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WHO BENEFITS FROM MERITOCRACY?

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ABSTRACT

Individuals from lower-income backgrounds are underrepresented in high-status occupations. This underrepresentation has coincided with increasing scrutiny of the "meritocratic" criteria shaping access to these positions. We study the equity impacts of a prominent example of meritocratic selection: civil service exams. To do so, we use evidence from the Pendleton Act, a historical reform that introduced such exams to select U.S. federal employees. We find that, although the reform increased the representation of "educated outsiders" (individuals with high education but limited connections), it reduced the representation of lower-SES individuals. This reduction was stronger among applicants from states with high educational inequality.

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An online appendix is available at http://www.nber.org/data-appendix/w30113

1 Introduction

Individuals from lower-income backgrounds are underrepresented in high-status occupations, including academics (Morgan et al., 2022; Stansbury and Schultz, 2023; Airoldi and Moser, 2024; Abramitzky et al., 2024), inventors (Bell et al., 2019), and politicians (Dal Bó et al., 2017; Thompson et al., 2019). This underrepresentation has coincided with growing scrutiny of the "meritocratic" criteria that shape access to these positions. Critics argue that while such criteria can help select more qualified workers, they often disproportionately exclude individuals from lower socioeconomic backgrounds (Markovits, 2020; Sandel, 2020). These concerns have also emerged in the public sector, where they are amplified by perceptions of 'out-of-touch' technocrats and rising distrust in government (Carnes, 2013; Kingsley, 1944).

In this paper, we study a prominent example of meritocratic selection: civil service exams. These exams are a hallmark of modern bureaucracies, with nearly 80% of countries using formal examinations to select some of their public employees (Teorell et al., 2011). Historically, these exams were introduced to improve workers' qualifications, with evidence suggesting that they achieved this goal in certain contexts (Grindle, 2012; Ornaghi, 2016; Aneja and Xu, 2022).

Yet, the equity impact of these exams remains unclear. On the one hand, they have sometimes been described as a tool for excluding ordinary people from government jobs (Hofstadter, 1955). According to this view, civil service exams were often introduced to legitimize the access of individuals with greater cultural and educational capital to positions of power (Bourdieu, 1998). On the other hand, exams have also been credited with *promoting* upward social mobility. For instance, Cronbach (1975) writes that "proponents of testing [...] have wanted to open doors for the talented poor, in a system in which doors are often opened by parental wealth and status."[1]

We investigate whether the introduction of civil service exams increased or decreased the representation of lower socio-economic status individuals in the context of the US Federal Government. Our analysis focuses on the 1883 Pendleton Act, a landmark reform in American history that introduced competitive exams for the selection of certain federal employees. Like in many countries today, federal jobs in late 19th-century America were highly coveted, offering higher salaries and greater stability than comparable private-sector jobs (Aron, 1987; Finan et al., 2017). Before the reform, these jobs were allocated at the discretion of government officials, often based on political or personal connections

[1] Similarly, Zhao (2014) describes the Chinese civil service exams as "a tool to identify and recruit the most capable and virtuous individuals into government instead of relying on members of the hereditary noble class."

(Aron, 1987). After the reform, however, certain positions were required to be allocated to the highest-scoring applicants through an open exam. We find that the reform *reduced* the representation of individuals from disadvantaged backgrounds while increasing the representation of the middle class. We argue that middle-class applicants benefited from the reform because they were overrepresented among "educated outsiders"–individuals with high levels of education but limited connections.

Our setting offers several empirical advantages for studying the equity consequences of civil service exams:

First, the reform allows us to compare the background characteristics of individuals hired to do the *same job* in the *same office*, some selected through exams and some selected through informal criteria. Moreover, we can exploit the fact that not all federal positions were initially subject to exams. Specifically, among positions in the Executive Departments in DC (our main focus in this paper), the reform exempted those at the bottom (such as laborers) and those at the top (such as bureau chiefs) of the bureaucratic hierarchy. We use this feature of the reform to estimate difference-in-differences models, comparing the characteristics of employees hired before and after the reform, in exempted and non-exempted positions.

Second, we observe unusually rich data on employees' socioeconomic backgrounds, including parental wealth, parental occupations, country of origin, and race. This enables us to assess the equity impacts of the reform beyond its effects on workers' ethnic and racial mix. To assemble these data, we first digitized federal personnel records spanning 1871 to 1893, roughly a decade before and after the reform. These records include employees' names, birthplaces, salaries, and job titles. We then used name-based matching techniques (Abramitzky et al., 2019) to link these records to US population censuses, enabling us to observe workers' socioeconomic backgrounds.

Third, unlike with more recent reforms, we can assess both the *short-* and the *long-run* effects of these exams. Doing so is important, as longer-term impacts of exams may differ might differ from their immediate ones. For instance, exams might initially benefit lower-SES candidates but then lose their equalizing force as exam-preparation tools (to which the rich might have better access) emerge. Indeed, we document a rapid emergence of such tools in our setting–including tutoring services and test-preparation books.[2]

Our main finding is that the reform led to an immediate and persistent decline in the representation of lower-SES individuals, lasting at least a decade. First, employees hired through exams came from families six percentile ranks higher in the national wealth distribution. This increase was driven by a reduced share of workers with parents at the

[2] We provide more details on this issue in Section 2.

bottom of the wealth distribution and an increase in the share of workers from upper-middle class families. Surprisingly, the upper class remained unaffected by the reform– they were and continued to be overrepresented. Second, the reform raised the share of employees with higher-status parental occupations. We find a five percentage points increase in the proportion of children of professional fathers (nearly a 50% increase) and a corresponding decline in the proportion of children of blue-collar fathers. Finally, the reform reduced the share of first- and second-generation immigrants, by four and seven percentage points, respectively.

Interestingly, although the reform reduced the representation of individuals from lower-*class* backgrounds, it did not alter the (already low) proportion of Black employees. This finding suggests that focusing solely on race would have provided an incomplete picture of the equity consequences of the reform.

There are two main channels through which the introduction of exams might have reduced the proportion of lower-SES workers. First, the shift away from patronage might have improved the prestige of government jobs, thus attracting more higher SES-applicants– a change in the applicant pool. Second, in a setting characterized by large class and racial educational inequalities, emphasizing education in hiring might have disproportionately favored higher-SES individuals– a change in who gets selected for a given applicant pool. Although we cannot fully disentangle between these two channels, the evidence is more consistent with the second interpretation.

We first introduce a conceptual framework formalizing this interpretation. This framework helps clarify *when* lower-SES might benefit from a shift toward "merit". In this framework, access to jobs depends on two attributes, "education" and "connections", both potentially tied to applicants' socioeconomic status. We conceptualize the reform as an increase in the weight of education in the hiring process. Therefore, the reform helps "educated outsiders": applicants with high education but few connections. Whether lower-SES individuals are helped by the reform depends on how "education" and "connections " are distributed across social groups. For instance, when there is a stronger positive association between social class and education than between social class and connections, increasing the relative importance of education could actually hurt the representation of lower-SES individuals.

We then present several pieces of evidence that support this interpretation:

First, we show that the reform indeed boosted the representation of "educated outsiders". Specifically, exam-appointed employees were likely better educated than those appointed through patronage: they were more likely to have held professional jobs–such as lawyer or accountant –prior to joining government, and were also more likely to have grown up in counties with higher per capita schools and teachers.[3] Moreover, exam-based hires were also more likely to lack the connections that facilitated access to patronage jobs: they were less likely to have a father who was himself a bureaucrat, less likely to grew up in DC, and less likely to hail from a county in which a majority of voters had supported the incumbent party (suggesting a decline in political favoritism).

Next, we show that middle-class individuals (the group which increased its representation after the reform) were likely overrepresented among the "educated outsiders". First, although such individuals were, on average, more educated than those from poorer backgrounds, in the pre-reform period they represented a *similar* fraction of workers in the white-collar positions that eventually became subject to exams. Moreover, we show that, after the reform, the representation of middle-class individuals in government jobs became closer to their representation in *private* sector white-collar jobs.

Finally, we document that exams most harmed the chances of lower-SES candidates when such applicants hailed from states with high inequality in access to schooling–namely, the places where lower-SES individuals were the least likely to acquire education and hence were unlikely to be part of the group of "educated outsiders".

One implication of our findings and conceptual framework is that, in our context, "connections" must have been distributed more equitably than "education." The most likely explanation for this pattern relates to the role of political *patronage* in an era marked by the rise of mass-based political parties and urban political machines (Brown and Halaby, 1987). In this setting, patronage may have "democratized" access to political connections, thereby facilitating the entry of lower-SES candidates into government jobs (James, 2006). Indeed, we show that groups historically associated with urban political machines–such as immigrants and urban residents–saw a decline in their representation in government jobs following the reform.

Before turning to the related literature and historical background, we offer a note on interpretation. Our findings *should not* be interpreted as indicating that selecting employees through patronage is preferable to selecting them through competitive civil service exams. Rather, they indicate that, if one worries both about equity and efficiency in public sector hiring decisions, exams might not necessarily lead to improvements on both margins as has often been argued.

Related Literature. Our paper most closely relates to the literature on the effects of civil service reforms, which has focused on whether such reforms improve government efficiency (Ornaghi, 2016; Xu, 2018; Estrada, 2019; Moreira and Pérez, 2021; Aneja and Xu,

[3] As censuses prior to 1940 do not include information on years of schooling, we cannot directly investigate if exam-appointed employees had completed more schooling.

2022).[4] By contrast, motivated by evidence on the importance of representation in public organizations (Kingsley, 1944; Neggers, 2018; Alsan et al., 2019; Xu, 2020), we examine their effects on bureaucrats' social origins. Although social scientists have long been interested in this issue (Elman, 1991; Bourdieu, 1998), a key innovation of our study is that we compare the representation of different social groups when selection is through exams to their representation under *alternative* selection criteria.[5] Our results suggest that assessing exams' potential equity impacts requires understanding how the attributes rewarded by exams–and by alternative systems–are distributed across social groups. For instance, historians argue that exams *promoted* social mobility in China, as they replaced a system in which access to jobs was based on membership in an "hereditary noble class" (Elman, 1991). By contrast, exams in our context replaced a patronage system in which connections were potentially available to the "common person" (Greene, 1984).[6]

Our findings on the representation of first- and second-generation immigrants in government relate to those of Kuipers and Sahn (2023). This study uses data from the decennial census and finds that civil service reforms *increased* the representation of immigrants in the local governments of small and medium-sized American cities. We complement this work by using detailed annual personnel records from the federal bureaucracy, compiling information on workers' *social class* in addition to their countries of origin, and focusing on a setting that allows us to more precisely isolate the impacts of exams *per se*. Indeed, a potential explanation for our differing findings is that local civil service reforms were broader in scope, often including the introduction of job security in addition to civil service exams. If foreign-born employees were already more prevalent before the reform, job security alone could have increased the *stock* of such employees. Another possible

[4] In a recent paper, we investigate the consequences of the Pendleton act for the functioning of the US Customs Service (Moreira and Pérez, 2021). We deviate from this paper with respect to the research question, data, and empirical strategy. First, while Moreira and Pérez (2021) studies the consequences of the reform for the *efficiency* of the US Customs Service, we focus on how the reform affected workers' social origins across the Federal administration. To do so, we digitize personnel records spanning every executive Department in DC (rather than just the Customs Service), and collect information on employees' family backgrounds by linking these records to population censuses. Finally, our current analysis exploits variation in exam requirements across *positions*, whereas Moreira and Pérez (2021) exploits variation across different customs-collection districts. Aneja and Xu (2022) investigates the consequences of the Pendleton Act for the *efficiency* of the US Postal Service.

[5] For instance, Bourdieu (1998) and Grindle (2012) argues that, in Western societies, the introduction of civil service exams was inconsequential for the social origins of government officials and that its primary goal was to legitimize the status quo. By contrast, Elman (1991) argues that, in Imperial China, exams facilitated social mobility (based on the fact that 40-60% of exam-selected candidates came from non-elite backgrounds). See Bai and Jia (2016) for a summary of historical studies in China.

[6] There are also papers studying the broader consequences of civil service exams, for example their impacts on development (Evans and Rauch, 1999; Rauch and Evans, 2000; Chen et al., 2020) or political outcomes (Theriault, 2003; Folke et al., 2011; Bostashvili and Ujhelyi, 2019).

explanation for the difference in findings is that we focus on the bureaucracy in a large city, where political machines targeting lower-class individuals may have played a more significant role in job allocation prior to the reform.[7]

More broadly, we contribute to the literature on personnel policies and workplace inequality.[8] Similar to ours, a number of studies in this literature focus on the equity implications of adopting more impersonal, less discretionary hiring criteria.[9] We contribute to this literature by providing some of the first evidence on the consequences of exams, a common (and controversial) recruitment tool.[10] Our findings show that reducing discretion might not necessarily improve the representation of lower-SES individuals.

Closest to our paper in this literature is Autor and Scarborough (2008), who find that, in a retail firm, the introduction of exams *did not* have adverse equity effects (as measured based on the proportion of hires from minority backgrounds). There are two potential explanations for our different findings. First, firms in the private sector might have weighted education substantially in their hiring decisions even in the absence of exams, so introducing exams in such context might have had more muted effects than in our setting. As we state above, this difference highlights how considering the *alternative* criteria to exams is crucial for assessing exams' equity implications. Second, we study exams introduced for highly desirable positions in which higher-SES individuals were already overrepresented prior to the reform. Therefore, relative to Autor and Scarborough (2008) (who focus on low-skill retail jobs), we study a context in which higher-SES individuals might have had more incentives to further expand their representation.

[7] Another source of difference may stem from our use of actual personnel records rather than census data. Personnel records may be particularly valuable given the challenges of accurately identifying government workers in 19th- and early 20th-century census data. Before 1930, the population census did not include a question on industry of employment. To address this issue, IPUMS imputes an industry classification based on a person's reported census *occupation*. This means, for instance, that a lawyer working for the government would only be classified as such if they reported being a "government lawyer" rather than just a "lawyer." Our personnel records further reduce measurement error relative to the census, as we can distinguish new employees (versus the existing stock) and determine whether a given position was subject to examination (versus being exempted).

[8] This literature has studied policies such as relying on referrals for hiring (Beaman et al., 2018), performance pay (Castilla, 2008), or more flexible salary structures (Biasi and Sarsons, 2022).

[9] For instance, Goldin and Rouse (2000) study the impacts of screening applicants using blind auditions. Mocanu (2022) studies the effects of introducing more impartial selection criteria in the context of the Brazilian public sector.

[10] Harvard Business Review (2015) reports that "about 76% of organizations with more than 100 employees rely on assessment tools such as aptitude and personality tests for external hiring".

2 Historical Background

2.1 Spoils System and the Civil Service Reform Movement

Before the reform, hiring decisions in the federal civil service were governed by the "spoils system." Under this system, appointments were based primarily on political and personal connections rather than on merit or "fitness for office." As Aron (1987) describes, "who an applicant knew counted at least as much as the skills he or she could demonstrate." Patronage jobs were used both to reward political supporters and to sustain political machines, often requiring workers to contribute a portion of their wages (Hoogenboom, 1968).

While pressure for civil service reform had been mounting since the 1860s, the timing of its enactment was driven by two key political events:

First, in July 1881, President James A. Garfield was shot by a disappointed office seeker (Garfield would die by September). This assassination provided reformers with a stark example of the negative consequences of the spoils system and brought civil service reform to the forefront of the national agenda. Soon after, Democratic Senator George H. Pendleton introduced a civil service reform bill.

Second, Democrats gained control of the House in 1882. Fearing they would lose the 1884 presidential election, Republicans backed the bill in an effort to protect Republican officeholders from politically motivated dismissals (Hoogenboom, 1959). In January 1883, President Chester Arthur signed the Civil Service Reform Act into law.

2.2 The Pendleton Act

Positions Subject to Exam. The act's main provision required that certain "classified" positions within the executive branch be filled through open, competitive, and anonymously graded exams (Civil Service Commission, 1893).[11] It divided the classified civil service–those subject to exams–into three groups: the "classified departmental service" for employees in the executive departments in Washington, D.C., the "classified Customs Service" for customs employees, and the "classified Postal Service" for postal workers.

The classified departmental service in Washington, D.C.–our main focus in this paper–was initially limited to employees (1) in clerical or technical positions and (2) with annual salaries between $900 and $1,800. In addition to exempting clerical workers with very low or very high salaries, the law excluded two other groups:

First, it exempted workers in hierarchical positions, such as bureau chiefs, elected officers, and employees requiring Senate confirmation. Second, it excluded those employed

[11] Employees in the legislative and judicial branches were exempt from exams.

"merely as laborers or workmen." Thus, the law primarily targeted the "middle" of the state hierarchy while exempting both the lowest and highest levels.[12]

Figure 1 displays the total number of workers in the Executive Departments in Washington, D.C., as well as the share employed in positions that became subject to exams between 1883 and 1893. The number of employees grew in the decade prior to the reform, reflecting the expansion of government functions in the postâCivil War era, but stabilized in the 1880s (Libecap and Johnson, 2007).[13]

The share of workers in positions subject to exams remained stable throughout the period, fluctuating around 60%. However, while this share remained relatively stable between 1883 and 1893 in the Executive Departments in Washington, D.C., it increased over this period in the federal government as a whole, as positions outside Washington, D.C. were gradually added to the classified system.[14]

Additional Provisions of the Law. In addition to introducing exams, the law required that positions in the classified departmental service be "apportioned" among states based on population. Consequently, applicants for these positions effectively competed only against others from their home state. In our analysis, we sometimes include home-state fixed effects to isolate the reform's impact from apportionment-induced changes in employees' regional origins. However, the results remain similar regardless of whether these fixed effects are included.

The law also prohibited the dismissal of employees who refused to pay political assessments or, more broadly, engage in political activities unrelated to their job. These provisions, however, applied to the *entire* federal civil service rather than only to employees in classified positions.[15] Thus, the key distinction between classified and non-classified employees was that the former were appointed through an exam, while the latter could still be appointed at the discretion of government officials.

Finally, although the act changed the method for filling certain federal positions, it is important to note that it did not grant tenure to employees: classified workers remained

[12]The classified customs and postal services were initially restricted to customs-collection districts and post offices with at least 50 employees, and to employees earning no less than $900 within these offices.

[13]The increase from 1881 to 1883 corresponds to expansions in the Pension Office of the Interior Department, which added 800 employees, and the Medical Department of the War Department, which added 300. The 1891 increase reflects the temporary hiring of 2,500 workers to tabulate the 1890 census.

[14]For instance, the Railway Mail Service was added to the classified service in 1889.

[15]Skowronek et al. (1982) notes that these provisions "applied to the entire service, not just those subject to merit appointments." The law explicitly stated that assessments could not be required from "*any* officer, clerk, or employee of the United States, or any department, branch, or bureau thereof, or from any person receiving any salary or compensation from moneys derived from the treasury of the United States" (Civil Service Commission, 1893).

subject to removal as administrations changed (Johnson and Libecap, 1994).[16] Later reforms introduced the principle that employees could only be removed for "just cause" (Johnson and Libecap, 1994).

Exam Characteristics. The law required that exams focus on practical knowledge relevant to an applicant's future position rather than academic content.[17] Applicants for copyist or clerk positions–the most common occupations in the classified service–were required to complete exams in four subjects: orthography, copying, penmanship, and arithmetic.[18] These subjects aligned with the standard curriculum taught in American common schools, known as the "three Rs"–reading, writing, and arithmetic.[19]

Applicants for positions requiring technical or scientific expertise were also required to take "supplementary" or "special" exams. Examples include the "meteorological clerk" exam in the Department of Agriculture and the "medical examiner" exam in the Pension Office. Figure A3 shows example exam questions.

The emphasis on practical skills differed from the approach taken in other countries (Hoogenboom, 1959). For instance, Grindle (2012) argues that 19th-century English civil service exams were designed so that their content would only be accessible to applicants with “elite educations at Oxford and Cambridge”. In contrast, the US Civil Service Commission maintained that “a common school education was sufficient to pass examination” (Hoogenboom, 1959). Indeed, applicants with only a “common school” education regularly took and passed the exams. Figure A4 shows that applicants with only a common school education were the largest group of applicants and featured a 55% passing rate.

How did Applicants Learn About and Prepare for the Exams? The law required for exams to be held throughout the country: Figure A5 shows the location of all exams from 1886 to 1893, with each circle drawn in proportion to the number of exams per location.[20] At the beginning of the year, the Civil Service Commission issued a pamphlet with exams' dates and locations (Civil Service Commission, 1893). Moreover, this information was

[16] "The power to remove for even the most partisan and selfish reasons remains unchanged" (Civil Service Commission, 1893). The only exception, as described above, was that employees–in all positions, not just those in classified jobs–could no longer be removed for refusing to perform a political service.

[17] The typical duties of a clerk involved "routine, repetitive tasks," often including recording and copying (Aron, 1987). Examples of such tasks include "note signing" and "writing and recording patents."

[18] The exam for clerks was referred to as the "general exam", whereas the exam for copyists was referred to as the limited exam". The general exam could additionally include subjects such as bookkeeping and U.S. history.

[19] "They include no foreign language, no technical word, no terms of art or science, no problem in algebra, geometry, trigonometry, or astronomy, no question concerning the history or geography of any foreign country; nothing, in short, beyond, and not everything within, the teaching of a good public school." (Civil Service Commission, 1893)

[20] For instance, there were a total of 286 exams between June 1892 and June 1893, with at least one exam per US state (Civil Service Commission, 1893, p.141, Table 1).

also regularly reported in local newspapers, as illustrated by the examples in Figure A6.[21]

Exam sample questions were available from the reports of the Civil Service Commission (Civil Service Commission, 1893). Over time, these sample questions also became available from non-governmental, test-preparation books.[22] Furthermore, applicants could also resort to the help of exam tutors. These tutors were available as early as 1883 (the year of the reform), as evidenced by the presence of newspaper ads offering their services (see Figure A6). This rapid market response to the reform is consistent with the high desirability of federal jobs in this period (Aron, 1987).

Appointing Procedure. Applicants who passed the exam were added to a list of eligible candidates. On the opening of a vacancy, the Civil Service Commission produced a list of the *top four* candidates for the position, with the ranking based solely on exam scores. For positions in the classified departmental service (which were subject to apportionment rules), these four names had to belong to applicants from states with the "strongest claim" to an appointment. Appointing officers were required to choose from these four candidates, drastically reducing hiring discretion.[23] An important deviation from meritocratic principles is that recruiters could ask for an employee of a *specific gender* (for instance, a "male clerk"). Indeed, 85% of exam-appointed employees in 1883–1893 were male.

How Attractive were these Positions? Clerkships in DC were "highly coveted and difficult to secure" (Aron, 1987). Panel (a) in Figure A4 shows the yearly number of applicants to the classified departmental service. From 1883 to 1893, nearly 150,000 individuals completed an exam to join the classified civil service, of which 30,000 applied to the departmental service in DC. Panel (b) shows that the fraction of applicants who obtained a passing grade was fairly stable over our period, hovering around 65%. Finally, Panel (c) shows, out of all applicants with a passing grade, the proportion who were appointed to a position. By 1893, only 23% of those who had obtained a passing grade in the previous decade had received an appointment.

Expected Effects of the Reform. Ex-ante, it is unclear whether such a reform would improve or worsen the representation of individuals from disadvantaged backgrounds. On the one hand, the historical literature emphasizes how applicants connected to influential individuals were more likely to secure positions under the patronage system. For instance, Ziparo (2017)'s analysis of application files finds that, among women appointed

[21] For instance, searching for the expression "civil service examination" in `newspapers.com` and restricting the search to US newspapers yielded 700 results for 1883, 1,300 for 1884, and 2,600 for 1885.

[22] For instance, in 1897 *Hinds and Noble* published the book "How to Prepare for a Civil-service Examination With Recent Questions and Answers" (Leupp, 1898).

[23] This number was further reduced to three in 1888 (Civil Service Commission, 1893, p.128).

to federal jobs in the 1860s, 71% had been recommended by a member of Congress.[24] Similarly, Aron (1987) describes a number of cases where employees secured their position through a family connection with a member of Congress. To the extent that individuals from disadvantaged backgrounds were less likely to have these social connections, the reform could have improved their representation.

On the other hand, Libecap and Johnson (2007) emphasize how patronage was "viewed as a means of democratizing the government" as "anyone with the right political connections could obtain a government job, at least for a short while."[25] According to this view, the rise of "mass-based political parties" in 19th-century US had created patronage opportunities for the "common person" (Greene, 1984). Indeed, a common argument in the historical literature is that "merit-based" reforms were introduced during the Progressive era with the intent of curtailing the influence and representation of "newcomers" including recent immigrants (Kuipers and Sahn, 2023).

3 Data

3.1 Federal Personnel Records

Our main source of data are the "Official Registers of the United States" (Department of the Interior, 1893) (henceforth, the Registers). The Registers were published biennially and contain detailed information on the Federal workforce, including employees' names, birthplaces, state of residence at the time of appointment, position, unit, and compensation. We digitized the 12 registers published between 1871 and 1893, roughly ten years before and after the reform.[26] Our main data include approximately 100,000 employee-years in the executive departments in DC, of which about 25,000 correspond to male *new hires* in these departments (our main focus).[27] Figure A2 shows an example page.

[24] Moreover, "of the successful applicants without congressional support, two had the support of president Andrew Johnson. Generals, police commissioners, governors, bankers, mayors, and clergymen all wrote women letters of recommendation for places in Washington, D.C." (Ziparo, 2017).

[25] Similarly, Johnson and Libecap (1994) write: "if anything, patronage was seen as promoting the ideals of equality and social mobility because it allowed the common person to fill public offices."

[26] Although the Registers include information on members of the military, we focus our analysis on *civil* servants. "Postmasters" are a large group of civil servants whose data we have not digitized. We chose not to digitize their data as the Registers include limited identifying information about these workers. For instance, they do not include their birthplaces, and in most cases they only include first name initials rather than a complete first name.

[27] Although our data enable us to observe the same employee over multiple years, we only include employees the first time they show up in the data (that is, we focus on the flow of *new hires*).

3.2 Linking the Personnel Records to the Census

We collected information on employees' backgrounds by linking each of the 1871–1893 Registers to the 1850, 1860, 1870, and 1880 population censuses. In the absence of numerical individual-level identifiers, we linked individuals using names, birthplaces (state for the US born and country of origin for foreigners), and approximate ages (assuming workers would have been between the ages of 18 and 60 at the time of their employment).[28]

Because the linking is based on potentially noisy information (for instance, due to transcription errors in the census), there is a trade-off between matching a higher proportion of records (the "matching rate") and reducing the proportion of false positives (Abramitzky et al., 2019). With this trade-off in mind, we implemented a linking strategy that uses a relatively stringent criteria to deem two observations as a match (and hence privileges avoiding false positives at the expense of the matching rate). In Online Appendix Section A, we validate this strategy by comparing the geographic locations of employees according to the personnel records to their locations in the *contemporary* census to which we match them (of course, we do not use such location for matching).[29]

Through this procedure, we obtained information about: (1) employees' socioeconomic backgrounds, including race, parental wealth, parental literacy, and parental occupations; and (2) employees' own occupations prior to joining the federal government. We obtained information on all parental characteristics (including wealth, which is only observed in the 1860 and 1870 censuses) for 12% of the sample, and on own occupation for 15% of the sample.[30] Note that these proportions are expected to be lower than in census-to-census matching as the registers contain relatively less identifying information than the census (for instance, they do not include exact ages or race).

Representativeness of Linked Data. Our analysis investigates how bureaucrats' backgrounds changed with the introduction of exams. To do so, we compare the characteristics of bureaucrats in positions subject to exams to the characteristics of those in exempted positions, before and after the reform. Since our sample *only* includes bureaucrats who were successfully linked to the census, for our analysis to be biased by selection into linking it would need to be that such selection changed *differentially* for exam and non-exam appointees after the reform. This is unlikely because our linking procedure is the same

[28] We chose these census years since 1850 is the first US population census to list persons individually, and there are no surviving records for the 1890 census.

[29] For instance, if our matches were correct we should find that most individuals listed as working in DC in 1881 are matched to individuals who lived close to DC in the 1880 census (which is what we find).

[30] Note that these two proportions are not expected to be the same: For us to observe parental characteristics, we need to observe an individual before the age of 18 and when living with their parents, whereas for us to observe a worker's own pre-civil service occupation, we instead need to observe them after they turned 18 but before they joined the civil service.

throughout all sample years and positions.

To further alleviate concerns about our results being driven by selection into the linked sample, we note here that: (1) employees hired through exams are not more (or less) likely to be linked (Table A1), (2) the results that *do not* require the linked data are similar when estimated in the subset of the data we successfully link (Table A2), (3) the results are similar when we reweight the sample to account for differences in the matching probability across individuals (Table A3 and Figure A1), and (4) the results are similar when use alternative cutoffs to determine whether we deem an observation as a match (Figure A1). We provide further details on the matching procedure in Online Appendix Section A.

3.3 Measures of Employees' Backgrounds

Parental Wealth. The 1860 and 1870 censuses include information on the dollar value of households' real estate and personal property.[31] We use the combined value of real estate and personal property to rank households in the *national* wealth distribution (although our results are similar if we use state-specific ranks), separately by census year and age of the household head.[32] For those employees for whom we observe parental wealth both in 1860 and 1870, we use the average rank across both years.

Note that, to observe parental wealth (or, more generally, any parental characteristic), our data require that we observe employees coresiding with their parents in the census. Hence, to minimize biases due to selective coresidence at later ages, whenever we focus on parental characteristics we restrict the sample to individual whom we observe with their parents in the census at the age of 17 or less.[33]

Father's Occupation and Parental Literacy. The 1850-1880 censuses include information on occupations, which we use to identify father's occupation for the individuals in our data. We split father's occupations into five categories: professional, non-professional white-collar, farmer, skilled blue collar, and unskilled.[34] This classification corresponds

[31] The 1850 census asked about real estate property but not about personal property. Enumerators in 1860 and 1870 were instructed to collect property information "inclusive of all bonds, stocks, mortgages, notes, livestock, plate, jewels, or furniture." The 1880 census did not include either of these questions.

[32] A complication with computing such rank is that the 1860 census did not list the Black enslaved population but the 1870 census did. Because the formerly enslaved population owned little wealth, white household heads observed in 1870 would mechanically tend to have higher ranks than those observed in 1860. To address this issue, our ranks are based on the *white* population. In addition, we base the rank on households with at least one child–as this is the relevant group for our intergenerational analysis. By 1860, 87% of white household heads with at least one child reported positive wealth. A related issue is that slave-owning families saw a decline in their wealth after emancipation. Hence, families observed in 1870 would tend to be poorer than those observed in 1860. However, our results are similar if we exclude Southern individuals from the sample (where most of these families resided).

[33] Among employees whom we observe at the age of 17 or less, 80% have a father present in the census.

[34] Professional occupations are those with a value below 100 in the 1950 Census Bureau occupational classification system (such as accountants and lawyers). Non-professional white-collar occupations are

to the scheme using five occupational categories in Long and Ferrie (2013). We focus on father's occupations as few mothers worked outside of their households in this period. For those employees for whom we observe their father's occupation more than once, we calculate the fraction of census years that their father spent in a given occupational category.[35] Finally, we also construct indicators of whether employees' parents were literate.

Nativity Status and Race. We observe workers' birthplace and race, as well as the corresponding information for their parents. We use this information to build indicators of whether workers are foreign born, whether both their parents are foreign born, and whether they are white. While we are able to observe *own* race and birthplace for all employees that we match to at least one census, we only observe *parental* birthplace for those that we match to a census in which they are observed coresiding with their parents.[36] Finally, note that, as the Registers include information on workers' birthplaces, we can also use this information directly (without linking to the census) when we investigate if the reform changed the share of foreign-born employees.

Summary Measures of Employees' Social Background. We compute two summary measures of employees' socioeconomic backgrounds. These measures are constructed such that a lower value corresponds to individuals from lower-SES backgrounds. First, we follow Kling et al. (2007) and compute a "summary index" equal to the unweighted average of the following standardized variables: parental wealth rank, an indicator of whether a worker's father was literate, an indicator of whether a worker's father was a professional, an indicator of whether a worker's father *was not* an unskilled worker, an indicator of whether a worker's parents were US born, and an indicator of whether the worker was white.[37] Second, we use factor analysis to compute the first principal component of the same set of variables, which we then normalize to have a mean of zero and a standard deviation of one.[38]

Focusing on these summary measures offers two main advantages. First, as we observe several workers' background characteristics, using a summary measure minimizes the risk of over rejecting the hypothesis that the reform did not affect workers' backgrounds. Second, using an index improves statistical power (Kling et al., 2007). Despite

those with a value between 200 and 500 (for example stenographers, and secretaries). Farmers are those with a value of 100. Skilled blue-collar are those with values between 500 and 700 (such as carpenters and shoemakers). Unskilled workers are those with a code above 700 (such as laborers).

[35] For instance, when we focus on whether someone's father was a professional, if we observe the same father twice we assign a value of 0.5 if the father is listed as a professional in one census but not in the other.

[36] The census did not include a question on parental birthplace until 1880.

[37] The variables are standardized by subtracting the control group mean and dividing by the control group standard deviation.

[38] There is a 0.9 correlation between both measures, so for brevity we mostly focus on the Kling et al. (2007) index.

these advantages, we also show results focusing on the specific components of the index.

Employees' Professional Backgrounds. In addition to observing workers' childhood socioeconomic backgrounds, we observe workers' own occupations *prior* to them joining the civil service. Whenever we link an employee to multiple censuses, we focus on their most recent pre-civil service occupation and restrict the sample to individuals who were at least 25 years old at the time we observe them in the census (so as to enable occupations to better reflect workers' professional attainment). We split workers into the same five occupational groups as when focusing on parental occupations.

Sample Sizes for Each Characteristic. Finally, note that the sample size varies depending on the specific characteristic we consider. For instance, while we only observe parental wealth for those employees that we find as children in the 1860 or 1870 censuses (as these were the only two censuses that included this variable), we observe parental occupations and parental birthplace for employees we find as children in any of the 1850-1880 censuses. Similarly, we observe race for any employee that we match to at least one census (regardless of the age at which we find them). Our findings on parental occupations and parental birthplaces are nevertheless similar if we restrict the sample to employees for whom we also observe parental wealth (see Table B3).

3.4 Civil Service Commission Reports

We combine our personnel records with two main pieces of information from the Civil Service Commission reports (Civil Service Commission, 1893):

First, the reports include a list of all exam-appointed employees in the classified *departmental service* in DC (our main focus in this paper). These lists were collected with the goal of keeping track of the apportionment of positions across states, and include employees' names, home state, initial department and compensation, examination taken, and appointment date. Using this list, we can identify which employees were hired through exams, as well as the exam that they took.[39] We find nearly 80% of the workers in this list in the personnel rosters. This proportion is in line with the rate of employee turnover in our data.[40] Figure A7 shows an example page listing employees appointed in 1883.

Second, the reports include a list of the *positions* subject to exam in each of the executive departments. These data enable us to identify the "treated" positions. Figure A8 shows an example page listing the positions subject to exam in the Treasury Department.

[39] Although these lists cover all hires to the classified departmental service, they *do not* cover employees in the classified customs and postal services (as these positions were not apportioned).

[40] Since our records capture the *stock* of employees every two years, workers might never show up in our data if they worked for less than two years in the civil service. Using our data, we estimate a two-year turnover rate of around 40%.

Summary Statistics. Table B1 shows summary statistics for employees in our baseline sample, separately based on whether or not they were appointed through an exam. Exam-appointed employees came from wealthier families, were more likely to have a professional father, less likely to have an unskilled father, less likely to be foreign born or have foreign-born parents, and more likely to be white.

4 Empirical Strategy

4.1 Main Estimating Equation

Our goal is to assess the extent to which selecting employees through exams changed bureaucrats' socioeconomic backgrounds. To do so, we compare the backgrounds of employees hired to perform the *same* job in the *same* unit, some of whom were recruited through patronage and some of whom were recruited through exams. We estimate:

$$y_{ipt} = \alpha_p + \alpha_t + \beta Exam_{pt} + \gamma X_{ipt} + \epsilon_{ipt} \tag{1}$$

where y_{ipt} corresponds to a characteristic of employee i hired in position p in year t, α_p are position fixed effects, and α_t are hiring-year fixed effects. A position is defined based on the combination of an occupation, a wage, a bureau, and a Department–for instance, *clerk, $1200, Pension Office, Interior Department*. Because the registers lack direct information on hiring year, we infer this information by comparing adjacent registers and identifying the first year in which a worker shows up in the data. Because, as described above, the reform established that positions in the Departmental Service in DC had to be apportioned across states, in our preferred specification X_{ipt} includes workers' home-state fixed effects. By including these fixed effects, we shut down the effects of the reform that stem from apportionment-induced changes in bureaucrats' regional origins (although, on practice, the inclusion of such fixed effects has only modest effects on our estimates). Throughout the analysis, we cluster standard errors at the level of the position.

Our main variable of interest is $Exam_{pt}$, which takes a value of one for exam-appointed employees. The most direct approach to identify such employees, which we use throughout the paper, is to use the published list of exam-appointed employees from the Civil Service Commission reports. The key benefit of this approach is that it enables us to precisely identify which employees were appointed through an exam. An alternative approach to identify exam-appointed employees would be to instead use an employee *position* and *hiring year* in combination with the rules of the reform. In Appendix Section B, we discuss why, due to the lack of direct information on hiring year, such approach is not useful in our context as it leads to severe measurement error in the identification of such employees.[41] In this Appendix Section, we also discuss how the lack of direct information on hiring year might create issues even when using our preferred approach for identifying exam-appointed employees (and provide evidence that such issues cannot rationalize our findings).

4.2 Challenges to Identification and Tests of the Identification Strategy

Our identification strategy requires that, in the absence of the reform, the backgrounds of workers in exam-appointed positions would have evolved on a parallel fashion to the backgrounds of workers in exempted positions. Since our control group is comprised of workers both in low- (such as laborers) and high- (such as unit chiefs) pay positions, a concern with our identification strategy is that the characteristics of such workers would have been on a different trend relative to those of workers in the positions subject to exams. This might have been the case, for instance, if the relative appeal of the public sector was differentially changing for workers in different parts of the skill distribution.

To address this concern, we assess whether the socioeconomic backgrounds of newly hired workers in the "treated" positions were on similar trends than those in the "control" positions prior to the reform. To do so, we estimate dynamic specifications of the form:

$$y_{ipt} = \alpha_p + \alpha_t + \sum_{t=1875}^{1893} \beta_t Exam_p \times \alpha_t + \gamma X_{ipt} + \epsilon_{ipt} \qquad (2)$$

where the β_t coefficients describe the evolution in the backgrounds of employees hired in "treated" and "control" positions during our sample period. Note, following our discussion above, that we identify treated and control positions in the pre-reform period using solely information on job titles, whereas we continue to identify exam-appointed individuals in the post-reform period using the list of exam-appointed individuals. The omitted category is workers hired in 1873, the first year in which we can identify new hires.[42]

Table B2 presents, for each of our main variables of interest, F-test statistics corresponding to the hypothesis that all pre-reform coefficients are equal to zero. The estimates correspond to our preferred specification, which includes home-state fixed effects. The table shows that, regardless of the outcome we consider, we do not reject such a null hypothesis. In Section 5, we also present graphic evidence consistent with the common

[41] See fourth discussion paragraph "Identification of Exam-Appointed Employees" in Appendix Section B.

[42] While we have collected personnel records starting in 1871, 1873 is the first year for which we know whether employees are *new hires* (based on comparing the list of employees in 1873 to the list in 1871).

trends assumption. Finally, Section 5 shows the robustness of our results to using alternative control groups.

A second concern is that, to the extent that appointing officers wished to retain hiring discretion, the reform might have incentivized hiring in the exam-exempted segments of the bureaucracy. In this case, our effects could stem from changes in the control group rather than by changes in the characteristics of exam-appointed employees. Indeed, in Moreira and Pérez (2021) we document such a response in the context of the Customs Service: requiring that employees making $900 or more a year were hired through exams led to a near *doubling* in the share of workers making less than this cutoff.

There are three reasons why this concern is less relevant in our context (i.e. the classified departmental service). First, the historical literature suggests that such manipulation was unlikely to occur in the executive departments in DC as these offices were under tighter control from the Civil Service Commission.[43] Indeed, Figure 1 shows that, in the classified departmental service, the share of positions that would have been subject to exam remained relatively flat (at about 60%) over our sample period.[44] Second, when we plot the data separately for the control and treatment groups, there is little indication of a post-reform change in the characteristics of workers in the control group (Figure B1). Indeed, consistent with this stability, our results are similar when we perform a simple before and after comparison of the backgrounds of employees in "treated"' positions (Table B10). Third, our results are similar when we use alternative control groups comprised of units in which no employee was subject to the reform (and where these spillovers were hence unlikely to occur) –for instance, workers in the judicial branch (see Figure B3).

4.3 Analysis Sample

Our baseline sample is restricted to workers in the Executive Departments in DC. We do so because, for these workers, we have exact information on which of them were appointed through an exam.[45] We note, however, that our results are similar if we include workers outside of DC or outside of the Executive Departments (for instance, in the Judicial branch) in our control group, or workers in the classified Customs Service to our treatment group (see Figures B3 and B4). In addition, we also restrict our baseline sample

[43] For instance, Civil Service Commission (1893) writes that "Turning to the custom-houses, the Commission is able to present much less satisfactory tables. The classification of the Customs Service has always been very imperfect. It has been classified by salary rather than by employment, and has been possible to take the employees out of the classified grades by lowering their salaries or by changing their designations."

[44] As described above, the 1891 decrease in the share of covered positions is driven by the addition of 2,500 workers hired temporarily to tabulate the 1890 census.

[45] This restriction excludes workers in the Executive departments outside of DC (such as those in the Customs Service), workers in the Judicial and Legislative branches, and workers in miscellaneous government agencies not affected by the reform.

to *male* employees. We do so for two reasons. First, as most women changed their last name upon marriage, it is challenging to track women across sources using their names.[46] Second, nearly 85% of exam-appointed employees were male, so restricting the sample to males further improves the comparability of the treatment and control groups. Our main results are nevertheless similar when we add females to the sample (see Figures B3 and B4). Table A4 shows the construction of our main sample.

5 Main Results: Exams and Socioeconomic Background

In this section, we ask if the reform facilitated or impeded the access of individuals from lower-SES backgrounds to government jobs. We focus on parental wealth, parental occupations, parental literacy, worker's countries of origin, and race.

Summary Index of Social Background. We first investigate the effects of the reform on the Kling et al. (2007) summary index of employees' socioeconomic background. This index aggregates information on parental wealth, parental occupations, parental literacy, nativity status, and race, and is built such that a lower value corresponds to lower SES.

Figure B1 shows the average of this index for newly hired workers, separately based on whether workers were employed in positions subject or not subject to exams. The figure shows that, throughout the period, workers in positions subject to exam had higher values of the index, but that this gap increases after the reform.

Table 1 estimates the specification in equation 1 and confirms that the reform was associated with an increase in workers' summary SES index. Specifically, Column 1 shows a 0.18 standard deviation increase in the value of such index. Column 3 also shows an increase (of 0.29 standard deviations) if we instead use the first principal component of the same set of characteristics included in the Kling et al. (2007) index. The estimates for both summary measures are similar regardless of whether or not we include fixed effects for workers' home state (odd versus even columns), suggesting that the effects are not driven by apportionment-induced changes in workers' regional origins.

Panel (a) of Figure 2 shows the estimates corresponding to equation 2, again focusing on the Kling et al. (2007) index. The pre-reform coefficients are sometimes positive and sometimes negative, and we do not reject the hypothesis that they are all jointly equal to zero (p-value: 0.33, Table B2). In contrast, all of the post-reform coefficients are positive and they are jointly statistically significant (p-value<0.01, Table B2).

The figure suggests a rapid increase in the index after the reform, with the estimates then declining in size to a value around 0.12 standard deviations. One likely explanation

[46] For instance, 40% of women aged 18 to 50 with an occupation in the 1880 census were either married or widowed. Indeed, our matching rates are lower for females (Figure A10).

for the initial jump and subsequent leveling of the effects is that lower-SES applicants might have required more time to "catch-up" with the contents of the exam. We note, however, that our main estimates are not driven by this initial jump–in fact, they are not driven by any particular post-reform year, see Figure B6 which shows the robustness of our estimates to excluding one post-reform year at a time.

Parental Wealth. We next investigate the consequences of the reform for the different components of the index, starting from parental wealth. Columns 1 and 2 in Panel (a) of Table 2 show that the reform led to an increase in employees' family wealth ranks. Specifically, exam-appointed employees came from families that were 6.2 percentile ranks higher in the wealth distribution, slightly above a 10% increase.

In columns 3 to 6, we compute separate ranks for personal property and real estate wealth –rather than a single rank based on their combined value. Differences in real estate wealth may simply reflect regional differences in home-ownership rates (rather than true differences in parental resources). The table shows increases for both measures and particularly so for personal wealth: Exam-appointed employees came from families 7 percentile ranks higher in the distribution of personal property wealth and 4 percentile ranks higher in the distribution of real estate wealth–although the latter increase is not significant once we add home-state fixed effects.

Panel (b) of Figure 2 shows dynamic estimates of the effects of the reform on parental wealth ranks (based on equation 2). The pre-reform coefficients are relatively small and we do not reject the hypothesis that they are jointly equal to zero (p-value: 0.39, see Table B2). In contrast, the post-reform coefficients are all positive and are jointly statistically significant (p-value<0.01, see Table B2). The estimates suggest a rapid increase in parental wealth following the reform. However, unlike when we focus on the summary index, the year-by-year estimates are less stable: they are the largest in 1885 and the smallest in 1887 and 1893.

Who Gained and Who Lost Representation? The reform increased employees' average parental wealth ranks. Such increases could be compatible with increases in the representation of the middle of the distribution at the expense of the bottom, or with increases in the representation of the top at the expense of the middle (or by some combination of the two).

To assess which groups increased and which groups decreased their representation, we split workers based on the wealth quintile of their parents. Panels (a) and (b) in Figure 3 show, for newly hired employees in the "treated" positions, their distribution across family wealth quintiles in the pre- and post-reform periods. Panels (c) and (d) show the same distribution but for employees in positions that did not become subject to exams.

Among workers in the treated positions, those who grew up in families in the top quintile were overrepresented prior to the reform (they accounted for about 35% of workers). However, there were small pre-reform differences in the relative representation of individuals from the bottom four quintiles: each of these groups accounted for about 15% of workers. After the reform, in contrast, we observe a sharp increase (from 15 to 25%) in the share of workers from the 60–80 quintile, which comes mostly at the expense of the bottom 20%.

Among those in positions *exempted* from exams, both the top and the bottom family wealth quintiles were overrepresented in the pre-reform period (Panel (c)). This bimodal distribution likely reflects the fact that exempted positions included both leadership (such as bureau chiefs) and low-pay positions (such as laborers). Yet, the overrepresentation of both the bottom and the top of the distribution remained similar after the reform (Panel (d)).

Panel (e) in Figure 3 confirms this pattern when we estimate equation 1 using as dependent variables indicators for belonging to each quintile of the parental wealth distribution. First, we find no change in the likelihood that an employee would belong to the top 20%. Second, we find an increased representation of employees from families in the 60-80 quintile, which comes at the expense of families in the bottom two quintiles (particularly the bottom 20%).

Father's Occupations and Parental Literacy. Panel (b) of Table 2 shows that the reform decreased the share of employees whose father had a lower-status occupation. First, exam-appointed employees were 2.4 percentage points less likely to have a father with an unskilled occupation (nearly a 30% decline). Combining all blue-collar occupations into a single group, we observe a 6 percentage points decline in the likelihood of having a father in this category (see Table B4). Second, exam-based hires were 5 percentage points more likely (relative to a baseline of 11%) to have a professional father. Moreover, there is also an increase in the share of children of farmers, although this effect is smaller (and loses statistical significance) once we include workers' home-state fixed effects.

Finally, Table B5 shows that exam-appointed employees were 2.6 percentage points more likely to have a literate father (relative to a sample mean of nearly 93%), although there is no such a gap when we focus on employees' mothers.

Country of Origin and Race. Table 3 shows that the reform reduced the share of immigrants (and their children) in government jobs. Columns 1 and 2 show that exam-appointed employees were 4 percentage points less likely to be foreign, nearly a 40% reduction. The decline in the share of immigrants, however, does not seem to be simply driven by a lack of English proficiency: Table B6 shows a decline in the share of immigrants from *English-speaking* countries.

An advantage of focusing on country of origin is that it does not require linking observations to the census (as birthplace was directly reported in the Registers). Table A2 shows a similar decline in the proportion of immigrants regardless of whether we use the full or the linked sample; if anything, the decline is *larger* when estimated in the full (non-linked) sample. Moreover, the difference between the estimates becomes even smaller (Table A2) as we reweight the linked sample to account for differences in the likelihood of matching an observation to the census.

In columns 3 and 4 of Table 3, we instead focus on the likelihood that an employee was the *child* of an immigrant. Unlike migrants themselves, their children were likely exposed to education in the US, perhaps limiting the disadvantages observed in the first generation. However, we find that exams also reduced their representation (a 7 percentage points decline, relative to a control group mean of about 20%).

Finally, in columns 5 and 6 we investigate if the reform changed bureaucrats' racial mix. The dependent variable in these columns is an indicator that is one for employees who reported being white in the census. Although the reports of the Civil Service Commission argue that the reform increased the representation of African Americans, we find no evidence that this was the case: the point estimates are very close to zero and enable us to rule out small changes in employees' racial mix.[47] Hence, even though the reform reduced the representation lower-SES individuals, it did so without changing workers' racial mix.

Heterogeneity by Type of Exam and Position. Although most employees were hired in relatively non-technical mid-tier positions such as copyists or clerks (whose exams only required a "common school" education), some workers were hired for higher-status positions (for example, positions whose exams required more specialized knowledge, or positions paying particularly high salaries). Indeed, one possibility is that the reduction in the representation of lower-SES individuals we document was specific to these higher-status positions. To assess this possibility, we investigate if the effects of the reform varied depending on the position to which a worker was appointed. To do so, we first estimate:

$$y_{ipt} = \alpha_p + \alpha_t + \beta_1 Clerical\,Exam_{pt} + \beta_2 Technical\,Exam_{pt} + \gamma X_{ipt} + \epsilon_{ipt} \tag{3}$$

where $Clerical\,Exam_{pt}$ is one if employee i is listed as having taken either the clerk or the copyist exam, and $Technical\,Exam_{pt}$ is one if the employee is listed as having taken

[47] "It is noticeable that a much larger proportion of colored people receive appointments under the civil-service law than under the old patronage system." (Civil Service Commission, 1893)

one of the more technical exams (for instance, the exam for meteorological clerks in the Department of Agriculture).

Panel (a) of Table B7 shows limited heterogeneity with respect to this dimension: exam-appointed employees, both in more and less technical positions, came from higher-SES families than patronage hires. This finding suggests that our results are not driven by those positions requiring more specialized knowledge.

In Panel (b) of Table B7, we instead split workers between those appointed into below- and above-median paying jobs. The table shows an increase in the socioeconomic status of workers appointed to both groups of jobs, although the point estimates suggest a larger increase among those appointed into the higher-paying ones.

Alternative Explanations. We next consider a number of alternative explanations, other than *the use of exams per se*, for the observed change in workers' backgrounds. First, since exams were held across the country, the reform might have changed workers' backgrounds simply by facilitating the recruitment of individuals from a broader set of locations. Similarly, since exam dates and locations were widely and publicly advertised, applicants who lived far from DC might have been more likely to learn about government jobs than before the reform.

An implication of both of these explanations is that our results should be driven by changes in employees' geographic origins. However, Table B9 shows that our results are similar as we include: (1) birthplace, (2) childhood state, or (3) childhood state by urban/rural fixed effects. Hence, the reform generated changes in the social backgrounds of workers *within* a location rather than simply changes in workers' geographical origins.

The next potential explanation is that lower-SES applicants might have difficulties adapting to *any new* recruitment system, irrespective of whether such system involves an exam or not. If this "disruption" channel explained our results, then the effect of the reform should have been short-lived. However, Figure 2 shows that, ten years after the reform, exam-appointed workers were still of higher social status.

The final alternative is that the effects that we capture are not driven by the reform but rather by the transition to a Democratic administration in 1884 (one year after the reform and after more than a decade of Republican presidencies). To test this hypothesis, we exploit the fact that the presidency went back to a Republican in 1888 and then back again to a Democrat in 1892. In contrast to this hypothesis, Table B8 shows that the increase in workers' socioeconomic status occurred *both* under Democratic and Republican presidencies (although the point estimates are larger while Democrats were in power). Moreover, the effects are not driven by any particular presidential transition: they are of similar magnitude when we exclude one post-reform year at a time (Figure B6).

Summary of Results. The reform increased bureaucrats' socioeconomic status: they had higher parental wealth, were more likely to have a literate father, more likely to have a professional father, and less likely to be the children of immigrants (or immigrants themselves). This increase occurred immediately after the reform and persisted for at least 10 years. Moreover, the increase is observed even when restricting the comparison to workers with similar geographic origins, and both during Democrat and Republican presidencies. These additional findings suggest that the changes we document were driven by the use of exams *per se* rather than by other features of the reform.

Summary of Robustness Checks . In Online Appendix B we discuss several robustness checks to our main results. Specifically, we show that our results are robust to: (1) including additional control variables to account for potential time-varying shocks differentially affecting treated and non-treated positions, (2) alternative definitions of the control and treatment groups (to limit concerns about spillovers across treated and non-treated positions), (3) more stringent definitions of which workers are considered new hires (to deal with concerns about measurement error in our identification of post-reform hires), (4) implementing a randomization inference approach, and (5) features of the linking strategy.

6 Why did Exams Decrease the Representation of Lower-SES Individuals?

Our interpretation of the results is that, by increasing the importance of education in the hiring process, the reform improved the chances of "educated outsiders" (individuals with high education but limited connections). Because middle class applicants were overrepresented in this group, the reform increased their representation. We first provide a conceptual framework that illustrates this interpretation. We then show evidence consistent with it and discuss additional channels through which exams might have affected workers' characteristics.

6.1 Conceptual Framework

Assume that obtaining a job depends on applicants' education ("e") and connections ("c"). We consider education broadly, including applicants' stock of knowledge as well as their ability to study for the exam. Connections could also be of various types, including personal (for instance, being related to a member of Congress) and political (for instance, having worked for the incumbent party) connections. Further, assume that e and c are potentially correlated with applicants' social class (s).

Applicants are hired if they are in the top $l\%$ of candidates in terms of their combined value of e and c; that is, if:

$$\alpha e + (1-\alpha)c > F^{-1}(1-l) \tag{4}$$

where F^{-1} is the inverse cdf function of $\alpha e + (1-\alpha)c$.

We interpret the reform as an increase in the value of α (the relative weight of education). Hence, a direct effect of the reform is to favor "educated outsiders": individuals with high values of "education" (e) but low values of "connections" (c).[48]

Whether the shift towards "merit" helps the poor or the rich depends on the link between e, c, and s. Figure B7 shows three possible cases. In Panel (a), social class has a stronger correlation with education than it has with connections. In this case, introducing exams disproportionately helps the chances of higher-SES children. In Panel (b), in contrast, social class is correlated with connections but has no relationship with education. Hence, introducing exams in this case benefits lower-SES children. Finally, in Panel (c) the "middle class" increases its representation after the reform: it has similar levels of connections than the "poor" but higher levels of education.

A simplification of this framework is that it abstracts from dynamic considerations. However, applicants might differ in their ability to adapt to exams, making the effects of the reform potentially different in the short and long run. For instance, higher-SES applicants might have more financial ability to resort to test-preparation tools such as hiring private tutors. If such test-preparation tools take time to emerge, higher-SES applicants' relative advantage would tend to increase over time. Alternatively, lower-SES applicants might need more time to "catch up" with the exams' content, thus being at a relative disadvantage early on. Our findings are more consistent with this latter possibility: the increase in employees' social status was the strongest immediately after the reform and levelled off subsequently.

6.2 Empirical Evidence

The Reform Increased the Representation of "Educated Outsiders". Our conceptual framework predicts that the reform should have increased the representation of "educated" individuals. Although censuses prior to 1940 do not include direct information on years of schooling, they do include information on occupations. Hence, we can assess if the reform brought workers whose pre-civil service occupation likely required higher educational attainment.[49]

[48] This framework abstracts from applicants' outside options. We do so to keep the framework parsimonious as the reform did not directly change workers' outside options.

[49] Literacy (which is included in the census) is a very coarse educational measure in this context as more than 90% of the adult white population was literate by 1880.

Table 4 shows that exam-appointed employees were 8 percentage points more likely to have held a *professional* occupation prior to joining the civil service (a 90% increase). These occupations are precisely the ones that would have required formal education, suggesting that the reform was successful in recruiting more educated workers. Interestingly, however, we also find an increase in the proportion of workers who were previously employed as farmers. This increase likely reflects the fact that the reform changed employees' social mix, increasing the proportion of those hailing from rural areas (see Table B12 and the discussion below).[50] These increases were mostly driven by a decrease in the likelihood that employees would have held a white-collar non-professional job prior to joining the civil service.[51]

Figure 4 shows the corresponding dynamic estimates. The figure shows a rapid increase in the share of workers who had a professional occupation prior to joining the civil service, together with a decrease in the share of those with white-collar non-professional jobs. The increase in the share of workers with a professional background persists 10 years after the reform, suggesting that the reform kept attracting workers with stronger educational backgrounds in the longer term.

Another implication of our interpretation is that exams should have disproportionately benefited those individuals who grew up in areas with better educational resources. To test this hypothesis, we combine information on employees' childhood counties (from our linked sample) with county-level data on per capita schools and teachers in 1850 (from Haines et al. (2010)).[52]

Panels (a) and (b) in Table 5 show that exam-appointed employees came from counties with higher per capita schools and teachers. The results are similar when we exploit variation within states (Column 3 in each of the panels) and within urban/rural areas (Column 4), suggesting that our results do not simply capture differences across broad regions of the country. Moreover, the results are also similar when we control for parental occupation (Column 5), parental birthplace (Column 6), and parental wealth (Column 7). This similarity suggests that the association we document is not mechanically explained by counties' educational resources being correlated with parental resources.

We next investigate if the reform brought "outsiders", that is, individuals who lacked connections and hence were unlikely to obtain a patronage job prior to the reform. A

[50]Note that farmers were a relatively educated group in this period: Among white males aged 18 or more in 1880, those employed as farmers had a 91% literacy rate (compared to 93% among non-farmers).

[51]White-collar non-professionals were on average less educated than professionals: In 1870, the average "occupational education score" among white-collar non-professionals was 23.4, whereas it was 82.2 among professionals. This score measures the share of individuals in an occupation with a college degree in 1950.

[52]We use 1850 because this is the last pre-reform census for which Haines et al. (2010) report these data.

challenge in testing this hypothesis is that informal connections are–by their own nature–difficult to observe. Hence, we proxy for them using four pre-determined measures:[53] First, we use an indicator that is one if the bureaucrat's father had worked for the federal government. Second, we construct an indicator that is one for bureaucrats who had the *same surname* as a member of Congress from their home state (using data from the *Biographical Directory of the US Congress* (Dodge and Koed, 2005)). Third, we use an indicator that is one for employees who spent their childhood in DC, the city that likely provided the best opportunities to develop informal political connections.[54] Finally, we construct a measure aimed at capturing workers' likely party affiliation. To do so, we combine the data on workers' county of residence prior to joining the civil service (from our linked sample) with county-level data on party vote shares (from ICPSR (1999)). We use this information to construct an indicator that is one if a majority of voters in the bureaucrats' county had voted for the incumbent party in the most recent presidential election.

Table 6 shows that exam-appointed employees were less likely to have a father who himself worked for the federal government (although the effect is not statistically significant), less likely to have spent their childhoods in DC, and less likely to hail from a county which voted for the incumbent party in the previous presidential election.

Although, in principle, all employees might have benefited from being "connected", it is likely that connections would have been more relevant for accessing jobs that required less technical skills. This would have been the case, for instance, if being hired required at least a minimum level of competency–thus making it harder to privilege connections when hiring for more technical positions (Brierley, 2019).

With this in mind, we assess if the decline in the importance of "connections" depended on the type of position to which the worker was appointed to. To do so, we estimate the specification in equation 3, which distinguishes between employees appointed to the less technical clerical positions and those appointed to positions requiring more specialized knowledge.

Table 6 shows that the decline in the likelihood of being connected comes almost exclusively from those individuals who were appointed to the relatively non-technical positions: Employees in such positions were 3 percentage points less likely to have a father who worked in the Federal Government (nearly a 50% decline), 8 percentage points less likely to have spent time in DC (a 25% decline), and 5 percentage points less likely to hail

[53]There are other, more informal connections which which our data do not enable us to capture. For instance, the historical literature highlights the importance of being connected to members of Congress (see, for instance, Ziparo (2017).

[54]Ziparo (2017) writes that: "Living in the epicenter of national political life, applicants from Washington, D.C., had an advantage in obtaining political influence."

from a county that voted for the incumbent party (a 10% decline). By contrast, there are much more limited effects among those appointed into the more technical jobs.

The Middle Class was Overrepresented Among the "Educated Outsiders". Exam-appointed employees were more likely to belong to the upper-middle class. Our interpretation of this finding is that the reform increased the share of such workers because they were overrepresented among the "educated outsiders". We offer two pieces of evidence that support this interpretation:

First, Panel (a) in Figure 3 shows that, prior to the reform, workers whose families belonged to the 60–80 quintile of the wealth distribution were similarly represented in the positions that eventually became subject to exams as those whose families belonged to the bottom three quintiles. Moreover, Figure B8 shows that this similar representation occurred despite the higher educational attainment of workers from the 60-80 quintile. This figure shows school attendance rates by age and parental wealth quintile, based on census data covering the entire 1870 US population.

Second, Figure B9 shows that the low representation of the 60–80 quintile in government jobs prior to the reform was unusual relative to its representation in comparable *private sector* jobs. This figure shows the distribution of private sector white-collar workers across parental wealth quintiles, based on a sample linking adults in the 1880 census to their childhood households in 1860.[55] The figure shows that, unlike in the case of civil servants prior to the reform, the likelihood of holding a white-collar job in the private sector grew monotonically with parental wealth ranks.

What Explains the Presence of Workers from Disadvantaged Backgrounds in Government Jobs Prior to the Reform? A surprising implication of our findings and conceptual framework is that, prior to the reform, lower-SES applicants must have been better "connected" than middle-class applicants. This implication is derived from the fact that these applicants had worse education than middle-class applicants (Figure B8) but nevertheless managed to obtain a similar share of government jobs (Figure 3).

A likely explanation for this pattern is that lower-SES workers might be more likely to engage in patronage politics than those from middle class backgrounds.[56] The historical literature suggests that this was indeed the case in our setting. Specifically, our period featured the emergence of the "urban political patronage machine" (Brown and Halaby, 1987). These machines emerged from the interaction between two major developments of 19th-century US: the rise of "mass-based political parties" (whose campaigns required

[55]This sample was built using the same algorithm that we use to link the personnel records to the census.

[56]For instance, Sorauf (1960) argues that political machines "flourished especially in those urban centers inhabited by large groups of immigrants and minorities-groups not yet integrated into American life."

large mobilizations of workers and resources), and the expansion of the urban working-class. These developments created opportunities for mutually beneficial exchanges: as the "urban immigrant and lower classes needed help", the machine provided "assistance and jobs in return for loyalty, labor, and votes" (Mashaw, 2010).

Our empirical findings are consistent with this explanation. First, we observe declines in the share of workers from counties in which the incumbent party had received a majority vote, suggesting that the reform indeed reduced political favoritism in job allocation (Table 6). Second, we observe declines in the share of immigrants (Table 3), a group often described as the primary target of urban political machines (Cornwell Jr, 1964). Finally, we find a decline in the share of workers from urban areas (Table B12); namely the locations where machines were the most active (Brown and Halaby, 1987).

The Reform Hurt the Chances of the Poor when Educational Inequality was High. One implication of our conceptual framework is that, the higher the educational inequality, the more negative the impact of a shift towards "merit" on the chances of lower-SES children. To test this implication, we exploit state-level differences in the link between access to schooling and parental wealth. Note that, due to the state apportionment rules, applicants to jobs in the classified departmental service were in practice only competing against others from their own home state (thus making within-state inequality relevant for such competition). Specifically, we use the 1870 census to compute, for each state s:

$$Inequality_s = \frac{\text{\% Children in school if family in top 20\% in state } s}{\text{\% Children in school if family in bottom 20\% in state } s} \tag{5}$$

This measure captures the ratio between: (1) the likelihood that a child ages 8-12 from a family in the top 20% of the wealth distribution would attend school, and (2) the same likelihood but for a child from the bottom 20%. This measure would be one in a state in which school attendance did not depend on parental wealth but above one when attendance was higher among children from wealthier backgrounds. Figure B2 shows substantial heterogeneity in this measure, with the highest values in the South and the lowest in parts of the Northeast.

Consistent with our interpretation, Table 7 shows that the increase in the summary index of socioeconomic background is about *twice as large* in the states with above-median inequality than in those below the median. Note, however, that the index increases in *both* groups of states.

Additional Exam-Related Channels: Exam-Induced Change in the Applicant Pool. So far, our discussion of mechanisms has focused on how, by changing the weight of education in hiring decisions, the reform might have affected *who* gets hired out of a given applicant pool. However, the reform may have also affected bureaucrats' characteristics indirectly, by changing the pool of those interested in such career. For instance, the reform might have increased the prestige of holding government jobs, thus increasing their appeal for higher-SES individuals. Although these indirect effects are inherent to any reform as the one we study (and the combined direct and indirect effect is what ultimately matters for representation), our evidence suggest that the effects we document are not solely driven by these indirect channels. First, if the effects were only driven by such changes, it is unclear why the effects would be stronger among applicants from states with high educational inequality. Second, it is also unclear why, *conditional on parental characteristics*, individuals from counties with better educational resources would have increased their representation after the reform. Third, the rapid change in bureaucrats' backgrounds that we observe seems inconsistent with the effects being driven by plausibly slower-to-change perceptions about the prestige of public employment.[57]

7 Conclusions

Using newly assembled data on the socioeconomic backgrounds of government employees, we find that the introduction of civil service exams for the selection of certain federal workers led to a persistent reduction in the representation of lower-SES individuals in the public sector: workers hired through exams came from wealthier families, were more likely to be the children of professionals, and were less likely to be the children of immigrants (or immigrants themselves). This reduction in the share of lower-SES individuals was stronger among employees from states with more unequal access to schooling. Interestingly, we find that the reduction in the proportion of lower-SES individuals occurred despite the fact that the reform did not substantially alter workers' racial mix, suggesting that focusing solely on race would have provided an incomplete picture of its equity impacts.

Our findings have implications for the broader debate on exams and meritocracy. Allocating opportunities based on exams is sometimes described as an equity-efficiency panacea, helping select the most qualified candidates while simultaneously increasing the representation of lower-SES individuals. Our results challenge this view: although using exams could, in principle, help select more qualified candidates, we show that these improvements *can* also come with some costs in terms of equity. More generally, our findings show that adopting less discretionary selection criteria might not necessarily help lower-SES individuals.

[57]Similarly, the reform might have reduced the representation of lower-SES individuals because such individuals were discouraged by the exam and hence did not apply to begin with.

While we study how civil service exams shaped bureaucrats' social origins, an important question that remains unanswered is whether lower-SES individuals were *on net* made worse off by the reform. The answer to this question is not obvious for a variety of reasons. For instance, lower-SES individuals might benefit the most from a well-functioning state, even if achieving this efficiency implies that they lose direct access to government jobs. We hope future work can shed light on the overall distributional implications of civil service reforms.

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TABLE 1: EFFECTS OF THE REFORM ON SUMMARY MEASURES OF EMPLOYEES' SOCIOECONOMIC BACKGROUND

	Summary Index		First Principal Component	
	(1)	(2)	(3)	(4)
Exam	0.180***	0.169***	0.290***	0.270***
	(0.0451)	(0.0468)	(0.0811)	(0.0856)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes
Observations	2944	2944	2944	2944
Mean of dep. var.	0.128	0.128	0.0514	0.0514

Notes: $***p < 0.01$, $**\,p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 and 2 is a summary index of employees' socioeconomic background computed using the approach in Kling et al. (2007). The index combines information on parental wealth rank, parental occupations, parental literacy, own and parental birthplace, and race. The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. In columns 3 and 4, the dependent variable is the first principal component of the same set of characteristics as in columns 1 and 2. All columns include hiring year and position fixed effects. The odd columns further include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

Table 2: Effects of the Reform on Parental Economic Backgrounds

(a) Parental Wealth Ranks

	Total		Personal		Real Estate	
	(1)	(2)	(3)	(4)	(5)	(6)
Exam	0.0647**	0.0624**	0.0708***	0.0694***	0.0489*	0.0427
	(0.0258)	(0.0273)	(0.0240)	(0.0259)	(0.0260)	(0.0263)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes
Observations	3034	3034	3034	3034	3034	3034
Mean of dep. var.	0.540	0.540	0.545	0.545	0.556	0.556

(b) Parental Occupations

	Professional		White-Collar Non-Prof		Farmer		Skilled Blue Collar		Unskilled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exam	0.0530**	0.0488**	-0.0119	-0.00378	0.0606***	0.0258	-0.0668**	-0.0455	-0.0375*	-0.0242
	(0.0239)	(0.0241)	(0.0347)	(0.0325)	(0.0234)	(0.0244)	(0.0327)	(0.0311)	(0.0216)	(0.0214)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4993	4993	4993	4993	4993	4993	4993	4993	4993	4993
Mean of dep. var.	0.0977	0.0977	0.187	0.187	0.270	0.270	0.282	0.282	0.129	0.129

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 and 2 of panel (a) is the rank of a bureaucrat father in the US wealth distribution. In columns 3 and 4, this rank is computed based solely on personal wealth, whereas in columns 5 and 6 it is based on real estate wealth. In panel (b), the dependent variable in each of the columns is an indicator that is one if a bureaucrat's father worked in a certain occupational category. Professional occupations are those with a value below 100 in the 1950 Census occupational classification system (such as lawyers and accountants). Non-professional white-collar are those with a value between 200-500 (for example, clerks). Farmers are those with a value of 100. Skilled blue-collar are those with a value between 500-700 (for example, carpenters). Unskilled are those with a value above 700 (for example, farm laborers). All columns include hiring year and position fixed effects. The odd columns further include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE 3: EFFECTS OF THE REFORM ON EMPLOYEES' COUNTRY OF ORIGIN AND RACE

	Immigrant		Immigrant Parents		White	
	(1)	(2)	(3)	(4)	(5)	(6)
Exam	-0.0473**	-0.0419**	-0.0732***	-0.0693***	0.00660	0.00327
	(0.0190)	(0.0176)	(0.0237)	(0.0240)	(0.00891)	(0.00956)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes
Observations	9238	9238	4822	4822	9238	9238
Mean of dep. var.	0.108	0.108	0.169	0.169	0.931	0.931

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 and 2 is an indicator that is one if the worker is foreign born. The dependent variable in columns 3 and 4 is an indicator that is one if both workers' parents are foreign born. The dependent variable in columns 5 and 6 is an indicator that is one if the workers is listed as being white in the census. All columns include hiring year and position fixed effects. The odd columns further include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE 4: EFFECTS OF THE REFORM ON EMPLOYEES' OWN OCCUPATIONAL BACKGROUND

	Professional		White-Collar Non-Prof		Farmer		Skilled Blue Collar		Unskilled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exam	0.0778*	0.0818*	-0.163***	-0.163***	0.0894**	0.0852**	-0.00165	0.00108	0.00826	0.00300
	(0.0428)	(0.0426)	(0.0564)	(0.0588)	(0.0347)	(0.0353)	(0.0270)	(0.0292)	(0.0333)	(0.0338)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3582	3582	3582	3582	3582	3582	3582	3582	3582	3582

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is an indicator that is one if a bureaucrat worked in a certain occupational category (as indicated by the column) prior to joining the civil service. When a bureaucrat is linked to more than one census with information on adult occupations, we use the most recent pre-civil service occupation. The sample is restricted to workers who were at least 25 year old at the time we observe them in the census. See notes to Table 2 for a definition of occupational categories. All columns include hiring year and position fixed effects. The odd columns further include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

Table 5: Educational Inputs During Childhood

(a) Per capita schools

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exam	0.177***	0.154***	0.130***	0.121***	0.147***	0.146***	0.123***
	(0.0375)	(0.0370)	(0.0364)	(0.0372)	(0.0388)	(0.0388)	(0.0438)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Childhood State FE	No	No	Yes	Yes	Yes	Yes	Yes
Urban FE	No	No	No	Yes	Yes	Yes	Yes
Parental Occupations FE	No	No	No	No	Yes	Yes	Yes
Parental Birthplace FE	No	No	No	No	No	Yes	Yes
Parental Wealth Rank	No	No	No	No	No	No	Yes
Observations	5498	5498	5498	5498	4691	4691	2866
Mean of dep. var.	-5.047	-5.047	-5.047	-5.047	-5.047	-5.047	-5.047

(b) Per capita teachers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exam	0.223***	0.189***	0.147***	0.139**	0.191***	0.190***	0.161**
	(0.0550)	(0.0540)	(0.0531)	(0.0539)	(0.0571)	(0.0571)	(0.0683)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Childhood State FE	No	No	Yes	Yes	Yes	Yes	Yes
Urban FE	No	No	No	Yes	Yes	Yes	Yes
Parental Occupations FE	No	No	No	No	Yes	Yes	Yes
Parental Birthplace FE	No	No	No	No	No	Yes	Yes
Parental Wealth Rank	No	No	No	No	No	No	Yes
Observations	5498	5498	5498	5498	4691	4691	2866
Mean of dep. var.	-5.579	-5.579	-5.579	-5.579	-5.579	-5.579	-5.579

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns of panel (a) is the (log) number of per capita schools in bureaucrats' childhood county of residence. The dependent variables in each of the columns of panel (b) is the (log) number of per capita teachers. When bureaucrats are linked to more than one census in which they are below the age of 18, we use the average of log per capita schools (or teachers) as the dependent variable. The data on per capita schools and teachers are from Haines et al. (2010). All columns include hiring year and position fixed effects. Columns 2 to 7 include additional control variable as indicated by the table. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE 6: EFFECTS OF THE REFORM ON THE LIKELIHOOD THAT EMPLOYEES WOULD BE "CONNECTED"

	Father Gov. Emp.		Lived in DC		Same Surname		Incumbent Party		Vote Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exam	-0.00863		-0.0564***		0.00346		-0.0484*		-0.0132	
	(0.0164)		(0.0190)		(0.00444)		(0.0261)		(0.00844)	
Clerical Exam		-0.0326*		-0.0810***		-0.00155		-0.0531*		-0.0218**
		(0.0175)		(0.0196)		(0.00573)		(0.0275)		(0.00946)
Technical Exam		0.0266		-0.0172		0.0119		-0.0397		0.00259
		(0.0303)		(0.0253)		(0.00899)		(0.0400)		(0.0112)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4993	4993	5860	5860	9238	9238	6416	6416	6416	6416
Mean of dep. var.	0.0538	0.0538	0.243	0.243	0.989	0.989	0.515	0.515	0.534	0.534

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$ The outcome in columns 1 and 2 is an indicator that is one if a bureaucrat's father is ever recorded in the census as working in industry 916 ("Federal public administration") based on the 1950 census industry classification. The outcome in columns 3 and 4 is an indicator that is one is a bureaucrat is ever observed living in Washington DC before the age of 18 (and prior to being employed in the federal administration). The outcome in columns 5 and 6 is an indicator that is one if a bureaucrat shared a surname with a current member of Congress from his own state. The outcome in columns 7 and 8 is an indicator that is one if the incumbent party had obtained a majority vote in bureaucrat's last county of residence in the most recent presidential elections. The outcome in columns 9 and 10 is instead the vote share of the incumbent party. $Exam$ is the coefficient corresponding to our baseline specification. $Clerical\ Exam$ is an indicator that is one for employees hired through exams as clerks or copyists. $Technical\ Exam$ is an indicator that is one for employees hired through exams in technical positions. All columns include hiring year, position, and home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE 7: HETEROGENEITY BY CHILDHOOD'S STATE EDUCATIONAL INEQUALITY

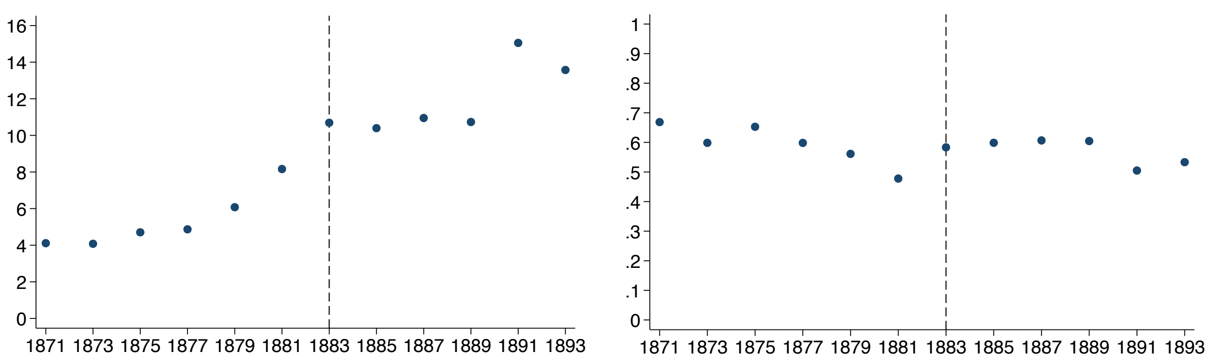
	Below Median Ineq.		Above Median Ineq.	
	(1)	(2)	(3)	(4)
Exam	0.143***	0.134**	0.228***	0.221***
	(0.0528)	(0.0551)	(0.0661)	(0.0701)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes
Observations	2204	2204	740	740
Mean of dep. var.	0.0883	0.0883	0.247	0.247

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of workers' socioeconomic backgrounds computed using the approach in Kling et al. (2007). The index combines information on parental wealth rank, parental occupations, parental literacy, own and parental birthplace, and race. The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. The sample in columns 1 and 2 of each panel is restricted to employees from states with below median inequality in access to schooling, as described in the main body of the paper. The sample in columns 3 and 4 is restricted to employees from states with above median inequality. All columns include hiring year and position fixed effects. The odd columns further include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

FIGURE 1: EXAM-COVERED EMPLOYEES IN THE DEPARTMENTAL SERVICE IN DC

(A) TOTAL NUMBER OF WORKERS (000S)

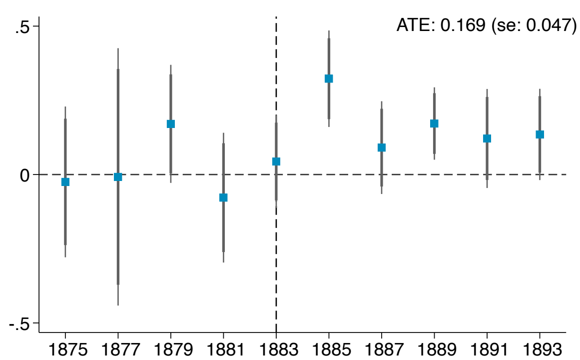
(B) % IN POSITIONS THAT BECAME SUBJECT TO EXAMS AFTER THE REFORM



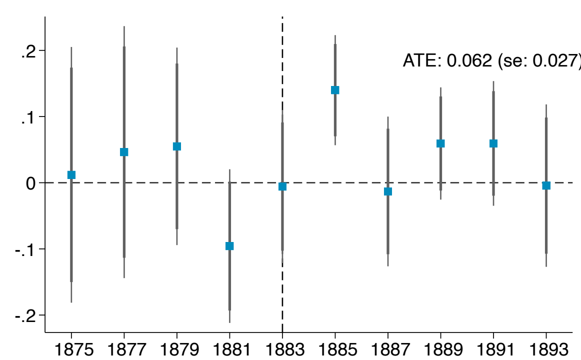
Notes: Panel (a) shows the total number of workers in the Executive Departments in Washington, DC. Panel (b) shows the share of such employees who worked in the positions that became subject to exams in the 1883-1893 period.

FIGURE 2: DYNAMIC EFFECTS OF THE REFORM ON THE SUMMARY INDEX OF EMPLOYEES' SOCIOECONOMIC BACKGROUND

(A) SUMMARY INDEX



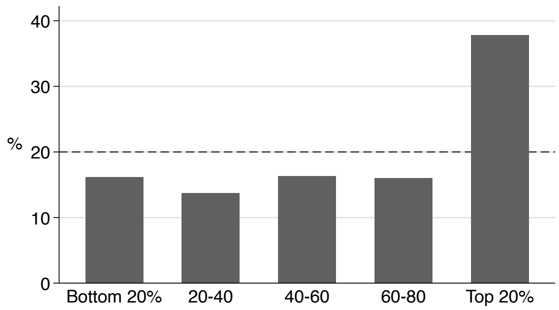
(B) PARENTAL WEALTH RANK



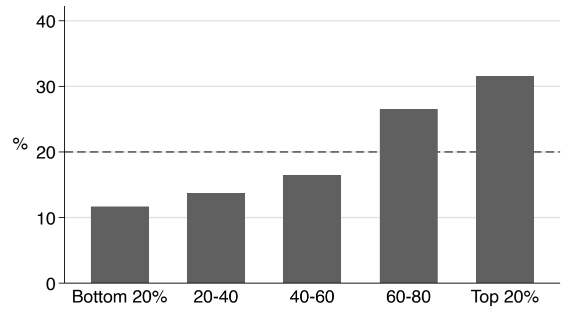
Notes: The dependent variable in panel (a) is a summary index of employees' socioeconomic background computed using the approach in Kling et al. (2007). The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. The figure reports estimates of the specifications described in equation 2. The figure shows the estimated coefficients around 90 and 95% confidence intervals (standard errors clustered at the position level). All specifications include hiring year, position and home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Panel (b) repeats the analysis using the rank of a bureaucrat father in the US national wealth distribution as the outcome.

Figure 3: Parental Wealth Quintiles of Government Employees, Before and After the Reform

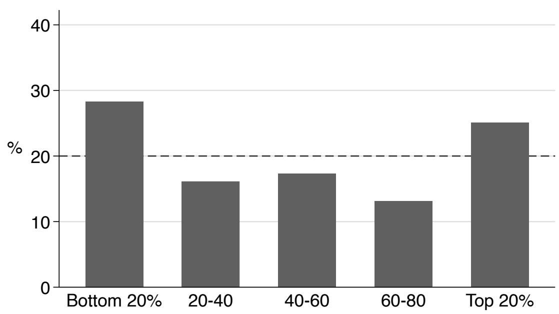
(a) Subject to Exams, Before



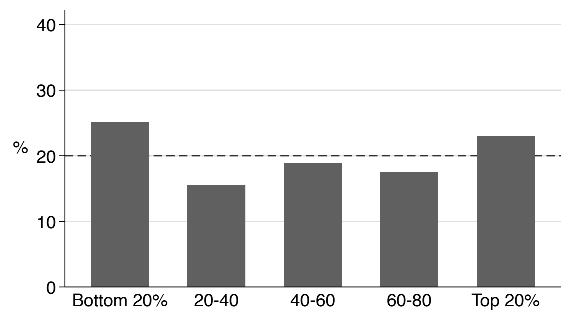
(b) Subject to Exams, After



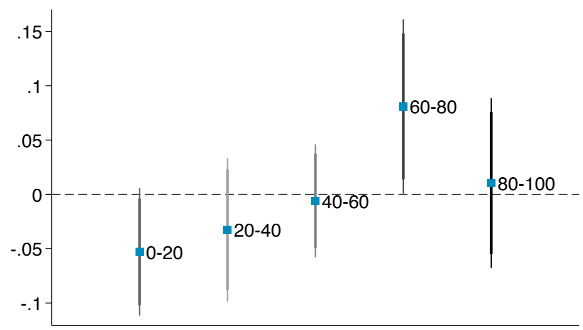
(c) Not Subject to Exams, Before



(d) Not Subject to Exams, After

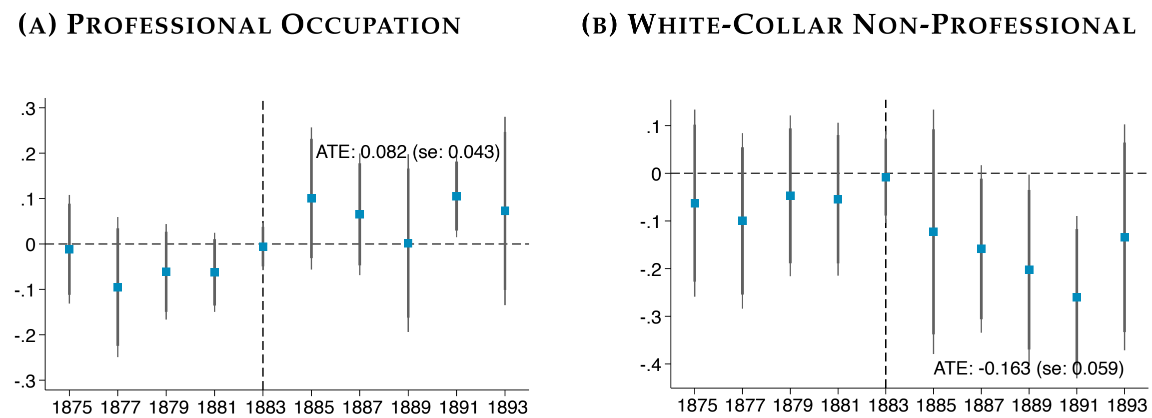


(e) Difference-in-Differences Estimates



Notes: Panels (a) to (d) show the distribution of workers across parental wealth quintiles for workers in positions subject and non-subject to exams, before and after the reform. Workers are classified as being in a position subject to exams if they worked in one of the position that became subject to exams in the 1883-1893 period (that is, the "treated" positions). Panel (e) shows difference-in-differences estimates in which the outcome variables are indicators for belonging to different quintiles of the parental wealth distribution. Each coefficient corresponds to a separate regression. The figure shows the estimated coefficients around 90 and 95% confidence intervals (based on standard errors clustered at the position level). All the regressions include hiring year, position and home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC.

FIGURE 4: DYNAMIC EFFECTS OF THE REFORM ON EMPLOYEES' OCCUPATIONAL BACKGROUND



Notes: The dependent variable in panel (a) is an indicator that is one if a worker was employed in a professional occupation prior to joining the civil service. The dependent variable in panel (b) is an indicator that is one if a worker was employed in a white-collar non-professional job. The sample is restricted to individuals who were at least 25 year old at the time we observe them in the census. The figures show estimates based on the specification in equation 2 in the main text. The estimated coefficients are shown around 90 and 95% confidence intervals (standard errors clustered at the position level). All the regressions include hiring year, position and home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC.

Online Appendix - Not for Publication

A Linking the Official Registers to the Census

Linking algorithm. Our algorithm has the following steps:

1. Clean names in the Registers and the Census to remove any non-alphabetic characters and account for common misspellings and nicknames (e.g. so that Ben and Benjamin would be considered the same name).
2. For each individual in the Registers, search for a potential match in the Census. Potential matches are individuals who:
   (a) Report the same birthplace (states for the US born, country for foreigners). We exclude observations in the Registers with no information on birthplace. Among employees in our baseline target sample, there are 3% with missing birthplace information.
   (b) Have a reported age in the census such that they would have been between 18 and 60 years old at the time we observe them in the Register (for instance, when linking the 1881 register to the 1850 census we look for people age 0 to 30 in 1850).
   (c) Have a first name and a last name within a Jaro-Winkler distance of c_1, where $c_1 \in [0, 1]$. The Jaro-Winkler distance is a string distance measure constructed such that a value of zero corresponds to two identical strings and a value of one corresponds to two strings with no common characters. We allow for non-identical strings to be considered a match to deal with transcription errors in the Census and for OCR errors in our digitization of the Official Registers. Intuitively, the lower the value of c_1 the more conservative our linking strategy (and hence the lower the number of cases we will match someone to an incorrect individual).
   (d) There is no other potential link with a first name and a last name within a Jaro-Winkler distance of c_2, where $c_2 \in (c_1, 1]$. That is, we impose that, if the closest individual is within a Jaro-Winkler distance of c_1, the second closest potential match needs to be at a distance of at least c_2 with $c_2 > c_1$. For a given value of c_1, a higher value of c_2 represents a more conservative choice.

Choosing c_1 and c_2. An advantage of our setting is that, for the Registers collected in 1871 and 1881, we can use the proximity of census years (1870 and 1880) to evaluate the quality of the matches as a function of the choice of c_1 and c_2. Specifically, we can compare the places of employment of individuals as reported in the Registers, to the places of residence of the individuals we match them to in the Census (of course, we do not use the place of residence as a criteria for matching). Intuitively, if our links are correct it should be the case that employees' place of employment in the Registers should coincide with their place of residence in the Census.

To perform this analysis, we focus on individuals who were employed in the Executive Departments in DC (our baseline target sample) in 1881 . We consider a match as having a "correct" place of residence if the person lived in the Baltimore-DC metropolitan area. The implicit assumption is that employees who lived outside of this area would have been very unlikely to be working for the federal government in DC. We note that, even in the absence of errors, we should not expect this proportion to be 100% since some individuals working in the Federal Government in 1881 might have just arrived to DC (since the 1880 census took place in June of 1880 and the 1881 register captures the stock of federal employees as of July 1st of 1881).[58]

Panel (a) of Figure A9 computes, out of all the observations that we deem as a match, the fraction of individuals who are living in the "correct" area of residence as a function of the Jaro-Winkler string distance cutoffs that we use. Panel (b) instead computes the fraction of individuals in the correct location, but expressed as a fraction of the total number of observations that we attempt to match (similar to a "matching rate").

This figure illustrates the trade-off between type 1 and type 2 errors (or "precision" and "recall"). Choosing low values of c_1 and high values of c_2 results in high levels of precision (i.e. low false positive rates), but at the expense of matching relatively few employees (low "recall"). For the baseline analysis, we chose a combination of cutoffs that gives a balanced weight to precision and recall. Specifically, we chose c_1 and c_2 so as to maximize the harmonic mean of precision and recall (a standard performance measure in the machine learning literature, often referred to as the F_1 score).[59] Maximizing this function using the 1881 Register-1880 Census links leads to a choice of $c_1 = 0.7$ and $c_2 = 0.7$. In the analysis, we show the robustness of our results to alternative choices of the linking parameters.

Matching Rates. Figure A10 shows the proportion of individuals that we match to at least one census (and to at least two, three and four, respectively) when using our baseline choice of parameters, by register year. In this figure, we focus on matches to censuses conducted before each register year. Panels (a) and (b) show the proportion of male and female employees that we match to at least one census, respectively. Panels (c) and (d) show the proportion of employees that we match to a census in which the individual is

[58] The census started collecting information on previous place of residence only in 1940, which makes it hard to estimate the proportion of individuals who would have just moved into DC in any given year.

[59] $F_1 = 2\dfrac{precision * recall}{precision + recall}$.

below the age of 18, whereas panels (e) and (f) show the analogous figure for those that we match to a census where the individual is more than 18.[60] In all cases, the figures show that we are more likely to match male than female employees.

Because the first population census listing persons individually took place in 1850, we are not able to find employees in their childhood households (i.e. when they were less than 18 years old) if they would have been more than 18 years old by 1850. For instance, among employees in the 1871 register we can only link to their childhood household those who are at most 39 by 1871.[61] As a consequence, we expect the proportion of individuals with at least one match to their childhood household to be higher for later years, which is indeed what we see in the data.[62] Similarly, we expect a lower proportion of individuals in later years to be matched to at least one adult observation (as the last census we include is 1880 and some employees would have been less than 18 years old by 1880).

Representativeness of Linked Data and Potential Biases from Linking. We implement several empirical exercises to alleviate the concern that our results could be driven by differential matching into the linked sample:

First, we estimate our main difference-in-differences specification using as outcome variables: (1) the total number of censuses to which we link an employee, or (2) and indicator that takes a value of one if the employee is linked to at least one census. Table A1 shows that there is little correlation between the likelihood of finding an individual in the census and whether or not this individual was appointed through an exam.

Second, Table A2 shows that our result on the share of foreign-born workers (which *does not* require the linked data since we can observe birthplaces directly from the Registers) is very similar regardless of whether we estimate it using the smaller linked sample or the full non-linked sample.

Finally, our main results are also similar when we reweight the data to account for selection into the linked sample on the basis of employees' characteristics (Table A3). To implement this exercise, we follow a standard approach in papers using linked historical data (see, for instance, Pérez (2017) and Abramitzky et al. (2021)). The approach has the following two steps:

1. We estimate a probit of the probability of matching using the following set of fixed effects: birthplace, home-state, register year, occupation, and compensation (in $100 intervals).

[60] Note that we cannot match an individual to more than two censuses while the individual is still below the age of 18, as censuses were conducted every 10 years.

[61] 18+(1871-1850).

[62] For instance, someone who is 35 years old in 1871 could be observed only once (as a 15 year old in 1850), whereas someone who is 35 years old in 1881 could be observed twice (either as a 5 years old in 1850 or as a 15 years old in 1860).

2. We reweight the data using the inverse matching probability based on the estimated probabilities in the probit.

B Robustness Checks

In this section, we provide further details on the various robustness checks to our main findings.

Time-Varying Shocks and Additional Control Variables. By requiring the apportionment of classified positions across states, the reform increased the share of workers from certain states at the expense of workers from others. Although we include home-state fixed effects to account for this channel, a concern is that the labor market in different states might have been on different trends, leading to differential changes in the selection of workers interested in government jobs. In this case, the effects we capture would not be those of introducing exams but rather those of increasing the representation of certain states. To address this concern, in Figures B3 and B4 we show that our results are similar when we include home-state times hiring-year fixed effects.

In Figure B6, we show that our results are similar when we exclude employees from one Executive Department at a time. This finding rules out the possibility that our results were driven by a change simultaneous to the reform and specific to a single department. Moreover, our results are also similar when we include department times hiring-year fixed effects (Figures B3 and B4).

Alternative Samples and Definitions of the Control Group. In our baseline analysis, the control group is comprised of employees in the Executive Departments in DC who worked in exam-exempted positions (that is, positions either at the bottom or the top of the state hierarchy). Figures B3 and B4 show that our results are robust to using a host of alternative control groups and sample definitions, namely: (1) using a control group including only bureaucrats at the bottom of the state hierarchy,(2) using a control group including only bureaucrats at the top, (3) dropping workers making more than $3000 or less than $600 from the control group (so as to increase the treatment-control group comparability), (4) adding workers outside of DC to the control group, (5) adding workers who were employed in DC but worked outside of the Executive departments to the control group, (6) adding female employees (both to the treatment and control groups), and (7) estimating a "before and after" specification (Table B10).[63]

[63] In this exercise, we restrict the sample to employees in the "treated" positions and compare their characteristics before and after the reform (net of position fixed effects). Specifically, we report the estimated value of β from the following equation:

$$y_{ipt} = \alpha_p + \beta After_t + \gamma X_{ipt} + \epsilon_{ipt}$$

Adding Customs Service Employees to the Treatment Group. In our baseline, the treatment group is comprised of workers in the Executive Departments in DC who worked in positions subject to exams. However, our data include another group of exam-appointed individuals: employees in the *classified* Customs Service.[64]

We chose to focus on employees in the Executive departments in DC for three reasons. First, we have exact information on which of them were appointed through an exam, thus enabling us to minimize measurement error in our "treatment". Second, Moreira and Pérez (2021) show that, in the Customs Service, the reform induced a distortion in districts' personnel structure. In particular, it caused a sharp increase in the share of workers making less than $900 (who were exempted from exams). Such a response complicates the interpretation of our design, as it implies that the reform affected *both* the treatment and the control positions. Finally, focusing on a sample of employees who all worked in DC improves the comparability between the treatment and control groups.

Yet, Figures B3 and B4 show that our results are robust to adding Customs Service employees to the treatment group. In this figure, we classify these employees as having been appointed through an exam based on their estimated hiring date, position, and Customs collection district.

Identification of Exam-Appointed Employees. In our main analysis, we identify exam-appointed employees by using the list of exam-appointed employees published by the Civil Service Commission. An alternative approach to identify such employees would be to use an employee *position* and *hiring year* in combination with the rules of the reform. However, a challenge with this approach is that we lack direct information on workers' hiring year. Although we can infer this information by comparing adjacent personnel rosters, this approach is imperfect since transcription errors or incomplete personnel data will make it such that some employees will be deemed as new hires even if they are not.

To illustrate why using this second approach can severely attenuate our estimates of the effects of exams, consider the following example. There were about 5,000 clerks in the Departmental Service in DC in 1885 (the first post-reform year for which we have data), of which only about 300 had been hired through exams. Assume that we incorrectly classify 10% of the 4,700 (5,000-300) clerks hired before the reform as post-reform hires (which, in turn, implies that we would classify them as "exam-appointed"). In this case, our "treatment" group in 1885 would be a combination of the actually treated (the 300 clerks hired through exams after the reform) and the incorrectly classified as treated (the 470 clerks incorrectly labeled as post-reform hires). Note that in this example even relatively

using the sample of employees in "treated" positions.

[64] As discussed above, we do not have data on employees in the classified *Postal* Service.

modest error rates in our identification of new hires lead to a large proportion ($470/(470+300) \approx 61\%$) of the "treatment" group being comprised of individuals not actually hired through exams.[65]

In contrast to exam-appointed employees (which we can identify based on the published list), we are forced to identify workers in our post-reform control group by comparing adjacent registers. Given the measurement error described above, this implies that our post-reform control group will include a combination of workers hired both before and after the reform. Hence, if employees hired after the reform had higher social status than those hired before (for reasons unrelated to exams), we could find an increase in the average social status of the treatment group even if exams were inconsequential for workers' backgrounds (as the treatment group will be 100% comprised of post-reform hires whereas the control group will include a mix of pre- and post-reform hires).

We conduct several exercises that suggest that this measurement error cannot rationalize our findings (see Figures B3 and B4). First, we show that our results are similar when we control for an individual *birth year*. Intuitively, if workers entered government jobs at similar ages, controlling for birth year effectively controls for a person's hiring year. Second, we show that our results are similar as we adopt a more stringent string distance cutoff to determine an employee hiring year. If our results were driven by missclassifying workers in the control group as post-reform hires, we should observe that our results become smaller as we apply a more stringent definition to identify such workers (which we do not). Third, the results are also not sensitive to refining the control group in the post-reform period so that it becomes closer to our "ideal" control group. Specifically, we drop from the control group in the post-reform period those individuals that worked in positions that had in principle an exam requirement according to the rules (and who then should have not been included in the control group absent measurement error).

Finally, note that, the more years apart we are from the reform, the "cleaner" the control group becomes (as employees hired prior to the reform progressively exit the government and hence no longer can be erroneously included in the post-reform control group). Hence, if our results were solely explained by measurement error, the size of our estimated effects should decrease monotonically over time as the control group approximates the "ideal" control group. However, we find that the results do not exhibit such pattern: while we estimate the strongest effects immediately after the reform, the effect sizes are of roughly the same magnitude four years after the reform than after a full decade. Similarly, for our findings to be explained by this measurement error there would

[65] Using our baseline definition of a new hire, the fraction of the "treatment" group that we would incorrectly classify as exam-appointed using this approach would be about 66%, similar to the example above.

need to be an upward trend in workers' socioeconomic status. Yet, Figure B1 shows that, if anything, the socioeconomic status of workers for which we are certain about their hiring year exhibits a *downward* trend. Taken together, these findings reassure us that this measurement error is unlikely to explain our findings.

Inference. Figure B5 shows that our results are robust to implementing a randomization inference approach. To do so, we randomly classify a group of workers (of equal size of our actual treatment group) as exam-appointed. We then estimate the "effects" of the reform using these placebo treatment groups, repeating the exercise 1,000 times. Reassuringly, these placebo estimates are centered around zero and much smaller in absolute value than the actual estimates.

Linking Strategy. First, Figure A1 shows that our results are similar when we use more or less conservative cutoffs for deeming an observation as a match. Second, Table A3 and Figure A1 show that our results are similar if we reweight the data to account for differences in the observable characteristics of matched and non-matched employees.[66] Finally, as discussed above, our results focusing on the proportion of foreign-born employees (which do not require linked data) are similar when estimated using either the linked or the non-linked samples (Table A2).

Missing Data on Migrants' Parental Characteristics. Because our information on parental characteristics is based on observing children living with their parents in the US census, we do not have this information for those employees who moved to the US as adults. This omission could be problematic because the reform reduced immigrants' representation (Table 3).

To deal with this issue, we implement an exercise, in the spirit of Lee (2009), in which we bound the bias that could result from this omission. Specifically, we re-estimate our main specification in an expanded sample in which we impute foreign-born employees three alternative values of the parental summary index: (1) the 10th percentile of the value observed among all employees of the same occupation in the pre-reform period, (2) the 50th percentile, and (3) the 90th percentile. This expanded sample is constructed such that the fraction of immigrants in a given year is the same as in the actual non-linked sample.

Table B11 shows that, even under the extreme assumptions that immigrants whose parental information we do not observe were at the 10th or the 90th percentile of the summary index, there would have still been an increase in this index.

[66]See Section A for further details.

TABLE A1: EXAMS AND THE LIKELIHOOD OF MATCHING AND EMPLOYEE TO THE CENSUS

	Number of matches		1+ Matches		+1 Matches to Childhood Info		1+ Matches to Adult Info	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exam	-0.0324	-0.0335	-0.00692	-0.00903	0.00840	0.00770	-0.0185	-0.0146
	(0.0332)	(0.0332)	(0.0186)	(0.0186)	(0.0104)	(0.0104)	(0.0150)	(0.0146)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	25442	25442	25442	25442	25442	25442	25442	25442
Mean of dep. var.	0.536	0.536	0.337	0.337	0.110	0.110	0.797	0.797

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 and 2 is an indicator that takes a value of one if a an employee is successfully matched to at least one observation in the census. The dependent variable in columns 3 and 4 is instead the total number of censuses to which an employee is matched to. All columns include hiring year and position fixed effects. The even columns further include home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors are clustered at the position level.

Table A2: Effects of the Reform on the Share of Foreign-Born Employees

	Full Sample		Linked Sample		Linked Sample, Reweighted	
	(1)	(2)	(3)	(4)	(5)	(6)
Exam	-0.0517***	-0.0464***	-0.0473**	-0.0419**	-0.0586**	-0.0511**
	(0.0163)	(0.0145)	(0.0190)	(0.0176)	(0.0229)	(0.0208)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	Yes	Yes
Observations	24375	24375	9238	9238	9238	9238
Mean of dep. var.	0.138	0.138	0.108	0.108	0.108	0.108

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is an indicator that takes a value of one if an employee is foreign born. The sample in columns 1 and 2 includes all employees in our target baseline sample. The sample in columns 3 to 6 includes only those employees that we successfully link to an observation in the census. In columns 5 and 6, we reweight the data to account for differences in the matching likelihood across individuals. All columns include hiring year and position fixed effects. The even columns further include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors are clustered at the position level.

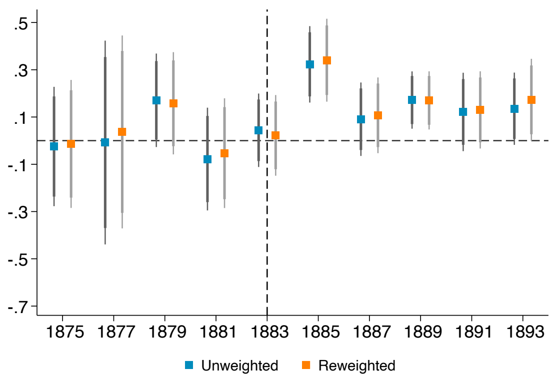
Table A3: Robustness to Reweighting

	Unweighted		Weighted	
	(1)	(2)	(3)	(4)
Exam	0.180***	0.169***	0.193***	0.182***
	(0.0451)	(0.0468)	(0.0460)	(0.0471)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes
Observations	2944	2944	2944	2944
Mean of dep. var.	0.128	0.128	0.128	0.128

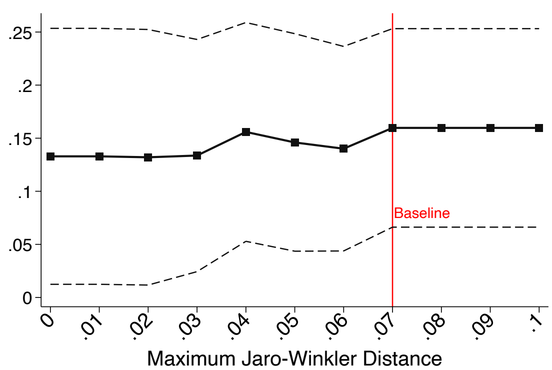
Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' socioeconomic background. The index is computed using the approach in Kling et al. (2007). The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. The table shows the sensitivity of the difference-in-differences estimates to reweighting the data to account for differences in the matching likelihood across individuals. Columns 1 and 2 present results using the unweighted data, whereas columns 3 and 4 use the reweighted data. All columns include hiring year and position fixed effects. The even columns further include employees' home-state fixed effects. The figure in panel (b) shows the sensitivity of the event-study estimates. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors are clustered at the position level.

FIGURE A1: ROBUSTNESS TO REWEIGHTING AND TO ALTERNATIVE LINKING CUTOFFS

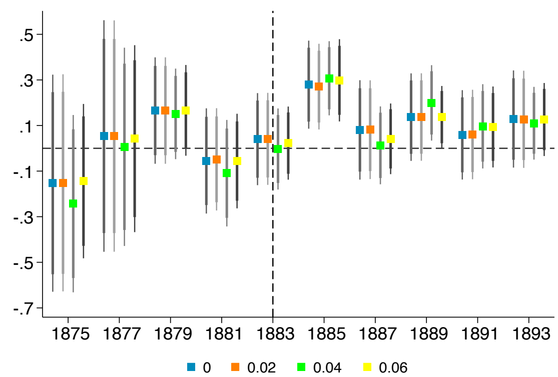
(A) REWEIGHTING, DYNAMIC SPECIFICATION



(B) ALTERNATIVE LINKING CUTOFFS, MAIN SPECIFICATION



(C) ALTERNATIVE LINKING CUTOFFS, DYNAMIC SPECIFICATION



Notes: The outcome variable in all panels is a summary index of employees' socioeconomic background computed using the approach in Kling et al. (2007). The index combines information on parental wealth rank, parental occupations, parental literacy, own and parental birthplace and race. The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. Panel (a) shows the sensitivity of the estimates corresponding to equation 2 to reweighting the data to account for differences in the matching likelihood across individuals. Panel (b) shows the estimated effects of the reform on the index (y-axis), as a function of the minimum Jaro-Winkler string distance above which an observation would no longer be considered a match (x-axis). Lower values of the Jaro-Winkler distance represent more conservative matches. The red vertical bar in panel (a) corresponds to the cutoff used in the baseline approach. Panel (c) shows the corresponding dynamic estimates when using alternative Jaro-Winkler cutoffs (as indicated by the figure's legend). The sample in all panels is restricted to newly hired employees in the Executive Departments in DC.

Figure A2: Example page, Official Register of the United States (1881)

DEPARTMENT OF STATE.

Name and office.	Where born.	Whence appointed.	Where employed.	Compensation.
Secretary of State.				
James G. Blaine	Pennsylvania	Maine	Washington	$8,000 00
Assistant Secretary of State.				
Robert R. Hitt	Ohio	Illinois	Washington	4,500 00
Second Assistant Secretary of State.				
William Hunter	Rhode Island	Rhode Island	Washington	3,500 00
Third Assistant Secretary of State.				
Walker Blaine	Maine	Maine	Washington	3,500 00
Chief Clerk.				
Sevellon A. Brown	New York	New York	Washington	2,500 00
Chief of Diplomatic Bureau.				
Alvey A. Adee	New York	District of Columbia	Washington	2,100 00
Chief of Consular Bureau.				
Francis O. St. Clair	New York	Maryland	Washington	2,100 00
Chief of Bureau of Indexes and Archives.				
John H. Haswell	New York	New York	Washington	2,100 00
Chief of Bureau of Accounts and Disbursing Clerk.				
Robert C. Morgan	New York	New York	Washington	2,100 00
Chief of the Bureau of Statistics.				
Michael Scanlan*	Ireland	New York	Washington	2,100 00
Translator.				
Henry L. Thomas	New York	New York	Washington	2,100 00
Clerks.				

Notes: This figure shows an example page corresponding to the 1881 edition of the "Official Registers of the United States" (Civil Service Commission, 1893). The page lists employees of the State Department.

Figure A3: Example Exam Questions

(a) Orthography

The Examiner pronounces each word and gives its definition as printed below. The competitor is required to write ONLY THE WORD *and* NOT *its definition.*

1. *Speech.* An address; an oration.
2. *Impeach.* To accuse; to bring into question.
3. *Conceited.* Vain; opinionated.

12. *Comical.* Droll; odd; ridiculous.
13. *Memorize.* To commit to memory.
14. *Disguise.* To mask; to muffle; to conceal the appearance of.

(b) Penmanship

[N. B.—The mark on penmanship is determined by legibility, neatness, and general appearance, and by correctness and uniformity in the formation of words, letters, and punctuation marks in the 2nd Exercise of the Third Subject—Writing from plain copy—and in the Exercise of the Fourth Subject—Letter-writing.]

(c) Copying

One of the Examiners dictates an exercise of not less than ten lines so distinctly that all the persons being examined can hear him. The passage is first read for information, and then dictated in phrases of five or six words, at the rate of from fifteen to twenty-five words per minute. If from any cause the competitor misses a word, he is cautioned not to pause, but to leave a blank space and go on with the next words he hears. Three minutes are allowed after the dictation for punctuation and correction.

(d) Arithmetic

Question 1. Add the following, placing the sum at the bottom:

79,654,321,908.35
47,776,013,703.30
92,773,331,673.25
7,774,910,336.15
44,297,794,329.37
6,105,733,266.59
232,173.63
8,859,367,397.45
42,223,001,764.86
63,337,476,074.03
2,335,602,047.90
293,827,764,501.77

(e) Meteorological clerk

SIXTH SUBJECT.—*Meteorology.*

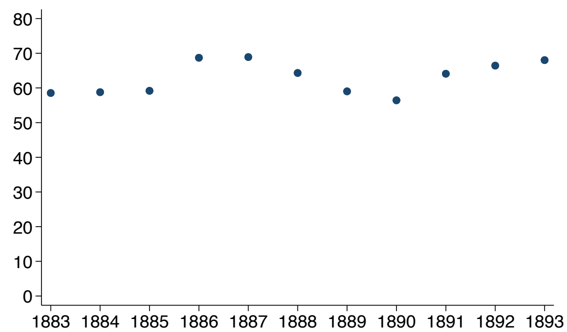
Question 1. What use is made of barometers by the Signal Service?
Question 2. Define an isothermal line.
Question 3. How does the sun heat the atmosphere?
Question 4. What instrument is used to measure the velocity of the wind?
Question 5. From what directions are the prevailing surface winds within the equatorial system?

Notes: Panels (a) to (d) show example question of the orthography exam, penmanship, copying and arithmetic exams. These exams were required for all applicants taking either the "general" (for clerks) or "limited" (for copyists) exams. Panel (e) shows an example question of the special exam for "meteorological clerks" in the Department of Agriculture. Source: Civil Service Commission (1893).

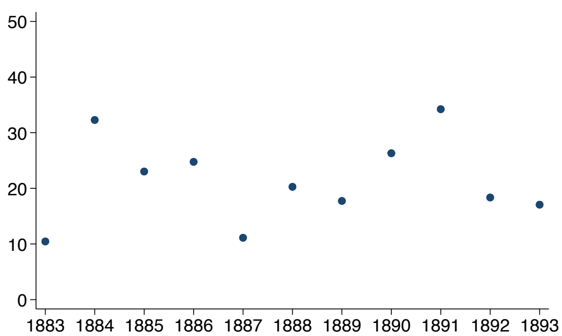
FIGURE A4: TOTAL NUMBER OF APPLICANTS AND EXAM PASSING RATES, BY YEAR AND EDUCATIONAL BACKGROUND

(A) NUMBER OF APPLICANTS

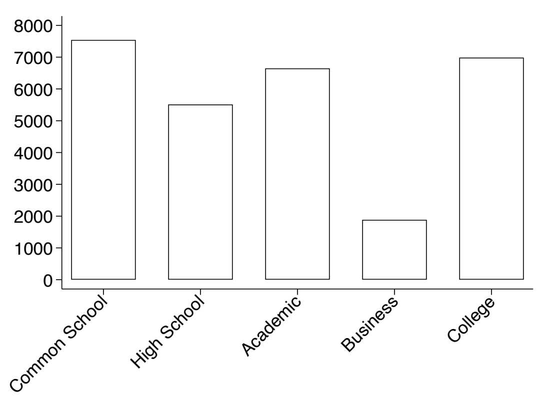
(B) PASSING RATE (%)



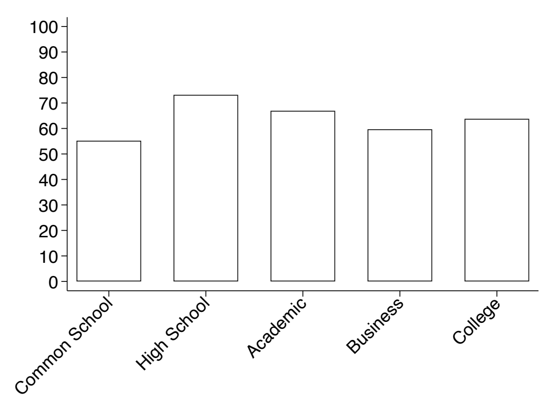
(C) APPOINTMENT RATE (%)



(D) NUMBER OF APPLICANTS, BY EDUCATIONAL BACKGROUND

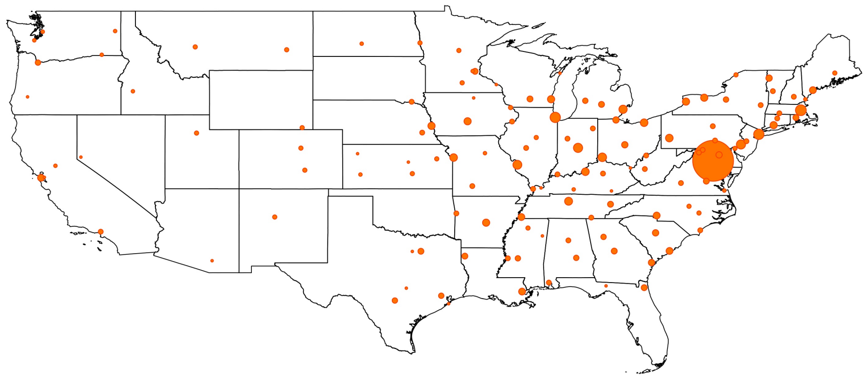


(E) PASSING RATES, BY EDUCATIONAL BACKGROUND



Notes: Panels (a) to (c) in this figure show the yearly number of applicants (Panel (a)), the share of applicants who obtained the minimum qualifying score (Panel (b)), and the share of appointed employees (out of those who obtained the minimum qualifying score) (Panel (c)). Panel (d) shows the number of applicants to the "Classified Departmental Service" in DC, by applicants' educational background. Panel (e) shows the fraction of such applicants who obtained a passing grade. The last two panels correspond to applicants who completed exams from 1886 to 1893. Data are based on Civil Service Commission (1893).

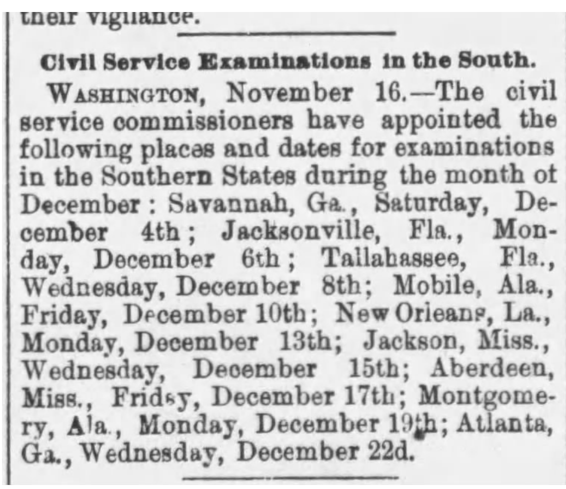
Figure A5: Location of Civil Service Exams, 1886-1893



Notes: This map shows the location of all civil service exams that took place from 1886 to 1893. The circles are drawn in proportion to the number of exams that took place in each location. The largest circle corresponds to Washington, DC, which hosted more than 300 exams in the period. Data are from the reports of the Civil Service Commission (Civil Service Commission, 1893).

Figure A6: Example Articles Advertising the Exams and Tutoring Services

(a) The Macon Telegraph, 11/17/1886

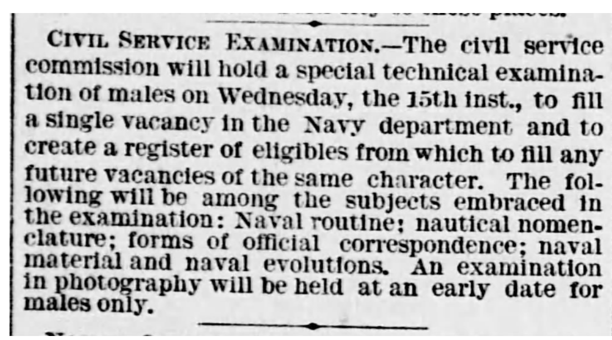


their vigilance.

Civil Service Examinations in the South.

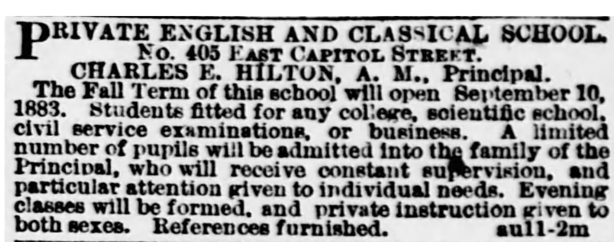
WASHINGTON, November 16.—The civil service commissioners have appointed the following places and dates for examinations in the Southern States during the month of December: Savannah, Ga., Saturday, December 4th; Jacksonville, Fla., Monday, December 6th; Tallahassee, Fla., Wednesday, December 8th; Mobile, Ala., Friday, December 10th; New Orleans, La., Monday, December 13th; Jackson, Miss., Wednesday, December 15th; Aberdeen, Miss., Fridsy, December 17th; Montgomery, Ala., Monday, December 19th; Atlanta, Ga., Wednesday, December 22d.

(b) The Evening Star, 12/10/1886



CIVIL SERVICE EXAMINATION.—The civil service commission will hold a special technical examination of males on Wednesday, the 15th inst., to fill a single vacancy in the Navy department and to create a register of eligibles from which to fill any future vacancies of the same character. The following will be among the subjects embraced in the examination: Naval routine; nautical nomenclature; forms of official correspondence; naval material and naval evolutions. An examination in photography will be held at an early date for males only.

(c) Evening Star, 9/19/1883



PRIVATE ENGLISH AND CLASSICAL SCHOOL.
No. 405 EAST CAPITOL STREET.
CHARLES E. HILTON, A. M., Principal.
The Fall Term of this school will open September 10, 1883. Students fitted for any college, scientific school, civil service examinations, or business. A limited number of pupils will be admitted into the family of the Principal, who will receive constant supervision, and particular attention given to individual needs. Evening classes will be formed, and private instruction given to both sexes. References furnished. au11-2m

(d) Evening Star, 10/17/1884

PUPILS PREPARED FOR SMITH, WELLESLEY and Vassar Colleges. Also for Civil Service Examination. 1006 N st. n.w. Pupils of all ages instructed in English, French and Music. Highest references. Apply to Principal. oc10-3m*

Notes: Panels (a) and (b) in this figure show example articles in which local newspapers announced the dates and location of the civil service exams. Panels (c) and (d) show newspaper articles offering tutoring services for applicants to the civil service. The images are from Newspapers.com.

FIGURE A7: EXAM-APPOINTED EMPLOYEES, CLASSIFIED DEPARTMENTAL SERVICE

APPENDIX TABLE 1.—*Appointments, promotions, separations, and restorations*

Name.	Legal resi-dence.	Appoint-ments to each state.	Whole number of appoint-ments.	Department to which certi-fied.	Grade for which certified.	Date of pro-bationary appointment.
Weller, Ovington E	Md	1	1	Postoffice	$1,000	Aug. 29, 1883
Hoyt, Miss Mary F	Conn	1	2	Treasury	900	Sept. 5, 1883
Keller, Benjamin F	Pa	1	3	War	1,000	Sept. 13, 1883
Brown, Edward N	N. Y	1	4	....do	1,000	....do
Bird, Frank W	Mass	1	5	....do	1,000	Sept. 19, 1883
Lewis, William H	Kans	1	6	....do	1,000	Sept. 21, 1883
Dubuar, Charles L	Mich	1	7	....do	1,000	....do
Smith, Harry W	Iowa	1	8	....do	1,000	Sept. 25, 1883
Pennywitt, William C	Ky	1	9	Postoffice	1,000	Sept. 27, 1883
Piles, Joseph W	Mo	1	10	War	1,000	Sept. 28, 1883
Chaplain, William M	N. C	1	11	....do	1,000	....do
Raymond, Thomas U	Ind	1	12	....do	1,000	Sept. 29, 1883
Chase, George W	R. I	1	13	....do	1,000	....do
Dudley, Irving B	Wis	1	14	....do	1,000	....do
Pyles, Miss Marion	Vt	1	15	Treasury	900	Oct. 1, 1883
Peake, James B	D. C	1	16	....do	900	Oct. 3, 1883

Notes: This figure shows an example page listing employees appointed to the classified departmental service. This page is from the 1886 report of the Civil Service Commission (Civil Service Commission, 1893).

FIGURE A8: POSITIONS SUBJECT TO EXAMS IN THE TREASURY DEPARTMENT

REPORT OF THE CIVIL SERVICE COMMISSION. 249

IN THE TREASURY DEPARTMENT AT WASHINGTON.

[June 30, 1892.]

CLASSIFIED SERVICE.

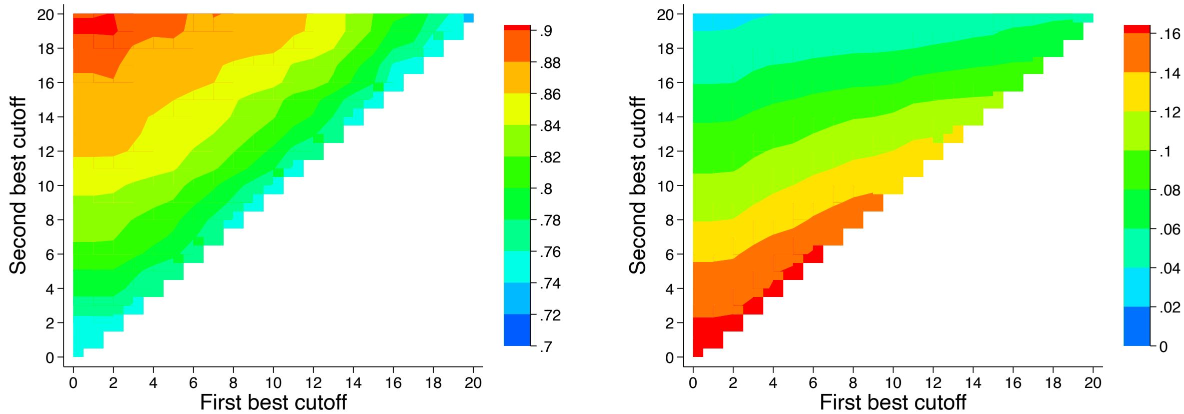
	Yearly salary.	Aggregate yearly salary.
I. *Places classified and excepted from examination.*		
1 adjuster of accounts	$2,000	$2,000.00
1 adjuster	1,500	1,500.00
1 assayer	2,200	2,200.00
1 assistant and chief clerk	2,500	2,500.00
1 assistant cashier	3,200	3,200.00
1 assistant in charge of office and topography, Coast Survey	2,000	2,000.00
1 assistant superintendent Treasury building	2,100	2,100.00
2 assistant tellers	2,250	4,500.00
1 attendant	720	720.00
4 binders at $4 per diem	....	5,840.00
2 binders	900	1,800.00
10 binders	840	8,400.00
1 bond clerk	1,600	1,600.00
11 cabinetmakers	1,000	11,000.00

Notes: This figure shows an example page listing the positions that were subject to exams in the Department of the Treasury. This page is from the 1892 report of the Civil Service Commission (Civil Service Commission, 1893).

Figure A9: Assessing the Performance of the Linking Algorithm

(a) % matches with matching place of residence

(b) % observations with matching place of residence



Notes: Panel (a) shows, out of all the individuals who are *linked* from the 1881 Register to the 1880 census, the share who are linked to someone living in a "correct" 1880 county of residence. "Correct" counties are those in the DC-Baltimore metropolitan area. Panel (b) shows, out of *all* the individuals that we attempt to link from the 1881 register to the 1880 census, the share who are linked to someone living in a "correct" county. These two statistics are plotted as a function of the parameters that we use to determine whether or not we deem an observation as a link. See A for more details on the choice of these parameters. The sample is restricted to workers in the Executive Departments in DC in the 1881 Register.

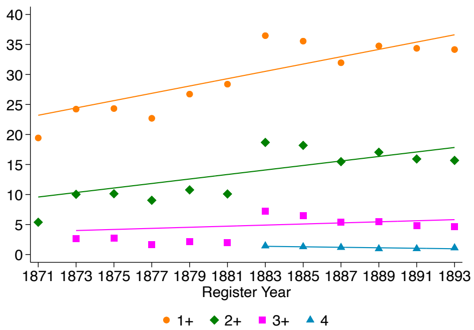
Table A4: Sample Construction

	N	%
Employee-Years in Executive Departments in DC (1873-1893)	99282	100
New Hires	42545	42.85
With Information on Parental Occupations	7439	17.49
Males	5052	11.87
With Information on Parental Wealth	4590	10.79
Males	3074	7.23
With Information on Own Occupation	4990	11.73
Males	3623	8.52

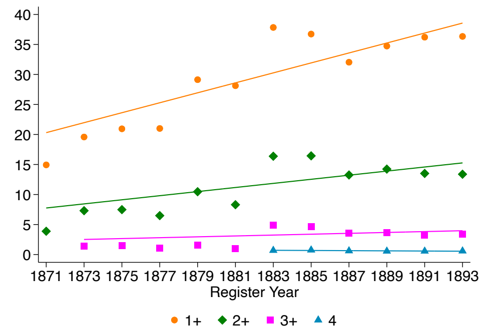
Notes: This table shows the construction of our baseline sample. We start from a list of employees who worked in the Executive Departments in DC in the 1873-1893 period. We then restrict this sample to those who are new hires, which we identify by comparing employee rosters in adjacent Registers. The table then reports the fraction of these individuals for whom we observe parental occupations, parental wealth, and own occupation prior to joining the civil service. Parental wealth is less frequently observed than parental occupations as it was only reported in the 1860 and 1870 censuses, whereas parental occupations are observed in every census year from 1850 to 1880.

Figure A10: Matching Rates, by Register Year

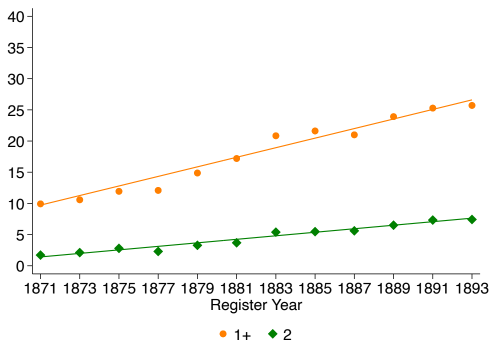
(a) % Matched, Male Employees



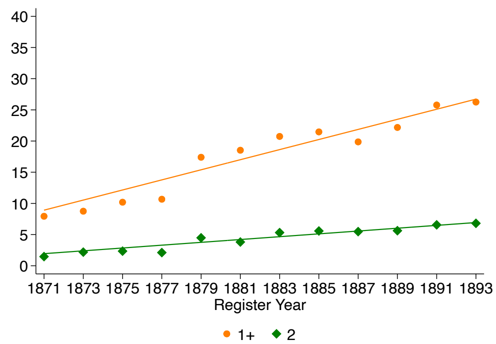
(b) % Matched, Female Employees



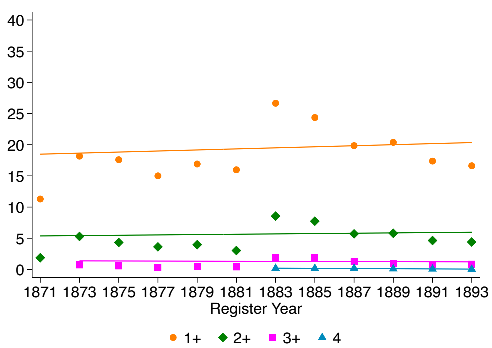
(c) % Matched as <18 year old, Males



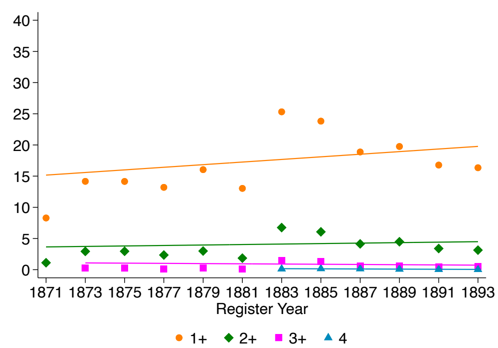
(d) % Matched as <18 year old, Females



(e) % Matched as $18+$ year old, Males

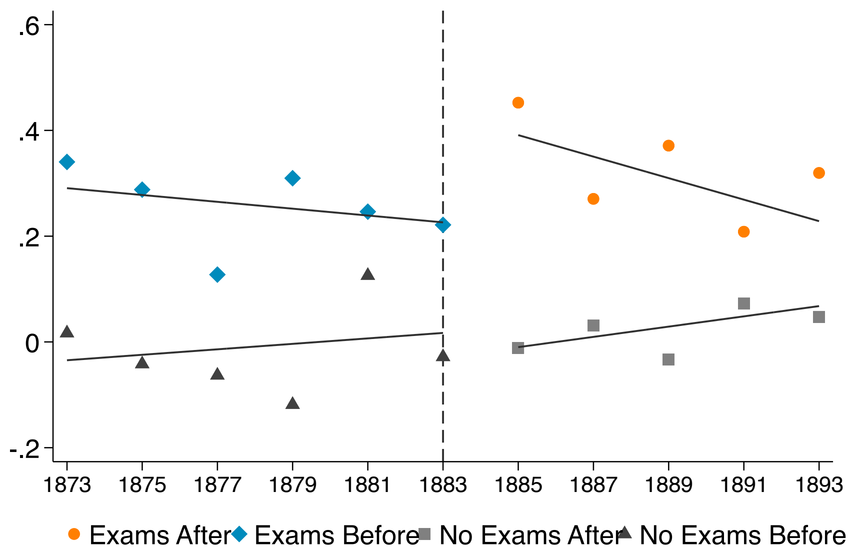


(f) % Matched as $18+$ year old, Females



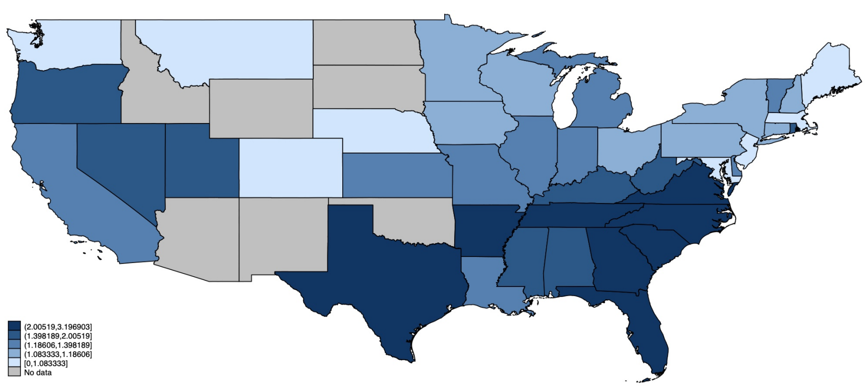
Notes: Panel (a) shows the proportion of male employees that we match to at least one, at least two, at least three or exactly four censuses in our baseline sample, by register year. Panel (c) shows the corresponding proportion when limiting the set of links to those in which employees were less than 18 year old at the time we observe them in the census (that is, the sample we use to measure employees' parental characteristics), whereas panel (e) shows such proportion when we only consider links in which employees were 18 or more (that is, the sample we use to measure workers' occupations prior to joining the civil service). Panels (b), (d) and (f) repeat the analysis for female employees. In all cases, we only include matches to population censuses that took place before the corresponding register. Note that individuals cannot be matched to more than two censuses while still being less than 18 years old (since censuses were conducted every 10 years). The sample is restricted to employees in the Executive Departments in DC.

Figure B1: Summary Index of Socioeconomic Background



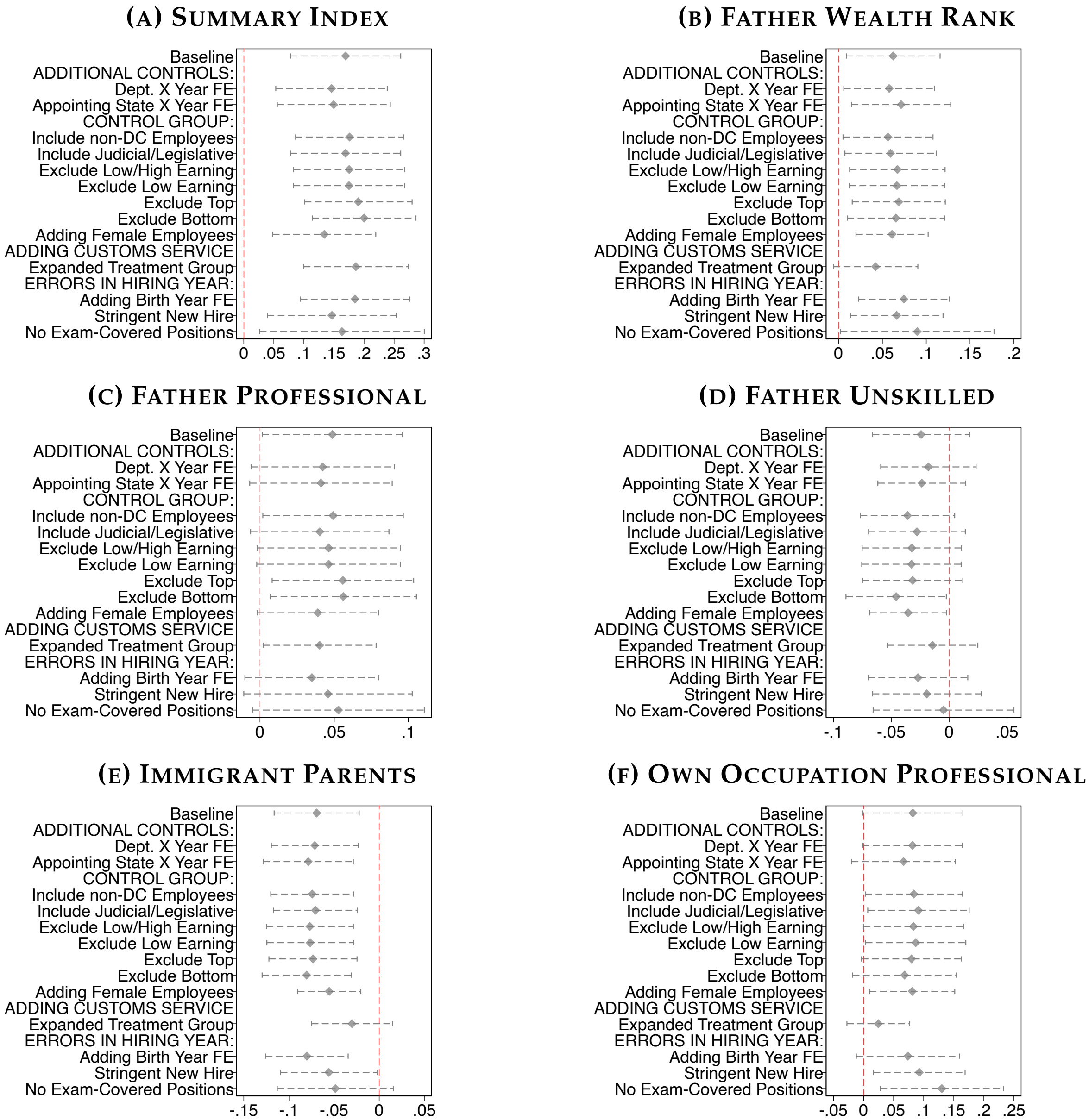
Notes: This figure shows the average value of the summary index of employees' socioeconomic background for workers in positions subject and non-subject to exam, by hiring year. Higher value in the index corresponds to a higher socioeconomic status. The sample is restricted to newly hired employees in the Executive Departments in DC.

Figure B2: Inequality in School Attendance, by State of Residence (1870)



Notes: This map shows the ratio between: (1) the likelihood that a child from a family in the top 20% of the wealth distribution would be in school, and (2) the likelihood that a child from the bottom 20% would be in school. These ratios are computed based on children aged 8-12 in the 1870 census (Ruggles et al., 2021).

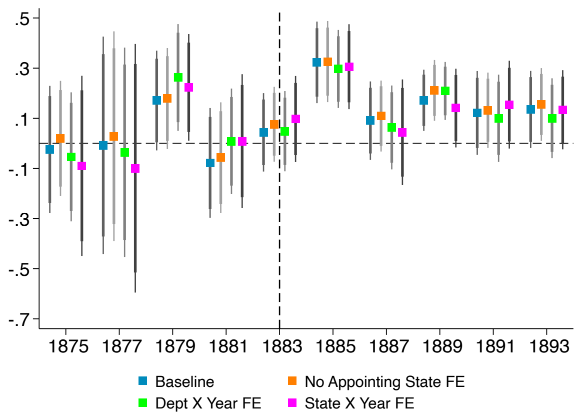
FIGURE B3: ROBUSTNESS TO ALTERNATIVE SPECIFICATIONS AND SAMPLES



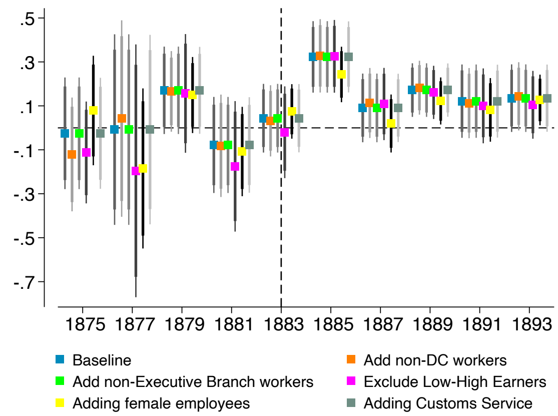
Notes: This figure presents the sensitivity of our estimates to a number of alternative specifications and samples. The top row in each figure presents our baseline estimate. Each panel corresponds to a different outcome variable. In the rows under "Additional controls", we add additional control variables to our baseline specification: (1) Department $\times$ hiring-year fixed effects, and (2) state of residence at the time of appointment $\times$ hiring-year fixed effects. In the row under "Control group", we use alternative definitions of the control group: (1) Including workers outside of DC, (2) including workers in the Judicial and Legislative branches of government, (3) excluding employees making less than \$600 or more than \$3000, (4) excluding employees making less than \$600, (5) excluding workers who were exempted from exams due to their low salaries, (6) excluding those who were exempted from exams due to being in hierarchical positions, and (7) adding female employees (both in the treatment and control group) to the sample. In the row under "Adding Customs Service", we use a expanded definition of the treatment group which includes employees in the "classified Customs Service" (that is, the workers in the Customs Service who worked in positions subject to exams). In the row under "Errors in hiring year", we (1) add birth year fixed effects to the regression, (2) use a more stringent definition of which observations we consider to be a new hire, and (3) drop from the control group in the post-reform period those workers in positions that should have been appointed through an exam according to the rules of the reform.

FIGURE B4: DYNAMIC EFFECTS OF THE REFORM ON THE SUMMARY INDEX OF EMPLOYEES' SOCIOECONOMIC BACKGROUND: ROBUSTNESS TO ALTERNATIVE SPECIFICATIONS

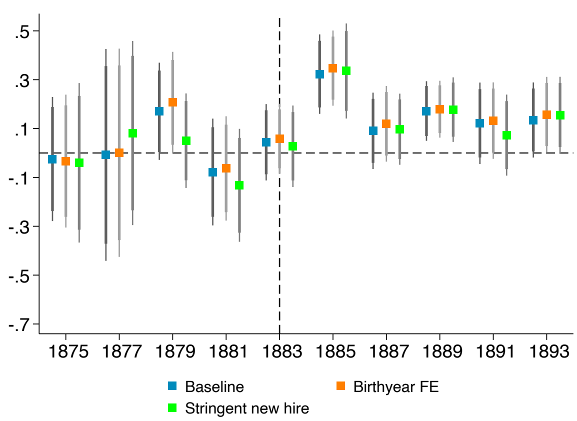
(A) ADDITIONAL CONTROL VARIABLES



(B) ALTERNATIVE CONTROL GROUPS

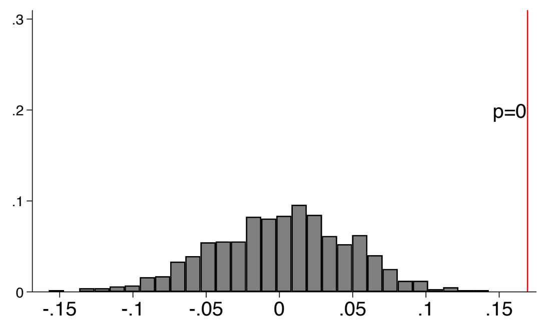


(C) ERRORS IN HIRING YEAR



Notes: This figure presents the sensitivity of the estimates in equation 2 to several alternative specifications. See notes to Figure B3 for details on the specifications included. The dependent variable in an index of workers' socioeconomic backgrounds computed using the approach in Kling et al. (2007). The index combines information on parental wealth rank, parental occupations, parental literacy, own and parental birthplace and race. The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status.

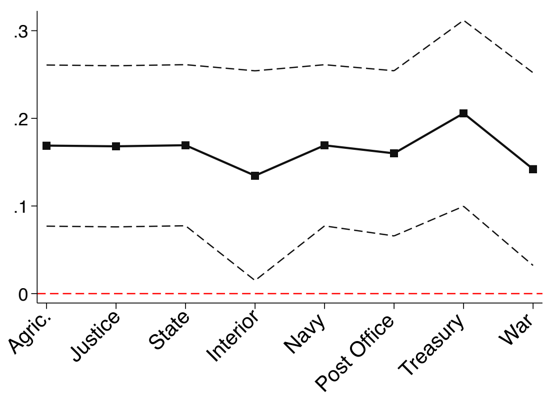
FIGURE B5: EFFECTS OF THE REFORM ON THE SUMMARY INDEX OF EMPLOYEES' SOCIOECONOMIC BACKGROUND: RANDOMIZATION INFERENCE



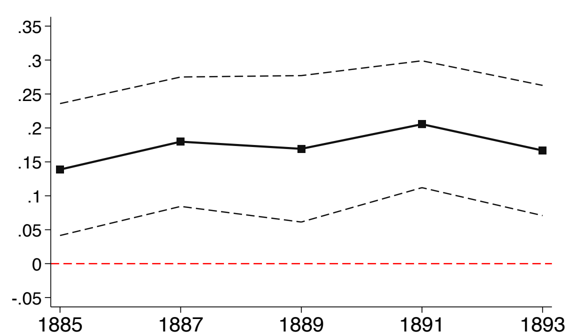
Notes: The outcome variable is the summary index of socioeconomic background computed using the approach in Kling et al. (2007). The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. This figure shows the empirical distribution of estimated effects when we implement a randomization inference approach. In this exercise, we randomly select a group of workers as the treatment group and estimate the "effects" of the reform using our baseline model. We repeat this exercise 1,000 times and plot the empirical distribution of estimated effects. The vertical red line corresponds to the estimated effect when we use the actual set of treated employees. The sample is restricted to newly hired employees in the Executive Departments in DC.

FIGURE B6: EFFECTS OF THE REFORM ON THE SUMMARY INDEX OF EMPLOYEES' SOCIOECONOMIC BACKGROUND: ROBUSTNESS TO EXCLUDING ONE DEPARTMENT AT A TIME OR ONE POST-REFORM YEAR AT A TIME

(A) EXCLUDING ONE DEPARTMENT



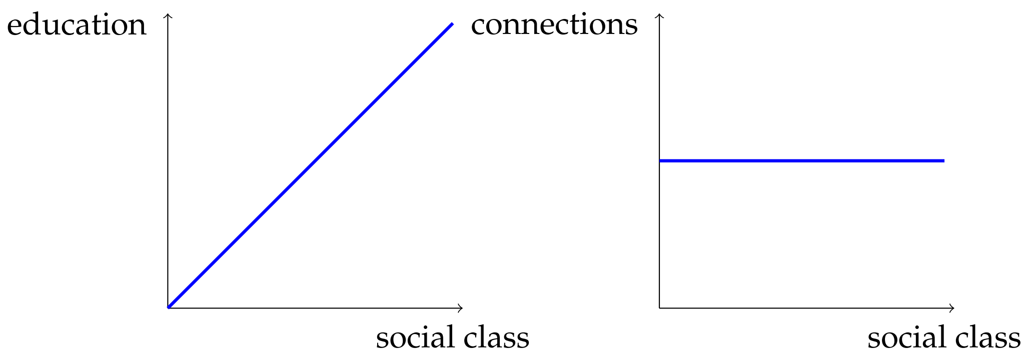
(B) EXCLUDING ONE POST-REFORM YEAR



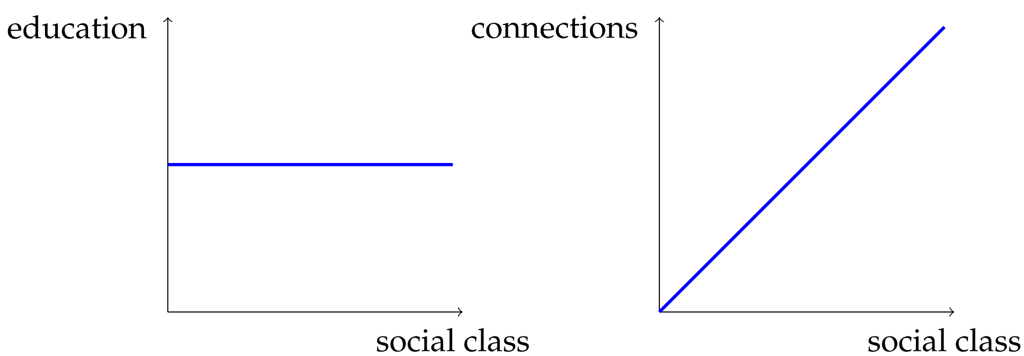
Notes: Panel (a) in this figure shows the sensitivity of the effects of the reform on the summary index of socioeconomic background (computed using the approach in Kling et al. (2007)) to excluding workers from one executive department at a time. The index combines information on parental wealth rank, parental occupations, parental literacy, own and parental birthplace and race. The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. The y-axis shows the estimated effect of exams on the summary index, whereas the x-axis shows the excluded department. The estimated effects are plotted around a 95% confidence interval. The sample is restricted to newly hired employees in the Executive Departments in DC. Panel (b) performs a similar analysis but instead excluding one post-reform year at a time.

FIGURE B7: AMBIGUOUS RELATIONSHIP BETWEEN EXAMS AND WORKERS' EXPECTED SOCIOECONOMIC BACKGROUNDS

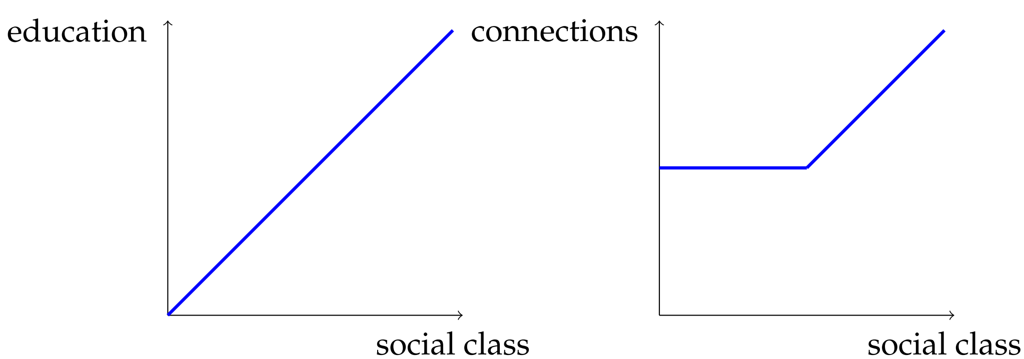
(A) EXAMS HELP THE "RICH"



(B) EXAMS HELP THE "POOR"

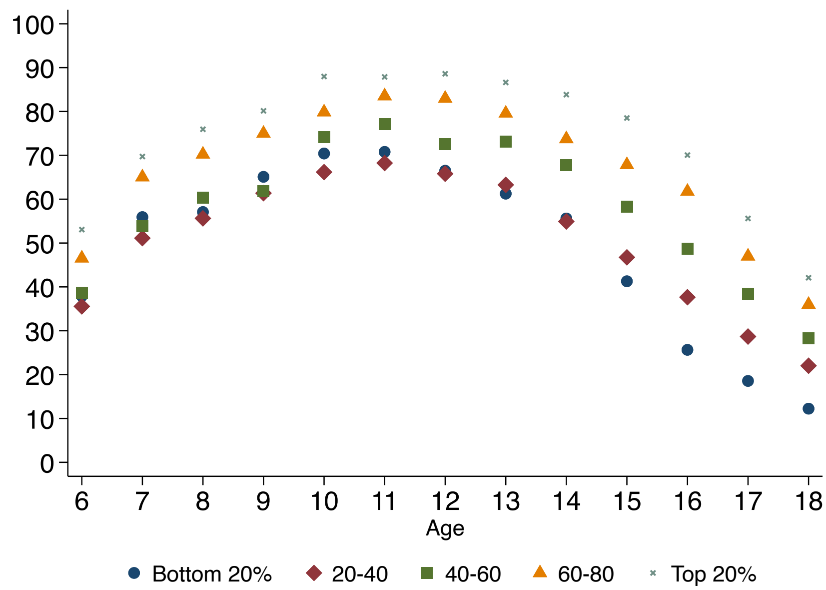


(C) EXAMS HELP THE "MIDDLE"



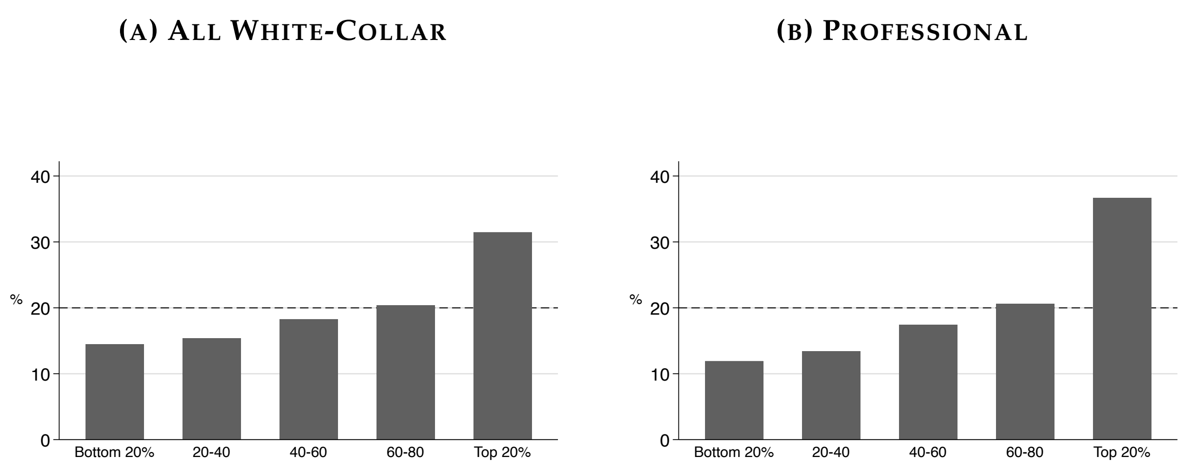
Notes: These figures illustrate the ambiguous relationship between the introduction of exams and the representation of workers from different socioeconomic backgrounds. Each panel depicts a hypothetical relationship between applicants' social class and education (on the left) and social class and connections (on the right). In our conceptual framework, workers are hired if they are among the top $t\%$ applicants in terms of their combined value of education and connections. We conceptualize the reform as an increase in the relative weight of education in the hiring process.

FIGURE B8: SCHOOL ATTENDANCE RATES (%) BY AGE AND PARENTAL WEALTH, 1870 CENSUS



Notes: This figure shows school attendance rates for children of different ages, based on the wealth quintile of their parents. The figure is based on a sample from the 1870 census (Ruggles et al., 2021).

FIGURE B9: PARENTAL WEALTH QUINTILES OF *Private Sector* WHITE-COLLAR WORKERS



Notes: This figure shows the parental wealth quintiles of private sector white-collar workers in 1880. Panel (a) includes all white-collar workers, whereas Panel (b) includes only those with a professional occupation. Professional occupations are those with a value of less than 100 in the 1950 Census occupational classification system (such as lawyers and accountants). Non-professional white-collar occupations are those with a value between 200 and 500 (for example, clerks). These figures are based on a sample linking adults in the 1880 census to their childhood households in the 1860 census.

TABLE B1: SUMMARY STATISTICS

	Non-Exam			Exam		
	Mean	Median	Observations	Mean	Median	Observations
	(1)	(2)	(3)	(4)	(5)	(6)
i. Parental Wealth Ranks						
Total	0.53	0.53	2637	0.62	0.68	437
Personal Property	0.53	0.55	2637	0.62	0.68	437
Real Estate Property	0.55	0.54	2637	0.62	0.65	437
ii. Parental Occupations						
Professional	0.09	0.00	4396	0.14	0.00	656
White-Collar Non-Prof	0.19	0.00	4396	0.15	0.00	656
Farmer	0.26	0.00	4396	0.37	0.00	656
Skilled Blue-Collar	0.29	0.00	4396	0.23	0.00	656
Unskilled	0.14	0.00	4396	0.08	0.00	656
iii. Demographics						
Immigrant	0.11	0.00	8409	0.05	0.00	935
White	0.93	1.00	8409	0.96	1.00	935
Father Immigrant	0.18	0.00	3963	0.11	0.00	596
iv. Own Occupation Prior to Civil Service						
Professional	0.11	0.00	3447	0.24	0.00	176
White-Collar Non-Prof	0.34	0.00	3447	0.23	0.00	176
Farmer	0.12	0.00	3447	0.21	0.00	176
Skilled Blue-Collar	0.23	0.00	3447	0.18	0.00	176
Unskilled	0.14	0.00	3447	0.09	0.00	176
iv. Connections						
Father Gov. Employee	0.06	0.00	4396	0.05	0.00	656
Grew Up in DC	0.27	0.00	5173	0.06	0.00	756
Same Surname as Congressman	0.01	0.00	8409	0.01	0.00	935
Incumbent Party	0.51	1.00	6051	0.53	1.00	875

Notes: This table shows summary statistics for employees appointed without the use of exams (Columns 1 to 3) and those appointed using exams (Columns 4 to 6). Employees appointed without the use of exams include pre-reform observations of workers in positions that became subject to exams post 1883. See footnotes to the tables in the main body of the paper for a definition of each of the variables included in this table.

Table B2: Pre- and Post-Reform Trends in Main Outcome Variables

Outcome	Pre-1883 Mean (1)	Pre-1883 p-value (2)	Post-1883 Mean (3)	Post-1883 p-value (4)
i. Family Background				
Summary Index	0.021	0.329	0.168	0.000
Parental Wealth Rank	0.002	0.395	0.048	0.009
Father Professional	-0.017	0.182	0.048	0.008
Immigrant Parents	0.003	0.443	-0.094	0.004
Immigrant	0.003	0.417	-0.046	0.005
ii. Own Occupation				
Professional	-0.016	0.925	0.084	0.070

Notes: Each row corresponds to a different outcome variable. Column 1 reports the mean value of the pre-reform coefficients based on estimating equation 2. Column 3 reports the analogous figure for the post-reform coefficients. Column 2 reports the p-value corresponding to the hypothesis that all the pre-reform coefficients are equal to zero. Column 4 reports the analogous p-value for the hypothesis that all the post-reform coefficients are equal to zero. Standard errors clustered at the position level.

Table B3: Sample Restricted to Employees with non-Missing Parental Wealth

(a) Parental Occupations

	Professional		White-Collar Non-Prof		Farmer		Skilled Blue Collar		Unskilled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exam	0.0932***	0.0898**	0.00415	0.0116	0.0337	0.00303	-0.0823**	-0.0647	-0.0473*	-0.0359
	(0.0345)	(0.0369)	(0.0464)	(0.0439)	(0.0339)	(0.0345)	(0.0409)	(0.0415)	(0.0245)	(0.0243)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3034	3034	3034	3034	3034	3034	3034	3034	3034	3034
Mean of dep. var.	0.101	0.101	0.206	0.206	0.238	0.238	0.274	0.274	0.135	0.135

(b) Employees' Country of Origin and Race

	Immigrant		Immigrant Parents		White	
	(1)	(2)	(3)	(4)	(5)	(6)
Exam	0.0154	0.0156	-0.0866***	-0.0828***	-0.00570	-0.00805
	(0.0141)	(0.0129)	(0.0321)	(0.0320)	(0.0128)	(0.0130)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes
Observations	3034	3034	2944	2944	3034	3034
Mean of dep. var.	0.0296	0.0296	0.177	0.177	0.954	0.954

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The sample is restricted to employees for whom we also observe parental wealth. In panel (a), the dependent variable in each of the columns is an indicator that is one if the father of a bureaucrat worked in a certain occupational category. When bureaucrats are linked to more than one census with information on their father's occupation, we use the fraction of census years that their father spent in a given occupation as our outcome variable. In panel (b), the dependent variable in columns 1 and 2 is an indicator that is one if the worker is foreign born. The dependent variable in columns 3 and 4 is an indicator that is one if both workers' parents are foreign born. The dependent variable in columns 5 and 6 is an indicator that is one if the workers is listed as being white in the census. All columns include hiring year and position fixed effects. The odd columns further include workers' home-state fixed effects. Standard errors clustered at the position level.

TABLE B4: EFFECTS OF THE REFORM ON PARENTAL OCCUPATIONS: ANY BLUE-COLLAR OCCUPATION

	Professional		White-Collar Non-Prof		Farmer		Any Blue Collar	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exam	0.0530**	0.0488**	-0.0119	-0.00378	0.0606***	0.0258	-0.104***	-0.0698*
	(0.0239)	(0.0241)	(0.0347)	(0.0325)	(0.0234)	(0.0244)	(0.0383)	(0.0359)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4993	4993	4993	4993	4993	4993	4993	4993
Mean of dep. var.	0.0977	0.0977	0.187	0.187	0.270	0.270	0.411	0.411

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is an indicator that is one if the father of a bureaucrat worked in a certain occupational category. When bureaucrats are linked to more than one census with information on their father's occupation, we use the fraction of census years that their father spent in a given occupational category as our outcome variable. All columns include hiring year and position fixed effects. The odd columns further include workers' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE B5: EFFECTS OF THE REFORM ON EMPLOYEES' PARENTAL LITERACY

	Father Literate		Mother Literate	
	(1)	(2)	(3)	(4)
Exam	0.0256**	0.0262**	0.000229	0.00199
	(0.0107)	(0.0112)	(0.0141)	(0.0142)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes
Observations	4993	4993	5369	5369
Mean of dep. var.	0.935	0.935	0.907	0.907

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is an indicator that is one if an employees' father (mother) was literate. All columns include hiring year and position fixed effects, even columns further include fixed effects based on employees' home state. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE B6: EFFECTS OF THE REFORM ON THE SHARE OF FOREIGN-BORN EMPLOYEES

	All Immigrants		Non-English-Speaking Immigrant		English-Speaking Immigrant	
	(1)	(2)	(3)	(4)	(5)	(6)
Exam	-0.0473**	-0.0419**	-0.0212*	-0.0202*	-0.0260**	-0.0217*
	(0.0190)	(0.0176)	(0.0115)	(0.0113)	(0.0129)	(0.0118)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes	No	Yes
Observations	9238	9238	9238	9238	9238	9238
Mean of dep. var.	0.108	0.108	0.0431	0.0431	0.0651	0.0651

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 and 2 is an indicator that is one if an employee is foreign. Columns 3 to 6 further split foreign-born individuals based on whether they are from a non-English-speaking or an English-speaking country. All columns include hiring year and position fixed effects, even columns also include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE B7: HETEROGENEITY BY TYPE OF POSITION

(A) CLERICAL VERSUS TECHNICAL

	Summary Index		First Principal Component	
	(1)	(2)	(3)	(4)
Exam	0.169***		0.270***	
	(0.0468)		(0.0856)	
Clerical Exam		0.171***		0.294***
		(0.0595)		(0.101)
Technical Exam		0.167***		0.235*
		(0.0601)		(0.125)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	Yes	Yes	Yes	Yes
Observations	2944	2944	2944	2944
Mean of dep. var.	0.128	0.128	0.0514	0.0514

(B) HIGH VERSUS LOW PAY

	Summary Index		First Principal Component	
	(1)	(2)	(3)	(4)
Exam	0.169***		0.270***	
	(0.0468)		(0.0856)	
Exam X Below Median Pay		0.133**		0.239***
		(0.0565)		(0.0869)
Exam X Above Median Pay		0.194***		0.292**
		(0.0567)		(0.114)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	Yes	Yes	Yes	Yes
Observations	2944	2944	2944	2944
Mean of dep. var.	0.128	0.128	0.0514	0.0514

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' socioeconomic background computed using the approach in Kling et al. (2007). The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. $Exam$ is the coefficient corresponding to our baseline specification. The variable $Clerical\,Exam$ in panel (a) takes a value of one for employees hired through exams as clerks or copyists. $Technical\,Exam$ takes a value of one for employees hired through exams in technical positions. The variable $Exam \times Below\,Median\,Pay$ in panel (b) is an indicator that is one for employees appointed through exams in below-median pay positions. $Exam \times Above\,Median\,Pay$ is similarly defined but for employees in above-median pay positions. All columns include hiring year and position fixed effects, even columns further include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE B8: EFFECTS BY DEMOCRAT VS REPUBLICAN PRESIDENCY

	(1)	(2)	(3)	(4)
Exam	0.180***		0.169***	
	(0.0451)		(0.0468)	
Exam X Democrat Presidency		0.209***		0.202***
		(0.0511)		(0.0533)
Exam X Republican Presidency		0.152**		0.138**
		(0.0591)		(0.0625)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	No	No	Yes	Yes
Observations	2944	2944	2944	2944
Mean of dep. var.	0.128	0.128	0.128	0.128

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' socioeconomic background computed using the approach in Kling et al. (2007). The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. $Exam$ is the coefficient corresponding to our baseline specification. $Democrat\ Presidency$ takes a value of one during the post-reform yeas in which the President was a democrat (1885 to 1889), whereas $Republican\ Presidency$ takes a value of one when the President was a Republican (1889 to 1893). The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

Table B9: Adjusting for Childhood Location

	(1)	(2)	(3)	(4)
Exam	0.169***	0.166***	0.163***	0.158***
	(0.0468)	(0.0406)	(0.0440)	(0.0457)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	Yes	Yes	Yes	Yes
Birth State/Country FE	No	Yes	No	No
Childhood State FE	No	No	Yes	No
Childhood State X Rural FE	No	No	No	Yes
Observations	2944	2944	2944	2944
Mean of dep. var.	0.128	0.128	0.128	0.128

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' socioeconomic background computed using the approach in Kling et al. (2007). The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. The table shows the sensitivity of the results to adding various location fixed effects based on bureaucrats' childhood location. When we observe individuals in more than one childhood location, we use the first location. All columns include hiring year, employees' home state, and position fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

Table B10: Robustness to Estimating a Before and After Specification

	Diff-in-Diff		Before-After	
	(1)	(2)	(3)	(4)
Exam	0.180***	0.169***		
	(0.0451)	(0.0468)		
After			0.140***	0.152***
			(0.0452)	(0.0436)
Year FE	Yes	Yes	No	No
Position FE	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes
Observations	2944	2944	1426	1426
Mean of dep. var.	0.128	0.128	0.236	0.236

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' socioeconomic background computed using the approach in Kling et al. (2007). The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. Columns 1 and 2 report results using our baseline specification. In columns 3 and 4 we instead report results from an specification in which we restrict the sample to employees in "treated" positions and simply compare their socioeconomic backgrounds before and after the reform. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE B11: ADJUSTING FOR MISSING DATA ON MIGRANTS' BACKGROUNDS

	(1) Baseline	(2) Percentile 10	(3) Median	(4) Percentile 90
Exam	0.169***	0.186***	0.138***	0.0987**
	(0.0468)	(0.0445)	(0.0412)	(0.0440)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	No	Yes	Yes	Yes
Observations	2944	3302	3302	3302
Mean of dep. var.	0.128	0.0686	0.136	0.183

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' socioeconomic background computed using the approach in Kling et al. (2007). The variables composing the index are normalized such that a higher value corresponds to a higher socioeconomic status. In columns 2 to 4, we expand our baseline sample by imputing a value of the summary index to foreign-born employees whom we do not observe as children in the US census. In column 2, we impute them a value equal to the 10th percentile of the summary index (computed among all employees with the same occupation in the pre-reform period), in column 3 we impute them the 50th percentile, and in column 4 we impute them the 90th percentile. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.

TABLE B12: SHARE OF EMPLOYEES FROM URBAN AREAS

	(1)	(2)	(3)	(4)
Exam	-0.151***	-0.0936***		
	(0.0300)	(0.0247)		
Clerical Exam			-0.209***	-0.138***
			(0.0345)	(0.0313)
Technical Exam			-0.0629*	-0.0284
			(0.0363)	(0.0347)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
Home State FE	No	Yes	No	Yes
Observations	4993	4993	4993	4993
Mean of dep. var.	0.441	0.441	0.441	0.441

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is an indicator that is one for employees who lived in urban areas prior to joining the civil service. If we link an individual to multiple censuses, we use the fraction of years in which they lived in an urban area as the dependent variable. $Exam$ is the coefficient corresponding to our baseline specification. $Clerical\ Exam$ is an indicator that is one for employees hired as clerks or copyists in the post-reform period. $Technical\ Exam$ is similarly defined but for employees hired in technical positions. All columns include hiring year and position fixed effects, even columns further include employees' home-state fixed effects. The sample is restricted to newly hired employees in the Executive Departments in DC. Standard errors clustered at the position level.