# 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 2,800 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 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.

# 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 to 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 ("Post Office"; today the Post Office is known at the United Postal Service, or USPS). 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 or 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 postal reform we study is part of a series of major reforms spurred by the landmark Pendleton Act of 1883. The Pendleton Act introduced merit-based hiring, shielding bureaucrats from political interference and arbitrary firing, and abolished assessment fees as a source of political contributions (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 long, gradual professionalization of the American federal bureaucracy to phase out the pernicious effects of the "spoils" system (Theriault, 2003). An initial wave of Congressional reforms in 1883 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 governmant 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 administrative data and personnel records of the U.S. Post Office from several sources. We draw upon Annual Reports of the Unites 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 Anual 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 2,800 cities for 1875-1905, as well as shed light on the channels through which the improvement occured by constructing detailed measures

of cost efficiency and per unit productivity. We then use data from patents 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, consistent with an increase in the reliability of the postal service. This increase in performance is not driven by an expansion of the civil service personnel and also holds in areas where the postal workforce remained constant. 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 of 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.

The marked increase in postal performance raises the question whether it was conducive to knowledge-based growth. Cities with a reformed post office 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 effects on innovation (Acemoglu et al., 2016). Using patent-level data that includes the timing, location, and parties involved (i.e., inventor or assignee) 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.

Collectively, our results 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 service delivery, and innovation. First, we contribute to the literature on the consequences of public sector reform in the U.S. A substantial literature exists on subnational civil service reforms enacted over the past century (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 state performance outcomes.[1] 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. 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 relatively little examination 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 in the Treasury Department's customs division implemented in 11 ports, finding improvements in the qualification of recruited individuals but no impact on revenue collection (Moreira and Perez, 2020). In a companion paper, Moreira and Perez (2022) study how the 1883 Pendleton 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. 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).[2] 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, which has for two centuries been the largest civil employer within the federal government. By focusing on this elemental state agency, our analysis spans 2,800 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 592 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 and performance, however, is scarcer – in part, owing to the difficulty of measuring performance. While most

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[1] At 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).

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

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., 2020; 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 – shares many classic features of civil service reforms observed even today (World Bank, 2000). The focus on the postal office also allows us to measure performance using delivery statistics – a central outcome that had direct impacts on how reliable information could be transmitted across the country.

Finally, we contribute to the literature on innovation and economic history (Moser, 2013, 2016). A large body of research focuses on the inventors and their incentives to innovate (Cook, 2014; Petralia et al., 2016; Akcigit et al., 2017, 2016; Bell et al., 2018). Research on innovation over the long run often focuses in particular on the role of intellectual property rights. In 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). Researchers have in turn identified a number of law-based drivers of innovation in the U.S. Khan (2005) demonstrates the role of lower patent application fees, Lamoreaux and Sokoloff (1999) 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. More generally, there exists a longstanding interest on the drivers of technological progress over time, such as how invention reacts at 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). Our work adds to this literature 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.

## 2  Context and data

### 2.1  Context

**U.S. Post Office.** Few institutions have been as central to modern statebuilding as the post office (John, 1995; Chong et al., 2014). In the United States, Alexis de Tocqueville in 1831 described the American postal system as a marvel of its day — a "great link between minds." Initiated by the Post Office Act of 1792, the USPS 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 the making of an informed citizenry. Postal systems reduced transaction costs, facilitating contracts and business relationships. Roper (1917), for example, described how the passage of money and goods "pass[ed] through the countless postal channels." 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. Postal service provided a system of banking and remittances, and facilitated job searches during a period of economic transition. The Post Office allowed investors to seek, pursue, and monitor investment opportunities, enhancing the productivity of capital. In short, "[t]he government's commitment to postal service formed part of the foundation for commercial growth." (Henkin, 2006).

**The Post Office as a Locus of Patronage and Corruption.** 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 partiallly 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.[3] In New York City, for example, employement 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 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, extracted a cost in terms of both quality and efficiency of federal government services. A system that experienced its first fiscal shortfall in 1833 ran twenty consecutive multimillion-dollar deficits at the close of the century. Many accounts of fraud and government ineptness involved the customs and postal services. One 19th century newspaper editor complained

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

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 was particularly salient. According to one report, "incompetency, neglect, confusion and drunkenness" that strained "credulity" prevailed in the New York Post Office, and on one occassion an incoming postmaster in New York City even discovered several hundred bags of neglected mail scattered throughout the building (Hoogenboom, 1959). Perceptions of corruption within the federal government led to the creation of a federal commission, the Jay Commission (U.S. House, 1877), which argued that patronage employees were often absent from their posts and the quality of service provided was considered poor at best. In other government reports, complaints were made against the New York and other large 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 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 from 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. Indeed, the statute was designed to target the Post Office because the provision of postal services was the major activity of the federal government in the nineteenth 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%.[4]

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 (Commission, 1893).[5] 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

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

[5]Under the original statute, the President was authorized to extend the provisions of the Pendleton Act to other post offices, and also to other branches of the service (Lyman, 1893). Numerous politcial science accounts (Van Riper, 1958; 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.

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. The Online Appendix provides a detailed documentation.

**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 occured in two big waves: the initial wave of 1883, implementing the reform in the 23 large cities across the U.S., and the wave of 1893 where 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 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 missent or misdirected packages.

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. We collect the annual statements for the period 1879–1901, covering a total of 2,806 cities.

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.[6] 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 in each year. To our knowledge, there has been no systematic examination of the postal service using these administrative data.[7] 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 521 large cities with free urban delivery.

**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. Given the sheer size of 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 other 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 298,042 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 (Akcigit 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. The resulting dataset

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[6]Free City Delivery Service began during the Civil War (U.S. Post Office Department, 1862). Joseph Briggs, a postal employee, convinced postal officials to deliver letters in Cincinatti, Ohio. The Free City Delivery Service expanded over time.

[7]Carpenter (2000) uses data on *rural* free delivery from three states to examine aggregate trends in the cost of postal service during 1890–1915. Our study studies postal performance across the entire nation for 1875–1905.

covers a total of 568,393 patents and 23,622 locations across the U.S.

## 2.3 Descriptive statistics

Figure I shows the locations of all post offices that were open between 1860–1905,[8] 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 compares the characteristics of post offices targeted by the reform to those not targeted, broken down for each reform wave. Panel A compares the characteristics of post offices selected for civil service reform to all remaining post offices in the US. Post offices that became civil service-classified earlier tended to be older and closer to Washington, DC. These post offices also tend to be located in counties with a higher population, greater rates of urbanization and a larger number of manufacturing establishments (Panel B). Finally, Panels C and D report the differences between reformed and unreformed cities for the subsamples for which we have data on delivery errors and free delivery. Once again, these statistics suggest that reformed post offices are more likely to be in larger places: they tend to be offices with a higher class, a larger number of mail carriers and a higher overall volume of mail. In our empirical analysis, we will explicitly assess the extent to which these level differences may affect causal inference by assessing pre-trends and comparing treatment and control locations of comparable sizes.

# 3 Postal reform and performance

## 3.1 Main specification and result

Our objective is to examine how the federal implementation of civil service rules affected postal performance and downstream economic outcomes. Using the data introduced above, we answer this question using a stacked event-study design to estimate the pooled effect of the postal reform on performance, tracking a panel of cities around each reform year. Let $j = \{1883, 1884, ...1893\}$ denote the reform year and $k = \{-4,...10\}$ 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:

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

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[8]The data on post offices is drawn from Richard Helbock's compilation, see Blevins and Helbock (2021).

where $\text{treat}_{ij} = 1$ if city $i$ is reformed in the event year $j$, and 0 otherwise. The indicator variable $\text{post}_{jk}$ is defined as $\text{post}_{jk} = \mathbf{1}[k \geq j]$, taking the value 1 post-reform, and 0 before. $\tau_{jk}$ are year fixed effects for each year around a reform.[9] 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 $\theta_{ij}$ separately for each reform year. Similarly, we cluster the standard errors for the error term $\varepsilon_{ijk}$ at the same $ij$-level, corresponding to the level of treatment.[10]

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 the delivery error rates by 12%. The results also hold – 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). Figure II provides visual evidence for the reform effects by reporting the estimates of the augmented Equation 1 where $\beta$ is allowed to vary by each year around the reform.[11] 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 the 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.

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. To ensure that our results are not driven by a comparison of large vs. small cities, we undertake several additional exercises. First, we can restrict the comparison of reform vs. non-reform cities to locations of similar size by using coarsened exact matching. Specifically, we match treatment and control cities based on the total city-level postal employment and county-level population. As column 3 shows, our results remain comparable even within the narrowly matched sample. Second, we can also restrict the sample to only the event studies of 1883 and 1893, respectively (columns 4-5).[12] Assuringly, as the results show, we observe a significant reduction in delivery errors across *both* major waves of the postal reform. Finally, 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 6 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. Taken together, the results suggest that the postal reform significantly reduced the amount of delivery errors.

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[9]This is equivalent to using reform-specific year fixed effects.

[10]Our results also hold when clustering at the city-level $i$.

[11]We estimate $\log(y_{ijk}) = \Sigma_{l=-4}^{10} \beta_l \text{treat}_{ij} \times \mathbf{1}[k = l] + \theta_{ij} + \tau_{jk} + \varepsilon_{ijk}$, with the year prior to the reform ($k = -1$) as the omitted category.

[12]The results are also not driven by particular cities but are comparable when dropping one reform city at a time (Figure A1) for the reform wave of 1883 or dropping each reform state at a time for the reform wave of 1893 (Figure A2).

## 3.2 Mechanisms and additional outcomes

The reduction in delivery errors we estimate as a result of the postal reform is striking. We probe deeper to shed light on the mechanisms underlying the observed effects. A prominent factor that may be driving the performance results is the expansion of the civil service. As described in subsection 2.1, the civil service reform went hand-in-hand with a range of alternative policies, including a marked increase in the size of the overall federal employment. To test whether our results are driven by an increase in postal employment, we now make use of the digitized Federal Registers which allow us to track the total employment of mail clerks and letter carriers bienially. Table III, column 1 shows the stacked event study estimates for the (log) total postal employment as the dependent variable. As the estimate shows, we indeed observe a significant and economically large increase in the postal employment post-reform. As columns 2-3 show, this increase is primarily driven by an expansion of the postal service in areas that did not have urban free delivery – a mail service that delivered letters and newspapers to the recipients' front door.[13] This differential increase is rapid and coincides with the implementation of the postal reform, as shown in the visual evidence (Figure III).

We provide two complementary empirical tests to show that the lower delivery errors are not driven by an increase in postal workers. First, we can use data on the overall number of postal workers to normalize the delivery errors. As column 4 of Table III shows, we still observe a marked and significant reduction in delivery errors per worker. Second, we can leverage the absence of an expansion of postal employment in areas with Free Delivery (see columns 2) as a placebo test. If the decline in delivery errors is in fact driven by the expansion of the postal service, we do not expect to see an improvement in delivery outcomes in areas with Free Delivery. While the increase in performance is indeed larger in cities that experienced a large postal expansion after the reform (i.e., cities without free delivery, see column 6), we continue to observe a significant and large decrease in delivery errors even in areas where the overall postal employment remained constant (column 5). This suggests that the effects we observe are unlikely to be entirely explained by an expansion of the postal workforce associated with the reform.

We can use additional rich data from the Urban Free Delivery for 1875–1891 to zoom in further.[14] The statistics from the Urban Free Delivery provide information about the number of staff, the overall volumes, as well as cost measures at the city-level. This allows us to obtain not only additional measures of performance but also examine more channels through which delivery outcomes might improve. To start with, we can use data on the annual number of letter carriers to confirm our result relying on biennial Register data. As column 1 of Table IV confirms, we do not observe a significant increase in local postal personnel in cities that were already covered by the Free Delivery, consistent with Table III. The result remains similar when relying on the matched sample

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[13]Table A1 shows that – consistent with the historical evidence on the expansion of urban free delivery – the expansion was concentrated in areas without free urban delivery in 1893. The increase in total employment is disproportionately driven by the increase in letter carriers.

[14]Unfortunately, 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 II) and for the subsample of locations for which we have Free Delivery Statistics.

with comparable postal employment and population (column 2).[15]

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. To test for this possibility, we make use of data on the overall mail volume. Inconsistent with this interpretation, Table IV, column 3 shows an *increase* in the overall volume of mail, implying an increase in productivity as reflected in the higher volume of mail handled per carrier (column 4). We do not observe a significant change in the overall cost per carrier, instead finding a decline in the cost per piece delivered by 13% (columns 5-6). If anything, the results thus suggest an increase in productivity, as measured by the amount of mail handled per carrier and the cost per piece. As before, we can corroborate the regression estimates with visual evidence (Figure IV). We observe a marked relative improvement after the implementation of the reform but no clear pre-trends. Again, these results are not driven by particular treatment cities but also hold when dropping one reform city at a time (Figure A3).

In Panel B, we make use of the rich data to dig further into the drivers of the observed increase in volumes. The postal reform led to an overall expansion of all types of delivered mail, ranging from letters, to postal cards and newspapers. We see a similar increase in the volume of collected mail, suggesting that a strengthened postal service also led to stronger local demand for its services. The results thus suggest that the postal reform led to a greater use of the postal service, consistent with an increase in communication and information flows.

## 4   Impact on patenting

The increase in information flow raises the intriguing question whether a more effective postal service affected economic outcomes. There are multiple channels through which the postal reform can plausibly affect the pace of innovation. First, the postal reform can directly affect the amount of knowledge diffusion through the significant increase in delivery volumes that we observe – for example, in the greater amount of newspapers that are delivered. Second, a more reliable postal service will also likely encourage the amount of communication between individuals and firms, thus reducing search costs and frictions in information flow.

Our main economic outcome is innovation, as measured by patenting. We focus on patenting for several reasons. To start with, innovation is a key driver of growth, and its production process relies heavily on knowledge diffusion and spillovers. More pragmatically, patent data provides rich information on both the timing, location and identities of the parties involved, allowing us to measure the pace of innovation at the city-level.

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 V. The specification follows

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[15]The remaining results all rely on the same stringent CEM sample. Table A2 also reports the results using the unmatched sample.

Equation 1, except that the main dependent variable is now the number of patents registered in a given city and year. Columns 1-2 provide estimates for the full sample, and the remaining columns 3-5 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. The result holds both when comparing among matched cities in the same state (column 2) and when we exclude locations and years without any patenting (column 3).[16] 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. As before, 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 A4, Figure A4 and Figure A5).

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 to have multiple inventors (column 4). The postal reform does, however, increase the likelihood of patents to be co-registered with a business. On average, 2.2% 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 2.7 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 the 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 patenting observed between any two cities in the US to 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 the 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

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[16]In Table A3, we also report the results using the unmatched sample.

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joint patenting at time $t$. We estimate,

$$y_{ijt} = \beta \text{both}_{ijt} + v_{ij} + \tau_t + \gamma' x_{ijt} + \varepsilon_{ijt} \qquad (2)$$

where $\text{both}_{ijt} = 1$ if both cities have a reformed postal office.[17] $v_{ij}$ are city-pair fixed effects. $\tau_t$ are year fixed effects, and $x_{ijt}$ are more flexible controls. Standard errors are clustered at the city-pair level $ij$, corresponding to the level of the treatment variation.

The results are reported in Table VI and confirm the disproportionate increase in patenting observed between entities located in cities that experienced the 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 US to exhibit jointly filed patents is declining with distance, consistent with increased communication costs inhibiting co-generation of knowledge. Interestingly, city-pairs that both experienced a reform see a significant increase in the amount of patenting. Evaluated against the elasticity of 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. The results remain comparable once we allow the impact of distance to vary by year (column 3). 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 a 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, 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 have been as central to modern statebuilding as the post office (Gallagher, 2016; John, 1998). Postal expansion in Western Europe and the United States in the nineteenth 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 mail, the Post Office connected people across a vast and expanding nation.

Our evidence underlines the importance of the postal service – and state capacity more broadly – in providing

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[17]Since the patenting data is not directional (i.e., $y_{ijt} = y_{jit}$), we collapse our data to the city-pair level.

an environment conducive to innovation. Our first set of results provide strong evidence that strengthened state capacity through civil service reform worked, increasing organizational performance and productivity. Importantly, we find that these benefits create downstream effects by fostering knowledge-based growth – innovation. Our results thus have broader implications for the role of the state in generating innovation and growth.

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# 6 Tables and Figures

Table I: Descriptive statistics of reformed and unreformed post-offices

|  | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
|  | Mean | Diff treat-control in year | | |
|  | control | 1883 | 1884-1892 | 1893 |
| *Panel A: All post offices* | | | | |
| Year established | 1877.4 | -81.396*** | -54.129*** | -45.970*** |
|  |  | (8.525) | (5.318) | (1.228) |
| Log(Distance to DC) | 6.31 | -0.645** | -0.105 | -0.186*** |
|  |  | (0.267) | (0.146) | (0.033) |
| Total number of post-offices | 88,894 | 88,917 | 88,924 | 89,433 |
| - of which treatment: | 0 | 23 | 30 | 539 |
| *Panel B: All counties* | | | | |
| Log(Total population) | 8.901 | 3.359*** | 2.357*** | 1.335*** |
|  |  | (0.169) | (0.115) | (0.048) |
| Share urban | 0.002 | 0.727*** | 0.460*** | 0.025*** |
|  |  | (0.054) | (0.050) | (0.005) |
| Manufacturing establishments | 44.519 | 2408.43*** | 575.51*** | 162.28*** |
|  |  | (557.04) | (92.12) | (10.45) |
| Total number of post-offices | 2,102 | 2,125 | 2,130 | 2,518 |
| - of which with treatment: | 0 | 23 | 28 | 416 |
| *Panel C: All cities with error data* | | | | |
| Log(Errors) | 3.506 | 4.477*** | 2.581*** | 1.705*** |
|  |  | (0.250) | (0.226) | (0.076) |
| Post office class | 2.538 | -0.926*** | -0.762*** | -0.951*** |
|  |  | (0.015) | (0.095) | (0.021) |
| Total number of post-offices | 1,695 | 595 | 515 | 1,459 |
| - of which with treatment: | 0 | 23 | 29 | 539 |
| *Panel D: All cities with free delivery data* | | | | |
| Log(Aggregate volume) | 14.251 | 2.764*** | 0.447*** | - |
|  |  | (0.227) | (0.153) |  |
| Log(Mail carriers) | 2.041 | 2.210*** | 0.376*** | - |
|  |  | (0.187) | (0.121) |  |
| Log(Cost per piece) | 6.437 | 0.388*** | -0.025 | - |
|  |  | (0.048) | (0.027) |  |
| Log(Mail handled per carrier) | 12.190 | 0.573*** | 0.073 | - |
|  |  | (0.098) | (0.072) |  |
| Total number of post-offices | 126 | 149 | 154 |  |
| - of which with treatment: | 0 | 23 | 29 |  |

*Notes:* Column 1 shows the mean for the unreformed cities. Columns 2-4 show the difference between reformed vs. unreformed cities for each reform wave. Reform years 1884-1892 are pooled due to the small number of reformed city in each year. The free delivery statistics in Panel D only cover the period up to 1892 so cannot be computed for the 1893 reform year. Robust standard errors are computed for columns 2-4. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table II: Delivery errors and civil service reform – Main result

|  | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
|  | Log(Number of delivery errors ) | | | | | |
| Mean of dep. var | 5.284 | 5.276 | 4.277 | 8.785 | 4.992 | 8.785 |
| Reform × Post | -0.123*** | -0.188*** | -0.189*** | -0.346** | -0.214*** | -0.435** |
|  | (0.045) | (0.049) | (0.096) | (0.175) | (0.056) | (0.176) |
| Reform wave × Year FEs | Y |  |  |  |  |  |
| Reform wave × City FEs | Y | Y | Y | Y | Y | Y |
| Reform wave × Year × State FEs |  | Y | Y | Y | Y | Y |
| CEM |  |  | Y |  |  |  |
| Sample |  | All |  | 1883 | 1893 | Early/late |
| Observations | 158,244 | 157,908 | 10,025 | 10,972 | 16,765 | 6,574 |

*Notes:* Relating delivery errors 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 × 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. CEM matches on the (log) county population and 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. Standard errors clustered at the city × reform-wave level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table III: Improvement in delivery outcomes is not driven by expansion of civil service

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | Log(Postal staff) | | | Delivery errors/Postal staff | |
| Mean of dep. var | 3.162 | 3.327 | 1.044 | 23.46 | 24.03 | 18.96 |
| Reform × Post | 0.663*** | -0.031 | 1.118*** | -11.658** | -9.909** | -29.589** |
| | (0.019) | (0.038) | (0.046) | (5.137) | (4.641) | (13.310) |
| Reform wave × City FEs | Y | Y | Y | Y | Y | Y |
| Reform wave × Year × State FEs | Y | Y | Y | Y | Y | Y |
| Sample | Full | Free deliv | Non Free deliv | Full | Free deliv | Non Free deliv |
| Observations | 362,368 | 43,947 | 317,850 | 68,623 | 32,149 | 36,004 |

*Notes:* Relating the (log) total number of postal workers (columns 1-3) and delivery errors (columns 4-6) to the postal reform in a stacked event-study design, centered around each reform year (See description Equation 1). Since the number of postal workers is only available biennially, we (log) linearly interpolate for the intermediate years. 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. Columns 2 and 5 restrict the sample to cities that had urban free delivery in the period 1875–1891. Columns 3 and 6 restrict the sample to cities without urban free delivery in the same period. Standard errors clustered at the city × reform-wave level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table IV: Free delivery service and 1883 civil service reform

| Panel A: | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | Carriers | Volume | Volume/carrier | Cost/carrier | Cost/Volume |
| Mean of dep. var | 3.694 | 3.695 | 16.43 | 12.73 | 6.803 | 0.969 |
| Reform 1883 × Post | 0.085 | 0.065 | 0.215** | 0.147** | 0.017 | -0.135** |
| | (0.096) | (0.073) | (0.104) | (0.058) | (0.022) | (0.056) |
| City FEs | Y | Y | Y | Y | Y | Y |
| State × Year FEs | Y | | | | | |
| CEM | | Y | Y | Y | Y | Y |
| Observations | 901 | 527 | 527 | 526 | 526 | 527 |
| **Panel B:** | (1) | (2) | (3) | (4) | (5) | (6) |
| | | Volume delivered | | | Volume collected | |
| | Letters | Postal cards | Newspapers | Letters | Postal cards | Newspapers |
| Mean of dep. var | 15.49 | 14.04 | 14.67 | 14.97 | 13.64 | 12.87 |
| Reform 1883 × Post | 0.183* | 0.211* | 0.203* | 0.277** | 0.206 | 0.232 |
| | (0.092) | (0.118) | (0.106) | (0.135) | (0.162) | (0.178) |
| City FEs | Y | Y | Y | Y | Y | Y |
| CEM | Y | Y | Y | Y | Y | Y |
| Observations | 527 | 527 | 527 | 527 | 527 | 527 |

*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. CEM matches on the (log) county population and 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. All dependent variables are log-transformed. Standard errors clustered at the city-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table V: 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 | 0.162 | 1.042 | 1.856 | 9.12e-05 | 0.0215 | 0.109 |
| Reform × Year | 0.125*** | 0.090*** | 0.064** | 0.000 | 0.027*** | 0.079*** |
| | (0.024) | (0.031) | (0.029) | (0.001) | (0.005) | (0.016) |
| Reform wave × City FEs | Y | Y | Y | Y | Y | Y |
| Reform wave × State × Year FEs | Y | Y | Y | Y | Y | Y |
| CEM | | Y | Y | Y | Y | Y |
| Sample | Full sample | | Conditional on non-zero patents | | | |
| Observations | 3,226,718 | 59,887 | 32,887 | 32,887 | 32,887 | 32,887 |

*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 × 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. CEM matches on the following covariates: log(county-level population) is measured by the closest census round prior to the introduction of the reform, 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. Columns 1-3 focus on the full sample and the dependent variable is the (inverse hyperbolic-sine transformed) number of patents registered. In columns 3-6, 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 × reform-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table VI: 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 | 0.0650 | 0.0650 | 0.0650 | 0.0650 | 1.281 |
| Both reformed | 0.061*** | 0.032*** | 0.038*** | 0.065*** | 0.259*** |
| | (0.005) | (0.004) | (0.004) | (0.007) | (0.086) |
| log(Distance) | -0.007*** | | | | |
| | (0.001) | | | | |
| Year FEs | Y | Y | Y | Y | Y |
| City-pair FEs | | Y | Y | Y | Y |
| log(Dist) $\times$ Year FEs | | | Y | Y | Y |
| Sample | | Full sample | | | Non-zero |
| Observations | 117,986 | 117,986 | 117,986 | 117,986 | 3,969 |

*Notes:* Relating the presence of any patenting 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 km) between the city-pair. 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure I: Spatial distribution of postal offices and treatment variation



(a) Data coverage



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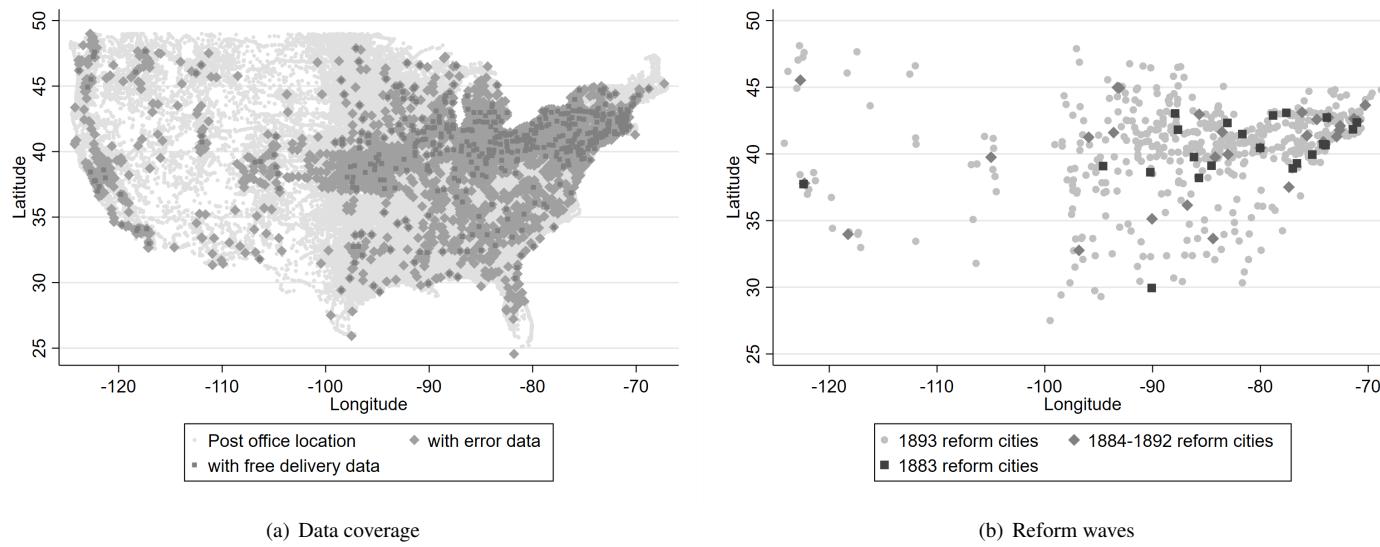
*Notes:* Panel (a) shows the locations of all post offices active between 1860-1905, as well as the locations for which we have performance data on delivery error rates and urban free delivery outcomes. Panel (b) shows the locations of reformed cities and their timing. There are 23 locations that are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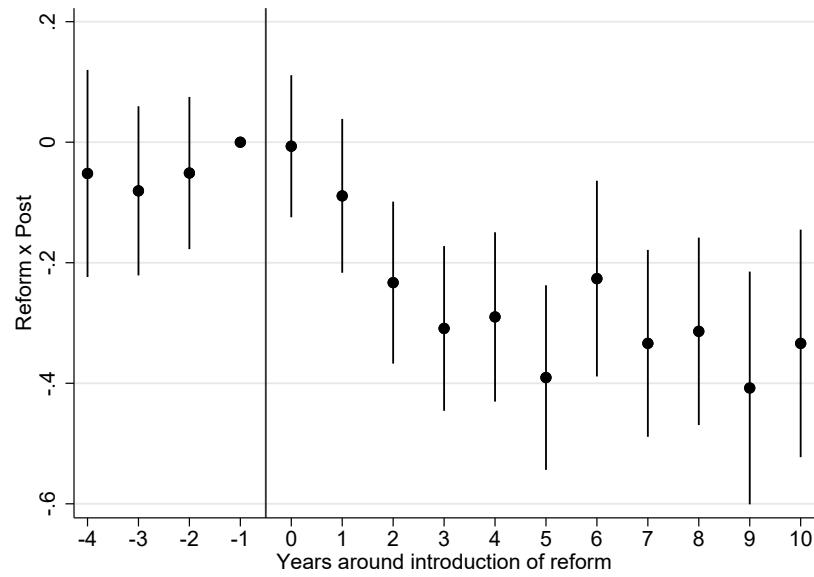


**Notes:** Figure reports an augmented version of Equation 1 (corresponding to Table II, column 2), where the estimate between treatment and control cities is allowed to vary for each year around the introduction of the reform. Reporting 95% confidence intervals. Standard errors clustered at the city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

Figure III: Expansion of postal employment around reform years, by Free Delivery status
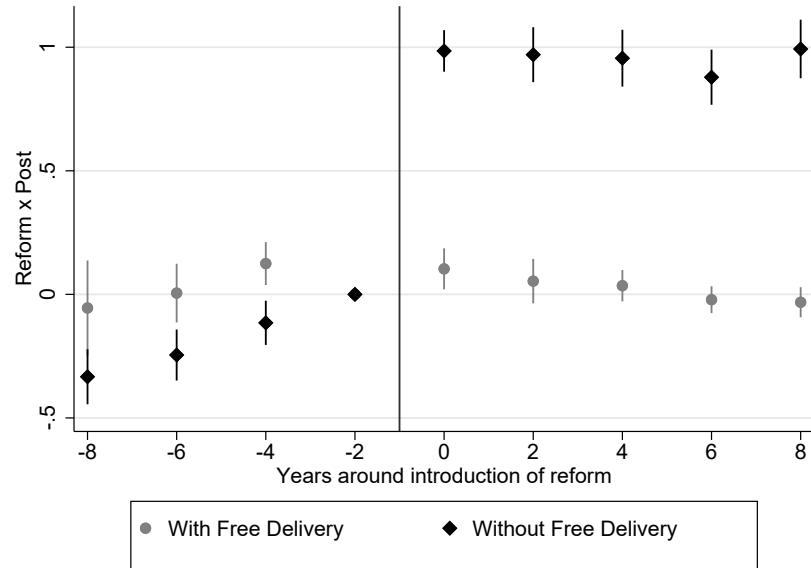


**Notes:** Figure reports an augmented version of Equation 1 (corresponding to Table II, column 2), where the estimate between treatment and control cities is allowed to vary for each year around the introduction of the reform, broken down by whether the city had Free Delivery between 1875–1891 or not. The dependent variable is the (inverse hyperbolic-sine transformed) total number of postal employment in a city (clerks and letter carriers combined). Reporting 95% confidence intervals. Standard errors clustered at the city × reform year-level.

Figure IV: Impacts of 1883 reform on volumes, employment and productivity in Free Delivery
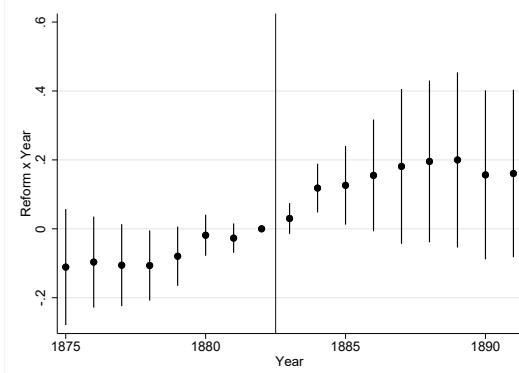


(a) Aggregate volume
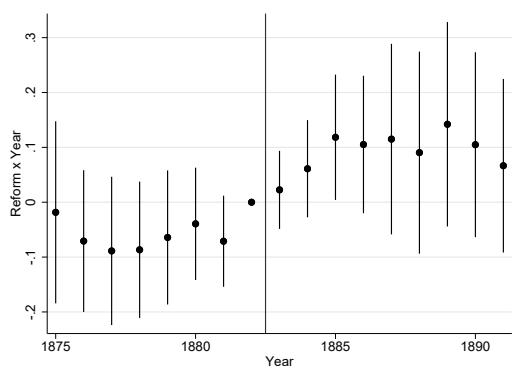
(b) Pieces handled per carrier
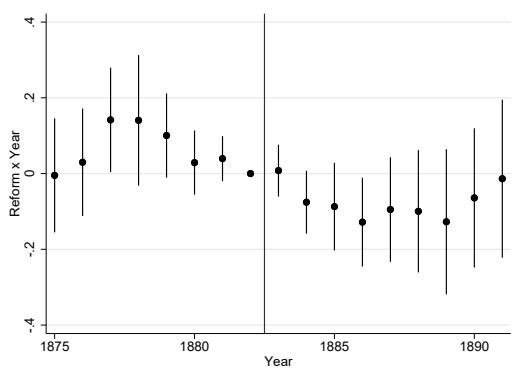
(c) Cost per piece

*Notes:* Figure reports an augmented version of Table IV, where the estimate between the matched treatment (reformed cities in 1883) and control (unreformed cities) is allowed to vary by each year. Reporting 95% confidence intervals. Standard errors clustered at the city-level.

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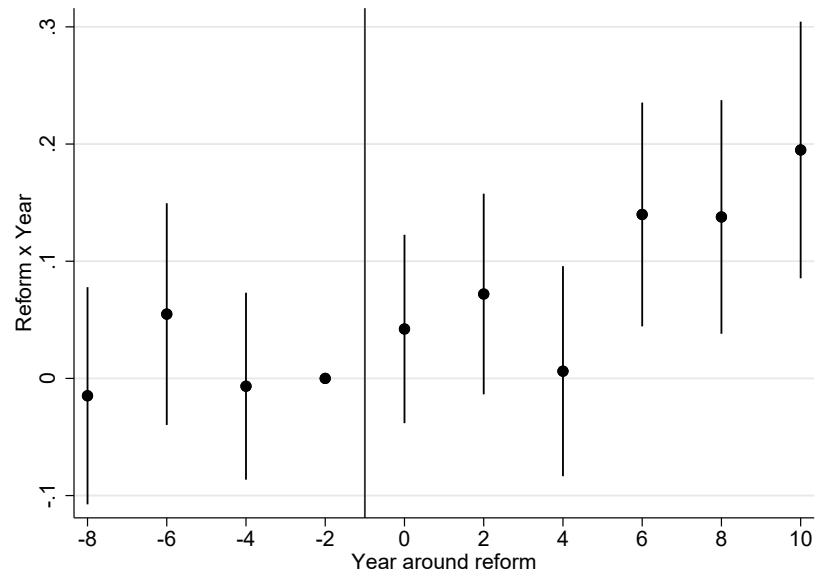


**Notes:** Figure reports an augmented version of Equation 1 (corresponding to Table V, column 3), where the estimate between the matched treatment and control cities is allowed to vary for each year around the introduction of the reform. Reporting 95% confidence intervals. Standard errors clustered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

# Online Appendix

Figure A1: Delivery errors – Robustness of 1883 reform wave results, dropping one treatment city at a time
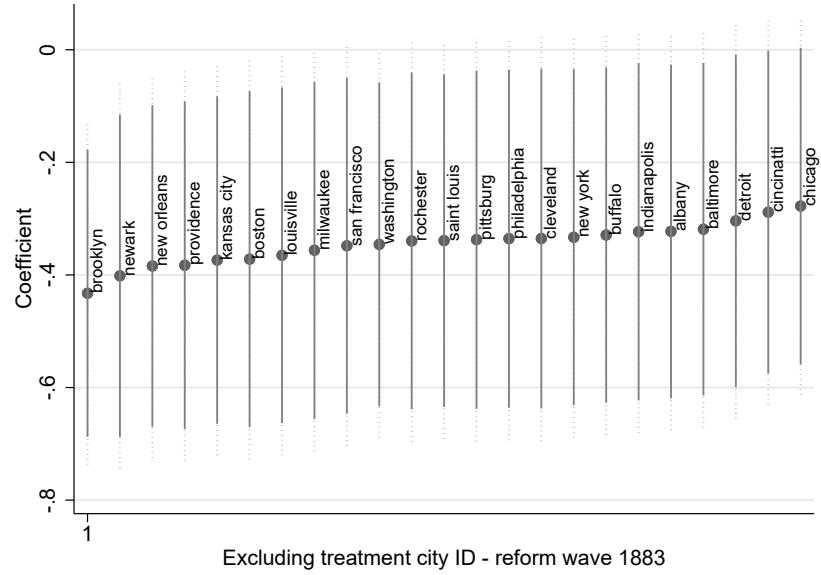


Excluding treatment city ID - reform wave 1883

**Notes:** Reporting coefficients of the Reform $\times$ Post estimate, restricting the analysis to only the 1883 reform (Table II, column 4) 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 A2: Delivery errors – Robustness of 1893 reform wave results, dropping one state at a time
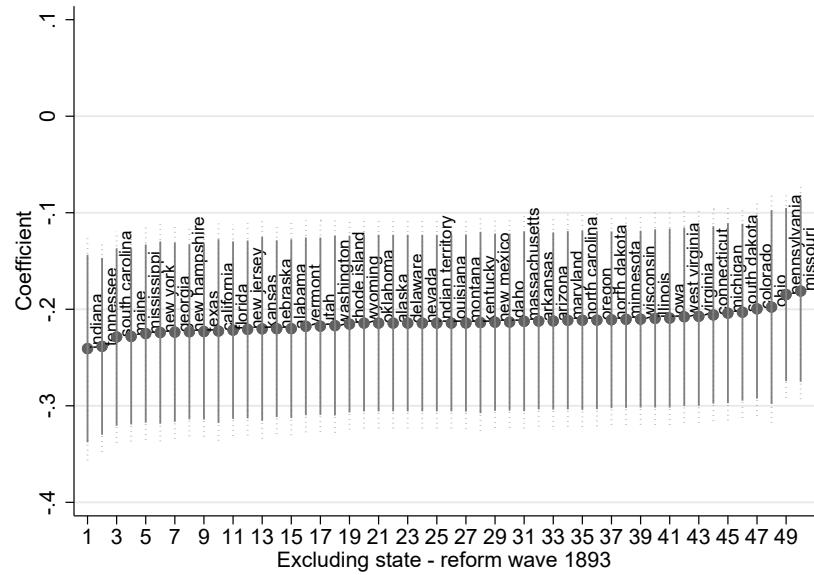


**Notes:** Reporting coefficients of the Reform × Post estimate, restricting the analysis to only the 1893 reform (Table II, column 5) and excluding each state at a time. Reporting 95% confidence intervals in dotted vertical lines, and 90% confidence intervals in solid line. Standard errors clustered at the city-level.

Figure A3: Free delivery volumes, employment and productivity – dropping one treatment city at a time
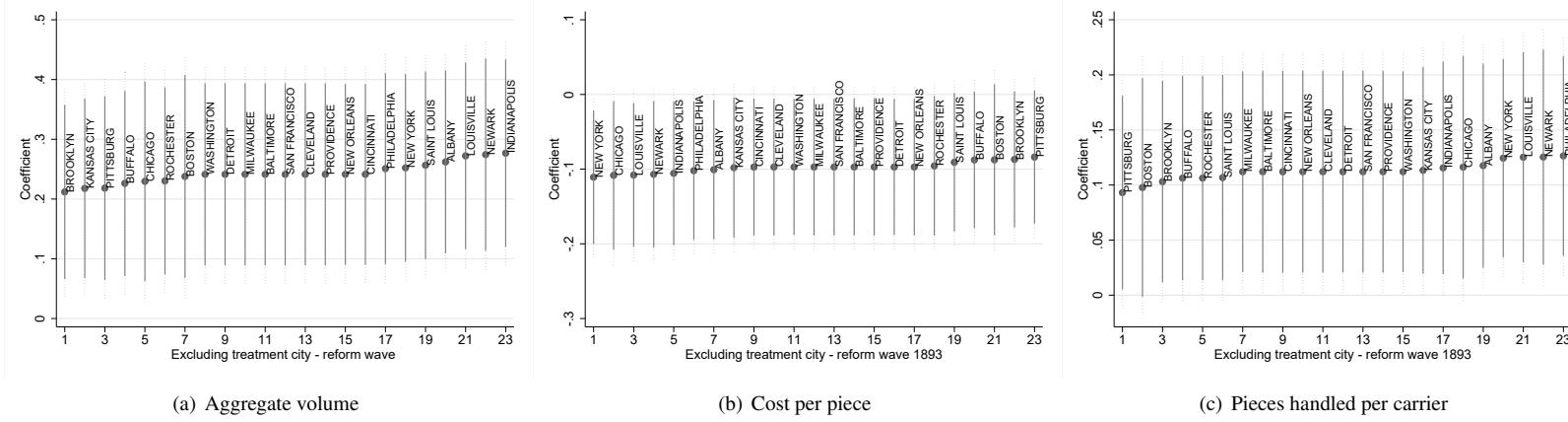


(a) Aggregate volume

(b) Cost per piece

(c) Pieces handled per carrier

*Notes:* Reporting coefficients of the Reform × Post estimate, restricting the analysis to only the 1883 reform (Table IV) 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 A4: Patenting – Robustness of 1883 reform wave results, dropping one treatment city at a time
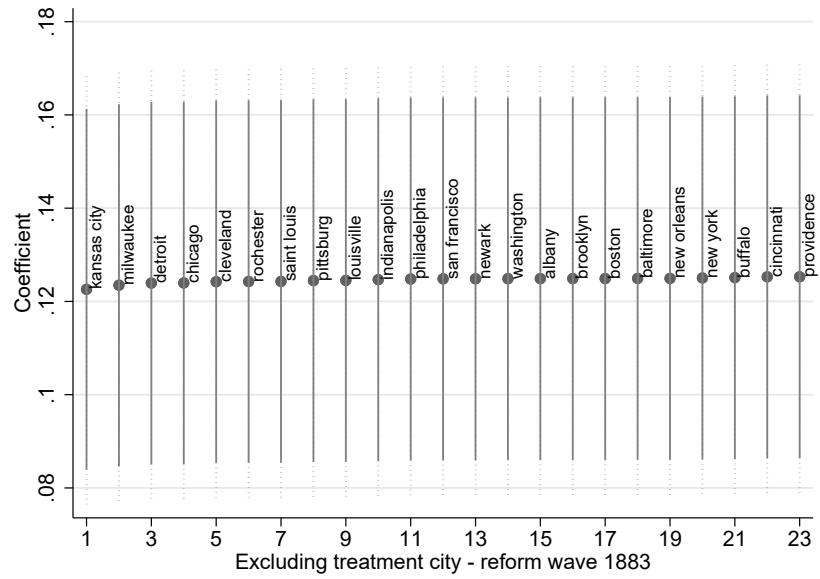


**Notes:** Reporting coefficients of the Reform × Post estimate, restricting the analysis to only the 1883 reform (Table V, column 2) and excluding each of the 23 treatment cities at a time. Reporting 95% confidence intervals in dotted vertical lines, and 90% confidence intervals in solid line. Standard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

Figure A5: Patenting – Robustness of 1893 reform wave results, dropping one state at a time
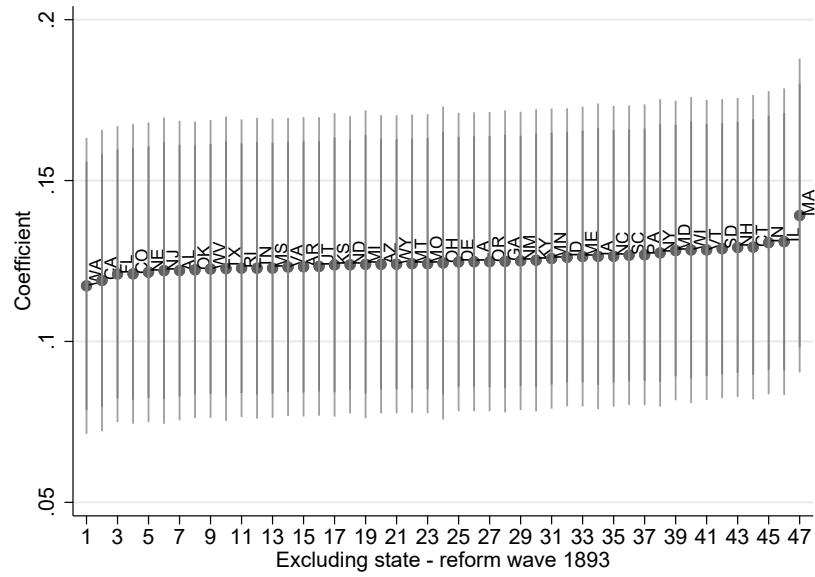


**Notes:** Reporting coefficients of the Reform × Post estimate, restricting the analysis to only the 1893 reform (Table V, column 2) and excluding each state at a time. Reporting 95% confidence intervals in dotted vertical lines, and 90% confidence intervals in solid line. Standard errors clustered at the city-level.

Table A1: Postal employment, civil service reform and expansion of urban free delivery

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Total postal employment (IHS) | | | | Clerks | Carriers |
| Mean of dep. var | 3.162 | 3.327 | 2.210 | 2.210 | 1.644 | 1.288 |
| Reform × Post | 0.663*** | -0.031 | 1.118*** | 1.118*** | 0.237*** | 2.030*** |
| | (0.019) | (0.038) | (0.047) | (0.047) | (0.061) | (0.048) |
| Reform wave × City FEs | Y | Y | Y | Y | Y | Y |
| Reform wave × State × Year FEs | Y | Y | Y | Y | Y | Y |
| Sample | Full | Free delivery | Non free delivery | Non-Free delivery 1893 reform wave | | |
| Observations | 362,368 | 43,947 | 317,850 | 30,924 | 30,924 | 30,924 |

*Notes:* Relating postal employment to the postal reform. In columns 1-4, the dependent variable is the (inverse hyperbolic sine transformed) total number of postal employment (mail clerks and letter carriers combined). In column 5, the dependent variable is the (inverse hyperbolic sine transformed) total number of mail clerks; in column 6, the dependent variable is the (inverse hyperbolic sine transformed) total number of letter carriers. Column 1 is the full sample. Columns 2 focuses on the subsample of cities that had Urban Free Delivery during 1875–1891. Columns 3-6 restricts the sample to cities without Urban Free Delivery, with columns 4-6 looking at the 1893 reform which is driving the increase. Reform is a dummy that is 1 if the city was reformed in the given 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. Standard errors clustered at the city-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: Free delivery service and 1883 civil service reform – unmatched sample

| Panel A: | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Total volume | | Carriers | Volume/carrier | Cost/carrier | Cost/piece |
| Mean of dep. var | 17.03 | 11.16 | 4.425 | 12.77 | 6.799 | 0.924 |
| Reform 1883 × Post | 0.218*** | 0.241** | 0.134** | 0.112** | 0.021* | -0.097* |
| | (0.067) | (0.091) | (0.060) | (0.055) | (0.012) | (0.055) |
| City FEs | Y | Y | Y | Y | Y | Y |
| State × Year FEs | | Y | Y | Y | Y | Y |
| Observations | 1,105 | 850 | 850 | 847 | 848 | 850 |

| Panel B: | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Volume delivered | | | Volume collected | | |
| | Letters | Postal cards | Newspapers | Letters | Postal cards | Newspapers |
| Mean of dep. var | 16.20 | 14.83 | 15.29 | 15.82 | 14.54 | 13.89 |
| Reform 1883 × Post | 0.206** | 0.260*** | 0.254** | 0.244** | 0.305*** | 0.190 |
| | (0.080) | (0.096) | (0.099) | (0.111) | (0.113) | (0.198) |
| City FEs | Y | Y | Y | Y | Y | Y |
| State × Year FEs | Y | Y | Y | Y | Y | Y |
| Observations | 850 | 850 | 850 | 850 | 850 | 850 |

*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 A3: City-level patents and civil service reform – unmatched sample

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Number of patents registered (IHS) | | | Multiple inventors | Any assignees | Multiple cities |
| Mean of dep. var | 0.162 | 0.162 | 1.354 | 1.19e-05 | 0.0126 | 0.0549 |
| Reform × Year | 0.118*** | 0.125*** | 0.101*** | -0.000 | 0.030*** | 0.117*** |
| | (0.024) | (0.024) | (0.021) | (0.001) | (0.003) | (0.012) |
| Reform wave × City FEs | Y | Y | Y | Y | Y | Y |
| Reform wave × State × Year FEs | | Y | Y | Y | Y | Y |
| Sample | Full sample | | | Conditional on non-zero patents | | |
| Observations | 3,226,808 | 3,226,718 | 337,010 | 337,010 | 337,010 | 337,010 |

*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 × 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. Columns 1-3 focus on the full sample and the dependent variable is the (inverse hyperbolic-sine transformed) number of patents registered. 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 × reform-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: City-level patents and civil service reform – by reform wave and early/late comparison

|  | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
|  | Number of patents registered (IHS) | | | | |
| Mean of dep. var | 0.162 | 0.145 | 0.215 | 0.180 | 0.815 |
| Reform $\times$ Year | 0.125*** | 0.357*** | 0.106*** | 0.118*** | 0.126*** |
|  | (0.024) | (0.069) | (0.025) | (0.024) | (0.026) |
| Reform wave $\times$ City FEs | Y | Y | Y | Y | Y |
| Reform wave $\times$ State $\times$ Year FEs | Y | Y | Y | Y | Y |
| Sample | Base | 1883 | 1893 | Early/late | Register |
| Observations | 3,226,718 | 322,458 | 326,920 | 649,378 | 191,617 |

*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. Column 5 restricts the sample to cities with mail clerks and letter carriers listed in the Registers. Standard errors clustered at the city $\times$ reform-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.