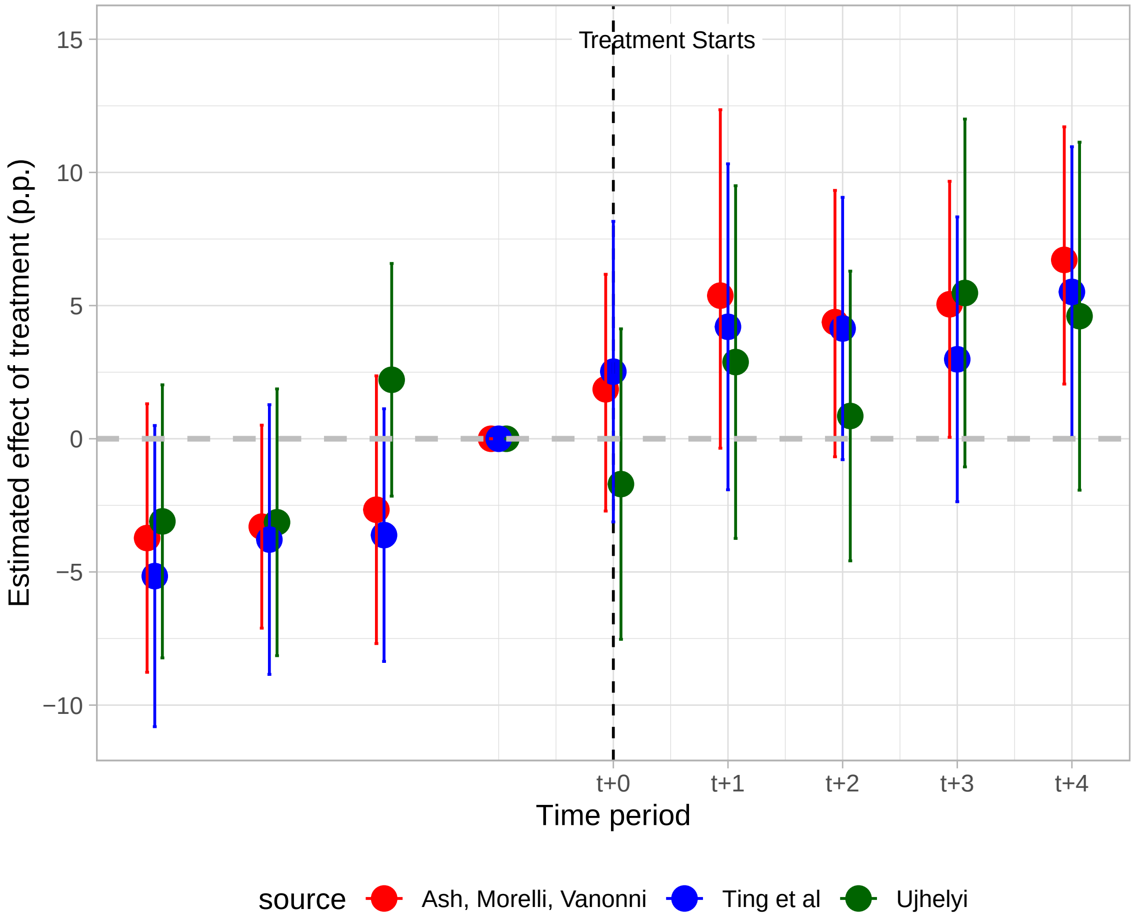
- **Deviation 6**: We initially aimed to provide qualitative evidence on the determinants of civil service reforms and remove states where adoption of the reform is most likely to be correlated with our dependent variable. After consideration of the data-collection process, we judged this unfeasible. Conducting a qualitative evaluation of primary sources from all 50 states would entail large-scale collection of primary archival materials from state legislatures and also would require we devise a scheme to analyze extensive records of voting patterns and debates within legislatures. This would be tantamount to a separate research project, one which evaluates the processes and dynamics behind the passage of reforms. In this paper, we instead focus on the effects of the meritocratic reforms (consequences) and leave the analysis of their introduction (causes) to future research efforts.

Additional (pre-registered) robustness checks:

- **On coding checks**: "To check that our results are not sensitive to the reform dates coded by Ash, Morelli, and Vannoni (2019), we will recode civil service reform using the dates reported by Ting et al. (2013) and then by Ujhelyi (2014) and reestimate Equation 1 with each alternative coding."

  Figure F-3 presents a comparison of the *PanelMatch* estimates for the three sources. Our effect sizes are largely consistent across the three datasets, although results using Ujhelyi

Figure F-3: Mahalanobis PanelMatch estimates: comparison of three sources for reform dates



(2014) are not statistically significant.

- "To check that the South does not exhibit different trends, we code the states from the deep South with a dummy δ_S and rerun our analysis excluding Southern states from our sample."

  We show that the result holds without the Southern states in Table H-2.

- "To check whether our results remain insensitive to possible differences in reelection rate data assembled by Ansolabehere, Ban, and Snyder (2017), by Klarner (2018), and by ourselves, we will add fixed effects for each data source."

  In Table F-1, we add data-source fixed effects (using a TWFE estimation). The main results remain unchanged.

Table F-1: Two-way fixed-effects estimates

	Dependent variable: reelection rate		
	(1)	(2)	(3)
post_reform	7.751***	3.650**	7.783***
	(2.260)	(1.542)	(2.319)
Election cycle FE	yes	yes	yes
State FE	yes	yes	yes
State-specific trends	no	yes	no
Data source FE	no	no	yes
N	2,535	2,535	2,535
R^2	0.775	0.826	0.775

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors, clustered by state, in parentheses.

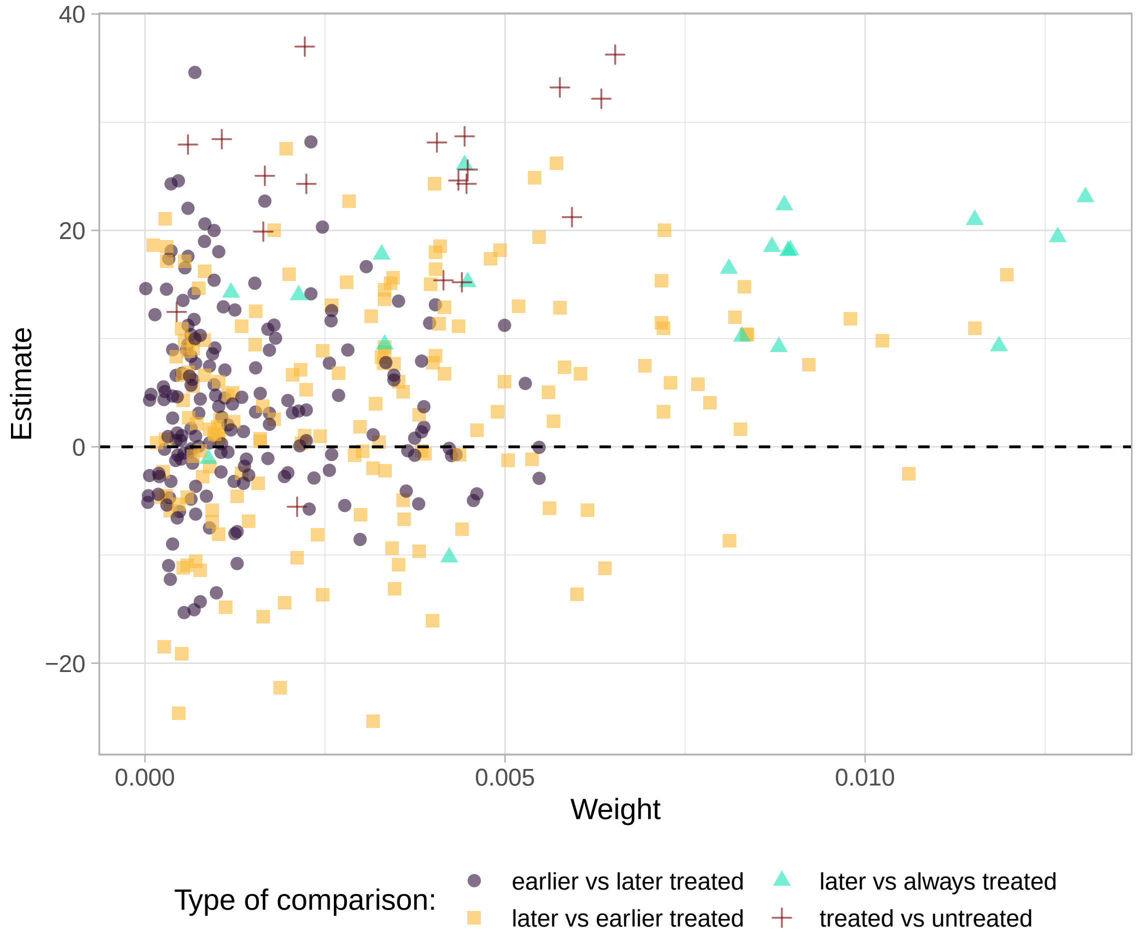
G Bias in two-way-fixed-effects estimations

Figure G-4 illustrates the estimates for every potential comparison and the weight each specific comparison was assigned when calculating the aggregate average treatment effect on the treated. Of these comparison groups, the later-versus-earlier and later-versus-always-treated are problematic, since they each include states that have received treatment in control groups. If the effects of reform are dynamic and evolve over time, this will bias estimates because states in the control group are also experiencing treatment effects.

These two potentially problematic comparisons are represented by the triangular and square points in Figure G-4. These comparisons have higher weighting than the unbiased comparisons we want to make; 93 percent of the TWFE estimate stems from these two problematic comparisons. In large part, these uneven weightings are driven by panel length and panel centrality (Goodman-Bacon 2021), which are not empirically useful or meaningful to our efforts to estimate the effect of treatment. This establishes that our target estimand is not accurately captured by the two-way fixed effects estimator and that bias could be induced by already-treated units serving as part of the control group.

In addition, a classic event study approach may suffer from under-identification and a short-run

Figure G-4: Goodman-Bacon weights in two-way fixed effects estimation



bias, both of which are especially pronounced when the control group is small (Borusyak, Jaravel, and Spiess 2024). In our case, Texas is a fully untreated case throughout the entire period, and hence this small control group problem applies throughout our estimation. Thus, given the structure of the reform process and the eventual adoption across almost all states, a standard event-study approach could induce bias.

H Two-way fixed-effects model results

We estimate a two-way fixed effects model leveraging within-state over-time adoption of civil service reform to estimate the average effect on reelection rates to the lower house. Because treatment effect estimates are impervious to the staggered introduction of treatment, they use inappropriate control groups. We are not very confident about the results of the TWFE estimation as a result (see

Figure G-4 and the accompanying discussion). However, it allows us to compare state reelection rates before and after reform and was pre-registered as an estimation strategy. The model is as follows, which we estimate using OLS:

$$Y_{st} = \beta_0 + \beta_1 \textit{Civil Service Reform}_{st} + D_s + T_t + \varepsilon_{st} \tag{A.2}$$

where Y_{st} is the reelection rate to the lower house in state s and year t; *Civil service reform* is an indicator variable (1/0) for whether the state has enacted civil service reform or not in state s and year t; D are state fixed effects, capturing time-invariant factors associated with state reelection rates; and T are year fixed effects accounting for common (cross-state) time trends in reelection rates. Finally, ε_{st} is a random error term clustered by state.

For the TWFE OLS model, we expect an improvement in reelection rates at the state level following reform. That is, we expect β_1 in Equation A.2 to be positive.

Table H-2 shows two-way fixed effects results. Column 1 estimates Equation A.2, where t is an election year; column 2 adds state-level term limits as a control variable; finally, column 3 adds a dummy variable for whether the size of the legislature changed.

Table H-2 shows that the impact of reform is estimated as an increase in reelection rates of 7.75 percentage points. The estimate remains statistically significant when we include term limits. The result is also robust to the inclusion of a control for changes in the size of the legislature. Finally, results continue to hold when we exclude southern states from the analysis. Results are not robust to the inclusion of state-decade fixed effects. We believe that this is a function of the reduced power such that state-decade fixed effects are soaking up variation which unfolds over several electoral cycles after reform.

I Randomization inference

Given that we have a long pre-treatment period, the parallel trends assumption is strong. It is also important, because imputation estimators assume parallel trends across the pre-treatment period. Testing the parallel trends assumption is functionally impossible given that it is dependent on a

Table H-2: Two-way fixed-effects estimates

	Dependent variable: reelection rate				
	(1)	(2)	(3)	(4)	(5)
Post-reform dummy	7.751***	7.987***	7.969***	7.711***	−0.682
	(2.260)	(2.333)	(2.326)	(2.313)	(1.237)
Term limits dummy		−14.901***	−14.961***	−15.024***	−13.297***
		(2.444)	(2.429)	(2.745)	(3.012)
Changing Legislature Size Dummy			−1.726	−2.890**	−1.119*
			(1.147)	(1.257)	(0.614)
Sample	All States	All States	All States	South Excluded	All States
Election year FE	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes
State-Decade FE					yes
N	2,535	2,535	2,535	2,015	2,535
R^2	0.775	0.790	0.791	0.796	0.897

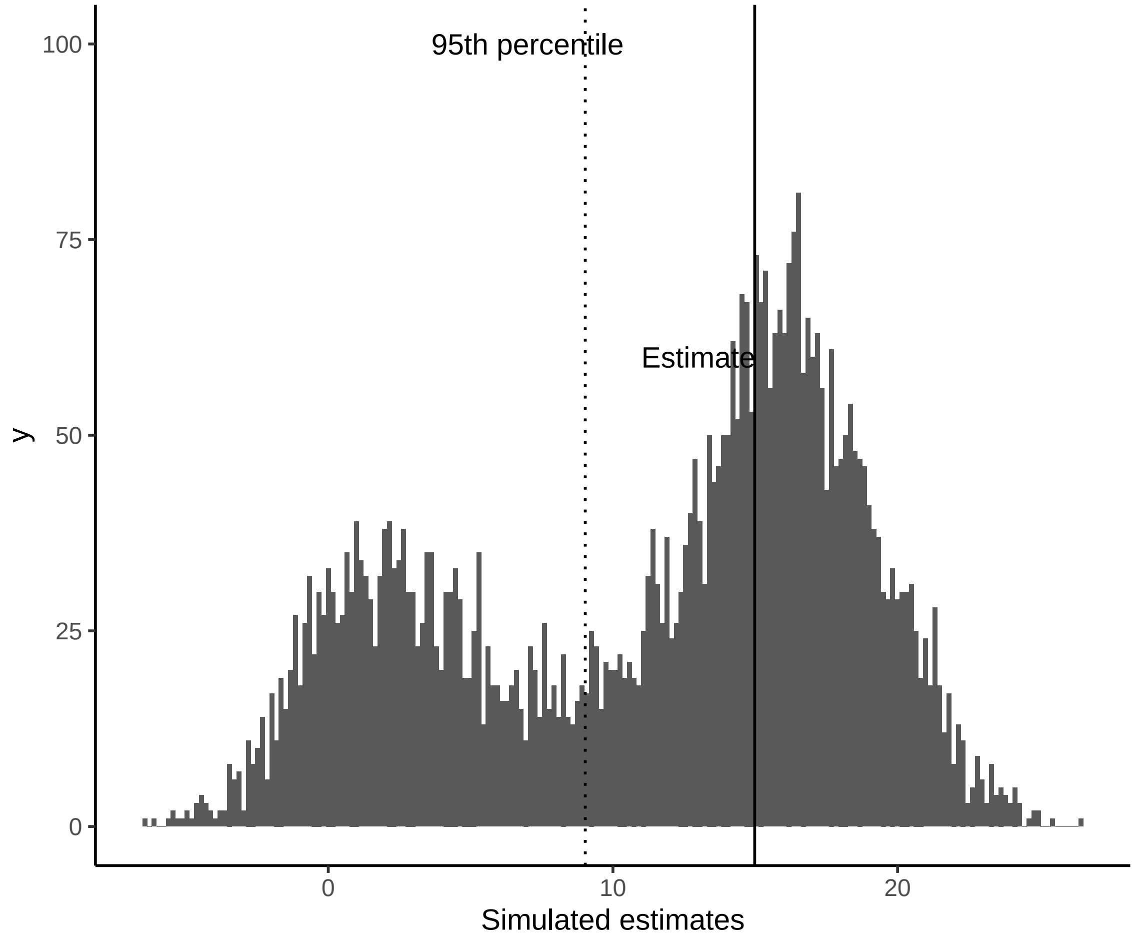
Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Standard errors, clustered by state, in parentheses.

counterfactual quantity. We attempt to test for parallel trends using placebos for different treatment timings. The logic is to create randomized treatment timings to generate placebo treatments; this is similar to testing during pre-treatment periods as if treatment had occurred. This approach is used in place of the more canonical 2x2 set-up, which we do not have; instead, we use placebo treatments to approximate the testing procedure for parallel trends in the 2x2 setup. Results appear in Figure I-6.

Operationalizing the randomization process implies rerunning the models over a set of simulated placebo treatment timings. The results of these placebo estimates are then plotted along with the estimate we obtain from the true data and the 95th percentile of the placebo estimates. The estimate we reference is the estimated stacked ATT from each imputation estimator. The effects of these randomized treatment timings are generally positive; this is to be expected since we see rising reelection rates over time.

The magnitude of the placebos are generally smaller than our estimated "true" effect. Particularly with the Gardner (2022) imputation estimator, we see that our estimate is comfortably beyond

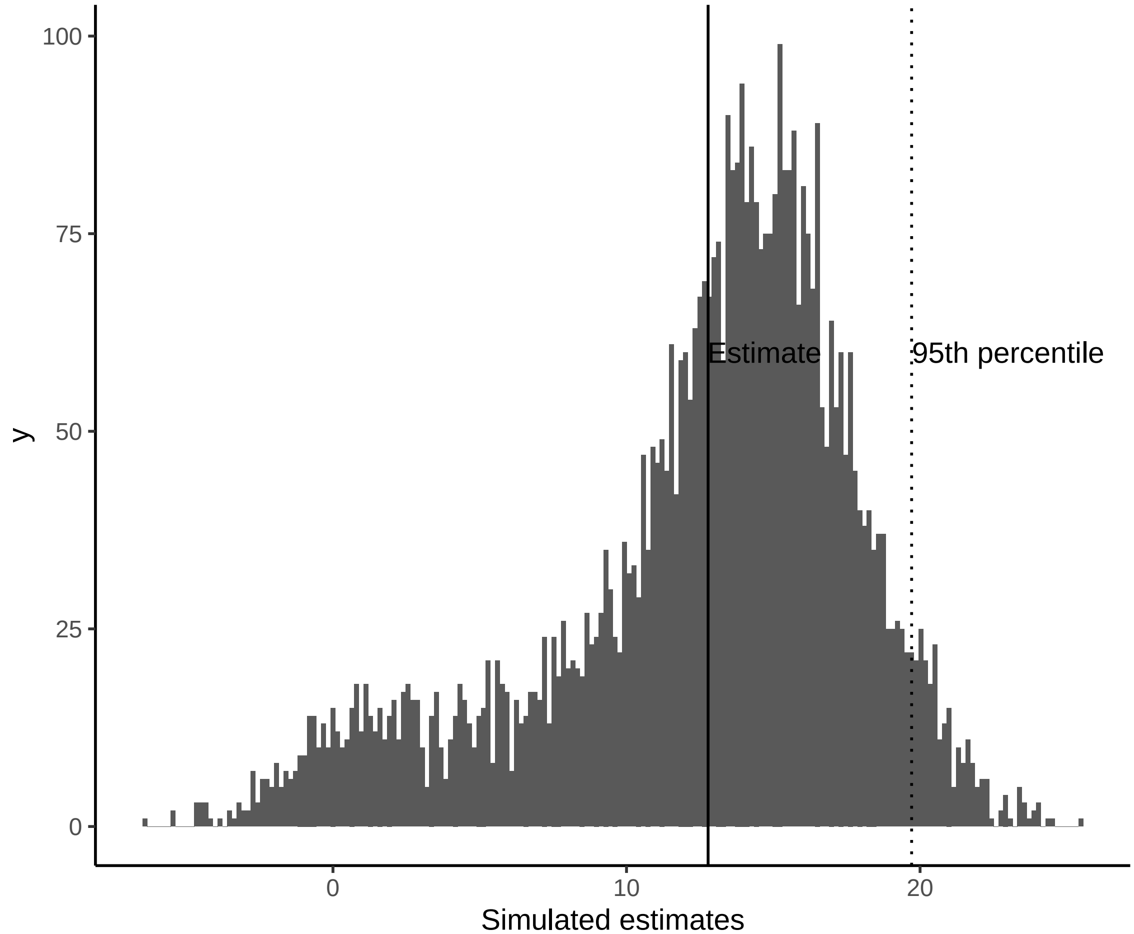
Figure I-5: Randomization inference with placebo treatments: using simulated treatment timings



the 95th percentile of the simulations. For the Borusyak, Jaravel, and Spiess (2024) estimator, we find that the simulated placebo treatments have a skew and that our estimate falls below the 95th percentile.

These placebo tests thus provide mixed evidence for the parallel trends assumption. With Borusyak, Jaravel, and Spiess (2024), we find our estimate far exceeding the simulations; using Gardner (2022), this is not the case. This difference arises from the changed control groups used by the two methods. We argue that the latter is the more appropriate method for our data but we present the Borusyak, Jaravel, and Spiess (2024) results as a way to show readers that even though the parallel trends assumption may not be met by all possible DiD methods, the basic results are similar across methods.

Figure I-6: Randomization inference with placebo treatments: using simulated treatment timings

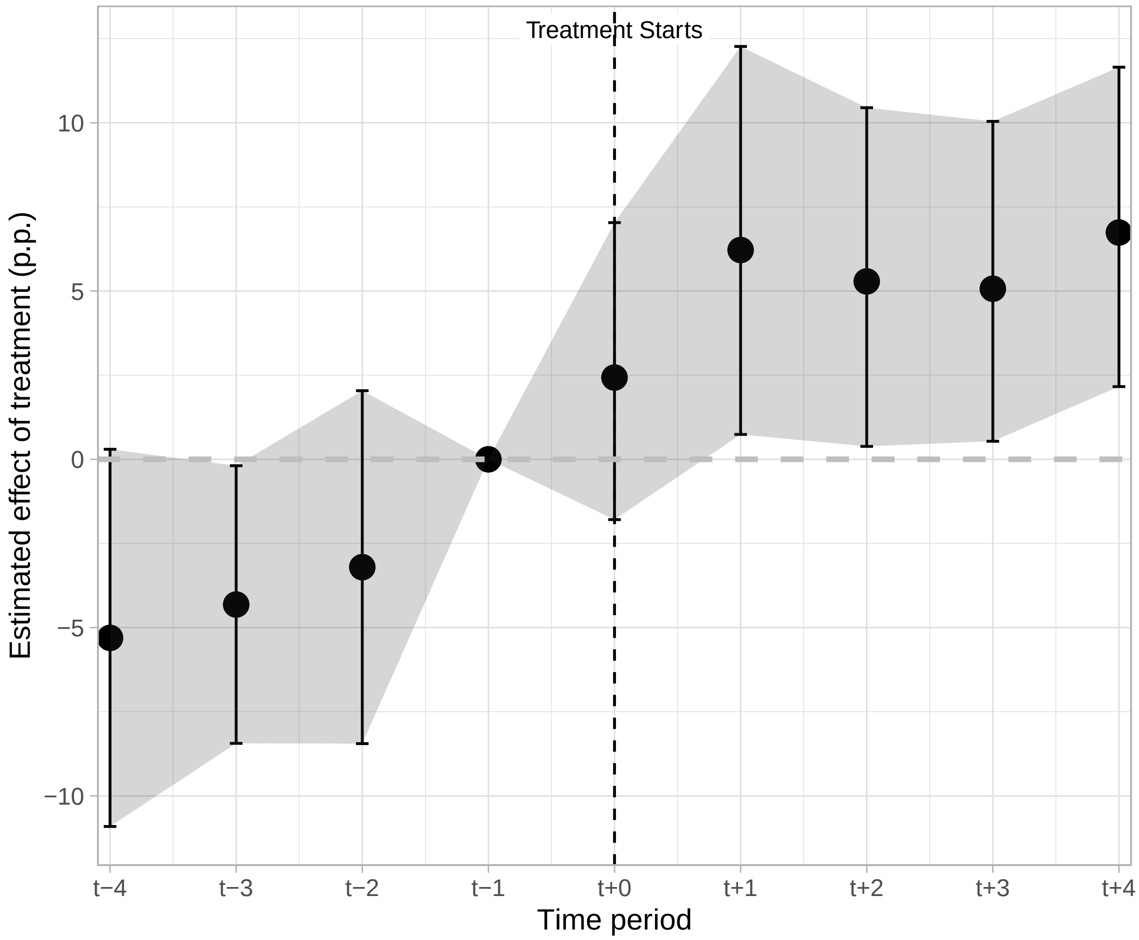


J PanelMatch robustness checks

Figure J-7 charts the results from PanelMatch unmatched estimates of the effects of reform on reelection rates in the election cycles subsequent to adoption. The 95 percent confidence bands are included around the coefficient point estimates. Results show that in the immediate aftermath of reform, reelection rates in reformed states increase relative to those in unreformed states. In the period immediately following adoption (*t+1*), the estimated effect is around 6 percentage points, although not statistically significant. By the following period — the second election after reform — the effect increases in size to around 10 percentage points and is statistically significant at the conventional 5 percent level. The effect stabilizes in the following electoral cycle and then slightly increases once again in the region of 12 percentage points four election cycles after reform.

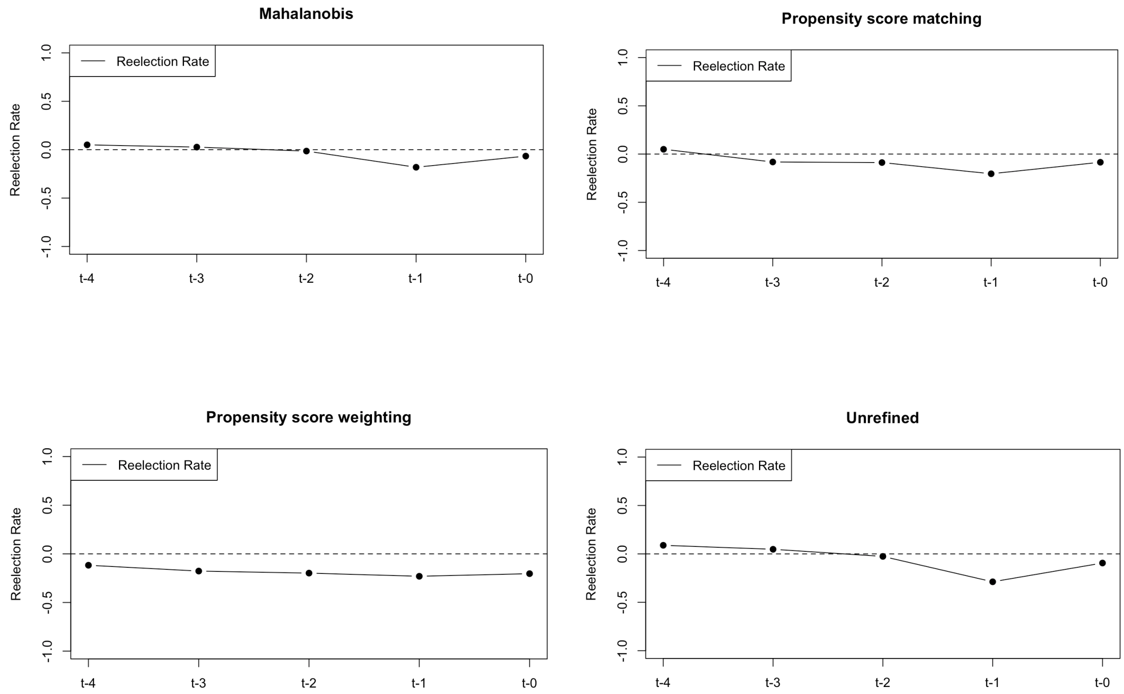
The effects of reform are also substantively significant over the course of elections following its adoption. The smallest effect of 5 percentage points in reform's immediate aftermath is larger than the effect we attain from the TWFE estimates (reported in Appendix H). In addition, the effect persists. We see a permanent shift compared to the pre-reform period, a shift that grows steadily as politicians in the legislature more often achieve reelection.

Figure J-7: Unrefined PanelMatch coefficient estimates



We use three matching procedures to make treated and control states more similar. Throughout, we utilize pre-reform reelection rates as the covariate of interest to create appropriate counterfactuals. In Figure J-8, we display how the three matching algorithms impact the differences between treated and control in the pre-treatment period. In the plots displayed in Figure J-8, we show the standard deviations from the treated units; a standard deviation of zero indicates a perfect pre-reform fit. We aim to construct a matched set with low pre-treatment differences to create a comparison where pre-reform trends in reelection rates are less pronounced.

Figure J-8: Covariate balance



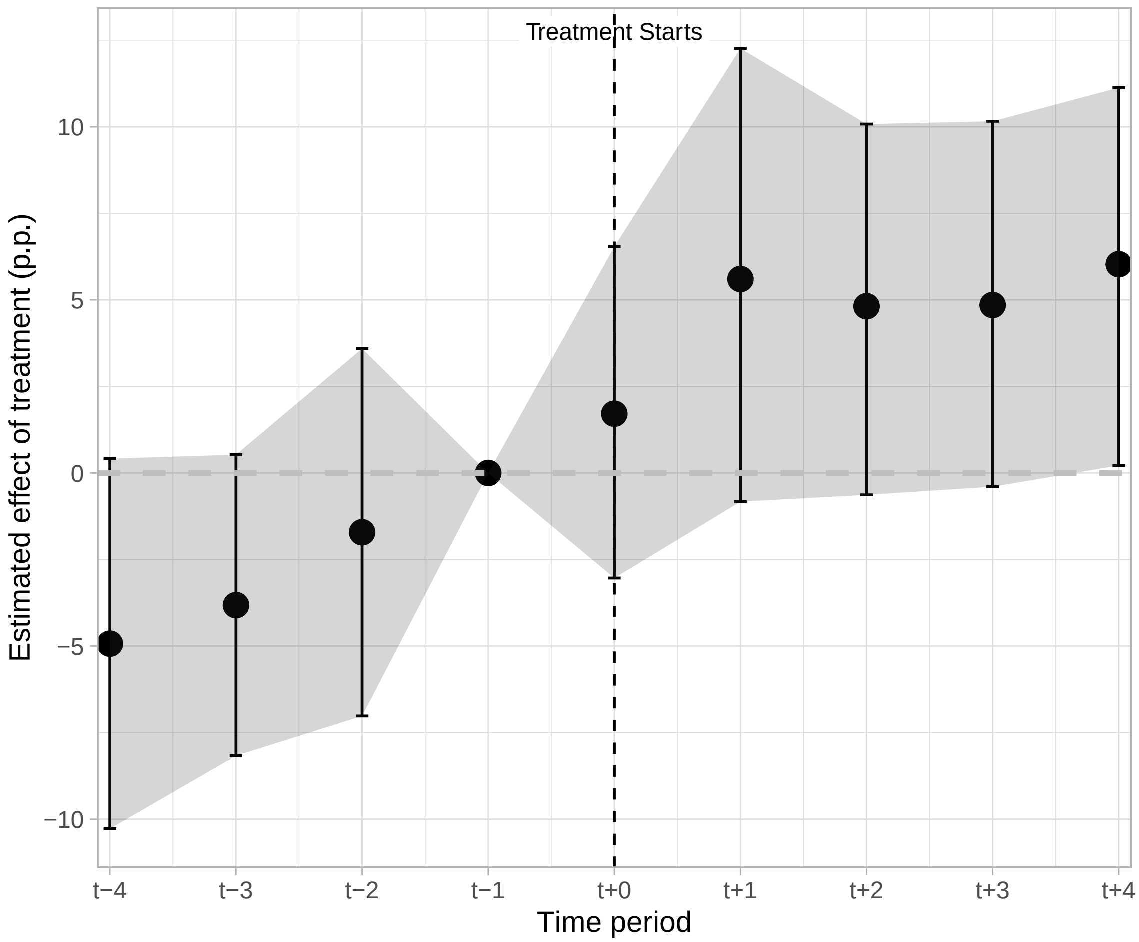
Note: We used the get_covariate_balance function with main = 'Mahalanobis' to generate the plots. The latest version of PanelMatch (v3) no longer supports Mahalanobis for covariate balance and the package is not backward compatible. To reproduce the figures, use PanelMatch version 2.2.2.

In the first panel in Figure J-8, we explore pre-trends in reelection rates between treated and control states. We find a good fit in the unrefined estimates, particularly in periods $t-4$ through $t-2$. We see a small difference in reelection rates emerge in the period $t-1$. Figure J-8 also shows that the Mahalanobis algorithm slightly improves the fit in $t-1$. Conversely, results of two propensity-score matching methods widen the differences compared to unrefined estimates. In the subsequent analyses, we use the Mahalanobis-derived set of states since this set provides more homogeneous reelection-rate trajectories for treated and control units.

To probe the robustness of our results, we rerun PanelMatch estimation and: (i) exclude Texas, since it never adopted reform and (ii) exclude term-limited politicians. Removing the never-taker state (Texas) to probe robustness is a useful exercise since there could be systematically different dynamics operating in a state that never reformed. Term limits could influence the magnitude of potential reelection rates to state houses and in addition are clustered towards the end of our time period. They could therefore distort the magnitude of our main results.

In Figure J-9, we show the main results excluding our never-treated observation. Estimates are similar to our main results. In the election cycles following reform, we see increased reelection rates even when Texas is not included in the comparison group.

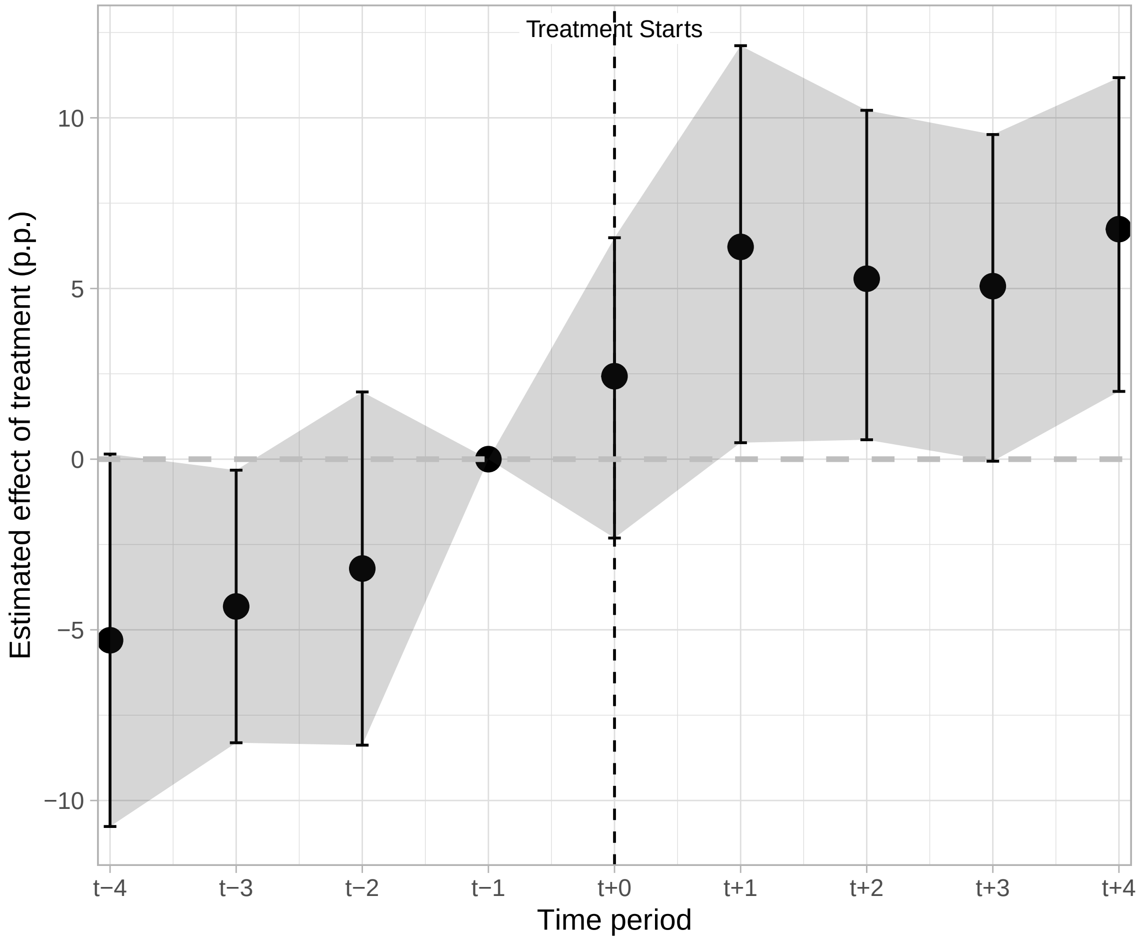
Figure J-9: PanelMatch estimates excluding Texas



In Figure J-10, we show results once we exclude states with term limits, all of which occur in the latter period under study. Results of this reestimation also look similar in magnitude to the main results.

As an additional robustness check, in Figure J-11, we show the main results estimated for two subsamples — before 1939 and after 1939 — to examine the possible impact of the Congressional amendment encouraging civil service reform at the state level. For the subsample of late-reformers (post-1939), we see a steady rise in reelection rates after reform up to approximately a 12 percentage

Figure J-10: PanelMatch estimates excluding states with term limits

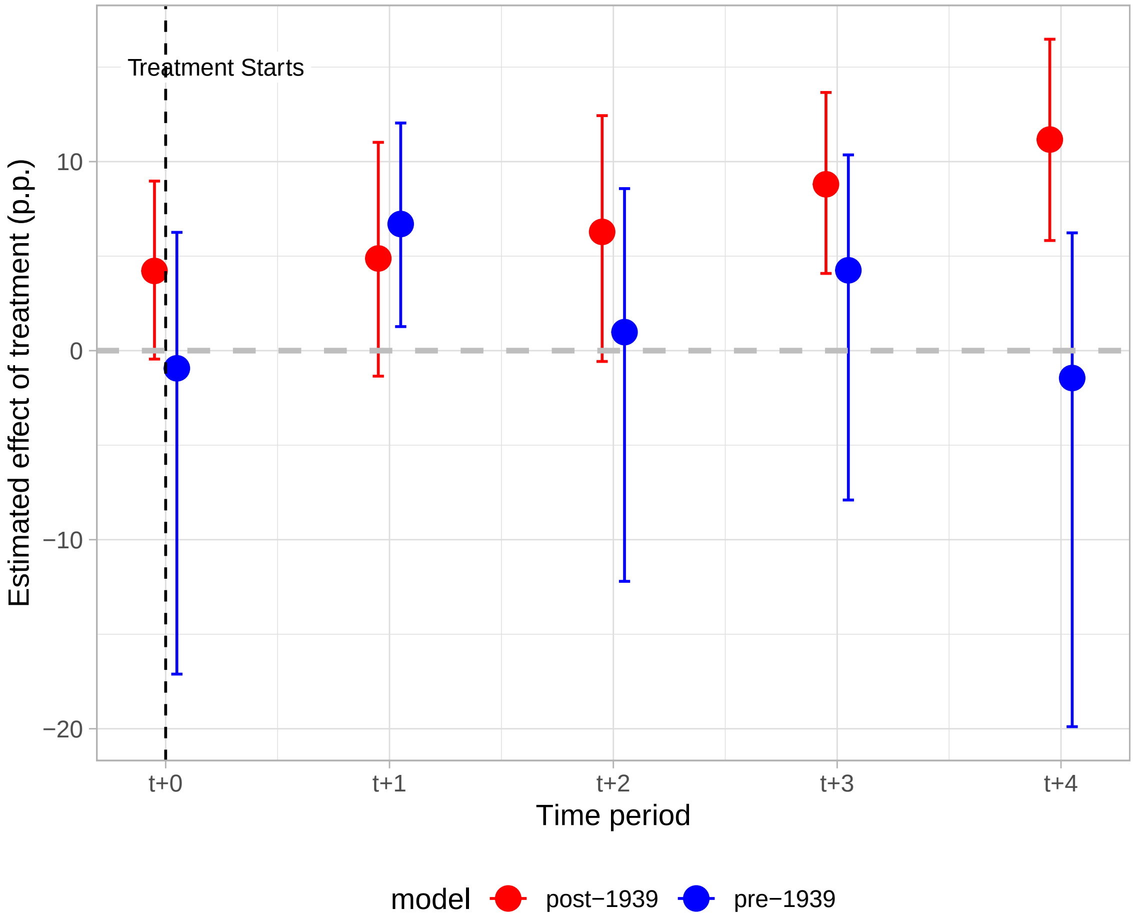


point increase. Conversely, we find small increases in the initial post-reform period for the earlier (pre-1939) reformed states, but as elections elapse the confidence intervals widen. This exploratory analysis suggests that later-reforming states are driving much of the overall positive effects across the entire sample.[10]

Lastly, we also test the longer-term effects of civil service reform on relection rates by expanding the temporal window beyond the five post-reform electoral cycles. Results appear in Figure J-12. We find consistently elevated reelection rates after reforms have been passed. The effects appear to stabilize in size around four electoral cycles after reform is passed, and they remain both substantively and statistically significant. The magnitude of effects is in the range of 7–10 percent-

[10]Note that we do not present pre-treatment placebo coefficients in Figure J-11 because there are not enough matched sets to estimate them.

Figure J-11: PaneMatch for subsets of state-cycles before and after 1939

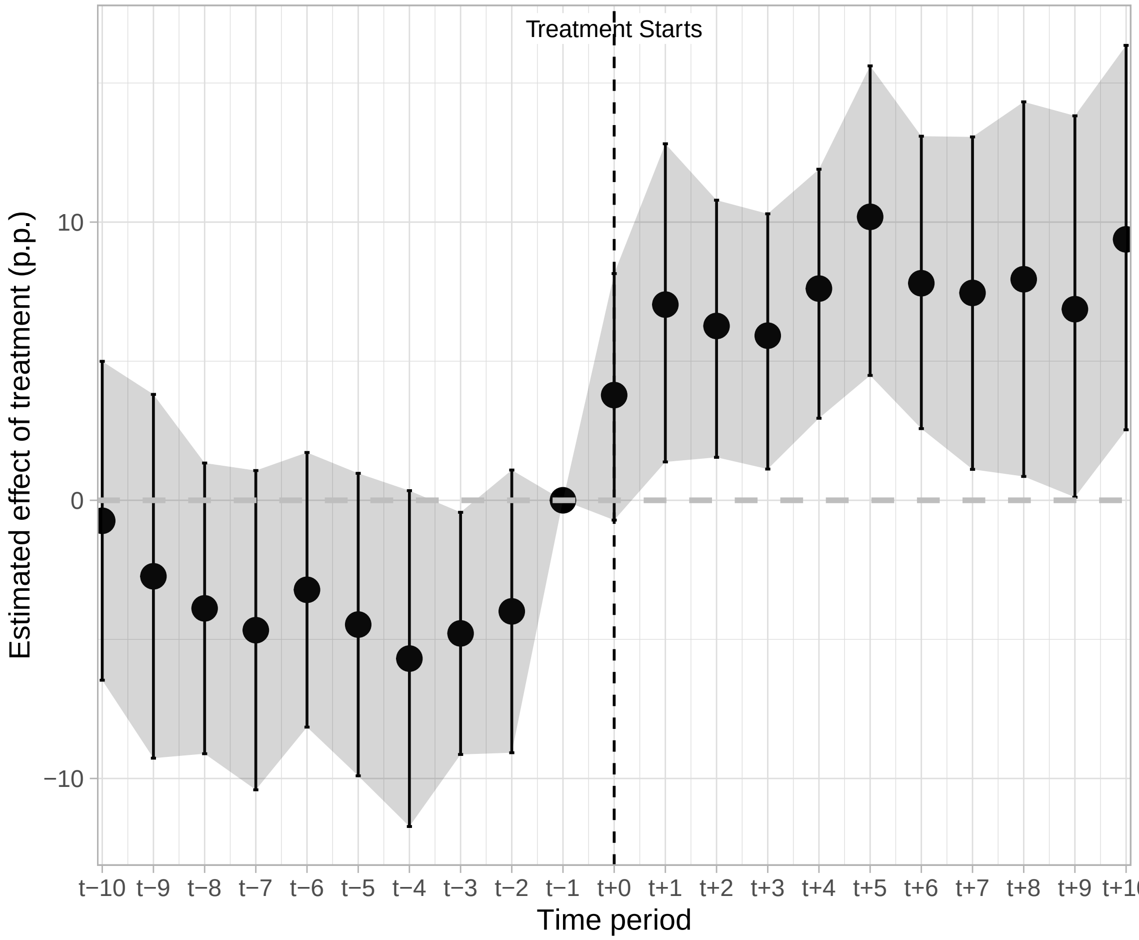


age points in the later post-reform periods. This is consistent with a generally increasing effect up until a point; there does appear to be stabilization after a few cycles, indicating that the effects are fully materialized.

K Comparing the rerunning-reelection gap before and after reform

We seek to run regression analyses for the nine states on which we have rerunning data. We omit Illinois, because the pre-reform period is too short to meaningfully interpret and we omit New York and Massachusetts because reform took place before 1900, when our data begin. We estimate regression models for the remaining six states with the gap between the rerunning and reelection rates as the dependent variable and civil service reform as the independent variable. This analysis tests whether rerunning and reelection have a stable relationship with each another. Evidence against a

Figure J-12: PaneMatch estimates across 10 post-reform election cycles



selection effect could consist of finding a significant difference in the gap before and after reform, where the difference between rates of rerunning and reelection was greater before than after reform. This would show that many incumbents tried to gain reelection but failed before reform, suggesting frustrated political ambition.

Table K-3 displays the estimates of how reform changes the gap between rerunning and reelection rates for the six available states. We find null results for all. The rates at which rerunners were elected are not affected by reform. Instead, the two rise together, with greater rerunning rising in parallel with reelection rates. This evidence is consistent with the descriptive evidence provided in the main text, where we saw consistent increases in both rerunning and reelection rates over the 20th century. When reelection rates are low, rerunning is also infrequent. Rising reelection rates are accompanied by increasing rerunning.

Table K-3: Effects of civil service reform on the gap between rerunning and reelection

	Dependent variable:					
	gap					
	CT	IA	IN	MI	OH	WA
	(1)	(2)	(3)	(4)	(5)	(6)
After reform	0.011	−0.009	0.013	−0.038	0.052	−0.037
	(0.013)	(0.029)	(0.033)	(0.028)	(0.068)	(0.031)
Constant	0.101***	0.130***	0.175***	0.119***	0.169**	0.125***
	(0.009)	(0.012)	(0.021)	(0.019)	(0.064)	(0.017)
Observations	32	37	30	37	18	33
R^2	0.021	0.003	0.006	0.048	0.036	0.043
Adjusted R^2	−0.012	−0.026	−0.030	0.021	−0.025	0.013
Note:				*p<0.1; **p<0.05; ***p<0.01		

We also examine whether the passage of civil service reform increases rerunning rates in the six states for which we have data and report the results in Table K-4. We observe no effect, in line with our result on the gap between rerunning and reelection remaining consistent pre- and post-reform. After reform is passed, rerunning and reelection rates both increase and a consistent share of rerunners are returned to office. Reform does not make politicians more successful at reelection; instead, it appears that politicians hone their skills and as a by-product, reelection rates increase in the period we study.

Table K-4: Effects of civil service reform on rerunning and reelection in six states

	Dependent variable:			
	Rerunning rate		Reelection rate	
	(1)	(2)	(3)	(4)
Post-reform dummy	0.141***	0.032	0.099***	0.019
	(0.016)	(0.031)	(0.019)	(0.028)
Sample	Six states	Six states	Six states	Six states
Election year FE	no	yes	no	yes
State FE	no	yes	no	yes
Observations	281	281	281	281
R^2	0.210	0.805	0.092	0.805
Adjusted R^2	0.207	0.741	0.089	0.741
Residual Std. Error	0.137 (df = 279)	0.079 (df = 211)	0.155 (df = 279)	0.083 (df = 211)
Note:			*p<0.1; **p<0.05; ***p<0.01	

Using more extensive data, we thus confirm earlier research reporting a close correspondence between rerunning and reelection rates for a sample of states during a ten year period (1925–1935), which was interpreted as demonstrating that most incumbents did not wish to retain office (Hyneman 1938).

L Government spending

To assess if reform affects legislative performance, we study whether reform appears causally linked to increases in state-level public spending. As shown in Figure 10, we find null effects of reform on spending. We now present auxiliary analyses regarding the relationship between state spending and reelection. We examine the correlation between per capita state expenditures and reelection rates to examine whether politicians are rewarded at the ballot box for prior government spending. Results are depicted in Figure L-13.

Figure L-13: Per capita expenditures and reelection rates

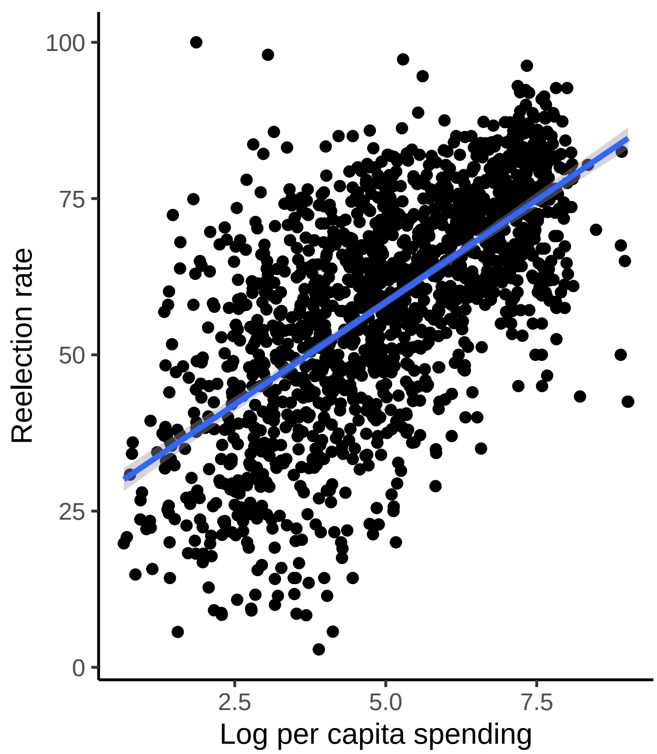


Figure L-13 shows that reelection rates and government spending are closely correlated. We suspect both to be features of the larger transformation of state politics that took place during the

20th century, as states gained more fiscal resources. Years when per capita spending is greater also have higher reelection rates to their state legislatures.

Table L-5: Effect of historic (lagged) spending on reelection rates

	Reelection rate				
	reel_lag1 1 cycle lag	reel_lag2 2 cycle lag	reel_lag3 3 cycle lag	reel_lag4 4 cycle lag	reel_lag5 5 cycle lag
	(1)	(2)	(3)	(4)	(5)
Log expenditures per capita	0.470	3.277***	4.311***	4.986***	6.160***
	(1.270)	(1.230)	(1.234)	(1.257)	(1.261)
Cycle FEs	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y
Observations	1,537	1,537	1,537	1,537	1,537

Note: *p<0.1; **p<0.05; ***p<0.01

We statistically estimate the relationship between lagged spending and reelection rates, testing the hypothesis that greater spending per capita heightens reelection rates in subsequent electoral cycles. Results appear in Table L-5. They show that lagged spending from previous election cycles has a significant and substantively large positive effect on reelection rates. The effect is largest further back in time, around five electoral cycles prior (10 years). Nevertheless, there is also a slightly smaller effect of spending four years before the current election (see column 2). In general, capita spending appears to impact the ability of politicians to gain reelection, with the stock of historic spending working in favor of politicians gaining office again. In this sense, legislative political careers become more stable with the growth in government.

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M Pre-analysis plan

Effects on Reelection Rates
of the Introduction of Merit Civil
Service Appointments in U.S. States

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Pre-analysis Plan

March 9, 2020

Research Motivation: We draw on theoretical work advanced in Golden, Nazrullaeva, and Wolton 2018. They theorize that there exists a difference in the types of individuals elected to public office depending on the rent-seeking opportunities while holding office. In settings with extensive opportunities for rent-seeking, individuals who run for office are more likely to be self-interested, whereas in settings where rent-seeking opportunities are curtailed, individuals who run for office are more likely to be public-spirited. They further theorize that voters prefer to elect public-spirited individuals. Assuming that voters are able to distinguish public-spirited from self-interested politicians, reelection rates will therefore vary systematically with the distribution of types who are elected to public office.

Working Hypothesis: We hypothesize that weak rule of law facilitates rent-seeking, and thus when the rule of law is strengthened, candidate types shift away from rent-seekers towards public-spirited individuals. As a testable implication, we hypothesize that reelection rates subsequently rise.

Analysis: We investigate this hypothesis at the level of state legislatures in the United States. State legislatures adopted civil service reform over a period that spanned 106 years — from 1883 to 1989. Of the fifty states, only one (Texas) has never adopted such legislation. Civil service reform put an end to patronage appointments in the state bureaucracy, which had allowed politicians discretion in appointing their own supporters to bureaucratic posts. As a result, civil service reform reduced the scope for rent-seeking by elected officials. We assemble and analyze data to investigate whether the adoption of civil service reform at the state level resulted in an improvement in the reelection rates of representatives in state legislatures.

Our analysis covers the period from 1946 to 2016. Although civil service reform began more than 50 years earlier — New York adopted civil service legislation in 1883, followed two years later by Massachusetts — only 19 states were under reform legislation by 1946. Thus, more than 60 percent of reform legislation was adopted in the years that followed the end of World War II. There is much greater election data availability for the entire matrix of reformed and unreformed states in the postwar period. For this reason, we confine our analysis to those years.

Data: We drew on data on civil service reform for different states and years from multiple sources. The first to assemble this data was Folke, Hirano, and Snyder 2011, which however reported the underlying data only in graphical format; this was followed by Ting et al. 2013, which presents what appears to be data identical to that of Folke, Hirano, and Snyder 2011 but in numerical (tabular) format. Subsequently, Ujhelyi 2014 released a dataset that reported different years than Ting et al. 2013 for the adoption of civil service reform for some states. Finally, Ash, Morelli, and Vannoni 2022 provides a thorough review of the earlier discrepancies in coding and adjudicates among dates, identifying the year in which legislation was formally adopted. We use the adoption date as coded by them and reported in Ash, Morelli, and Vannoni (2022, table A1, col 4).

We combine the data on civil service reform adoption dates provided by Ash, Morelli, and Vannoni 2019 with data drawn from a variety of sources (see below) that provides (ideally) candidate-level information about state legislative election results. Our goal is to collect all election cycles for all fifty US states for the period from 1946 to 2016.[11]

For data on reelection rates, we begin with a state-level election dataset available at Dataverse

[11] Alaska and Hawaii joined the Union in 1959. Alaska adopted civil service reform in 1960 and Hawaii in 1955; we have reelection data for both states only as of 1958. In effect, therefore, both states are always coded as reformed for the period for which we have reelection data.

and assembled by Ansolabehere, Ban, and Snyder 2017. This dataset gives candidate-level state legislative election returns for many states between 1890 and 1978. However, data is available only very sparsely prior to 1900.We combine this dataset with one assembled and made publicly available by Carl Klarner (Klarner 2018); his dataset extends that of Ansolabehere, Ban, and Snyder 2017 and gives candidate-level state legislative returns from 1968 to 2016. We use data on lower houses (and the unicameral legislature for Nebraska).

After combining these two sources, we are still left with some missingness. To fill in the missingness, we collected (or are still collecting) additional data directly from state legislative offices on who served in each legislative period. These data are made available in .pdf format; we input them electronically. Some state legislatures provide candidate-level data whereas most provide only lists of elected representatives. With the latter, we can calculate reelection rates but we lose information that would allow us to calculate the size of the margin of the winner.

Table M-6 shows the source of data by state for each election cycle that we study, as well as where current missingness is located as of this writing (August 11, 2025). As of this writing, we still need to collect data for approximately 57 election cycles of 1,697 that we study.

Estimation: To estimate the effect of civil service reform, we employ both a staggered difference-in-differences model and an event study approach.

The staggered difference-in-differences model leverages within-state over-time changes in civil service laws — here, whether patronage appointments are allowed or not — to estimate the average effect of civil service reform on reelection rates to the lower house. This allows us to compare state reelection rates before and after reform. The model is as follows, and we estimate it using OLS:

$$Y_{st} = \beta_0 + \beta_1 \mathit{Civil\ Service\ Reform}_{st} + D_s + T_t + \epsilon_{st} \tag{A.2}$$

where Y_{st} is the reelection rate in the lower house in state s and year t; *Civil service reform* is an indicator for whether the state has enacted civil service reform (1) or not (0), thus prohibiting patronage appointments, in state s and year t; D are state fixed effects, capturing time-invariant factors predicting state reelection rates; and T are year fixed effects accounting for common (across-state) time trends in reelection rates. Finally, ϵ_{st} is a random error term clustered by state.

States that adopted reform prior to 1946 are always coded as treated. States that adopt reform after 1946 change state and therefore the composition of the control group changes over time.

The event study shares many similarities with the staggered difference-in-differences approach, though it also allows for more flexibility. Still denoting Y_{it} the reelection rate in the lower house in state i in time t, D a state fixed effect, and T a year fixed effect, we run the following OLS regression:

$$Y_{st} = \beta_0 + \sum_{k=1}^{K} \beta_k Cycle_{st}^{k} + D_s + T_t + \epsilon_{st}, \tag{A.3}$$

where $Cycle^k$ is an indicator variable that equals 1 if a civil service reform has been implemented k cycles ago (e.g., $Cycle^3$ takes value 1 if it is the *third* election post-reform of the civil service). To avoid fully saturating the model and have some indicator variables estimated on very few observations, we impose $K = 10$ and group all reforms that are more than ten cycles old under $Cycle^{10}$. In other words, $Cycle^{10}$ takes the value 1 if it is at least the tenth election occurring after the civil service reform.

In case of missing data for one (or more) election cycle(-s) in a state, our preferred specification will drop the state entirely from the analysis. We will also perform other (less conservative) estimations using the reduced dataset for that state; i.e. we will omit an election cycle.

Expected Results: For the difference-in-differences model, we expect a statistically significant improvement in reelection rates at the state level. That is, we expect β_1 in Equation A.2 to be positive.

For the event study, we have two theoretically motivated expectations. First, we expect some of the β_ks to be positive. Second, for all positive βs, we expect the regression coefficients to be weakly increasing (formally, for all $j, m \in \{1, \ldots, 10\}$ such that $j < m$ and $\beta_j \geq 0$, then $\beta_j \leq \beta_m$). Since we test for the statistical significance of several variables, we will also correct our statistical test for multiple hypothesis testing.

Robustness of Results: To examine whether the results that we expect are robust, we will perform the following procedures.

1. Sensitivity analyses: Difference-in-differences (Equation A.2)

1. To check that our results are not sensitive to the inclusion of any particular state, we will reestimate our specification dropping each state one at a time. This will also allow us to verify that results hold even without the inclusion of open-primary Louisiana and unicameral Nebraska.
2. To check that the South does not exhibit different trends, we code the states from the deep South with a dummy δ_S, and control for a trend t in reelection rates for the deep South states $\delta_S t$ to Equation A.2.
3. To account for change in states' partisanship patterns over time, we will also run our specification including state-decade fixed effects (a common approach to long historical data; e.g., Fowler and Hall 2018).
4. To check that our results are not sensitive to the particular elements of the staggered implementation of the reform, we will follow the approach outlined in Goodman-Bacon 2019 and study the decomposition of the difference-in-differences estimate $\hat{\beta}_1$ in Equation A.2 that compares timing groups (states that are early versus late adopters) and investigate their weights.

2. Sensitivity analyses: Event study (Equation A.3)

1. To check that our results are not sensitive to the inclusion of any particular state, we will reestimate the model dropping each state one at a time. This will also allow us to verify that results hold even without the inclusion of open-primary Louisiana and unicameral Nebraska.
2. To check that the South does not exhibit different trends, we code the states from the deep South with a dummy δ_S, and control for a trend t in reelection rates for the deep South states $\delta_S t$ to Equation A.3.
3. To account for change in states' partisanship patterns over time, we will also run our specification including state-decade fixed effects (a common approach to long historical

data; e.g., Fowler and Hall 2018).

4. To check that our results are not sensitive to the particular coding of the $Cycle$ variable, we will reestimate our model with the upper bound moving from $K = 5$ to infinity (i.e., without right-censoring of the $Cycle$ variable).

5. To check that our results are not driven by pre-trends, we will reestimate Equation A.3 with indicator variables for three periods prior to the reform (i.e., the sum will go from $k = -3$ to $K = 10$).

6. To provide an additional check that our results are not due to spurious correlation, we will randomly allocate treatment dates (years of civil service reform) in our sample and rerun the analysis using Equation A.3. The empirical estimates obtained from these simulations will then be compared to the empirical estimate obtained using the actual reform dates.

3. Coding checks:

1. To check that our results are not sensitive to the reform dates coded by Ash, Morelli, and Vannoni (2019), we will recode *Civil service reform* using the dates reported by Ting et al. (2013) and then by Ujhelyi 2014 and reestimate Equation A.2 with each alternative coding.

2. To check whether our results are not sensitive to possible systematic differences in reelection rate data assembled by Ansolabehere, Ban, and Snyder 2017, by Klarner 2018, and by ourselves, we will add fixed effects for each data source.

3. The timing of reform may not be as-if random. To check that our results are not driven by legislators trying to pass civil service reform to win the upcoming election, we will (following Folke, Hirano, and Snyder (2011)) reestimate our preferred specification dropping the electoral cycle just before the reform and the electoral cycle immediately after the reform is adopted (conditional on data availability).

4. For the same reason as the prior item, we will provide qualitative evidence on the determinants of civil service reforms and will remove states where adoption of the reform is

most likely to be correlated with our dependent variable.

Table M-6: Data for all states by availability and source, 1946–2016

code	state	source	first year	last year	reform date	missing data
AL	Alabama	Snyder	1946	1966	1939	
		Klarner	1970	2014		
AK	Alaska	Snyder	1958	1966	1960	joined the Union in 1959
		Klarner	1968	2016		
AZ	Arizona	Snyder	1946	1966	1968	
		Klarner	1968	2016		
AR	Arkansas	Snyder	1952	1966	1969	1946, 1948, 1950
		Klarner	1968	2016		
CA	California	Snyder	1946	1966	1913	
		Klarner	1968	2016		
CO	Colorado	Snyder	1946	1966	1918	
		Klarner	1968	2016		
CT	Connecticut	Snyder	1948	1966	1937	1946
		Klarner	1968	2016		
DE	Delaware	Snyder	1950	1966	1966	1946, 1948
		Klarner	1968	2016		
FL	Florida	Snyder	1952	1966	1967	1946, 1948, 1950
		Klarner	1968	2016	1967	
GA	Georgia	State archives	1946	1954	1945	
		Snyder	1952	1966		
		Klarner	1968	2016		
HI	Hawaii	Snyder	1958	1966	1955	joined the Union in 1959
		Klarner	1968	2016		
ID	Idaho	Snyder	1952	1966	1967	1946, 1948, 1950
		Klarner	1968	2016		
IL	Illinois	Snyder	1952	1966	1905	1946, 1948, 1950
		Klarner	1968	2016		
IN	Indiana	Snyder	1948	1966	1941	1946
		Klarner	1968	2016		
IA	Iowa	Snyder	1946	1966	1966	
		Klarner	1968	2016		
KS	Kansas	Snyder	1946	1966	1941	1948
		Klarner	1968	2016		

Table M-6: Data for all states by availability and source, 1946–2016 *(continued)*

code	state	source	first year	last year	reform date	missing data
KY	Kentucky	Snyder	1951	1967	1960	1947, 1949
		Klarner	1969	2016		
LA	Louisiana	State archives	1947	1947	1952	
		Snyder	1952	1966		1946, 1948, 1950
		Klarner	1968	1972		
ME	Maine	Snyder	1952	1966	1937	1946, 1948, 1950
		Klarner	1968	2016		
MD	Maryland	Snyder	1946	1966	1921	
		Klarner	1970	2014		
MA	Massachusetts	Snyder	1946	1966	1885	
		Klarner	1968	2016		
MI	Michigan	Snyder	1946	1966	1940	
		Klarner	1968	2016		
MN	Minnesota	State archives	1946	1948		
		Snyder	1946	1966	1939	
		Klarner	1968	2016		
MS	Mississippi	Snyder	1951	1967	1976	1947
		Klarner	1971	2015		
MO	Missouri	Snyder	1946	1966	1945	1948
		Klarner	1968	2016		
MT	Montana	Snyder	1952	1966	1976	1946, 1948, 1950
		Klarner	1968	2016		
NE	Nebraska	Snyder	1950	1968	1975	1946, 1948
		Klarner	1958	2016		
NV	Nevada	Snyder	1952	1966	1953	1948, 1950
		Klarner	1968	2016		
NH	New Hampshire	State archives	1946	1948	1950	
		Snyder	1950	1966		1950 (no names)
		Klarner	1968	2016		
NJ	New Jersey	Snyder	1947	1967	1908	
		Klarner	1969	2015		
NM	New Mexico	Snyder	1948	1966	1961	1946
		Klarner	1968	2016		

Table M-6: Data for all states by availability and source, 1946–2016 *(continued)*

code	state	source	first year	last year	reform date	missing data
NY	New York	Snyder	1946	1966	1883	
		Klarner	1968	2016		
NC	North Carolina	State archives	1946	1958	1949	
		Snyder	1952	1966		
		Klarner	1970	2016		
ND	North Dakota	Snyder	1952	1966	1975	1946, 1948, 1950
		Klarner	1968	2016		
OH	Ohio	Snyder	1952	1966	1913	1946, 1948, 1950
		Klarner	1968	2016		
OK	Oklahoma	Snyder	1946	1966	1959	
		Klarner	1968	2016		
OR	Oregon	Snyder	1946	1966	1945	
		Klarner	1968	2016		
PA	Pennsylvania	Snyder	1946	1966	1963	
		Klarner	1968	2016		
RI	Rhode Island	Snyder	1946	1966	1939	
		Klarner	1968	2016		
SC	South Carolina	Snyder	1952	1966	1969	1946, 1948, 1950
		Klarner	1968	2016		
SD	South Dakota	Snyder	1952	1966	1973	1946, 1948, 1950
SD	South Dakota	Klarner	1968	2016		
TN	Tennessee	State archives	1946	1946	1937	
		Snyder	1952	1966		
		Klarner	1968	2016		
TX	Texas	Snyder	1952	1966	none	1946, 1948, 1950
		Klarner	1968	2016		
UT	Utah	Snyder	1952	1966	1963	1946, 1948, 1950
		Klarner	1968	2016		
VT	Vermont	State archives	1946	1948	1950	
		Snyder	1952	1966		
		Klarner	1986	2016		
VA	Virginia	State archives	1947	1951	1943	
		Snyder	1949	1967		

Table M-6: Data for all states by availability and source, 1946–2016 *(continued)*

code	state	source	first year	last year	reform date	missing data
		Klarner	1969	2015		
WA	Washington	Snyder	1946	1966	1961	
		Klarner	1968	2016		
WV	West Virginia	Snyder	1948	1966	1989	1946
		Klarner	1968	2016		
WI	Wisconsin	Snyder	1946	1966	1905	
		Klarner	1968	2016		
WY	Wyoming	Snyder	1950	1966	1957	1946, 1948
		Klarner	1968	2016		

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N Codebook

Codebook for "Dataset on Individuals Serving in State Legislatures in the United States, 1900–2016"

Ivan Fomichev and Miriam Golden

2025-02-24

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Dataset description

Assembles individual-level data on all legislators serving in state legislatures in the 50 U.S. states over time. Characteristics collected include first and last name, legislative period, whether reelected into the subsequent legislature and, for 9 states, whether the individual sought reelection into the next legislature. Data is for the lower house or the unicameral assembly. Years generally span 1900 to the late-1940s but in some cases go as far as 2016.

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How to Cite this Dataset

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2 Funding

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3 Overview

3.1 Aims of the project

The aims of this project are to assemble data on individual legislators serving in U.S. state legislatures over the entire 20^{th} centery and into the 21^{st} in order to calculate rates of reelection and, to the extent possible, rerunning rates. Existing datasets of elected legislators that we consulted and supplemented are Ansolabehere, Ban, and Snyder (2017) and Klarner (2018). The first mainly covers the period from 1920 to 1968 and the second covers 1967 to 2016. To calculate complete reelection rates by legislature-election, we collected information on all legislators who were missing from both these other datasets for any legislature between 1900 and 2016.

The posted dataset includes only the data collected by Golden and Nazrullaeva for the years not available in Ansolabehere, Ban, and Snyder (2017) or Klarner (2018). Researchers seeking data on other years should consult either Ansolabehere, Ban, and Snyder (2017) or Klarner (2018).

All rerunning data was collected specifically for the current project and is included in the posted dataset.

3.2 Variables collected

Variables collected are: legislators' first and last names, the legislative period (the year the legislature was convened), and whether the individual was an incumbent in the election at time t. In some cases, we were able to retrieve party affiliation and the constituency of legislators, although these data were not systematically collected.

For nine states, we also collected data on whether incumbent legislators chose to run again for the subsequent election at time $t+1$. Additionally, the dataset contains quality control measures used to assess the accuracy of rerunning coding. Table 1 lists all the variables included in the dataset.

Table 1: Variables in the Dataset

Variable	Description
state	U.S. state abbreviation.
year	Year of the election.
id	ID of the representative.
name_full	Full name of the representative.
last	Last name of the representative.
first	First name of the representative.
middle	Middle name or initial of the representative (if available).
party	Political party affiliation of the representative (if available).
county	The county represented by the legislator (if available).
inc	Binary indicator for incumbency status at the moment of the election at time t. 1: incumbent, 0: non-incumbent.
runs_next_election	Binary indicator for whether the representative reran for election at time t+1. 1: reran, 0: did not rerun.

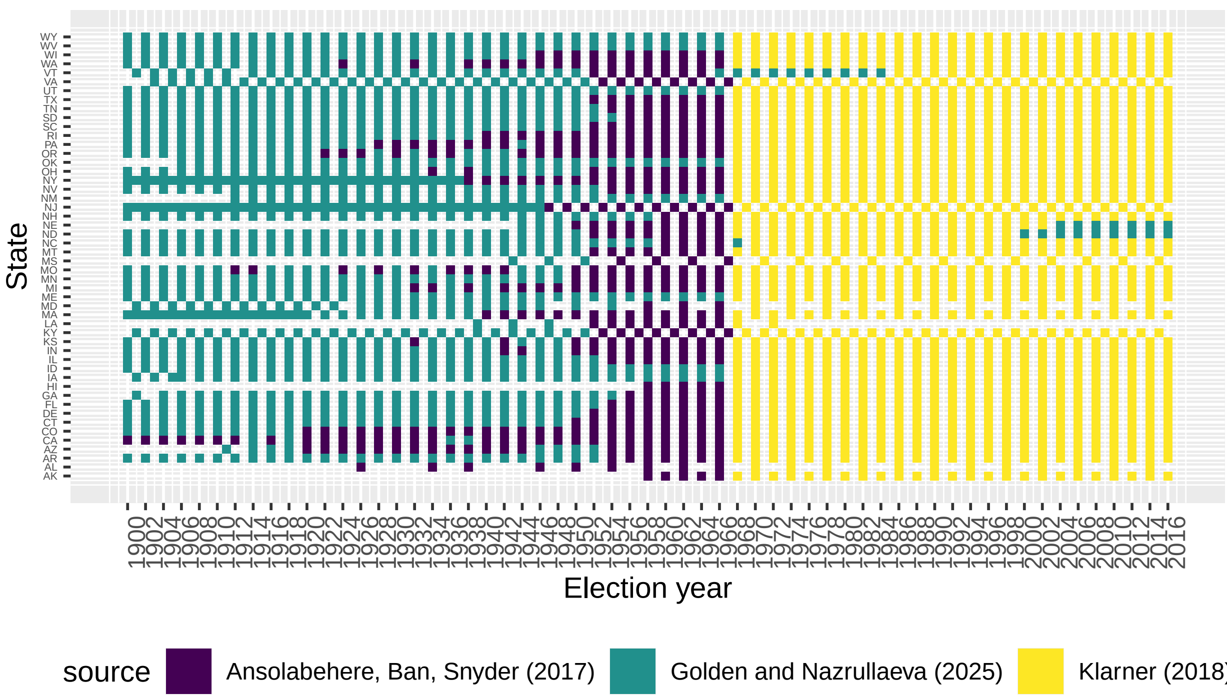
Table 1: Variables in the Dataset *(continued)*

Variable	Description
ocr_match_same_year	Measure to check if the representative's name could be found in the register of votes for the same election year t. Proxy for OCR quality and the accuracy of rerunning coding.
rerun_mismatch	Binary indicator for representatives who won in t+1 but were mistakenly classified as not rerunning.

4 General instructions

4.1 Selection criteria

Aiming to fill the gaps in Ansolabehere, Ban, and Snyder (2017) and Klarner (2018), we included all the legislatures in the period from 1900 to 2016 that were not included in either. The graph below shows which state-years are included in the current dataset and which by Ansolabehere, Ban, and Snyder (2017) or Klarner (2018).



4.2 States and legislative periods included

4.3 Sources

The sources for the data that we collected include legislative journals, legislative manuals, reports compiled by legislative research commissions, official legislature websites, and official state registers. Where information could not be acquired online, we collected data through direct communication with states' legislative assembly offices, archives, and libraries.

Table 2 lists the sources of our data by state and indicates whether the data were manually processed or automatically extracted into a tabular format suitable for analysis.

Table 2: Years Covered, Data Sources, and Data Extraction Techniques for All States

state	years	source_name	source_type	method
Arizona	1946-1952	Arizona Memory Project	Website	Manual
Arizona	1911-1918	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Arkansas	1901-1945	Historical Report Of The Secretary Of State, 2018	Scan	Automatic (R/Python)
Arkansas	1947-1953	Historical Report Of The Secretary Of State, 2018	Scan	Manual
California	1914, 1918, 1936, 1938	Journal Of The Assembly	Scan	Automatic (R/Python)
Colorado	1913-1917	Colorado Legilsators Past And Present	Website	Manual
Colorado	1901-1911, 1919	House Journal Of The General Assembly	Scan	Automatic (R/Python)
Connecticut	1898-1948	Connecticut General Assembly Members	Website	Automatic (R/Python)
Delaware	1901-1945	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Delaware	1947-1951	Journal Of The House Of Representatives	Scan	Manual
Florida	1898-1952	Membership Of The Florida House Of Representatives By County	Scan	Automatic (R/Python)
Georgia	1940-1954	Georgia's Official Register	Scan	Manual
Georgia	1899-1939	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Idaho	1890-1966	Idaho Blue Book	Scan	Automatic (R/Python)
Illinois	1903-1913, 1945-1953	Blue Book Of Illinois	Scan	Manual
Illinois	1901-1903, 1913-1945	Journal Of The House Of Representatives	Scan	Automatic (R/Python)

Indiana	1901-1941	House Journal	Scan	Automatic (R/Python)
Indiana	1947-1949	House Journal	Scan	Manual
Iowa	1837-1966	Historical Tables Of The Iowa Legislature	Website	Automatic (R/Python)
Kansas	1901-1911, 1935-1943	House Journal	Scan	Automatic (R/Python)
Kansas	1913-1931, 1943-1949	House Journal	Scan	Manual
Kentucky	1900-1951	Kentucky General Assembly Membership 1900-2005	Scan	Manual
Louisiana	1939, 1943, 1947	Membership In The Louisiana House Of Representatives 1812 - 2020	Scan	Manual
Maine	1819-1966	Maine State Law & Legislative Reference Library	Xlsx	Xlsx
Maryland	1901-1945	Archives Of Maryland, Historical List House Of Delegates, 1790-1990	Website	Automatic (R/Python)
Maryland	1947-1955	Journal Of The House Of Representatives	Website	Automatic (R/Python)
Massachusetts	1900-1951	Wikipedia, Xxxx Massachusetts Legislature	Website	Automatic (R/Python)
Michigan	1900-1930, 1936, 1940	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Minnesota	1934-1948	General Election Returns For Minnesota	Scan	Manual
Minnesota	1900-1932	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Mississippi	1943, 1947, 1951	Hand Book : Biographical Data Of Members Of Senate And House, Personnel Of Standing Committees	Scan	Manual
Missouri	1899-1935	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Missouri	1945-1949	Journal Of The House Of Representatives	Scan	Manual

Montana	1901-1943	House Journal Of The Legilsative Assembly	Scan	Automatic (R/Python)
Montana	1945-1951	House Journal Of The Legilsative Assembly	Scan	Manual
Nebraska	1944-1948, 2004-2016	Nebraska Blue Book	Scan	Manual
Nevada	1900-1952	Journal Of The Assembly	Scan	Manual
New Hampshire	1900-1927	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
New Hampshire	1929-1960	State Archives Office	Xlsx	Xlsx
New Jersey	1898-1946	Minutes Of Votes And Proceedings Of The General Assembly Of The State Of New Jersey	Scan	Automatic (R/Python)
New Mexico	1922-1966	New Mexico State Legislature Legislative Council Service	Native Pdf	Automatic (R/Python)
New York	1901-1938	Journal Of The Assembly	Scan	Automatic (R/Python)
North Carolina	1969	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
North Carolina	1900-1942	North Carolina Government, 1585-1974: A Narrative And Statistical History. Edited By John L. Cheney, Jr	Scan	Automatic (R/Python)
North Dakota	1900-1917, 1921-1937, 1941-1943, 1919, 1939	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
North Dakota	1945-1951	Journal Of The House Of Representatives	Scan	Manual
Ohio	1900-1910, 1925-1933, 1937-1945	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Ohio	1947-1951, 1912-1922	Journal Of The House Of Representatives	Scan	Manual
Oklahoma	1906-1966	Historic Members	Website	Automatic (R/Python)

Oregon	1900-1920, 1928, 1932, 1938-1942	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Pensylvannia	1901-1927, 1945	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Rhode Island	1901-1929	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Rhode Island	1930-1938	Journal Of The House Of Representatives	Scan	Manual
South Carolina	1901-1943	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Tennessee	1945	House Archives - Tn General Assembly	Scan	Manual
Tennessee	1898-1944, 1946-1952	House Archives - Tn General Assembly	Website	Automatic (R/Python)
Texas	1900-1950	Texas Legislators: Past & Present	Website	Manual
Utah	1895-1966	Utah State Legislature, Legislators By Year	Website	Manual
Vermont	1900-1943, 1967-1985	Journal Of The House Of Representatives	Scan	Automatic (R/Python)
Vermont	1945-1951	Journal Of The House Of Representatives	Scan	Manual
Virginia	1900-1935	A History Of The Virginia House Of Delegates	Website	Automatic (R/Python)
Virginia	1937-1951	A History Of The Virginia House Of Delegates	Website	Manual
Washington	1901-1937	House Journals Of The State Of Washington	Scan	Automatic (R/Python)
West Virginia	1900-1966	West Virginia Archives And History	Website	Automatic (R/Python)
Wisconsin	1916-1944	Journal Proceedings Of The Wisconsin Legislature	Scan	Automatic (R/Python)
Wisconsin	1900-1914	Wisconsin Blue Book	Scan	Manual
Wyoming	1890-1966	Wyoming Legislator Database	Website	Manual

5 Data collection procedures

5.1 General information

Data were collected between 2020 and 2023.

5.1.1 Digitizing archival records in R

In total, the data for 1309 legislative periods in 47 states were extracted from scanned (.pdf) journals of the lower house or other historical documents. Where the scan quality allowed, we digitized such documents using primarily the programming environment R and, to a lesser extent, Python. In such cases, the procedure was the following:

1) First, we identified and selected pages in the PDF documents that contained the lists of assembly members. This list was either in the form of a dedicated table or as a roll call of the representatives in the first legislative session. In cases of roll call lists, we sometimes missed legislators who were absent in the first session, but the prevalence of such cases was under 5%.

2) Second, we extracted text line by line from the specified pages and combined them in a data frame. If the PDF document did not contain a recognized text layer, or if the latter was incomplete or contained too many inaccuracies, the Tesseract OCR engine was used to digitize text from the images.

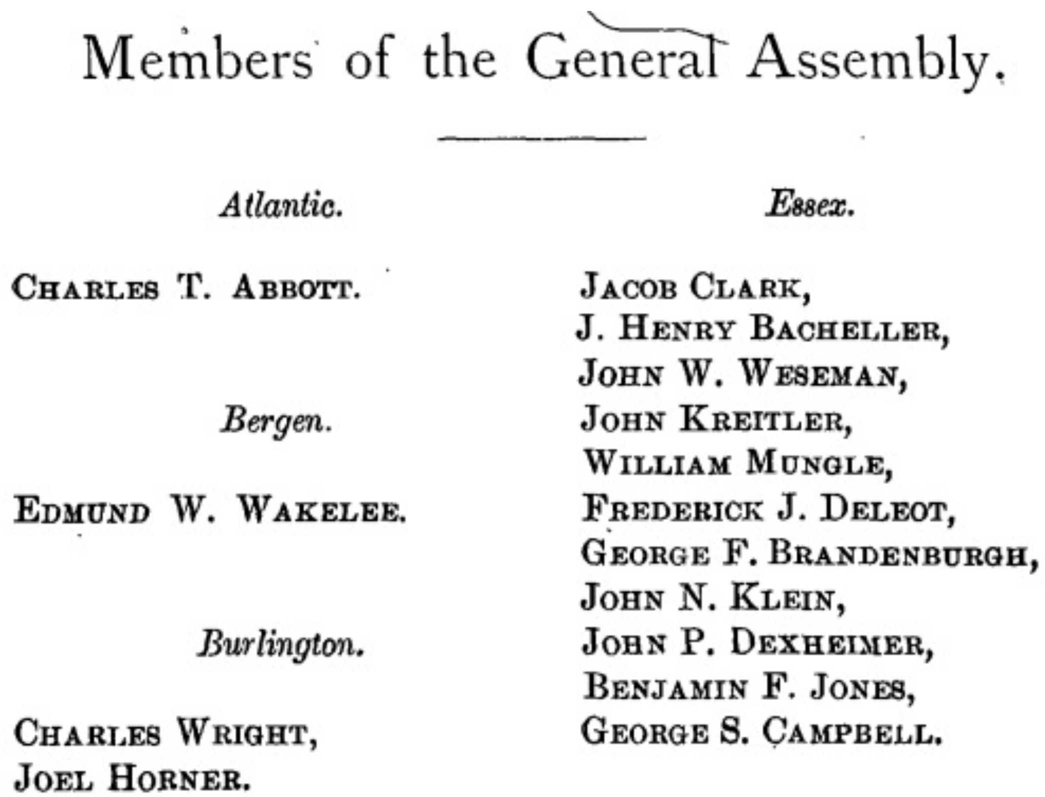
3) Third, we extracted legislators' names from rows containing raw text. The main task here was to identify which part of a string contained a name and what repeated pattern could be used to separate it from the rest of the string. Where possible, we sought to extract other legislator characteristics, such as party affiliation and/or county where elected.

4) Fourth, we cleaned and standardized names to correct misspellings and formatting inconsistencies. That is, we checked for consistency in names' format, ensuring first-middle-last name order is the same across states for all legislators. As we were dealing with scanned versions of documents without the native text layer, we also encountered occasional misspellings. To deal with them, we:

   a) Checked for atypical characters in legislators' names and manually corrected those. In particular, we checked for the presence of uncommon non-letter characters, two punctuation symbols in a row, two upper-case letters in a row, two letters that rarely go together (such combinations as kk, xx, yy, hh), a white space not followed by an upper-case letter or two lower-case letters separated by a white space etc. This method, however, did not allow to spot less obvious misspellings, such as "Pattereon" instead "Patterson", for example.
   b) To address those types of errors, we then compared legislators' last names to the list of the most common 151,000 last names in the US. If a legislator's last name was not present in the list, we first searched for a legislator elected in a different year with a surname that differed from that suspicious last name by no more than one character and was present in the list of the most common last names. If such

a name was found, we replaced our original suspicious name with it. If there was no such name, we manually checked the spelling, and, if necessary, corrected it. For example, suppose we have a legislator with the surname "Pattereon" elected in 1901. This surname is misspelled and therefore not present in the list of the most common last names. We then search for a similar last name among legislators elected at a year $\neq$ 1901 and, finding "Patterson" among them, replace the original last name with it.

As an example, below is an extract from the legislative journal of the state of New Jersey, a typical pdf document we worked with to extract the names of legislators.



Members of the General Assembly.

Atlantic.

CHARLES T. ABBOTT.

Bergen.

EDMUND W. WAKELEE.

Burlington.

CHARLES WRIGHT,
JOEL HORNER.

Essex.

JACOB CLARK,
J. HENRY BACHELLER,
JOHN W. WESEMAN,
JOHN KREITLER,
WILLIAM MUNGLE,
FREDERICK J. DELEOT,
GEORGE F. BRANDENBURGH,
JOHN N. KLEIN,
JOHN P. DEXHEIMER,
BENJAMIN F. JONES,
GEORGE S. CAMPBELL.

Table 3 demonstrates how this page is represented as a data frame in R (raw data, the left column). Then, after some data cleaning and manipulation — separating columns by a pattern, splitting rows with more than two names etc. — legislator names appear as in the right column.

As one can see, there is a spelling error in the name "George S. Campbell," whose middle initial was digitized as "8" instead of as "S" (a common issue due to the similarity between the two symbols). This was dealt with at the last stage of the data preparation process when spelling errors in legislators' names were manually corrected state-by-state.

5.1.2 General Issues with Coding Rerunning and Reeleciton

In general, to identify a legislator as reelected, we need to confirm that the individual was elected to the legislature at time t and at $t-1$. In practice, this means we are looking for

raw_line	cleaned_line
Members of the GeneraT Assembly,	Charles T. Abbott
	Jacob Clark
Atlantia. Essex.	J. Henry Bacheller
	John W. Weseman
Charles T. Abbott. Jacob Clark,	John Kreitler
J. Henry Bacheller,	William Mungle
John W. Weseman,	Edmund W. Wakelee
Bergen. John Kreitler,	Frederick J. Deleot
William Mungle,	George F. Brandenburgh
Edmund W. Wakelee. Frederick J. Deleot,	John N. Klein
George F. Brandenburgh	John P. Dexheimer
John N. Klein,	Benjamin F. Jones
Burlingtm. John P. Dexheimer,	Charles Wright
Benjamin F. Jones,	George 8. Campbell
Charles Wright, George 8. Campbell.	Joel Horner
Joel Horner.	NA

Table 3: PDF text read as raw lines and after cleaning

the same name in two consecutive legislative journals. Although this task is straightforward yet time-consuming for a human, it can be challenging for a computer for two reasons.

First, the same name can be written differently between two journals, or between the list of elected legislators in a journal and the register of voters which contains the names of all candidates who ran in an election. For example, the person can be recorded as "J. J. Smith" in the register of votes but as "James John Smith" in the corresponding legislative journal. To deal with this problem, we match legislators by their first name initials and surnames. This way, J. J. Smith and James John Smith are considered to be the same legislator. At the same time, it creates the possibility that James John Smith from the list of representatives will be matched to Jake John Smith. Although such inaccuracies are possible, their distribution should be random and should not bias the dataset.

Second, since we are mostly working with data extracted from pdf files without a native text layer, there are some spelling errors due to the poor quality of the scans. Because of this, James John Smith can be recorded in our data as Iames John Smith. To address this, we employ fuzzy matching, allowing names that differ by one character to be considered the same. This way, Iames John Smith will be matched to James John Smith. However, it also means that two different names might be inaccurately matched and a legislator wrongly coded as, for instance, reelected. We do not consider this an important problem, since these spelling errors should be minimal in number and distributed randomly.

5.1.3 Coding Reelection

We coded a legislator from state S as reelected from the legislature convened at time t if a person with the same name appears in our dataset among state S legislators at time $t-1$ (i. e. elected in the previous election). To identify reelected representatives, we use first name initial and surname, allowing one character to differ between the names at t and $t-1$ for a given legislator to be coded as reelected.

5.1.4 Coding Rerunning

We identify as rerunners at time t those representatives whose names were found in a list of candidates at time $t+1$. Again, we used initials and last names, allowing one character to be different. This time, however, instead of digitizing registers of votes (or other documents with the candidates' names) into a tidy dataset and then comparing it with the dataset of representatives, we worked directly with raw texts. The reason we do so is because official registers of votes are usually significantly more difficult to digitize and put in the tidy format: primary this is because there are several times more inputs to process in comparison to the list of elected representatives, and also because the layout of a document can be more complex, e.g. with both vertically and horizontally positioned text on the same page.

In general terms, the procedure consisted in checking if a given representative's name from a legislature at a time t could be found in the OCR text layer of a scan of an official register of votes (or other official document of a similar type) that contained the names of candidates who run in the election at time $t+1$. In other words, we took "James John Smith" from the list of representatives elected in 1900 and checked if a person with the same name appeared in the list of candidates in the 1902 election, repeating this procedure for all legislators from the state in a given year.

More specifically, the procedure was the following:

1) Read the register of votes as raw text, remove all non-letter characters, indentations and double white spaces.

2) For each representative from our dataset, take their initials and last name, and search for it in the text. Here we used regular expressions of the form **(J[a-z]{0,10} J[a-z]{0,10} Smith){e<=1}** to account for the fact that first and middle names can be written either as initials or in full format. In other words, J. J. Smith could be matched to James John Smith, James J. Smith, J. J. Smith, or J. J. Smlth.

This approach allows us to calculate rerunning rates relatively fast and without any need to digitize and tabulate registers of votes, but at the cost of not being able to work with low quality OCR text layers containing too many spelling errors. In such cases, the sensitivity of our results would be too low and the proportion of false negatives (representatives who rerun but are not identified as such) too high.

To distinguish between the state-years for which our approach is sensitive enough and those for which it is not, we use two measures. First, we calculate how many representatives from our list elected at time t we were able to find in the register of votes for the **same year**, which serves a proxy for the quality of the scanned data. For example, we took "James John Smith" from the list of representatives elected in 1900 and checked if the algorithm was able to find this name in the list of all candidates in the *1900* election, repeating this procedure for all state legislators in a given year. In an ideal case, for each state-year the number of representatives identified this way should be equal to the number of legislators we have on our list, since all elected representatives should be on the list of candidates. Whenever the number is smaller, it indicates underperformance of the procedure caused by low quality of the text layer. Figure 1 reports the results.

Second, we used reelection, which was coded using cleaner data and a more robust procedure, as an additional benchmark for the accuracy of the rerunning variable. Whoever is coded as reelected in the next election should, by definition, run in that election. Therefore, if a candidate is marked as reelected but not as running in the election, it indicates a likely inaccuracy in the latter coding. Figure 2 reports the results. We do not filter by these variables, but preserve the data as is. We suggest reasonable critical values for these two variables would be 0.8 and 0.05 respectively.

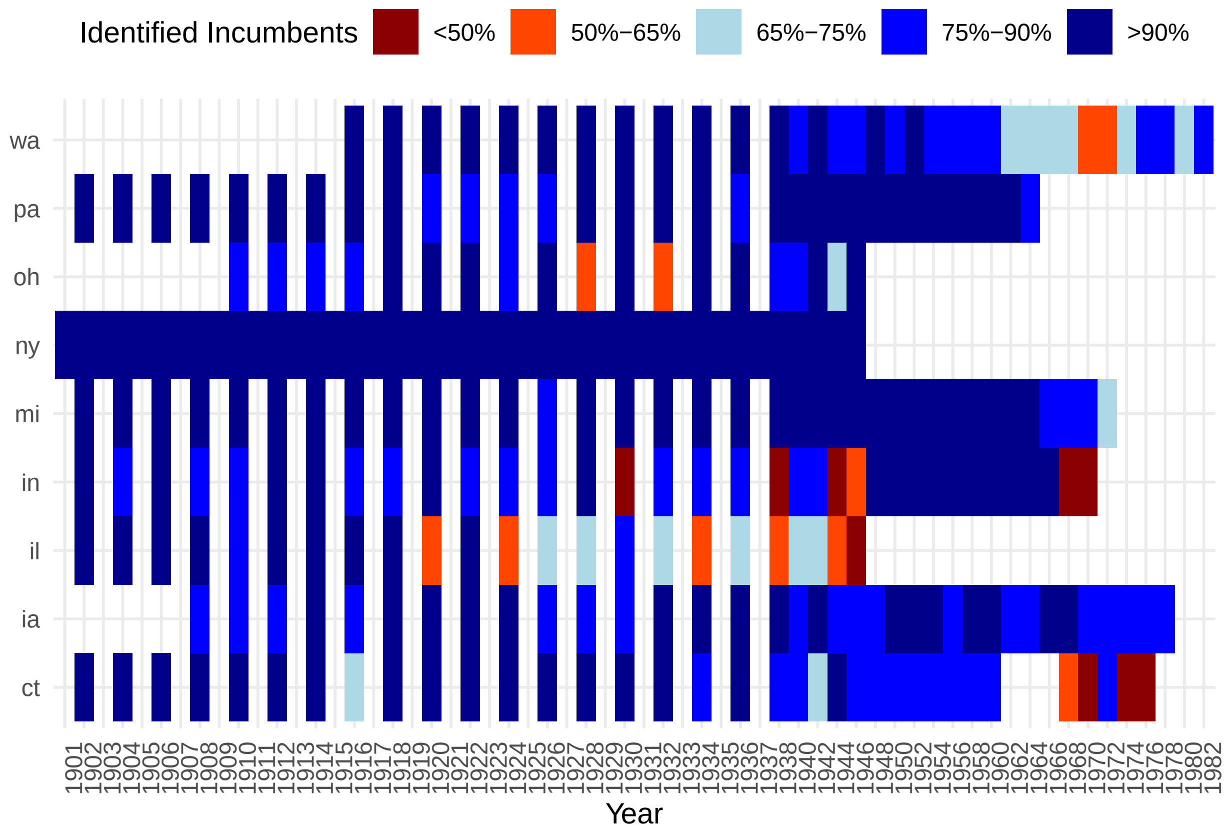


Figure 1: Percentage of Identified Legislators at Time t

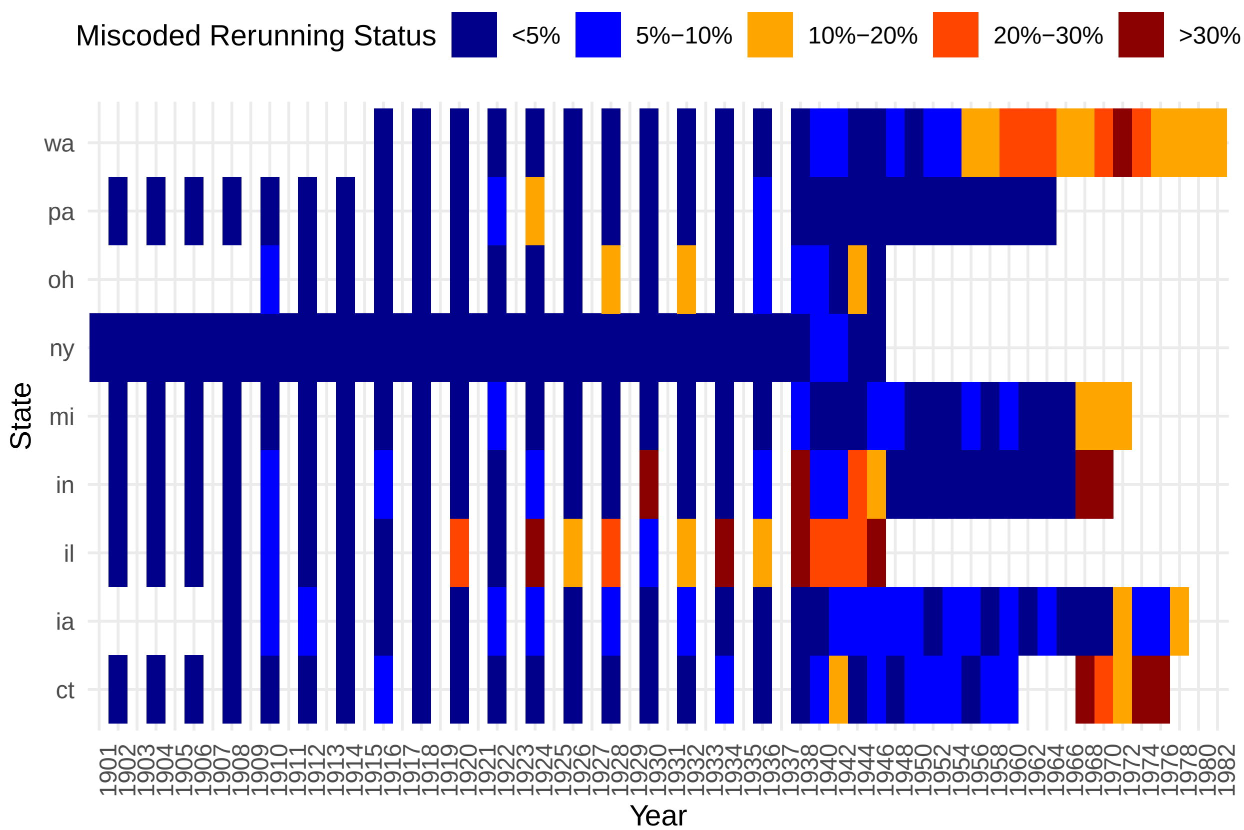


Figure 2: Miscoded Rerunners

Codebook References

Ansolabehere, Stephen, Pamela Ban, and James M. Snyder Jr. 2017. "State Legislative Historical Elections." 2017. https://doi.org/10.7910/DVN/LEMNXZ, Harvard Dataverse, V1, UNF:6:8UQYfDIsmII/tgD+Hrv/8Q== [fileUNF].

Klarner, Carl. 2018. "State Legislative Election Returns, 1967–2016." 2018. https://doi.org/10.7910/DVN/3WZFK9, Harvard Dataverse, V3, UNF:6:pV4h1CP/B8pHthjjQThTTw== [fileUNF].