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STRENGTHENING STATE CAPACITY:
POSTAL REFORM AND INNOVATION DURING THE GILDED AGE

Abhay Aneja
Guo Xu

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Strengthening State Capacity: Postal Reform and Innovation during the Gilded Age
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ABSTRACT

We use newly digitized records from the U.S. Post Office to study how strengthening state capacity affects public service delivery and innovation in over 2,700 cities between 1875–1905. Exploiting the gradual expansion of a major civil service reform, cities with a reformed postal office experience fewer errors in delivery, lower unit costs, and an increase in mail handled per worker. This improvement goes with greater information flow, as measured by increased volumes of mail and newspapers. We use personnel data to show that reformed offices see a decline in turnover and an increase in merit-based retention, consistent with a reduction in political interference. We observe more joint patenting involving inventors and businesses from different cities, suggesting that a more effective postal service contributed to innovation and growth during the Gilded Age.

Abhay Aneja
University of California at Berkeley
School of Law
225 Bancroft Way
Berkeley, CA 94720
aneja@berkeley.edu

Guo Xu
Haas School of Business
University of California at Berkeley
2220 Piedmont Avenue
Berkeley, CA 94720
and NBER
guoxu@haas.berkeley.edu

1 Introduction

State capacity is a key driver of development and growth ([Besley and Persson, 2009](#)). A long tradition of economic thought considers an effective bureaucracy synonymous with high state capacity. Yet, there is relatively little causal evidence on how improvements in bureaucratic effectiveness can affect aggregate outcomes.

In this paper, we show that an effective bureaucracy has first-order impacts on the quality of public service delivery, and ultimately aggregate economic outcomes. We study how a series of civil service reforms during the Gilded Age affected the performance of a key bureaucracy – the U.S. Post Office (today, the Post Office is known as the United Postal Service or USPS). The Post Office was established in 1792, with President George Washington’s signing of the Postal Service Act. The Post Office was – and remains – the largest civilian bureaucracy in the U.S. During the 19th century, a period of territorial expansion, postal service connected distant locations across the nation through an intricate network of offices and sorting facilities staffed by mail carriers, clerks, and postmasters. The development of a network of post offices enhanced communication between far-flung individuals and communities, advancing the idea of nationhood ([John, 1995](#)). In addition to projecting the power of the American state ([Acemoglu and Robinson, 2019](#); [Chong et al., 2014](#)), the Post Office contributed to making America a developed economy ([Rogowski et al., 2021](#); [Feigenbaum, 2015](#)).

The policy intervention we study is part of a series of major reforms to the administrative state spurred by the landmark Pendleton Act of 1883. The Act introduced merit-based hiring, shielded bureaucrats from political interference and arbitrary firing, and abolished mandatory political contributions from government employees ([Johnson and Libecap, 1994](#); [Hoogenboom, 1959](#)) – in effect increasing state capacity by introducing “Weberian” management practices, nowadays considered a hallmark of good governance ([Weber, 1922](#); [Evans and Rauch, 1999, 2000](#); [Besley et al., 2021](#)). The Act triggered a gradual professionalization of the American federal bureaucracy to phase out the pernicious effects of the “spoils” system ([Theriault, 2003](#)). An initial set of congressional reforms under the original 1883 statute affected postal personnel in 23 major cities. Subsequently, federal merit-based personnel policies spread to the post offices of over 500 cities under a Presidential order issued in 1893. To isolate the causal impact of civil service policy changes on federal government performance and downstream economic outcomes, we thus use this staggered roll-out of civil service reform in a difference-in-differences design to compare places that were reformed to those that remained (yet) unreformed.

Underpinning our study is a large-scale digitization of historical performance and personnel records of the U.S. Post Office. We draw upon Annual Reports of the United States Civil Service Commission (CSC) to trace out the gradual expansion of the reform across more than 500 cities. We link the timing of the personnel policy reform to annual post office outcome data from the Annual Reports of the Postmaster General and the Annual Reports of the Superintendent of the Railway Mail Service (RMS). These data allow us to study how the postal reform affected errors in the delivery of mail across 2,730 cities from 1875-1905, as well as detailed measures of cost

efficiency and per unit productivity. To shed light on how the civil service reform affected the postal workforce, we also digitized individual-level data on the universe of clerks and carriers from the Official Registers of the United States for 1877–1901. This data contains rich details on the assignment, career progression, and background characteristics of postal workers. Finally, we use patent data to study how greater communication owing to a more effective postal service affected the pace and spatial distribution of patenting.

Our main findings are that the postal reform improved government performance and economic outcomes. First, relating delivery errors to the staggered expansion of the postal reform, we find a marked decrease in the number of delivery errors by 11%, consistent with an increase in the reliability of the postal service. Second, when examining the mechanisms through the lens of the urban free delivery service, we find that the improved performance increases the overall volume of mail delivered *and* collected, suggesting greater demand for the postal service and an increase in information transmission through newspapers, postal cards, and letters. Importantly, we find an increase in the efficiency of postal delivery: we observe an increase in the amount of mail handled per worker, as well as a decrease in unit costs per mail handled. Making use of individual-level personnel records, we also show that reformed cities experience a sharp decline in turnover. This reduction in turnover is especially pronounced during electoral years, consistent with a reduction in political interference. Lastly, we also find evidence that higher-quality bureaucrats are less likely to exit post-reform. The combined evidence thus points to a personnel channel through which civil service reform increased organizational performance.

The considerable improvement in postal service delivery raises the question of whether it was conducive to knowledge-based growth. Cities with reformed post offices were more likely to be exposed to more information and ideas. Moreover, filing patent applications with the federal government was likely easier given a more reliable postal service. We thus consider the link between state capacity and productive economic activity by considering the effects on innovation (Acemoglu et al., 2016; Hanlon et al., 2022). Using patent-level data that includes the timing, location, and parties involved in each patent granted, we find that classified civil service cities experienced a marked increase in patenting. Strikingly, these patents are more likely to be assigned to businesses (which maintain the property right) and involve inventors and firms from multiple cities. Using a gravity model to estimate patenting intensity for pairs of cities across the U.S., we find a significant differential increase in patenting for cities that were both reformed, partly attenuating the negative impact of distance. Taken together, these findings suggest that the postal reform not only spurred more innovative activity but also changed the geography of innovation during the Gilded Age by connecting inventors and businesses from distant locations. Our results thus provide evidence that a key public institution – the U.S. Post Office – was a major driver of growth, facilitating innovation in the late 19th century. Importantly, we provide evidence that strengthening state capacity through Weberian reforms can have first-order effects on not just the quality of public service delivery. Ultimately, though, we also show that improvements in government service delivery are likely to have macroeconomic implications for productivity growth and technological change.

Our paper has implications for research at the intersection of economic history, the political economy of public service delivery, and innovation. First, we contribute to the literature on the consequences of public sector reform in the U.S. (Lineberry and Fowler, 1967). This literature assesses the political economy aspects of state and municipal-level civil service reforms (Folke et al., 2011; Bostashvili and Ujhelyi, 2019), as well as the impact of such reforms on policy and performance outcomes (Rauch, 1995; Ujhelyi, 2014; Ornaghi, 2019).¹ Our study extends this work by studying the effect of federal reform, which spread across federal government agencies at the turn of the 19th century, beginning with the passage of the Pendleton Act (Hoogenboom, 1959; Carpenter, 2000; Johnson and Libecap, 2007; Theriault, 2003). In contrast to the robust literature on the effects of civil service reform at the subnational level, there is less study of the real effects of the nation’s original signature set of reforms transforming the federal bureaucracy from a patronage-ridden workforce into a modern bureaucracy. More recent work studying the Pendleton Act has focused on the first wave of reforms implemented in D.C. (Moreira and Perez, 2022), or in the Treasury Department’s customs division (Moreira and Perez, 2020).² We complement these studies by providing evidence from the Post Office, a government function synonymous with state presence (Ellis and Ellis, 1958; Carpenter, 2000; Geloso and Makovi, 2020; Rogowski et al., 2021).³ Previous work documents the effects of postal expansion to areas of the country that were previously untouched by this core state function (Perlman and Schuster, 2016; Acemoglu et al., 2016). We add to this literature by considering the impact of intensive-margin improvements to personnel within the Post Office. By focusing on this elemental state agency which has for two centuries been the largest civil employer within the federal government, our analysis spans 2,730 cities from all corners of the nation. Our examination also leverages not only the changes brought about by the Pendleton Act, but later reform waves that implemented civil service rules in many more cities through the end of the 19th century – allowing us to observe a total of 599 cities that had classified post offices in our sample. Finally, by considering downstream economic outcomes we provide new evidence on the economic importance of bureaucratic performance.

Second, we contribute to the literature on bureaucracy and development (Besley et al., 2021). There is by now a large body of work that studies how personnel 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 merit-based incentives (Muralidharan and Sundararaman, 2011; Khan et al., 2015, 2019; Leaver et al., 2021; Deserranno et al., 2021) and demonstrated how opaque links between effort and reward reduce performance (Xu, 2018; Bertrand et al., 2019; de Janvry et al., 2020). Evidence on how merit-based selection affects recruitment

¹At the local and state levels, merit-based recruitment and civil service protections were originally introduced to improve local government hiring. These changes reflected a view that “[t]he field of administration is a field of business. It is removed from the hurry and strife of politics.” (Wilson, 1887, p. 209). The seminal work of Rauch (1995), for example, documents how Progressive Era-civil service reform produced greater investment in roads, sewerage, and water infrastructure at the city level. Similarly, Ujhelyi (2014) and Ornaghi (2019) find that civil service reforms in the mid-20th century affected overall government spending and improved policing, respectively.

²Moreira and Perez (2020) study the implementation of the 1883 Pendleton Act across 11 reformed ports, finding improvements in the qualification of recruited individuals but no impact on revenue collection. In a companion paper, Moreira and Perez (2022) study how the 1883 Act affects the composition of recent hires in executive departments in D.C., documenting an increase in “educated outsiders” at the expense of individuals from disadvantaged backgrounds.

³Moreover, the Post Office has for generations served as an engine of social mobility (Boustan and Margo, 2009).

and performance, however, is scarcer – in part, owing to the difficulty of measuring performance. While most of the work finds meaningful impacts on the observed composition of merit hires (Moreira and Perez, 2020; Mocanu, 2022), the impacts on performance are mixed, ranging from non-merit-based or discretionary methods comparing favorably to merit-based allocation rules (Weaver, 2021; Voth and Xu, 2022) to no effect (Moreira and Perez, 2020) and positive impacts (Dahis et al., 2022; Mehmood, 2021). We complement this literature by taking a “holistic” view, providing evidence on how a package of merit-based reform measures affects organizational performance and aggregate outcomes. The reform we study – arguably one of *the* textbook civil service reforms in Western history – shares many classic features of civil service reforms observed even today (World Bank, 2000). Moreover, our focus on the Post Office also allows us to measure performance in a straightforward, tractable way. We focus on delivery statistics about errors, volume, cost – all of which collectively signal how reliable information could be transmitted across the country.

Finally, we contribute to the literature on innovation and economic history (Moser, 2016). Existing research focuses on innovators over time (Cook, 2014; Petralia et al., 2016; Akcigit et al., 2017, 2016; Bell et al., 2018), as well as on the role of intellectual property systems.⁴ Researchers have in turn identified a number of law-based drivers of innovation in the U.S (Khan, 2005; Lamoreaux and Sokoloff, 1999b; Moser, 2012).⁵ Other research on the drivers of technological progress over time examines how innovative activity reacts to moments of major necessity (Rosen, 1994; Ruttan, 2006; Gross and Sampat, 2020; Hanlon, 2015), or the direction of innovation during public health emergencies (Woolliscroft, 2020). More closely related to our work is Hanlon et al. (2022), who show that the replacement of distance-based postage with a uniform fee lowered communication costs, increasing knowledge flow and innovation. Our work complements these findings by studying the role of improved government infrastructure, focusing on the role of the state in fostering innovation. To our knowledge, there is relatively little evidence of how improving state institutions – which can increase incentives to innovate by improving long-distance communication and increasing firm investments – can increase innovation. Our findings largely exist outside the canonical debate on the effects of patent protection that relate to the tradeoff between the incentives for invention and monopoly-driven welfare losses (Nordhaus, 1969), but rather provide novel evidence for how state capacity can increase productive activity within an economy.

⁴In the U.S., intellectual property rights institutions extend back to the advent of American patent law, “the world’s first modern patent institution” (Khan and Sokoloff, 2001).

⁵Khan (2005) demonstrates the role of lower patent application fees, Lamoreaux and Sokoloff (1999b) find that U.S. patenting increased in the late 19th century with the emergence of professional patent agents, and Moser (2005, 2012) documents the impact of patent protection on the direction of innovation.

2 Context and data

2.1 Context

U.S. Post Office. Few institutions have been as central to modern state building as the post office (John, 1995; Chong et al., 2014). In the U.S., Alexis de Tocqueville in 1831 described the American postal system as a marvel of its day — a “great link between minds” (Tocqueville, 1969). Initiated by the Post Office Act of 1792, the U.S. Post Office was a key institutional actor that affected growth and technological change in the U.S. (Acemoglu et al., 2016). During the 19th century, the Post Office facilitated communication and the spread of information to far-flung states and contributed to the making of an informed citizenry. Postal systems facilitated economic activity and business relationships. Indeed, business-related mail comprised a substantial portion of total mail correspondence in the 19th century (Henkin, 2008). Roper (1917) describes how the passage of money and goods “pass[ed] through the countless postal channels.” The postal service allowed sellers to advertise goods (e.g., through sales catalogs) and to complete sales (often via mail orders) without travel to and from urban centers. It also provided a system of banking and remittances, and allowed investors to find investment opportunities. In short, “[t]he government’s commitment to postal service formed part of the foundation for commercial growth.” (Henkin, 2008).

Patronage and Corruption in the U.S. Post Office. As one of the major organs of the American state, the Post Office was also the largest government employer in the 19th century. By 1816, 69% of the federal civilian workforce were postmasters and by 1841 the figure had grown to 79% (John, 1998). For good reason, the *New York Times* in 1852 described the Post Office as “the mighty arm of civil government.” 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.

However, while an important institution for fostering economic growth (Acemoglu et al., 2016), the Post Office prior to the late 19th century was rife with corruption and inefficiency (Carpenter, 2001). One likely cause of postal inefficiency was the “spoils system,” or patronage-based hiring. Until the early 20th century most federal employees – including even low-level clerks – were hired at least partially on the basis of political considerations (Johnson and Libecap, 1994). Under the patronage system that existed prior to the Pendleton Act, patronage workers were expected to be politically active on the behalf of their benefactors, engage in campaign work, and contribute part of their salaries in the form of political assessments.⁶ In New York City, for example, employment in the post office (as well as other government postings) depended on “patronage, political assessment, and partisan subservience,” and thus led to “short terms of office and easy and frequent removals” (Eaton, 1910). Postal hires nationwide were similarly politically motivated. Patronage-based hires were often influenced by congressional ties. President Abraham Lincoln, for instance, admitted making “very few nominations

⁶These government workers were also removed routinely following the electoral defeats of their political benefactors (Fowler, 1943).

to important local positions without the previous consent of the [state Congressional] delegation” (Fish, 1905). As the number of post offices nationwide grew, so did the number of employees hired (Carpenter, 1999).

The growth of government, alongside the practice of political patronage, had costs in terms of both the quality and efficiency of federal government services. In 1877, a blue-ribbon Congressional committee (known as the Jay Commission because it operated under the supervision of John Jay, a well-known civil service reformer) found that patronage hires were often absent from their posts and the quality of government work was “poor at best” (Johnson and Libecap, 1994). Many specific accounts of government ineptitude involved federal postal services. The Post Office often experienced deficits during the 19th century (Carpenter, 2001). One 19th century newspaper editor complained in a column that his paper was “exceedingly harassed, and much injured, by the long-enduring irregularity of the mails, and the excessive carelessness or gross ignorance...in a good many of the post offices” (Foley, 1997). Problems were particularly acute in cities, where patronage politics were salient. One report described “incompetency, neglect, confusion and drunkenness” in the New York Post Office, where on one occasion an incoming postmaster in New York City even discovered many bags of undelivered mail scattered throughout the building (Hoogenboom, 1959). In other government reports, complaints were made against staff in post offices for incompetence in mail delivery (U.S. House, 46th Congress, 3rd Session).

The Pendleton Act. In 1883, Congress passed the Pendleton Act, which created a merit-based civil service by replacing hiring based on favoritism with personnel decisions based on competitive exams and performance. The passing of the Act was a culmination of pro-reform sentiments that had been rising since the end of the Civil War, catalyzed by the assassination of President Garfield by a disappointed office-seeker in 1881 (Theriault, 2003).⁷ The Pendleton Act was a comprehensive statute that included other aspects aside from merit-based recruitment. The Act provided workers with protection from political removal, as they were vulnerable to being fired summarily during periods of political turnover (Masur, 2013). The policy was intended in part to open access to government jobs to qualified candidates who lacked political influence and connections.

Under the 1883 reform, the government immediately “classified” around 10 percent of federal employees as formal members of the protected civil service. Candidates for classified jobs were selected through open, competitive examinations. During the first two decades of the reform era, postal employees were “classified” based on the city in which they worked (Hoogenboom, 1959). Classified post offices (those in which clerks and carriers were subject to exams) were initially restricted to post offices with at least 50 employees – meaning that most of the positions classified were located in urban post offices. 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.⁸ The statute was designed to target the Post

⁷As Hoogenboom (1961) argues, “Garfield’s assassination gave reformers a simple, emotion-packed illustration that the previously uninterested masses could easily understand. The spoils system equaled murder.”

⁸The Civil Service Commission report of 1883 describes the choice as a means to “enforce ... broadly enough to fairly test ... without making it so general as to involve serious inconvenience in case of failure”; it was hypothesized that “abuses to be suppressed increase in

Office because the provision of postal services was the major activity of the federal government in the 19th century (Johnson and Libecap, 1994). 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 grew to 80%.⁹

Importantly, the law also delegated to the president 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).¹⁰ The practical effect of Harrison’s order was to extend 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. We exploit the gradual roll-out of the civil service reform across a decade to study the postal reform via a staggered difference-in-differences design.

2.2 Data

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

Reform rollout. The Pendleton Act established that employees in certain “classified” positions would need to be selected through open, competitive examinations (United States Civil Service Commission, 1883). To document the roll-out of the civil service reform 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 the civil service reforms, providing 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 the selected candidates. Importantly, these reports include lists of locations that were *classified* in the sense that formal civil service rules applied, allowing us to track the expansion of civil service reform within the Post Office over time. As Figure I shows, the expansion of the postal reform occurred in two big waves: the initial wave of 1883, implementing the reform in 23 large cities across the U.S., and the wave of 1893 when the postal reform was expanded to all remaining 556 cities with free delivery. In between these two waves, 30 cities became classified as they crossed the fifty-employee threshold.

Government performance measures. Our performance measures are drawn 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 First Assistant Postmaster-General and the Annual Report of the General Superintendent of the Railway

geometrical ratio with the magnitude of business in the offices.”

⁹At the same time, the number of federal jobs expanded almost fivefold to 562,255.

¹⁰Under 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). Van Riper (1958) and Skowronek (1982) emphasize the critical role played by presidents – especially progressive presidents – in invoking authority granted in the Pendleton Act to extend civil service protections.

Mail Service (RMS). Our main measure of performance captures errors in delivery, reported in the *Statement of errors in the distribution and forwarding of mails*, which are recorded in the Annual Reports of the Railway Mail Service. Historical evidence suggests that distribution errors were relatively common during the 19th and early 20th century, making them a sensible outcome for us to study (White, 1910). These delivery errors arose due to routing errors committed by post offices when directing mail to different cities.

For our main measure, we use the total number of errors on incorrect slips. The use of these error statistics has a few advantages. To start with, errors were meticulously recorded within the postal administration and frequently used to benchmark the performance of post offices. Importantly, these errors – while attributed to particular post offices – were not recorded by the offices themselves. Rather, they were recorded by agents of the RMS, which collected and helped transport the mail to be routed to distant locations. That these postal workers were not associated with the performance of a city’s post office alleviates concerns over selective 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 overall extent of delivery errors. We collect the annual statements for the period 1879–1901, covering a total of 2,730 cities in our sample.¹¹

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 (FAPG). The First Assistant Postmaster General was the deputy responsible for overseeing the Post Office’s Free Delivery Service, a postal innovation that allowed letter carriers to deliver mail to customers by hand.¹² The FAPG’s Annual Report in turn provided a statistical appendix with data 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.¹³ We use these secondary measures to complement our main performance measure and explore the mechanisms through which impacts (if any) occur. Unfortunately, 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.

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 1816–1959 (see Aneja and Xu (2021) for a more detailed description). Given the sheer size of

¹¹Unfortunately, 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.

¹²Free City Delivery Service began during the Civil War (United States Post Office Department, 1862). Joseph Briggs, a postal employee, convinced postal officials to deliver letters in Cincinnati, Ohio. The Free City Delivery Service expanded over time.

¹³Carpenter (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 1875–1905.

the U.S. Post Office, 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 are 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 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.

Patent data. To measure the downstream impact of civil service reform on real economic outcomes, we examine innovation. We focus on patents because they are a commonly used measure of innovation in the empirical literature on technological change and economic growth (Akçigit et al., 2017). To measure the economic impact of city-level postal reform, we use the *HistPat* dataset created by Petralia et al. (2016). This data includes all issued U.S. patents filed from 1790 to 1978 collected from USPTO-digitized patent images. For each patent, the data includes details about the year of registration, the inventors and assignees involved, as well as their locations. Assignees are entities – mostly businesses – that enjoy the property rights to the patents. We aggregate the patent-level data to the city-year level, constructing a city-year panel from 1875–1905. We restrict the sample to the subset of locations for which we have error data and personnel records. The resulting dataset covers a total of 400,835 patents and 2,407 locations across the U.S.

2.3 Descriptive statistics

Figure I shows the locations of all post offices that were open between 1860–1905,¹⁴ distinguishing locations by the availability of performance data (Panel (a), and by the timing of the postal reform Panel (b)). As Figure I, Panel (a) shows, post offices were ubiquitous across the entire nation: quite literally, the presence of post offices delineated the boundaries of the United States, resonating with the widely held view that in the 19th century, “the postal system *was* the central government” (John, 1998).

As Figure I, Panel (b) also shows, the rollout of the civil service reform was uneven across space but touched most parts of the nation. To formally assess the differential pattern of the rollout, Table I, Panel A compares the characteristics of cities¹⁵ targeted by the reform to those not targeted. 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 (Panel B). In our empirical analysis, we explicitly assess the extent to which these differences may affect causal inference by assessing pre-trends and comparing locations of comparable sizes.

¹⁴The data on post offices is drawn from Richard Helbock’s compilation, see Blevins and Helbock (2021).

¹⁵We use the term cities interchangeably with the term post office. Post office subsidiaries within a city are referred to as branches.

3 Postal reform and performance

3.1 Main specification and performance result

Our objective is to examine how the federal implementation of civil service rules affected postal performance, as well as whether bureaucratic improvements had downstream effects on economic outcomes. Using the data introduced above, we answer this question using a “stacked” event-study design to estimate the pooled effects of the postal reform. We begin by studying the effects on post office performance, tracking a panel of cities around each reform year (where “reform year” indicates the year in which civil service rules apply in a place – either due to statute or executive order). Let $j = \{1883, 1884, \dots, 1893\}$ denote the reform year and $k = \{-4, \dots, 10\}$ the years centered around a reform (so that $k = 0$ is the year of the reform). For city i , reform year j and the k -th year around the reform, we estimate:

$$y_{ijk} = \beta \text{treat}_{ij} \times \text{post}_{jk} + \theta_{ij} + \tau_{jk} + \varepsilon_{ijk} \quad (1)$$

where $\text{treat}_{ij} = 1$ if city i is reformed in the event year j , and 0 otherwise. The variable y_{ijk} is the outcome of interest. The indicator variable post_{jk} is defined as $\text{post}_{jk} = \mathbf{1}[k \geq j]$, taking the value 1 post-reform, and 0 before. τ_{jk} are reform-specific year fixed effects. Since cities can serve both in the treatment and control groups multiple times (e.g., Oakland, CA is untreated until 1888, serving as a comparison city for cities reformed between 1883–1887), we estimate the city fixed effects θ_{ij} separately for each reform year. To identify the causal effect of the reform, we require that reformed and unreformed cities evolve along common trends in the absence of the policy. We provide supporting evidence by investigating pre-trends and assessing the robustness of the estimates when using more restrictive comparison groups. Finally, we cluster the standard errors for the error term ε_{ijk} at the same ij -level, corresponding to the level of treatment.

[Table II](#) reports the main performance results. The dependent variable is the (log) number of delivery errors. Column 1 reports the baseline specification corresponding to [Equation 1](#). On average, reformed cities experience a reduction in delivery error rates by 11%. The result also holds – if anything increasing in magnitude – when restricting the comparison to only cities in the same state through the inclusion of state and reform-specific time fixed effects (column 2). To assess the possibility that our estimate of interest is driven by differential trends, [Figure II](#) provides visual evidence for the reform effects by reporting the estimates of the augmented [Equation 1](#) where β is allowed to vary by each year around the reform.¹⁶ As the figure shows, we observe no marked pre-trends prior to the introduction of the reform. After the introduction of postal reform, however, we observe a clear reduction in error rates in reformed cities relative to those cities that were not reformed. The gap in delivery errors opens up until the fifth year and remains large thereafter.

¹⁶We estimate $\log(y_{ijk}) = \sum_{l=-4}^0 \beta_l \text{treat}_{ij} \times \mathbf{1}[k = l] + \theta_{ij} + \tau_{jk} + \varepsilon_{ijk}$, with the year prior to the reform ($k = -1$) as the omitted category.

As [Table I](#) shows, cities affected by civil service reform differed from those that were unaffected by reform. For example, since the assignment to treatment was governed by the size of a city’s postal staff in the earlier reform waves, reformed cities are mechanically larger than unreformed cities. While the absence of pre-trends in post office delivery performance suggests that reformed and unreformed cities evolved along common trends prior to the policy taking effect, we conduct a range of additional checks to assess whether federal civil service reform’s effect is confounded by observable differences across reformed vs. unreformed cities. First, in [Table II](#), column 3, we control for the full set of time-interacted baseline characteristics shown in [Table I](#). Despite a large number of controls, we find the resulting point estimate virtually unchanged.

Second, given the knowledge of the exact assignment rule governed by post office size for the pre-1893 reform waves, we can also explicitly account for the non-random assignment by coarsely matching reformed cities to the subset of unreformed cities that are of comparable size. We do so by using coarsened exact matching ([Iacus et al., 2012](#)), matching reformed cities to control cities within each reform year based on the joint distribution of postal staff size and county size. Given the stringent matching criteria, we are able to find matched counterparts for 38% of our reform cities.¹⁷ Importantly, as [Appendix Table AI](#) shows, the resulting matched reformed and unreformed cities are much more comparable. [Table II](#), column 4 shows the reform estimates on the matched sample. Despite restricting the sample to a significantly tighter set of comparison cities with similar postal staff and county population size, we still find a significant reduction in delivery errors.

Third, we can provide an even tighter comparison group to estimate the reform effects of 1883. Since reformed cities are larger than unreformed ones, there may be a concern that locations that were reformed in our study period are very different from locations that remained unreformed throughout. To alleviate this concern, column 5 re-estimates the 1883 reform effects by restricting the set of control cities to those who were *eventually* reformed in 1893. As the estimate shows, our results remain comparable – if anything increasing in magnitude. Finally, as [Figure I](#), Panel (b) shows, the expansion of the postal reform was concentrated in two waves – the initial reforms of 1883 targeting the largest cities, and the bigger, second wave of reforms of 1893 expanding to the remaining urban areas. In [Appendix Table AII](#), we show that our results hold when estimated separately for each of the two major reform waves of 1883 and 1893.¹⁸ We also demonstrate that the results are not driven by outliers. The results hold when dropping one reform city at a time ([Appendix Figure AI](#)) for the 1883 reform wave, or even dropping each state at a time for the 1893 reform wave ([Appendix Figure AII](#)). Taken together, the results suggest that the postal reform significantly reduced the number of delivery errors.

Heterogeneity analysis. The broad roll-out of the civil service reform across the entire country (see [Figure I](#), Panel (b)) provides for substantial variation in local conditions that allows us to explore heterogeneities. We do

¹⁷We match coarsely on post office size and county population using 14 bins for each variable. The number of bins is determined by Sturge’s rule, the default option in the implementation of [Iacus et al. \(2012\)](#). Matching on comparable post office and county size implies that the biggest reformed cities and most of the smallest unreformed will be omitted due to a lack of overlap in the joint distribution.

¹⁸This robustness also addresses concerns that the pooled estimate from the staggered difference-in-differences is driven by treatment heterogeneity ([de Chaisemartin and D’Haultfoeuille, 2022](#)).

not find that the improvement in performance was significantly different in places with greater access to railroad or existing canal infrastructure (Appendix [Table AIII](#), Panel A, columns 1-2). We find that the reform effects are more muted in cities that already had a pre-existing Western Union branch in 1874, suggesting substitutability between the telegraph and postal service (column 3). Interestingly, we find that the reform effects appear to be slightly larger in places with more nascent local institutions ([Table AIII](#), Panel B). In particular, the reform effect is stronger in counties with lower tax capacity and in “frontier” counties ([Bazzi et al., 2020](#)).¹⁹ We also find that the reform effect is slightly smaller in the U.S. South, but these difference are not statistically significant.

3.2 Changes in inputs, workload, and productivity

The results above provide robust evidence that postal reform improved public service delivery as measured by the number of delivery errors. We probe deeper to shed light on the mechanisms underlying the observed effects. To that end, we focus on a subset of cities – those with free city delivery service – for which we have granular data on inputs, workloads, and measures of productivity.²⁰ Our main result on delivery errors also holds in this sample, suggesting that zooming in on this subset of the Post Office will provide generalizable evidence.

We first explore whether the reduction in delivery errors is due to an expansion of the postal labor force. As described in [subsection 2.1](#), the civil service reform went hand-in-hand with a range of alternative policies, including an increase in federal hiring. To test whether our results are driven by 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 by city aggregating the individual-level records. To obtain annual counts, we linearly interpolate between the missing years.

[Table III](#), column 1 reports the estimates based on the total personnel counts. We use the same “stacked” event study design ([Equation 1](#)), except that the dependent variable now is the (log) total postal employment. As the estimate shows, there is a significant increase in employment post-reform. Among locations that had free urban delivery, reformed cities saw an expansion of the postal staff by 14%. This expansion, however, does not fully explain the improvement in performance. In column 2, the dependent variable is the total delivery errors per worker. As the estimate shows, the reduction in delivery errors also holds a on per-worker basis.

An alternative mechanism through which performance might have improved post-reform is an overall reduction in workload. Even if overall employment levels remained comparable, the decline in error rates could coincide with a reduction in workload due to a decline in the overall mail volumes. We can use data on overall mail volumes from the Free Delivery statistics for 1875–1891 to test this channel.²¹ Inconsistent with this interpre-

¹⁹We rely here on [Bazzi et al. \(2020\)](#), who find that areas in the U.S. that were isolated on the frontier for longer periods of time during the 19th century have a persistently stronger culture of “rugged individualism.”

²⁰Recall that the city postal service facilitated free delivery of mail in urban cities starting in the 1860s (see [subsection 2.2](#)).

²¹Unfortunately, the Free Delivery statistics are only reported until 1891, preventing us to look at the later expansion of the postal reform in a comparable event study window. Our delivery results also hold for the 1883 expansion ([Table AII](#)) and for the subsample of locations for which we have Free Delivery Statistics.

tation, [Table III](#), column 3 shows an *increase* in the overall volume of mail. This increase in mail occurs across all types of mail delivered – letters, postal cards, and newspapers (Appendix [Table AIV](#)). Interestingly, we also find an increase in productivity as reflected in the higher volume of mail handled per carrier (column 4). Finally, we can use the rich data to derive a cost-based measure of productivity in column 5. We find that the reform leads to a decrease in the per-unit cost of delivering mail. [Figure III](#) shows the corresponding visual evidence for the productivity measures. As before, there is a marked relative improvement after the reform but no clear pre-trends. Consistent with an improvement in productivity, we also find a decrease in errors per mail handled (column 6). The combined evidence therefore strongly suggests that the reform increased productivity.

3.3 Personnel turnover and retention

There are multiple channels through which the civil service reform could have affected worker productivity. We use micro-level personnel records of the civil service to shed light on the potential drivers of the productivity gain. We first focus on reduced turnover as our preferred channel before briefly discussing alternatives.

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.” [Hoogenboom \(1959\)](#) further describes how 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.”

Turnover. To test whether the civil service reform indeed reduced turnover, we leverage micro-level personnel data from the Official Registers. We use a “stacked” design analogous to [Equation 1](#), except that the outcome of interest is an indicator variable for civil servant turnover, and the corresponding unit of observation now is an individual-year. To ensure comparability, we restrict the sample to the subset of cities for which we have data on delivery. We allow time fixed effects to vary flexibly by tenure and occupation (clerk or carrier). The identifying variation remains at the city-level, and we cluster the standard errors accordingly.

The results on bureaucratic turnover are reported in [Table IV](#). The civil service reform significantly reduced turnover (column 1). The magnitude is sizable, amounting to a reduction of 18% relative to the mean turnover rate. [Figure IV](#) provides visual evidence. As with the delivery results, there are no marked pre-trends but a decline in turnover in reformed cities after the federal civil service policy is implemented. This result again holds when including all time-interacted controls of [Table I](#), and when restricting the analysis to the subset of coarsely

matched set of reform and non-reform locations (columns 2-3). Finally, we observe a decline in turnover even for those who were selected under the pre-Pendleton system (column 4). This finding is consistent with a change in personnel practices, as opposed to differences in the selection of officers due to the reform.

Role of retention. While federal civil service reform shielded public employees from political interference, “the Civil Service Law does not provide for permanency of tenure. On the contrary, it specifically provides for the removal of incompetent employees.” (Roper, 1917, p. 292). Motivated by the historical record, [Table V](#) considers whether the civil service reform affected patterns of retention – another margin for improving performance. The dependent variable now is a dummy that is 1 if the civil servant exits, and 0 otherwise. Consistent with the reduction in political interference, the increase in exit during presidential election years is significantly more muted in cities where the civil service reform was implemented (columns 1-2).

To test if the civil service reform also led to a changing pattern of retention by skill, we further ask whether higher qualified individuals were differentially likely to exit post-reform. Unfortunately, measures of education are not directly available from the personnel records. To obtain a measure of quality, we link the personnel records to the full count census of 1870, 1880, and 1900. While the historical census does not record education-levels, we can rely on information about the education requirements of the occupations in which the individuals worked *prior* to joining the civil service. This measure captures the share of college-educated individuals working in the same occupation. Columns 3-4 provide a sample split by whether the education requirements were above (high) or below (low) median. While more suggestive given the relatively low match rate and coarseness of the quality measure ([Appendix A](#)), we find that – if anything – higher qualified individuals are *less* likely to exit. We thus interpret the combined evidence as consistent with federal civil service reform reducing political interference and increasing merit-based retention.

While we do not rule out alternative channels – such as an improved selection or assignment of officers to tasks – the evidence we find in support of these alternative mechanisms is significantly weaker. Based on our coarse measures of quality, we find only slight improvements in the selection of workers ([Appendix Table AV](#)); we also find no evidence that the allocation of talent became more assortative after the reform, as evidenced in the assignment of qualified workers to higher paid positions ([Appendix Table AVI](#)). In [Appendix A](#), we provide a more detailed discussion of these alternative channels. Ultimately, since the civil service reform came with a bundle of policies, it is likely that the improvement in performance is driven by multiple channels. Our personnel-level findings, however, provide evidence that personnel policies – a reduction in turnover due to political interference – played a significant role in improving performance. In the U.S., these results complement findings from [Moreira and Perez \(2020\)](#), who show a similar reduction in turnover when studying the impact of the Pendleton Act in American customs offices. More broadly, our preferred turnover channel also resonates with findings in modern-day developing country settings, where high turnover rates among public sector workers have been associated with lower performance ([Iyer and Mani, 2012](#); [Akhtari et al., 2022](#)).

4 Postal reform and innovation

The increase in information flow raises the intriguing question of whether a more effective postal service affected downstream economic outcomes. We focus on innovation since it is a key driver of economic growth, and its production process relies heavily on knowledge diffusion and spillovers (Aghion and Howitt, 1992; Kantor and Whalley, 2014). Moreover, the 19th century witnessed a significant amount of patenting activity (Akcigit et al., 2017). The rapid growth in innovation and patenting during the 19th century were in part facilitated by a state-created institutional infrastructure that allowed for the growth of a market for technology (Lamoreaux and Sokoloff, 2001). Innovation is a particularly sensible outcome in our setting as it often requires the spreading and development of new knowledge, which may be influenced by the extent to which ideas can be shared and discussed. Mokyr (2005), for example, explains that “access to useful knowledge created the opportunities to recombine its components to create new forms that would expand the volume of knowledge at an even faster rate.” We focus on patenting as a proxy for innovation, as is standard in the literature (Jaffe, 1989; Hausman, 2022; Hanlon et al., 2022). More pragmatically given our goal of testing for city-level economic effects of bureaucracy reform, patent data provides rich information on both the timing of issuance and the location and identities of the parties involved, allowing us to measure the pace of innovation at the city level.

There are multiple channels through which the postal reform can plausibly affect the pace of innovation as measured by patenting. First, a more reliable and efficient postal service likely improved market access (and by extension market size) – which could in turn increase returns to innovative activity (Melitz, 2003). Consistent with this view, studies of the 19th century U.S. suggest that increased market access contributed to the increase in patenting activity. Lamoreaux and Sokoloff (1999a), for example, find that canals in the Northeast witnessed large increases in patenting activity due to increased market access between states and counties. Second, postal reform can directly affect knowledge diffusion through the significant increase in delivery volumes that we observe. Recent evidence demonstrates that access to information leads to increases in local invention (Andrews, 2019). Indeed, throughout the 19th century, the postal service facilitated long-distance communication and access to information (John, 1995). Third, improvements in postal efficiency could also have had broader impacts on economic activity, which in turn is conducive to innovation. In all these cases, there should be a positive relationship between civil service reforms and patenting.

Given these possible avenues through which improvements in a particular form of state capacity may affect innovation, it is reasonable to examine whether postal reform played a causal role during what some have called the “Golden Age” of American invention (Gordon, 2016; Akcigit et al., 2017). We investigate whether the postal reform affected the amount of patenting in two ways. First, we adopt the same “stacked” event-study design to ask if cities that experienced the postal reform exhibit an increase in patenting relative to places that were not reformed. The results are shown in Table VI. The specification follows Equation 1, except that the

main dependent variable is now the number of patents registered in a given city and year. Given the right-skewed nature of the outcome that also frequently involves zero patenting, we apply the inverse hyperbolic sine (IHS) transformation. Columns 1-2 provide estimates for the full sample, and columns 4-6 restrict the sample to city-years that exhibit non-zero amounts of patenting. Consistent with the increase in information flow, we find that cities experiencing postal reforms also exhibit an increase in patenting (column 1). This result holds also when we time-interact all control variables (column 2), or when restricting the comparison to only localities with comparable postal employment and county population (column 3). [Figure V](#) provides visual evidence for the effect, allowing the gap between reformed and unreformed cities to vary around the implementation year. Reformed cities see a differential increase in patenting relative to cities that were not reformed. Given the downstream nature of the outcome, the increase takes longer to build up. Although noisier given the more aggregate outcome, we do not find strong evidence for marked pre-trends. Finally, we also probe the robustness of our results by showing that the patent effects are not driven by a particular reform wave, treatment cities, or states ([Table AVII](#), [Figure AIV](#) and [Figure AV](#)).

The increase in patenting we observe is striking. In columns 4-6, we probe further to understand the patterns of this increase. The patent data not only allows us to observe the location of patentees but also provides information on the number of inventors and assignees as well as their locations. This allows us to shed further light on how the postal reform affected the nature of patenting. Interestingly, we do not find that the postal reform changed the number of co-inventors. In our sample period, virtually all patents involve a single inventor, and we do not find that the reform increased the average likelihood of a patent having multiple inventors (column 4). The postal reform does, however, increase the likelihood of patents being co-registered with a business. On average, 1.4% of the patents in our sample period are co-registered with a business (assignee). Here, we find that the postal reform increases the likelihood of an average patent in a given city-year to have any assignees by 2.8 p.p., a sizeable effect relative to the mean of the dependent variable. Finally, we find that the reform increases the likelihood that patents are jointly registered by individuals across different locations. While the inventors and assignees of the bulk of patents are located in the same city, reformed locations experience more patenting across cities, consistent with greater information flow encouraging joint innovation across space.

4.1 Impact on cross-city patenting

To study how postal reform affects the spatial distribution of patenting, we can further make use of our patenting data to estimate a gravity model relating the amount of co-patenting observed between any two cities in the U.S. to distance and the presence of postal reforms. We thus turn our attention to the subset of patents that are filed by entities across multiple cities. We construct a panel of city-pairs between any two cities that ever filed a patent in our study period. We can then ask whether cities that both experienced postal reform are more likely to see an increase in the amount of patenting. For the pair of city i and city j , let y_{ijt} thus denote the amount of

joint patenting at time t .²² We estimate,

$$y_{ijt} = \beta \text{both}_{ijt} + \gamma \log(\text{dist}_{ij}) + \tau_t + \varepsilon_{ijt} \quad (2)$$

where $\text{both}_{ijt} = 1$ if both cities have a reformed postal office. $\log(\text{dist}_{ij})$ captures the (log) distance in miles between the two cities. τ_t are year fixed effects. Standard errors are clustered at the city-pair level ij , corresponding to the level of the treatment variation.

The results are reported in [Table VII](#) and confirm the disproportionate increase in patenting observed between entities located in cities that experienced postal reform. In columns 1-3, we focus on whether we observe *any* patenting between entities in a given city-pair and year, capturing the extensive margin. Column 1 reports the basic gravity specification described in [Equation 2](#). The likelihood of any two locations in the U.S. to exhibit jointly filed patents is declining with distance, consistent with increased communication costs inhibiting the co-generation of knowledge. In contrast, city-pairs that both experienced a reform see a significant increase in the amount of patenting. Evaluated against the elasticity of co-patenting with respect to distance, this magnitude is economically large. In column 2, we include city-pair FEs, restricting the comparison to only city-pairs that both become reformed during the sample period of 1875–1905. These fixed effects ensure that our results are not driven by certain cities (e.g., larger ones) more likely to co-patent with each other. Even after the inclusion of these stringent bilateral FEs, cities that are both reformed are 3.2 p.p. more likely to exhibit any co-patenting in a given year. Finally, we can go even further by partialing out city and time-specific confounders in column 3. We do this by including dummies for each city in a given city-pair and allowing these city FEs to vary by whether the city is reformed or not. These fixed effects directly address potential issues over the non-random timing of the reform adoption at the city-year level. Reassuringly, the results remain very comparable.

Finally, columns 4-5 look at patenting intensity using the IHS-transformed number of patents as the dependent variable. As before, we observe an increase in the amount of patenting for cities that both experience postal reform. The result also holds for the subsample of city-pair and years that experience non-zero numbers of patenting throughout the study period (column 5), suggesting that the postal reform increased the amount of patenting across locations both at the extensive and intensive margin.

5 Conclusion

In this paper, we demonstrate that the quality of government institutions matters by focusing on how personnel reform affected 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

²²Since the patenting data is not directional, $y_{ijt} = y_{jit}$ and we do not “duplicate” the data.

as the post office (Gallagher, 2016; John, 1998). 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 century, “the postal system was the central government” (John, 1998). 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 roll-out 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 urban 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 (Aghion and Howitt, 1992; Acemoglu et al., 2016). Focusing on patenting, we show that reformed cities also experienced an increase in the pace of innovation. Importantly, and consistent with the role of the postal service in facilitating communication, we find an increase in co-patenting across cities. While co-patenting declines with distance, we find that pairs of reformed cities are significantly more likely to co-patent after the implementation of the reform – evidence that a more effective postal service reshaped the geography of patenting across the nation.

A growing body of work has provided micro-level evidence on the selection and incentives of bureaucrats (Finan et al., 2015; Pepinsky et al., 2017; Lim and Snyder, 2021; Besley et al., 2021). These exciting developments resonate with an older literature that has focused on the quality of administration and economic growth at the aggregate level (Weber, 1922; Evans and Rauch, 1999, 2000; Woo-Cumings, 1999). This paper, linking a major civil service reform to personnel outcomes, organizational performance, and aggregate outcomes, contributes to connecting these two strands of work. Our results underline the importance of the postal service – and state capacity more broadly – in providing an environment conducive to fostering knowledge-based growth.

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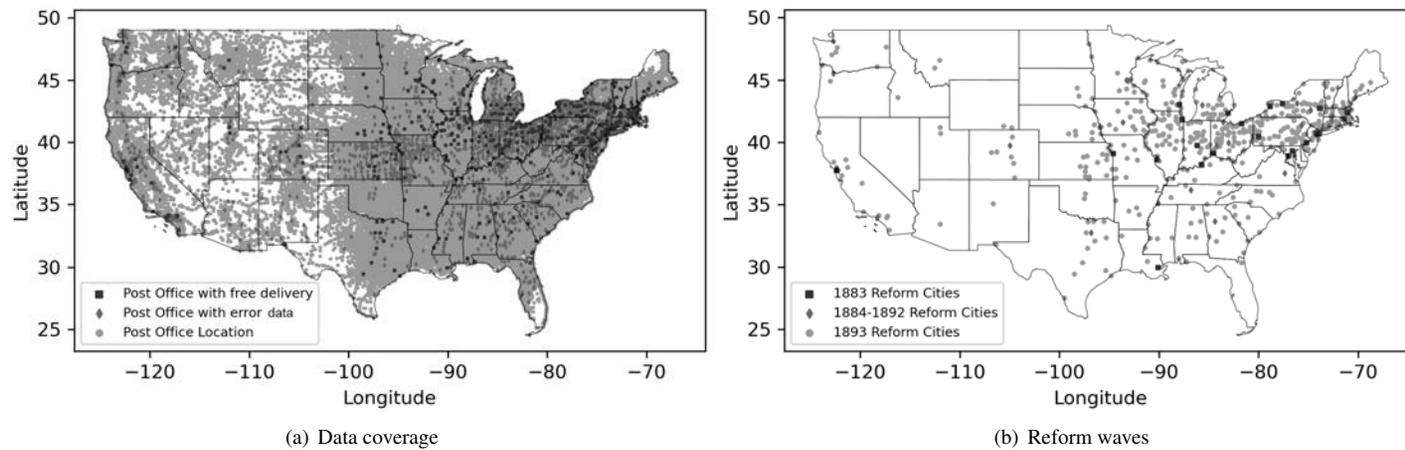
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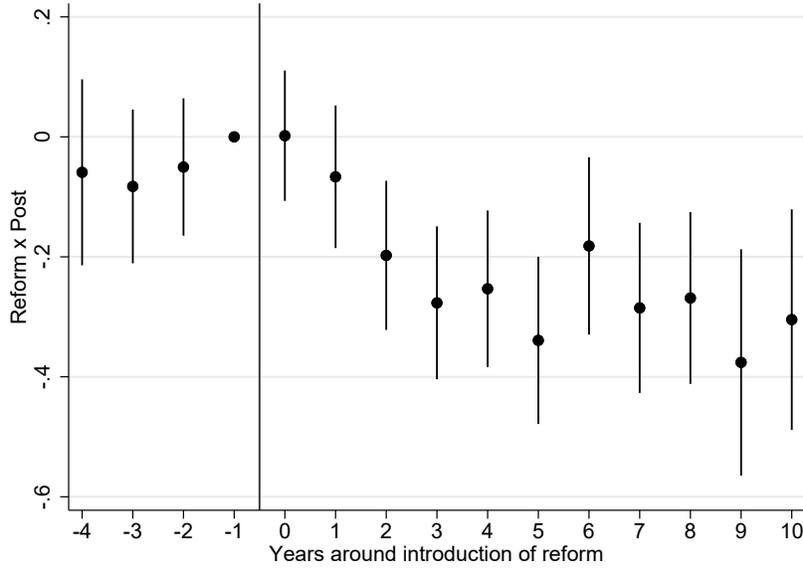
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Figure I: Spatial distribution of postal offices and treatment variation



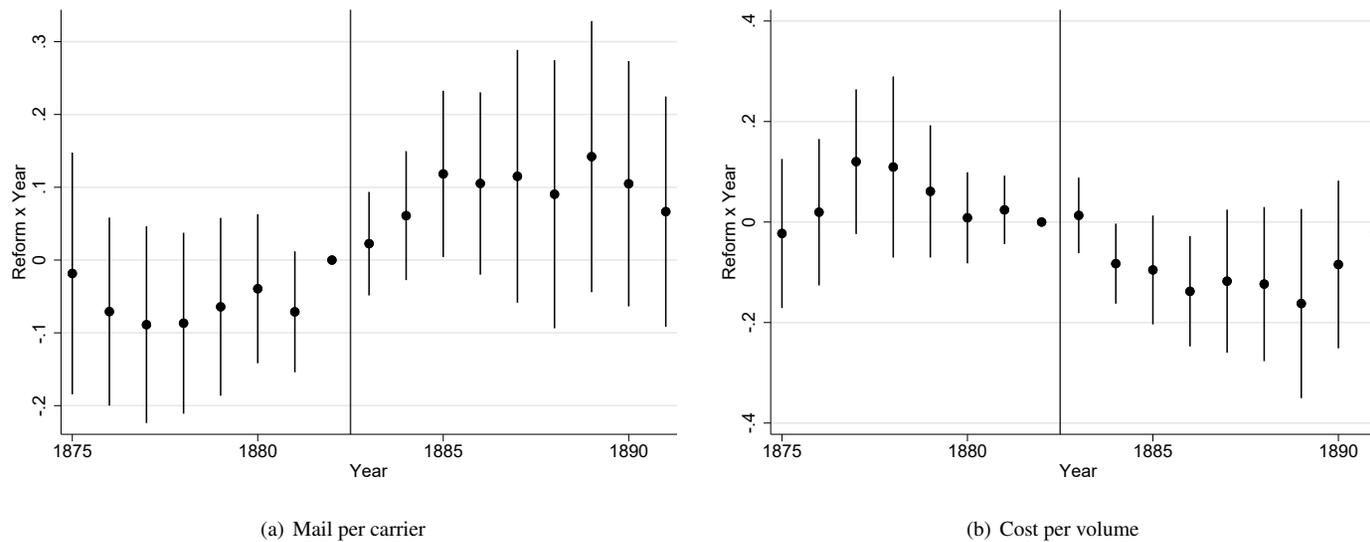
Notes: Panel (a) shows the locations of all post offices active between 1860-1905, as well as the locations for which we have performance data on delivery error rates and urban free delivery outcomes. Panel (b) shows the locations of reformed cities and their timing. There are 23 locations that are reformed in 1883, 30 locations between 1884–1892, and 556 locations in 1893.

Figure II: Error rates for reform vs. non-reform cities around reform years



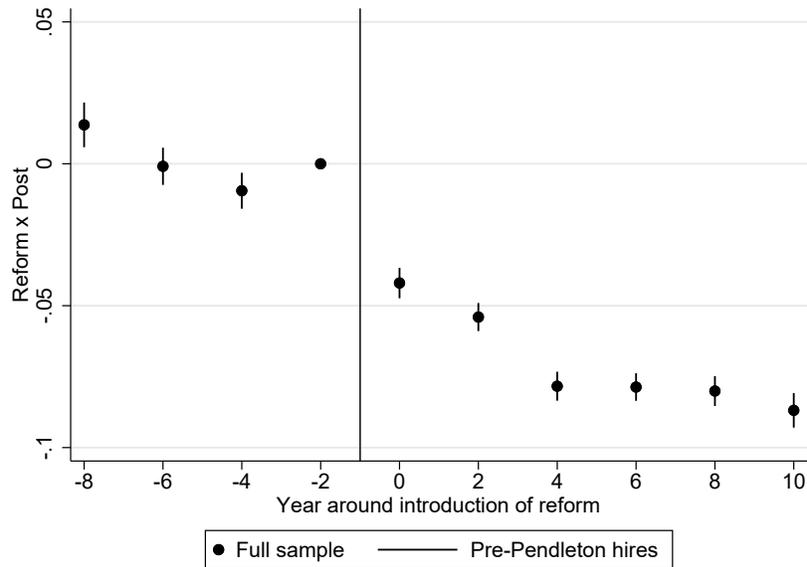
Notes: Figure reports an augmented version of Equation 1 (corresponding to Table II, column 2), where the estimate between treatment and control cities is allowed to vary for each year around the introduction of the reform. Reporting 95% confidence intervals. Standard errors clustered at the city \times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

Figure III: Delivery productivity and cost efficiency for reform vs. non-reform cities around Pendleton Act 1883



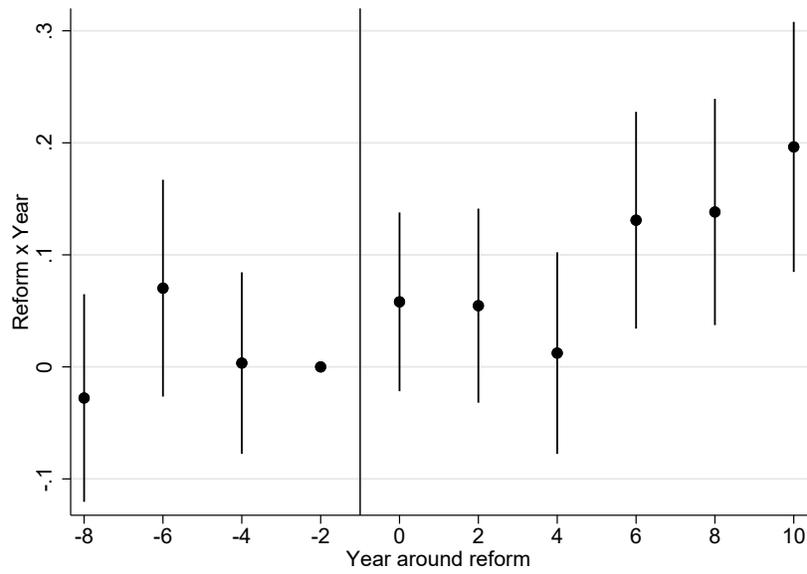
Notes: Figure reports an augmented version of Equation 1 (corresponding to Table III, column 3), where the estimate between treatment (reformed cities in 1883) and control (unreformed cities) is allowed to vary by each year. Panel (a) shows the event study with (log) overall volume per carrier as the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

Figure IV: Personnel turnover for reform vs. non-reform cities around reform years



Notes: Figure reports an augmented, individual-level version of [Equation 1](#) (corresponding to [Table IV](#), column 1), where the estimate between treatment (reformed cities in 1883) and control (unreformed cities) is allowed to vary by each year. The dependent variable `turnover` is a dummy that is 1 if the civil servant entered or exited the service. Reporting 95% confidence intervals. Standard errors clustered at the city-level.

Figure V: Patents filed for reform vs. non-reform cities around reform years



Notes: Figure reports an augmented version of Equation 1 (corresponding to Table VI, column 3), where the estimate between the matched 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 \times reform year-level.

Table I: Descriptive statistics of reformed and unreformed cities

| | (1) Control group Mean | (2) SD | (3) Difference Treat - Control |
|---------------------------------|------------------------------|-----------|--------------------------------------|
| <i>Panel A: City-level</i> | | | |
| Log(Post office staff) | 1.24 | 1.29 | 1.785*** (0.055) |
| Log(Delivery errors) | 4.40 | 1.81 | 1.690*** (0.084) |
| Year established | 1835.93 | 30.64 | -14.259** (1.528) |
| Log(Distance to DC) | 6.27 | 0.87 | -0.312*** (0.043) |
| Western Union in 1874 | 0.78 | 0.40 | 0.300*** (0.017) |
| Total number of cities | 2,707 [1,831] | | 2,730 [1,854] |
| - of which treatment: | 0 [0] | | 599 [587] |
| <i>Panel B: County-level</i> | | | |
| Log(Total population) | 10.16 | 0.89 | 0.855*** (0.036) |
| Share urban | 0.04 | 0.14 | 0.143*** (0.012) |
| Non-white share | 0.10 | 0.18 | -0.056*** (0.008) |
| Manufacturing establishments | 238.24 | 463.79 | 365.036*** (46.490) |
| Frontier county | 0.08 | 0.27 | -0.081*** (0.010) |
| Rail access | 0.95 | 0.21 | 0.022*** (0.005) |
| Existing canals | 0.11 | 0.31 | 0.083*** (0.016) |
| Total number of counties | 1,412 [1,367] | | 1,418 [1,377] |
| - of which with treatment city: | 0 [0] | | 472 [466] |

Notes: Columns 1 and 2 show the mean and standard deviation for the unreformed (control) cities. Column 3 show the difference between reformed vs. unreformed cities, conditional on reform wave FEs. Observation counts (total number of cities / counties) reported as the maximum number of observations, with the minimum number reported in square brackets. Robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table II: Delivery errors and civil service reform

| | (1) | (2) | (3) | (4) | (5) |
|--|---------------------|--------------------------------|----------------------|---------------------|---------------------|
| | | Log(Number of delivery errors) | | | |
| Mean of dep. var | 4.301 | 4.295 | 4.395 | 5.124 | 5.108 |
| Reform \times Post | -0.110** (0.043) | -0.144*** (0.062) | -0.165*** (0.061) | -0.278** (0.112) | -0.434** (0.176) |
| Reform wave \times Year FEs | ✓ | | | | |
| Reform wave \times City FEs | ✓ | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times Year \times State FEs | | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times Year \times Controls | | | ✓ | | |
| Sample | | Full sample | | CEM | Early/late |
| Observations | 163,051 | 162,616 | 120,964 | 7,126 | 6,633 |

Notes: Relating delivery errors to the postal reform in a stacked event-study design (See description [Equation 1](#)). The unit of observation is the reform wave \times city \times year. Reform is a dummy that is 1 if the city experienced a postal 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 3 includes all (time-interacted) variables from [Table I](#) as control variables. CEM matches on the (log) county population and (log) city-level postal employment as measured by the total number of clerks and carriers in a city in the year prior to the introduction of the reform (column 4). In column 5, the sample is restricted to the 1883 reform wave (“early”) and using only yet unreformed cities (“later”) as the control group. Standard errors clustered at the city \times reform-wave level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table III: Total employment, mail volume, productivity and civil service reform

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|---------------------|---------------------|-----------------------|-------------------|---------------------|---------------------|
| | Total workers | Errors / Workers | Mail volume | Volume / Staff | Cost / Volume | Errors / Volume |
| Mean of dep. var | 2.850 | 0.281 | 15.51 | 12.60 | 1.016 | 0.0210 |
| Reform \times Post | 0.141*** (0.036) | -0.138** (0.052) | 0.219*** (0.057) | 0.098* (0.050) | -0.097** (0.048) | -0.013** (0.006) |
| Reform wave \times Year FEs | | | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times City FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times State \times Year FEs | ✓ | ✓ | | | | |
| Sample | All reform waves | | 1883 reform wave only | | | |
| Observations | 43,647 | 43,647 | 1,102 | 1,102 | 1,102 | 772 |

Notes: Relating total employment, mail volume, and measures of productivity to the postal reform in a stacked event-study design. The unit of observation is a reform wave \times city \times year. Reform is a dummy that is 1 if the city experienced a postal 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 dependent variables are log-transformed. Columns 1-2 use personnel data from the Official Registers, covering all reform waves. In columns 3-6 the sample is restricted to the 1883 reform wave only, for which granular measures are available from the Free Delivery Statistics. Standard errors clustered at the city \times reform-wave level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table IV: Personnel turnover, and civil service reform

| | (1) | (2) | (3) | (4) |
|---|-------------------------------------|----------------------|----------------------|----------------------|
| | Bureaucrat turnover (entry or exit) | | | |
| Mean of dep. var | 0.515 | 0.515 | 0.556 | 0.451 |
| Reform \times Post | -0.091*** (0.004) | -0.141*** (0.009) | -0.029*** (0.010) | -0.178*** (0.006) |
| Reform wave \times City FEs | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times State \times Year FEs | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times Year \times Occupation \times Tenure FEs | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times Year \times Controls | | ✓ | | |
| Cohorts included | All cohorts | | CEM | Pre-reform |
| Observations | 581,732 | 581,732 | 310,433 | 270,698 |

Notes: Relating turnover (entry or exit) to the postal reform in a stacked event-study design. The unit of observation is a reform wave \times individual \times city \times year. Reform is a dummy that is 1 if the city experienced a postal 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 1-2 rely on the full sample. Column 2 includes all (time-interacted) variables from [Table I](#) as control variables. In column 3, the sample is restricted to the cities that could be coarsely matched based on (log) county population size and (log) total postal employment. Column 4 restricts the sample to only those individuals who entered the civil service prior to the implementation of the reform. Standard errors clustered at the city \times reform-wave level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table V: Retention and civil service reforms by election years and predicted qualification

| | (1) | (2) | (3) | (4) |
|---|----------------------|----------------------|------------------------|----------------------|
| | | | Exit | |
| Mean of dep. var | 0.217 | 0.121 | 0.163 | 0.130 |
| Reform \times Post | -0.121*** (0.006) | -0.059*** (0.005) | -0.313*** (0.025) | -0.240*** (0.029) |
| Sample cut | <u>Election year</u> | | <u>Education score</u> | |
| | Yes | No | High | Low |
| Difference in coefficients | -0.061*** (0.008) | | -0.072* (0.037) | |
| Reform wave \times City FEs | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times Year \times Occupation \times Tenure FEs | ✓ | ✓ | ✓ | ✓ |
| State \times Year around reform FEs | ✓ | ✓ | ✓ | ✓ |
| Observations | 258,033 | 277,583 | 43,351 | 28,216 |

Notes: Relating exit to the postal reform in a stacked event-study design and to heterogeneity by electoral year and individual qualification. The unit of observation is a reform wave \times individual \times city \times year. Reform is a dummy that is 1 if the city experienced a postal 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 1-2 split the sample by whether it is a presidential election year or not. Columns 3-4 split the sample by whether the individual's measure of qualification is above or below median. We use the `edscor50` as the measure of qualification, computed as the share of individuals with college degree in 1950 who worked in the civil servants' occupation prior to entry into the civil service. Standard errors clustered at the city \times reform-wave level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table VI: City-level patents and civil service reform

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--|------------------------------------|--------------------|---------------------|--------------------|---------------------|---------------------|
| | Number of patents registered (IHS) | | | Multiple inventors | Any assignees | Multiple cities |
| Mean of dep. var | 1.630 | 1.630 | 0.996 | 3.32e-05 | 0.0137 | 0.0603 |
| Reform \times Post | 0.103*** (0.025) | 0.076** (0.036) | 0.087*** (0.031) | 0.000 (0.001) | 0.028*** (0.004) | 0.111*** (0.013) |
| Reform wave \times City FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times State \times Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times Year \times Controls | | ✓ | | | | |
| Sample | Full sample | | CEM | | Full sample | |
| Observations | 90,419 | 90,419 | 70,719 | 90,419 | 90,419 | 90,419 |

Notes: Relating the number of patents registered to the postal reform in a stacked event-study design, centered around each reform year (See description [Equation 1](#)). The unit of observation is the reform wave \times city \times year. Reform is a dummy that is 1 if the city experienced a postal 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 1-2 focus on the full sample and the dependent variable is the (inverse hyperbolic-sine transformed) number of patents registered. Column 2 includes all (time-interacted) variables from [Table I](#) as control variables. Column 3 reports the estimate where reformed and unreformed cities are coarsely matched based on (log) county population and (log) city-level postal employment as measured by the total number of clerks and carriers in a city in the year prior to the introduction of the reform. In columns 4-6, we focus on the subset of city-years that exhibit non-zero amounts of patenting. The dependent variable in column 4 is the share of patents registered involving multiple inventors. In column 5, the dependent variable is the share of patents registered involving any assignees. In column 6, the dependent variable is the share of patents registered involving inventors and/or assignees from different cities. Standard errors clustered at the city \times reform-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

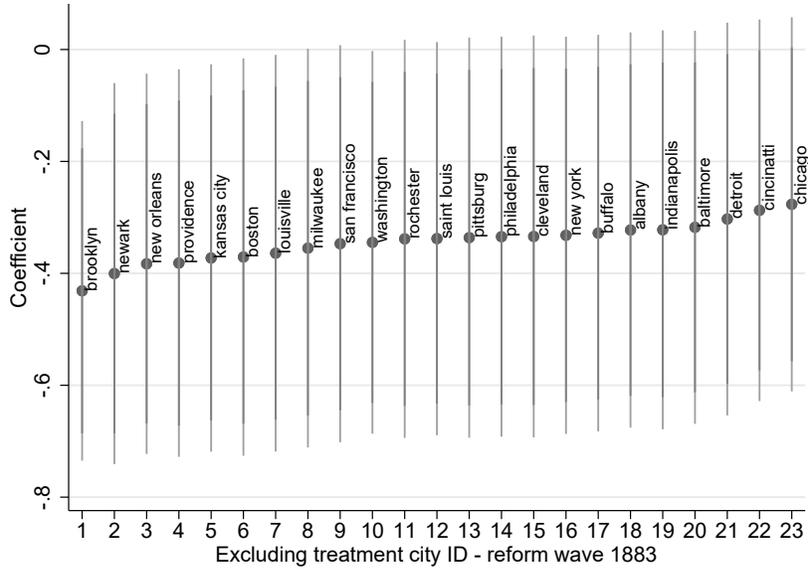
Table VII: Joint patenting between cities and exposure to civil service reform

| | (1) | (2) | (3) | (4) | (5) |
|------------------------|--------------------------------------|---------------------|---------------------|---------------------|--------------------|
| | Any joint patenting ($\times 100$) | | | IHS # patents | |
| Mean of dep. var | 0.0569 | 0.0569 | 0.0569 | 0.0650 | 1.141 |
| Both reformed | 0.061*** (0.005) | 0.032*** (0.003) | 0.034*** (0.004) | 0.064*** (0.008) | 0.300** (0.136) |
| log(Distance) | -0.007*** (0.001) | | | | |
| Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ |
| City pair FEs | | ✓ | ✓ | ✓ | ✓ |
| City FEs \times Post | | | ✓ | ✓ | ✓ |
| Sample | Full sample | | | Non-zero | |
| Observations | 117,986 | 117,986 | 117,986 | 117,986 | 6,714 |

Notes: Relating the presence of any joint patenting (and the number of patents between two cities) to the postal reform. The unit of observation is the city-pair \times year. Both reformed is a dummy that is 1 if both cities in the city-pair have implemented the postal reform, and 0 otherwise. log(Distance) is the log(Distance in miles) between the city-pair. City-pair FEs are fixed effects for each city-pair, and City FEs \times Post are dummies for each city included in the city-pair interacted with whether the city was reformed or not. Columns 1-4 focus on the full sample, where the dependent variable of columns 1-3 is a dummy (rescaled by 100 for readability) that is 1 if any patent was registered involving inventors and/or assignees from the given city-pair in a year. In column 4, the dependent variable is the (inverse hyperbolic-sine transformed) number of patents registered. Column 5 restricts the sample to city-pair \times years with non-zero amount of joint patenting. Standard errors clustered at the city-pair level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

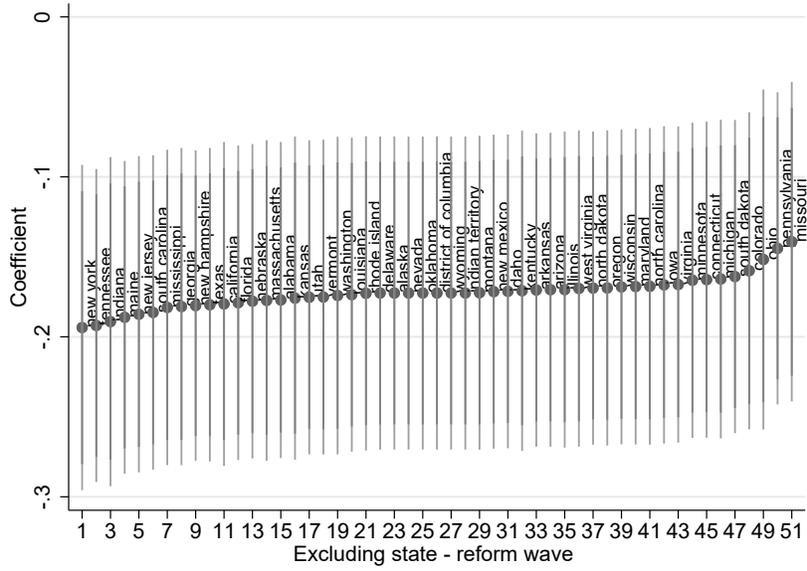
Online Appendix - Not for publication

Figure AI: Delivery errors – Robustness of 1883 reform wave results, dropping one treatment city at a time



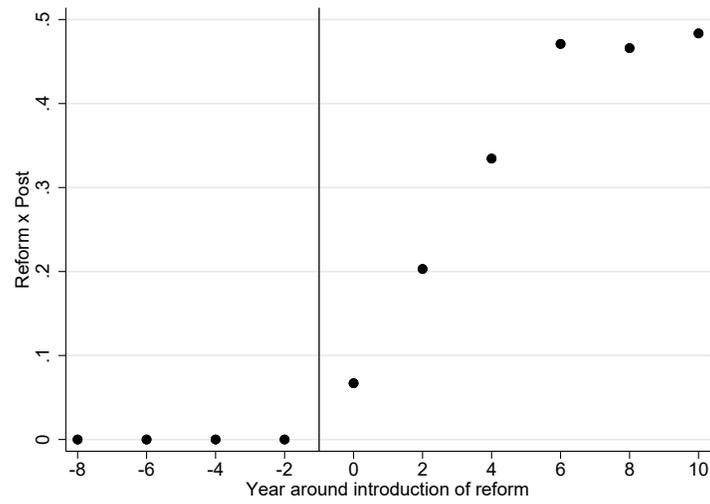
Notes: Reporting coefficients of the Reform \times Post estimate, restricting the analysis to only the 1883 reform (Appendix Table AII, column 2) and excluding each of the 23 treatment cities at a time. Reporting 95% confidence intervals in dotted vertical lines, and 90% confidence intervals in solid line. Standard errors clustered at the city-level.

Figure AII: Delivery errors – Robustness of 1893 reform wave results, dropping one state at a time



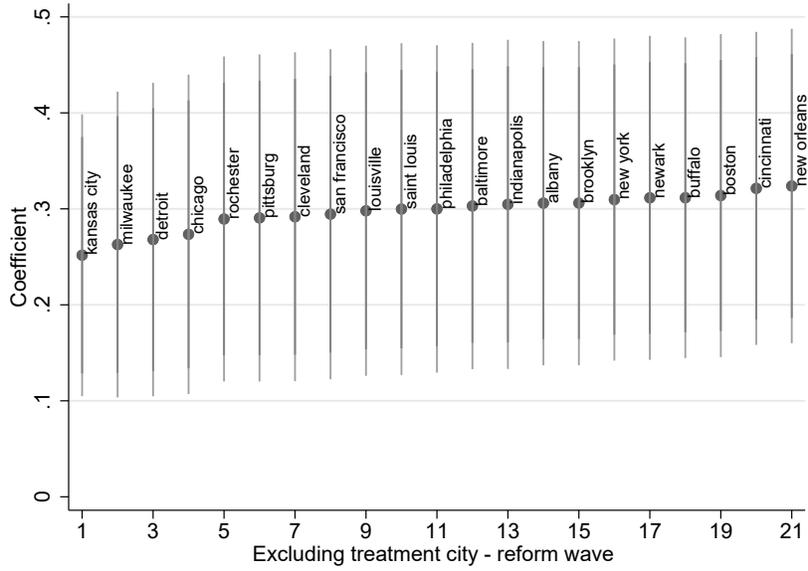
Notes: Reporting coefficients of the Reform \times Post estimate, restricting the analysis to only the 1893 reform (Appendix Table AII, column 3) and excluding each state at a time. Reporting 95% confidence intervals in dotted vertical lines, and 90% confidence intervals in solid line. Standard errors clustered at the city-level.

Figure AIII: Share of civil servants recruited via competitive entry



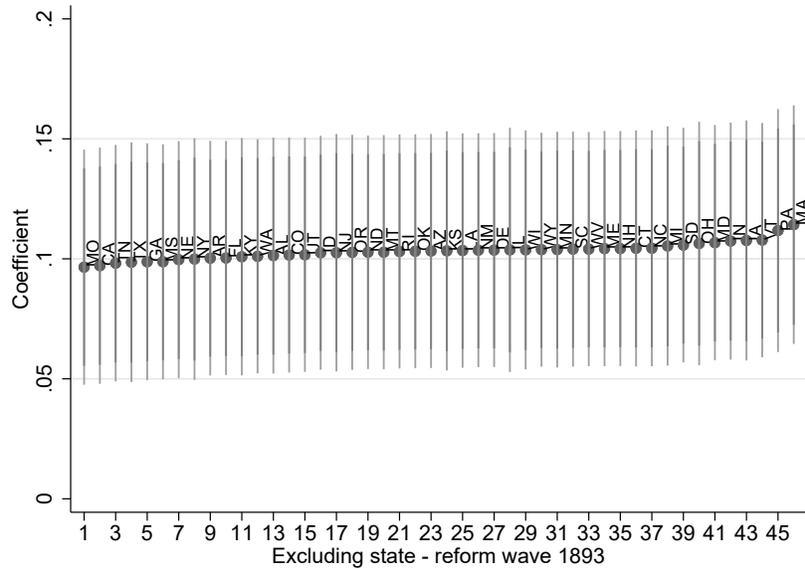
Notes: Figure reports an augmented version of Equation 1, where the estimate between the matched treatment (reformed cities in 1883) and control (unreformed cities) is allowed to vary by each year. The dependent variable is a dummy that is one if the individual is hired via competitive exam (i.e., entered after the implementation of civil service reform in a given city). Reporting 95% confidence intervals. Standard errors clustered at the city-level.

Figure AIV: Patenting – Robustness of 1883 reform wave results, dropping one treatment city at a time



Notes: Reporting coefficients of the Reform \times Post estimate, restricting the analysis to only the 1883 reform (Appendix Table AVII, column 2) and excluding each of the 23 treatment cities at a time. Reporting 95% confidence intervals in dotted vertical lines, and 90% confidence intervals in solid line. Standard errors clustered at the city-level.

Figure AV: Patenting – Robustness of 1893 reform wave results, dropping one state at a time



Notes: Reporting coefficients of the Reform \times Post estimate, restricting the analysis to only the 1893 reform (Appendix Table AVII, column 3) and excluding each state at a time. Reporting 95% confidence intervals in dotted vertical lines, and 90% confidence intervals in solid line. Standard errors clustered at the city-level.

Table AI: Descriptive statistics of reformed and unreformed post-offices – CEM

| | (1) | (2) | (3) |
|-----------------------------------|------------------|----------------------------|--------------------|
| | Mean | Diff treat-control in year | |
| | control | Raw | CEM |
| <i>Panel A: Post office-level</i> | | | |
| Log(Post office staff) | 1.24 | 1.785*** (0.055) | 0.110 (0.165) |
| Log(Delivery errors) | 4.40 | 1.690*** (0.084) | 0.445 (0.274) |
| Year established | 1835.93 | -14.259*** (1.528) | 5.434 (12.021) |
| Log(Distance to DC) | 6.27 | -0.312*** (0.043) | 0.543 (0.441) |
| Western Union in 1874 | 0.78 | 0.300*** (0.017) | 0.184 (0.151) |
| Total number of post-offices | 2,707 [1,831] | 2,730 [1,854] | 845 [809] |
| - of which treatment: | 0 [0] | 599 [587] | 502 [500] |
| <i>Panel B: County-level</i> | | | |
| Log(Total population) | 10.16 | 0.855*** (0.036) | 0.170 (0.111) |
| Share urban | 0.04 | 0.143*** (0.012) | 0.008 (0.008) |
| Non-white share | 0.10 | -0.056*** (0.008) | -0.066 (0.042) |
| Manufacturing establishments | 238.24 | 365.036*** (46.490) | 49.535 (37.747) |
| Frontier county | 0.08 | -0.081*** (0.010) | 0.040** (0.016) |
| Rail access | 0.95 | 0.022*** (0.005) | -0.001 (0.005) |
| Existing canals | 0.11 | 0.083*** (0.017) | -0.011 (0.027) |
| Total number of counties | 1,412 [1,367] | 1,418 [1,377] | 724 [723] |
| - of which with treatment: | 0 [0] | 472 [466] | 196 [196] |

Notes: Column 1 shows the mean and standard deviation for the unreformed (control) cities. Column 2 shows the difference between reformed vs. unreformed cities, conditional on reform wave FEs. Column 3 shows the same difference for the sample matched using coarsened exact matching (CEM), where cities are matched based on (log) county population and (log) total postal employment. Observation counts (total number of cities / counties) reported as the maximum number of observations, with the minimum number reported in square brackets. Robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table AII: Delivery errors and civil service reform by 1883 Pendleton Act and 1893 Executive Order

| | (1) | (2) | (3) |
|--|--------------------------------|----------|-----------|
| | Log(Number of delivery errors) | | |
| Mean of dep. var | 4.301 | 4.490 | 4.234 |
| Reform \times Post | -0.110** | -0.345** | -0.173*** |
| | (0.043) | (0.174) | (0.050) |
| Reform wave \times Year \times State FEs | Y | Y | Y |
| City FEs | Y | Y | Y |
| Reform waves | All | 1883 | 1893 |
| Observations | 163,051 | 10,987 | 17,406 |

Notes: Relating delivery errors to the postal reform in a stacked event-study design, centered around each reform year and broken down by the 1883 and 1893 reform waves (See description [Equation 1](#)). The unit of observation is the reform wave \times city \times year. Reform is a dummy that is 1 if the city experienced a postal 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 \times reform-wave level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table AIII: Delivery errors, postal reform, and spatial heterogeneities

| <i>Panel A: Infrastructure</i> | | | | | | |
|--|--------------------------------|--------------------|------------------------|----------------------|------------------------|----------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Log(Number of delivery errors) | | | | | |
| Mean of dep. var | 4.341 | 3.659 | 4.519 | 4.265 | 4.489 | 3.575 |
| Reform × Post | -0.155*** (0.045) | -0.312 (0.246) | -0.200* (0.103) | -0.137*** (0.049) | -0.090* (0.048) | -0.476*** (0.176) |
| Reform wave × City FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Reform wave × Year × State FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Sample cut | <u>Rail access</u> | | <u>Existing canal</u> | | <u>Western Union</u> | |
| | Y | N | Y | N | Y | N |
| Difference in coefficients | 0.156 (0.237) | | -0.063 (0.113) | | 0.385** (0.177) | |
| Observations | 150,969 | 10,600 | 19,116 | 143,192 | 127,301 | 34,228 |
| <i>Panel B: Tax capacity and geography</i> | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Log(Number of delivery errors) | | | | | |
| Mean of dep. var | 4.658 | 4.022 | 3.761 | 4.341 | 4.373 | 4.267 |
| Reform × Post | -0.158 (0.120) | -0.867* (0.457) | -0.646*** (0.188) | -0.135*** (0.046) | -0.114 (0.090) | -0.166*** (0.052) |
| Reform wave × City FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Reform wave × Year × State FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Sample cut | <u>Tax capacity</u> | | <u>Frontier county</u> | | <u>Southern states</u> | |
| | High | Low | Yes | No | Yes | No |
| Diff in coeffs | 0.709 (0.473) | | -0.511*** (0.185) | | 0.052 (0.103) | |
| Observations | 39,142 | 49,475 | 12,657 | 149,517 | 42,857 | 119,718 |

Notes: Relating delivery errors to the postal reform in a stacked event-study design and sources of spatial heterogeneity (See description [Equation 1](#)). The unit of observation is the reform wave × city × year. Reform is a dummy that is 1 if the city experienced a postal 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: In columns 1-2, the sample is split by whether the county has access to railroads. In columns 3-4, the sample is split by whether the county has access to existing canals. In columns 5-6, the sample is split by whether the city has a Western Union branch in 1874. Panel B: In columns 1-2, the sample is split by whether local tax per population is above or below median. In columns 3-4, the sample is split by whether the county is a frontier county or not ([Bazzi et al., 2020](#)). In columns 5-6, the sample is split by whether the city is located in a southern state or not. Standard errors clustered at the city × reform-wave level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table AIV: Type of mail collected / delivered and 1883 civil service reform

| | (1) | (2) | (3) | (4) | (5) | (6) |
|--------------------|-------------------------|---------------------|---------------------|-------------------------|---------------------|-------------------|
| | <u>Volume delivered</u> | | | <u>Volume collected</u> | | |
| | Letters | Postal cards | Newspapers | Letters | Postal cards | Newspapers |
| Mean of dep. var | 14.48 | 13.07 | 13.88 | 14.03 | 12.73 | 12.02 |
| Reform 1883 × Post | 0.176*** (0.063) | 0.276*** (0.073) | 0.243*** (0.067) | 0.225*** (0.084) | 0.272*** (0.090) | 0.270* (0.137) |
| City FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 1,102 | 1,102 | 1,102 | 1,102 | 1,102 | 1,102 |

Notes: Relating free delivery service outcomes to the postal reform. 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 reformed as part of the Pendleton Act of 1883, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. All dependent variables are log-transformed. Standard errors clustered at the city-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table AV: Characteristics of new entrants and civil service reform

| | (1) | (2) | (3) | (4) |
|---|---------|----------|----------------|---------------|
| | Age | Literate | Prev. Occscore | Prev. Edscore |
| Mean of dep. var | 28.84 | 0.972 | 3.066 | 0.125 |
| Reform \times Post | 0.987 | 0.016* | 0.042 | -0.015 |
| | (0.658) | (0.009) | (0.045) | (0.018) |
| Reform wave \times City FEs | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times Year \times Occupation \times Tenure FEs | ✓ | ✓ | ✓ | ✓ |
| State \times Year around reform FEs | ✓ | ✓ | ✓ | ✓ |
| Observations | 42,913 | 25,072 | 17,898 | 17,823 |

Notes: Relating characteristics of new entrants to the postal reform. The unit of observation is the reform wave \times individual \times city \times year. The sample is restricted to individuals that could be linked to the census. In columns 3-4, the sample is further restricted to those that could be linked to a census pre-dating the civil servant's entry into public service (Moreira and Perez, 2022). Reform is a dummy that is 1 if the city was reformed, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. All dependent variables are log-transformed. Occscore is the occupational income score, calculated as the imputed income of the individual's occupation based on the 1950 census. Edscore is the education score computed as the share of individual's with college education that work in the same occupation in 1950. Standard errors clustered at the city-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table AVI: Salary progression and returns to qualification – pre-reform entrants

| | (1) | (2) | (3) |
|--|---------|------------------------|---------|
| | | Log(Salary) | |
| Mean of dep. var | 6.305 | 6.371 | 6.466 |
| Reform × Post | 0.002 | 0.048 | 0.062 |
| | (0.011) | (0.039) | (0.074) |
| Sample cut | - | <u>Education score</u> | |
| | | High | Low |
| Difference in coefficients | | -0.014 | |
| | | (0.085) | |
| Reform wave × City FEs | ✓ | ✓ | ✓ |
| Reform wave × Year × Occupation × Tenure FEs | ✓ | ✓ | ✓ |
| State × Year around reform FEs | ✓ | ✓ | ✓ |
| Observations | 268,689 | 21,774 | 12,552 |

Notes: Relating compensation (a measure of career progression) of pre-reform entrants to the postal reform. The unit of observation is the reform wave × individual × city × year. The sample is restricted to individuals that entered prior to the reform. Reform is a dummy that is 1 if the city was reformed, and 0 otherwise. Post is a dummy that is 1 if the year is after the reform year of interest. Columns 2-3 split the sample by whether the individual’s measure of qualification is above or below median. We use the `edscor50` as the measure of qualification, computed as the share of individuals with college degree in 1950 who worked in the civil servants’ occupation prior to entry into the civil service. Standard errors clustered at the city-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table AVII: City-level patents and civil service reform – by reform wave and early/late comparison

| | (1) | (2) | (3) | (4) |
|--|------------------------------------|---------------------|---------------------|---------------------|
| | Number of patents registered (IHS) | | | |
| Mean of dep. var | 1.627 | 1.808 | 1.950 | 1.915 |
| Reform \times Post | 0.104*** (0.025) | 0.297*** (0.083) | 0.080*** (0.026) | 0.094*** (0.026) |
| Reform wave \times City FEs | ✓ | ✓ | ✓ | ✓ |
| Reform wave \times State \times Year FEs | ✓ | ✓ | ✓ | ✓ |
| Sample | Base | 1883 | 1893 | Early/late |
| Observations | 90,804 | 4,941 | 15,028 | 19,969 |

Notes: Relating the number of patents registered to the postal reform in a stacked event-study design, centered around each reform year (See description [Equation 1](#)). The unit of observation is the reform wave \times city \times year. Reform is a dummy that is 1 if the city experienced a postal 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 restricts the sample to only the 1883 reform wave. Column 3 restricts the sample to only the 1893 reform wave. Column 4 looks at the 1883 reform wave by comparing cities reformed in 1883 to those who are eventually treated in 1893. Standard errors clustered at the city \times reform-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A Alternative mechanisms

We discuss two alternative mechanisms – differential selection and assignment – that could also theoretically contribute to the observed performance effects. While we are ultimately agnostic about identifying and quantifying the contribution of each mechanism, the evidence we find for these channels are relatively weaker.

Selection. Since the civil service reform introduced competitive examination in lieu of patronage, it is likely that the change in the selection regime also affected the selection of civil servants. Unfortunately, the personnel records do not contain many individual-level characteristics, thus requiring us to link the civil servants to the full count census. We do so by following the same procedure as in [Aneja and Xu \(2021\)](#), linking individuals uniquely based on their full name, birth state, and current state.

Overall, we do not find strong evidence that the selection of civil servants is a key driver of the performance results. Our conclusion is based on three findings. First, competitive examinations only affected recent hires, whereas the other provisions of the Pendleton Act (e.g., the removal of arbitrary firings) applied immediately to *all* covered civil servants. Importantly, the pace at which new recruits entered through the competitive exams was not sufficiently fast to substantially alter the composition of the workforce. As [Figure AIII](#) shows, the share of civil servants recruited after the implementation of the reform hovered around 50% even ten years after the reform. Second, we do not find that the entrants who entered post-reform differ significantly on observable characteristics. As [Table AV](#) shows based on the measures available from the full count census, we do not find strong evidence for significant changes in the background characteristics. Civil servants who entered post-reform are of comparable age, and have worked in similar occupations prior to joining the service – as measured by the occupational income score and the educational income score. While we observe a slight improvement in the literacy rate, the magnitude is small – 1.6% relative to the mean literacy rate of 97.2% among civil servants – and only marginally significant. Finally, the selection results from [Table AV](#) are based on a match rate of 9%. While this is very comparable to match rates obtained in related work ([Moreira and Perez, 2020, 2022](#)), we are cautious in interpreting the results as we only observe a small subset of all entrants.

Salary progression and assignment. The civil service reform also introduced broader “merit-based” personnel policies. To the extent that the reform improved the allocation of talent to positions, it is possible that better assignment could contribute to the observed performance gain. We test this hypothesis in [Table AVI](#). To separate out effects due to differential selection (if any) from effects of general changes in personnel policies, we conduct the analysis on the subsample of individuals who were already serving prior to the implementation of the reforms. As column 1 of [Table AVI](#) shows, the reform did not come with a change in the general salary schedule (e.g., a salary hike). In columns 2-3, we test whether the allocation of talent (as measured using the education score obtained from census linking) changes markedly. As columns 2-3 show, we do not find that individuals with higher education scores are more likely to see a salary increase post-reform.