Strengthening State Capacity: Civil Service Reform and Public Sector Performance during the Gilded Age*

Abhay Aneja
UC Berkeley

Guo Xu
UC Berkeley & NBER

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Abstract

We use newly digitized records from the Post Office to study the effects of strengthened state capacity between 1875-1901. Exploiting the implementation of the Pendleton Act – a landmark statute which shielded bureaucrats from political interference – across U.S. cities over two waves, we find that civil service reform reduced postal delivery errors and increased productivity. These improvements were most pronounced during election years when the reform dampened bureaucratic turnover. We provide suggestive evidence that reformed cities witnessed declining local partisan newspapers. Separating politics from administration, therefore, not only improved state effectiveness but also weakened the role of local politics.

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*Abhay Aneja: aneja@berkeley.edu (UC Berkeley, School of Law, 225 Bancroft Way, Berkeley, CA 94720.); Guo Xu: guo.xu@berkeley.edu (corresponding author, UC Berkeley, Haas School of Business, 2220 Piedmont Ave, Berkeley, CA 94720.). We thank participants at the NBER DAE meeting, NBER L&E Meeting, Harvard/MIT Development Seminar, Harvard Political Economy seminar, Berkeley Economic History Seminar, Berkeley Law Faculty Seminar, Berkeley GSPP Research Seminar, Duke Law Faculty Seminar, UVA Law & Economics Seminar, Columbia PE Seminar, Yaw Law LEO Seminar, UT Empirical IP Conference, Culp Colloquium, UC Merced, UPF, Sciences Po, PUC, USC, LSE, Northwestern Kellogg, Princeton, NYU Law, UW Law, and Stanford for their helpful suggestions. We are grateful to the UC Berkeley Institute for Research on Labor and Employment (IRLE) and the Hellman Foundation for generously providing financial assistance needed for data collection. We are grateful to Miao Ben Zhang and Steven Callander for their excellent discussions. We thank John Friedman for excellent research assistance.

1 Introduction

State capacity is a key driver of development and growth (Besley and Persson, 2009). Civil service reforms are at the core of efforts to strengthen state capacity in developing and developed countries alike (World Bank, 2000). While the exact nature of these reforms varies, a common objective is to create an impartial civil service (Besley et al., 2021; Evans and Rauch, 1999). This objective is rooted in a long history of intellectual thought that considers a non-partisan bureaucracy characterized by life-long careers as an essential precondition for a well-functioning state (Weber, 1922). Yet, despite the apparent importance of civil service reforms, evidence on whether and how such reforms affect state effectiveness remains relatively scarce.

In this paper, we show that a strengthened bureaucracy has first-order impacts on the quality of public service delivery. We provide empirical evidence from America's watershed civil service reform – the implementation and expansion of the landmark 1883 Pendleton Act within the U.S. Post Office.[1] Prior to the Pendleton Act, the U.S. federal government operated under the "spoils system" – a patronage-based structure that gave elected politicians and their respective political parties discretion over the hiring of bureaucrats. The spoils system allowed local parties to leverage federal appointments to amass substantial power – namely, through "the plunder of the salaries of the public servants for filling the party treasury... the use of the power of removal for compelling those servants to be party henchmen...and the use of the power of appointment for bribing and rewarding for party ends those not in the public service" (Eaton, 1885). As a result, corruption and powerful political machines dominated politics during the Gilded Age (1870–1900).

The Pendleton Act sought to reduce the influence of politics in administration and to reign in the power of political parties over the staffing and functioning of government. The statute replaced discretionary appointments with rule-based hiring, shielded bureaucrats from political interference, and abolished mandatory political contributions from bureaucrats (Hoogenboom, 1959; Johnson and Libecap, 1994). This seminal reform thus triggered a gradual transition of the federal administrative state towards a modern public organization that insulated bureaucrats from political forces (Theriault, 2003) – a move towards personnel practices commonly equated with "good" governance today (Evans and Rauch, 1999, 2000). Despite the importance of this milestone reform, though, there is limited evidence for its effectiveness in improving bureaucratic performance and the quality of service delivery.

The public organization we examine – the Post Office – provides an ideal setting to study the impacts of civil service reform. First, in terms of personnel, the Post Office was – and remains today – the largest civilian agency within the federal government. Second, the Post Office was the principal method of long-distance communication in an expanding nation during the 19th century, thereby underscoring its economic importance. By providing the infrastructure needed to transmit personal correspondence and business information, the Post

[1] The Post Office was established in 1792, with President George Washington's signing of the Postal Service Act. Today, the Post Office is known as the United States Postal Service or USPS.

Office contributed to rapid economic growth during that time period (John, 1995; Rogowski et al., 2021). Crucially for the purpose of being able to assess the impact of a nationwide reform, local post offices are present in virtually all parts of the country and perform the same functions everywhere – which is the collection and delivery of mail. This government service is carried out via a staff composed of carriers and clerks. The decentralized structure of the Post Office Department allows us to observe comparable measures of performance (e.g., delivery errors) and inputs (e.g., number of workers, personnel characteristics) across time and space. We combine these measures with the expansion of reform coverage to nearly 600 cities to identify the effects of the reform and the channels through which they operated.

Underpinning our study is a large-scale digitization of historical performance and personnel records of the U.S. Post Office. We begin by using the Annual Reports of the United States Civil Service Commission to trace out the gradual expansion of the reform across nearly 600 cities.[2] We then link the timing of the civil service reform to annual data on service quality and operations from two sources: the Annual Reports of the Postmaster General and the Annual Reports of the Superintendent of the Railway Mail Service. We examine the effects of two major civil service reform waves (1883 and 1893), which allows us to examine how this policy affected post office delivery errors across 1,763 cities from 1879-1901. We also examine detailed measures of cost efficiency and per-unit productivity. To study how federal civil service reform affected the size and composition of the postal workforce, we also digitized individual-level data on the universe of clerks and carriers from the Official Registers of the United States (the "Official Registers" or "Registers") for the period 1877–1901. This source contains rich data on the assignment, career progression, and background characteristics of postal workers. Finally, we use newspaper data to study how a depoliticized, more effective Post Office affected partisan politics and the strength of local parties – as measured by the circulation and presence of partisan newspapers.

One challenge to measuring the causal effect of civil service reforms is that such reforms are not randomly assigned, but reflect deliberate policy choices (Ujhelyi, 2014a). This raises the concern that the local adoption of civil service reform is correlated with city-specific factors that also affect postal performance. One advantage of our setting is that the reform's rollout pattern was decided at the federal level. The reform initially covered larger cities before expanding to smaller urban areas. While the difference-in-differences approach helps address time-invariant sources of endogeneity, we also leverage institutional knowledge of the assignment rule. The first wave of the Pendleton Act of 1883 applied civil service reforms to cities with more than 50 postal workers. The later expansion via Presidential order in 1893 was similarly size-based, depending on city population. These features allow us to augment our standard difference-in-differences by adopting a "control function" approach where we explicitly control for the size-based assignment variable in a time-varying fashion.

Our main finding is that civil service reform improved the quality of public service delivery. When relating postal delivery errors to the staggered expansion of the civil service reform, we find a significant and marked

[2] We use the term cities interchangeably with the term post office. Post office subsidiaries within a city are referred to as branches.

decrease in the number of delivery errors by 19–22%, consistent with an increase in the reliability of the postal service. This result holds when explicitly controlling for the assignment rule (based on post office staff size) and when using a large number of flexible, time-interacted controls to account for observable baseline differences between reformed and unreformed cities. Breaking down the reform effects by each of the two major reform waves of 1883 and 1893, we observe similar dynamic patterns with no pre-trends, providing evidence for both the external and internal validity of our estimates. We also show that the reform effects are not driven by any particular city or state and explicitly rule out confounders such as the concurrent improvement of complementary communications infrastructure, or complementary local reforms. Finally, we corroborate our main finding based on delivery error data using a second set of productivity measures collected from an independent data source. Leveraging rich statistics from the free delivery service available for cities affected by the initial rollout of civil service protections under the 1883 Pendleton Act, we find that the civil service reform also led to significant productivity gains, as measured by the overall volume per carrier or the cost per volume.

Having documented substantial improvements in public service delivery driven by federal civil service reform, we leverage our granular data on inputs to study the underlying mechanisms. We first leverage the personnel records of clerks and carriers from the U.S. Official Registers to rule out that the civil service reform increased the staff numbers in reformed cities. Turning next to an analysis of the selection margin, we use both individual traits reported in the Official Registers and additional background characteristics obtained by linking these personnel records to the complete count Decennial Censuses ("Census") to show that the reform did not significantly change the selection of personnel. We then use data on the overall mail volume to reject the possibility that the improved performance arose due to lower workload.

We end our discussion of mechanisms underlying the improved post office performance by conducting heterogeneity tests to shed light on the drivers of the observed performance gain. Under the spoils system, insecurity of tenure was a natural consequence of tying the fortunes of bureaucrats to elected politicians who often turned over (Hoogenboom, 1959; Johnson and Libecap, 1994). Using data on the careers of postal employees, we uncover substantial political cycles in exit among rank-and-file workers coinciding with presidential election years. These political cycles in turnover are dampened after the civil service reform, resulting in significant increases in the bureaucrats' average duration of tenure. Using individual-level data, we find that the positive effects on retention apply to all bureaucrats serving in a reformed office, irrespective of their time of hire. Importantly, we find that the reduction in delivery errors and productivity improvements are largest during election years. Rather than improving the quality of service delivery by selecting higher-performing workers, civil service reform mainly improved bureaucrat performance by severing the ties between politicians and bureaucrats.

Finally, we consider whether civil service reform loosened the grip of partisan political actors at the local level. A major justification for reform of the civil service was to reduce the influence that local political parties had over the hiring of government employees (Mashaw, 2009). Before the passage of the Pendleton Act, local

political parties influenced the allocation of patronage posts, and in turn relied on partisan loyalists appointed to government positions for both political and financial support. By reducing the availability of federal jobs that could be used to secure political support, civil service reform likely reduced the power of local parties. To test this hypothesis, we exploit the fact that newspapers during the Gilded Age were often partisan and backed by local political parties. This allows us to use the presence of explicitly partisan newspapers as a proxy for the strength of partisan politics. Indeed, the expansion of the Pendleton Act coincided with a marked increase in non-partisan newspapers in the U.S. (Gentzkow et al., 2015), raising the question whether part of these stylized patterns can be attributed to the civil service reform. Using data on newspaper presence and circulation, we show that the civil service reform led to a decrease in partisan newspapers and an increase in the circulation of independent newspapers, consistent with a weakened role of local patronage politics. While suggestive, these findings underline the "double-dividend" of strengthened state capacity: civil service reform did not only result in a more effective bureaucracy but likely also helped reshape local politics in the U.S.

1.1 Related work

Our findings contribute to several strands of literature. First, our work contributes to the literature on the impacts of state capacity (Besley and Persson, 2009, 2010). While a larger body of work has documented long-run effects of state capacity across countries and regions (Dincecco and Katz, 2016; Besley et al., 2021; Dell et al., 2018), there is relatively less work on impact of efforts to weaken entrenched political actors, improve governance, and reduce patronage through civil service reform. On the subject of the effects of civil service reforms on policy, Ornaghi (2019) and Ujhelyi (2014b) show that civil service reforms can affect policy outcomes (using city and state reforms in the U.S.). Bostashvili and Ujhelyi (2019) show that state-level civil service reforms have the effect of stabilizing state government spending by weakening the political budget cycle. Finally, on the political implications of civil service reforms, Folke et al. (2011) find that state adoption of merit-based civil service systems weakened incumbent parties in the U.S. (as measured by state legislative wins). What differentiates our study from prior work is that we show the delinking of politics from public administration, effects on personnel, and effects on public service quality in a unified analysis. Moreover, the context – the first nationwide civil service reform in the U.S. – is an important setting.

Since policies that shape state capacity are often implemented across entire administrations or administrative units, a major challenge is the lack of counterfactuals. Existing work has relied on cross-country comparisons (Evans and Rauch, 1999; Cornell et al., 2020), as well as comparisons across a limited number of units, such as states or districts (Mehmood, 2022; Liu et al., 2022; Moreira and Perez, 2022a; Mocanu, 2022). The challenges arising from limited identifying variation are further exacerbated by the fact that bureaucratic performance is often difficult to measure, with many studies thus resorting to studying the reduced form effects on downstream economic outcomes (Rauch, 1995). Closest to our work is research by Moreira and Perez (2022a), who relate the

rollout of the Pendleton Act in the Treasury's Customs Service across eleven districts to revenue collection. Our study complements this work by focusing on the Post Office, which has long been one of the largest agencies of the federal government. The nationwide reach of the Post Office allows us to estimate the federal reform's effects "at scale" (Muralidharan and Niehaus, 2017) by leveraging the expansion of civil service reform from 23 to nearly 600 cities. Moreover, we can use a measure of public service delivery quality – delivery errors – that is readily comparable across affected and unaffected cities.

Second, we contribute to the literature on the personnel economics of the state. There is now a large body of work that studies how policies aimed at introducing merit-based personnel practices affect performance (Dal Bó et al., 2013; Finan et al., 2015). Much of this work has shown the effectiveness of incentives (Khan et al., 2015, 2019; Deserranno et al., 2021), demonstrated how opaque links between effort and reward weaken performance (Xu, 2018; Bertrand et al., 2019; de Janvry et al., 2023), or focused on the role of bureaucratic selection (Moreira and Perez, 2022a; Mocanu, 2022; Weaver, 2021; Voth and Xu, 2022; Dahis et al., 2023; Mehmood, 2022; Riaño, 2022). By leveraging both city-level and individual-level data, we connect the "macro" literature on civil service reform to this fast-growing micro-level literature. In contrast to Moreira and Perez (2022a) who focus primarily on the introduction of competitive examinations as the key margin for affecting bureaucrat performance, our result suggests that the Pendleton Act's measurable influence on performance worked through creating a civil service that is shielded from political interference. By providing evidence that the civil service reform improved performance by creating stable careers through the insulation of bureaucrats from electoral cycles, our results resonate with the literature on the costs of political turnover (Iyer and Mani, 2012; Akhtari et al., 2022; Colonnelli et al., 2020).

Finally, we contribute to research on American economic history in a few key ways. Most directly, we advance the literature on the evolution of the American government during an important period of political and economic development. Existing studies primarily assess the political economy aspects of state and city-level civil service reforms at the turn of the 19th century (Folke et al., 2011), as well as the impact of such changes on policy and performance outcomes (Ujhelyi, 2014b; Ornaghi, 2019). We build on this literature, as well as on the work of Moreira and Perez (2022a,b), by measuring the impact of the nation's signature set of *federal* civil service reforms on performance within the Post Office. We also link our findings of improved bureaucratic performance to measures of economic performance in the spirit of Evans and Rauch (1999), thus suggesting that state investments in depoliticized and efficient public service delivery have downstream consequences. By showing that increased newspaper circulation is driven by non-partisan rather than partisan sources, we contribute to research on the source of declining media partisanship at the end of the 19th century (Gentzkow et al., 2006; Petrova, 2011; Hirano and Snyder Jr, 2020). Lastly, we contribute to research on the role and structure of the Post Office in American economic history (Boustan and Margo, 2009; Acemoglu et al., 2016).

2 Historical context

U.S. Post Office. Post offices have been a key ingredient for state building in several countries (Rogowski et al., 2021; Geloso and Makovi, 2020; Andrabia, 2020; Chong et al., 2014; Maclachlan and Muse, 2011). The U.S. Post Office was created by the Post Office Act of 1792, and has been viewed by some as a key institutional driver of growth and social progress. During its early years, the Post Office facilitated communication and the spread of information between distant areas of the nation, and contributed to the development of an informed citizenry (John, 1995). Indeed, Alexis de Tocqueville described the American postal system as a "great link between minds," and admired how mail traveled from eastern cities like New York and Philadelphia to (then) sparsely-populated areas like Detroit located on America's western frontier (Tocqueville, 1969). The expansion of mail service also facilitated economic transactions across greater distances, allowing sellers of goods to advertise their products using catalogs and to complete sales via mail orders. Roper (1917), for instance, describes how money and goods "pass[ed] through the countless postal channels." Relatedly, the Post Office provided a system of banking and allowed investors to find opportunities. Business-related mail comprised a substantial portion of total correspondence in the 19th century, leading some to conclude that America's "commitment to postal service formed... the foundation for [nationwide] growth" (Henkin, 2007).

One can see the potential for the Post Office to transform the nation economically, politically, and socially by observing its reach after just a century of existence. Figure 1, Panel (a) shows the locations of all post offices that were open between 1860–1905 (Blevins, 2021), and highlights the ubiquity of this aspect of U.S. state capacity. The presence of post offices in all corners of the nation underscores the widely-held view that in the 19th century, "the postal system was *the* central government" (John, 1995) (emphasis added). Panel (b) highlights the subset of cities whose post offices are ultimately affected by civil service reform.

The Patronage era of the American government. The U.S. government grew substantially during the 19th century in response to factors such as westward expansion, commercial development, and the demands of the Civil War. The Post Office was the nation's largest federal agency – "the mighty arm of civil government," according to *The New York Times*. At the onset of the Pendleton Act, postal workers alone made up 34% of the entire federal workforce, larger in terms of labor force than any other department (US Census, 1880). Moreover, given the importance of the Post Office to economic activity, the personnel responsible for this central communications infrastructure had the potential to shape American growth.

Despite its role in facilitating communication and economic activity, the early Post Office was rife with corruption and inefficiencies (Carpenter, 2001). One likely source of organizational ineffectiveness was the "spoils system," a system of patronage-based hiring and firing that pervaded much of the federal bureaucracy. For much of the 19th century, many federal employees were hired at least partially based on political considerations (Johnson and Libecap, 1994). Politically-motivated personnel decisions were commonplace in many post of-

fices, which hired large numbers of staff. Under the spoils system, patronage employees were expected to be politically active on behalf of their benefactors by assisting in political campaign activities and contributing part of their salaries to politicians in the form of "political assessments." Because bureaucrats were tied to particular politicians or parties, high levels of bureaucratic turnover were also commonplace during this period (Fowler, 1943). In New York City, for example, the dominance of partisan considerations led to "short terms of office and easy" as well as "frequent removals" (Eaton, 1910).

The spoils system, which included high levels of politically-motivated employee turnover, imposed significant costs in terms of both the quality and efficiency of service provision. Bureaucrat ineptitude caused in part by a corrupt patronage system hampered the quality of postal service. Johnson and Libecap (1994), for instance, describe the anger of political constituents about the impact of patronage on public services during the years preceding federal reform: "[s]candals and charges of fraud and inefficiency were linked to the country's largest post offices and customhouses. Patronage was blamed, and the president and members of Congress with major federal facilities in their districts were increasingly pressured to reform the system" (p. 20). Newspapers, which comprised a substantial fraction of mail volume, complained of the "long-enduring irregularity of the mails, and the excessive carelessness or gross ignorance...[within] a good many of the post offices" (Foley, 1997).

Problems were particularly acute in cities, where patronage politics were salient. In the New York City Post Office, for instance, one report described the "incompetency, neglect, confusion and drunkenness" of postal staff. Indeed, one incoming postmaster prior to the early 1880s stumbled upon many bags of undelivered mail that were scattered throughout the post office building (Hoogenboom, 1959). By the mid-1870s, even members of Congress acknowledged that the quality of government service provision was often "poor at best," and that the administrative state required reform (Johnson and Libecap, 1994).

The Pendleton Act. In 1883, Congress passed the Civil Service Reform Act – better known as the Pendleton Act – which aimed to limit the influence of politics over public administration of the laws through a bundle of measures. The law created a merit-based civil service by replacing hiring based on discretion with rule-based personnel decisions relying on competitive exams and performance. The Pendleton Act also provided workers with protection from political removal, as they were vulnerable to being fired summarily during periods of political turnover (Masur, 2013). Finally, it outlawed for all federal workers the use of "assessments," fees that civil servants were asked to pay in return for their appointment.[3]

The passing of the Act was a culmination of pro-reform sentiment that had been developing since the end of the Civil War. Federal legislation was ultimately catalyzed by the assassination of President Garfield by a disappointed office-seeker in 1881 (Theriault, 2003). Under the initial 1883 statute, the government immediately

[3]The ban on assessment fees applied evenly across the entire government when the Pendleton Act was implemented. The effects we document using our difference-in-differences design, therefore, occur in addition to any effect caused by the ban on assessment fees, which we are unable to separately identify in this setting.

"classified" around 10 percent of federal employees as formal members of the protected civil service (Johnson and Libecap, 1994). Civil service protections applied initially to the federal workforce in D.C., as well as to large customs houses and post offices across the country. Among these departments, the Post Office was by far the largest affected department in terms of affected manpower, making up a third of the total federal workforce.[4] The statute was designed such that the Post Office was among the first departments to be affected by civil service reform. The targeting of the Post Office was significant, since the provision of postal services was an important obligation of the federal government in the 19th century (Johnson and Libecap, 1994).

Candidates for classified jobs were selected through open, competitive examinations and protected from arbitrary firing. During the first two decades of the reform era, postal employees were classified as protected civil servants based on the city in which they worked (Hoogenboom, 1959). Classified post offices were initially restricted to post offices with at least 50 employees – meaning that most of the positions classified were located in urban post offices.[5] According to the Civil Service Commission, this threshold choice was guided by both an attempt to experiment and target large post offices where the negative consequences of patronage were deemed more serious (United States Civil Service Commission, 1883). Over subsequent years, though, additional local post offices became "classified" for purposes of civil service protections. By 1921, the proportion of the federal civilian workforce covered by the civil service system had grown to 80%. Importantly, the law also delegated to the President the authority to issue executive orders moving additional jobs from patronage into the new civil service system. In 1893, President William Henry Harrison issued an executive order that classified all post offices with free delivery service as civil service-protected (United States Civil Service Commission, 1893).[6] Harrison's order extended civil service reform to 556 additional cities, expanding the civil service reform well beyond the biggest 23 large cities that were covered in the initial 1883 reform wave.

3 Data and descriptive statistics

3.1 Data sources

We combine multiple sources of administrative and personnel data to construct our main dataset. Here, we briefly describe the main sources of the data.

Government performance measures. Our performance measures come from two sources of administrative data that were compiled regularly by the U.S. Post Office during the 19th century: the Annual Report of the General Superintendent of the Railway Mail Service (Railway Mail Service, 1883) and the Annual Report of

[4] In comparison, the customs office – the second unit covered by the Pendleton Act – made up 3% of the federal workforce (estimate based on Census and Official Registers data for 1880 and 1881, respectively).

[5] Unlike the customs (c.f. Moreira and Perez (2022a)), there were no exemptions and the classification applied to all clerks and carriers.

[6] We describe free postal delivery service in more detail in section 3. Under the original statute, the President was authorized to extend the provisions of the Pendleton Act to other post offices, and also to other branches of the service (Lyman, 1893).

the First Assistant Postmaster General (United States Post Office Department, 1883). Our main measure of post office performance captures errors in delivery, and was recorded in a series of annual Post Office statistics, the *Statement of errors in the distribution and forwarding of mail*. These statistics were systematically recorded in the Annual Report of the General Superintendent of the Railway Mail Service, and were frequently used to benchmark the performance of post offices.

The Railway Mail Service (RMS) was primarily responsible for intercity mail delivery during the nineteenth and first half of the twentieth century. The delivery of mail across cities relied on the accurate collection and routing of mail by clerks working in local post offices. Postal clerks would collect and group together all articles of mail (letters, newspapers, etc.) that were intended for a particular destination. To facilitate sorting and transport by the RMS, the *Postal Laws and Regulation* required clerks to place a label, called a "facing slip" on top of a packet of mail items that indicated the destination and the name of the processing clerk in the origin city (United States Post Office, 1879). These slips not only facilitated a more efficient distribution process, but also created a record of all errors or irregularities made by postal clerks in sending cities.

Mistakes were identified by clerks of the RMS who, in the process of sorting mail for intercity distribution, examined all iterms within a bundle for errors in where mail was directed. Recorded errors were then double-checked by another expert examiner (White, 1910). Records of mistakes by either sending clerks or RMS clerks were then submitted to supervisory postal employees to track employee performance. These errors recorded on facing slips thus became a way city post offices were monitored.

The historical statistics report two measures of delivery errors at the city-year level: an extensive margin measure capturing the number of slips that contained errors, as well as a measure that captures both the extensive and intensive margin – the total number of errors on returned slips. We focus on the latter measure, the total number of errors on returned slips – which we subsequently refer to as "delivery errors." This measure allows us to capture variation at both the extensive and intensive margin, providing an accurate representation of the total number of mistakes made by origin post offices.[7] Finally, a key advantage of this performance measure is that the RMS was an independent arm of the U.S. Post Office. As such, it was not associated with a particular post office or subject to the city-specific rollout of the civil service reform, alleviating concerns over strategic misreporting. A natural limitation of this measure is that it only captures errors in the delivery of mail sent across different cities, which is likely to understate the total number of delivery errors. We collect the annual statements for the period 1879–1901, covering a total of 2,319 cities in our sample.[8]

The second key source of information on Post Office performance that we use comes from another administrative source, the Annual Report of the First Assistant Postmaster General. The First Assistant Postmaster General

[7] The correlation coefficient between both measures of errors – the number of incorrect slips returned and the total number of errors on returned slips – is 0.97. Our results hold using both measures from the General Superintendent's Annual Reports.

[8] Unfortunately, we were unable to find annual statements going back earlier than 1879. This limits the amount of pre-period we can examine for the 1883 reform.

(FAPG) was the deputy responsible for overseeing the Post Office's City Free Delivery service, a departmental innovation that allowed letter carriers to deliver mail to a recipient's home within certain cities. The FAPG's Annual Report provided a statistical appendix relating to the operation and performance of the Post Office's free delivery service. These data provide a unique portrait of the local free delivery operation, allowing us to measure the number of carriers in service, the amount of mail delivered and collected, the number of pieces handled, the cost of service, and the total amount of postage each year. To our knowledge, there has been no systematic examination of the postal service using these administrative data.[9] We use these secondary measures to complement our main performance measure and explore the mechanisms through which impacts (if any) occur. A limitation is that these series are only reported up to 1891, and we digitize the annual statements for the period 1875–1891. This covers a total of 512 large cities with free delivery service. Figure 1, Panel (a) summarizes our data availability. As the figure shows, we cover all urban areas across the nation.

Personnel records. To document how the postal reform affects the composition of the personnel and their career progression, we digitized the series of the Official Registers of the United States, Part II ("Official Registers" or "Registers"). Issued biennially, the Official Registers listed every employee of the U.S. federal government for the time period 1816–1921, and high-ranking federal officials thereafter. Given the large size of the U.S. Post Office in terms of workforce, the personnel listing of the Post Office was published separately as Part II of the publication series. We digitized personnel data on all clerks and letter carriers for the time period 1877–1901, covering our main sample period. Clerks and letter carriers make up the personnel of each city's post office and are thus the main occupations that were targeted by the postal reform. These are also occupations for which the Registers record detailed information about the names, birth states, work locations, and salaries. The availability of birth state and work locations, in particular, is crucial to allow us to link our data to the full count Decennial Census to obtain additional background characteristics. Overall, we digitized a total of 3,108 pages, corresponding to a total of 297,932 individual-year observations.

Data on newspaper partisanship. To measure the impact of civil service reform on local outcomes, we examine newspaper partisanship and circulation. This choice is guided by the historical literature, which documents a significant rise in independent newspapers during our study period (Gentzkow et al., 2006; Petrova, 2011; Hirano and Snyder Jr, 2020). We use data from Gentzkow et al. (2011) to obtain the number of partisan and independent newspapers with their circulation at the city-level for each presidential election. The underlying data comes from digitized historical newspaper directories (*Rowell's & Ayer's American Newspaper Directories*) that were used by advertisers. Our final sample covers 1,037 cities for all election years between 1879–1900.

[9] Carpenter (2000) uses data on *rural* free delivery from three states to examine aggregate trends in the cost of postal service during 1890–1915. Our study studies postal performance across the entire nation from 1875–1901.

3.2 Empirical patterns of civil service reform rollout

Reform rollout. The Pendleton Act was designed to shield civil servants from political interference. Under the statute, employees in classified positions would be selected through open, competitive examinations, and would also be protected from firing without cause (United States Civil Service Commission, 1883). As described above, the original statute affected postal jobs in a subset of cities, and these protections were subsequently rolled out to other places. To determine the timing of federal civil service rules across cities, we use the Annual Reports of the United States Civil Service Commission. First issued in 1883, these annual volumes document the progress of civil service reform throughout the nation. They report information on the coverage of civil service protections across locations, positions, and departments, as well as statistics on the number of examinations, the number of appointments, and aggregate statistics on candidates selected for federal jobs. Importantly for our purposes, these reports include lists of "classified" locations where formal civil service rules applied, thus allowing us to track the expansion of civil service reform within the Post Office Department over time. We use this information to create a city-year panel of civil service coverage within American post offices.

We begin our analysis by examining the determinants of civil service reform across the nation. Figure 1, Panel (b) displays the rollout of the reform during our study period. The rollout of the civil service reform was uneven across space but touched most parts of the nation. Reform occurred in two primary waves: the initial wave of the 1883 statute brought reform to 23 large cities, and the wave of 1893 which saw an expansion of civil service coverage to all then-unclassified 556 post offices that offered free delivery services. During the period between these two expansion waves, 30 cities became classified as they crossed the 50 employees threshold of the 1883 Act. In our main analysis, we focus exclusively on the 1883 and 1893 reform waves. Unlike cities that are gradually reformed as they grow past the designated 50-employee threshold between 1884-1892, assignment to treatment for the two major waves left less room for endogenous anticipatory responses.[10]

To formally assess the differential pattern of the rollout, Table 1 compares the characteristics of cities targeted by the reform to those not targeted. Panel A reports city-level baseline characteristics, and Panel B reports county-level characteristics. Given the size-based rollout of the Pendleton Act, reformed cities differ from unreformed ones in a variety of ways. For example, cities with post offices that became civil service-classified earlier tended to have more postal employees and a higher number of delivery errors. Reformed cities also have older post offices and were located closer to Washington, DC. These cities tended to be located in counties with a higher population, greater rates of urbanization, and a larger number of manufacturing establishments. Over time as the civil service reform expands, these differences become smaller but remain sizable.

[10] In later robustness checks, we also show that our results are comparable when including all reform waves

4 Civil service reform and government performance

4.1 Empirical strategy

Our objective is to examine how the implementation of federal civil service reform affected post office performance. Using the data introduced in section 3, we answer this question using a "stacked" event study design. This research design considers each reform wave (e.g., the initial 1883 statute, or the 1893 expansion via executive order) as a separate sub-experiment around which we construct a difference-in-differences estimate using cities affected and unaffected in that year. We then "stack" all individual event-specific difference-in-differences to estimate the pooled effect of the civil service reform across all waves.[11] As such, let $j = \{1883, 1893\}$ denote the reform wave, and let k be the years before or after civil service protections come into effect. Since k is centered around the date each reform wave takes effect, negative values are years leading up to a civil service reform event, and $k = 0$ denotes the year of the reform. We restrict the pooled sample to the window for which we have coverage in all sub-experiments. For delivery errors, for example, the window covers $k = \{-4, ...8\}$. For city i, reform year j, and the k-th year around the reform, we estimate:

$$y_{ijk} = \beta \text{treat}_{ij} \times \text{post}_k + \theta_{ij} + \tau_{jk} + \gamma' x_{ijk} + \varepsilon_{ijk} \tag{1}$$

where $\text{treat}_{ij} = 1$ if city i is reformed in the reform wave j, and 0 otherwise. The variable y_{ijk} is the outcome of interest, such as the (log) delivery errors. The variable post_k is defined as $\text{post}_k = \mathbf{1}[k \geq 0]$, taking the value 1 post-reform, and 0 before. τ_{jk} are reform-specific year fixed effects, which absorb common temporal shocks and reform effects that applied universally (e.g., the prohibition of assessments). Since cities can serve both in the treatment and control groups (e.g., Alameda, CA, is untreated until 1893, serving as a comparison city for cities reformed in 1883), we estimate the city fixed effects θ_{ij} separately for each reform wave.[12] The parameter β is the key estimate of interest, capturing the impact of experiencing the civil service reform relative to control cities that do not change their reform status in reform year j. For causal inference, we require that reformed and unreformed cities evolve along common trends in the absence of the reform.

The main identification concern is that reformed cities are not randomly assigned – as shown in Table 1, reformed cities differ from unreformed ones in a variety of ways. Although the inclusion of city fixed effects helps alleviate concerns over level differences, differences across reformed and unreformed cities could still affect the outcome of interest in a time-varying way. For example, if larger cities are both more likely to be reformed and experience improvements in complementary infrastructure (e.g., the telegraph), a simple difference-in-differences will mistakenly attribute effects of such infrastructure improvements to the reform.

[11] In contrast to a traditional two-way fixed effects, this research design makes explicit the comparison groups in each period, avoiding econometric issues that a growing literature has highlighted (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021). See the review of Roth et al. (2023) for more details.

[12] In our "stacked" panel, control cities are thus excluded if they become reformed within a given event study j's time window.

There are several ways in which we can address this issue. First, we can use the high frequency nature of the delivery error data to distinguish changes at the time of civil reform from slower moving factors. By examining year-by-year estimates, we can be more confident that we are capturing the effect of the specific institutional change. The availability of annual data is thus a significant advance in our study period, since economic outcomes for studies examining this time period are often measured using census data that are commonly collected at decadal frequency. Second, we can leverage institutional knowledge about the assignment rule (based on the post office size threshold) to adopt a control function approach by including the time-interacted baseline number of postal employees in the vector of controls x_{ijk}.[13] Third, we can extend this approach to flexibly control for a wide range of time-interacted baseline characteristics. We use "post-double-selection" to select the covariates out of a battery of 31 variables (Belloni et al., 2014).[14] This approach avoids overfitting and provides a principled approach to covariate selection. Finally, we also include state $\times$ year fixed effects to restrict the comparison to cities within the same state, thus allowing for tigher treatment-control comparisons. We cluster the standard errors for the error term ε_{ijk} at the ij-level, corresponding to the level of treatment assignment.[15]

4.2 Effects on postal performance

Effects on delivery quality. Table 2 presents the main performance results. The dependent variable is the (log) number of delivery errors in a city and year.[16] The columns vary the specification and samples to probe the robustness of the results. Overall, reformed cities experience a sizable reduction in delivery errors.

Column 1 first reports the baseline specification for Equation 1 relying on a simple set of fixed effects without time-varying controls. On average, reformed cities experience a reduction in delivery errors by 22%. In columns 2-3, we include time-interacted controls to assess the extent to which baseline differences across reformed vs. unreformed cities drive our results. Since the assignment of cities to civil service reform depended deterministically on the total postal employment within an office, column 2 includes the time-interacted baseline postal employment as a control function.[17] In column 3, we probe this result further by flexibly controlling for all available baseline characteristics using post-double selection for data-driven covariate selection (Belloni et al., 2014). Out of the 31 baseline characteristics, this exercise adds three additional baseline covariates in addition to postal employment: (log) city population size, the county-level share of employees in education, and

[13] We refrain from referring to this approach as a regression discontinuity design due to the lack of observations around the 50 employees threshold. Instead, our preferred interpretation considers including time-interacted controls more generally as relying on a conditional independence assumption, whereby – conditional on the time-interacted covariates (and the city and time fixed effects), we isolate the exogenous city-year variation stemming from the rollout of the civil service reform.

[14] Table 1, Table A1, and Appendix Appendix B report descriptive statistics and the source of the full set of variables.

[15] Our results also hold when clustering at the city-level, as well as the county $\times$ reform-wave level or the county-level to account for spatial autocorrelation in the errors (Appendix Table A2).

[16] We do not have data on mail volumes with comparable coverage. In subsection 5.1, however, we use data on mail volumes for the subset of cities with free delivery to show that the reform also reduced the delivery error rates.

[17] The results are comparable using log-transformation and more flexible, non-linear functional forms (e.g., up to fourth degree polynomials).

the presence of a Western Union office.[18] Not surprisingly given the employment-based assignment threshold, these characteristics capture size-related differences across civil service reformed and unreformed cities. Reassuringly, the inclusion of these additional time-interacted controls leaves the estimates relatively unchanged in terms of sign, magnitude, and statistical significance. Throughout the specifications, the reform magnitude for the combined waves of 1883 and 1893 lies between 19–22%.

A key assumption for the difference-in-differences design to identify the causal effect of civil service reform on post office performance is that treatment and control cities would have experienced common trends in the absence of the reform ("parallel trends"). While this assumption is not directly testable, we can provide supporting evidence for its validity by investigating the presence of pre-trends. Figure 2 provides visual evidence for the reform's effects on delivery errors by reporting the estimates of the augmented Equation 1 – a flexible version of Table 2, column 3 – where β is allowed to vary by each year.[19] As Figure 2 shows, we observe little difference between the trends of reformed and unreformed cities prior to the introduction of the reform.[20] After the introduction of postal reform, there is a clear reduction in delivery errors in reformed cities relative to those cities that were not reformed. This gap in delivery errors increases gradually before leveling out.

In Figure 3, we reproduce the visual event study evidence broken down by the two major reform waves, with columns 4–5 of Table 2 providing the corresponding point estimates. As the figure shows, the dynamic reform effects look strikingly similar across the two main reform waves, with the 1883 reform less precisely estimated – likely due to the smaller number of reform cities. In line with the scale-up literature (e.g., Muralidharan and Niehaus (2017)), we find that the reform effects are larger in the initial 1883 expansion but continue to remain sizable in the subsequent large-scale expansion a decade later. The robustness of our effects by reform wave is reassuring. The fact that the same empirical pattern emerges across two different decades suggests that the reform is not driven by any single historical event that coincided with the reform (e.g., a decline in postal rates in the 1880s that affected larger cities), bolstering the external validity of the findings.[21] The stable pattern across two reform waves also alleviates concerns about time-varying heterogeneous treatment effects that a newer literature increasingly highlights (Roth et al., 2023). More explicitly, Appendix Figure A1 compares our "stacked" event study estimates with alternative estimation methods developed for staggered difference-in-differences. The dynamic reform effects are again comparable across approaches.

Effects on productivity. Despite the robust evidence of the impact of federal civil service reform on delivery errors, potential concerns may remain over the interpretation of the outcome measure. For example, fewer reports could reflect *worse* performance if errors are not even discovered or if mail is not even routed across cities. We thus complement our primary outcome measure using the secondary set of productivity measures.

[18] Appendix Table B2 reports the baseline covariates selected through post-double-selection for all subsequent regressions.

[19] We estimate $\log(y_{ijk}) = \Sigma_{l=-4}^{8} \beta_l \text{treat}_{ij} \times \mathbf{1}[k=l] + \theta_{ij} + \tau_{jk} + \gamma' x_{ijk} + \varepsilon_{ijk}$, with the pre-reform year ($k=-1$) as the omitted category.

[20] Formally testing for differential pretrends $\text{treat}_{ij} \times k$ on the subsample prior to the reform using yields an estimate of 0.030 (se: 0.047), with a corresponding p-value of 0.52.

[21] In Appendix Table A3, we also show that our results hold when including all reform waves.

We leverage another source of rich data on the operation of the Post Office Department's City Free Delivery service. These data are available for cities affected by the initial rollout of civil service protections under the 1883 Pendleton Act (see subsection 3.1). We use two measures of productivity: volume handled per carrier and per-unit cost of processing mail. While the free delivery service provides these added outcome measures, a limitation is that they are only available for a smaller subset of 512 larger cities covering the period 1875–1891. As a result, the stacked regression of Equation 1 simplifies to a simple difference-in-differences centered around the 1883 reform wave, with errors clustered at the city-level. Our main result on delivery errors also holds in this sample, suggesting that zooming in on this subset of the Post Office to explore mechanisms will still provide generalizable evidence. Given the smaller sample, however, we can no longer include the stringent State $\times$ Year fixed effects which restrict the empirical analysis to within-state-year comparisons.[22]

The regression results are shown in Table 3. In columns 1-2, we focus on volume per carrier as the outcome. Column 1 reports the baseline specification controlling for post office employment. Column 2 reports the specification with the time-interacted controls selected via PDS. Consistent with the improvement in performance shown in Table 2, there is a significant increase in the volume per carrier of 8–14%. Columns 3-4 corroborate these results by showing a similar improvement in per-unit cost-efficiency (total cost divided by the total volume of mail processed) by 10–13%. Column 3 reports the baseline specification while column 4 reports the version based on PDS. Figure 4 shows the visual evidence. There is a marked relative improvement after the reform, but no clear pre-trends. The combined evidence – relying on both delivery errors and productivity measures derived from two independent sources – suggests that civil service reform improved postal performance.

Robustness checks. We provide a range of robustness checks to address concerns over more specific confounders. First, a range of infrastructural improvements took place during the 19th century that may have shaped information flows, such as the development of the railroads, canals, and the telegraph network. If complementary public infrastructure expanded concurrently with the reform, part of our reform effects may capture general improvements in communication infrastructure. In Appendix Table A4, we test if civil service reform is associated with the expansion of the telegraph and railroads, the two main ways through which information was transmitted across distance in the 19th century. We do so by leveraging complete count Decennial Census data to compute the change in telegraph and railroad employment at the county-level between 1880-1900. As the estimates show, we do not find that the reform is significantly associated with the expansion of the telegraph and railroad, as proxied by employment.

We also investigate confounders related to developments within the Post Office iself. The implementation of civil service protections within the Post Office took place against the backdrop of several institutional changes that affected postal service during the second half of the 19th century. These developments included the advent of free city delivery by carriers in major cities (1863), allowing for money orders to eliminate cash transactions

[22] While the sign of the point estimate remains comparable, the inclusion of State $\times$ Year FEs results in attenuated and noisier estimates.

via mail (1864), and the penny postal cards (1873). As the types of services diversified and demand for mail service grew, the Post Office also increased its internal oversight capacity. Particularly relevant for our study was the official re-organization of postal inspectors under the newly-created Division of Postal Inspectors and Mail Depredations in 1875. This period witnessed a modest expansion of staff responsible for carrying out new administrative responsibilities – many of which were related to a spate of new obscenity laws, beginning with the Comstock Laws of 1873 (Carpenter, 2001). Postal inspectors were in turn tasked with identifying fraudulent or obscene material that traveled through the postal system.

Given these institutional changes, one potential empirical concern is that we are confounding the effects of civil service reform with the expanding oversight scope and policing capacity given to postal inspectors in the decade prior to Pendleton Act. While obscenity laws generally applied nationwide, it is nevertheless possible that postal inspection was targeted at large cities where vice was viewed as particularly rampant. Given these concerns, we test whether inspection capacity increased within reformed cities after civil service reform took effect (Appendix Table A5). Reassuringly, we see virtually no effect of civil service reform on the number of inspectors working in affected cities. This finding suggests that the improved postal performance we observe is unlikely to be driven by an expansion in inspection capacity.

Finally, we also consider whether our results are spuriously driven by other concurrent civil service reforms. As section 2 describes, the Pendleton Act initially covered substantial parts of the Customs Office in addition to the Post Office. In Appendix Table A6, columns 1-3, we show that our reform effects are similar in size and precision when dropping all cities with a customs office, as well as when we drop only the subset of 11 with customs that were reformed. We also consider the possibility that postal reform is associated with concurrent municipal civil service reforms. The federal reform we study predates most of the municipal reforms that occurred in the 20th century (Rauch, 1995; Ornaghi, 2019). We show in column 4 that our main results hold when dropping all cities that experience municipal reforms during our study period (Appendix Table A6). More generally, we demonstrate that the results are not driven by outliers. The main finding holds when dropping one reform city at a time (Appendix Figure A2) for the 1883 reform wave, as well as when dropping each state at a time for the 1893 reform wave (Appendix Figure A3). Taken together, the results suggest that the civil service reform within the Post Office significantly improved post office performance.

5 Drivers of increased performance – mechanisms

The results above provide robust evidence that postal reform improved public service delivery. As a landmark reform, the Pendleton Act provided a blueprint for many civil service reforms that would follow. Like many civil service reforms today, the Pendleton Act was comprised of distinct provisions, thus creating multiple channels that could affect post office outcomes. We now investigate these potential underlying mechanisms.

5.1 Changes in inputs and workload

Changes in labor inputs. The nineteenth century was a period of significant government growth (Mastrorocco and Teso, 2023). As such, we first explore whether the reduction in delivery errors coincides with an expansion of the postal labor force. To test whether our results are associated with an increase in labor inputs, we now make use of the digitized Official Registers, which allow us to track the total employment of mail clerks and letter carriers biennially. We compute the total postal workforce for each city by aggregating the individual-level records. To obtain annual counts, we linearly interpolate between the missing years.

Table 4, columns 1-2 report the estimates based on the total personnel counts. We use the same "stacked" difference-in-differences design (Equation 1), except that the dependent variable now is the (log) total postal employment. There is no strong evidence that the civil service reform significantly affected overall postal employment. The point estimate is positive but quantitatively small, corresponding to an increase in staff of 1.4-1.7%, and statistically insignificant. Columns 3-4 further increase our confidence in our main finding by demonstrating that the main performance result holds even when computing delivery error per worker. These results suggest that the improved performance we observe is unlikely to be driven by increasing labor input.

Changes in worker quality. Even if the total number of postal workers remains unaffected, civil service reform may have improved post office performance by improving the quality of serving bureaucrats. Indeed, a large literature has documented the importance of the selection margin for explaining differences in bureaucrat performance (Dal Bó et al., 2013; Besley et al., 2021). For example, replacing discretionary political appointments with rule-based selection could have attracted higher quality candidates, and thus improved bureaucratic selection. In our context, exam-based selection only applied to new entrants. Replacing the existing personnel was a protracted process,[23] making improved selection *a priori* perhaps a less plausible channel.[24]

Nevertheless, we test for the selection channel by analyzing whether the composition of hires changes significantly after the implementation of federal civil service reform. A challenge to the study of bureaucratic selection in the 19th century is the lack of data on human capital. We overcome this challenge in several ways. First, we focus on patronage-related measures from the personnel records (the Official Registers). Measures from these records are available for all postal workers, thus eliminating concerns about selection bias. The individual-level results are shown in Table 5, Panel A.[25] Overall, we do not find significant changes in the composition of the hired postal workforce. Bureaucrats hired post-reform are not differentially likely to be appointed from their home state, nor are they more or less likely to be foreign-born or belong to a nationality associated with significant political machines.

[23] Even eight years after civil service rules took effect, almost 20% of the average reformed post office still comprised of patronage appointees (Appendix Figure A4).

[24] Implicitly, this argument assumes that workers are substitutes in the production process. To the extent there are strong complementarities, removing "bottlenecks" alone can disproportionately increase postal output.

[25] Appendix Figure A5 reports the corresponding dynamic effects for all individual-level characteristics.

Second, we complement these personnel measures from the Registers by linking these records to the full count U.S. Decennial Censuses of 1880 and 1900. This linkage allows us to obtain age (a proxy for previous work experience), literacy (an admittedly crude measure of human capital), and race as additional characteristics. We link our personnel data to the Census based on name, birth state, and current state, following Aneja and Xu (2021).[26] Table 5, Panel B reports the results for the subset of matched civil servants. Reassuringly, we again do not find that civil service reform significantly changed the traits of hired workers. While we find a slight increase in the age and literacy of hired civil servants, the magnitudes are small and statistically insignificant. Similarly, we do not find that the Pendleton Act increased the share of female or non-white civil service entrants. In addition to finding little change in worker composition, we also find no evidence of significant changes in the pay of bureaucrats that may indicate improvements in worker quality (Appendix Table A7).

In sum, the collective body of evidence – the slow rate at which civil servants were hired through the new exam scheme, as well as the absence of large observable changes in the composition of hires – suggests that a change in selection is unlikely to be the main margin of adjustment. While our observable measures of civil servant characteristics are arguably crude given data limitations, many of the point estimates are precisely estimated, allowing us to rule out even relatively small changes in certain individual traits. In subsection 5.2, we also provide additional evidence using retention by holding constant the selection margin, showing that reform effects applied equally to offers that were selected *prior* to the exams. The absence of uniformly strong effects thus suggests that improved bureaucrat selection is unlikely to be the primary channel through which civil service reform improved the quality of public service delivery offered by American post offices.

Changes in demand. Yet another mechanism through which performance might have improved post-reform is through an overall reduction in workload. Even if overall employment levels and the quality of workers remained comparable, the decline in error rates could coincide with a reduction in workload due to a decline in the overall mail volumes. Admittedly, we believe that declining demand for mail services is unlikely to be a confounder in our setting given the increasing scale of postal operations during the 19th century (Blevins, 2021).[27] Nevertheless, we can use data on overall mail volumes from the free delivery statistics for 1875–1891 to test this channel explicitly.[28] The results are reported in Table 6, with Appendix Figure A6 reporting the corresponding dynamic effects. If anything, the results show an *increase* in the overall volume of mail, though the point estimate is statistically insignificant. In Appendix Table A9, we disaggregate the overall collection and delivery volumes by type of mail delivered. Once again, the results are inconsistent with the interpretation that the improved performance reflects a decline in delivery volume.

[26] We obtain a match rate of 36%, which compares favorably to match rates obtained in related work. Importantly, the match rate does not vary significantly with the reform (Appendix Table A8) and our results are comparable when reweighting the matched sample based on observable characteristics (Appendix Table C2). Appendix C provides additional details on the matching procedure.

[27] Moreover, if demand for mail services was increasing during this period, such changes would presumably produce *upward* pressure on delivery mistakes due to increased workload, thus plausibly rendering our effect an underestimate of the true effect of civil service reform.

[28] Unfortunately, the free delivery statistics are only reported until 1891. Our delivery results also hold – albeit noisier given the small sample size – for the 1883 expansion and for the subsample of locations for which we have free delivery statistics (Appendix Table A3).

Figure 5 shows the relationship between delivery errors and aggregate postal volume for reformed and unreformed cities. The measures are residualized by partialing out city and year fixed effects, as well as the total postal employment interacted with year fixed effects. Panel (a) shows the relationship for the subsample of cities that are reformed in 1883, while Panel (b) shows the relationship for the unreformed cities in the same reform wave. As the figure shows, delivery errors tend to increase with higher volumes.[29] The introduction of the civil service reform, however, weakened the relationship between errors and volume. Strikingly, we do not observe the same change when examining the relationship in the sample of cities that remain unreformed. This difference in the changing slope across reformed and unreformed cities is statistically significant (Appendix Table A10). This suggests that civil service reform helped change the "technology" of production, flattening the relationship between delivery errors and overall mail volume.

5.2 Reduced turnover and political cycles

Prior to the introduction of federal civil service protections, high turnover was a by-product of serving "at the pleasure" of politicians. High turnover was not only a phenomenon around party transitions. As Hoogenboom (1959) explains regarding the pre-reform period, "individuals could anticipate early dismissal from office, for tenure was extremely insecure ... even if his party remained in power, a civil servant was not secure in his position ... [t]hese removals were caused by factional struggles." The resulting high level of churn not only impeded learning-on-the-job, but also limited incentives for workers to exert effort or invest in job-specific skills: "morale was low in a civil service largely composed of misfits employed on a temporary basis ... it was impossible for an esprit de corps or loyalty to office or agency to develop in an atmosphere of nervous tension." Motivated by the historical narrative, we directly test whether the Pendleton Act reduced political cycles.

Political cycles in exit rates. To test whether the civil service reform indeed reduced turnover, we again leverage micro-level personnel data from the Official Registers. We use a "stacked" design analogous to Equation 1, except that the outcome now is the exit rate. The results on bureaucratic turnover are reported in Table 7. As the mean of the dependent variable shows, the exit rate in the 19th century federal bureaucracy was high: the unconditional probability of exit in a given year was 17%. The civil service reform helped significantly reduce the exit rate by 3.7–4 p.p (Columns 1–2). Compared to the mean, this decline is sizable, corresponding to a relative decline of 21–23%. Importantly, the decline in exit is driven presidential electoral cycles. To show this, we restrict the sample to a balanced panel to ensure that the time heterogeneity we uncover are not driven by changes in the composition of cities. As columns 3–4 show, the reform effects hold equally in the balanced panel (column 3) and are driven by exits happening right after a federal election year (column 4). Figure 6 complements the regression results with visual evidence. The figure shows the mean exit rates for reformed

[29] The relationship between delivery errors and volumes is flatter among unreformed cities, reflecting the fact that reformed and unreformed cities differ along many dimensions (see Table 1). We focus on the *change in slopes within* reformed and unreformed cities.

vs. unreformed cities centered around the first post-reform election.[30] Strikingly, the figure shows the presence of significant political cycles in the exit rate, consistent with elected politicians exercising their discretion over appointments to restaff the civil service.[31] These political cycles, however, decline significantly for elections that occur after the civil service reform is implemented.[32]

As a natural consequence of the reduced exit rate, we observe a move towards longer and more stable careers. In Table 7 columns 5–6, we relate the civil service reform to the mean years of service, a measure of experience. The results document a significant and economically large improvement in the average tenure. While the mean years of tenure is 1.7 years for the full sample, the average experience in reformed cities increases by 0.78-0.98 years. Overall, our results are thus consistent with the Pendleton Act severing the ties between politics and administration, thereby giving rise to a professionalized service characterized by stable careers.

Bureaucrat performance during election years. The reduction of turnover and increase in bureaucrat experience is likely to be conducive to greater performance (Akhtari et al., 2022). We now directly connect the dampening of political cycles in exit to bureaucrat performance. Similar to the results presented in Table 7, we ask whether the performance improvements are particularly pronounced during election years. We restrict the sample to a balanced panel in order to ensure that the time-heterogeneity is not driven by composition changes. The results are shown in Table 8. Consistent with the disruptive nature of politically-driven turnover, we find that the reduction in delivery errors is driven by election years (column 1). Prior to the reform, these were the years characterized by high turnover. The productivity gains, as measured by greater volume per worker or higher cost-efficiency (columns 2-3), are equally higher in election years. Taken together, these results lead us to conclude that the reduction of patronage-driven turnover was a prominent channel through which civil service reform improved performance.

General vs. exam-specific reduction in exit rates. Finally, we probe deeper to understand who is driving the higher post-reform retention. Specifically, we ask whether the reduction in exit applies to all serving officers or is only restricted to those who were recruited through the exam-based selection process. This distinction has implications for the interpretation of our results: if the reform increased retention even for civil servants who were appointed under the spoils system, our results are less likely to reflect changes driven by differential bureaucrat selection. In contrast, if the lower exit rates were only driven by those who were hired through competitive exams, the results would provide a stronger case for the role of bureaucratic selection.

In Table 9, we thus study individual-level heterogeneity. The specification remains the "stacked" event study design, except that the unit of analysis is now the individual-year. Since exit rates vary strongly with tenure, we

[30] For completeness, Appendix Figure A7 also shows the raw exit rates over time for the two major reform waves of 1883 and 1893.

[31] These political cycles are also found outside of the Post Office in other parts of the federal government (see Moreira and Perez (2022a) for evidencce in the the Customs Office, and Mastrorocco and Teso (2023) for the broader non-postal federal workforce).

[32] Federal employment data is published biennially in odd years, while federal elections occur quadriennally in even years. Our measure of "election year" captures the first observable year after the election, or the year a new presidential term began.

also introduce reform-specific tenure experience FEs. Column 1 of Table 9 replicates the reduction in exits post-reform at the individual-level. In column 2, we restrict the sample to only individuals who were *already* serving when the reform was implemented. Holding selection constant, the results suggest that the retention effects even accrue to those who were appointed by patronage.[33] In column 3, we restrict the sample to only the post-reform period, when we observe the exit rates for those entering before and after the reform operating in the same incentive environment. As the point estimate shows, we do not find that the exit rates are significantly different for those who entered after the competitive exams were introduced. In column 4, we include both regressors jointly to decompose the general retention effect from the cohort-specific selection effect. The results suggest that the improvement in retention (i.e., lower exit) holds for all civil servants, irrespective of their entry cohort. This strongly suggests that the primary channel through which the civil service reform worked was by limiting arbitrary firing, as opposed to improving selection.

6 Civil service reform and the weakening of local political parties

A central motivation for civil service reform was to reduce the power of local parties over the hiring of government employees (Mashaw, 2009). While the power of appointments rested *de jure* with the federal government, hiring decisions were *de facto* delegated to the local level during the patronage era, due to the scale of the government and costs associated with centralized monitoring by federal officials (section 2). This gave influential local politicians substantial political rents (Hoogenboom, 1959). By limiting the scope for political interference and politically-motivated personnel decisions, civil service reform likely reduced the power of local party machines, which depended on patronage for political and financial support. By shielding rank-and-file civil servants from political interference, it is possible that the Pendleton Act not only improved postal performance, but also helped weaken the power of local political parties.

Unfortunately, a major constraint to studying the downstream political effects of the Pendleton Act is the lack of data on either city-level elections or local party activities during our study period. Guided by research on the history of American political development, we use the local presence and circulation of explicitly partisan newspapers as a proxy for the strength of partisan politics. During the 19th century, the newspaper was not only the main source of news but was also central to political parties' local-level organizational activity (Baldasty and Rutenbeck, 1988). Most papers were associated with a particular party, with the editor often being considered a party officer. Local parties supplied readership (requiring members to subscribe), and newspapers promoted party ideas and candidates (Kaplan, 2002; Baldasty and Rutenbeck, 1988). Political parties also supported newspapers financially, with parties funding explicitly partisan newspapers (Kaplan, 2002).

While the time period during which the federal government adopted civil service reforms coincided with the

[33] For completeness, Appendix Figure A9 reports the corresponding dynamic effects.

well-noted decline in the number of explicitly partisan newspapers (Petrova, 2011; Gentzkow et al., 2015), how civil service reform impacts the presence of partisan newspapers is, in theory, ambiguous. On the one hand, given that civil service reform reduced the power of party elites, it is reasonable to postulate that civil service reform also had potential downstream effects on agents supported by parties – such as the partisan press. With fewer patronage jobs to offer, local parties lost membership – which in turn may have reduced the readership of party-controlled newspapers. Without the support of a strong membership base or political assessments, parties were potentially less able to support the partisan press financially, thus leading to a *decrease* in the presence of political newspapers.[34] On the other hand, civil service reform may also *increase* the importance – and thus improved the viability – of partisan newspapers. Patronage jobs for party loyalists (and party newspaper readers) were not the only form of patronage that officials controlled during the nineteenth century. Newspaper editors also received direct support from politicians in the form of government printing contracts and other forms of subsidies (Dyer, 1980). As such, to the extent that political parties needed party-run newspapers to mobilize the electorate in support of a candidate – and were no longer able to secure votes by the promise of government positions – one might reasonably expect them to use other methods such as government subsidies to preserve the strength of partisan newspapers. Despite the importance of the Pendleton Act and the rise of non-partisan media in U.S. economic history, however, there has been to this point little work that aims to quantitatively establish the link between civil service reform and the proliferation of partisan media.

To test the hypothesis that civil service reform weakened the role of local politics, we use city-level newspaper data from Gentzkow et al. (2011). The data provides lists of newspapers along with their circulation and partisanship for each city and election-year (see section 3 for a detailed description of the data). We collapse the data to compute the total number of newspapers and circulation for each city and election-year, broken down by partisanship. We then adopt the same "stacked" event study design to ask if cities affected by civil service reform also exhibit changes in the number of partisan and non-partisan newspapers and their circulation.

Table 10 presents the results.[35] The dependent variable in columns 1-2 measures the total number of newspapers that are political or independent. The number of explicitly partisan newspapers declines significantly after the reform (column 1).[36] While the average number of political newspapers in a city-year is 1.8, the number declines by 0.2 in reformed cities, reflecting a relative decline by 11%. In contrast, we observe an increase in independent newspapers (column 2). Columns 3-5 focus on circulation for cities where local newspapers are present. Consistent with the extensive margin effects, civil service reform decreases the circulation share of political newspapers while significantly increasing the share of independent newspapers in circulation (columns

[34] The channel we propose – which links the removal of patronage jobs to fewer political rents – is not exclusively limited to post office employment. The only other federal organization that was affected by civil service protections in the Act's initial years were the Treasury's custom houses (Hoogenboom, 1959). Among those 11 cities with reformed customs, all also saw their post office reformed, thus making it impossible to separately estimate the effect of the customs reform on the presence of political newspapers.

[35] Since newspaper data is only available for election-years for the period 1879–1900, the number of observations is somewhat smaller.

[36] Figure A10 shows the visual evidence, centering elections around the reform. A limitation of the data from Gentzkow et al. (2011) is that we only have a single pre-period. Nonetheless, the figure suggests a sharp decline and subsequent leveling out.

3–4). Overall, this results in a relative decline in the share of political newspapers in circulation (column 5). While the intensive margin estimates are less precisely estimated, the combined evidence suggests that civil service reform indeed helped increase the presence and circulation of independent media.

7 Conclusion

Institutions that protect civil servants from political interference are nowadays considered defining features of a well-functioning state. While political appointees make up less than 1% of the U.S. federal civil service today (Spenkuch et al., 2023), this was not always the case. Prior to the introduction of the Pendleton Act of 1883, virtually all federal workers were politically appointed. In this paper, we studied the first major expansion of civil service protections in the U.S. We show that a strengthening of state capacity through civil service reform improved the quality of a major government institution: the United States Postal Service ("Post Office" or "USPS"). Arguably, there are few government institutions that have been as central to modern state building as the post office (Gallagher, 2016; John, 1995). Postal expansion in Western Europe and the United States in the 19th century produced "greater strides in the improvement of communication than had taken place in all previous centuries" (Howe, 2007, p. 5). For most Americans in the 19th and early 20th centuries, "the postal system was the central government" (John, 1995). By facilitating the flow of information and knowledge through the mail, the Post Office connected people across a vast and expanding nation.

We leverage the gradual rollout of federal civil service reform across cities and rich data on delivery errors to demonstrate that the Pendleton Act – a hallmark civil service reform – indeed led to a strengthening of state capacity. Cities that were covered by the civil service reform saw a significant reduction in delivery errors. Making use of personnel data and rich statistics on city free delivery, we show that the reduction in delivery errors likely reflected an increase in productivity. Finally, we use newly digitized personnel records of postal workers to open the black box of the organization and shed light on the mechanisms. Consistent with the historical literature (Johnson and Libecap, 1994; Hoogenboom, 1959), we find that the civil service reform led to a significant reduction in turnover and more stable careers.

As a key infrastructure, the functioning of the postal service is likely to have broader aggregate impacts (Gentzkow et al., 2011). We provide suggestive evidence that a depoliticized and more efficient postal service helped pave the way for the rise of independent newspapers. Leveraging data on newspaper partisanship and circulation, we find that the civil service reform significantly weakened the presence of partisan newspapers. In a context where political newspapers served at the behest of local party leaders (Petrova, 2011), our results thus indicate the role of civil service reform in weakening the power of local parties. Our findings thus suggest a "double-dividend" of civil service reform: separating politics from administration in a rank-and-file bureaucracy not only improved public sector performance, but also helped weaken the role of local partisanship.

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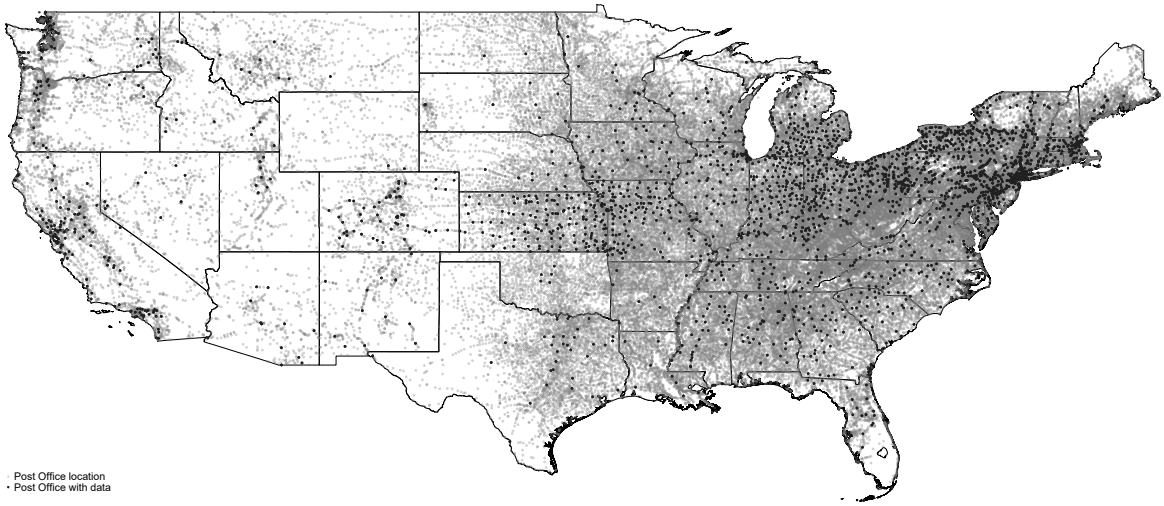
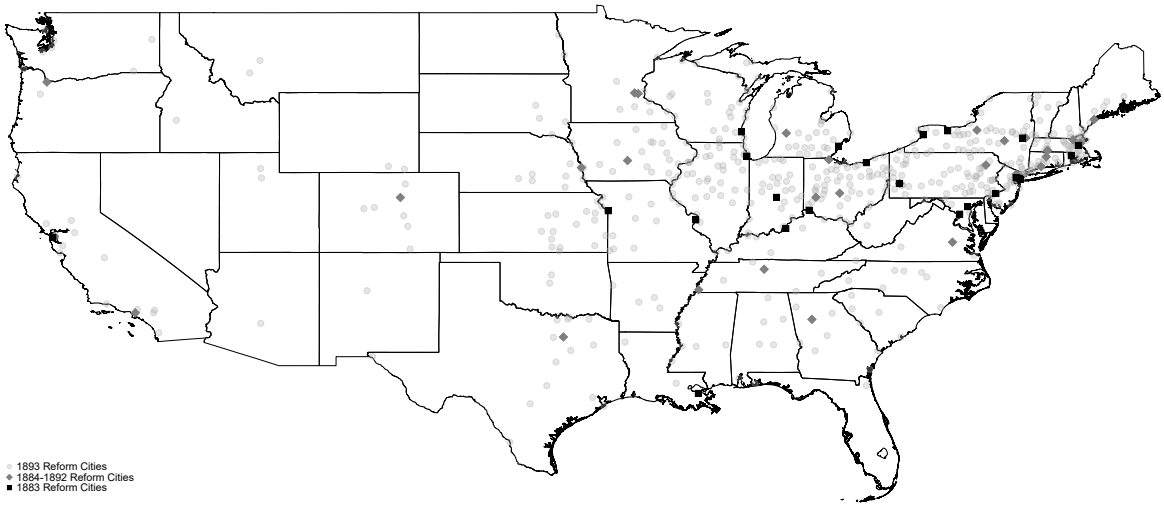
Figure 1: Spatial distribution of postal offices and expansion of the civil service reform



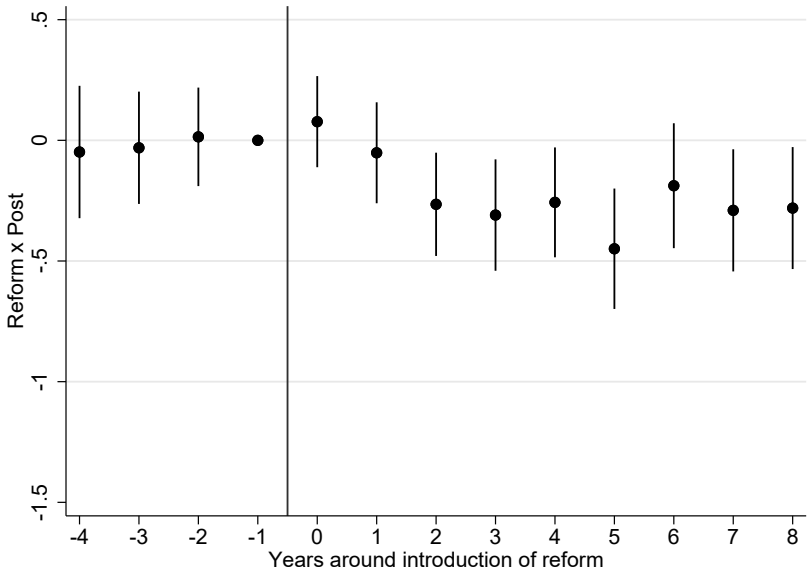
(a) All post offices



(b) Expansion of civil service reform

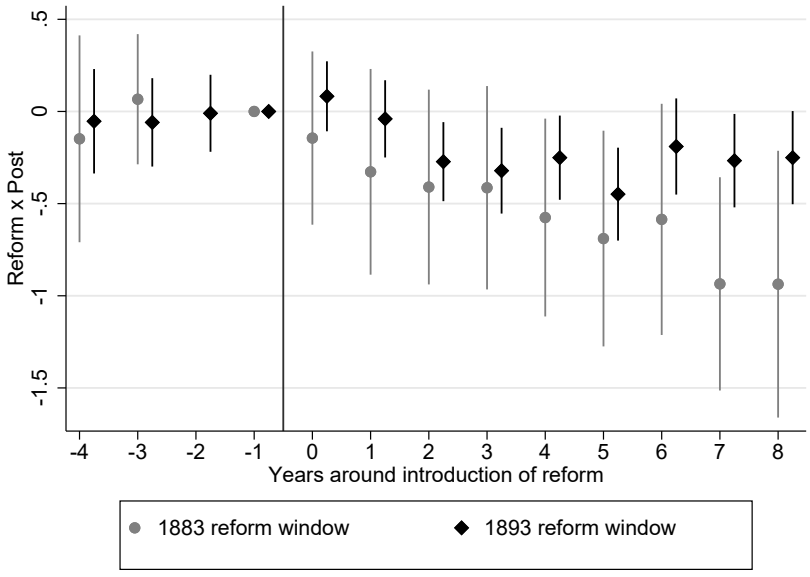
Notes: Panel (a) shows the locations of all post offices open between 1860-1905, as well as the locations for which performance data on delivery error rates or free delivery outcomes are available. Panel (b) shows the locations of reformed post offices and their timing. There are 23 locations that are reformed in 1883, 30 locations between 1884–1892, and 556 locations in 1893. The shapefiles are from National Weather Service (2023).

Figure 2: Delivery errors for reform vs. non-reform cities around reform years



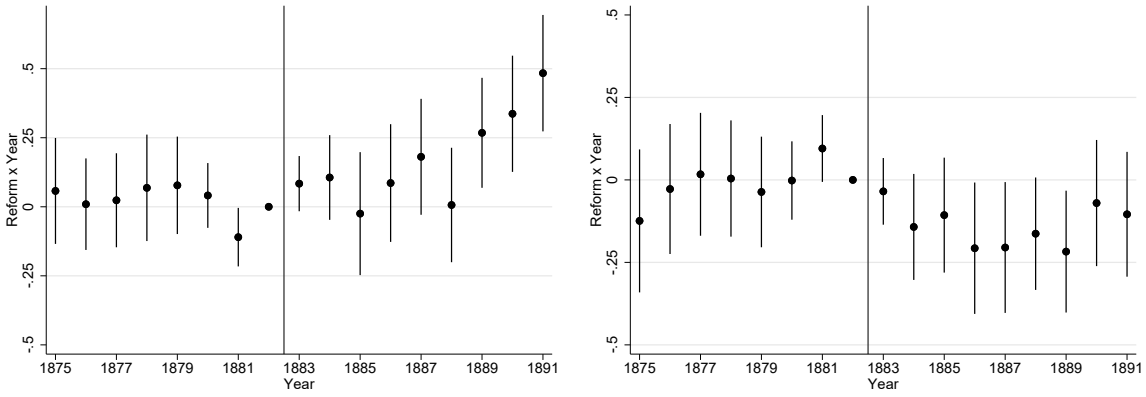
Notes: Figure reports an augmented version of Equation 1 (corresponding to Table 2, column 3), where the estimated difference between treatment and control cities is allowed to vary for each year around the introduction of the reform. Reporting 95% confidence intervals. Standard errors clustered at the city × reform year-level.

Figure 3: Delivery errors for reform vs. non-reform cities by reform wave



Notes: Figure reports an augmented version of Equation 1 (corresponding to Table 2, column 3), where the estimated differences between treatment and control cities are allowed to vary for each year around the introduction of the reform, and are estimated separately for the 1883 and 1893 reform windows. Estimate and confidence interval are missing for relative year $k = -2$ for the 1883 reform wave results due to the absence of an RMS Annual Report for 1881. Reporting 95% confidence intervals. Standard errors clustered at the city × reform year-level.

Figure 4: Delivery productivity and cost efficiency for reform vs. non-reform cities around the reform

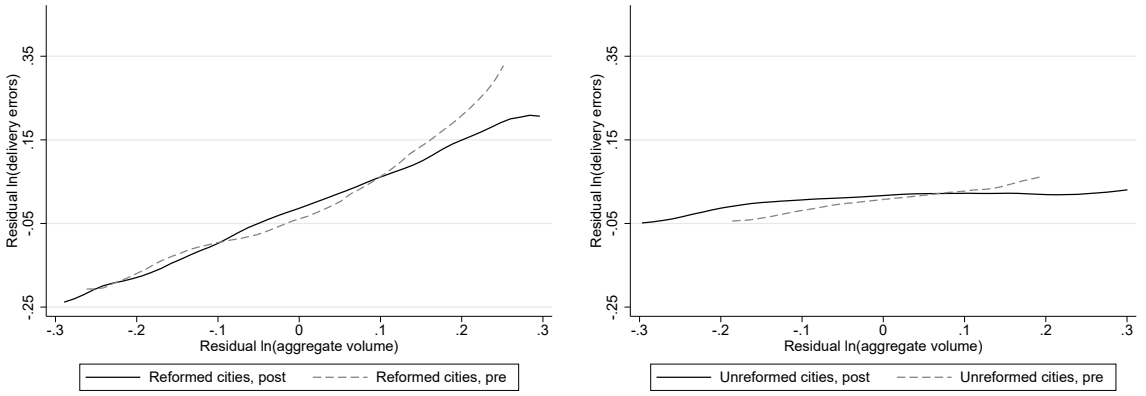


(a) Mail per carrier

(b) Cost per volume

Notes: Figure reports an augmented version of Equation 1 (corresponding to Table 3, columns 2 and 4), where the estimated differences between treatment units (reformed cities in 1883) and control units (unreformed cities) are allowed to vary by each year. Panel (a) shows the event study with (log) overall volume per carrier as the dependent variable, whereas panel (b) shows the (log) overall cost per volume. Reporting 95% confidence intervals. Standard errors clustered at the city-level.

Figure 5: Delivery errors and aggregate mail volume around the reform

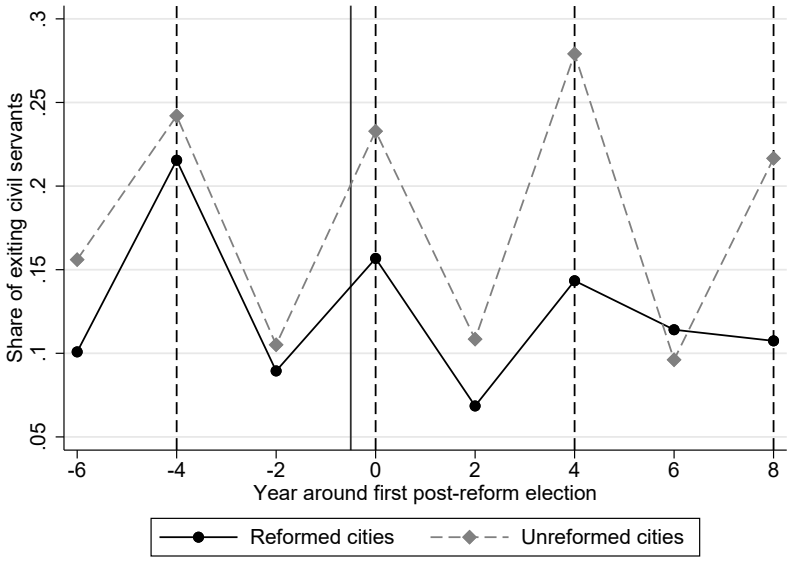


(a) Reformed cities

(b) Unreformed cities

Notes: Figure shows the relationship between (log) residual delivery errors and (log) residual aggregate mail volume (before and after the reform) using a local polynomial. Both variables are residualized by partialing out city and year FEs, as well as total employment $\times$ year FEs. The sample focuses on the 1883 reform wave, covering cities that are reformed and unreformed between 1875-1891. Panel (a) shows the relationship for cities that are reformed in 1883, and Panel (b) shows the relationship for cities that remain unreformed.

Figure 6: Exit rates among postal workers in reformed vs. unreformed cities around the reform



Notes: Figure shows the demeaned exit rates (i.e., the share of postal workers last observed in a given year) among postal workers in reformed (black solid line) vs. unreformed (gray solid line) cities around the reform. Exit rates are demeaned within each reform wave $\times$ city. Solid vertical line indicates the year of the reform, and dashed vertical lines indicate presidential election years. Sample includes both major reform waves.

Table 1: Descriptive statistics of reformed and unreformed cities

	(1)	(2)	(3)	(4)	(5)
	Mean	Difference treatment-control in reform wave			
	control	1883	1884-1892	1893	Pooled
Panel A: Post office-level					
Post office staff	3.38	170.144	18.964	3.923	15.483
		(51.125)	(1.698)	(0.230)	(3.731)
Log(City population)	8.39	3.320	2.270	1.646	1.793
		(0.176)	(0.091)	(0.037)	(0.039)
Log(Delivery errors)	4.41	4.286	2.787	1.758	2.021
		(0.261)	(0.181)	(0.084)	(0.083)
Year established	1837.06	-33.598	-11.026	-15.303	-16.020
		(8.622)	(5.576)	(1.562)	(1.515)
Log(Distance to DC)	6.28	-0.496	-0.006	-0.353	-0.335
		(0.269)	(0.152)	(0.045)	(0.043)
Longitude	-88.66	3.048	1.708	7.355	6.697
		(2.295)	(2.791)	(0.678)	(0.643)
Latitude	39.54	-0.182	0.713	1.992	1.775
		(0.584)	(0.603)	(0.187)	(0.174)
U.S. South	0.29	0.028	-0.096	-0.265	-0.236
		(0.087)	(0.069)	(0.023)	(0.021)
Western Union branch	0.78	0.070	0.174	0.349	0.321
		(0.010)	(0.014)	(0.020)	(0.017)
Total number of post-offices	1,738	593	1,589	1,454	1,763
- of which treatment:	0	23	29	547	599
Panel B: County-level					
Log(Total population)	10.16	1.781	1.165	0.762	0.855
		(0.171)	(0.116)	(0.037)	(0.036)
Labor force participation rate	0.361	0.022	0.037	0.015	0.017
		(0.007)	(0.007)	(0.004)	(0.003)
Share urban	0.042	0.648	0.470	0.080	0.144
		(0.055)	(0.045)	(0.009)	(0.013)
Share literate	0.857	0.030	0.033	0.077	0.071
		(0.011)	(0.018)	(0.008)	(0.007)
Non-white share	0.106	-0.014	-0.015	-0.063	-0.056
		(0.019)	(0.031)	(0.009)	(0.009)
Foreign-born share	0.133	0.104	0.089	0.063	0.067
		(0.022)	(0.019)	(0.006)	(0.005)
Log(Mean occscore)	2.95	0.154	0.152	0.118	0.123
		(0.014)	(0.015)	(0.007)	(0.006)
Number of land-grant universities	0.052	-0.010	0.085	0.030	0.032
		(0.043)	(0.063)	(0.013)	(0.013)
Manufacturing establishments	238.241	2113.55	601.22	220.314	365.00
		(558.06)	(211.87)	(18.26)	(46.48)
Frontier county	0.085	-0.012	-0.084	-0.092	-0.086
		(0.043)	(0.005)	(0.012)	(0.011)
Rail access	0.948	0.059	0.066	0.017	0.024
		(0.009)	(0.009)	(0.006)	(0.005)
Existing canals	0.101	0.257	0.129	0.083	0.096
		(0.103)	(0.079)	(0.016)	(0.016)
Number of party changes	0.217	0.236	0.041	0.066	0.070
		(0.087)	(0.061)	(0.015)	(0.015)
Turnout	0.673	-0.035	-0.085	0.012	0.004
		(0.043)	(0.029)	(0.007)	(0.007)
Republican vote share	0.443	-0.030	0.008	0.055	0.049
		(0.033)	(0.025)	(0.008)	(0.008)
Democrat vote share	0.427	0.042	0.001	-0.041	-0.035
		(0.034)	(0.026)	(0.008)	(0.008)
Total number of counties	1,191	484	1,110	1,048	1,203
- of which with treatment:	0	23	29	438	472

Notes: Column 1 shows the mean for the unreformed (control) cities. Columns 2-4 show the difference between reformed and unreformed cities for each reform wave. Column 5 shows the pooled difference, conditional on reform wave FEs. Observation counts (total number of cities for Panel A, total number of counties for Panel B) report the maximum number of observations. See Appendix B for a description of the data sources, and Appendix Table A1 for additional county-level characteristics. Robust standard errors are reported in parentheses.

Table 2: Civil service reform and public sector performance

	(1)	(2)	(3)	(4)	(5)
	Log(Number of delivery errors)				
Mean of dep. var	4.459	4.459	4.459	4.781	4.302
Reform × Post	-0.220	-0.194	-0.200	-0.517	-0.181
	(0.058)	(0.063)	(0.080)	(0.239)	(0.084)
Reform wave					
× City FEs	✓	✓	✓	✓	✓
× Year FEs × State FEs	✓	✓	✓	✓	✓
× Year FEs × Postal employment		✓			
× Year FEs × PDS controls			✓	✓	✓
Sample		Full sample		1883	1893
Observations	18,856	18,856	18,856	6,201	12,655

Notes: Relating (log) delivery errors to the civil service reform in a stacked event-study design (see description Equation 1). The unit of observation is the reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Column 2 includes the (time-interacted) total number of postal workers (measured in the initial year) as control variables. Column 3 includes time-interacted controls selected via post-double-selection (PDS, see Belloni et al. (2014)). The covariates selected via PDS are reported in Appendix Table B2. Columns 4-5 restrict the sample to only the 1883 and 1893 reform wave, respectively. Standard errors clustered at the city × reform-wave level.

Table 3: Civil service reform and productivity (1883 reform wave)

	(1)	(2)	(3)	(4)
	Log(Volume/carrier)		Log(Cost/Volume)	
Mean of dep. var	12.43	12.43	1.111	1.111
Reform 1883 × Post	0.084	0.137	-0.100	-0.129
	(0.046)	(0.068)	(0.043)	(0.068)
City FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Year FEs × Postal employment	✓		✓	
Year FEs × PDS controls		✓		✓
Observations	2,880	2,880	2,888	2,888

Notes: Relating (log) total mail volume per carrier (columns 1-2) and (log) total cost per volume (columns 3-4) to the civil service reform in a stacked event-study design. The sample is restricted to the 1883 reform wave and covers 1875–1891. The unit of observation is a city × year. Reform 1883 is a dummy that is 1 if the city was covered by the civil service reform in 1883, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Columns 1 and 3 include the (time-interacted) total number of postal workers as control variables. Columns 2 and 4 include time-interacted controls selected via post-double-selection (PDS, see Belloni et al. (2014)). The covariates selected via PDS are reported in Appendix Table B2. Standard errors clustered at the city-level.

Table 4: Postal employment, delivery errors per worker and civil service reform

	(1)	(2)	(3)	(4)
	Log(Postal staff)		Delivery errors/staff	
Mean of dep. var	1.548	1.548	0.321	0.321
Reform × Post	0.014	0.017	-0.081	-0.057
	(0.040)	(0.041)	(0.018)	(0.022)
Reform wave × City FEs	✓	✓	✓	✓
Reform wave × Year × State FEs	✓	✓	✓	✓
Reform wave × Year × Postal employment	✓		✓	
Reform wave × Year × PDS controls		✓		✓
Observations	18,707	18,707	18,707	18,707

Notes: Relating (log) total postal employment (postmaster, clerks, and carriers) and the delivery errors per worker (scaled × 100) to the civil service reform in a stacked event-study design. The unit of observation is the reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Columns 1 and 3 include the (time-interacted) total number of postal workers as control variables. Columns 2 and 4 include time-interacted controls selected via post-double-selection (PDS, see Belloni et al. (2014)). The covariates selected via PDS are reported in Appendix Table B2. Standard errors clustered at the city × reform-wave level.

Table 5: Civil service reform and individual-level characteristics of hires

Panel A: All hired civil servants	(1)	(2)	(3)	(4)
	Same state	Foreign-born	German	Irish
Mean of dep. var	0.628	0.0983	0.0244	0.0187
Reform × Post	-0.014	0.007	0.007	-0.001
	(0.014)	(0.007)	(0.004)	(0.004)
Reform wave × City FEs	✓	✓	✓	✓
Reform wave × Year × State FEs	✓	✓	✓	✓
Reform wave × Year × Job FEs	✓	✓	✓	✓
Reform wave × Year × Controls	✓	✓	✓	✓
Observations	68,977	68,977	68,977	68,977
Panel B: Census-linked hires	(1)	(2)	(3)	(4)
	Age	Literacy	Female	White
Mean of dep. var	28.40	0.864	0.114	0.966
Reform × Post	0.826	-0.002	-0.015	-0.000
	(1.038)	(0.027)	(0.026)	(0.012)
Reform wave × City FEs	✓	✓	✓	✓
Reform wave × Year × State FEs	✓	✓	✓	✓
Reform wave × Year × Job FEs	✓	✓	✓	✓
Reform wave × Year × Controls	✓	✓	✓	✓
Observations	22,465	22,465	22,465	22,465

Notes: Relating individual-level characteristics of recruited civil servants to the implementation of the civil service reform. Newly-recruited civil servants are identified as workers first observed in the personnel data. To avoid truncation (since all workers are first observed in the earliest year of our data), we exclude the first year of our personnel records, thus covering 1879–1901. The unit of observation is an individual × reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Panel A looks at the full sample of recruited civil servants, and Panel B looks at the subset of those who could be linked to the U.S. Decennial Censuses. All specifications include (time-interacted) total postal employment and (log) city population as controls. Standard errors clustered at the city × reform-wave level.

Table 6: Total mail volume, collections, deliveries and civil service reform (1883 reform wave)

	(1)	(2)	(3)	(4)
	Log(Total volume)		Log(Collected)	Log(Delivered)
Mean of dep. var	14.69	14.69	13.81	14.57
Reform 1883 × Post	0.122	0.129	0.257	0.104
	(0.075)	(0.130)	(0.166)	(0.121)
City FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Year FEs × Postal employment	✓			
Year FEs × PDS controls		✓	✓	✓
Observations	2,947	2,947	2,947	2,947

Notes: Relating (log) total mail volume (columns 1-2), (log) total mail collected (column 3), and (log) total mail delivered (column 4) to the implementation of the civil service reform in a stacked event-study design. The sample is restricted to the 1883 reform wave and covers 1875–1891. The unit of observation is a city × year. Reform 1883 is a dummy that is 1 if the city was covered by the civil service reform in 1883, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Column 1 includes the (time-interacted) total number of postal workers as control variables. Columns 2–4 include time-interacted controls selected via post-double-selection (PDS, see Belloni et al. (2014)). The covariates selected via PDS are reported in Appendix Table B2. Standard errors clustered at the city-level.

Table 7: Civil service reform and personnel turnover

	(1)	(2)	(3)	(4)	(5)	(6)
	Exit rate				Mean experience	
Mean of dep. var	0.173	0.173	0.148	0.148	1.727	1.727
Reform × Post	-0.037	-0.040	-0.041	0.023	0.967	0.779
	(0.016)	(0.016)	(0.023)	(0.028)	(0.114)	(0.138)
Reform × Election				0.054		
				(0.050)		
Reform × Post × Election				-0.130		
				(0.053)		
Reform wave × City FEs	✓	✓	✓	✓	✓	✓
Reform wave × Year × State FEs	✓	✓	✓	✓	✓	✓
Reform wave × Year × Postal employment	✓				✓	
Reform wave × Year × PDS controls		✓	✓	✓		✓
Balanced sample			✓	✓		
Observations	9,502	9,502	4,297	4,297	9,232	9,232

Notes: Relating the exit rate (columns 1-3) and mean experience (columns 4-5) to the civil service reform in a stacked event-study design. The exit rate is defined as the share of postal workers in a city that is last observed in the data. To avoid truncation (since all workers are last observed in the final year of our data), we exclude the last year of our personnel records, thus covering 1877-1899. The mean experience is the average years postal workers have served in a given post-office and year. The unit of observation is a reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Columns 1 and 5 include the (time-interacted) total number of postal workers as control variables. Columns 2–4, and 6 include the time-interacted controls selected via post-double-selection (PDS, see Belloni et al. (2014)). The covariates selected via PDS are reported in Appendix Table B2. Standard errors clustered at the city × reform-wave level.

Table 8: Performance, electoral cycles, and civil service reform

	(1)	(2)	(3)
	Error rate	Volume/Carrier	Cost/Volume
Mean of dep. var	0.278	12.59	1.019
Reform × Post	0.002	-0.003	-0.066
	(0.034)	(0.117)	(0.109)
Reform × Election	0.060	-0.090	0.078
	(0.040)	(0.038)	(0.041)
Reform × Post × Election	-0.075	0.194	-0.157
	(0.041)	(0.072)	(0.073)
Reform wave × City FEs	✓	✓	✓
Reform wave × Year × State FEs	✓		
Reform wave × Year FEs		✓	✓
Reform wave × Year × PDS controls	✓	✓	✓
Balanced panel	✓	✓	✓
Sample	All waves	1883 reform wave	
Observations	9,902	1,085	1,088

Notes: Relating delivery errors (column 1), (log) overall mail volume per carrier (column 2), and (log) cost per volume (column 3) to the civil service reform in a stacked event-study design. The unit of observation is a reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Election is a dummy that is 1 in a presidential election year, and 0 otherwise. Column 1 covers all reform waves, and columns 2-3 restrict the sample to only the 1883 reform wave. All specifications include time-interacted controls selected via post-double-selection (PDS, see Belloni et al. (2014)). The covariates selected via PDS are reported in Appendix Table B2. Standard errors clustered at the city × reform-wave level.

Table 9: Civil service reform and exit, broken down by the time of hiring

	(1)	(2)	(3)	(4)
			Exit	
Mean of dep. var	0.121	0.146	0.124	0.121
Reform × Post	-0.032	-0.160		-0.053
	(0.009)	(0.022)		(0.014)
Reform × Post-reform entry cohort			0.014	0.026
			(0.014)	(0.014)
Reform wave × City FEs	✓	✓	✓	✓
Reform wave × Experience FEs	✓	✓	✓	✓
Reform wave × Year × Job FEs	✓	✓	✓	✓
Reform wave × Year × State FEs	✓	✓	✓	✓
Reform wave × Year × Controls	✓	✓	✓	✓
Sample	Full sample	Pre-reform entrants	Post-reform period	Full sample
Observations	143,754	68,853	104,938	143,754

Notes: Relating individual-level exit to the civil service reform in a stacked event-study design. The unit of observation is an individual × reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Post-reform entry cohort is a dummy that is 1 if the individual entered (i.e., was first observed) after the implementation of the civil service reform. Columns 1 and 4 report results based on the full sample. Columns 2–3 split the sample by whether individuals entered before (Post-reform entry cohort=0) or after the reform (Post-reform entry cohort=1). All specifications include (time-interacted) total postal employment and (log) city population as controls. Standard errors clustered at the city × reform-wave level.

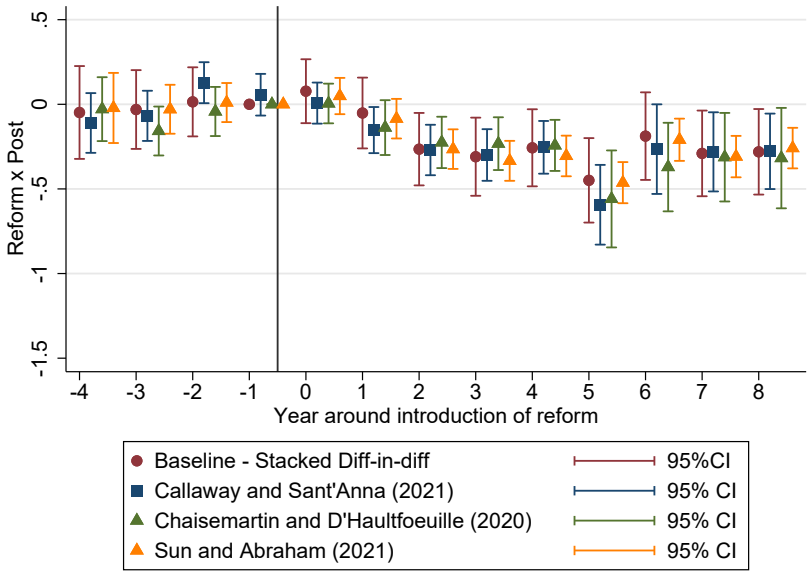
Table 10: Civil service reform and the decline in local partisan newspapers

	(1)	(2)	(3)	(4)	(5)
	Newspaper number		Circulation		Circ. share
	Political	Independent	Political	Independent	political
Mean of dep. var	1.821	0.191	5.578	0.834	0.907
Reform × Post	-0.195	0.085	-0.266	0.800	-0.044
	(0.079)	(0.043)	(0.779)	(0.477)	(0.025)
Sample	Election years				
Reform waves	All reform waves				
Data	Gentzkow et al. (2011)				
Reform wave × City FEs	✓	✓	✓	✓	✓
Reform wave × State × Year FEs	✓	✓	✓	✓	✓
Reform wave × Year × PDS controls	✓	✓	✓	✓	✓
Observations	4,235	4,235	3,758	3,758	3,758

Notes: Relating downstream effects to the civil service reform in a stacked event-study design. In columns 1-2, the dependent variable is the total number of newspapers in a given city and year, broken down by whether they are political (i.e., affiliated with a political party) or not (independent). In columns 3-4, the dependent variable is the corresponding number of newspaper circulation (in 1,000) that is political or independent. Column 5 is the share of newspapers in circulation that is political. All specifications include the time-interacted controls selected via post-double-selection (PDS, see Belloni et al. (2014)). The covariates selected via PDS are reported in Appendix Table B2. Standard errors clustered at the city × reform-wave level.

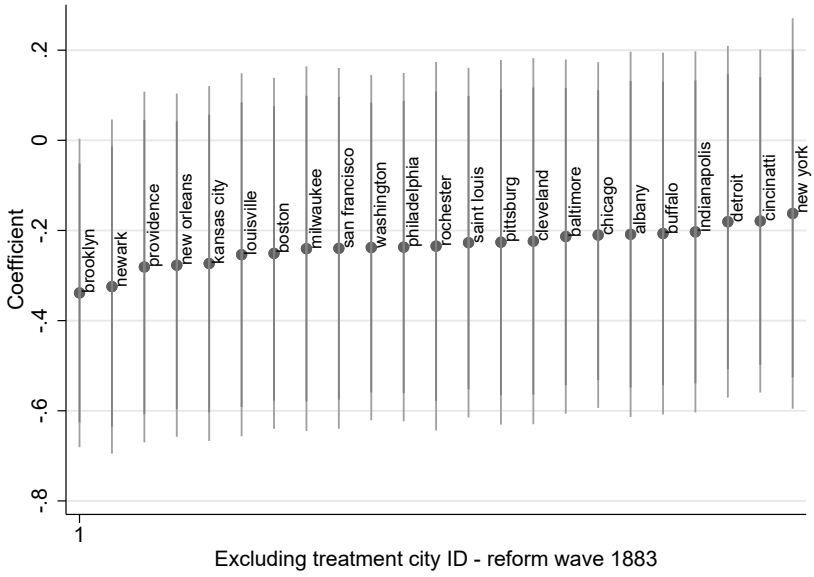
A Online Appendix for "Strengthening State Capacity: Civil Service Reform and Public Sector Performance during the Gilded Age," by Abhay Aneja and Guo Xu

Figure A1: Event study evidence – stacked DD and twoway FEs



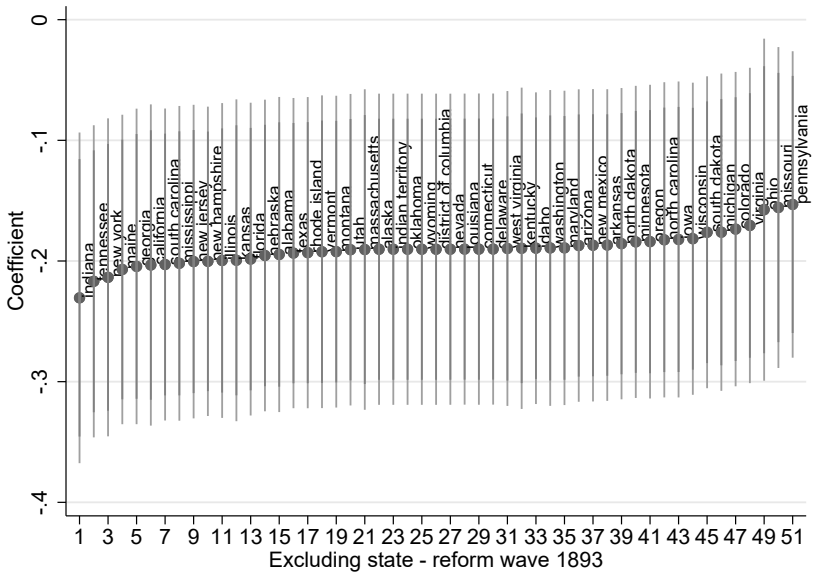
Notes: Figure compares the dynamic estimates for the stacked difference-in-differences with twoway FE panel estimates following Callaway and Sant'Anna (2021); de Chaisemartin and D'Haultfœuille (2020); Sun and Abraham (2021). The estimations of the twoway FEs are implemented using the Stata commands `csdid`, `did_multiplegt`, and `eventstudyinteract`. Reporting 95% confidence intervals.

Figure A2: Delivery errors – robustness of the 1883 reform wave results, dropping one treatment city at a time



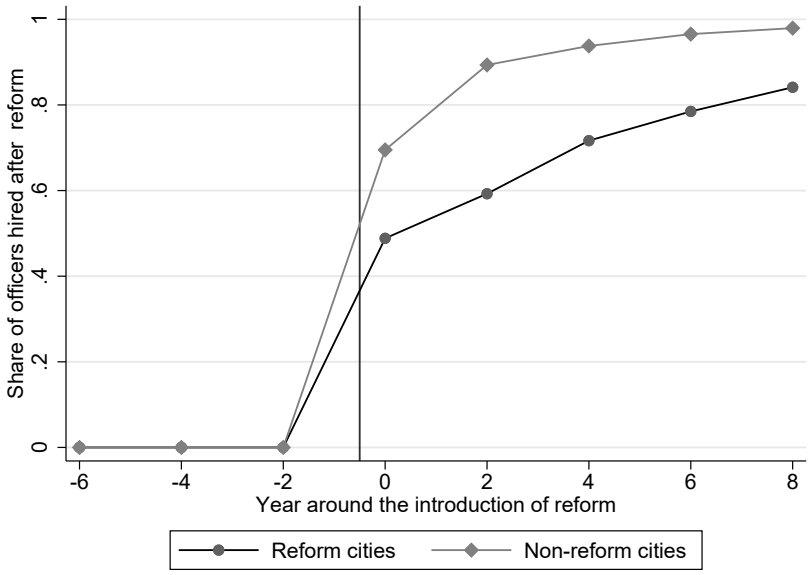
Notes: Reporting coefficients of the Reform × Post estimate, restricting the analysis to only the 1883 reform period (Table 2, column 2) and excluding each of the 23 treatment cities, one at a time. Reporting 95% confidence intervals with light gray vertical lines, and 90% confidence intervals with dark gray lines. Standard errors clustered at the city-level.

Figure A3: Delivery errors – robustness of the 1893 reform wave results, dropping one state at a time



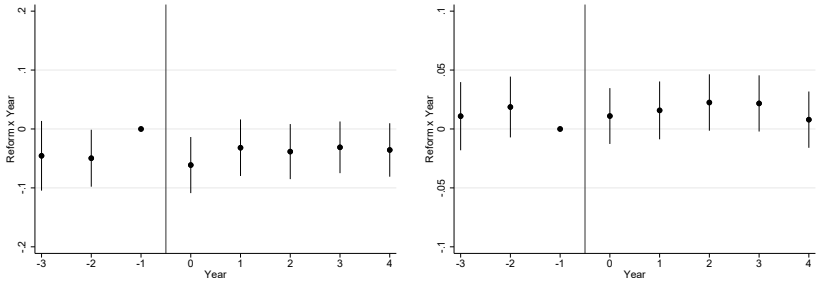
Notes: Reporting coefficients of the Reform × Post estimate, restricting the analysis to only the 1893 reform period (Table 2, column 2) and excluding each state one at a time. Reporting 95% confidence intervals with light gray vertical lines, and 90% confidence intervals with dark gray lines. Standard errors clustered at the city × reform-wave level.

Figure A4: Share of civil servants hired after the reform event, by reform status



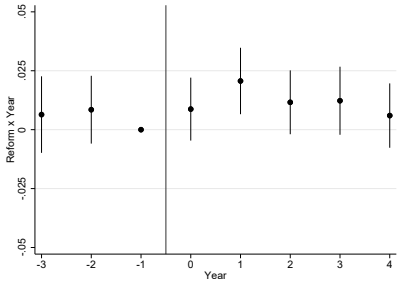
Notes: Figure shows the share of civil servants who were hired after the reform event (i.e., prior to 1883 or 1893, respectively) in reformed and unreformed cities. The year of entry is measured as the first year in which a given individual is observed in the personnel data. The outcome is shown relative to the year after the introduction of the reform.

Figure A5: Individual-level characteristics of hires and civil service reform

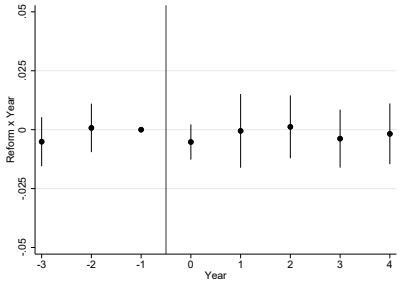


(a) Same state

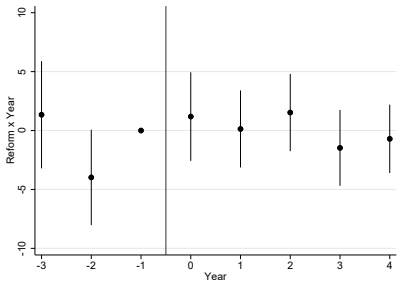
(b) Foreign-born



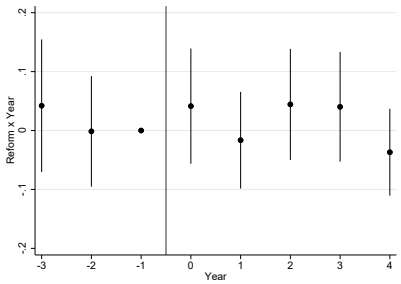
(c) German



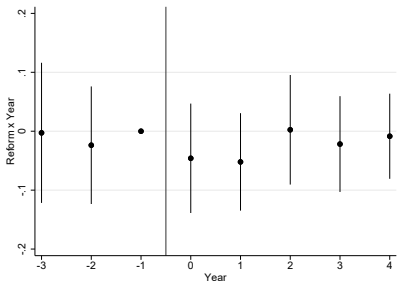
(d) Irish



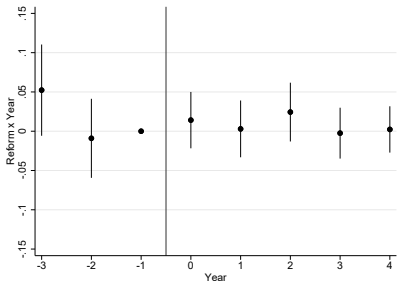
(e) Age



(f) Literacy



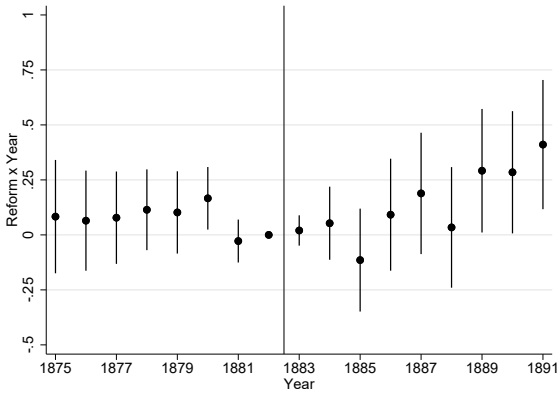
(g) Female



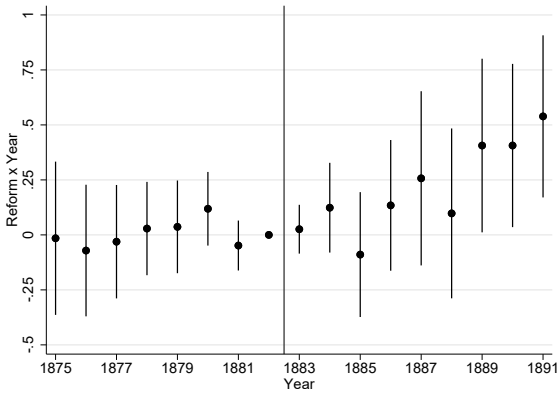
(h) White

Notes: Figure reports estimates from an augmented version of Equation 1 (corresponding to Table 5), where the estimated difference between the treatment and control cities is allowed to vary for each year around the introduction of the reform. Outcome for each estimation listed below each of six panels. Reporting 95% confidence intervals. Standard errors clustered at the city $\times$ reform year-level.

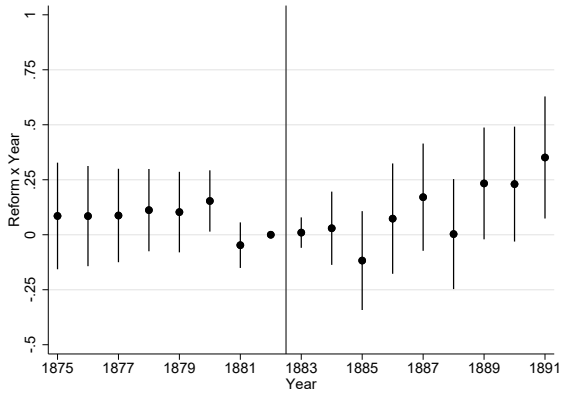
Figure A6: Total mail volume, collections, deliveries and civil service reform (1883 reform wave)



(a) Total mail volume



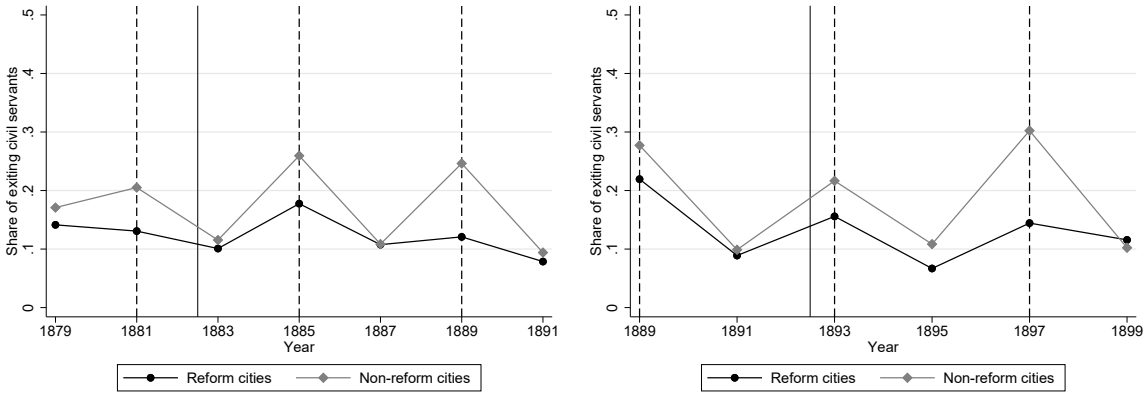
(b) Collected



(c) Delivered

Notes: Figure reports estimates from an augmented version of Equation 1 (corresponding to Table 6, columns 2–4), where the estimated difference between the treatment and control cities is allowed to vary for each year around the introduction of the reform. Outcome for each estimation listed below each of 3 panels. Reporting 95% confidence intervals. Standard errors clustered at the city-level.

Figure A7: Exit rate for reform vs. non-reform cities around the reform, 1883 and 1893 reform waves

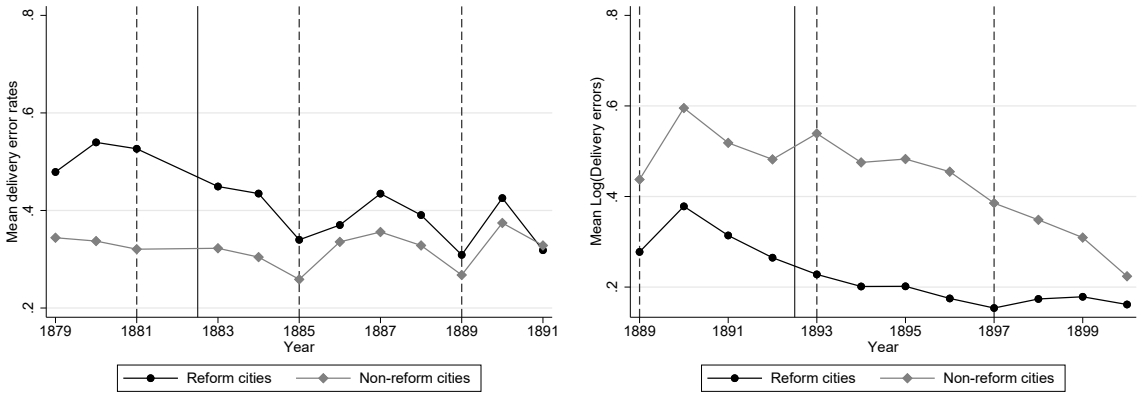


(a) 1883 reform wave

(b) 1893 reform wave

Notes: Figure shows the raw share of civil servants who exit the postal service in a given year. Exit is defined as observing an individual for the last time in the personnel data. Panel (a) focuses on the 1883 reform wave, and Panel (b) focuses on the 1893 reform wave. Dashed lines mark presidential election years and the solid line marks the reform.

Figure A8: Delivery error rates for reform vs. non-reform cities, 1883 and 1893 reform waves

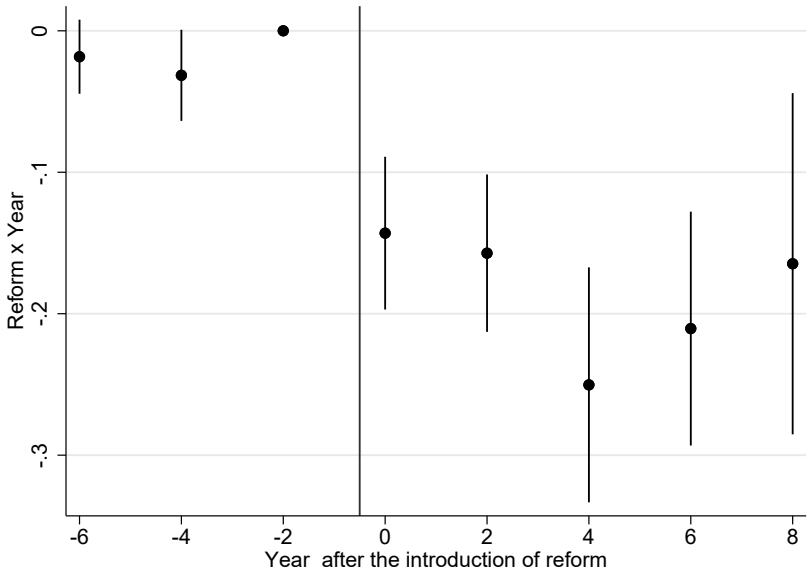


(a) 1883 reform wave

(b) 1893 reform wave

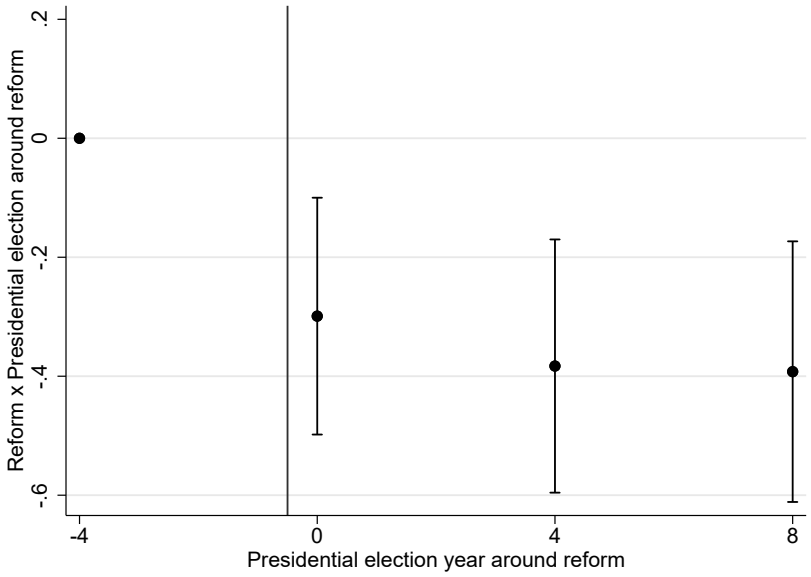
Notes: Figure shows the raw mean delivery error rates in a given year. Panel (a) focuses on the 1883 reform wave, and Panel (b) focuses on the 1893 reform wave. Dashed lines mark presidential election years and the solid line marks the reform.

Figure A9: Civil service reform and exit, pre-reform entrants



Notes: Figure reports estimates from an augmented version of Equation 1 (corresponding to Table 9, column 2), where the estimated difference between the treatment and control cities is allowed to vary for each year around the introduction of the reform. Reporting 95% confidence intervals. Standard errors clustered at the city × reform year-level.

Figure A10: Number of political newspapers in reform vs. non-reform cities around reform years



Notes: Figure reports estimates from an augmented version of Equation 1 (corresponding to Table 10, column 1), where the estimated difference between the treatment and control cities is allowed to vary for each year around the introduction of the reform. Reporting 95% confidence intervals. Standard errors clustered at the city × reform year-level.

Table A1: Descriptive statistics of reformed and unreformed post-offices – additional characteristics

	(1)	(2)	(3)	(4)	(5)
	Mean	Difference treatment-control in reform wave			
	control	1883	1894-1892	1893	Pooled
County-level (all in 100%)					
Share of federal government workers	0.186	0.202	-0.017	0.057	0.060
		(0.210)	(0.046)	(0.044)	(0.040)
Share of postal workers	0.069	0.020	0.020	0.010	0.011
		(0.016)	(0.006)	(0.002)	(0.002)
Share of state government workers	0.144	0.136	0.078	0.021	0.032
		(0.019)	(0.009)	(0.004)	(0.004)
Share of telephone workers	0.003	0.013	0.012	0.003	0.004
		(0.002)	(0.002)	(0.000)	(0.000)
Share of telegraph workers	0.067	0.042	0.038	0.032	0.033
		(0.010)	(0.010)	(0.003)	(0.002)
Share of railway workers	0.934	0.080	0.617	0.350	0.354
		(0.132)	(0.156)	(0.054)	(0.049)
Share of education workers	0.761	-0.172	0.008	0.176	0.143
		(0.028)	(0.038)	(0.017)	(0.016)
Total number of counties	1,122	477	1,050	994	1,134
- of which with treatment:	0	23	28	431	464

Notes: Table reports additional census characteristics. Column 1 shows the mean for the unreformed (control) cities. Columns 2-4 show the difference between reformed and unreformed cities. Column 5 shows the pooled difference, conditional on reform wave FEs. Observation counts report the maximum number of counties. See Appendix B for a description of the data sources. Robust standard errors are reported in parentheses.

Table A2: Robustness of inference to alternative clustering of standard errors – all outcomes

	(1)	(2)	(3)	(4)	(5)	
		Estimated standard error (SE) clustering at:				
Dependent variable	Coefficient estimate	City × Reform-wave	City	County × Reform-wave	County	
Log(Number of delivery errors)	-0.200	(0.0804)	(0.0803)	(0.0819)	(0.0815)	Table 2, Column 3
Log(Volume/carrier)	0.137	(0.0683)		(0.0689)		Table 3, Column 2
Log(Cost/volume)	-0.129	(0.0684)		(0.0690)		Column 4
Log(Postal staff)	0.017	(0.0406)	(0.0405)	(0.0402)	(0.0401)	Table 4, Column 2
Delivery errors/staff	-0.057	(0.0222)	(0.0219)	(0.0220)	(0.0217)	Column 4
Same state	-0.014	(0.0144)	(0.0130)	(0.0144)	(0.0132)	Table 5, Panel A, Col 1
Foreign-born	0.007	(0.0072)	(0.0065)	(0.0073)	(0.0067)	Column 2
German	0.007	(0.0041)	(0.0035)	(0.0042)	(0.0036)	Column 3
Irish	-0.001	(0.0035)	(0.0033)	(0.0035)	(0.0032)	Column 4
Age	0.826	(1.0379)	(0.9325)	(1.0476)	(0.9311)	Panel B, Column 1
Literacy	-0.002	(0.0267)	(0.0243)	(0.0254)	(0.0227)	Column 2
Female	-0.015	(0.0263)	(0.0241)	(0.0268)	(0.0245)	Column 3
White	-0.000	(0.0122)	(0.0121)	(0.0122)	(0.0118)	Column 4
Log(Total volume)	0.129	(0.1302)		(0.1298)		Table 6, Column 2
Log(Collected)	0.257	(0.1658)		(0.1675)		Column 3
Log(Delivered)	0.104	(0.1205)		(0.1201)		Column 4
Exit rate	-0.130	(0.0532)	(0.0533)	(0.0532)	(0.0532)	Table 7, Column 2
Mean experience	0.779	(0.1376)	(0.1376)	(0.1375)	(0.1375)	Column 5
Error rate	-0.075	(0.0414)	(0.0410)	(0.0413)	(0.0409)	Table 8, Column 1
Log(Volume/carrier)	0.194	(0.0717)		(0.0717)		Column 2
Log(Cost/volume)	-0.157	(0.0731)		(0.0720)		Column 3
Exit	-0.160	(0.0216)	(0.0243)	(0.0217)	(0.0243)	Table 9, Column 2
Newspaper number – political	-0.195	(0.0786)	(0.0788)	(0.0781)	(0.0782)	Table 10, Column 1
Newspaper number – independent	0.085	(0.0426)	(0.0431)	(0.0425)	(0.0429)	Column 2
Circulation – political	-0.266	(0.7787)	(0.7822)	(0.8119)	(0.8177)	Column 3
Circulation – independent	0.800	(0.4774)	(0.4778)	(0.4784)	(0.4790)	Column 4
Circulation share – political	-0.044	(0.0252)	(0.0260)	(0.0257)	(0.0264)	Column 5

Notes: Summary table that shows for each outcome reported in the paper the robustness of the standard errors to alternative levels of clustering. Each row summarizes the results for one outcome. Column 1 shows the point estimate of the main coefficient of interest. Columns 2–5 report the different standard errors. Column 2 is the preferred level of clustering.

Table A3: Civil service reform and delivery errors – robustness to alternative samples (I)

	(1)	(2)	(3)
	Log(Number of delivery errors)		
Mean of dep. var	4.459	4.285	5.321
Reform × Post	-0.194	-0.149	-0.234
	(0.063)	(0.058)	(0.191)
Reform wave × City FEs	✓	✓	✓
Reform wave × Year × State FEs	✓	✓	✓
Reform wave × Year FEs × Postal employment	✓	✓	✓
Sample	1883 & 1893	1883–1893	Free delivery
Observations	18,856	84,008	8,338

Notes: Relating delivery errors to the civil service reform in a stacked event-study design, centered around each reform year. Column 1 corresponds to the baseline specification in Table 2. The sample in Column 2 includes the 29 cities that become reformed between 1884–1892 due to having (endogenously) passed the 50-employees threshold. Column 3 restricts the sample to cities that have free postal delivery services during our study period. The unit of observation is the reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Standard errors clustered at the city × reform-wave level.

Table A4: Civil service reform and expansion of complementary infrastructure

	(1)	(2)	(3)	(4)
	Log Δ employment 1880-1900			
	Telegraph		Railway	
Mean of dep. var	0.0255	0.0255	0.400	0.400
Reform × Post	0.001	0.004	0.096	0.102
	(0.003)	(0.004)	(0.066)	(0.086)
Reform wave × State FEs	✓	✓	✓	✓
Reform wave × Postal employment	✓		✓	
Reform wave × PDS controls		✓		✓
Observations	1,669	1,669	1,669	1,669

Notes: Long regression relating changes in (log) employment among telegraph (columns 1-2) and railway (columns 3-4) workers to the expansion of the civil service reform. The employment numbers are measured for the county in which a city is located, and are computed based on the full-count Decennial Censuses for 1880 and 1900. The unit of observation is the reform wave × city. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Standard errors clustered at the city × reform-wave level.

Table A5: Expansion of postal inspectors and civil service reform

	(1)	(2)	(3)
	Number of inspectors		Any inspector
Mean of dep. var	0.0309	0.0309	0.00606
Reform × Post	0.012	0.006	-0.003
	(0.015)	(0.012)	(0.002)
Reform wave × City FEs	✓	✓	✓
Reform wave × State FEs	✓	✓	✓
Reform wave × Postal employment	✓		
Reform wave × PDS controls		✓	✓
Observations	17,980	17,980	17,980

Notes: Relating total postal employment of inspectors (inspectors of mail depredations, inspectors of money-order services, inspectors of free delivery service) to the civil service reform in a stacked event-study design. The unit of observation is the reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Column 1 includes the (time-interacted) total number of postal workers as control variable. Columns 2–3 include time-interacted controls selected via post-double-selection (PDS, see Belloni et al. (2014)). Standard errors clustered at the city × reform-wave level.

Table A6: Civil service reform and delivery errors – robustness to alternative samples (II)

	(1)	(2)	(3)	(4)
	Log(Number of delivery errors)			
Mean of dep. var	4.459	4.304	4.430	4.423
Reform 1883 × Post	-0.194	-0.191	-0.193	-0.192
	(0.063)	(0.069)	(0.064)	(0.065)
Sample	Baseline	No port cities	No reformed customs offices	No municipal reforms
Reform wave × City FEs	✓	✓	✓	✓
Reform wave × Year FEs × State FEs	✓	✓	✓	✓
Reform wave × Year × Postal employment	✓	✓	✓	✓
Observations	18,856	17,063	18,700	18,046

Notes: Relating delivery errors to the civil service reform in a stacked event-study design, centered around each reform year. Column 1 presents the results for the baseline sample. Column 2 drops all port cities (i.e., cities with a customs office). Column 3 drops all cities that also experienced civil service reform within the customs office. Column 4 drops all cities that experienced a municipal civil service reform episode during the study period (data from Rauch (1995)). The unit of observation is the reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Standard errors clustered at the city × reform-wave level.

Table A7: Salary and civil service reform

	(1)	(2)	(3)
	Log(Annual salary)		
Mean of dep. var	6.146	6.427	5.696
Reform × Post	0.053	0.045	0.058
	(0.041)	(0.041)	(0.064)
Reform wave × City FEs	✓	✓	✓
Reform wave × Experience FEs	✓	✓	✓
Reform wave × Year × State FEs	✓	✓	✓
Reform wave × Year × Job FEs	✓	✓	✓
Reform wave × Year × Controls	✓	✓	✓
Sample	Full sample	Pre-reform entrants	New hires
Observations	142,770	68,191	62,642

Notes: Relating individual-level (log) annual salary to the civil service reform in a stacked event-study design. The unit of observation is an individual × reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Column 1 reports results based on the full sample. Column 2 restricts the sample to individuals who entered before the reform, and column 3 restricts the sample to new hires. All specifications include (time-interacted) total postal employment and (log) city population as controls. Standard errors clustered at the city × reform-wave level.

Table A8: Individual-level census match rate of hires and civil service reform

	(1)	(2)	(3)
	Hire is matched to census=1		
Mean of dep. var	0.332	0.332	0.332
Reform × Post	-0.010	-0.007	-0.001
	(0.012)	(0.012)	(0.016)
Reform × City FEs	✓	✓	✓
Reform × Year × State FEs	✓	✓	✓
Reform × Year × Job FEs		✓	✓
Reform × Controls			✓
Observations	68,977	68,977	68,977

Notes: Relating a dummy for whether a recruited civil servant matches to the Decennial Census to the implementation of the civil service reform. Newly recruited civil servants are identified as workers first observed in the personnel data (source is the Official Registers series). To avoid truncation (since all workers are first observed in the earliest year of our data), we exclude the first year of our personnel records, thus covering 1879–1901. The unit of observation is an individual × reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. All specifications include (time-interacted) total postal employment and (log) city population as controls. Standard errors clustered at the city × reform-wave level.

Table A9: Civil service reform and mail delivery/collection, disaggregated by type (1883 reform wave)

Panel A: Volume delivered (in log)	(1)	(2)	(3)
	Letters	Postal cards	Newspapers
Mean of dep. var	13.68	12.15	13.19
Reform 1883 × Post	0.114	0.178	0.146
	(0.123)	(0.143)	(0.129)
Observations	2,945	2,945	2,945
Panel B: Volume collected (in log)	(1)	(2)	(3)
	Letters	Postal cards	Newspapers
Mean of dep. var	13.17	11.75	11.04
Reform 1883 × Post	0.301	0.089	0.124
	(0.162)	(0.177)	(0.233)
City FEs	✓	✓	✓
Year FEs	✓	✓	✓
Year FEs × PDS controls	✓	✓	✓
Observations	2,945	2,945	2,945

Notes: Relating mail delivered and collected through the city free delivery service to civil service reform, focusing separately on each type of mail item for the 1883 reform wave. The unit of observation is the city × year, and the sample period is 1875–1891. Reform 1883 is a dummy that is 1 if the city was covered by civil service reforms in 1883, and 0 otherwise. Post is a dummy that is 1 if the year is after civil service reform takes effect under the Pendleton Act. Standard errors clustered at the city-level.

Table A10: Delivery errors and aggregate volume, before and after the reform (1883 reform wave)

	(1)	(2)
	Log(Delivery errors)	
Mean of dep. var	8.747	5.574
Log(Aggregate volume)	1.899	-0.170
	(0.397)	(0.217)
Log(Aggregate volume) × Post	-0.515	0.309
	(0.308)	(0.225)
Sample	Reformed	Unreformed
City FEs	✓	✓
Year FEs	✓	✓
Year FEs × Postal employment	✓	✓
Difference Log(Aggregate volume) × Post	-0.825	
	(0.368)	
Observations	275	2,210

Notes: Relationship between delivery errors and aggregate mail volume, broken down by reform vs. unreformed cities (columns 1–2), before and after federal civil service reform. The sample period is 1875–1891. The unit of observation is the city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform under the Pendleton Act, and 0 otherwise. Post is a dummy that is 1 if the year is after civil service reform takes effect under the Pendleton Act. Standard errors clustered at the city-level.

B Data sources of covariates

Table B1: Description of baseline covariates

Variable	Description	Source
Post office staff	Number of post officers (clerks, carrier, postmaster) in city	US Official Registers (1883)
City-level population	Total city/town population	Ruggles et al. (2024)
Land-grant universities	Number of land-grant universities in county	IPED (2022)
Western Union office	Dummy for whether a city has a Western Union office in 1874	Western Union (1874)
Year post office opened	The year the post office was established in city	US GPO (1883)
Southern state	AL, AR, DE, FL, GA, KY, LA, MD, MS, MO, NC, OK, SC, TN, TX, VA, WV	Own calculation
Distance to D.C.	Distance (in miles) between town/city and D.C.	Own calculation
Latitude	Town/city latitude	Google (2024)
Longitude	Town/city longitude	Google (2024)
County-level population	Total county population	Haines (2006))
Foreign-born share	Sbare of foreign-born in the county	Bazzi et al. (2020)
Urban share	Urban population share in county	Bazzi et al. (2020)
Non-white share	County-level share of non-white population	Ruggles et al. (2024), Bazzi et al. (2020)
Frontier county	Dummy for whether a county is a "frontier county"	Bazzi et al. (2020)
Railway	Dummy for whether a county has railroad access	Bazzi et al. (2020)
Canal	Dummy for whether a county has canal access	Bazzi et al. (2020)
Manufacturing establishments	County-level share of manufacturing establishments	Ruggles et al. (2024), Haines (2006)
Share literate	Share of literate in the county	Ruggles et al. (2024)
Labor force participation rate	Share of county population in labor force	Ruggles et al. (2024)
Occupational income score	County-level average occupational income score	Ruggles et al. (2024)
Share of Democrat votes	County-level Democrat congressional vote share	Clubb et al. (2006)
Share of Republican votes	County-level Republican congressional vote share	Clubb et al. (2006)
Turnout	County-level turnout	Clubb et al. (2006)
Number of party switches	Share of elections between 1872-1882 in which county's majority party changes	Clubb et al. (2006), own calculation

Variable	Description	Source
Share of workers in education	County-level share of workers in educational services (IND1950=888)	Ruggles et al. (2024)
Share of workers in federal government	County-level share of workers in federal public administration (IND1950=916, excluding postal service)	Ruggles et al. (2024)
Share of workers in state government	County-level share of workers in state public administration (IND1950=926)	Ruggles et al. (2024)
Share of workers in telephone	County-level share of workers in telecommunications – telephone (IND1950=578)	Ruggles et al. (2024)
Share of workers in telegraphy	County-level share of workers in telecommunications – telegraph (IND1950=579)	Ruggles et al. (2024)
Share of workers in railway	County-level share of workers in railroads and railway express service (IND1950=506)	Ruggles et al. (2024)
Share of workers in post office	County-level share of workers in postal service (IND1950=906)	Ruggles et al. (2024)

Notes: Summary description of all covariates (see also Table 1 and Appendix Table A1) and their data sources.

Table B2: Post-double-selection covariates for each regression

Table	Column	Baseline covariates selected via Post-Double-selection (Total #)
Table 2	Column 3	Post office staff, Log(City-level population), Share of workers in education, Western Union office (4)
	Column 4	Post office staff, Log(City-level population), Share of workers in state government, Manufacturing establishments (4)
	Column 5	Post office staff, Log(City-level population), Share of employees in education (3)
Table 3	Column 2	Post office staff, Log(City-level population) (2)
	Column 4	Post office staff, Log(City-level population) (2)
Table 4	Column 2	Log(City-level population), Log(County-level population), Share of employees in education (3)
	Column 4	Log(City-level population), Share of employees in education, Western Union office (3)
Table 6	Columns 2, 4	Post office staff, Log(City-level population), Share of employees in railway (3)
	Column 3	Post office staff, Log(City-level population) (2)
Table 7	Column 2	Post office staff, Manufacturing establishments (2)
	Column 3	Post office staff, Urban share, Share of employees in telephone, Log(County-level population), Manufacturing establishments (5)
	Column 6	Post office staff, Year post office opened, Log(City-level population), Manufacturing establishments (4)
Table 8	Column 1	Post office staff, Log(City-level population) (2)
	Column 2	Post office staff, Log(City-level population), Share of workers in telephone, Share of workers in railway (4)
	Column 3	Post office staff, Log(City-level population), Share of workers in telephone (3)
Table 10	Columns 1-2	Post office staff, Log(Occupational income score), Western Union office, Log(City-level population), Log(County-level population) (5)
	Column 3	Post office staff, Log(Occupational income score), Western Union office, Log(City-level population), Log(County-level population), Manufacturing establishments (6)
	Column 4	Post office staff, Log(Occupational income score), Western Union office, Canal, Log(Distance to D.C.), Log(City-level population), Log(County-level population), Manufacturing establishments (8)
	Column 5	Post office staff, Log(Occupational income score), Western Union office, Log(City-level population), Log(County-level population) (5)

Notes: Covariates selected via Post-Double-selection (Belloni et al., 2014) for each regression specification.

C Census linking

We match the personnel records from the Official Registers of the United States (the "Registers") to the U.S. full count Decennial Census to obtain additional individual background characteristics. Each postal worker is matched based on the full name (first name, middle name/initial, last name), birth state, and current state of employment. We proceed by using different combinations, successively relaxing the matching restrictions:

- Step 1: First name + middle name + last name + birth state + current state
- Step 2: First name + middle name initial + last name + birth state + current state
- Step 3: First name + last name + birth state + current state
- Step 4: First name + last name + birth state

Given the limited number of variables that are available for matching, we pursue a conservative approach to ensure we do not overmatch by linking incorrect individuals (i.e., false positives). To start with, we always match individual names exactly. Second, we discard candidate matches of Census respondents who were younger than 18 and older than 65 when they are observed in the personnel records. Third, we restrict our matches to only individuals who are uniquely matched to the Census.

Although the Census data should, in principle, allow nearly every postal worker to be matched, match rates obtained through automated linking methods during this historical period rarely exceed 30–40% (Abramitzky et al., 2021). In this setting, there are multiple reasons why a postal worker may not be matched to the Decennial Census. First, transcription errors may occur both in the personnel records and the historical census data. Second, name variations may exist in the Decennial Censuses (e.g., Rick vs. Richard). Third, postal workers with common names residing in populous states will often have multiple potential counterparts in the census (e.g., John Smith from New York), making it difficult to identify the correct individual in the absence of unique identifiers such as social security numbers. Fourth, since the Decennial Census data is only available at a decadal frequency, individuals may have passed away or migrated between the year they were recorded in the personnel records and the year the census was taken. To increase the odds of finding individuals in the census data, we thus link each individual observed in the personnel record to the U.S. Decennial Censuses of 1880 and 1900.[37] Overall, we obtain a match rate of 34%. This match rate is comparable to those obtained in related census-linking exercises (Abramitzky et al., 2021; Aneja and Xu, 2021; Moreira and Perez, 2022a,b)

[37] While aggregate data exists for the 1890 Decennial Census, the micro-level data for the 1890 U.S. Decennial Census is unavailable as the records were destroyed in a fire in 1921.

Table C1: Characteristics of Census-linked vs. non-linked workers

	(1)	(2)	(3)	(4)
	Mean characteristics		Differences	
	Matched	Unmatched	Raw	Conditional
Panel A: Individual characteristics				
Log(Salary)	5.81	5.83	-0.025	0.016
			(0.007)	(0.007)
Clerk	0.59	0.60	-0.011	-0.006
			(0.002)	(0.002)
Same state	0.53	0.67	-0.141	-0.147
			(0.002)	(0.002)
Foreign-born	0.11	0.11	0.008	0.011
			(0.002)	(0.002)
German	0.03	0.03	0.001	0.002
			(0.001)	(0.001)
Irish	0.02	0.02	0.001	0.002
			(0.001)	(0.001)
Panel B: City-level characteristics				
Post office staff	143.80	182.09	-38.284	-25.967
			(1.660)	(1.514)
Log(City population)	10.57	10.81	-0.231	-0.137
			(0.011)	(0.010)
Observations	65,499	126,304	191,803	191,803

Notes: Column 1 shows the mean for the postal workers who could be matched to the Decennial Census. Column 2 shows the mean for the postal workers who could not be matched to the Census. (control) cities. Column 3 shows the raw mean difference between matched vs. unmatched cities. Column 4 shows the mean difference between matched vs. unmatched cities, conditional on year FEs. Observation counts report the maximum number of observations. See Appendix B for a description of the data sources. Robust standard errors are reported in parentheses.

In Appendix Table C1, we compare the traits of Census-linked individuals to those who were not linked. We report differences in means both unconditionally (column 3) and conditional on year FEs (column 4). While matched and unmatched postal workers significantly differ on many observable characteristics, these differences are, in terms of magnitude, relatively small. For example, the raw mean salary difference between matched and unmatched workers is only 1.6%, and differences across other individual characteristics are likewise economically small (Panel A). In Panel B, we report the mean characteristics of the cities in which the matched and unmatched postal workers work. Match rates are significantly higher in smaller post offices and cities.

The observed differences in the characteristics of matched and unmatched officers shown in Appendix Table C1 raise the question whether selection can affect our findings on worker quality (Table 5). If the match rate is significantly associated with the reform rollout, for example, differences in match rates may partly mask any actual change in the characteristics of post-reform hires. Reassuringly, however, we do not find that the match rate is significantly correlated with the rollout of the civil service reform (Appendix Table A8). Finally, we can use inverse probability weights (IPW). IPW is a non-parametric procedure by which individual observations are re-weighted according to the estimated probability that they are part of the matched sample. IPW purges

estimates of selection bias provided that selection is well captured by observable characteristics. Appendix Table C2 shows the reweighted results based on the observed characteristics of Appendix Table C1. As the table shows, the results remain comparable and we do not observe significant changes in the characteristics of hired officers post-reform. In terms of point estimates, the magnitudes remain economically small.

Table C2: Individual-level characteristics of hires and civil service reform – IPW reweighting

	(1)	(2)	(3)	(4)
	Age	Literacy	Female	White
Mean of dep. var	28.40	0.864	0.114	0.966
Reform × Post	0.906	0.001	-0.016	-0.002
	(1.058)	(0.027)	(0.027)	(0.013)
Reform wave × Year × State FEs	✓	✓	✓	✓
Reform wave × Year × Job FEs	✓	✓	✓	✓
Reform wave × City FEs	✓	✓	✓	✓
Reform wave × Controls	✓	✓	✓	✓
Observations	22,465	22,465	22,465	22,465

Notes: Relating individual-level characteristics of recruited civil servants to the implementation of the civil service reform, using inverse probability weights (IPW). The Census-linked sample is reweighted to be representative of the population in terms of salary, occupation (clerk/carrier), being born in the same state as the state of service, and being foreign-born. Newly recruited civil servants are identified as workers first observed in the personnel data. To avoid truncation (since all workers are first observed in the earliest year of our data), we exclude the first year of our personnel records, thus covering 1879–1901. The unit of observation is an individual × reform wave × city × year. Reform is a dummy that is 1 if the city was covered by the civil service reform in the reform wave, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. All specifications include (time-interacted) total postal employment and (log) city population as controls. Standard errors clustered at the city × reform-wave level.