Who Benefits from Meritocracy?*

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PRELIMINARY DRAFT

Abstract

Does screening applicants using exams hurt or help the chances of candidates from disadvantaged backgrounds? Although a common critique to exams is that they might negatively impact the chances of applicants from poorer backgrounds, exams might replace other more discretionary criteria in which such applicants are at an even worse disadvantage. We answer this question using evidence from the 1883 Pendleton Act, which introduced competitive exams for selecting some employees in the US federal government. While the reform increased the representation of "educated outsiders" (individuals with high education but limited connections), it reduced the share of individuals from disadvantaged backgrounds. This decline was driven by an increased representation of the middle class, with limited change in the representation of applicants from upper-class backgrounds. The drop in the representation of workers from poorer backgrounds was stronger among applicants from states with more unequal access to schooling.

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1 Introduction

Screening applicants based on their performance in an exam is common in many contexts, ranging from college admissions to the recruitment of civil servants. Although using exams could facilitate the selection of more qualified individuals, a common critique to this approach is that it might have a disproportionately negative impact on the chances of applicants from disadvantaged backgrounds.[1] Indeed, there has been a recent push to limit the influence of exams in different selection processes, often fueled by concerns that their use could conflict with the goal of achieving a diverse pool of recruits.[2]

At the same time, exams might replace other selection criteria in which applicants from poorer backgrounds are at an even worse disadvantage. For instance, using exams might limit the importance of personal connections, or might reduce the influence of (potentially biased) subjective assessments. Moreover, exams could have different equity implications in the short- and in the long-run, as applicants from different social backgrounds might differ in their ability to "game" a new system (Campbell, 1979).[3] Ultimately, whether exams hurt or help the chances of applicants from less privileged backgrounds is an empirical question, and one whose answer depends on how exams fare relative to other more discretionary selection criteria.

Answering this question, however, faces three key empirical challenges. First, to understand whether exams hurt or help the chances of applicants from disadvantaged backgrounds, one needs to compare the representation of such applicants when selection is through exams to their representation under alternative selection criteria. Doing so is challenging because, for a given recruiting institution and position, applicants are usually all screened through the same procedure. Second, to the extent that the effects of exams might be different in the short- and in the long-run, obtaining a comprehensive picture of their impacts also requires a long-term perspective. However, such perspective is not possible when focusing on recent policy changes.[4] Finally, the analysis requires hard-to-obtain intergenerational information so as to observe applicants' socioeconomic backgrounds.

This paper investigates whether screening applicants using exams hurts or helps the chances of candidates from less privileged backgrounds. To do so, we use evidence from one of the earliest attempts to implement "meritocratic" ideals in US history: the introduction, after the passing of

[1]See Autor & Scarborough (2008); Hoffman *et al.* (2018); Estrada (2019); Muñoz & Prem (2020); Moreira & Pérez (2020) for the effects of reduced hiring discretion on recruits' qualifications.

[2]The use of exams has been recently questioned in a number of contexts, ranging from the recruitment of police officers (see CBS, 2020) to high school and college admissions (see for instance New York Post, 2018 and New York Times, 2021). An earlier example is the discontinuation (after a lawsuit arguing that the exam discriminated against minority applicants) of the *Professional and Administrative Career Examination* during Carter's administration.

[3]For instance, a common critique to the SAT is that applicants from wealthy backgrounds have become particularly efficient at "gaming" the exam through the use of expensive tutoring and medical exemptions, see for example CNBC, 2019.

[4]For example, a number of US colleges have recently dropped the SAT requirement. While an evaluation of the short-run impacts of this change has not (to the best of our knowledge) yet been conducted, it is in principle possible. However, such evaluation would not answer the question of how dropping the SAT would affect admitted students' backgrounds in the longer run (as students and their families adjust to the new system).

the 1883 Pendleton Act, of competitive exams for the selection of some federal employees. Before the passing of this law, government jobs were allocated at the discretion of government officials and often based on political and personal connections (Aron, 1987). After its passing, in contrast, some positions had to be allocated to those who obtained the top scores in an open exam. We find that this change reduced the representation of applicants from disadvantaged family backgrounds in government jobs, 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.

Beyond being an appealing empirical setting for investigating the link between exams and inequality, there are two reasons why studying how exams affected the social background of government employees is in itself particularly important. First, several studies show that *who* is in office, both at lower and higher levels of the state hierarchy, matters for the types of policies that are selected and the effectiveness of their implementation (Keiser *et al.*, 2002; Pande, 2003; Chattopadhyay & Duflo, 2004; Beaman *et al.*, 2012; Riccucci *et al.*, 2014; Xu, 2020). Hence, it is important to understand the degree to which exams –a widely used recruitment tool across bureaucracies in the world– generate a "representative bureaucracy" (Kingsley, 1944). Second, government jobs have historically been avenues of upward mobility for underrepresented groups (King *et al.*, 1995). Thus, it is crucial to assess the extent to which exam-based recruitment facilitates or impedes the access of these groups.[5]

To conduct the analysis, we have assembled a new dataset with information on the social and economic backgrounds of government employees. First, we digitized US federal government personnel records spanning 1871 to 1893. These records include employees' names, annual salary, job title and office. Second, we linked these data to US population censuses, using name-based matching techniques (Abramitzky *et al.*, 2019). These data enable us to observe the family backgrounds (including parental wealth and occupations) of bureaucrats who were appointed before and up to 10 years after the passing of the reform.

Our identification strategy exploits the fact that not all federal positions were initially subject to exams. Specifically, among positions in the Executive Departments in DC, the reform exempted those at the bottom (such as laborers) and those at the top (such as bureau chiefs) of the state hierarchy. We use this feature of the reform to estimate a difference-in-differences model, comparing the characteristics of employees hired before and after the reform, in exempted and non-exempted positions. In other words, we ask if individuals hired to do the *same job* in the *same office* were of a different social background when hired through exams rather than through patronage.

We find that the reform led to an immediate decline in the share of employees from disadvantaged backgrounds, which then persisted for at least 10 years after the reform's implementation. First, employees hired through exams came from families that were 6.5 percentile ranks higher in the national wealth distribution. This increase was driven by a reduced representation of ap-

[5]For instance, minorities in the US are overpresented among public employees (Laird, 2017). Of course, this overrepresentation does not imply that discrimination does not exist or that it has not existed within the public sector (see for instance Aneja & Xu (2020) for an example of discrimination against Black federal workers in the early 20th century).

plicants with parents at the bottom of the distribution, together with an increase in the share of workers from middle- and upper-middle class families. Second, the reform increased the share of employees with higher-status parental occupations: we find a 5 percentage points increase in the proportion of children of professionals (a nearly 50% increase), together with a similar decrease in the proportion of children of blue-collar workers. Finally, the reform led to a 4 percentage points decline in the share of foreign-born employees, as well as a 7 percentage points decline in the share of employees with foreign-born parents. Interestingly, this increased elitism occurred despite the exam was based on content that should have in principle been accessible for applicants with only a "common school" education.[6]

Why did the reform improve the representation of the middle class (at the expense of applicants from disadvantaged backgrounds)? Our interpretation is that, by increasing the relative weight of formal education in hiring decisions, the reform helped "educated outsiders": individuals with high levels of education but low levels of connections. As middle-class applicants were overrepresented in this group, the reform increased their chances of obtaining a government job. We present several pieces of evidence that support this interpretation.

First, we show that employees hired through exams were likely better educated than those appointed through patronage: they were more likely to have held a professional occupation –such as lawyers and accountants– prior to joining government than patronage employees, and they were also more likely to have spent their childhoods in counties with higher per capita schools.[7] Second, exam-based hires were also more likely to lack the personal and political connections that would have facilitated access to patronage jobs: they were less likely to have a father who was himself a bureaucrat, less likely to have grown up in DC, and less likely to come from a county in which a majority of voters supported the incumbent party (suggesting a decline in political favoritism). Third, we show that exams had the most negative effects on the chances of applicants from disadvantaged backgrounds when applicants came from states with high levels of inequality in access to schooling– namely the places in which the children of the poor were the least represented among "educated outsiders". Overall, these results suggest that the reform indeed increased the representation of "educated outsiders" in government positions.

Next, we document that middle-class individuals were likely overrepresented among the "educated outsiders". First, although middle-class applicants were more educated than those from poorer backgrounds, in the pre-reform period they represented a similarly small fraction of workers in the federal positions that would become subject to exams. This similar representation is consistent with the idea that, in the pre-reform period, the poor may have compensated for their relatively lower education by being more likely to be engaged in patronage politics –presumably because their worse outside options made them an easier target for political machines. Second, we

[6]"Common school" is the name that was used to refer to public elementary schools in 19th century US. Indeed, people with only a "common school" education regularly took and passed the exam. We provide further details about the content of the exams in Section 2.

[7]Since population censuses prior to 1940 do not include information on years of schooling, we cannot directly investigate if employees hired through exams had completed more years of education.

show that, after the reform, the representation of middle-class individuals in government positions became closer to their representation in comparable *private sector* white-collar jobs. This convergence suggests that the relatively low representation of the middle class in government positions prior to the reform was unusual relative to their level of education.

Finally, a potential complementary explanation is that exam-based recruitment might have changed the public's perception of the prestige of holding a government job, thus changing the pool of individuals interested in such positions. Although this effect is inherent to any move towards "meritocracy" (and the combined effect of changes in screening and changes in the applicant pool is still policy-relevant), we find evidence that such change in preferences is unlikely to drive of our findings: we observe a rapid change in bureaucrats' social backgrounds, which seems inconsistent with plausibly slower to change perceptions about the prestige of public employment.

Our study contributes to three main areas of the literature. First, we contribute to the literature on the use of exams in the workplace, which has to date focused on whether exam-based hiring enables the selection of more productive individuals (Hoffman *et al.*, 2018; Estrada, 2019). In contrast, we study exams' implications for workers' social origins. We do so in an important context: the public sector is the largest employer in many countries, and civil service exams as the ones we study are a common recruitment tool.[8] Closest to our paper is Autor & Scarborough (2008), which shows that the introduction of job testing did not hurt the chances of minority applicants in a retail firm. We deviate from this study in three main ways. First, the long-run nature of our data enable us to measure both the immediate and the longer-term consequences of exams. Doing so is particularly important in our context as, unlike Autor & Scarborough (2008), we study a change implemented by a large and prominent employer (which could presumably have led to institutional and market responses in the long-run). Second, we are able to characterize employees' social backgrounds beyond their minority status. While we also find limited evidence that the reform changed employees' racial mix, we show that it nevertheless led to a more elitist civil service. Finally, we focus on a context (the public sector) in which diversity is *per se* especially important.

Our second main contribution is to the literature investigating the "social origins" of government employees (see for instance Bourdieu (1998), Dal Bó *et al.* (2017) and Thompson *et al.* (2019)). This literature has focused on documenting the extent to which the social composition of civil servants and political leaders corresponds to that of the general population. By contrast, we investigate how such social origins *change* with the method used to select government employees. Our findings suggest that civil service exams are not simply a mechanism for legitimizing the status quo (as argued by scholars such as Bourdieu (1998)), but rather that they could be consequential for bureaucrats' social origins. The notion that exams are consequential is consistent with the evidence in Bai & Jia (2016), who find that the *abolition* of civil service exams in China led to social unrest –presumably because commoners who had hoped to gain access to the elite through exams were prevented from doing so.[9]

[8] Nearly 80% of countries use formal examinations to select some of their public employees (Teorell *et al.*, 2011).

[9] Bai & Jia (2016) study the effect of the abolition of civil service exams on social unrest in China. The authors discuss several hypotheses that may explain why such abolition led to greater revolutionary participation. One interpretation

Finally, we contribute to the literature on civil service reforms. These studies have focused on understanding the political economy and electoral consequences of these reforms (Theriault, 2003; Bostashvili & Ujhelyi, 2019; Folke *et al.*, 2011), as well as their effects on bureaucratic performance (Rauch *et al.*, 1995; Ornaghi, 2016; Xu, 2018; Moreira & Pérez, 2021).[10] Instead, we focus on the implications of these reforms for bureaucrats' social origins. This is an important margin, as achieving a "representative bureaucracy" is an explicit goal in many countries.[11] Our findings suggest that an unintended consequence of these reforms might be to generate a more elitist civil service.[12]

2 Historical Background

2.1 Spoils System and the Civil Service Reform Movement

Prior to the Pendleton Act, hiring decisions in the federal civil service were ruled by the "spoils system". Under this system, appointment to office was based primarily on political and personal connections rather than on "fitness for office" (Ziparo, 2017). As described by Aron (1987), "who an applicant knew counted at least as much as the skills he or she could demonstrate".

While pressure for the adoption of a merit reform had been mounting since the 1860s, the exact timing of the passing of the Pendleton act is related to two political events. First, in July of 1881, newly elected president James A. Garfield was shot by a disappointed office seeker (Garfield would die by September). This assassination put civil service reform at the center of the political stage, and provided reformists with a powerful example of the negative consequences of the spoils system. Soon after the assassination, in December of 1881, Democratic senator George H. Pendleton introduced a bill with the aim of reforming the civil service. Second, Democrats took control of the House in March of 1882. Fearing that they would lose the 1884 presidential election, Republicans supported the civil service reform bill with the aim of protecting Republican office-holders

is that the abolition of exams led to dissatisfaction among the commoners who had hoped to join the political elite through the exam. A crucial difference between our setting and the one in Bai & Jia (2016) is that, in our context (a competitive democracy) the "common person" could have in principle accessed government positions both through political patronage or through exams. In contrast, in a non-democratic country such as early 20th century China, the abolition of the exam may have eliminated *the only* route the poor had to access government jobs.

[10] In a recent paper, we investigate the consequences of the Pendleton act for the functioning of the US Customs Service (Moreira & Pérez, 2021). We deviate from Moreira & Pérez (2021) in terms of research question, data and empirical strategy. First, while Moreira & Pérez (2021) studies the consequences of the reform for the *efficiency* of the US Customs Service, we focus on how the reform affected the social origins of civil servants 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' parental wealth and occupations by linking these records to population censuses. Finally, our analysis exploits variation in exam requirements across *positions*, whereas Moreira & Pérez (2021) exploits variation in requirements across different customs-collection districts.

[11] For instance, the "First Merit Principle" of the US Merit Systems Protection Board is that "Recruitment should be from qualified individuals from appropriate sources in *an endeavor to achieve a work force from all segments of society*."

[12] We also contribute to the literature on the Pendleton Act, a landmark legislation in US history (Theriault, 2003; Johnson & Libecap, 1994a,b; Libecap & Johnson, 2007; Moreira & Pérez, 2021). There is also a substantial historical literature on the Pendleton act and the civil service reform movement, see for instance Hoogenboom (1968) and chapter 12 in White (2017).

from politically motivated dismissals (Hoogenboom, 1959). In January of 1883, President Chester A. Arthur signed the Pendleton Civil Service Reform Act into law.

2.2 The Pendleton Act

Positions Subject to Exam. The act's main provision was to establish that employees in certain "classified" positions within the Executive branch of government would need to be selected through open, competitive and anonymously graded exams (United States Civil Service Commission, 1883).[13] The act divided the classified (that is, subject to exams) civil service into three branches: the "classified departmental service" for employees in the executive departments in DC, the "classified Customs Service" for Customs Service employees, and the "classified Postal Service" for postal workers.

The classified departmental service in DC –our main focus in this paper– was initially restricted to employees who: (1) were in clerical or technical positions, and (2) received annual salaries of no less than $900 and no more than $1800. In addition to exempting clerical workers with very low or very high salaries, the law also exempted workers in hierarchical positions (bureau chiefs, elected officers, employees requiring Senate's confirmation) and those employed "merely as laborers or workmen". Hence, in essence, the law targeted the "middle" of the state hierarchy while exempting both the bottom and the top. Finally, the customs and postal classified services were restricted to customs-collection districts and post-offices with at least 50 employees, and to employees making no less than $900 within these offices.

Although the act affected only 10% of the civilian labor force initially, it authorized the president to include additional positions through executive order (United States Civil Service Commission, 1883). In our period of analysis (up to 1893), there were two changes affecting the classified departmental service in DC. First, in 1885 the lower salary limit for clerical workers was decreased from $900 to $720 and the upper limit of $1800 was removed (Commission, 1885). Second, in 1888 the lower salary limit was eliminated. These two changes, however, had very limited effects on the coverage of the classified departmental service in DC: By 1883, 90% of the clerical positions that would be covered by the reform paid between $900 and $1800, and only 5% paid less than $720.[14] Hence, in practice, our analysis compares workers in positions that were subject to exam at some point from 1883 to 1893 (that is, clerical and technical workers) to workers in positions that were not (that is, hierarchical workers and laborers/workmen), before and after 1883.[15]

Figure 1 shows the total number of workers in the Executive Departments in DC, as well as the share who worked in positions that would be covered by exams after 1883.[16] Here, we define a

[13] Employees in the Legislative and Judicial branches of government were all exempted from exams.

[14] Most (about 80%) workers in classified positions were employed as "clerks".

[15] Less than 2% of employees in positions that would be covered by exams made more than $1,800. Out of those employees in classified positions paying less than $900 a year, half corresponded to assistant printers in the Bureau of Engraving and Printing. Since this low-paid position was not apportioned among the States, we do not have information on the names of the employees appointed to this job.

[16] The Executive Departments in this period were Agriculture, Interior, Justice, Navy, Post Office, State, Treasury and War.

position as "covered by exams" if it became part of the classified service at any point between 1883 and 1893. The total number of employees grew in the decade prior to the Pendleton Act, reflecting the expansion of government functions (Libecap & Johnson, 2007).[17] Note that, since our analysis includes office fixed effects, offices created after the reform do not contribute to the identification of the effects of exams.[18] Growth in the number of employees seems to level off during the 1880s, both in exempted and non-exempted positions (as reflected by the stability of the share of workers in exempted positions, which fluctuated around 60% throughout the period).[19]

Additional Provisions of the Law. In addition to introducing exams, the law established that positions in the classified departmental service in DC would need to be "apportioned" among states according to their population. An important consequence of this rule is that applicants to the classified departmental service were in practice mostly competing against other applicants from their own state of residence. In the empirical analysis, we sometimes include fixed effects corresponding to bureaucrats' state of residence at the time of their appointment. Including these fixed effects enables us to shut down the effects of the reform that stem from apportionment-induced changes in employees' regional origins.[20]

Although it changed the method used to fill certain federal positions, it is important to note that the act *did not* grant tenure to employees in these positions: "classified" workers remained open to the possibility of removal as administrations changed (Johnson & Libecap, 1994a).[21] Later reforms (in particular, the 1912 Lloyd-La Follette Act) further increased the stability of federal employment by introducing the notion that employees could only be removed for "just causes" (Johnson & Libecap, 1994a).

Exam Characteristics. The law established that exams had to focus on practical knowledge relevant to an applicant's future position rather than on formal academic training.[22] Applicants to the positions of copyist or clerk (the most common occupations within the classified service) were required to complete exams in four subjects: orthography, copying, penmanship and arithmetic.[23] These subjects corresponded to the typical curricula taught in "common schools" at the

[17]The jump from 1881 to 1883 corresponds to a large expansion of the Pension Office in the Interior Department, which added nearly 800 employees, and the Medical Department within the War Department, which added nearly 300 employees.

[18]For instance, the Bureau of Labor Statistics –which later on became the Department of Labor– within the Interior Department was created in 1884.

[19]The increase in the total number of positions in 1891 corresponds primarily to the addition of 2,500 workers in the Census office within the Department of the Interior. These workers were hired temporarily to tabulate the 1890 census and were exempted from exams. Our results are robust to excluding these workers from the control group.

[20]Apportionment of offices in DC was in theory established in 1865, but actual practice did not follow apportionment rules prior to the Pendleton act (Hoogenboom, 1968).

[21]"The power to remove for even the most partisan and selfish reasons remains unchanged" (United States Civil Service Commission, 1883). The only exception is that employees could no longer be removed for refusing to perform a political service or paying a political assessment, although this provision applied to all positions (rather than to just the "classified" ones).

[22]The typical duties of a clerk would entail "routine, repetitive tasks", often involving recording and copying (Aron, 1987). Examples of such tasks include "note signing" (for clerks in the Treasury Department), or "writing and recording patents" for clerks in the General Land Office within the Interior Department.

[23]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 US history.

time, namely the "three Rs" of **r**eading, w**r**iting and a**r**ithmetic.[24] Applicants to positions requiring technical, professional or scientific knowledge were further required to take "supplementary" or "special exams". Examples of such specialized exams include the "meteorological clerk" exam in the Department of Agriculture, or the "medical examiner" exam in the Department of the Interior's Pension Office. Panels (a) to (d) in Figure A3 show one example question for each of the subjects (orthography, copying, penmanship and arithmetic) that were required for applicants to the positions of clerk or copyist. Panel (e) shows an example question for applicants to the position of "meteorological clerk". Such sample questions were available from the annual reports of the Civil Service Commission.[25]

The emphasis on practical skills differs from other civil service exams. For instance, Grindle (2012) argues that a reform mandating exam-based recruitment in mid 19th-century England did not lead to changes in bureaucrats' social origins since exams were designed such that their contents would only be accessible to those with "elite educations at Oxford and Cambridge".[26] In contrast, the US Civil Service Commission maintained that "a common school education was sufficient to pass examination" (Hoogenboom, 1959).[27] Indeed, applicants with only a "common school" education regularly took (and passed) the exams. Figure A4 shows the number of applicants of different educational backgrounds, as well as the share who obtained a passing grade: applicants with a common school education were the largest group of applicants, and about 55% of them actually passed the exam.[28]

Appointing Procedure. Applicants who obtained a passing grade were added to a register of eligible candidates.[29] On the opening of a vacancy, the Commission produced a list of the *top four* candidates (on the basis of exam results), from which the head of the Department filling the vacancy would need to choose –thus drastically reducing hiring discretion.[30] For positions in the classified departmental service (which were subject to state apportionment rules), the top four names had to correspond to individuals from states with the "strongest claim" on the basis of apportionment.[31] For positions in the Customs and Postal services (where apportionment was not

[24] "Common schools" is the name that was used to refer to public elementary schools in 18th and 19th century US (Kaestle, 1983).

[25] Over time, these questions also became available from non-governmental publications. 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).

[26] For instance, the UK exams would include sections on Latin and Greek. A similar example in which the design of the exam made it unlikely that individuals from poorer backgrounds would pass it is the exam for civil servants in India during the British Empire. For instance, the exam was initially only held in London, thus limiting the ability of Indian applicants to participate. Moreover, the exam contents favored classic British liberal education rather than practical knowledged (Barua, 2003).

[27] Another important difference with reforms in Europe is the focus on mid- rather on high-level offices.

[28] "Common schools" is the name that was used to refer to public elementary schools in 19th century US. Unfortunately, we do not have information on the educational background of those who were actually appointed.

[29] Applicants who failed the exam were prevented from retaking it for a period of six months unless they obtained an special authorization from the Civil Service Commission (Commission, 1885).

[30] This number was further reduced to three in March of 1888 (Commission, 1886, p.128).

[31] The Civil Service Commission kept track of the number of employees appointed by each state. If there were no certified candidates from the top priority state, the Commission would produce a list of candidates from the state that was next in the order of apportionment (until filling the vacancy).

required), the list of top applicants would need to correspond to the top four candidates regardless of their state of residence. Hence, while barely passing the exam made an applicant "eligible" for a government job, the higher the score the higher the chances of ultimately obtaining a position. An important deviation from meritocratic principles is that appointing officers could ask for an employee of a specific gender (for instance, a "male clerk").[32] Indeed, nearly 85% of the employees hired through exams in the 1883-1893 period were male.

The law also required for exams to be held throughout the country: Figure A6 shows the location of all exams that took place from 1886 to 1893, with each circle drawn in proportion to the number of exams happening in each location.[33] For instance, there were a total of 286 exams held between June 1892 and June 1893, with at least one exam happening in each US state (United States Civil Service Commission, 1893, p.141, Table 1). Panel (a) in Figure A5 shows the yearly number of applicants to the classified departmental service. From 1883 to 1893, nearly 150,000 individuals completed an exam to join any branch of the classified civil service, out of which 30,000 applied to join the classified departmental service in DC. Panel (b) shows that the fraction of applicants to the departmental service who obtained a passing grade was fairly stable over our sample period, hovering around 65%. Finally, Panel (c) shows the proportion of applicants with a passing grade who were ultimately appointed. The first employee to be appointed to the classified departmental service did so in August of 1883. By 1893, 23% of those who had applied to the classified departmental service and obtained a passing grade had received an appointment.[34]

Expected Effects of the Reform. It is unclear whether such a reform would facilitate or impede the access of individuals from disadvantaged backgrounds to government jobs. On the one hand, applicants connected to influential individuals were more likely to secure positions under the patronage system. The historical literature emphasizes the importance of these personal connections in determining the likely success of an application. For instance, Ziparo (2017)'s analysis of application files finds that, among women who were successful applicants to federal jobs in the 1860s, 71% had been personally recommended by a member of Congress.[35] Similarly, Aron (1987) describes a number of cases where employees secured their position through a personal or family connection with a member of Congress.[36] To the extent that individuals from disadvantaged backgrounds were less likely to have these social connections, the reform could have facilitated their access to government jobs.

Moreover, access to a "common school" education was relatively widespread in mid 19th cen-

[32]This rule was in place until after World War I (Van Riper, 1976).

[33]Figure A7 shows an example calendar of examinations for the period spanning January 1886 to June of 1887.

[34]These figures imply that an average of about 14,000 applicants took an exam in each of the first eleven years after the reform (about 28 out of 100,000 people based on the US population in 1880). Of those who applied to the classified departmental service, 15% ended up receiving an appointment (23% of 65%). As a comparison, the Indian Civil Service exam is completed by about one million applicants every year (75 applicants every 100,000 people). Of these, only about 1,000 are appointed yearly (0.1%).

[35]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)

[36]For instance, she describes the case of Austine Snead, a would-be clerk in the Treasury Department who received assistance from Senator Guthrie, a friend of her mother.

tury US, at least for white children. Figure B7 shows school attendance rates by age and parental wealth quintile for white children aged 6 to 18 in the 1870 population census. By 1870, close to 80% of white children aged 10 years old attended school. Although children with wealthier parents were more likely to attend school, attendance rates for children age 10 were above 60% even among those with parents in the bottom two quintiles of the wealth distribution.[37] Indeed, among individuals age 20 to 50 in the 1880 census (the last pre-reform census), the literacy rate was above 90% (see Figure B9).

On the other hand, Libecap & Johnson (2007) emphasize how patronage was "viewed as a means of democratizing the government" since "anyone with the right political connections could obtain a government job, at least for a short while."[38] According to this view, in the context of a competitive party system as 19th-century US, patronage offered the "common person" a route to a government job. In contrast, a system based on competitive exams faced the risk of creating a "monopoly of office holding on the part of a particular class" (Commission, 1884, p. 49).[39] Consistent with this view, Hoogenboom (1959) argues that "the merit system recruited persons of a higher social status".[40]

3 Data

3.1 Federal Personnel Records

Our main source are the "Official Registers of the United States" (Department of the Interior, 1871-1893) (hereby, the Registers). The Registers contain detailed information about the Federal workforce, including employees' names, place of birth, state of residence at the time of appointment, job title, government unit, location of post, and compensation. We digitized information corresponding to the 12 registers published between 1871 and 1893 (the Registers were published biennially), roughly ten years before and after the passing of the Pendleton act. Although the Registers also include information on members of the Army and the Navy, we focus our analysis on *civil* servants.[41] Our data include information on approximately 450,000 employee-years. Of these, about 100,000 correspond to employee-years in the executive departments in DC (our main focus in this paper). Figure A2 shows an example page corresponding to the 1881 Register. This page lists employees working in the Internal Revenue Service within the Treasury Department.

[37] B8 shows school attendance rates by parental *occupation*. School attendance rates for children age 10 were close to 70% for children of unskilled workers (the group with the lowest average attendance).

[38] A similar quote can be found in Johnson & Libecap (1994a) "Indeed, if anything, patronage was seen as promoting the ideals of equality and social mobility because it allowed the common person to fill public offices (Van Riper, 1985, pp. 30-60)."

[39] Similarly, Van Riper (1976) describes how opponents of the reform feared that it would lead to an "aristocracy of office-holders".

[40] However, this statement is based on an analysis of individuals' own occupations rather than family characteristics.

[41] One quantitatively important category of civil servants for which we have not digitized the data is that of postmasters. We chose to not digitize the data since, unfortunately, the Registers include much less identifying information about them than for other employees. For instance, they do not include information on employees' birthplaces and in most cases they only include a person's initials rather than a complete first name.

3.2 Measuring Employees' Social and Professional Backgrounds

Linking the Personnel Records to Population Censuses. We collected information on employees' socioeconomic backgrounds by linking the Registers to US population censuses, using name-based matching techniques (Abramitzky *et al.*, 2019). Specifically, we used workers' names, birthplaces and approximate ages to link each of the 1871-1893 Registers to the 1850, 1860, 1870 and 1880 censuses.[42] Through this procedure, we obtained information about: (1) employees' family backgrounds, including characteristics such as race, parental wealth and parental occupations; and (2) employees' occupations prior to joining the federal government. We provide further details on the linking strategy and sensitivity checks in Online Appendix Section A. However, we note here that: (1) employees hired through exams are not more (or less) likely to be matched to the census (Table A1), (2) the results that *do not* require the linked data are very similar when estimated in this linked sample (Table A2), and (3) the results are similar when we reweight the sample to account for differences in the matching probability across different individuals (Table A3).

Parental Wealth. The 1860 and 1870 censuses asked all household heads to report the total dollar value of their real estate and personal property wealth.[43] We use the combined value of real estate and personal property wealth to rank households in the *national* (although we obtain similar results if we compute state of residence-specific ranks) wealth distribution, separately by census year and by age of the household head.[44] For those employees for whom we observe parental wealth both in 1860 and 1870, we use the average rank across both census years as our baseline measure. Throughout the analysis, we focus on wealth *percentile* ranks, following the recent intergenerational mobility literature (see for instance Chetty *et al.* (2018) and Abramitzky *et al.* (2021)).

Parental Occupations. We split parental occupations into five broad categories: professional, non-professional white-collar, farmer, skilled blue collar and unskilled.[45] For those employees

[42] Specifically, we assumed workers would have been between the ages of 18 and 60 at the time of their employment in the civil service. We chose these census years since the 1850 is the first US population census to list persons individually, and there are no surviving individual-level records for the 1890 census.

[43] The 1850 census included a question on the value of real estate but not on the value of personal property. The 1880 census did not include either of these questions. Census enumerators in 1870 were instructed to collect personal wealth information such that it was "inclusive of all bonds, stocks, mortgages, notes, live stock, plate, jewels, or furniture; but exclusive of wearing apparel. No report will be made when the personal property is under one hundred dollars."

[44] To do so, we use samples of the 1860 and 1870 censuses to construct a rank of household heads on the basis of reported wealth. A complication with computing such rank is that the 1860 Census did not list the Black enslaved population. In contrast, the 1870 Census (which took place after emancipation) did include the formerly enslaved population (about 12% of the total US population by 1870). Since the formerly enslaved population had very low levels of wealth, white household heads observed in 1870 would mechanically tend to have higher wealth rankings than in 1860. To avoid this issue, we construct wealth ranks that are based just on the white population. In addition, we base the rank on households with at least one child–as this is the relevant comparison group for our intergenerational analysis. About 87% of white household heads with at least one child had positive levels of wealth in 1860, whereas that proportion was 80% in the 1870 census. A related issue is that families who owned slaves saw a decline in their wealth levels after emancipation (Ager *et al.*, 2019). Hence, white families observed in 1870 would tend to be poorer than those observed in 1860. However, we show that our results are similar if we limit our sample to employees for whom we observe parental wealth in 1870.

[45] We focus on father's occupations since very few mothers worked outside of their households in this time period. Professional occupations are those with a value of less than 100 in the 1950 Census Bureau occupational classification

for whom we observe parental occupations in more than one census, we calculate the fraction of census years that their parents spent in a given occupational category.[46] Unlike wealth (which is only reported in the 1860 and 1870 censuses), occupations are recorded in every census from 1850 to 1880. Hence, when we focus on parental occupations we have a larger sample than we focus on parental wealth (as we have more censuses in which we can find employees living with their parents).[47]

Nativity Status and Race. We observe workers' birthplace and race, as well the corresponding information for their parents. We use this information to construct indicators of whether workers are foreign born, whether their parents are foreign born, and whether they are white. Note that, since the Registers include information on workers' birthplaces, we can also use these data directly (without linking to the census) when we investigate whether the reform changed the likelihood of workers being foreign born.

Summary Measures of Employees' Social Background. We compute two summary measures of employees' social background. These measures are constructed such that a lower value corresponds to individuals from more disadvantaged backgrounds. First, we follow Kling *et al.* (2007) and compute a "summary index" equal to the average of the following characteristics' z-scores: parental wealth rank, an indicator that takes a value of one if a worker's father was professional, an indicator that takes a value of one if a worker's father did not hold an unskilled occupation, an indicator that takes a value of one if a worker's parents were US born, and an indicator that takes a value of one if the worker was white. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation, so that each component of the index has mean zero and standard deviation one for the control group. As described in Kling *et al.* (2007), this aggregation "improves statistical power to detect effects that go in the same direction". Second, we use factor analysis to compute the first principal component of the same set of characteristics, which we then normalize to have a mean of zero and a standard deviation of one.[48]

Finally, note that the nature of our data requires that we observe employees coresiding with their parents so as to observe parental characteristics. Hence, to minimize biases due to selective coresidence at later ages, whenever we focus on parental characteristics we restrict the sample to employees whom we observe in the census below the age of 18 (and *prior* to them joining the civil service).[49]

Employees' Professional Backgrounds. We also observe workers' occupations *prior* to joining

system. Examples of such occupations include accountants, lawyers and teachers. Non-professional white-collar occupations are those with a value between 200 and 500. Examples of such occupations include bank tellers, stenographers, typists, and secretaries. Farmers are those with a value of 100. Skilled blue-collar are those with values between 500 and 700 (examples include carpenters and electricians). Finally, unskilled workers are those with a code above 700 (examples include laborers and housekeepers).

[46] For instance, when we focus on whether an individual's father had a professional occupation, we assign a value of 0.5 to those cases in which the father is listed as having a professional occupation in one census but not in the other.

[47] Although we obtain similar results if we restrict the sample to employees with non-missing information on parental wealth so as to keep a consistent sample across all our outcomes.

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

[49] Among employees whom we observe below the age of 18, 80% have a father present in the census.

the civil service.[50] Similar to when we focus on parental occupations, we split occupations into five categories: professional, non-professional white-collar, farmer, skilled blue-collar and unskilled. In those cases in which we link an employee to multiple censuses, we focus on the most recent pre-civil service occupation. When we focus on workers' own professional achievements prior to joining the bureaucracy, we restrict the sample to workers who were at least 25 year old at the time we observe them so as to enable occupations to better reflect workers' educational attainment.

3.3 Identifying Employees Appointed Through Exam

We combine the personnel records with annual data from the Civil Service Commission reports (United States Civil Service Commission, 1883). In particular, the reports include a list of all employees hired through exams in the classified *departmental service* in DC. These lists were collected with the goal of keeping track of the apportionment of positions across states, and include employees' names, state of residence at the time of appointment, initial department and compensation, examination taken, and appointment date.[51] Using this list, we are able to precisely identify which employees were hired through exams, as well as the exact exam taken by each of them. These lists cover all hires to the classified departmental service from 1883 onward, but *do not* cover employees hired in the classified customs and postal services (since these positions were not apportioned across states). Figure A10 shows an example page listing employees appointed through exams in 1883.

In addition to including a list of the employees hired through exams, the reports include a detailed list of the *positions* that were subject to examinations in each of the executive departments. Figure A11 shows an example page listing the positions subject to exam in the Treasury Department. These data enable us to precisely identify the set of "treated" positions.

3.4 Summary Statistics

Table B2 shows summary statistics for employees in our baseline sample, separately based on whether or not they had been appointed through an exam. Employees appointed through exams came from wealthier families, were more likely to have a father who worked as a professional worker (and less likely to have an unskilled father), were less likely to be foreign born or have foreign-born parents, and were more likely to be white.

4 Empirical Strategy

The main goal of our analysis is to investigate the extent to which selecting employees on the basis of competitive exams changed bureaucrats' social origins. To do so, our empirical strategy

[50] Unfortunately, censuses prior to 1940 do not include information on individual earnings or years of schooling.

[51] The one exception is that, as described above, these lists do not include the names of employees hired for the position of printing assistant in the Bureau of Engraving and Printing (since these low-paid positions were not apportioned across states).

compares the characteristics of employees hired before and after the reform (first difference), in positions exempted and non-exempted from exams (second difference). We estimate:

$$y_{ipt} = \alpha_p + \alpha_t + \beta Exam_p \times After_t + \gamma X_{ipt} + \epsilon_{ipt} \quad (1)$$

where y_{ipt} corresponds to a characteristic of employee i in position p in year t, α_p are position fixed effects, and α_t are hiring-year fixed effects. A position is defined as a combination between an occupation, a compensation, a bureau and a Department–for instance, *clerk, $1200, Pension Office, Department of the Interior*. The inclusion of position fixed effects implies that our analysis compares workers hired to perform the *same* job in the *same* government unit, but who were recruited under two different hiring regimes (patronage versus exams). Moreover, the inclusion of hiring-year fixed effects enables us to net out changes in worker characteristics stemming from aggregate changes in the economy (for instance, changes in the relative attractiveness of the public sector). Our interaction of interest is $Exam_p \times After_t$: $Exam_p$ takes a value of one if the employee worked in one of the "treated" positions (that is, those that became subject to exams), and $After_t$ takes a value of one for workers hired after the reform. Throughout the analysis, we cluster standard errors at the level of the position.

In addition, we also estimate event-study specifications of the form:

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

where the β_t coefficients describe the evolution in the characteristics of employees hired in positions subject and non-subject to exams during our sample period. The omitted category are workers hired in 1873, the first year in our data for which we can identify newly hired employees.[52] This specification enables us to investigate the extent to which the reform had different effects in the shorter (immediately afters its passing) and longer (10 years after its passing) term.

As described above, the reform established that positions in the Departmental Service in DC had to be apportioned across states on the basis of population. Since this apportionment could by itself lead to changes in bureaucrats' social origins (to the extent that it led to changes in bureaucrats' regional origins), in our preferred specification X_{ipt} includes fixed effects corresponding to employees' state of residence at the time of their appointment (state "whence appointed"). By including these fixed effects, we shut down the effects of the reform that stem from compositional changes in bureaucrats' regional origins. In practice, however, the inclusion of such fixed effects has usually modest effects on our estimates.[53]

Challenges to Identification and Tests of the Identification Strategy. Our control group is comprised of workers who were both in low- (such as laborers) and high-paid leadership positions

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

[53] Although employees had to provide proof that they resided in a given state, a concern is that employees had incentives to claim that they resided in a state that had initially less appointed employees (so as to increase their chances of appointment). However, our results are similar if we use workers' *birthplace* fixed effects (see Figure B3).

(such as unit chiefs). A first concern is that the characteristics of such workers would have been on a different time trend relative to those of workers in the positions subject to exams. This might have been the case, for instance, if the relative attractiveness of the public sector was differentially changing for workers in different parts of the skill distribution.

To address this concern, Table B3 presents F-test statistics corresponding to the hypothesis that all pre-reform event-study coefficients are equal to zero for our main variables of interest. In all cases, we do not reject the null hypothesis that all pre-reform event-study coefficients are equal to zero. In Section 5, we also present graphic evidence consistent with the common trends assumption. Finally, subsection 5.1 show the robustness of our results to using alternative definitions of the control group.

The second concern is that the reform might have induced a relabelling of positions. Specifically, if appointing officers wanted to avoid the constraints of the reform, they might have decided to increase hiring in the exempted segments of the bureaucracy. In this case, our effects could be coming from a change in the characteristics of the control group rather than by changes in the characteristics of those appointed in covered positions. Indeed, in Moreira & Pérez (2020) we document such manipulation in the context of the classified Customs Service: imposing a requirement that employees making 900$ or more a year were hired through exams led to a nearly doubling in the share of workers making less than this cutoff.

There are three reasons why such concern is less likely to be relevant in our context. First, the historical literature suggests that such manipulation was much less likely to occur for positions in the executive departments in DC, since positions in DC were under tighter control from the Civil Service Commission.[54] Indeed, Figure 1 shows that the share of positions that would have been subject to an exam remained relatively constant (at about 60%) over our time period in the classified departmental service.[55] Second, when we plot the data separately for the control and treatment groups, there is no indication of a sharp change in the characteristic of the control group after the reform. This finding suggests that our findings are driven by changes in the characteristics of employees in the treatment group rather than by changes of those in the control group. Third, our results are similar when we use alternative control groups for which such manipulation was less likely to occur –for instance, workers in offices outside of DC that were not directly affected by the reform, or workers in the Legislative and Judicial branches of government (see Figure B3).

Sample Restrictions. In our main analysis, we focus on the effects of the reform on the social backgrounds of employees 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 (rather than having to infer this information based on their position and estimated hiring date), as well

[54]For instance, United States Civil Service Commission (1890) 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."

[55]As described above, the 1891 decrease in the share of covered positions is driven by the addition of 2,500 workers in the Census office. These workers were hired on a temporary basis to tabulate the 1890 census and were exempted from exams.

as their appointment date and exact exam taken. Hence, we restrict our baseline sample to employees in the Executive Departments in DC.[56] We note, however, that our results are similar if we include workers outside of DC or outside the Executive Departments (for instance, in the Legislative or Judicial Departments) to our control group (see Figure B3). In addition to focusing on the Executive Department, we also restrict our baseline sample 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 different sources using information on their names. For instance, 40% of women aged 18 to 50 who reported an occupation in the 1880 census were either married or widows (who would have likely preserved their married name). Indeed, our matching rates are lower for female employees (Figure A13). Second, nearly 85% of the employees appointed through exams were male, so restricting the sample to male employees improves the comparability of the treatment and control groups. Our main results are nevertheless similar when we add female employees to the sample (see Figure B3). Table B1 illustrates the construction of our baseline sample.

5 Main Results: Exams and the Social Origins of Government Employees

In this section, we ask if the reform facilitated or impeded the access of individuals from disadvantaged backgrounds to government jobs. We focus on parental wealth, parental occupations and worker's demographics.

Summary Index of Social Background. We first investigate the effects of the reform on the summary index of employees' social background described in Section 3. This index aggregates information on parental wealth, parental occupations and demographics (nativity status and race), and is constructed such that a lower value corresponds to individuals from more disadvantaged backgrounds. A benefit of using this index is that doing so improves statistical power (Kling *et al.*, 2007), although this comes at the cost of making the results harder to interpret. Later, we present estimates for specific components of the index.

Figure B1 shows the average value of this index for newly hired workers in positions subject and non-subject to exams, from 1873 to 1893. The figure shows that workers in positions subject to exam had higher values of the index than those in exempted positions throughout the sample period. However, this gap appears to increase in the post-reform period.

Table 1 confirms that workers selected through exams had a higher value of the summary index. In this table, we estimate the specification in equation 1 using this index as the outcome variable. Column 1 shows that the index was 0.17 higher among exam-based hired than among employees hired through patronage.[57] Column 3 shows a similar increase if we instead use the

[56] This restriction excludes workers in the Executive departments who worked outside of DC such as those in the Postal and Customs services, workers in the Judicial and Legislative departments, and workers in miscellaneous government agencies not affected by the reform. Examples of such agencies include the "Government Printing Office" and the "National Home for Disabled Volunteer Soldiers".

[57] As described in Kling *et al.* (2007), the point estimates show "where the mean of the treatment group is in the

principal component measure as our summary index. The estimates are also similar regardless of whether or not we include fixed effects for workers' state of residence at the time of appointment (odd versus even columns), suggesting that the effects are not driven by changes in workers' regional origins due to apportionment.

Figure 2 shows the corresponding event-study estimates. The pre-reform event-study coefficients are sometimes positive and sometimes negative, and we do not reject the hypothesis that they are all jointly equal to zero (p-value: 0.34, see Table B3). In contrast, all of the post-reform event-study coefficients are positive and they are jointly statistically significant (p-value<0.01, see Table B3). The figure suggests a rapid increase in the summary index after the reform, with the estimates then declining in size (but remaining positive) and fluctuating around 0.15: By 1893 (10 years after the reform), workers appointed through exams were still of higher social status than those hired through patronage.

To benchmark this result, consider that the pre-reform median value of the summary index was 0.25 among workers employed as "clerks", whereas it was -0.12 among those employed in the lower-paying occupation of "laborer". Hence, the increase in the summary index corresponds to about half the pre-reform gap between "clerks", a white-collar occupation with a median annual compensation of $1400 in the pre-reform period, and "laborers", a blue collar occupation with a median annual compensation of $660.

Parental Wealth. We next investigate the consequences of the reform for the different components of the index, starting from parental wealth ranks. Figure B2 shows average parental wealth ranks for workers in positions subject and non-subject to exams, from 1873 to 1893. Workers in positions subject to exams had higher average parental wealth throughout the period. However, similar to what we observe for the summary index, the figure suggests a differential increase in the average parental wealth rank of workers in positions subject to exams after the reform.

Columns 1 and 2 in Table 2 confirm that employees hired through examinations had higher average parental wealth than those hired through patronage. Specifically, employees hired through exams came from families that were 6.2 percentile ranks higher in the national wealth distribution, slightly above a 10% increase; prior to the reform, the average employee had a family wealth at the 54 percentile of the wealth distribution.

In columns 3 to 6, we compute separate ranks for personal property and for real estate wealth – rather than a single rank based on their combined value. Differences in levels of real estate wealth may simply reflect differences between urban and rural areas, or regional differences in homeownership rates. It is reassuring that the average rank increases for both measures, and particularly so for personal wealth: Workers appointed through exams came from families that were 7 percentile ranks higher in the distribution of personal property wealth and 5 percentile ranks higher in the distribution of real estate property.

Figure 3 shows event-study estimates of the effects of the reform on total parental wealth. Similar to when we focus on the summary index, the pre-reform event-study coefficients are relatively

distribution of the control group in terms of standard deviation units."

small and we do not reject the hypothesis that they are jointly equal to zero (p-value: 0.39, see Table B3). In contrast, all of the post-reform event-study coefficients are positive and they are jointly statistically significant (p-value<0.01, see Table B3). In particular, the estimates suggest a rapid increase in parental wealth relative to employees in the control group. 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.

Which Groups Won and Which Groups Lost Access? Employees hired through examinations had higher average parental wealth than those hired through patronage. Such increases could be compatible with increases in the representation of the middle class at the expense of the children of the poor, or with increases in the representation of the upper class at the expense of the middle (or by some combination of the two).

To investigate which groups increased and which groups decreased their representation as a result of the reform, we split individuals based on the wealth quintile of their parents. Panels (a) and (b) in Figure 4 show, for employees in positions that became subject to exams after 1883, 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 in the 1883-1893 period.

Among workers in positions subject to exams, workers who grew up with families in the top quintile were overrepresented prior to the reform. Specifically, individuals from the top 20% accounted for about 35% of workers in such positions. However, there were no large differences in the relative representation of individuals from the bottom four quintiles: each of these groups accounted for about 15% of workers prior to the reform. After the reform, in contrast, we observe a sharp increase (from 15 to 25%) in the proportion of workers from the 60-80 quintile. This increase seems to come mostly at the expense of workers in the bottom two quintiles, with more limited changes in the share of workers from the top quintile.

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 highly-paid leadership positions (such as bureau chiefs) as well as low-paid positions (such as laborers). However, this overrepresentation of both the bottom and the top of the parental wealth distribution appears to continue in the post-reform period (Panel (d)).

Panel (e) in Figure 4 confirms this pattern when we run separate regressions in which the dependent variables are indicators for belonging to the different quintiles 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 the middle and upper middle class (that is, families between the 40 and the 80 percentiles of the wealth distribution), which comes at the expense of a reduced representation of families in the bottom two quintiles (particularly the bottom 20%).

Parental Occupations. Table 3 shows that the reform led to a shift towards employees with higher-status parental occupations. First, employees hired through exams were 2.4 percentage points less likely to have a father who worked in an unskilled occupation (nearly a 30% decline). Indeed, combining all blue-collar occupations (skilled blue - plus unskilled) 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, they were 5 percentage points more likely (relative to a baseline of 11%) to have a father with a professional occupation. Finally, there is also an increase in the share of children with a farmer father, although this effect is smaller (and loses statistical significance) once we include fixed effects corresponding to workers' state of residence at the time of appointment.

Nativity Status and Race. We next ask if the reform changed the demographic characteristics of government employees. We focus on three characteristics: whether workers were foreign born, whether their parents were foreign born, and whether they were white.

Table 4 shows that the reform reduced the representation of immigrants (and their children) in government jobs. Columns 1 and 2 show that employees appointed through exams were 4 percentage points less likely to be foreign born, nearly a 40% reduction. This result could reflect the fact that immigrants, who had had less exposure to US education, might have been at a disadvantage relative to the US-born when completing the exams. The decline in the share of immigrants, however, does not seem to be simply driven by a lack of familiarity with English: Table B9 shows that there is a large decline in the share of immigrants from *English-speaking* countries, and in particular of those of Irish origin –a disadvantaged group at the time.

An advantage of looking at immigrant status as an outcome is that it does not require us to link observations to the census (as place of birth was directly reported in the Registers data). Hence, it is possible to assess the sensitivity of the results to using the linked sample. Table A2 shows that we find a similar decline in the likelihood that an employee would be foreign born regardless of whether we use the full sample or the sample of records that we successfully link to the census; if anything, the decline is larger when estimated in the non-linked sample. Moreover, the difference between the estimates that we obtain in the linked and non-linked samples becomes even smaller (Column 3 versus Columns 1 and 2 in 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 4, we instead focus on the likelihood that an employee would have been the children of a migrant. Unlike migrants themselves, children of immigrants would have presumably been exposed to education in the US (perhaps limiting the disadvantages observed in the first generation). However, we find that exams also reduced their representation: there is a substantial decline in the proportion of children of immigrants–nearly 8 percentage points, relative to a control group mean of about 20%.

Finally, in columns 5 and 6 we investigate if the reform led to a different racial mix in government positions. The dependent variable in these columns is an indicator that takes a value of one if an employee reported being white in the census. Although the reports of the Civil Service Commission argue that the reform led to an increased representation of African Americans in the federal administration, we find limited 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.[58] This finding is perhaps not surprising in light of the fact that African Americans had limited access to educational resources but also represented a very small proportion of federal workers prior to the reform.

Summary of Results. Our findings indicate that workers appointed through exams were of a higher social class than those appointed through patronage: they had higher levels of parental wealth, were more likely to be the children of professionals, and were less likely to be the children of immigrants (or immigrants themselves). This increase in social status occurred immediately after the passing of the reform and persisted at least 10 years after its implementation.

5.1 Robustness of Main Results

This subsection provides evidence on the robustness of 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 employees in exempted and non-exempted positions, (2) using alternative definitions of the control group, (3) using alternative definitions of which workers are considered new hires in a given year, (4) implementing a randomization inference approach, and (5) features of the linking strategy.

Time-Varying Shocks and Additional Control Variables. The apportionment of positions across states implied that individuals from certain states increased their representation after the reform. Although we include state "whence appointed" fixed effects to account for this channel, a concern is that the labor market might have been on different trends in different states, leading to differential changes in the selection of workers interested in government jobs. In this case, the effects we capture will not be those of transitioning from patronage to exams, but rather the effects of increasing the representation of certain states. To deal with this concern, in Figure B3 we show that our results are similar when we include appointing state times hiring-year fixed effects. These controls account for time-varying differences in labor market opportunities across states which might have influenced the likelihood of applying to government jobs.

In Figure B5, we show that our results are similar when we exclude one department at a time. The y-axis in this figure shows our estimated effects, whereas the x-axis shows the excluded department. This finding rules out that our results would have been driven by a concurrent change that took place in only a specific department. Indeed, our results are similar when we include department times hiring-year fixed effects (Figure B3).

After a long period of Republican dominance (starting with the Presidency of Ulysses S. Grant in 1869), the Presidency went back to Democratic hands in 1884. A concern is that the effects that we capture might not be driven by the introduction of exams, but rather by the fact that patronage appointments were of a different social background under Republican and Democrat administrations. However, since the Presidency went back to a Republican in 1888 and then back again to a

[58] The reports of the Civil Service Commission argue that "It is noticeable that a much larger proportion of colored people receive appointments under the civil-service law than under the old patronage system." (United States Civil Service Commission, 1891)

Democrat in 1992, we can investigate whether the effects of exams depended on whether the Presidency was on Democrat or Republican hands. Table B5 shows that the increase in bureaucrats' social status persisted both under Democratic and Republican control of the Presidency, although the point estimates are larger while Democrats were in power.

Alternative 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 positions exempted from exams (that is, positions either at the bottom or at the top of the state hierarchy). Figure B3 shows that our results are robust to using a number of alternative definitions of the control group. First, our results are similar when we use a control group constituted by either: (1) only bureaucrats at the bottom of the state hierarchy, or (2) only bureaucrats at the top of the state hierarchy. Second, the results are also similar when we drop individuals making more than \$3000 and less than \$600 from the control group (so as to increase the comparability of the treatment and control groups). Third, we add workers outside of DC to the control group.[59] Finally, we add workers who were employed in DC but who were employed outside of the Executive departments (and hence worked in units in which no employee was affected by the reform, such as the Legislative and Judicial Departments). The stability of the results to different definitions of the control group makes it unlikely that our results would be driven by changes in the control rather than in the treatment group.

Alternative Definitions of a New Hire. We next investigate the robustness of our results to the definition of a new hire. In the baseline specification, we define a worker as being a new hire if there is no worker listed in the previous register who has the same name, same birthplace, same appointment state, and is employed in the same Department. A concern with this approach is that errors in the registers data (or in our digitization) might lead us to deem a worker as a new hire even if that worker was already employed by the federal government. In Figure B3, we adopt a more conservative definition of a new hire. Specifically, we define a worker as being a new hire if there is no employee listed in the previous register with a name within a Jaro-Winkler distance of 0.1 of a worker's name (rather than using the exact name) and regardless of birthplace. Again, we find very similar results when using this alternative definition.

Inference. Figure B4 shows that our results are robust to implementing a randomization inference approach. To do so, we randomly label a group of employees (of equal size of our actual treatment group) as having been hired through an exam. We then estimate the "effects" of the reform using these placebo treatment groups, repeating this exercise 1,000 times. The figure shows that our placebo estimates are all centered around zero and are significantly smaller in absolute value than our actual estimates.

Linking Strategy. We also test the robustness of our results to various features of the linking strategy. First, Figure A1 shows that our results are similar when use more or less conservative cutoffs for deeming an observation as a match. Second, Table A3 shows that our results are also similar if we reweight the data to account for differences in the observable characteristics of matched ver-

[59]When we do so, we add place of employment to our definition of a position.

sus non-matched employees.[60] Finally, as discussed above, our results focusing on the likelihood that employees would be foreign (which do not require linked data) are similar when estimated on the linked and the non-linked samples (Table A2).

6 Mechanisms

Our interpretation of the findings is that, by changing the relative weights of "education" and "connections" when screening employees, the reform disproportionately hurt the chances of applicants from certain social origins (thereby decreasing their representation). We first provide a simple conceptual framework that illustrates this argument. We then show empirical evidence consistent with this interpretation and discuss alternative mechanisms.

6.1 Conceptual Framework

Assume that obtaining a government job depends on applicants' education ("e") and connections ("c"). Connections can be of a personal (for instance, being the children of a member of Congress) as well as a political (for instance, having been a campaign worker for the incumbent party) nature. We also think of education broadly, encompassing applicants' stock of knowledge as well as their ability to study for the exam. Further, assume that e and c are both potentially correlated with applicants' family wealth (w).

Applicants are hired if they are above a cutoff "l" in terms of their combined values of e and c, that is:

$$\alpha e + (1 - \alpha)c > l \tag{3}$$

We interpret the reform as an increase in the value of α (that is, the relative weight of education in hiring decisions). Hence, a direct effect of the reform is to favor the "educated outsiders": individuals with high values of e but low values of c.[61]

Whether the shift towards "merit" helps the poor, the rich or the middle class depends on the relationship between e, c and w. Figure B6 illustrates three possible cases. In Panel (a), wealth has a stronger correlation with education than it has with connections. In this case, introducing exams disproportionately helps the chances of the children of the rich. In Panel (b), in contrast, wealth is correlated with connections but has no relationship with education. Under these conditions, introducing exams would increase the relative representation of the children of the poor. 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. Note that, even if there is a

[60] To do so, we estimate a probit model of the likelihood of matching to an observation in the census based on workers' birthplace, state whence appointed, department, position, compensation and register year. We then reweight the data based on the inverse of the estimated matching probability.

[61] Note that this framework abstracts from applicants' outside options (that is, we assume that anyone who is above the cutoff is going to be hired). We do so in order to keep the framework parsimonious, as the reform did not change applicants' outside options.

positive relationship between education and wealth, increasing the weight given to education *does not* necessarily favor the children of the rich.

The increased emphasis on education may also affect bureaucrats' characteristics indirectly, by changing the set of individuals interested in such career. For instance, the reform might have increased the prestige of holding a government job, thus increasing the appeal of a civil service career for individuals of higher social status. Such indirect effects are akin to those in other settings. For instance, changes in college admission practices that increase students' expected ability could make the signal of a college degree more informative, thus leading to changes in the characteristics of applicants interested in such degree. We interpret our estimates of the impacts of the reform as encompassing both its direct (through changes in screening) and indirect (through changes in the applicant pool) effects.

A simplification of this framework is that it abstracts from dynamic considerations. However, applicants of different social backgrounds might differ in their ability to adapt to a new system, making the effects of the reform potentially different in the short and long run. For instance, applicants from wealthier backgrounds might have more resources to invest in preparing for the exams (for instance, by using tutors), thus rendering exams more advantageous for them as time goes by.[62] Alternatively, applicants from poorer backgrounds might need more time to "catch up" with the contents of the exams, and hence be at a relative disadvantage early on. Note that our conceptual framework can incorporate these dynamic considerations by allowing for a time-varying relationship between e, c and w.

6.2 Empirical Evidence

The Reform Increased the Representation of "Educated Outsiders". Our conceptual framework indicates that the reform should have brought more "educated" individuals to government jobs. 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 occupation likely required higher educational attainment.[63]

Table 5 shows that employees hired through exams were 9 percentage points more likely to have held a *professional* occupation prior to joining the civil service (a 30% increase). These are precisely the occupations which 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 the social mix of government employees, increasing the proportion of those coming from rural areas (see Table B10 and the discussion below).[64] These

[62] For example, Sundell (2014) describes how "after the introduction of competitive exams for the British Indian Civil Service in the nineteenth century, private tutors that provided instruction specifically for the tests, "crammers", sprang up."

[63] The censuses do include information on literacy. However, literacy is a very coarse measure of human capital in this context as the vast majority of the adult white population was literate by 1880.

[64] Also, note that farmers were not a particularly uneducated group in this time period: Among white adult males

increases were mostly driven by a decrease in the likelihood that employees would have held a white-collar non-professional occupation prior to joining the civil service.[65]

Figure 5 shows the corresponding event-study estimates. The figure shows a rapid increase in the share of workers who had a professional occupation prior to joining the civil service, which is accompanied by a decrease in the share of those in white-collar non-professional jobs. The increase in the share of workers with a professional background does not seem to fade out 10 years after the reform. This finding suggests that the reform continued to attract workers with stronger professional backgrounds in the longer term.

An additional implication of our proposed interpretation is that exams should have benefited individuals who had better access to education growing up. To test this hypothesis, we use data from Haines *et al.* (2010) on the number of per capita schools by US county in 1850.[66] Panel (a) in Figure B10 shows the number of schools per children aged 5-14 across different US counties in 1850. The figure shows that there was substantial heterogeneity across counties in the local availability of schools. Counties in the top quintile of the per-capita school distribution had an average of 7.3 schools every 1000 school-aged children, whereas counties in the bottom quintile had only 0.24. Panel (b) instead shows school attendance rates among children in the same age group. As it is clear from comparing both panels, there is a strong correlation between school availability and attendance rates (see also Figure B11 which directly shows this correlation).

Table 6 shows that individuals appointed through exams came from counties that had higher per capita schools by 1850. The outcome variable in this table is the log of per capita schools in employees' childhood county of residence. This result is similar when we exploit variation within states (Column 3) and within urban/rural areas (Column 4), suggesting that it does not simply capture differences across broad regions of the country. Moreover, the results are also similar when we additionally control for parental characteristics such as occupation (Column 5), birthplace (Column 6) and wealth (Column 7). This similarity makes it unlikely that the association we document can be explained by the correlation between the availability of schools and parental social status.

We next investigate if the reform brought "outsiders", that is, individuals who lacked personal and political connections and hence were unlikely to obtain a job through patronage. A challenge in testing this hypothesis is that informal connections are –by their own nature– difficult to observe. Hence, we proxy for these connections using four alternative measures, each intended to capture different types of connections that applicants might have benefited from. First, to capture "nepotistic" connections, we construct an indicator that takes a value of one if the bureaucrat's

aged 18 or more in 1880, those employed as farmers had a 91% literacy rate (compared to 93% among those who were not farmers).

[65] The two largest occupations among white-collar non-professional workers are "managers, officials and proprietors" and "salesmen and sales clerks". White-collar non-professional workers were likely much less educated than professional workers: In 1870, the average "Occupational education score" among white-collar non-professional workers was 23.4, whereas it was 82.2 among professionals. This score corresponds to the proportion of individuals in a given occupation who have a college degree based on the 1950 census.

[66] We use 1850 because this is the last pre-reform census for which Haines *et al.* (2010) report data on the number of schools per county.

father reported working for the federal government in the census. Second, we construct an indicator that takes a value one if the bureaucrat had the *same surname* as a contemporary member of Congress from their own state of residence or birth.[67] Third, we construct an indicator that takes a value of one if the bureaucrat spent part of their childhood in Washington, DC. This measure captures the notion that people who grew up in DC were more likely to be exposed to informal political connections than those who grew up elsewhere.[68] Finally, we construct a measure aimed at capturing bureaucrats' likely political affiliation. Specifically, we combine the information on bureaucrats' last county of residence prior to joining the civil service (from our linked sample), with county-level data on vote shares by party in historical presidential elections (from university Consortium for Political & Research (1999)). We then use these two sources of information to construct an indicator that takes a value of one if a majority of voters in bureaucrats' county had voted for the incumbent party (Republicans from 1873 to 1885, Democrats from 1885 to 1889, Republicans from 1889 to 1893 and Democrats from 1893 until the end of our sample period) in the most recent presidential elections. An important feature of all of these measures is that they are pre-determined with respect to bureaucrats' employment in the civil service.

Table 7 shows that employees appointed through exams were less likely to have a father who himself worked for the federal government (although the effect is not statistically significant at the conventional levels), were less likely to have spent time in DC during childhood, and were less likely to hail from a county which voted for the incumbent party in the previous presidential elections.

As discussed above, the federal government employed workers in positions with varying degrees of technical requirements. It is plausible that connections would have been more relevant for accessing positions that required less technical skills. This would have been the case, for instance, if appointing officers cared about hiring workers with at least a minimum level of competency– thus making it harder to privilege personal and political connections when hiring for technical positions (Brierley, 2019).

With this in mind, we assess if the effects of the reform on the likelihood that an individual would be "connected" varied depending on the position to which a bureaucrat was appointed. We estimate:

$$Connected_{ipt} = \alpha_p + \alpha_t + \beta Exam \times Clerk_{ipt} + \beta Exam \times Technical_{ipt} + \gamma X_{pt} + \epsilon_{ipt} \qquad (4)$$

where $Exam \times Clerk_{ipt}$ takes a value of one if employee i is listed as having taken either the clerk or the copyist exam, and $Exam \times Technical_{ipt}$ takes a value of one if the employee is listed as having taken one of the various technical exams (for instance, the exam for meteorological clerks

[67] To do so, we used the *Biographical Directory of the US Congress* (Dodge & Koed, 2005) to compile information on Congressmen names.

[68] For instance, Ziparo (2017) writes that: "Living in the epicenter of national political life, applicants from Washington, D.C., had an advantage in obtaining political influence. Powerful men lived, ate, and walked among them. In 1861, Abraham and Mary Lincoln wrote letters of recommendation for Ann Sprigg, their landlady during Lincoln's single term in Congress in the late 1840s."

in the Agriculture department).

Table 7 shows that the decline in the likelihood of being connected comes nearly exclusively from those individuals who were appointed to the relatively non-technical clerical 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 from a county that voted for the incumbent party (a 10% decline). In contrast, there is a much more limited decline in these probabilities among those who took the more technical exams.

The Middle Class was Overrepresented Among the "Educated Outsiders". Employees hired through exams were more likely to belong to the upper-middle class. Our interpretation of this result is that the reform increased the proportion of such workers because they were overrepresented among the "educated outsiders". We provide two pieces of evidence that suggest that this was the case.

First, Panel (a) in Figure 4 shows that, prior to the reform, workers whose families belonged to the 60-80 quintile of the wealth distribution were similarly represented in positions that would become subject to exams than those whose families belonged to the bottom three quintiles. Moreover, Figure B7 shows that this similar representation occurred despite workers from the 60-80 quintile had higher educational attainment than those from the bottom three quintiles. This figure shows school attendance rates by age and parental wealth quintile, based on census data covering the entire 1870 US population.

Second, Figure B12 shows that the low representation of the 60-80 quintile prior to the reform was unusual relative to their representation in comparable private sector jobs. This figure shows the distribution of workers across parental wealth quintiles for workers who held white-collar jobs in the private sector, based on a sample linking adults in the 1880 census to their childhood households in 1860.[69] The figure shows that, unlike the case of civil servants in the pre-reform period, the likelihood of holding a white-collar occupations in the private sector grows monotonically with parental wealth quintile.

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, applicants from poorer backgrounds 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 B7), but anyway managed to obtain a similar share of government jobs (see Figure 4).

The most likely explanation for this pattern is that workers from disadvantaged backgrounds (who typically face worse outside options) might be more likely to be targeted for patronage jobs than those from the middle class.[70] The historical literature suggests that this was indeed also the

[69]This sample was constructed using the exact same algorithm that we use to link the personnel records to the census.

[70]For instance, Sorauf (1960) argues that rising wages in the private sector reduced the ability of politicians to obtain political gains from patronage jobs: "private employment has become progressively more attractive with rising wage levels, union protections and securities, unemployment compensation, pension plans, and fringe benefits. Viewed by

case in our context. In particular, the post-Civil War period featured the preeminence of the "urban political patronage machine" (Brown & Halaby, 1987).[71] These political machines have been described as emerging in response to the needs of working-class city dwellers in a context of rapid urbanization: The "urban immigrant and lower classes needed help", and the machine provided "assistance and jobs in return for loyalty, labor, and votes" (Mashaw, 2010). As a consequence of this exchange, the patronage mechanism "drew unprecedented numbers of ordinary citizens into the channels of political life" (James, 2006).

Our empirical findings are consistent with this interpretation. First, as described above, we observe declines in the share of workers coming from a county in which the incumbent party had received a majority vote. This finding suggests that the reform indeed reduced political favoritism in the allocation of jobs. Second, we also observe declines in the share of immigrants and their children in government positions, a group that has been described as the primary target of urban political machines (see for instance Cornwell Jr (1964)).[72] Moreover, we observe declines in the share of employees of Irish origin, an ethnicity that has been particularly associated with political machines in this period (Erie, 1990). Finally, we also find a sharp decline in the share of workers who grew up in urban areas (Table B10); namely the locations where political machines were the most active (Brown & Halaby, 1987).

The Reform Hurt the Chances of the Poor when Inequality in Access to Education was High. An implication of our conceptual framework is that a shift towards "merit" should have the most negative impacts on the chances of children from poorer backgrounds when educational resources are very unequally distributed (that is, under an scenario such as the one depicted in panel (a) of Figure B6). To investigate if this was the case in our context, we exploit variation in the levels of inequality in access to schooling across different US states. Note that, because of the apportionment rules, applicants to jobs in the classified departmental service were in practice mostly competing against individuals from their own state of residence. Moreover, the decentralized nature of public education in the US translated into large regional differences in access to schooling (see Lindert (2004) and Figure B11).

Specifically, we use cross-sectional data from the 1870 population census to compute, for each state s:

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

most Americans as a short-term, desperation job alternative, the patronage position has lost considerable value as a political incentive." The increased attractiveness of the private sector relative to patronage jobs made political machines reliant on the poor: "They (political machines) flourished especially in those urban centers inhabited by large groups of immigrants and minorities-groups not yet integrated into American life, often poor and insecure and bewildered by the traditions of American politics. The machine spoke to them in the simple terms of a job, of sympathy in city hall, and of food and fuel to soften the hardest times.(...) But the machine, and the politics of the underprivileged on which it rests, is surely on the decline."

[71] Brown & Halaby (1987) defines a "political machine" as a "political party that joins a particularistic style of mobilization-one rooted in favoritism and the use of material inducements".

[72] Indeed, "the common explanation ties the rise and fall of patronage machines to the rise and fall of immigrant urban electorates." (Reid Jr & Kurth, 1992)

This measure corresponds to 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. This ratio would be close to one in a state with broad access to schooling regardless of parental wealth, but significantly above one when educational resources are unequally distributed. Figure B13 shows the distribution of this measure across US states based on the school attendance rates of children aged 8 to 12 in the 1870 census. Inequality in access tended to be the highest in the South and the lowest in parts of the Northeast.

Table 8 reports results in which we split the sample based on whether states had below or above median levels of educational inequality. Panel (a) shows that the increase in the summary index is about twice as large in the high-inequality states. Note, however, that the index increases *both* in the below- and in the above-median inequality states.

In contrast, Table B11 shows that there is no such heterogeneity when we focus on workers' *own* occupation prior to joining the civil service: there is a similar increase in the likelihood that an employee would have held a professional occupation both in the low- and in the high-inequality states. This finding suggests that the reform was successful in bringing "educated outsiders" from both low- and high-inequality states, but what varied is *who* these educated outsiders were in terms of social class.

6.3 Alternative Interpretations and Discussion

A first alternative interpretation is that the effects of the reform were not driven by the use of exams *per se*, but rather by the fact that, as exams were held throughout the country, the reform facilitated the access of workers from a more diverse set of locations. To assess this possibility, we investigate how our results change as we include: (1) birthplace, (2) childhood state, (3) childhood state by urban/rural fixed effects (based on place of residence in the earliest census in which we observe an individual). Intuitively, if the effects of the reform were simply coming from changes in the geographic origins of government employees, we should observe that these effects are muted once we compare individuals who grew up in similar locations.

Table B8 shows that this explanation seems to play at most a modest role: we observe similar increases in the summary index of social status when looking *within* locations of residence (or within birthplaces). Hence, changing employees' geographic origins does not appear to be a quantitatively important channel for explaining our results.[73]

As discussed above, the increased emphasis on education may have affected bureaucrats' social origins indirectly, by changing the characteristics of individuals interested in such career. Although this effect is inherent to any move towards "meritocracy" (and the combined effect of changes in screening and changes in the applicant pool is still policy-relevant), two pieces of evidence suggest that the effects we document are not solely driven by changes in the applicant pool. First, we observe a rapid change in bureaucrats' social backgrounds. This sharp change seems inconsistent

[73] Also, note that our results are similar when we include fixed effects corresponding to workers' state of residence at the time of appointment.

with the effects being driven by plausibly slower to change perceptions about the prestige of public employment. Second, if the effects were only driven by changes in the applicant pool, it is unclear why such effects would be stronger among applicants from states with high inequality in access to schooling.

Finally, we note that our data and empirical design do not allow us to distinguish if the reform decreased the chances of applicants from disadvantaged backgrounds because such applicants performed worse in the exams, or simply because applicants from poorer backgrounds were discouraged by the exam and hence did not apply to begin with. We note, however, that this combined effect is what matters ultimately for representation.

7 Conclusions

Do exams help or hurt the chances of applicants from disadvantaged backgrounds? And under what circumstances are exams more likely to be detrimental for the chances of such applicants? This paper studied these questions in the context of the 1883 Pendleton Act, which introduced competitive exams for the selection of some federal employees. Comparing the backgrounds of employees in exempted and non-exempted positions before and after the reform, we find that exams led to increased elitism among public employees: employees 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 increased elitism persisted over time and was stronger among applicants from states with high inequality in access to schooling. Hence, our findings suggest that, while patronage might bring less qualified employees, it appears to recruit them from relatively disadvantaged groups rather than from a "mediocre" elite.

A remarkable feature of our findings is that this increased elitism occurred despite the exam, unlike in other settings, was based on content that should have been accessible for applicants with modest educational backgrounds. Moreover, the exams were practical and aimed at testing applicants' aptitude for a specific position rather than general knowledge (Hoogenboom, 1959). To the extent that an "ideal exam" would share these attributes, our findings might represent a best case scenario of the equity consequences of exams.

Our findings also have implications for the broader debate on exams and meritocracy. Allocating opportunities based on exams is sometimes pictured as an equity-efficiency panacea.[74] Our results challenge this view: although selecting individuals using exams can in principle increase efficiency, we show that it can also reduce the representation of applicants from disadvantaged backgrounds. Moreover, our results highlight the importance of understanding which attributes are rewarded under a counterfactual selection criteria, as well as the forces shaping the distribution of such attributes across different groups in society. For example, a patronage system that used connections as the alternative to exams led to more representation of the poor in our context (a competitive democracy), but could have potentially led to less representation in a non-democratic

[74] See for instance Autor & Scarborough (2008).

country.

Importantly, while we investigate how exams shaped the social origins of government officials, an interesting question that remains unanswered is whether the poor themselves were made worse off by the reform. The overall effect of the reform on the economic outcomes of the poor might be ambiguous for two main reasons. First, applicants from disadvantaged backgrounds that were displaced by the reform might have ended up with a similar career path than the one they would have had in the absence of the reform. Second, individuals from disadvantaged backgrounds might be the ones who benefit the most from having a well-functioning state, even if achieving this efficiency implies that they might lose direct access to government jobs. We hope future work can shed light on the overall distributional implications of exam-based selection.

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TABLE 1: THE FAMILY BACKGROUND OF EXAM-BASED HIRES. SUMMARY INDICES

	Summary Index		First Principal Component	
	(1)	(2)	(3)	(4)
Exam X After	0.185***	0.173***	0.298***	0.274***
	(0.0518)	(0.0531)	(0.0901)	(0.0932)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
App. State FE	No	Yes	No	Yes
Observations	2944	2944	2944	2944

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 and 2 is a summary index of employees' social background computed using the approach in Kling *et al.* (2007). The index combines information from the following characteristics: parental wealth rank, an indicator that takes a value of one if a worker's father had a professional occupation, an indicator that takes a value of one if a worker's father did not hold an unskilled occupation, an indicator that takes a value of one if a worker's parents were US born, and an indicator that takes a value of one if the worker was white. 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. $Exam$ is an indicator that takes a value of one if the employee was appointed through an examination. All columns include hiring year and position fixed effects. The odd columns further include fixed effects based on employees' state "whence appointed". Standard errors clustered at the position level.

TABLE 2: THE FAMILY BACKGROUND OF EXAM-BASED HIRES. PARENTAL WEALTH RANK

	Total		Personal		Real Estate	
	(1)	(2)	(3)	(4)	(5)	(6)
Exam X After	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
App. State FE	No	Yes	No	Yes	No	Yes
Observations	3034	3034	3034	3034	3034	3034

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 and 2 is the rank of a bureaucrat father in the US national wealth distribution. Wealth is computed based on the combined values of real estate and personal property. These ranks are computed separately by census year (1860 and 1870) and by age (that is, relative to all fathers of the same age). When a bureaucrat is linked to more than one census with information on parental wealth, we use the average rank across both census years as our outcome variable. In columns 3 and 4, this rank is computed based solely on personal property, whereas in columns 5 and 6 it is based solely on real estate property. $Exam$ is an indicator that takes a value of one if the employee was appointed through an examination. All columns include hiring year and position fixed effects. The odd columns further include fixed effects based on employees' state "whence appointed". Standard errors clustered at the position level.

TABLE 3: THE FAMILY BACKGROUND OF EXAM-BASED HIRES. PARENTAL OCCUPATIONS

	Professional		White-Collar Non-Prof		Farmer		Skilled Blue Collar		Unskilled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exam X After	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
App. State FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4993	4993	4993	4993	4993	4993	4993	4993	4993	4993

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is an indicator that takes a value of one if the father of a bureaucrat worked in a certain occupational category (as indicated by the column). When a bureaucrat is linked to more than one census with information on parental occupations, we use the fraction of census years that parents spent in a given occupational category as our outcome variable. Professional occupations are those with a value of less than 100 in the 1950 Census occupational classification system. Examples of these occupations include lawyers and accountants. Non-professional white-collar are those with a value between 200 and 500 (for example, clerks). Farmers are those with a value of 100. Skilled blue-collar are those with a value between 500 and 700 (for example, carpenters). Unskilled are those with a value of 700 or more (for example, farm laborers). $Exam$ is an indicator that takes a value of one if the employee was appointed through an examination. All columns include hiring year and position fixed effects. The odd columns further include fixed effects based on employees' state "whence appointed". Standard errors clustered at the position level.

TABLE 4: THE FAMILY BACKGROUND OF EXAM-BASED HIRES. DEMOGRAPHICS

	Immigrant		Immigrant Parents		White	
	(1)	(2)	(3)	(4)	(5)	(6)
Exam X After	-0.0473**	-0.0419**	-0.0830***	-0.0823***	0.00660	0.00327
	(0.0190)	(0.0176)	(0.0254)	(0.0258)	(0.00891)	(0.00956)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes
App. State FE	No	Yes	No	Yes	No	Yes
Observations	9238	9238	4505	4505	9238	9238

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 the worker is foreign born. The dependent variable in columns 3 and 4 is an indicator that takes a value of one if both workers' parents are foreign born. The dependent variable in columns 5 and 6 is an indicator that takes a value of one if the workers is listed as being white in the census. $Exam$ is an indicator that takes a value of one if the employee was appointed through an examination. All columns include hiring year and position fixed effects. The odd columns further include fixed effects based on employees' state "whence appointed". Standard errors clustered at the position level.

TABLE 5: THE PROFESSIONAL BACKGROUND OF EXAM-BASED HIRES

	Professional		White-Collar Non-Prof		Farmer		Skilled Blue Collar		Unskilled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exam X After	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
App. 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 takes a value of one if a bureaucrat worked in a certain occupational category (as indicated by the column). When a bureaucrat is linked to more than one census with information on adult occupations, we use the most recent occupation as long as it corresponds to a census conducted prior to the corresponding register. 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 3 for definition of occupational categories. $Exam$ is an indicator that takes a value of one if the employee was appointed through an examination. Standard errors clustered at the position level.

Table 6: Exams and per-capita schools in applicants' childhood county

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exam X After	0.170^{***}	0.147^{***}	0.123^{***}	0.114^{***}	0.137^{***}	0.136^{***}	0.114^{**}
	(0.0372)	(0.0370)	(0.0362)	(0.0371)	(0.0391)	(0.0391)	(0.0452)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
App. 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	5515	5515	5515	5515	4706	4706	2874

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is the (log) number of per capita schools in bureaucrats childhood county of residence. When bureaucrats are linked to more than one census below the age of 18, we use the average of log per capita schools as the dependent variable. The data on per capita schools are from Haines *et al.* (2010). $Exam$ is an indicator that takes a value of one if the employee was appointed through an examination. All columns include hiring year and position fixed effects. Columns 2 to 7 include additional characteristics as indicated by the table. Standard errors clustered at the position level.

TABLE 7: EXAM-BASED HIRES MORE LIKELY TO BE "OUTSIDERS"

	Father Gov. Emp.		Lived in DC		Same Surname		Incumbent Party		Vote Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exam X After	-0.00863		-0.0564***		0.00189		-0.0484*		-0.0132	
	(0.0164)		(0.0190)		(0.00172)		(0.0261)		(0.00844)	
Exam X Clerk		-0.0326*		-0.0810***		0.000382		-0.0531*		-0.0218**
		(0.0175)		(0.0196)		(0.00208)		(0.0275)		(0.00946)
Exam X Tech.		0.0266		-0.0172		0.00453		-0.0397		0.00259
		(0.0303)		(0.0253)		(0.00353)		(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
App. State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4993	4993	5860	5860	25442	25442	6416	6416	6416	6416

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$ The outcome in columns 1 and 2 is an indicator that takes a value of 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 takes a value of 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 takes a value of one if a bureaucrat shared a surname with a current member of Congress from his own state of birth or appointment. The outcome in columns 7 and 8 is an indicator that takes a value of 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 X Clerk" takes a value of one if an employee took the general or limited exams for clerks. "Exam X Tech." takes a value of one if an employee took one of the technical or supplementary exams that were required for employees in more technical positions. All columns include hiring year and position fixed effects. The odd columns further include fixed effects based on employees' state "whence appointed". Standard errors clustered at the position level.

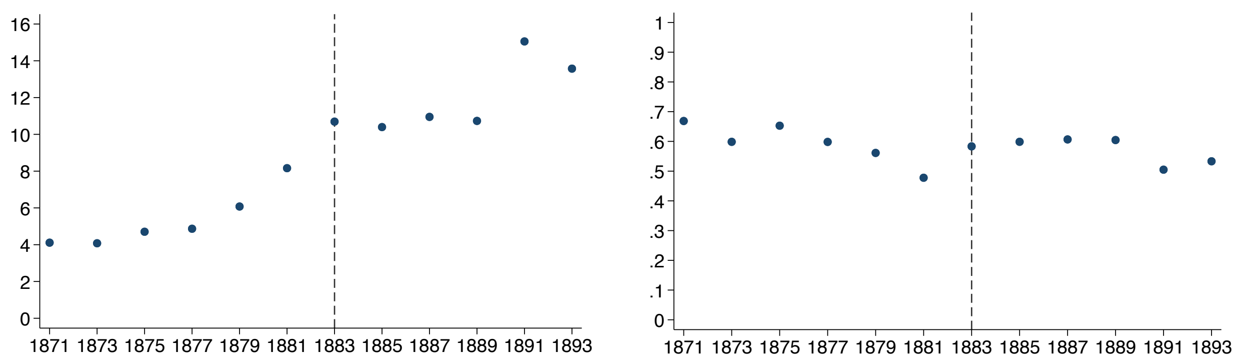
TABLE 8: HETEROGENEITY BY STATE OF RESIDENCE INEQUALITY IN ACCESS TO SCHOOLING

	Below Median Ineq.		Above Median Ineq.	
	(1)	(2)	(3)	(4)
Exam X After	0.142**	0.137**	0.231***	0.222***
	(0.0571)	(0.0594)	(0.0796)	(0.0805)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
App. State FE	No	Yes	No	Yes
Observations	2204	2204	740	740

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index computed using the approach in Kling *et al.* (2007). The sample in columns 1 and 2 in 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 fixed effects based on employees' state "whence appointed". Standard errors clustered at the position level.

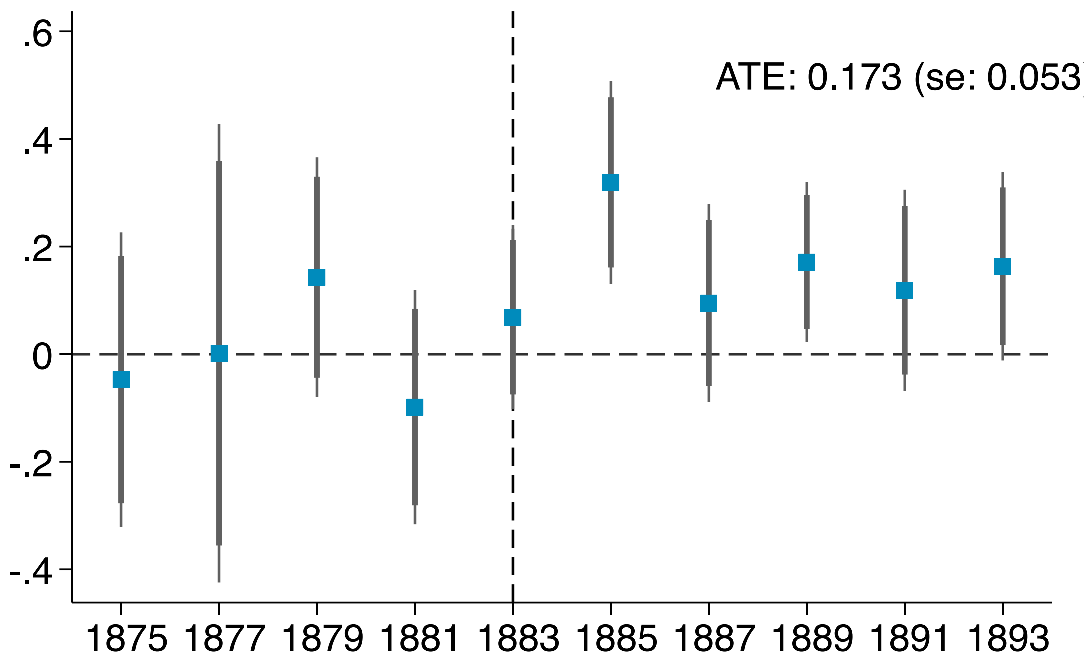
FIGURE 1: COVERAGE OF EXAMS FOR WORKERS IN THE DEPARTMENTAL SERVICE IN DC

(A) TOTAL NUMBER OF WORKERS (000S) **(B) % IN POSITIONS SUBJECT TO EXAMS**



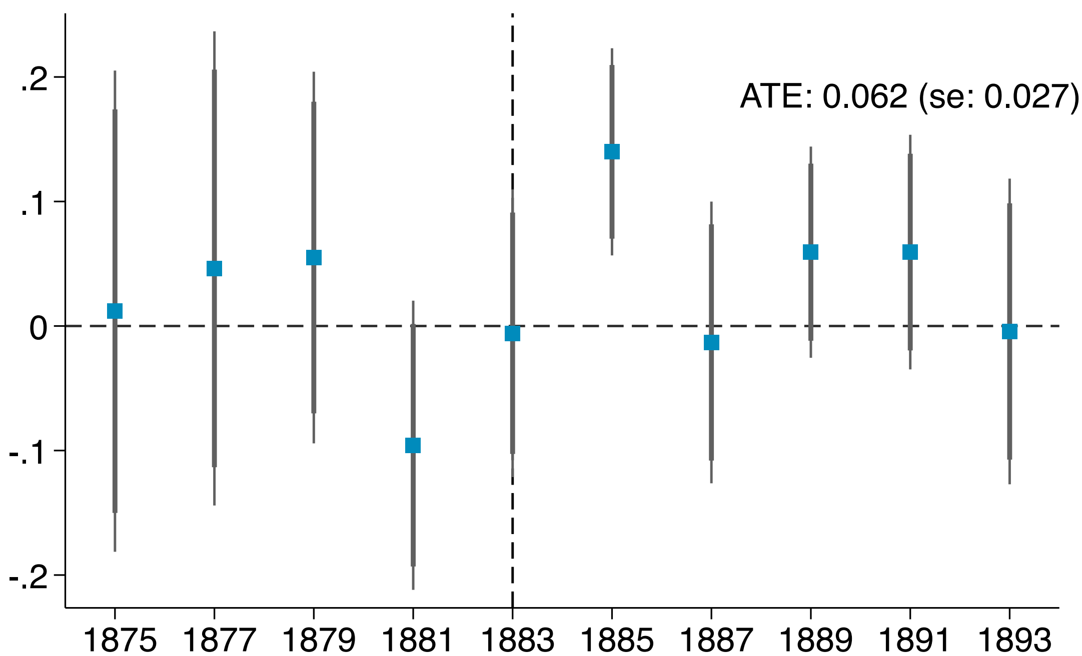
Notes: Panel (a) shows the total number of workers in the Departmental Service in Washington, DC. Panel (b) shows the proportion of such employees who worked in positions that became subject to exams after 1883. A position is coded as being subject to exams if it required an exam at any point from 1883 to 1893.

FIGURE 2: SOCIAL BACKGROUND OF EXAM-BASED HIRES



Notes: The dependent variable is a summary index of employees' social class. The index is based on the approach in Kling *et al.* (2007), and it is constructed by combining information on parental wealth rank, parental occupations, parental birthplace and race. The figure reports estimates of event-study specifications as described in equation 2. The figure shows the estimated coefficients around 90 and 95% confidence intervals (based on standard errors clustered at the position level). All specifications include hiring year, position and state "whence appointed" fixed effects.

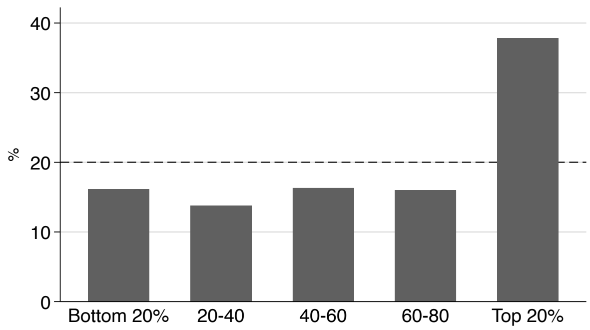
FIGURE 3: THE FAMILY BACKGROUND OF MERIT HIRES. PARENTAL WEALTH RANK



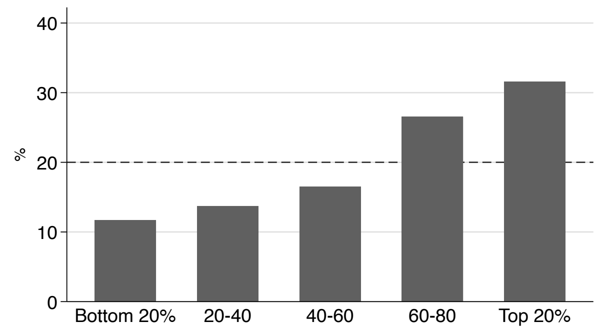
Notes: The dependent variable is the rank of a bureaucrat father in the US national wealth distribution. These ranks are computed separately by census year (1860 and 1870) and by age (i.e. relative to all fathers of the same age).

Figure 4: Parental Wealth Quintiles of Government Employees

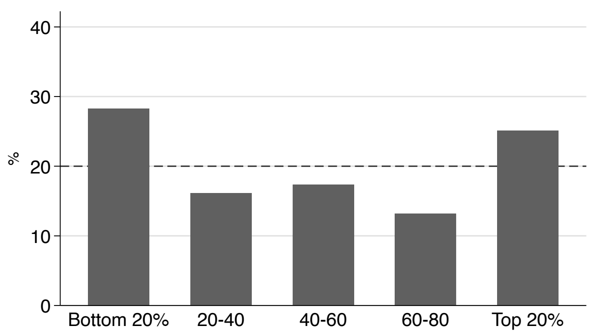
(a) Subject to Exams, Before



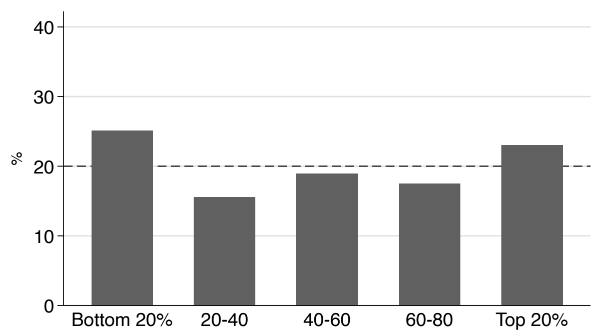
(b) Subject to Exams, After



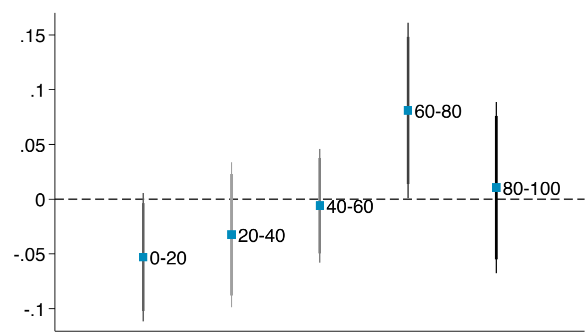
(c) Not Subject to Exams, Before



(d) Not Subject to Exams, After



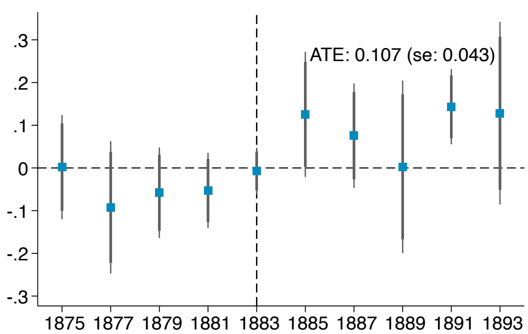
(e) Difference-in-Differences Estimates



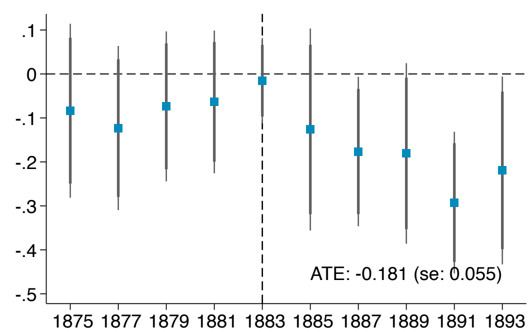
Notes: Panels (a) to (d) show the distribution of workers across parental wealth quintiles for workers in positions subject and non-subject to exams, before and after the reform. Panel (e) shows the estimates of difference-in-differences regressions in which the outcome variables are indicators for belonging to different quintiles of the wealth distribution. Each coefficient corresponds to a separate regression. All specifications include hiring year, position and state "whence appointed" fixed effects. The figure shows the estimated coefficients around 90 and 95% confidence intervals (based on standard errors clustered at the position level).

FIGURE 5: THE PROFESSIONAL BACKGROUND OF EXAM-BASED HIRES, EVENT-STUDY

(A) PROFESSIONAL OCCUPATION



(B) WHITE-COLLAR NON-PROFESSIONAL



Notes: The dependent variable in panel (a) is an indicator that takes a value of 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 takes a value of one if a worker was employed in a white-collar non-professional occupation. 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 estimate of event-study specifications as described in equation 2. The figure shows the estimated coefficients around 90 and 95% confidence intervals.

Online Appendix-Not for Publication

A Linking the Official Registers to the Census

Our linking strategy uses information on employees' names and place of birth. Unlike census-to-census links, we lack precise information on an individual's age that could be use to disambiguate between matches with similar names and places of birth.[75] In addition, we also lack direct information on an individual's gender (other than the information contained in names). As a result of this limitation, we are only able to uniquely identify individuals who are unique based on their combination of place of birth and full name.

Our linking 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 Register, search for a potential match in the Census. Potential matches are individuals who:

   (a) Report the same place of birth (states for the US born, country for foreigners). We exclude observations in the official registers which lack 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 65 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 aged 0 to 35 in 1850). This restriction is aimed to capture the fact that government was unlikely to employ both very young and very old individuals.

   (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 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

[75] We can however use the fact that individuals are not expected to work at very young and very old ages, which helps with disambiguation in some cases. Specifically, we assume that individuals working in the federal government are between the ages of 18 and 65.

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 that we match them to in the Census (of course, we do not use the place of residence as a criteria for matching).

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

Panel (a) of Figure A12 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 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.

This figure illustrates the trade-off between type 1 and type 2 errors (or "precision" and "recall") in the case of 1881 register to 1880 census links. 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 people (low "recall"). For the baseline analysis, we chose a combination of cutoffs that gives a balanced weight to precision and recall. Specifically, we choose 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).[76] Maximizing this function using the 1881 Register-1880 Census links leads to a choice of $c_1 = 0.7$ and $c_2 = 0.7$.

Figure A13 shows the proportion of individuals that we match to at least one census (and to at least 2, 3 and 4, respectively) by register year when using our baseline choice of parameters. 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 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. In all cases, the figures

[76] $F_1 = 2\frac{precision * recall}{precision + recall}$.

show that we are more likely to match male than female employees.

Because the first population census listing free 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[77] at the time they worked in government. The Registers themselves do not include information on ages, but we can obtain this information when linking either the 1870 census to the 1871 register or the 1880 census to the 1881 register). By 1871, about 35% of the employees were 40 years old or more. 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.[78] Similarly, we also expect a lower proportion of individuals in later register 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, particularly those employed in later years).

Representativeness of linked data. In our main analysis, we assess how the social backgrounds of bureaucrats changed with the introduction of exams. Our sample in this analysis *only* includes employees of the US federal government who were successfully linked to at least one observation in the census. Specifically, we compare the characteristics of bureaucrats in positions subject to exams to the characteristics of those in exempted positions, before and after the reform. Hence, for our analysis to be biased by selection it would need to be the case that selection into linkage changed *differentially* for individuals in positions subject to exams after the reforms. This is unlikely because our linking procedure is exactly the same throughout all sample years and across all positions within government.

To further alleviate this concern, 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.

Next, Table A2 shows that our result on the share of foreign-born workers (which *does not* require the linked data since we can observe birthplace directly from the Registers) is very similar when we estimate it using the smaller 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).

[77] 18+(1871-1850).

[78] Individuals in later register years are easier to find as a child in at least one census. 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).

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

	(1) At least 1 match	(2) N. of matches
Exam	-0.0335 (0.0332)	-0.00903 (0.0186)
Year FE	Yes	Yes
Position FE	Yes	Yes
App. State FE	Yes	Yes
Observations	23199	23199

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in column 1 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 variables in column 2 is instead the total number of censuses to which an employee is matched to. Standard errors are clustered at the position level.

TABLE A2: EFFECTS OF EXAMS ON THE SHARE OF FOREIGN-BORN EMPLOYEES

	(1) Full Sample	(2) Linked Sample	(3) Linked Sample, Reweighted
Exam X After	-0.0464*** (0.0145)	-0.0419** (0.0176)	-0.0508** (0.0209)
Year FE	Yes	Yes	Yes
Position FE	Yes	Yes	Yes
App. State FE	Yes	Yes	Yes
Observations	22223	7892	7852

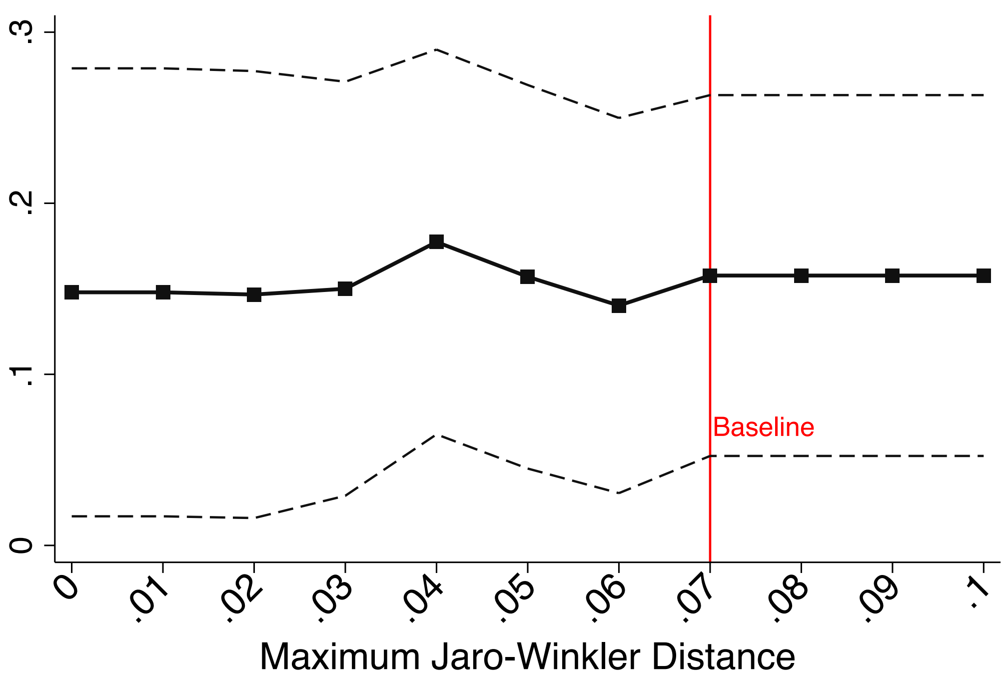
Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is an indicator that takes a value of one in an employee is foreign born. The sample in column 1 includes all employees in our target baseline sample. The sample in column 2 includes only those employees that we successfully link to an observation in the census. Standard errors are clustered at the position level.

Table A3: Robustness to Reweighting the Linked Sample

	Unweighted		Weighted	
	(1)	(2)	(3)	(4)
Exam X After	0.185***	0.173***	0.207***	0.192***
	(0.0518)	(0.0531)	(0.0536)	(0.0535)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
App. State FE	No	Yes	No	Yes
Observations	2944	2944	2890	2890

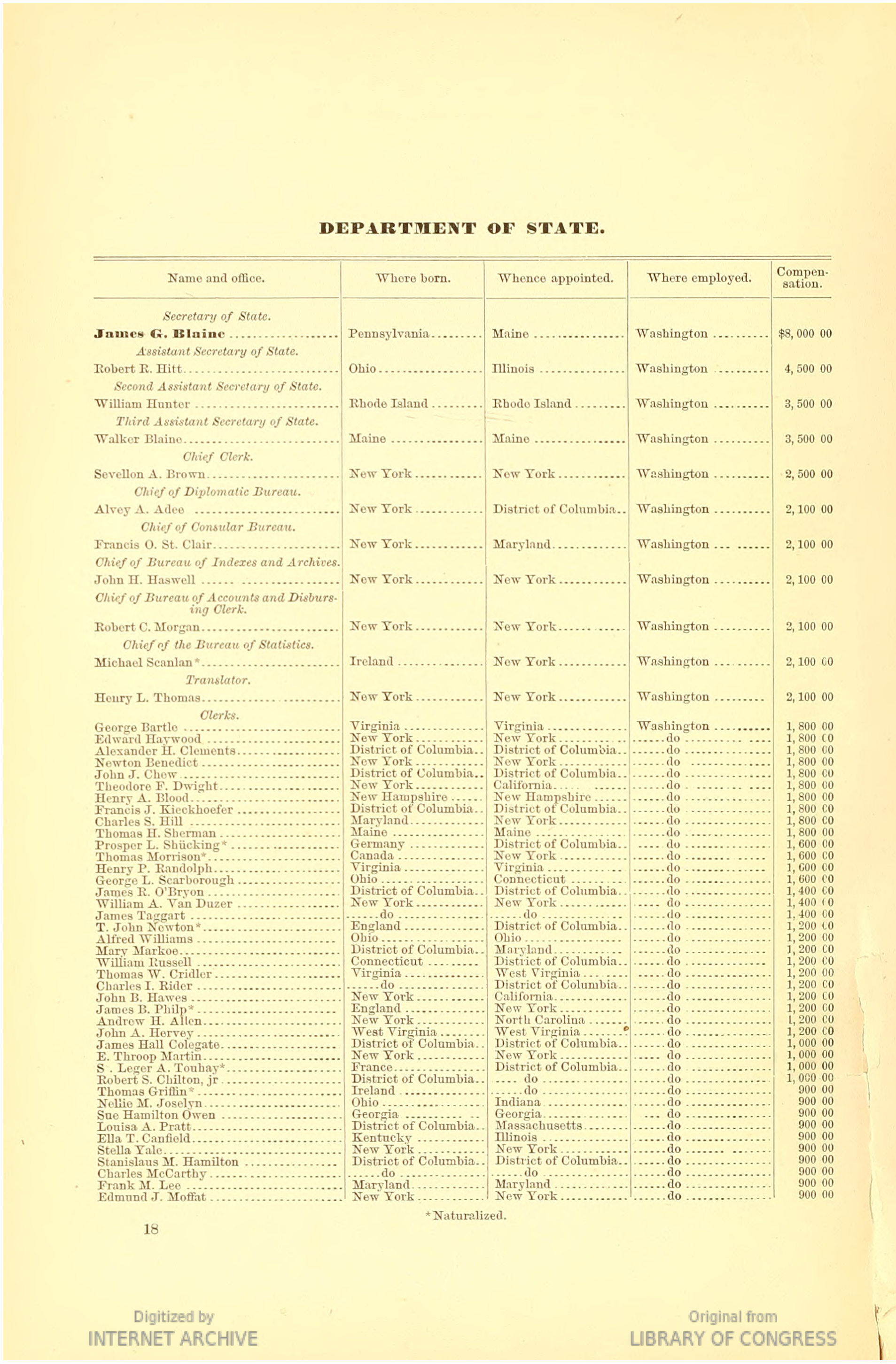
Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' social class. The index is based on the approach in Kling *et al.* (2007), and it is constructed by combining information on parental wealth rank, parental occupations, parental birthplace and race. The table shows the sensitivity of the estimates to reweghting the data to account for differences in the characteristics matched to the census.

Figure A1: Robustness to Alternative Linking Cutoffs



Notes: This figure shows the estimated effect of the reform on the summary index of employees social backgrounds (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: A Jaro-Winkler distance of zero correspond to two identical strings, whereas a distance of one correspond to two strings with no common characters. The red vertical bar corresponds to the cutoff used in the baseline approach.

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.				
George Bartle	Virginia	Virginia	Washington	1,800 00
Edward Haywood	New York	New York	do	1,800 00
Alexander H. Clements	District of Columbia	District of Columbia	do	1,800 00
Newton Benedict	New York	New York	do	1,800 00
John J. Chew	District of Columbia	District of Columbia	do	1,800 00
Theodore F. Dwight	New York	California	do	1,800 00
Henry A. Blood	New Hampshire	New Hampshire	do	1,800 00
Francis J. Kieckhoefer	District of Columbia	District of Columbia	do	1,800 00
Charles S. Hill	Maryland	New York	do	1,800 00
Thomas H. Sherman	Maine	Maine	do	1,800 00
Prosper L. Shücking*	Germany	District of Columbia	do	1,600 00
Thomas Morrison*	Canada	New York	do	1,600 00
Henry P. Randolph	Virginia	Virginia	do	1,600 00
George L. Scarborough	Ohio	Connecticut	do	1,600 00
James R. O'Bryon	District of Columbia	District of Columbia	do	1,400 00
William A. Van Duzer	New York	New York	do	1,400 00
James Taggart	do	do	do	1,400 00
T. John Newton*	England	District of Columbia	do	1,200 00
Alfred Williams	Ohio	Ohio	do	1,200 00
Mary Markoe	District of Columbia	Maryland	do	1,200 00
William Russell	Connecticut	District of Columbia	do	1,200 00
Thomas W. Cridler	Virginia	West Virginia	do	1,200 00
Charles I. Rider	do	District of Columbia	do	1,200 00
John B. Hawes	New York	California	do	1,200 00
James B. Philp*	England	New York	do	1,200 00
Andrew H. Allen	New York	North Carolina	do	1,200 00
John A. Hervey	West Virginia	West Virginia	do	1,200 00
James Hall Colegate	District of Columbia	District of Columbia	do	1,000 00
E. Throop Martin	New York	New York	do	1,000 00
S. Leger A. Touhay*	France	District of Columbia	do	1,000 00
Robert S. Chilton, jr	District of Columbia	do	do	1,000 00
Thomas Griffin*	Ireland	do	do	900 00
Nellie M. Joselyn	Ohio	Indiana	do	900 00
Sue Hamilton Owen	Georgia	Georgia	do	900 00
Louisa A. Pratt	District of Columbia	Massachusetts	do	900 00
Ella T. Canfield	Kentucky	Illinois	do	900 00
Stella Yale	New York	New York	do	900 00
Stanislaus M. Hamilton	District of Columbia	District of Columbia	do	900 00
Charles McCarthy	do	do	do	900 00
Frank M. Lee	Maryland	Maryland	do	900 00
Edmund J. Moffat	New York	New York	do	900 00

*Naturalized.

18

Notes: This figure shows an example page corresponding to the 1881 edition of the "Official Registers of the United States" (Department of the Interior, 1871-1893). The page lists employees of the Department of State in Washington, DC.

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.
4. *Seated.* Placed on a seat; fixed; settled.
5. *Dyeing.* The act of staining or coloring cloth.
6. *Dying.* Losing life; expiring.
7. *Mutable.* Changeable; capable of change.
8. *Diffidence.* The state of being diffident.
9. *Felicitate.* To make happy; to delight.
10. *Hesitate.* To pause; to delay.
11. *Commissary.* One delegated to some trust; an officer.
12. *Comical.* Droll; odd; ridiculous.
13. *Memorize.* To commit to memory.
14. *Disguise.* To mask; to muffle; to conceal the appearance of.
15. *Agreed.* Settled by consent.
16. *Impede.* To hinder; to delay; to retard.
17. *Perseverance.* Persistence in an undertaking; constancy.
18. *Interference.* A clashing; interposition; opposition.
19. *Consciously.* Knowingly.
20. *Guidance.* The act of guiding; direction.

(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

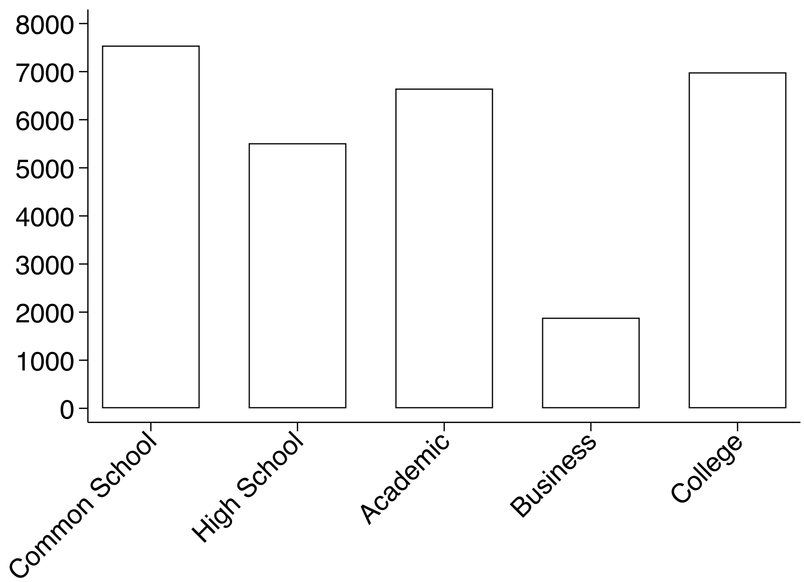
SIXTH SUBJECT.—*Meteorology.*

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?
Question 6. Give Loomis's explanation of the formation of dew.
Question 7. State the conditions that favor the formation of hoar frost.
Question 8. State the accepted classification of clouds.
Question 9. Define a storm.
Question 10. In what respect do cyclones or hurricanes differ from tornadoes?

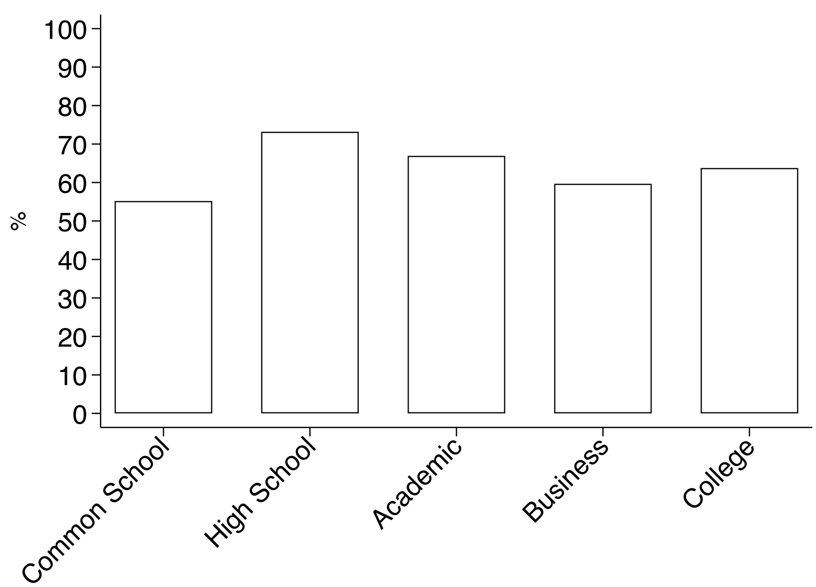
Notes: The panels in this figure show example questions of the civil service exam. The figure in panel (a) shows an example question of the orthography exam. This exam was required for all applicants taking either the "general" (for clerks) or "limited" (for copyists) examinations. The figures in panels (b), (c) and (d) show example questions of the penmanship, copying and arithmetic exams. All of these exams were also required for applicants taking the general or the limited examinations. The figure in panel (e) shows an example question for the special exam for "meteorological clerks" in the Department of Agriculture.

FIGURE A4: TOTAL NUMBER OF APPLICANTS AND EXAM PASSING RATES, BY EDUCATIONAL BACKGROUND 1887-1893

(A) NUMBER OF APPLICANTS



(B) PASSING RATE

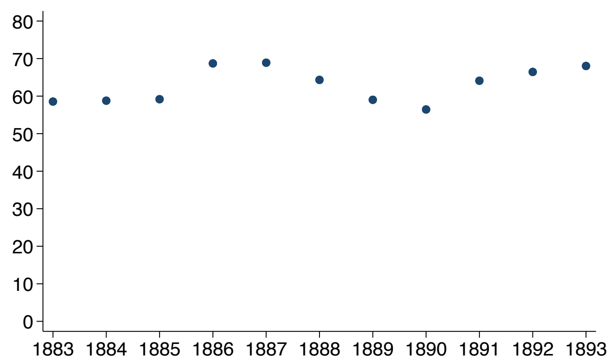


Notes: Panel (a) shows the number of applicants to the "Classified Departmental Service" in DC, by applicants' educational background. Panel (b) shows the fraction of such applicants who obtained a passing grade. These figures correspond to applicants who completed exams from 1886 to 1893, and are based on data from the "Annual Reports of the Civil Service Commission" (Commission, 1897).

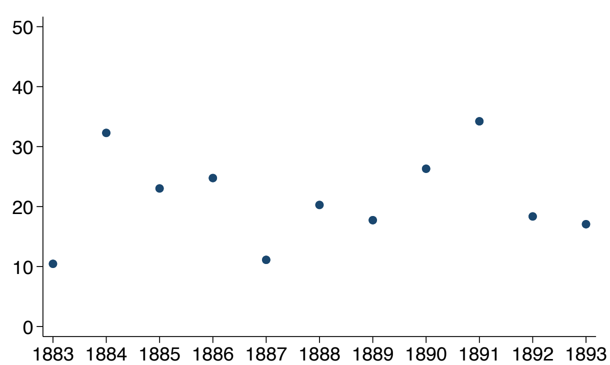
Figure A5: Total Number of Applicants and Exam Passing Rates, by Year

(a) Number of Applicants

(b) Passing Rate

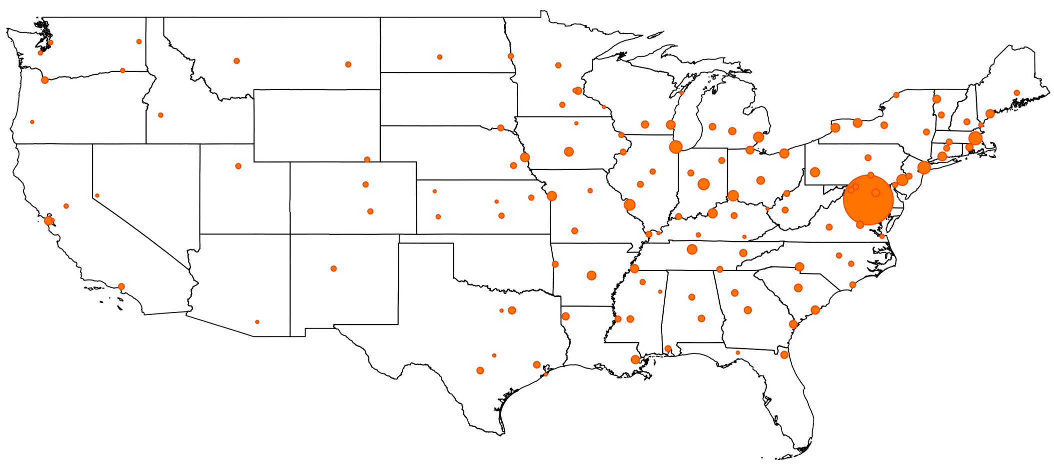


(c) Appointment Rate



Notes: This figure shows the total number of applicants (Panel (a)), the share of applicants who obtained the minimum qualifying score (Panel (b)), and the share of appointed employees to the classified departmental service (Panel (c)), based on data from the reports of the Civil Service Commission (United States Civil Service Commission, 1893).

FIGURE A6: 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 points are drawn in proportion to the number of exams that took place in a given location. The largest point corresponds to Washington, DC, where there were more than 300 exams in this time period.

Figure A7: Calendar of Classified Departmental Service Examinations, 1886-1887

TABLE 1.—*Showing places and dates of examinations for the classified departmental service, January* 16, 1886, *to June* 30, 1887, *both inclusive.*

ALABAMA.

Montgomery, May 8, 1886.
Montgomery, December 20, 1886.
Mobile, December 10, 1886.

ARKANSAS.

Little Rock, May 4, 1886.
Little Rock, February 12, 1887.
Fort Smith, February 15, 1887.

CALIFORNIA.

San Francisco, April 20, 1886.
San Francisco, September 18, 1886.

COLORADO.

Denver, April 26, 1887.
Pueblo, May 11, 1887.

CONNECTICUT.

New Haven, August 5, 1886.
New Haven, May 5, 1887.
Hartford, May 19, 1887.

DISTRICT OF COLUMBIA.

January 25, 30, 1886.
February 8, 13, 18, 20, 24, 25, 27, 1886.
March 2, 6, 13, 17, 27, 1886.
April 2, 10, 13, 17, 19, 24, 1886.
May 8, 15, 19, 22, 29, 1886.
June 5, 8, 12, 19, 23, 26, 28, 1886.
July 2, 3, 8, 10, 12, 13, 14, 17, 19, 20, 22, 24, 26, 30, 31, 1886.
August 5, 6, 7, 11, 14, 19, 21, 28, 30, 31, 1886.
September 4, 9, 11, 18, 25, 1886.
October 9, 16, 23, 30, 1886.
November 6, 9, 13, 20, 27, 1886.
December 4, 8, 11, 16, 17, 18, 21, 23, 27, 1886.
January 3, 8, 13, 15, 22, 26, 29, 31, 1887.
February 5, 12, 19, 25, 26, 1887.
March 1, 4, 5, 6, 12, 17, 19, 26, 31, 1887.
April 2, 9, 16, 30, 1887.
May 7, 9, 14, 19, 21, 28, 1887.
June 4, 7, 9, 11, 22, 25, 1887.

FLORIDA.

Jacksonville, December 6, 1886.
Tallahassee, December 8, 1886.

GEORGIA.

Atlanta, May 11, 1886.
Atlanta, December 22, 1886.
Savannah, March 25, 1886.
Savannah, December 4, 1886.

IDAHO.

Boisé City, May 3, 1887.

ILLINOIS.

Bloomington, April 13, 1887.
Cairo, February 22, 1886.
Chicago, February 6, 1886.
Chicago, April 6, 16, 1886.
Chicago, August 5, 1886.
Chicago, September 29, 1886.

ILLINOIS—continued.

Chicago, April 12, 1887.
Springfield, May 3, 1886.

INDIANA.

Evansville, February 24, 1887.
Fort Wayne, March 18, 1887.
Indianapolis, April 6, 1886.
Indianapolis, May 5, 1886.
Indianapolis, August 7, 19, 1886.
Indianapolis, April 9, 1887.
La Fayette, April 11, 1887.

IOWA.

Davenport, April 15, 1887.
Des Moines, April 24, 1886.
Des Moines, April 18, 1887.
Mason City, April 19, 1887.

KANSAS.

Newton, May 14, 1887.
Topeka, May 17, 1887.

KENTUCKY.

Lexington, February 3, 1887.
Louisville, April 6, 1886.
Louisville, June 29, 1886.
Louisville, February 5, 1887.

LOUISIANA.

New Orleans, July 16, 1886.
New Orleans, December 13, 1886.

MAINE.

Portland, May 24, 1886.
Portland, November 18, 1886.
Portland, May 12, 1887.

MARYLAND.

Baltimore, January 22, 1887.
Hagerstown, May 12, 1887.

MASSACHUSETTS.

Boston, February 16, 1887.
Boston, April 6, 1886.
Boston, July 22, 1886.
Boston, August 4, 1886.
Boston, May 9, 1887.
Springfield, May 18, 1887.

MICHIGAN.

Detroit, April 6, 24, 1886.
Detroit, August 3, 1886.
Detroit, March 11, 1887.
Grand Rapids, March 16, 1887.
Lansing, March 14, 1887.

MINNESOTA.

Mankato, April 21, 1887.
Minneapolis, April 6, 22, 1886.
Saint Paul, April 23, 1886.

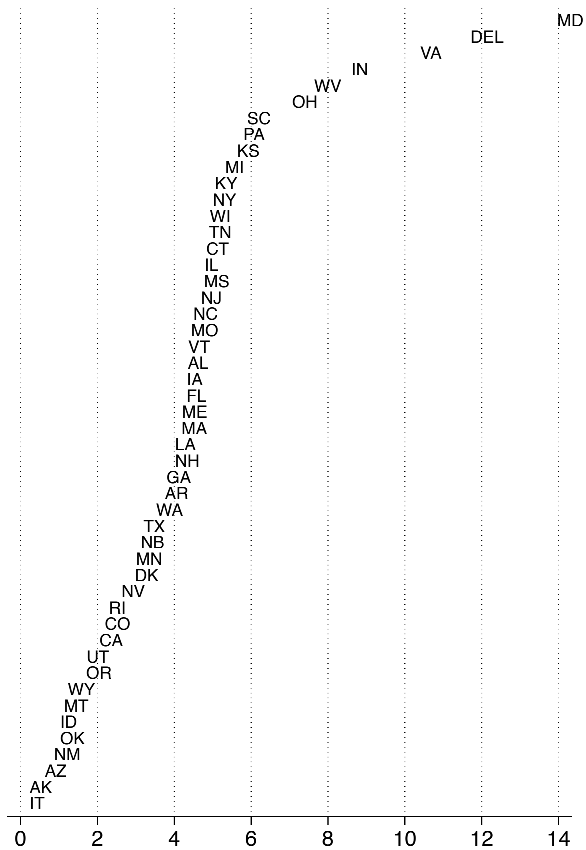
MISSISSIPPI.

Aberdeen, December 17, 1886.
Jackson, May 6, 1886.
Jackson, December 15, 1886.

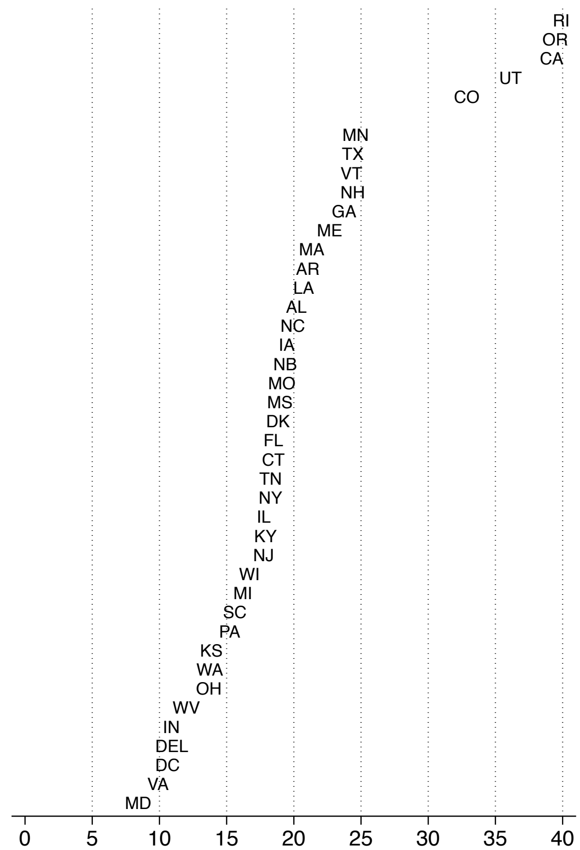
Notes: This shows an example calendar of examinations to join the classified departmental service. The calendar corresponds to the 1887 fiscal year.

FIGURE A8: TOTAL NUMBER OF APPLICANTS AND SHARE APPOINTED, BY STATE

(A) APPLICANTS EVERY 10,000 POP.



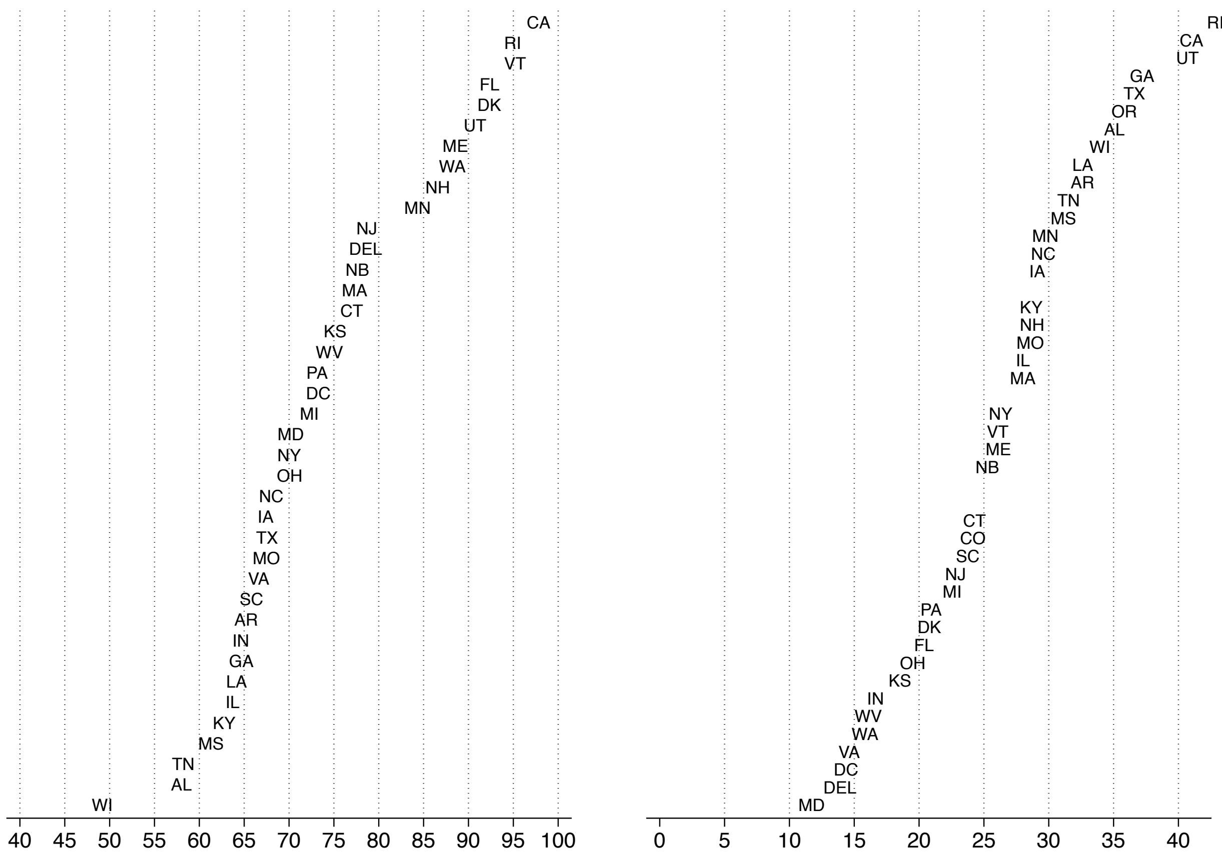
(B) NUMBER APPOINTED PER 100 APPLICANTS



Notes: Panel (a) shows the per capita number of applicants to the classified departmental service for each US state. Panel (b) shows the per capita number of appointed employees to the classified departmental service.

FIGURE A9: EXAM PASSING RATES AND SHARE APPOINTED, BY STATE

(A) EXAM PASSING RATE **(B) NUMBER APPOINTED PER 100 PASSING**



Notes: Panel (a) shows the passing rate for applicants to the classified departmental service for each US state. Panel (b) shows the share of appointed employees among those who obtained a passing grade.

FIGURE A10: EMPLOYEES APPOINTED THROUGH EXAMS TO THE CLASSIFIED DEPARTMENTAL SERVICE

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

Name.	Legal residence.	Appointments to each state.	Whole number of appointments.	Department to which certified.	Grade for which certified.	Date of probationary 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 from the Civil Service Commission Reports (Commission, 1886) listing employees appointed to the classified departmental service.

FIGURE A11: POSITIONS SUBJECT TO EXAMS IN THE TREASURY DEPARTMENT

IN THE TREASURY DEPARTMENT AT WASHINGTON.

[June 30, 1892.]

CLASSIFIED SERVICE.

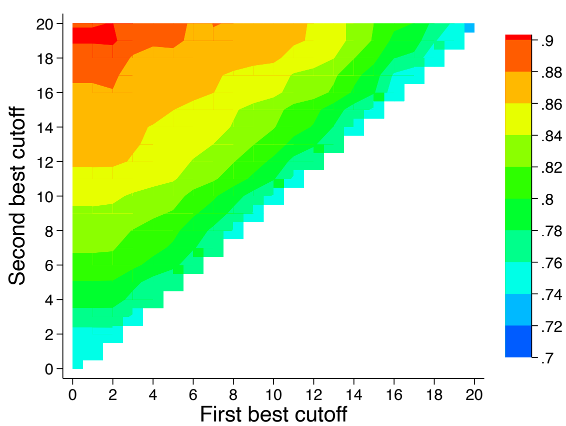
	Yearly salary.	Aggregate yearly salary.
I. *Places classified and excepted from examination.*		
1 adjuster of accounts	$2, 000	$2, 000. 00
1 adjuster	1, 500	1, 500. 00
1 assayer	2, 200	2, 200. 00
1 assistant and chief clerk	2, 500	2, 500. 00
1 assistant cashier	3, 200	3, 200. 00
1 assistant in charge 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
1 cabinetmaker	720	720. 00
1 chief clerk	3, 000	3, 000. 00
2 chief clerks	2, 500	5, 000. 00
1 chief clerk	2, 400	2, 400. 00
1 chief clerk	2, 250	2, 250. 00
2 chief clerks	2, 000	4, 000. 00
1 chief clerk	1, 800	1, 800. 00
1 cashier	3, 600	3, 600. 00
1 chief of Bureau of Engraving and Printing	4, 500	4, 500. 00
1 chief of division	3, 500	3, 500. 00
2 chiefs of divisions	3, 000	6, 000. 00
2 chiefs of divisions	2, 750	5, 500. 00
16 chief of divisions	2, 500	40, 000. 00
6 chiefs of divisions	2, 250	13, 500. 00
4 chiefs of divisions	2, 200	8, 800. 00
13 chiefs of divisions	2, 100	27, 300. 00
36 chiefs of divisions	2, 000	72, 000. 00
1 chief of division	1, 800	1, 800. 00
1 chief of division	1, 400	1, 400. 00
1 chief of division at $9.60 per diem	....	3, 004. 80
1 clerk to Treasurer	1, 800	1, 800. 00
1 clerk to Secretary	2, 400	2, 400. 00
1 clerk to disbursing clerk	1, 200	1, 200. 00
1 coin clerk	1, 400	1, 400. 00
1 deputy head of bureau	3, 200	3, 200. 00
1 deputy head of bureau	2, 800	2, 800. 00
1 deputy head of bureau	2, 000	2, 000. 00
2 disbursing clerks	2, 500	5, 000. 00
2 disbursing clerks	2, 000	4, 000. 00
1 distributer of stock	1, 252	1, 252. 00
1 distributer of stock	1, 200	1, 200. 00
1 electrotyper and photographer	1, 800	1, 800. 00
4 elevator conductors	720	2, 880. 00
1 examiner	2, 500	2, 500. 00
1 foreman of bindery, at $5 per diem	....	1, 825. 00
1 foreman of laborers	1, 000	1, 000. 00
1 foreman of cabinet shop	1, 500	1, 500. 00
1 Government actuary	1, 800	1, 800. 00
1 inspector of furniture	3, 000	3, 000. 00
1 mechanician	1, 250	1, 250. 00
1 plate printer	1, 600	1, 600. 00
4 plate printers	1, 000	4, 000. 00
2 plate printers	900	1, 800. 00
366 plate printers, piece rates	....	*457, 473. 09
4 plate printer's helpers	700	2, 800. 00
3 private secretaries to assistant secretaries	1, 800	5, 400. 00
1 skilled laborer	840	840. 00
3 skilled laborers	720	2, 160. 00
1 superintendent stamp vault	2, 000	2, 000. 00
1 superintendent national currency	3, 500	3, 500. 00
1 superintendent national bank redemption agency	3, 500	3, 500. 00
3 tellers	2, 500	7, 500. 00
1 topographer and hydrographer	1, 800	1, 800. 00
1 vault clerk	2, 500	2, 500. 00
39 engravers, various salaries	....	* 68, 041. 80
2 apprentices to engraving	320	640. 00
1 apprentice to engraving	780	780. 00
2 apprentices to pressmen	320	640. 00

* The amount of compensation paid them during the fiscal year 1892.

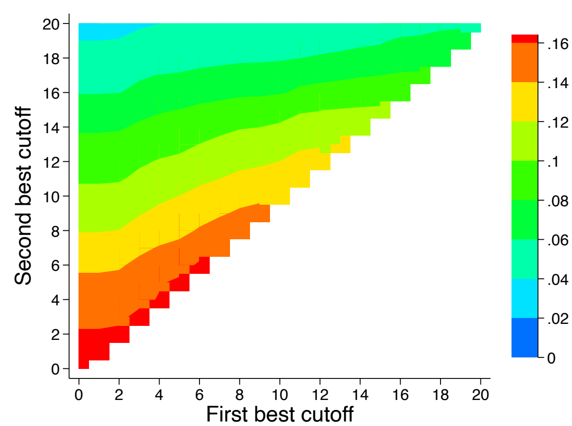
Notes: This figure shows an example page from the Civil Service Commission Reports (Commission, 1886) listing the positions that were subject to exams in the Department of the Treasury.

Figure A12: Positive Prediction Value and True Positive Rate

(a) % matches with matching place of residence



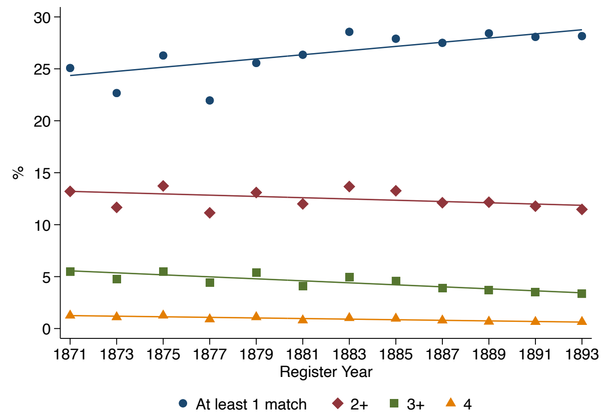
(b) % observations with matching place of residence



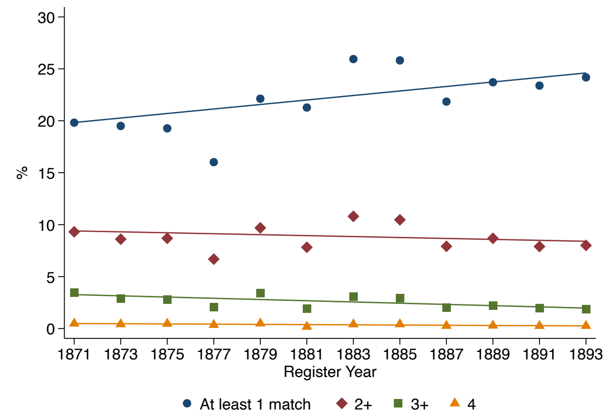
Notes: Panel (a) shows, out of all the individuals that we deem as a match, the proportion of individuals who are matched to someone living in a matching county of residence (based on their 1881 place of employment according to the Official Registers). Panel (b) shows, out of all the individuals that we attempt to match, the proportion of individuals who are matched to someone living in a matching county of residence. The sample is restricted to those workers initially employed in Washington, DC. These two statistics are drawn as a function of the parameters that we use to determine whether or not we consider an observation as a match.

Figure A13: Matching Rates, by Register Year

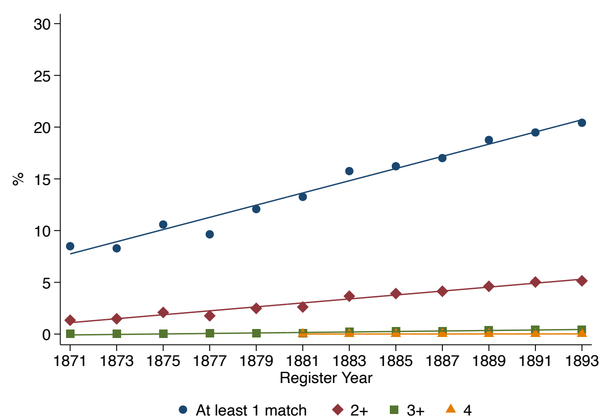
(a) % Matched, Male Employees



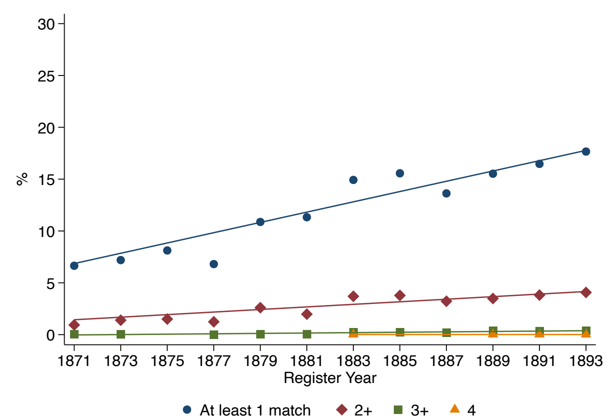
(b) % Matched, Female Employees



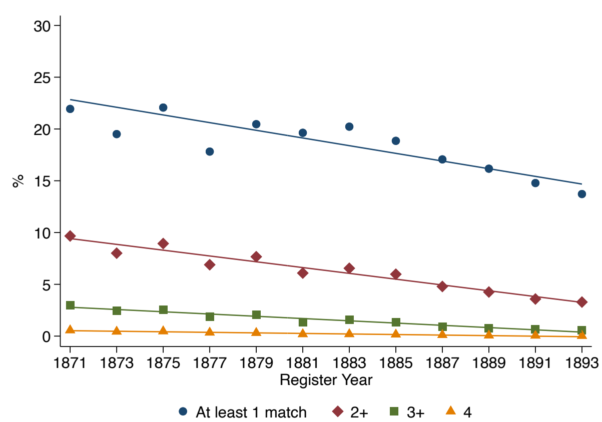
(c) % Linked as < 18 years old, Males



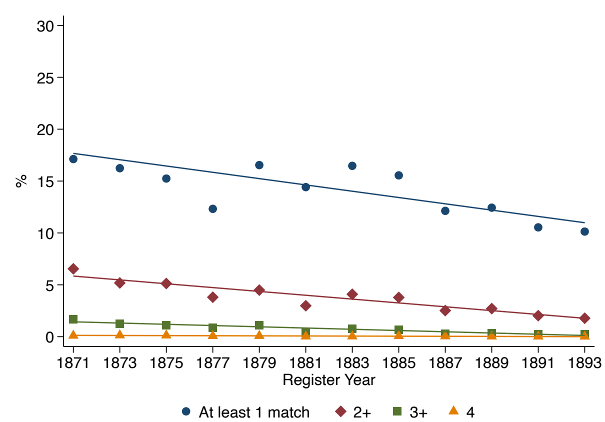
(d) % Linked as < 18 years old, Females



(e) % Linked as 18+ years old, Males



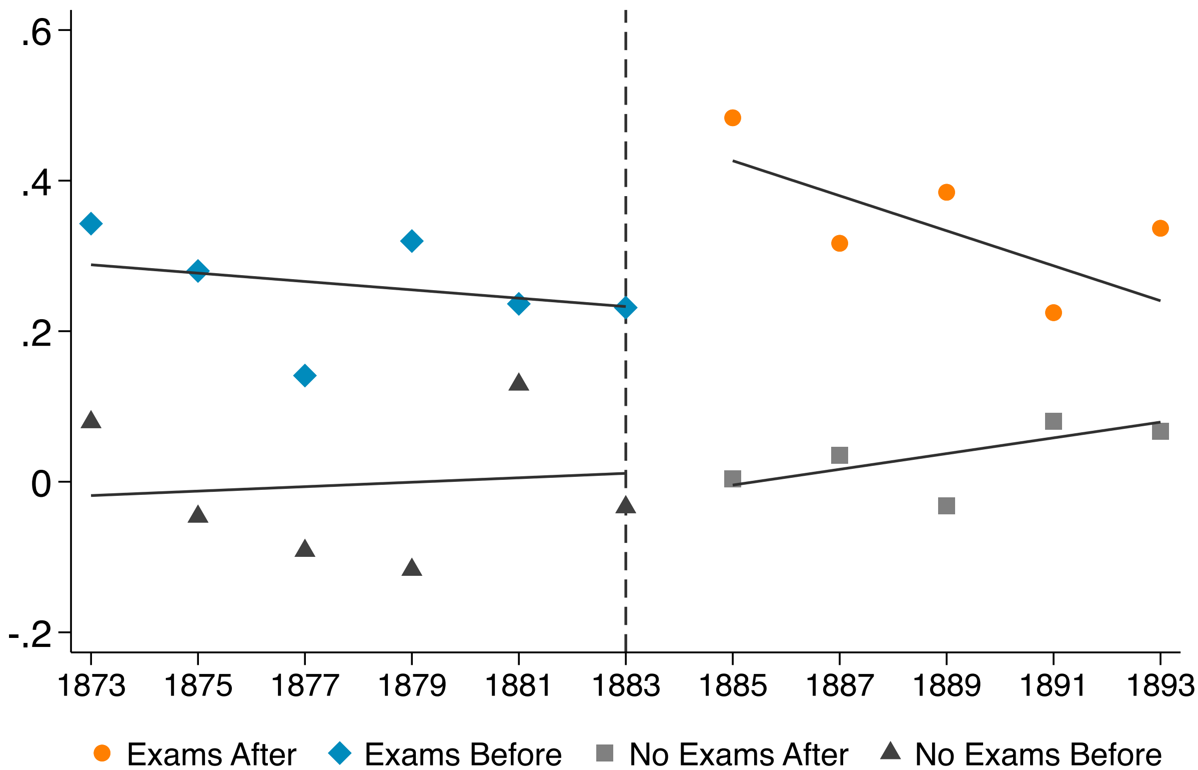
(f) % Linked as 18+ years old, Females



Notes: Panel (a) in this figure shows the proportion of individuals in each register that we match to at least one, at least two, at least three or exactly four censuses in our baseline sample. Panel (b) shows the corresponding proportion for individuals that we find when they are less than 18 years old, whereas panel (c) shows the proportion that we find when they are 18 or more. In all cases, we only include matches to population censuses that took place before the corresponding register.

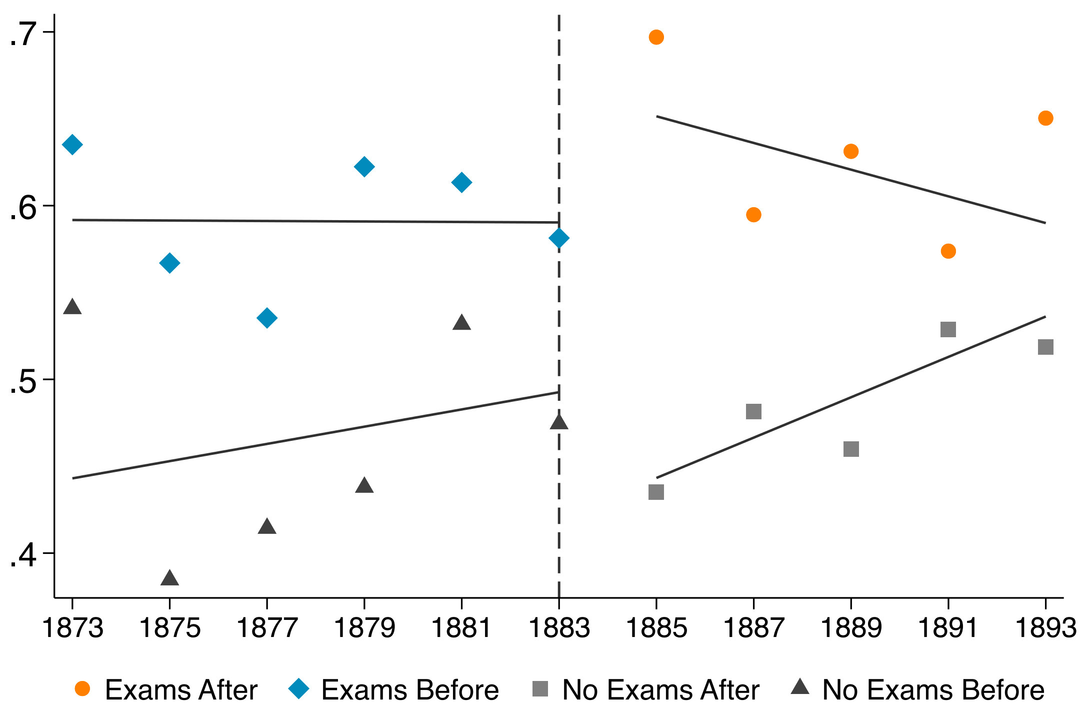
B Additional Results

FIGURE B1: SUMMARY INDEX OF SOCIAL CLASS



Notes: This figure shows the average value of the index for workers in positions subject and non-subject to exam, by hiring year. Positions are coded as subject to exam if they required an exam after 1883.This figure shows the average parental wealth rank of workers in positions subject and non-subject to exams from 1873 to 1893.

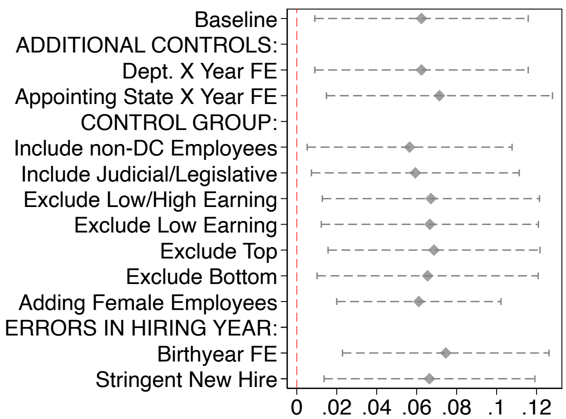
FIGURE B2: PARENTAL WEALTH RANKS



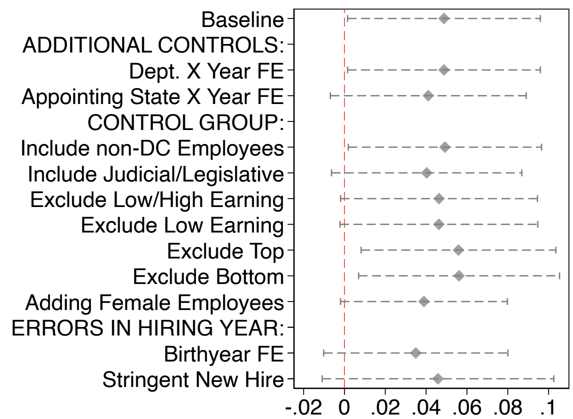
Notes: Panel (a) shows the average value of the index for workers in positions subject and non-subject to exam, by hiring year. Positions are coded as subject to exam if they required an exam after 1883.This figure shows the average parental wealth rank of workers in positions subject and non-subject to exams from 1873 to 1893.

Figure B3: Robustness

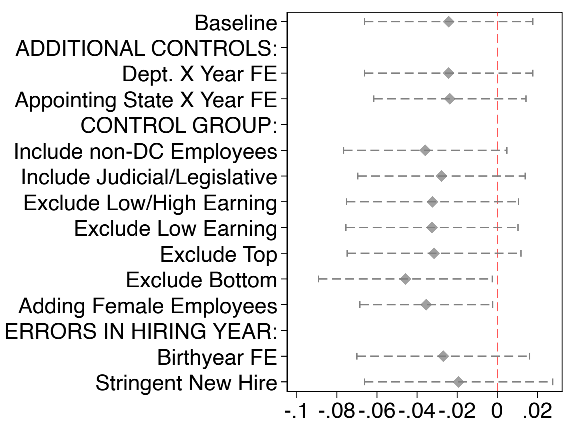
(a) Father Wealth Rank



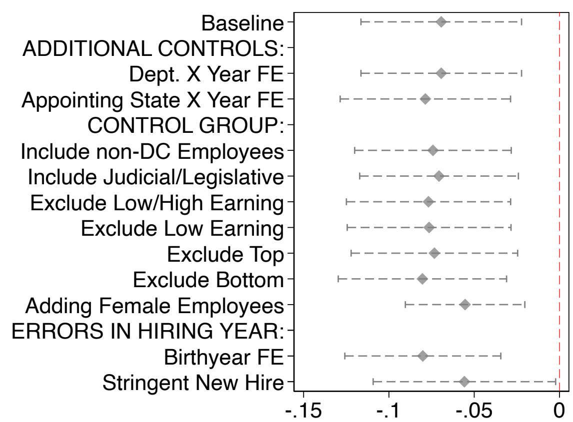
(b) Father Professional



(c) Father Unskilled

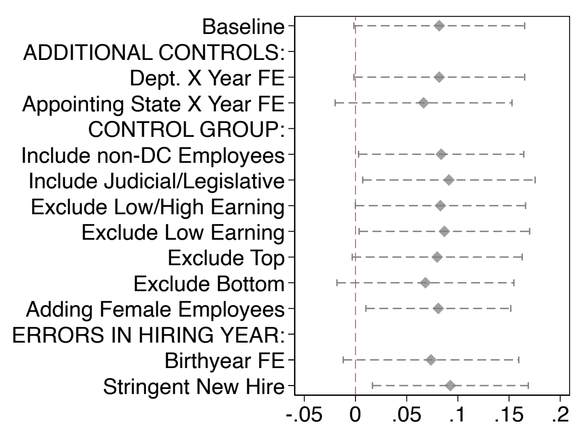


(d) Immigrant Parents



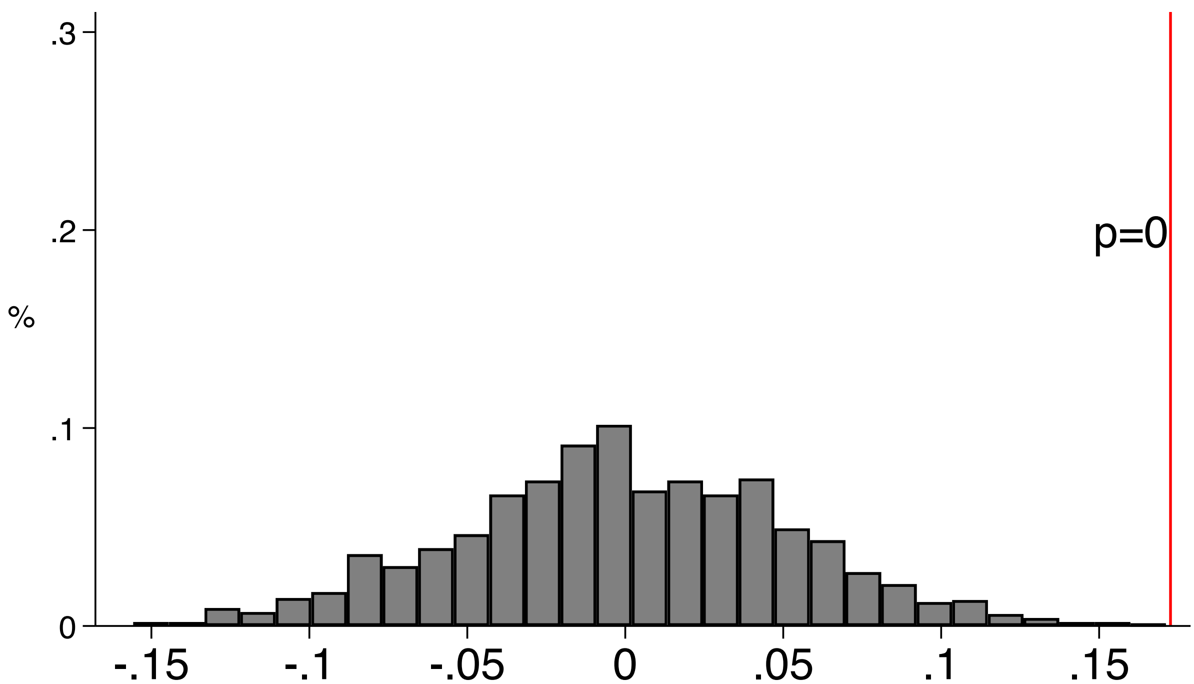
(e) Father Index

(f) Own Occupation Professional



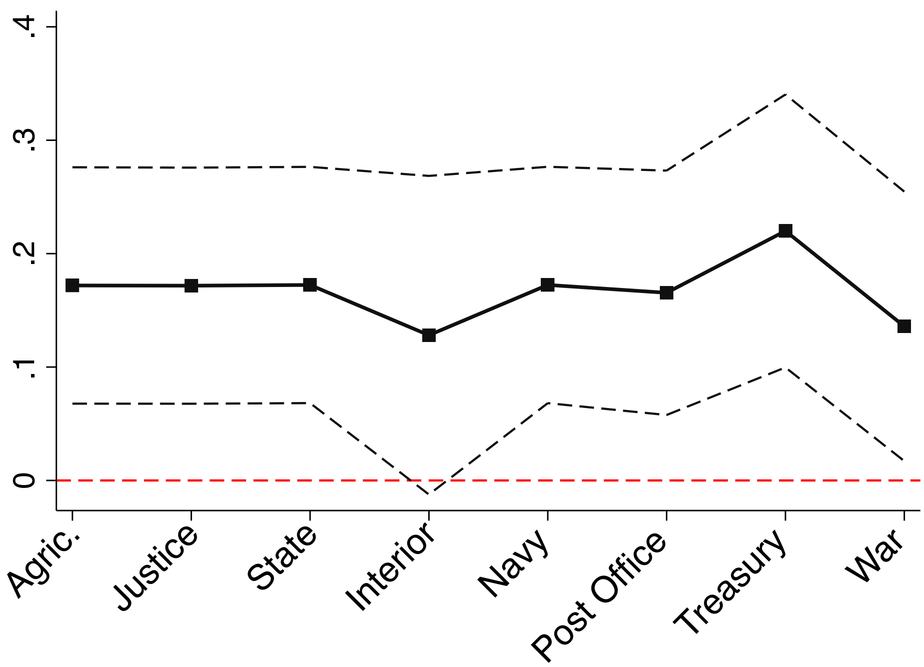
Notes: This figure presents the sensitivity of our difference-in-differences estimates to a number of alternative specifications and samples. The top row in each figure presents our baseline estimate. In the rows under "Additional controls" we add additional control variables to our baseline specification: (1) Department times hiring year fixed effects, and (2) State of residence at the time of appointment times 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, (3) excluding from the control group employees making less than or more than, (4) excluding from the control group workers who were exempted due to their low salaries, (5) excluding from the control group those who were exempted due to being in hierarchical positions. In the row under "Errors in hiring year", we assess the sensitivity of our results to potential errors in identifying employees as new hires. Specifically, we add (1) birth year fixed effects and (2) use a more stringent definition of a new hire.

FIGURE B4: RANDOMIZATION INFERENCE



Notes: This figure shows the empirical distribution of estimated effects when we implement a randomization inference approach. In this exercise, we randomly select a treatment group of workers and estimate the "effects" of the reform using our baseline differences-in-differences model. We repeat this exercise 1,000 times and plot the empirical distribution of estimated effects. The vertical red line corresponds to our estimated effect when we use the actual set of treated employees. The outcome variable is the summary index of social class computed using the approach in Kling *et al.* (2007).

FIGURE B5: EXCLUDING ONE DEPARTMENT AT A TIME



Notes: This figure shows the sensitivity of the effects of the reform on the summary index of social class (computed using the approach in Kling *et al.* (2007)) to excluding workers from one executive department at a time. The y-axis shows the estimated effect of exams on average parental wealth rank, whereas the x-axis shows the excluded department. The estimated effects are plotted around a 95% confidence interval.

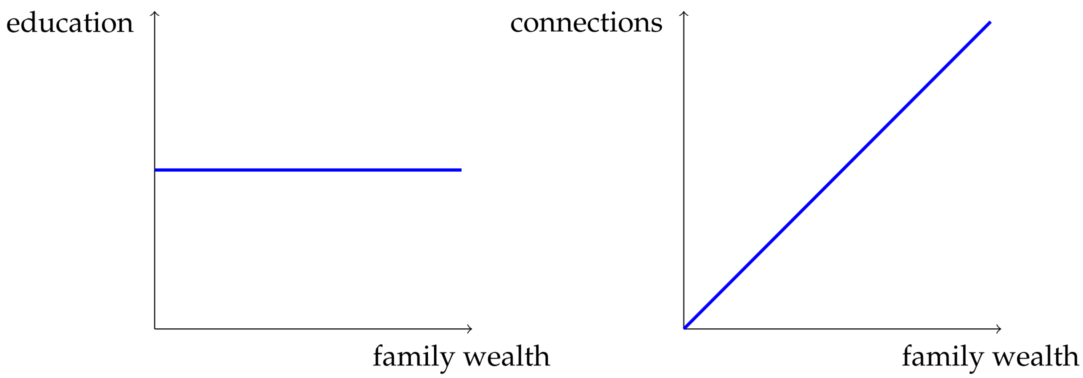
TABLE B1: SAMPLE CONSTRUCTION

	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
With Information on Parental Wealth	4590	10.79
With Information on Own Occupation	4990	11.73

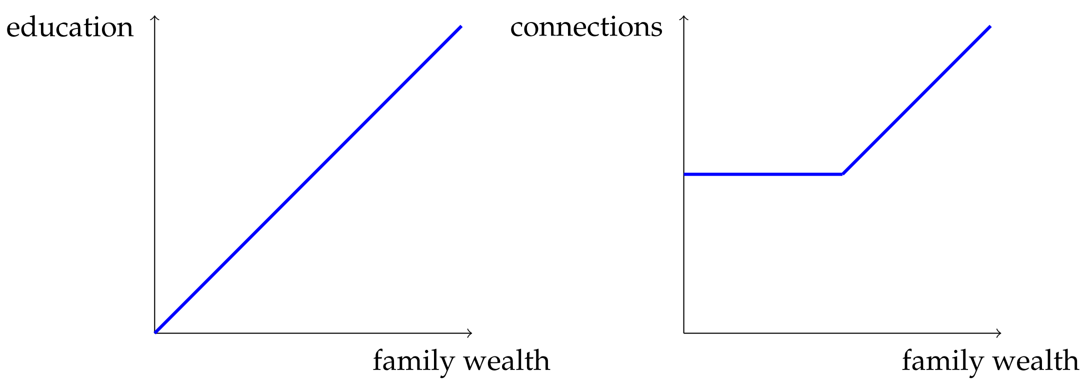
This table shows the construction of our baseline sample. We start from a list of employees who worked in the Executive Departments in DC in the 1873-1893 period. We then restrict this sample to those who are new hires (that is, those employees who were not listed in the Register of the previous year). The table then reports the fraction of of these individuals for whom we observe parental occupations, parental wealth and own occupation prior to joining the civil service.

FIGURE B6: AMBIGUOUS RELATIONSHIP BETWEEN EXAMS AND ECONOMIC MOBILITY

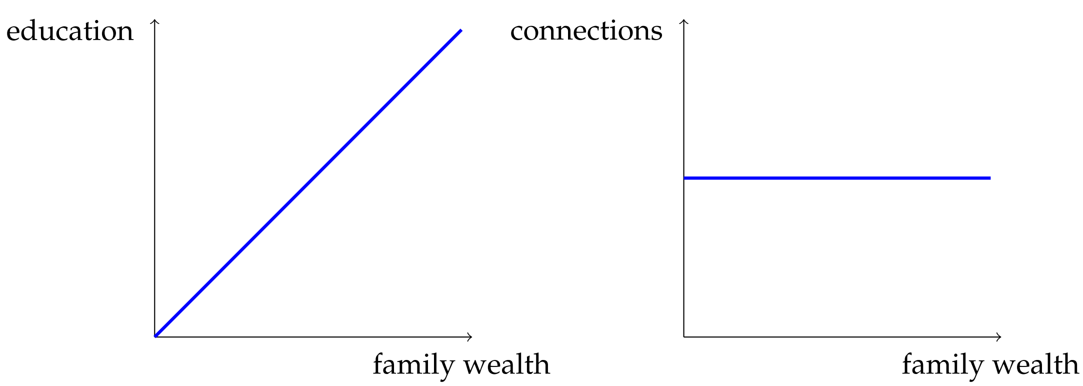
(A) EXAMS HELP THE "POOR"



(B) EXAMS HELP THE "MIDDLE"

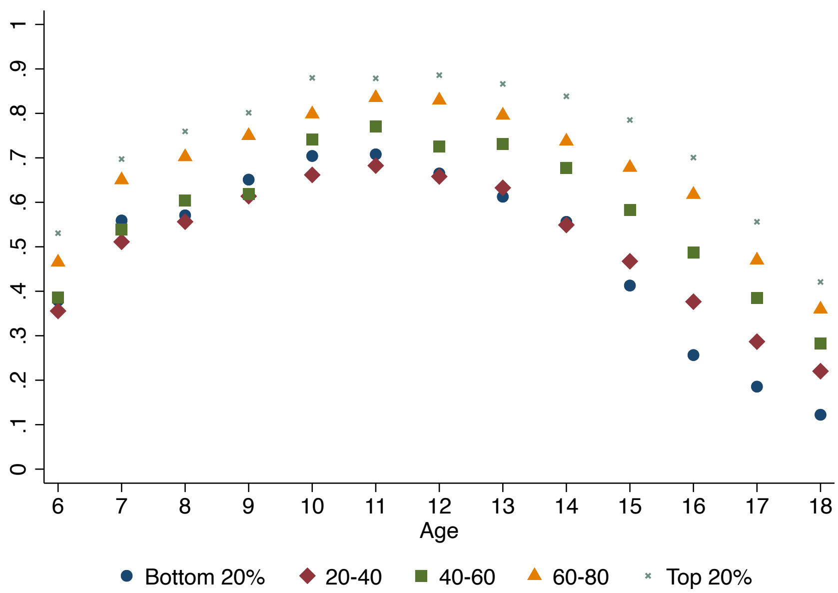


(C) EXAMS HELP THE "RICH"



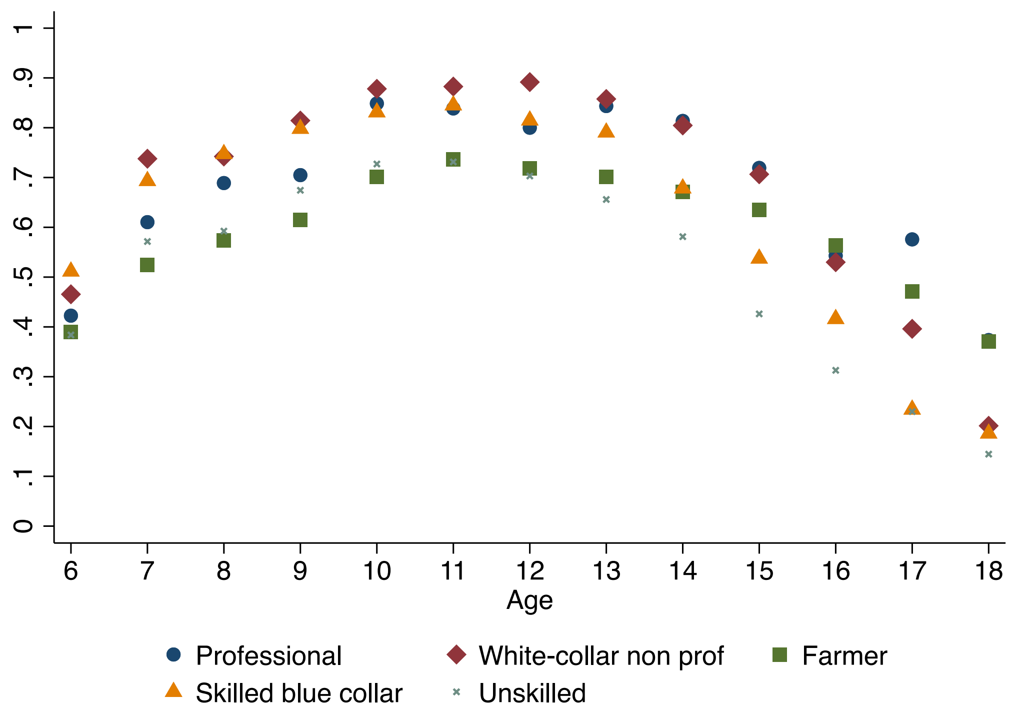
Notes: These figures illustrate the ambiguous relationship between the introduction of exams and the representation of workers from different social backgrounds. Each panel depicts a hypothetical relationship between family wealth and education and family wealth and connections. In our conceptual framework, workers are hired if they are above a certain threshold 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 B7: SCHOOL ATTENDANCE RATES BY AGE AND PARENTAL WEALTH QUINTILE



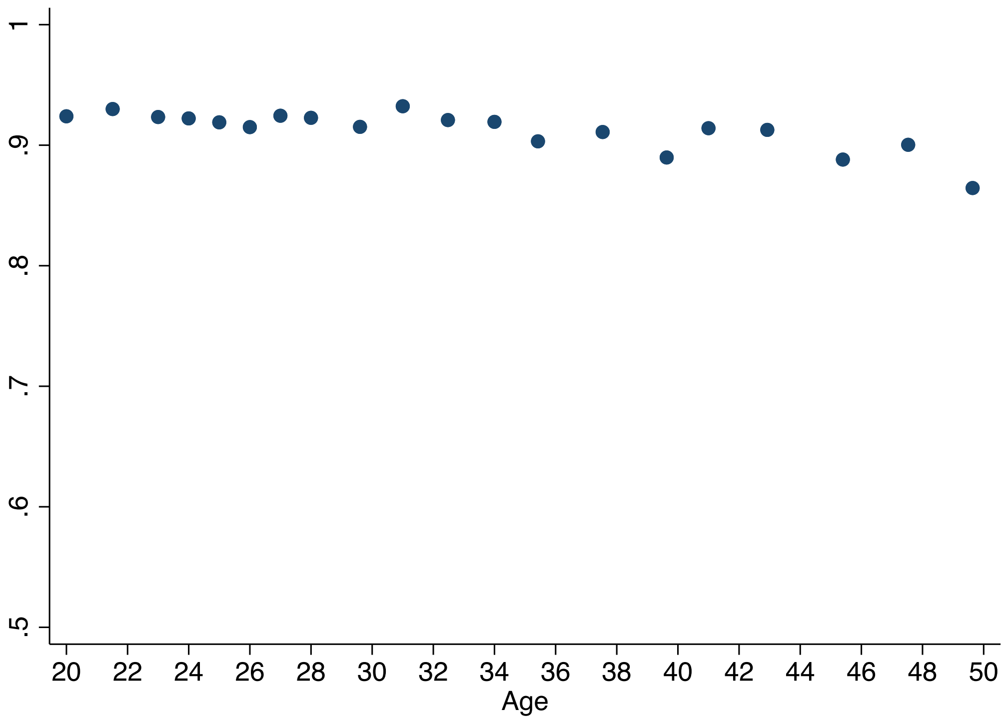
Notes: This figure shows school attendance rates for children of different ages, based on the wealth quintile of their parents. The figure is based on cross-sectional data from the 1870 population census.

FIGURE B8: SCHOOL ATTENDANCE RATES BY AGE AND PARENTAL OCCUPATION, 1870 CENSUS



Notes: This figure shows school attendance rates for children of different ages, based on the occupation of their father. The figure is based on cross-sectional data from the 1870 population census.

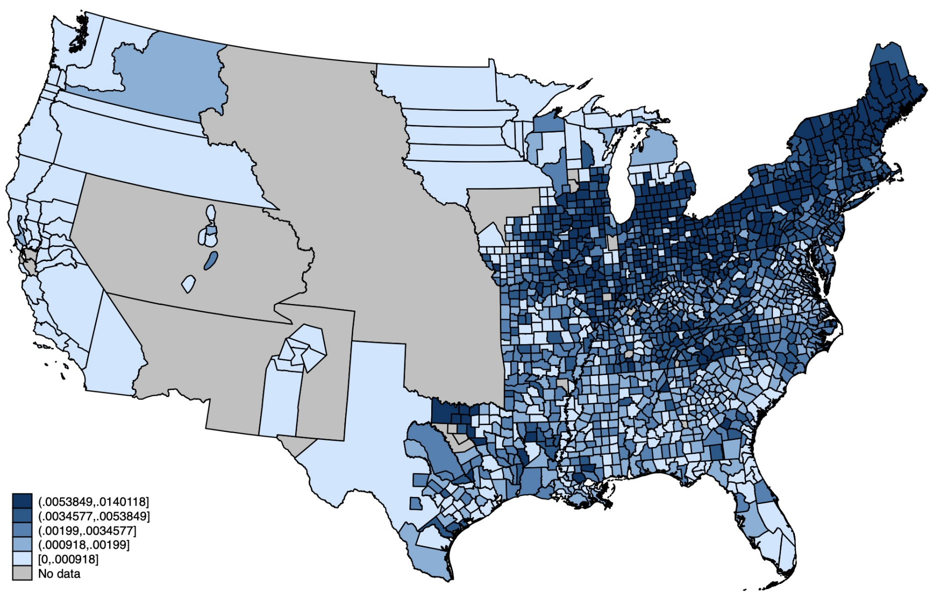
FIGURE B9: LITERACY RATE BY AGE AMONG ADULTS IN 1880 CENSUS



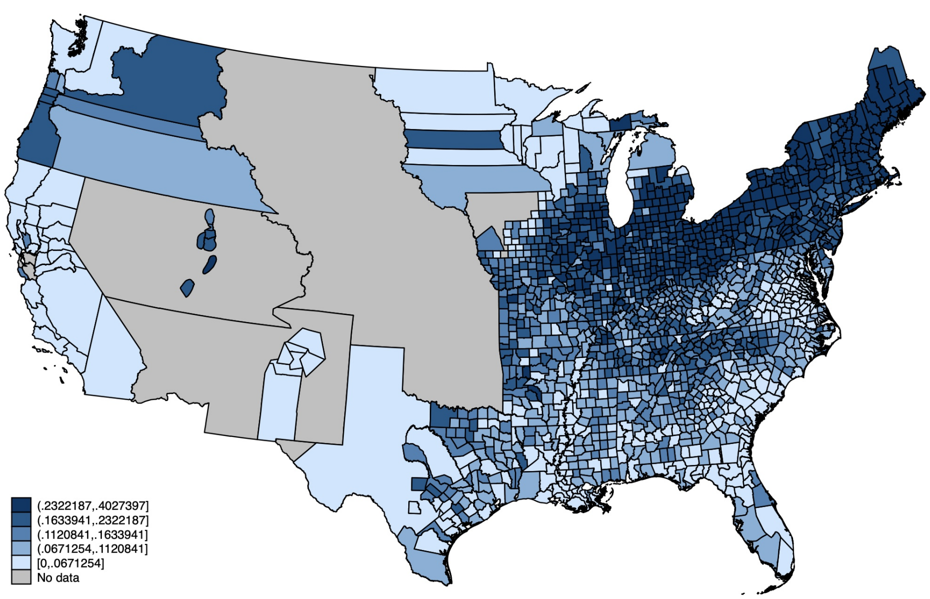
Notes: This figure shows literacy rates by age based on cross-sectional data from the 1880 population census.

Figure B10: Number of schools and school attendance rates across US counties, 1850

(a) Schools per capita

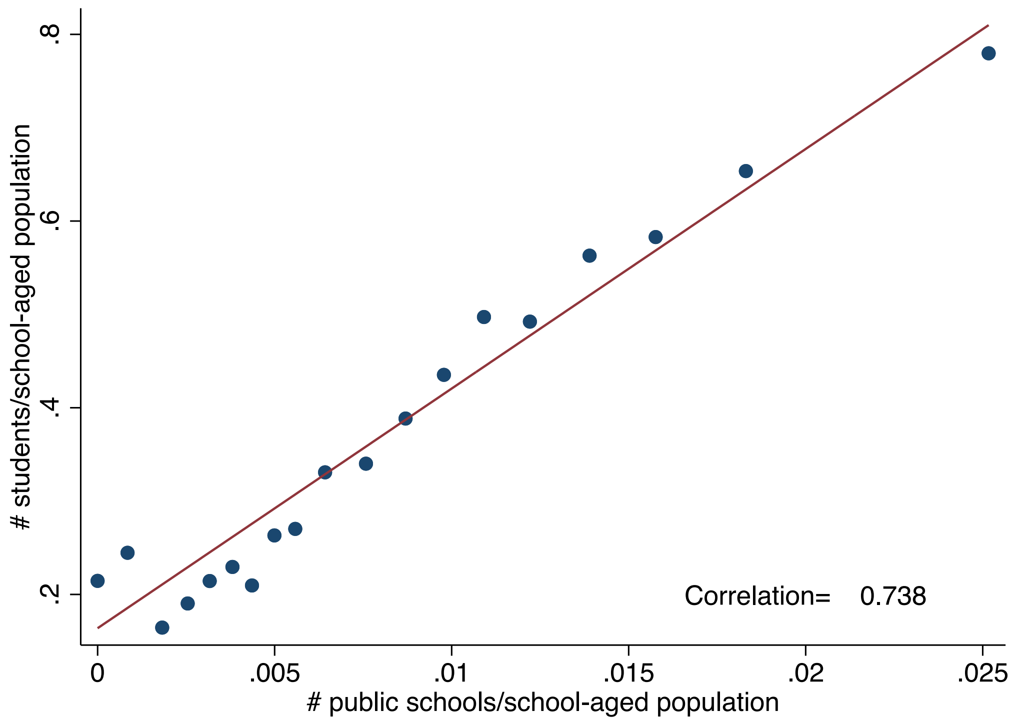


(b) Students per capita



This figure shows schools per capita (panel (a)) and the number of students per capita (panel (b)) across US counties in 1850, using data from Haines *et al.* (2010).

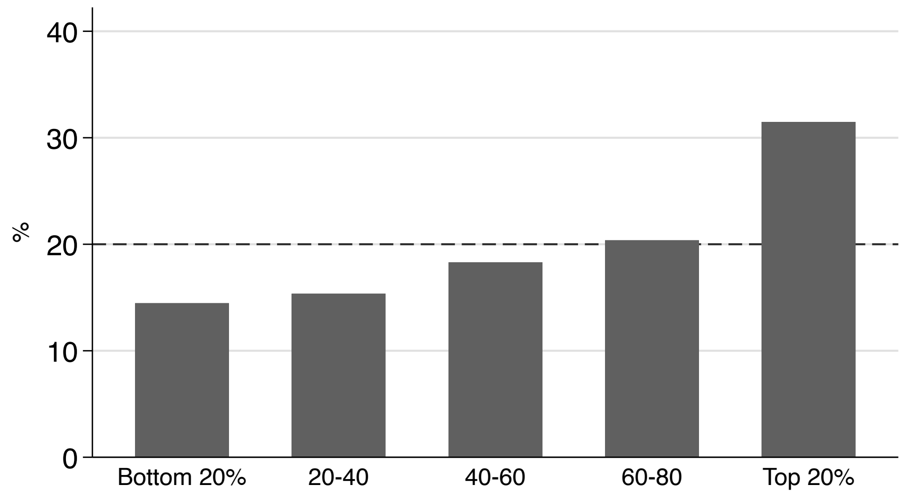
FIGURE B11: RELATIONSHIP BETWEEN SCHOOL ATTENDANCE RATES AND PER CAPITA SCHOOLS ACROSS US COUNTIES, 1850



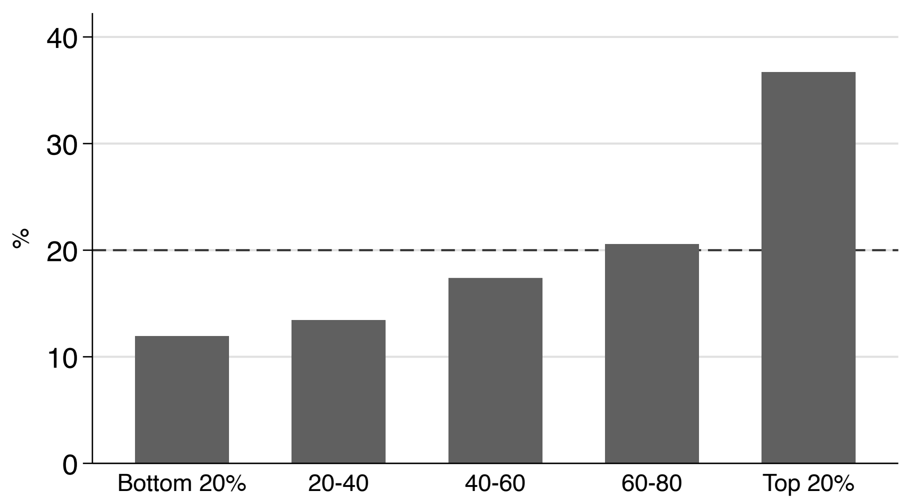
Notes: This figure shows a binned scatterplot of the relationship between school attendance rates in 1850 and the number of schools per children aged 5 to 14. The data are from Haines *et al.* (2010).

FIGURE B12: PARENTAL WEALTH QUINTILES AMONG PRIVATE SECTOR WHITE-COLLAR WORKERS

(A) ALL WHITE-COLLAR

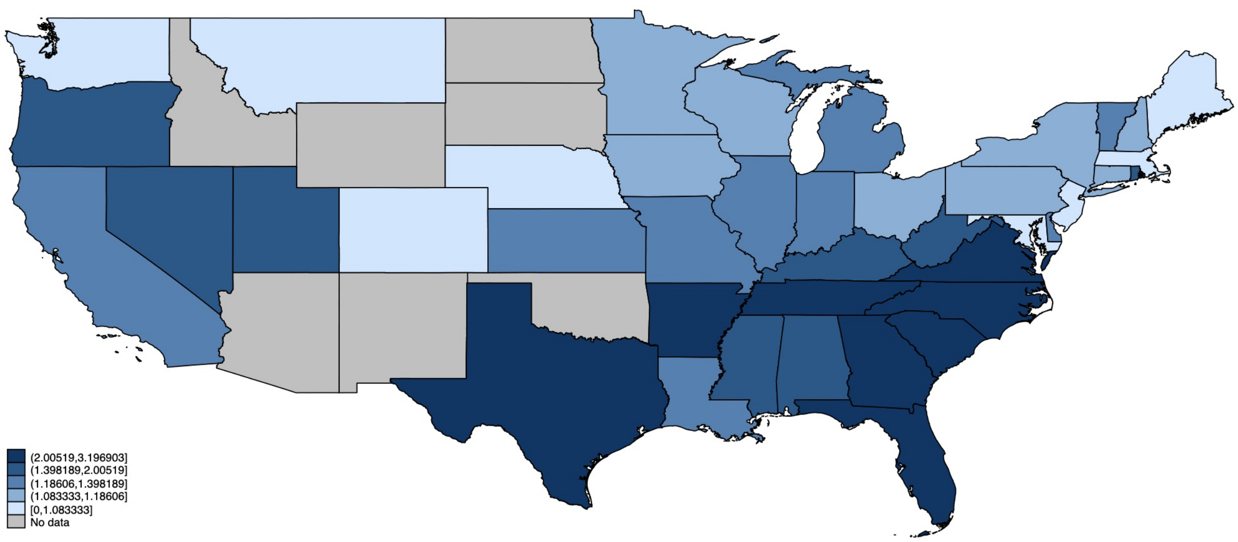


(B) PROFESSIONAL



Notes: This figure shows the the parental wealth quintiles of private sector white-collar workers. Panel (a) includes all white-collar workers, whereas Panel (b) those with a professional occupation. These figures are based on a sample linking adults in the 1880 census to their childhood census in 1860.

FIGURE B13: INEQUALITY IN ACCESS TO SCHOOLING BY STATE, 1870 CENSUS



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 US Population Census.

TABLE B2: SUMMARY STATISTICS

	Non-Exam			Exam		
	Mean	Median	Observations	Mean	Median	Observations
	(1)	(2)	(3)	(4)	(5)	(6)
i. Parental Wealth Ranks						
Total	0.52	0.52	9232	0.60	0.65	1095
Personal Property	0.53	0.54	9232	0.61	0.68	1095
Real Estate Property	0.54	0.52	9232	0.61	0.65	1095
ii. Parental Occupations						
Professional	0.09	0.00	15581	0.15	0.00	1615
White-Collar Non-Prof	0.21	0.00	15581	0.17	0.00	1615
Farmer	0.24	0.00	15581	0.35	0.00	1615
Skilled Blue-Collar	0.30	0.00	15581	0.21	0.00	1615
Unskilled	0.14	0.00	15581	0.08	0.00	1615
iii. Demographics						
Immigrant	0.12	0.00	90650	0.05	0.00	5576
Father Immigrant	0.18	0.00	14151	0.10	0.00	1461
White	0.94	1.00	31030	0.97	1.00	2320
iv. Own Occupation Prior to Civil Service (N=4990)						
Professional	0.09	0.00	4783	0.22	0.00	207
White-Collar Non-Prof	0.28	0.00	4783	0.20	0.00	207
Farmer	0.09	0.00	4783	0.18	0.00	207
Skilled Blue-Collar	0.18	0.00	4783	0.15	0.00	207
Unskilled	0.24	0.00	4783	0.15	0.00	207
iv. Connections (N=207)						
Father Gov. Employee	0.06	0.00	15581	0.05	0.00	1615
Grew Up in DC	0.30	0.00	18670	0.07	0.00	1867
Same Surname as Congressman	0.00	0.00	93691	0.01	0.00	5591

Notes: This table shows summary statistics for employees appointed without the use of exams (Column 1 to 3) and those appointed through exams. Parental wealth ranks are based on information from those bureaucrats we can successfully link to either the 1860 or the 1870 censuses.

TABLE B3: 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				
Parental Wealth Rank	0.011	0.395	0.241	0.009
Summary Index	0.067	0.422	0.867	0.001
Father Professional	-0.087	0.182	0.242	0.008
Immigrant Parents	0.014	0.443	-0.471	0.004
Immigrant	0.013	0.417	-0.232	0.005
ii. Own Occupation				
Professional	-0.079	0.925	0.422	0.070

Notes: Each row in this table corresponds to a different outcome variable. Columns 1 and 2 focus on the pre-reform event-study coefficients, whereas Columns 3 and 4 focus on the post-reform coefficients. Column 1 reports the mean value of the pre-reform event-study coefficients based on estimating equation 2 in the paper. Column 3 reports the analogous figure corresponding to post-reform coefficients. Column 2 reports the p-value corresponding to the hypothesis that all the pre-1883 event-study coefficients are equal to zero. Column 4 reports the analogous p-value for the hypothesis that all post-reform event-study coefficients are equal to zero. Standard errors clustered at the district level.

Table B4: The Family Background of Exam-Based Hires, Parental Occupations

	Professional		White-Collar Non-Prof		Farmer		Any Blue Collar	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exam X After	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
App. State FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4993	4993	4993	4993	4993	4993	4993	4993

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is an indicator that takes a value of one if the father of a bureaucrat worked in a certain occupational category (as indicated by the column). When a bureaucrat is linked to more than one census with information on parental occupations, we use the fraction of census years that parents spent in a given occupational category as our outcome variable. Professional occupations are those with a value of less than 100 in the 1950 Census occupational classification system. Examples of these occupations include lawyers and accountants. Non-professional white-collar are those with a value between 200 and 500 (for example, clerks). Farmers are those with a value of 100. Any blue collar are those with a value above 500 (for example, carpenters or laborers). $Exam$ is an indicator that takes a value of one if the employee was appointed through an examination. All columns include hiring year and position fixed effects. The odd columns further include fixed effects based on employees' state "whence appointed". Standard errors clustered at the position level.

TABLE B5: DIFFERENTIAL EFFECTS BY DEMOCRATIC OR REPUBLICAN PRESIDENCY

	(1)	(2)	(3)	(4)
Exam X After	0.185***		0.173***	
	(0.0518)		(0.0531)	
Exam X Democrat Presidency		0.220***		0.213***
		(0.0584)		(0.0604)
Exam X Republican Presidency		0.152**		0.134*
		(0.0681)		(0.0718)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
App. State FE	No	No	Yes	Yes
Observations	2944	2944	2944	2944

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is a summary index of employees' social background computed using the approach in Kling *et al.* (2007). $Democrat\ Presidency$ 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). Standard errors clustered at the position level.

TABLE B6: HETEROGENEITY BY EXAM CHARACTERISTICS

	Summary Index		First Principal Component	
	(1)	(2)	(3)	(4)
Exam X After	0.173***		0.274***	
	(0.0531)		(0.0932)	
Exam X Clerk		0.166**		0.283**
		(0.0672)		(0.113)
Exam X Tech.		0.183***		0.261*
		(0.0684)		(0.133)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
App. State FE	Yes	Yes	Yes	Yes
Observations	2944	2944	2944	2944

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' social background computed using the approach in Kling *et al.* (2007).

Table B7: Heterogeneity by Type of Position

	Summary Index		First Principal Component	
	(1)	(2)	(3)	(4)
Exam X After	0.173***		0.274***	
	(0.0531)		(0.0932)	
Exam X Below Median Pay		0.127*		0.224**
		(0.0674)		(0.109)
Exam X Above Median Pay		0.204***		0.309**
		(0.0658)		(0.124)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
App. State FE	Yes	Yes	Yes	Yes
Observations	2944	2944	2944	2944

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index of employees' social background computed using the approach in Kling *et al.* (2007).

Table B8: Mechanisms: Location

	(1)	(2)	(3)	(4)
Exam X After	0.173***	0.170***	0.170***	0.163***
	(0.0531)	(0.0450)	(0.0504)	(0.0520)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
App. 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

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is a summary index of employees' social background computed using the approach in Kling *et al.* (2007). The table shows the sensitivity of the results to adding various location fixed effects based on bureaucrats' childhood place of residence. Standard errors clustered at the position level.

TABLE B9: EFFECTS OF EXAMS ON THE SHARE OF FOREIGN-BORN EMPLOYEES

	All Immigrants		Non-English-Speaking Immigrant		English-Speaking Immigrant		Irish	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exam X After	-0.0498***	-0.0447***	-0.0161*	-0.0126	-0.0337***	-0.0321***	-0.0191***	-0.0174***
	(0.0167)	(0.0148)	(0.00892)	(0.00842)	(0.00984)	(0.00869)	(0.00657)	(0.00584)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
App. State FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	25103	25103	25103	25103	25103	25103	25103	25103

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 an employee is foreign born. The dependent variable in columns 3 and 4 is an indicator that takes a value of one if an employee is foreign born and from a non-English-speaking country. The dependent variable in columns 5 and 6 is an indicator that takes a value of one if an employee is foreign born and from an English-speaking country. The dependent variable in columns 7 and 8 is an indicator that takes a value of one if the worker was Irish. Standard errors are clustered at the position level.

TABLE B10: THE REFORM DECREASED THE SHARE OF EMPLOYEES FROM URBAN AREAS

	(1)	(2)	(3)	(4)
Exam X After	-0.151***	-0.0936***		
	(0.0300)	(0.0247)		
Exam X Clerk			-0.209***	-0.138***
			(0.0345)	(0.0313)
Exam X Tech.			-0.0629*	-0.0284
			(0.0363)	(0.0347)
Year FE	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes
App. State FE	No	Yes	No	Yes
Observations	4993	4993	4993	4993

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is an indicator that takes a value if an employee is listed as living in an urban area prior to joining the civil service. All columns include hiring year and position fixed effects. Standard errors are clustered at the position level.

TABLE B11: PROFESSIONAL BACKGROUND OF EXAM-BASED HIRES, HETEROGENEITY BY STATE OF RESIDENCE INEQUALITY IN ACCESS TO SCHOOLING

	Professional		White-collar Non-Prof		Farmer		Skilled Blue Collar		Unskilled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exam X After	0.0934**	0.162	-0.207***	-0.230**	0.0695*	0.146*	-0.00505	0.0909	0.0402	-0.120
	(0.0399)	(0.107)	(0.0711)	(0.0944)	(0.0391)	(0.0778)	(0.0374)	(0.0866)	(0.0347)	(0.0742)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
App. State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2818	764	2818	764	2818	764	2818	764	2818	764
Sample	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in each of the columns is an indicator that takes a value of one if a bureaucrat worked in a certain occupational category (as indicated by the column). When a bureaucrat is linked to more than one census with information on adult occupations, we use the most recent occupation as long at it corresponds to a census conducted prior to the corresponding register. 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 3 for definition of occupations. The sample in the odd columns is restricted to employees from states with below median inequality in access to schooling. The sample in the even columns is restricted to employees from states with above median inequality. All columns include hiring year and position fixed effects. Standard errors are clustered at the position level.