Who Benefits from Meritocracy?*

Diana Moreira
UC Davis

Santiago Pérez
UC Davis and NBER

PRELIMINARY DRAFT

Abstract

Does screening applicants using exams hurt or help the chances of disadvantaged children? Although a common critique to exams is that they might have a disproportionately negative impact on applicants from poorer backgrounds, exams might replace other selection criteria in which these applicants are at an even worse disadvantage. We study the equity implications of the 1883 Pendleton Act, which introduced competitive exams for selecting some federal employees. While the reform increased the representation of "educated outsiders" (individuals with high education but limited connections), it reduced the representation of workers from disadvantaged backgrounds. This decline was driven by an increased representation of workers from middle-class backgrounds, with limited change in the representation of the upper-class. The decrease in the representation of workers from poorer backgrounds was stronger among applicants from states with more unequal access to schooling. These findings suggest that using exams might indeed pose an equity-efficiency trade-off.

*dsmoreira@ucdavis.edu, seperez@ucdavis.edu. We thank Luiza Aires and Lisa Pacheco for outstanding research assistance, and Enrique Pérez for help with data collection. We have benefited from comments of Sandra Black, Rick Hornbeck, Chris Meissner, Noam Yutchmann, as well as by seminar participants at *Corporación Andina de Fomento*, NBER Postdocs Meeting, University of British Columbia, Melbourne University, University of Ottawa, University of Southern Denmark and the 2021 Annual Cliometrics Conference.

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 equity goal of achieving a diverse pool of recruits.[2]

At the same time, however, 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. 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.

This paper studies the equity implications of one of the earliest attempts to implement "meritocratic" ideals in US history: the introduction, after the passing of 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 highest score in an open exam. We find that this change reduced the representation of applicants from disadvantaged family backgrounds in government jobs. This decline was driven by an increased representation of middle-class applicants, with little change in the representation of the upper-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 "merit" 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 generate a "representative bureaucracy" (Kingsley, 1944). Second, government jobs have historically been avenues of upward mobility for underrepresented

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

[2] For instance, in 1981, the Carter administration agreed to replace the *Professional and Administrative Career Examination* after a lawsuit arguing that it discriminated against minority applicants. More recently, French President Emmanuel Macron announced plans to shut down the *Ecole Nationale d'Administration*–which selects students on the basis of competitive exams– due to concerns of elitism (BBC, 2021). The use of standardized exams has also been the subject of criticisms in the context of US college admissions (see for instance Rothstein (2004)).

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.

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 bureaucrats' family backgrounds (including parental wealth and occupations), as well as their labor market outcomes prior to joining the civil service.

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 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 a reduction in the share of employees from disadvantaged backgrounds. 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 applicants with parents at the bottom of the distribution, together with an increase in the share of workers from middle- and upper-middle class families. Moreover, the reform also 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 been accessible for applicants with only a "common school" education.[3]

Why did the introduction of exams improve the prospects of middle-class applicants (at the expense of those 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 three pieces of evidence that support this interpretation. First, we show that the reform indeed increased the representation of "educated outsiders". In particular, employees hired through exams were 10 percentage points more likely to have held a professional occupation –such

[3] "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.

as lawyers and accountants– prior to joining government than patronage employees .[4] 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 grown up in DC and less likely to have a father who was himself a bureaucrat. Second, we document that middle-class individuals were overrepresented among the "educated outsiders": Although being 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. Moreover, after the reform, the representation of middle-class individuals in these positions became closer to their representation in comparable white-collar positions in the private sector. Finally, 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– presumably the places in which the children of the poor were the least represented among "educated outsiders".

We discuss two additional interpretations for the reduced representation of applicants from poor backgrounds. After the reform, recruitment occurred throughout the country, away from Washington D.C.. This geographical expansion *per se* could have affected the selection of candidates. However, the increase in social status remains even when we restrict the comparison to employees who grew up in the same location. A second possibility is that exam-based recruitment could have gradually changed the appeal of government jobs. 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 over the positions is unlikely to be an important driver 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 inequality and social mobility. 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.[5] Moreover, the long-run nature of our analysis enables us to understand both the immediate and the longer-term (after individuals had time to adjust to the new system) consequences of exams. Closest to our paper is Autor & Scarborough (2008), which shows that the introduction of job testing did not hurt the chances of minority applicants to retail jobs. We deviate from this study in three main ways. First, 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 (as measured based on parental wealth and occupations). Second, applicants in Autor & Scar-

[4]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.

[5]Teorell *et al.* (2011) shows that nearly 80% of countries use formal examinations to select some of their public employees.

borough (2008) are screened using a *personality* test, whereas workers in our setting are selected through an exam in which formal education is likely more relevant for performance.[6] Finally, we focus on a context (the public sector) in which diversity is *per se* particularly important.

Second, we contribute to the literature investigating the "social origins" of civil servants (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 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.

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).[7] Instead, we focus on the *equity* implications of these reforms. This is an important margin, as achieving a "representative bureaucracy" is an explicit goal in many countries.[8] Our findings suggest that these reforms could pose an equity-efficiency trade-off, whereby improvements in workers' qualifications come at the expense of a more elitist civil service.[9]

2 Historical Background

2.1 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

[6] We provide details on the characteristics of the civil service exams in Section 2.

[7] In a recent related 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 in this paper exploits variation in exam requirements across *positions*, whereas Moreira & Pérez (2021) exploits variation in requirements across different customs-collection districts.

[8] For instance, the "First Merit Principle" of the US Merit Systems Protection Board is that âRecruitment should be from qualified individuals from appropriate sources in *an endeavor to achieve a work force from all segments of society*, and selection and advancement should be determined solely on the basis of relative ability, knowledge, and skills, after fair and open competition which assures that all receive equal opportunity."

[9] 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 a substantial historical literature on the Pendleton act and the civil service reform movement, see for instance Hoogenboom (1968).

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

2.2 The Pendleton Act

Positions Subject to Exam. The act established that employees in certain "classified" positions would need to be selected through open, competitive and anonymously graded exams (United States Civil Service Commission, 1883). 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 –our main focus in this paper– was initially restricted to employees receiving annual salaries of no less than $900 and no more than $1800. The law also exempted "laborers or workmen", as well as bureau chiefs, elected officers, and those who required Senate's confirmation. 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 initially affected only 10% of the civilian labor force, 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 in classification rules affecting the classified departmental service. First, in 1885 the lower salary limit 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: By 1883, 90% of positions that would be covered by the reform paid between $900 and $1800, and only 5% paid less than $720.[10] Hence, in practice, our analysis will compare workers in positions that were subject to exam at some point from 1883 to 1893 to workers in positions that were not, before and after 1883.[11]

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

[11] 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

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.[12] Here, we define a 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).[13] 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.[14] Growth in the number of employees seems to level off after 1883, both in exempted and non-exempted positions (as reflected by the stability of the share of workers in exempted positions).[15]

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 population. A consequence of this rule is that applicants to the classified departmental service were in practice only 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 as to shut down the effects of the reform that stem from apportionment-induced changes in employees' regional origins.[16]

Although it changed the method used to fill certain federal positions, it is important to emphasize 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).[17] 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 formal academic training. Applicants to the positions of copyist or clerk (the most common occupations within the classified service) were required to complete exams on orthography, copying, penmanship and arithmetics.[18] Applicants to

on the names of employees appointed to this job.

[12]The Executive Departments are Agriculture, Interior, Justice, Labor, Navy, Post Office, State, Treasury and War.

[13]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.

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

[15]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.

[16]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).

[17]"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).

[18]The exam for clerks was referred to as the *general exam*, whereas the exam for copyists was referred to as the *limited*

positions requiring technical, professional or scientific knowledge were required to take "supplementary" or "special exams". Examples of such 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. Panel (a) in Figure A2 shows an example question of the arithmetic exam for applicants to the position of copyist, whereas Panel (b) 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.[19]

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". In contrast, the US Civil Service Commission maintained that "a common school education was sufficient to pass examination" (Hoogenboom, 1959).[20] Indeed, applicants with only a "common school" education regularly took (and passed) the exams. Figure A3 shows the number of applicants of different educational backgrounds, as well as the share with a passing grade: applicants with a common school education were the largest group of applicants, and about 55% of them actually passed the exam.[21]

Appointing Procedure. Applicants who obtained a passing grade were added to a register of eligible candidates. 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.[22] 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.[23] For positions in the Customs and Postal services (where apportionment was not required), the list of top applicants would need to correspond to the top four candidates regardless of their state of residence. An important deviation from meritocratic principles is that appointing officers could ask for an employee of a specific gender (for instance, a "male clerk").[24]

Exams were held throughout the country: Figure A5 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. 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 A4 shows the yearly number of applicants to the classified

exam. The general exam could additionally include subjects such as bookkeeping and US history.

[19] 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).

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

[21] "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.

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

[23] The Civil Service Commission kept track of the number of employees appointed by each state.

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

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 those who obtained a passing grade who were appointed: By 1893, 23% of those who had applied to the classified departmental service and obtained a passing grade had received an appointment.[25]

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, those 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) 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.[26] Similarly, Aron (1987) describes a number of cases where employees secured their position through a personal or family connection with a member of Congress.[27] To the extent that individuals from disadvantaged backgrounds were more likely to lack these social connections, the reform should have facilitated their access to government jobs.

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."[28] 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).[29] Consistent with this view, Hoogenboom (1959) argues that "as reformers had hoped, the merit system recruited persons of a higher social status".[30]

[25]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. 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 every year (0.1%).

[26]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)

[27]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.

[28]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)."

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

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

3 Data

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, job title, government unit, place of birth, state of residence at the time of appointment, location, 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.[31] Our data include information on approximately 450,000 employee-years. Of these, about 90,000 correspond to employee-years in the executive departments in DC (our main focus in this paper). Figure A1 shows an example page corresponding to the 1881 Register. This page lists employees working in the Internal Revenue Service within the Treasury Department.

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.[32] Through this procedure, we obtained information about: (1) employees' family backgrounds, including characteristics such as 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), and (2) the results that *do not* require the linked data are very similar when estimated in this linked sample (Table A2).

Measuring Employees' Social Backgrounds. The linked sample enables us to observe a number of characteristics related to employees' social backgrounds. When we focus on characteristics (such as wealth or occupations) of bureaucrats' parents, measuring these characteristics requires observing bureaucrats while living with their parents in the census. In these cases, we focus on employees whom we observe in the census below the age of 18 (and *prior* to them joining the civil service).

First, we observe parental wealth. The 1860 and 1870 censuses asked all household heads to report the dollar value of real estate and personal property wealth.[33] We use the combined value

[31] 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.

[32] 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.

[33] 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.

of real estate and personal property wealth to rank households in the *national* (although we obtain similar results if we compute a state of residence-specific rank) wealth distribution, separately by census year and by age of the household head.[34] 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)).

Second, we observe parental occupations.[35] We split occupations into five broad categories: professional, non-professional white-collar, farmer, skilled blue collar and unskilled.[36] For those employees for whom we observe parental occupations in more than one census, we calculate the fraction of census years that their parents spent in a certain occupational category.[37] 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.[38]

Third, we observe workers' nativity status (their own and their parents' birthplaces), as well as their race. 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 focus on whether workers are foreign born.

Finally, 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

[34] 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.

[35] We focus on father's occupations since extremely few mothers worked outside of their households in this time period.

[36] Professional occupations are those with a value of less than 100 in the 1950 Census Bureau occupational classification 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).

[37] 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.

[38] 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.

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.[39]

Measuring Employees' Professional Backgrounds. We also observe workers' occupations *prior* to joining the civil service.[40] 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 last pre-civil service occupation. We also 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.

Identifying Employees Appointed Through Exam. We combine the personnel records with 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.[41] 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 A8 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 A9 shows an example page listing the positions subject to exam in the Treasury Department.

Summary Statistics. Table B1 shows summary statistics for employees in our baseline sample, separately based on whether or not employees 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 a foreign-born father), and were more likely to be white.

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

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

[41] 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).

4 Empirical Strategy

The goal of our analysis is to investigate the extent to which selecting employees on the basis of competitive exams changed bureaucrats' background characteristics. To do so, our empirical strategy compares the characteristics of employees hired to perform the *same* job in the *same* government unit, but who were recruited under two different hiring regimes (patronage versus exams). We estimate:

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

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. 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 positions that became subject to exams, and $After_t$ takes a value of one for workers hired after the reform. 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*. Throughout the analysis, we cluster standard errors at the level of the position.

In addition, we also estimate event-study specifications:

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

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.[42] This specification enables us to trace out the dynamic effects of the reform.

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 appointment. 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 modest effects on our estimates.[43]

In our main analysis, we focus on employees in the Executive Departments in Washington, DC. We do so because, for these workers, we have exact data on which of them were appointed through an exam.

Challenges to Identification. The first concern is that the characteristics of workers in ex-

[42] 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).

[43] 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. However, our results are similar if we use workers' *birthplace* fixed effects see Figure B1).

empted positions would have been on a different time trend relative to those of workers in non-exempted positions. Table B2 presents F-test statistics corresponding to the hypothesis that all pre-reform event-study coefficients are equal to zero for our main outcomes of interest. In all cases, we do not reject the null hypothesis that all pre-reform event-study coefficients are significantly different from zero. In Section 5, we also present graphic evidence of the common trends assumption. In addition, we 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 these were under tighter control from the Civil Service Commission.[44] 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.[45] 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, offices outside of DC that were not directly affected by the reform).

Sample Restrictions. In our baseline analysis, we impose three sample restrictions. First, we restrict the sample to employees in Washington, DC. We do so in order to improve the comparability between our treatment and control groups, since nearly all employees hired through exams in the classified departmental service (that is, our treatment group) worked in DC. Second, we restrict the sample to employees in the Executive Departmental service. This restriction excludes workers in the Judicial and Legislative departments, as well as those in miscellaneous government agencies not affected by the reform.[46] Finally, we restrict the sample to *male* employees. We do so for two

[44] 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."

[45] 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.

[46] Examples of such agencies include the "Government Printing Office" and the "National Home for Disabled Volunteer Soldiers".

reasons: First, since during this time period 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). Second, as described above, appointing officers could ask for an employee of a certain gender. Hence, male and female employees were in practice not competing with each other in the exams. Our main results are nevertheless similar when we impose alternative sample restrictions, including adding female employees to the sample (see Figure B1).

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. Later, we present estimates for specific components of the index.

Figure 2a shows the average value of this index for 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. The table shows that the index was 0.17 higher among exam-based hired than among employees hired through patronage.[47] The estimates are 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.

Figure 2b shows the corresponding event-study estimates. The pre-reform event-study coefficients are all close to zero and we do not reject the hypothesis that they are jointly equal to zero (p-value: 0.34, see Table B2). In contrast, all of the post-reform event-study coefficients are positive and they are jointly statistically significant (p-value<0.01, see Table B2). The figure suggests a rapid increase in the summary index after the reform, with the estimates then fluctuating around 0.2: By 1893 (10 years after the reform), workers appointed through exams were still of higher social status than those hired through patronage.

[47] As described in Kling *et al.* (2007), the point estimates show "where the mean of the treatment group is in the distribution of the control group in terms of standard deviation units."

To benchmark this result, consider that, among workers employed as "clerks" prior to the reform, the median value of summary index was 0.3, whereas it was 0.04 among those employed in the lower-paying occupation of "laborer". Hence, the increase in the summary index corresponds to about 80% of the pre-reform gap between "clerks" and "laborers".

Parental Wealth. We next investigate the consequences of the reform for the different components of the index, starting from parental wealth ranks. Figure 3 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, 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 home-ownership 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 wealth.

Figure 4 shows event-study estimates of the effects of the reform on parental wealth. Similar to when we focus on the summary index, the pre-reform event-study coefficients are relatively small and we do not reject the hypothesis that they are jointly equal to zero (p-value: 0.39, see Table B2). In contrast, all of the post-reform event-study coefficients are positive and they are jointly statistically significant (p-value<0.01, see Table B2). In particular, the estimates suggest a a rapid increase in parental wealth relative to employees in the control group.

Which Groups Won and Which Groups Lost Access? Employees hired through examinations had higher average parental wealth. 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 5 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 become subject to exams.

Among workers 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). This overrepresentation of both the bottom and the top of the parental wealth distribution appears to continue in the post-reform period (Panel (d)).

Among those in positions subject to exams, workers from the top quintile were also 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.

Panel (e) in Figure 5 confirms this pattern when we run separate regressions in which the dependent variables are indicators for belonging to the different quintiles of the 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.

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). 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 their race.

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 decline might reflect the fact that immigrants, who had had less exposure to US education, were likely 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 driven simply by a lack of familiarity with English: Table B4 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 particularly 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.

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. 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. The reports of the Civil Service Commission argue that the reform led to an increased representation of African Americans in the federal administration.[48] However, 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.

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).

5.1 Robustness

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 in different states might have been on different time trends, 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 B1 we control for appointing state $\times$ year fixed effects. These controls account for differences in labor market opportunities across states which might have influenced the likelihood of applying to government jobs.

In our baseline analysis, the control group is comprised of workers who worked in the Executive Departments in DC and who were both at the bottom and the top of the state hierarchy. Figure B1 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 as control group either: (1) only bureaucrats at the bottom of the state hierarchy, or (2) only top bureaucrats. Second, the results are also similar when we drop from the control group individuals making more than $3000 and less than %600 (so as to increase the comparability of the treatment and control groups). Third, we add workers

[48] "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)

who worked outside of DC to the control group. When we do so, we add place of employment to our definition of a position. Finally, we add workers who were employed in DC but who were employed outside of the Executive departments (for instance, in the Legislative and Judicial Department). 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.

We then 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 in the same Department who has the same name, birthplace and appointment state. In a robustness check, 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), regardless of birthplace. Again, we find very similar results when using this alternative definition (Figure B1).

In Figure B3, 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 $\times$ Year fixed effects (Figure B1).

In Figure B2 we implement a randomization inference approach. To do so, we randomly assign 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.

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

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

Further, assume that e and c are potentially correlated with applicant's family wealth (w). We think of connections broadly, including personal as well as political connections.

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

Whether the shift towards "merit" helps the poor, the rich or the middle class depends on the the relationship between e, c and w. Figure B4 illustrates three possible cases. In the case in Panel (a), wealth has a stronger correlation with education than with connections. In this case, exams disproportionately help 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, exams would increase the relative representation of the children of the poor. Finally, in Panel (c) the "middle class" increases its representation: it has similar levels of connections than the "poor", but higher levels of education. Note that, even if there is a 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 the composition of bureaucrats indirectly, by changing the applicant pool. For instance, the reform might increase the prestige of holding a government job, thus leading to changes in the characteristics of individuals interested in such a job. 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 reform as encompassing both its direct (through changes in screening) and indirect (through changes in the applicant pool) effects.

An important simplification in this framework is that it considers the levels of "education" and "connections" as given and not affected by the reform. In principle, however, applicants might have decided to accumulate further human capital in response to the reform, particularly in the longer term. Such response is less likely in this context since public employment was a rather small share of total employment, making it unlikely that individuals would have changed their human capital accumulation decisions in response to the reform.[49]

6.2 Empirical Evidence

The Reform Increased the Representation of "Educated Outsiders". Our conceptual framework suggests 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 stronger educational attainment.[50]

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

[49] The US population was about 50 million by the 1880 census. By 1893, a total of just 43,000 federal positions were under the merit system (United States Civil Service Commission, 1893).

[50] 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.

precisely the occupations which would have required formal education beyond a common school, suggesting that the reform was successful in recruiting more educated workers. Interestingly, 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 who came from rural areas.[51] These 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.

Figure 6 shows the corresponding event-study estimates. The figure shows a rapid increase in the share of workers who had 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.

We next investigate if the reform brought "outsiders", that is, individuals who lacked 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 three alternative measures: First, whether a bureaucrats' father was himself a public employee. Specifically, we construct an indicator that takes a value of one if a bureaucrat's father reports working for the federal government in the census. Second, whether the bureaucrat grew up 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 living elsewhere. Third, whether a bureaucrat had the *same surname* as a current member of Congress from his own appointing or birth state.[52] Finally, we combine the information of the three measures into a single "connections index" using the approach in Kling *et al.* (2007). An important feature of all of these measures is that they are pre-determined with respect to bureaucrats' own employment in the civil service. Each of these measures captures different types of connections that applicants might have had.

Table 6 shows that the reform brought individuals who were less likely be connected according to each of our three measures, although only the likelihood that a bureaucrat would have spent some time in DC during childhood is statistically significant at the conventional levels.

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

[51] Also, note that farmers were not a particularly uneducated group in this time period: Among white adult males aged 18 or more in 1880, those employed as farmers had a 91% literacy rate (compared to 93% among those who were not farmers).

[52] To do so, we used the *Biographical Directory of the US Congress* to compile information on Congressmen names.

would be "connected" varied depending on the position to which a bureaucrat was appointed. We estimate:

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

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 in the Agriculture department).

Table 6 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. In contrast, there is no decline in this likelihood among those appointed to technical positions.

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 5 shows that, prior to the reform, workers whose families belonged to the 60-80 quintile were similarly represented in positions that would become subject to exams than those whose families belonged to the bottom three quintiles. Moreover, Figure B5 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 B6 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.[53] 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.

The Reform Hurt the Chances of the Poor when Inequality in Access to Education was High. An implication of our conceptual framework is that the shift towards "merit" should have the most negative impacts on the chances of children from poorer backgrounds when educational resources are very unequally distributed. To investigate if this was the case, we exploit variation in the levels of inequality in access to schooling based on employees' state of residence at the time of appointment. 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 (Lindert, 2004).

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

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

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

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.

Table 7 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.

In contrast, Table B5 shows there is no such heterogeneity when we focus on workers' *own* occupation prior to joining the civil service. 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

A first alternative 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 B3 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.[54]

As discussed above, by changing the costs and benefits of a government position, the introduction of exams could have led to changes in the applicant pool. Hence, the changes in bureaucrats' social origin that we observe would not be driven by the change in the method for screening employees, but rather by changes in the characteristics of individuals interested in such job. 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), the timing of the effects that we document seems to suggest a role for the screening procedure: we observe a rapid change in

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

bureaucrats' social backgrounds, which seems inconsistent with plausibly slower to change perceptions about the prestige of public employment.

7 Conclusions

In this paper, we studied whether selecting individuals using exams helps or hurts the chances of children from disadvantaged backgrounds. Our analysis focused on the 1883 Pendleton Act, which introduced competitive exams to select certain federal employees. Comparing the background of employees in exempted and non-exempted positions before and after the reform, we find that the reform 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).

Our findings suggest that selecting individuals using exams could pose an equity-efficiency trade-off. On the one hand, we find that employees selected through exams have better occupational backgrounds than those selected through patronage, which could lead to efficiency gains. On the other hand, such an improvement in professional background could come at the cost of increased elitism.

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 also have these attributes, our findings likely represent a best case scenario of the equity consequences of exams.

References

ABRAMITZKY, RAN, BOUSTAN, LEAH PLATT, ERIKSSON, KATHERINE, FEIGENBAUM, JAMES J., & PÉREZ, SANTIAGO. 2019. *Automated Linking of Historical Data*. Tech. rept. NBER WP 25825.

ABRAMITZKY, RAN, BOUSTAN, LEAH, JACOME, ELISA, & PEREZ, SANTIAGO. 2021. Intergenerational Mobility of Immigrants in the United States over Two Centuries. *American Economic Review*, **111**(2), 580–608.

ARON, CINDY SONDIK. 1987. *Ladies and gentlemen of the civil service: Middle-class workers in Victorian America*. Oxford University Press.

AUTOR, DAVID H, & SCARBOROUGH, DAVID. 2008. Does job testing harm minority workers? Evidence from retail establishments. *The Quarterly Journal of Economics*, **123**(1), 219–277.

BAI, YING, & JIA, RUIXUE. 2016. Elite recruitment and political stability: the impact of the abolition of China's civil service exam. *Econometrica*, **84**(2), 677–733.

BEAMAN, LORI, DUFLO, ESTHER, PANDE, ROHINI, & TOPALOVA, PETIA. 2012. Female leadership raises aspirations and educational attainment for girls: A policy experiment in India. *science*, 1212382.

BOSTASHVILI, D., & UJHELYI, GERGELY. 2019. Political Budget Cycles and the Civil Service: Evidence from Highway Spending in US States. *Journal of Public Economics*, **175**(C), 17–28.

BOURDIEU, PIERRE. 1998. *The state nobility: Elite schools in the field of power*. Stanford University Press.

BRIERLEY, SARAH. 2019. Combining patronage and merit in public sector recruitment. *Journal of Politics*.

CHATTOPADHYAY, RAGHABENDRA, & DUFLO, ESTHER. 2004. Women as policy makers: Evidence from a randomized policy experiment in India. *Econometrica*, **72**(5), 1409–1443.

CHETTY, RAJ, HENDREN, NATHANIEL, JONES, MAGGIE R, & PORTER, SONYA R. 2018. *Race and economic opportunity in the United States: An intergenerational perspective*. Tech. rept. National Bureau of Economic Research.

COMMISSION, UNITED STATES CIVIL SERVICE. 1884. *Report of the United States Civil-Service Commission*. US Government Printing Office.

COMMISSION, UNITED STATES CIVIL SERVICE. 1885. *Report of the United States Civil-Service Commission*. US Government Printing Office.

COMMISSION, UNITED STATES CIVIL SERVICE. 1886. *Report of the United States Civil-Service Commission*. US Government Printing Office.

COMMISSION, UNITED STATES CIVIL SERVICE. 1897. *Report of the United States Civil-Service Commission*. US Government Printing Office.

DAL BÓ, ERNESTO, FINAN, FREDERICO, FOLKE, OLLE, PERSSON, TORSTEN, & RICKNE, JOHANNA. 2017. Who becomes a politician? *The Quarterly Journal of Economics*, **132**(4), 1877–1914.

DEPARTMENT OF THE INTERIOR. 1871-1893. *Official Register of the United States*. US Government Printing Office.

ESTRADA, RICARDO. 2019. Rules versus discretion in public service: Teacher hiring in Mexico. *Journal of Labor Economics*, **37**(2), 545–579.

FOLKE, OLLE, HIRANO, SHIGEO, & SNYDER, JAMES M. 2011. Patronage and elections in US states. *American Political Science Review*, **105**(3), 567–585.

GRINDLE, MERILEE S. 2012. *Jobs for the Boys*. Harvard University Press.

HOFFMAN, MITCHELL, KAHN, LISA B, & LI, DANIELLE. 2018. Discretion in hiring. *The Quarterly Journal of Economics*, **133**(2), 765–800.

HOOGENBOOM, ARI. 1959. The Pendleton Act and the civil service. *The American Historical Review*, **64**(2), 301–318.

HOOGENBOOM, ARI ARTHUR. 1968. *Outlawing the spoils: a history of the civil service reform movement, 1865-1883*. Vol. 50. University of Illinois Press.

JOHNSON, RONALD N, & LIBECAP, GARY D. 1994a. Patronage to merit and control of the federal government labor force. *Explorations in Economic History*, **31**(1), 91–119.

JOHNSON, RONALD N, & LIBECAP, GARY D. 1994b. The" Problem of Bureaucracy". *Pages 1–11 of: The federal civil service system and the problem of bureaucracy*. University of Chicago Press.

KEISER, LAEL R, WILKINS, VICKY M, MEIER, KENNETH J, & HOLLAND, CATHERINE A. 2002. Lipstick and logarithms: Gender, institutional context, and representative bureaucracy. *American political science review*, **96**(3), 553–564.

KING, DESMOND S, *et al.* 1995. *Separate and unequal: Black Americans and the US federal government*. Oxford University Press.

KINGSLEY, J DONALD. 1944. Representative bureaucracy. *Representative Bureaucracy*, 12.

KLING, JEFFREY R, LIEBMAN, JEFFREY B, & KATZ, LAWRENCE F. 2007. Experimental analysis of neighborhood effects. *Econometrica*, **75**(1), 83–119.

LEUPP, F.E. 1898. *How to Prepare for a Civil-service Examination: With Recent Questions and Answers*. Hinds & Noble.

LIBECAP, GARY D, & JOHNSON, RONALD N. 2007. *The Federal Civil Service System and the Problem of Bureaucracy: The Economics and Politics of Institutional Change*. University of Chicago Press.

LINDERT, PETER H. 2004. *Growing public: Volume 1, the story: Social spending and economic growth since the eighteenth century*. Vol. 1. Cambridge University Press.

MOREIRA, DIANA, & PÉREZ, SANTIAGO. 2020. Civil Service Reform and Organizational Practices: Evidence from the 1883 Pendleton Act.

MOREIRA, DIANA, & PÉREZ, SANTIAGO. 2021. *Civil Service Reform and Organizational Practices: Evidence from the Pendleton Act*. Tech. rept. National Bureau of Economic Research.

MUÑOZ, PABLO, & PREM, MOUNU. 2020. Managers' Productivity and Labor Market: Evidence from School Principals. *Documentos de Trabajo*.

ORNAGHI, ARIANNA. 2016. Civil service reforms: Evidence from US police departments. *Job Market Paper*.

PANDE, ROHINI. 2003. Can mandated political representation increase policy influence for disadvantaged minorities? Theory and evidence from India. *American Economic Review*, **93**(4), 1132–1151.

RAUCH, JAMES E, *et al.* 1995. Bureaucracy, Infrastructure, and Economic Growth: Evidence from US Cities during the Progressive Era. *American Economic Review*, **85**(4), 968–979.

RICCUCCI, NORMA M, VAN RYZIN, GREGG G, & LAVENA, CECILIA F. 2014. Representative bureaucracy in policing: Does it increase perceived legitimacy? *Journal of public administration research and theory*, **24**(3), 537–551.

ROTHSTEIN, JESSE M. 2004. College performance predictions and the SAT. *Journal of Econometrics*, **121**(1-2), 297–317.

TEORELL, JAN, DAHLSTRÖM, CARL, & DAHLBERG, STEFAN. 2011. The QoG expert survey dataset. *Available at SSRN 3569575*.

THERIAULT, SEAN M. 2003. Patronage, the Pendleton Act, and the Power of the People. *The Journal of Politics*, **65**(1), 50–68.

THOMPSON, DANIEL M, FEIGENBAUM, JAMES J, HALL, ANDREW B, & YODER, JESSE. 2019. *Who Becomes a Member of Congress? Evidence From De-Anonymized Census Data*. Tech. rept. National Bureau of Economic Research.

UNITED STATES CIVIL SERVICE COMMISSION. 1883. *Report of the United States Civil-Service Commission*. US Government Printing Office.

UNITED STATES CIVIL SERVICE COMMISSION. 1890. *Report of the United States Civil-Service Commission*. US Government Printing Office.

UNITED STATES CIVIL SERVICE COMMISSION. 1891. *Report of the United States Civil-Service Commission*. US Government Printing Office.

UNITED STATES CIVIL SERVICE COMMISSION. 1893. *Report of the United States Civil-Service Commission*. US Government Printing Office.

VAN RIPER, PAUL P. 1976. *History of the United States civil service*. Greenwood Press.

XU, GUO. 2018. The costs of patronage: Evidence from the british empire. *American Economic Review*, **108**(11), 3170–98.

XU, GUO. 2020. Bureaucratic Representation and State Responsiveness: The 1918 Pandemic in India.

ZIPARO, JESSICA. 2017. *This Grand Experiment: When Women Entered the Federal Workforce in Civil War–Era Washington*. UNC Press Books.

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, it is the first principal component based on 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 average across census years 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 "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 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. $Exam$ 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: EXAM-BASED HIRES MORE LIKELY TO BE "OUTSIDERS"

	Father Gov. Emp.		Lived in DC		Same Surname		Connections Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exam X After	-0.00863		-0.0564***		0.00189		-0.0516*	
	(0.0164)		(0.0190)		(0.00172)		(0.0313)	
Exam X Clerk		-0.0326*		-0.0810***		0.000382		-0.114***
		(0.0175)		(0.0196)		(0.00208)		(0.0363)
Exam X Tech.		0.0266		-0.0172		0.00453		0.0401
		(0.0303)		(0.0253)		(0.00353)		(0.0561)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Position FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
App. State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4993	4993	5860	5860	25442	25442	4993	4993

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 (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 index that combines information on the outcomes in columns 1 to 6. "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.

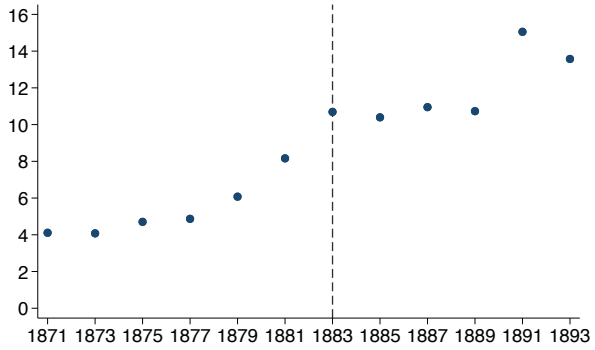
TABLE 7: HETEROGENEITY BY STATE OF RESIDENCE INEQUALITY IN ACCESS TO SCHOOLING

	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

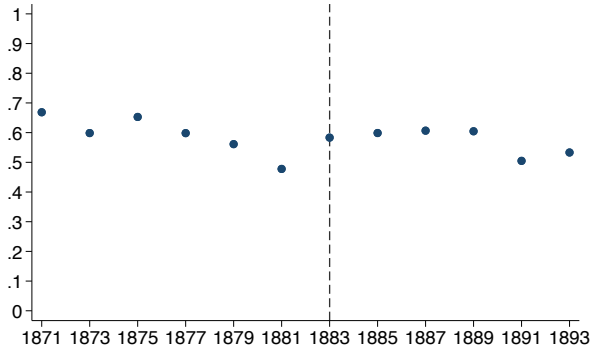
Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable is a summary index computed using the approach in Kling *et al.* (2007). The sample in columns 1 and 2 in each panel is restricted to employees from states with below median inequality in access to schooling. The sample in columns 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)



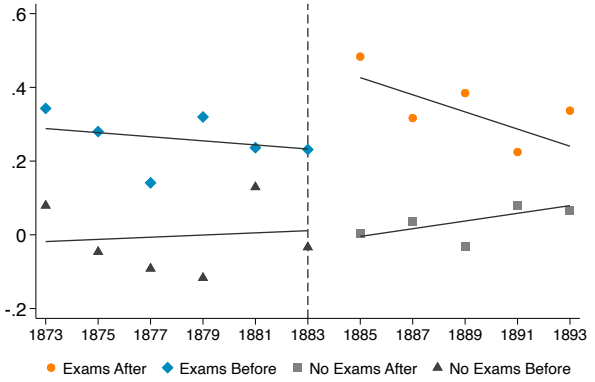
(B) % IN POSITIONS SUBJECT TO EXAMS



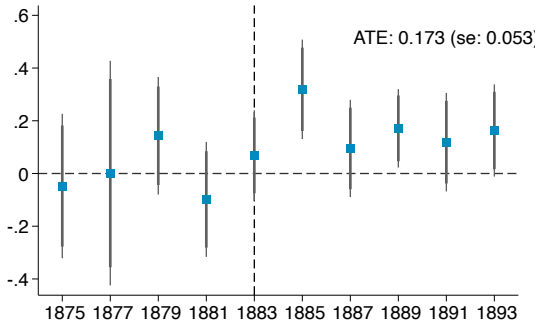
Notes: Panel (a) shows the total number of workers in the Departmental Service in DC. Panel (b) shows the proportion of such employees who worked in positions that were subject to exams after 1883. A position is coded as being subject to exams if it required an exam at any point from 1883 to 1893.

FIGURE 2: SOCIAL BACKGROUND OF EXAM-BASED HIRES

(A) AVERAGE VALUES

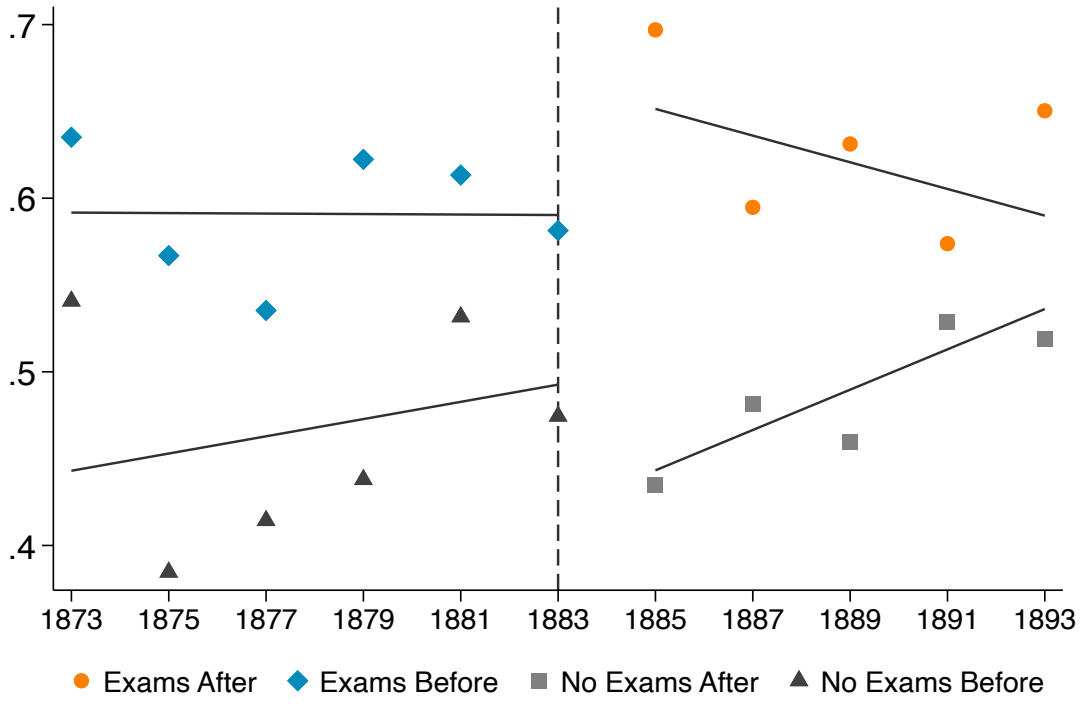


(B) EVENT-STUDY COEFFICIENTS



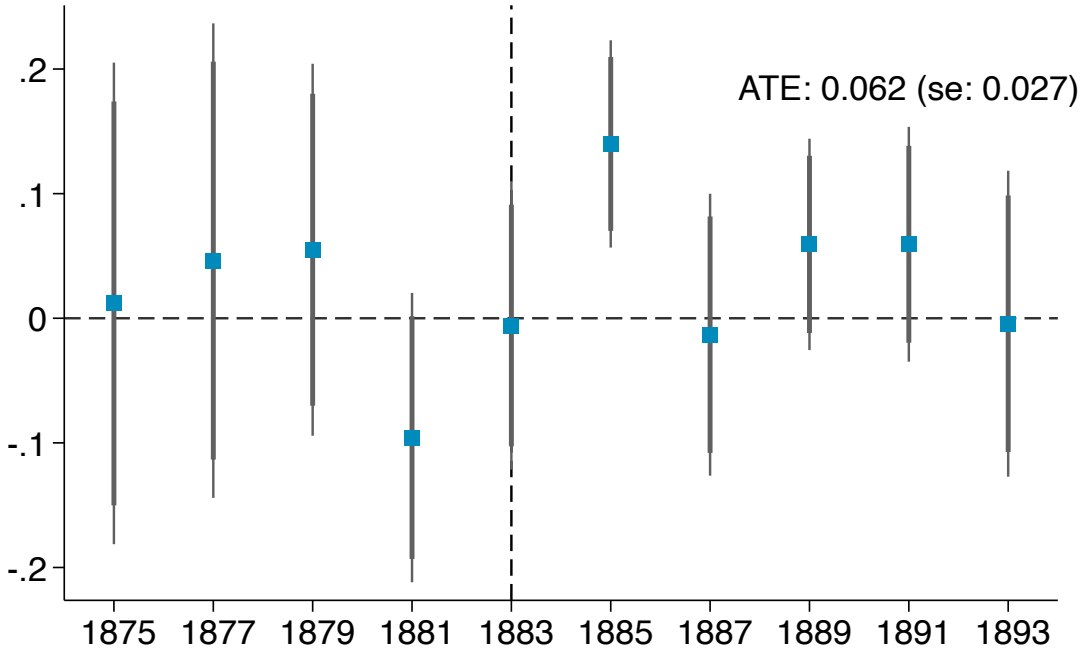
Notes: The dependent variable in both these figures is a summary index of employees' social class. The index is based on the approach in Kling *et al.* (2007), and it is constructed by combining information on parental wealth rank, parental occupations. Panel (a) shows the average value of the index for workers in positions subject and non-subject to exam, by hiring year. Panel (b) 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: PARENTAL WEALTH RANKS



Notes: This figure shows the average parental wealth rank of workers in positions subject and non-subject to exams from 1873 to 1893.

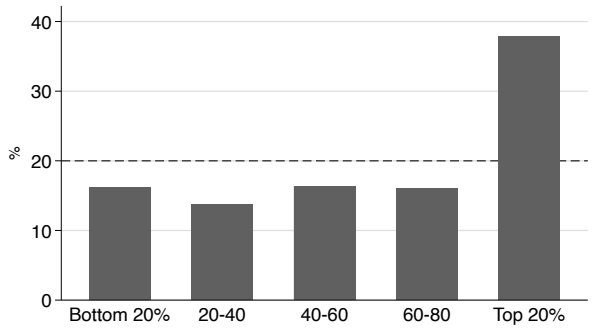
FIGURE 4: THE FAMILY BACKGROUND OF MERIT HIRES. PARENTAL WEALTH RANK



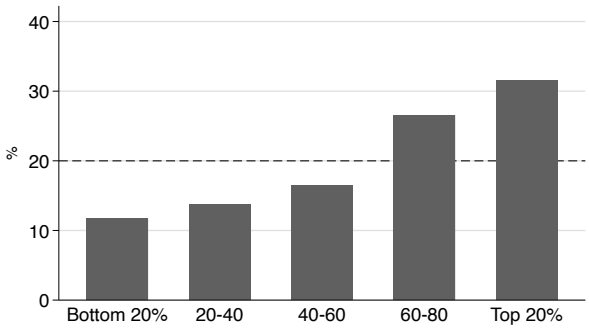
Notes: The dependent variable is the rank of a bureaucrat father in the US national wealth distribution. These ranks are computed separately by census year (1860 and 1870) and by age (i.e. relative to all fathers of the same age).

FIGURE 5: PARENTAL WEALTH QUINTILES OF GOVERNMENT EMPLOYEES

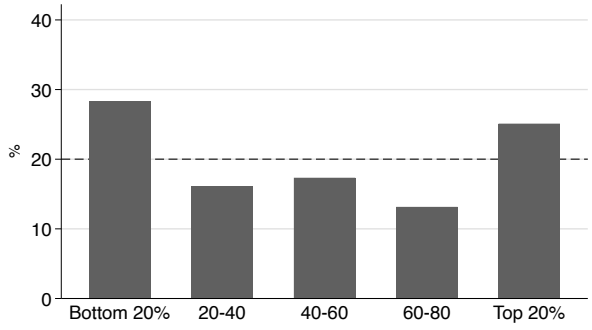
(A) SUBJECT TO EXAMS, BEFORE



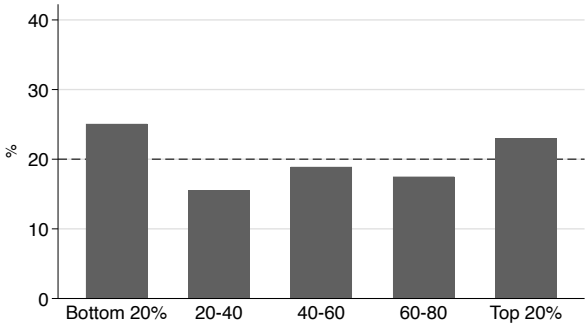
(B) SUBJECT TO EXAMS, AFTER



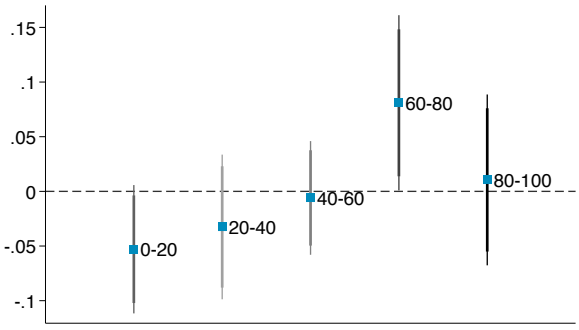
(C) NOT SUBJECT TO EXAMS, BEFORE



(D) NOT SUBJECT TO EXAMS, AFTER



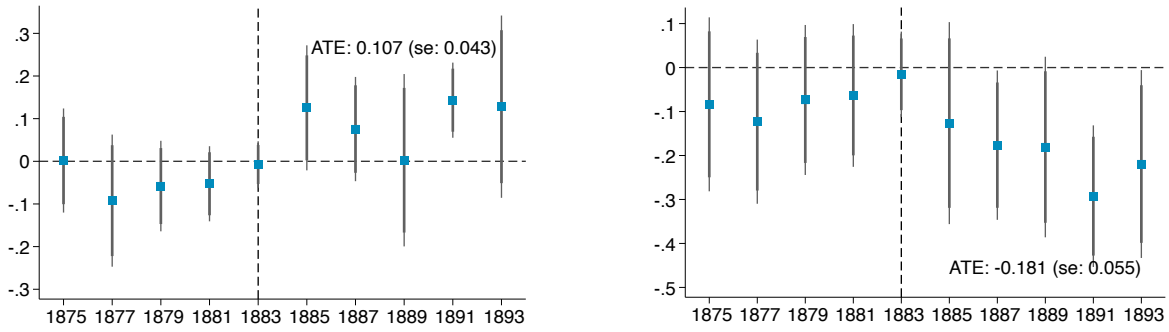
(E) DIFFERENCE-IN-DIFFERENCES ESTIMATES



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 6: 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 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.[55] 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 x% of 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 of the Register (for instance, when linking the 1881 register to the 1850 census we look for people aged 0 to 35 in 1850).

   (c) Have a first name and a last name within a Jaro-Winkler distance of c_1, where $c_1 \in [0, 1]$. The Jaro-Winkler distance is a string distance measure 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 lower the number of cases we will match someone to an incorrect individual.

   (d) There is no other potential link with a first name and a last name within a Jaro-Winkler distance of c_2, where $c_2 \in (c_1, 1]$. That is, we impose that, if the closest individual is within a Jaro-Winkler distance of c_1, the second closest potential match needs to be at a distance of at least c_2 with $c_2 > c_1$. For a given value of c_1, a higher value of c_2 represents a more conservative choice.

[55] 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.

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 Washington, DC in 1881. 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 A10 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).[56] Maximizing this function using the 1881 Register-1880 Census links leads to a choice of $c_1 =$ and $c_2 =$.

Figure A11 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. Panel (a) shows the proportion that we match to a census when the individual is below the age of 18 at the time of the census, whereas panel (b) shows the analogous figure for those that we match to an observation where the individual is more than 18.

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[57] at the time they worked in government. The Registers themselves do not include information on ages, but

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

[57] 18+(1871-1850).

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.[58] 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 background and characteristics 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 classified positions, before and after the implementation of the reforms. 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 classified occupations 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.

Finally, 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.

[58] 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	-0.00903
	(0.0332)	(0.0186)
Year FE	Yes	Yes
Position FE	Yes	Yes
App. State FE	Yes	Yes
Observations	23199	23199

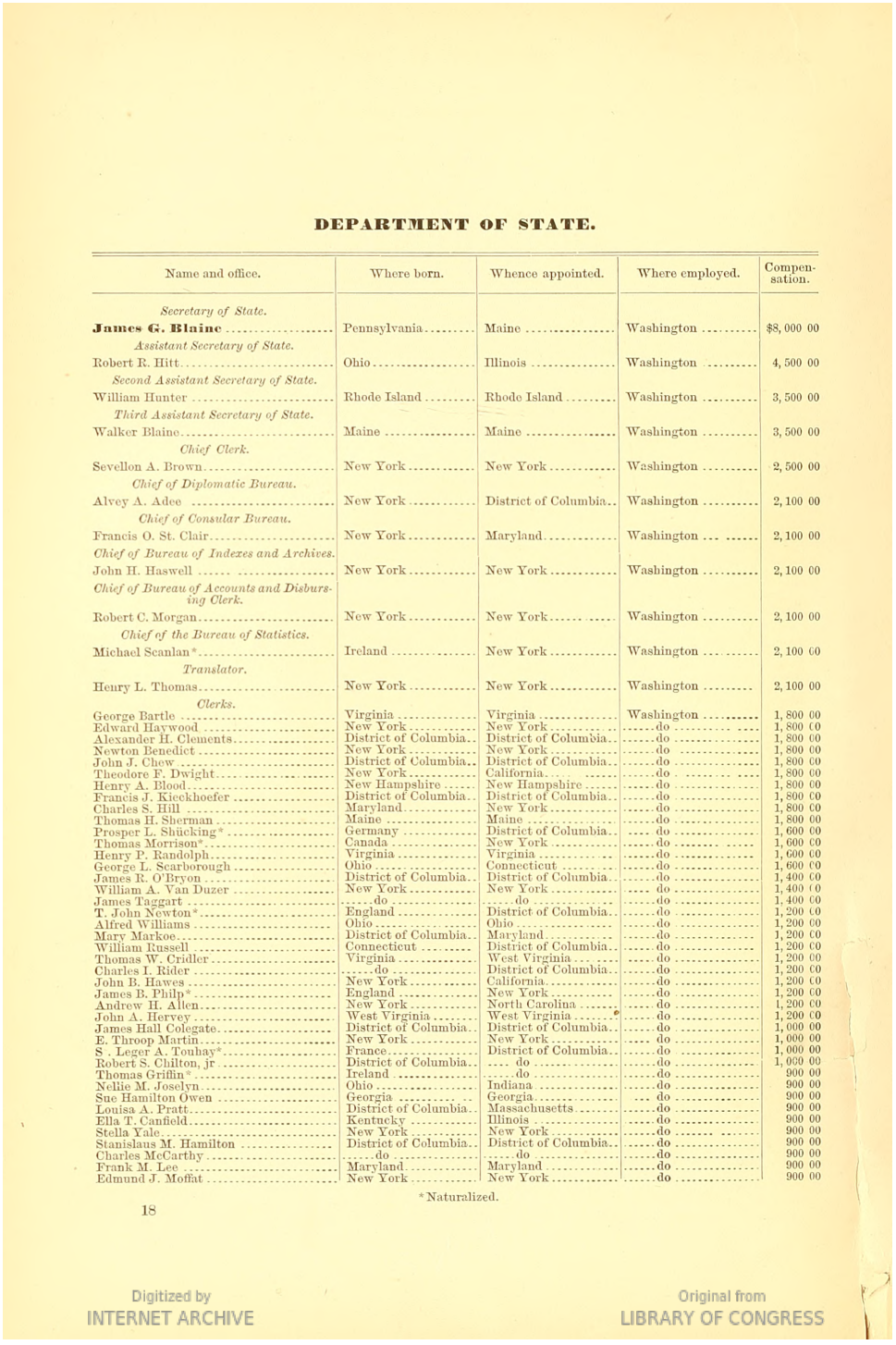
Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in column 1 is an indicator that takes a value of one if a an employee is successfully matched to at least one observation in the census. The dependent variables in column 2 is instead the total number of censuses to which an employee 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
Exam	-0.0419***	-0.0323**
	(0.0148)	(0.0127)
Year FE	Yes	Yes
Position FE	Yes	Yes
Appointing State FE	Yes	Yes
Observations	51233	28504

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.

FIGURE A1: 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

Digitized by INTERNET ARCHIVE
Original from LIBRARY OF CONGRESS
Generated for guest (University of California, Davis) on 2018-09-12 20:37 GMT / http://hdl.handle.net/2027/loc.ark:/13960/t1qf9gj6b
Public Domain / http://www.hathitrust.org/access_use#pd

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 A2: EXAMPLE EXAM QUESTIONS

(A) ARITHMETIC EXAM

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

(B) METEOROLOGICAL CLERK EXAM

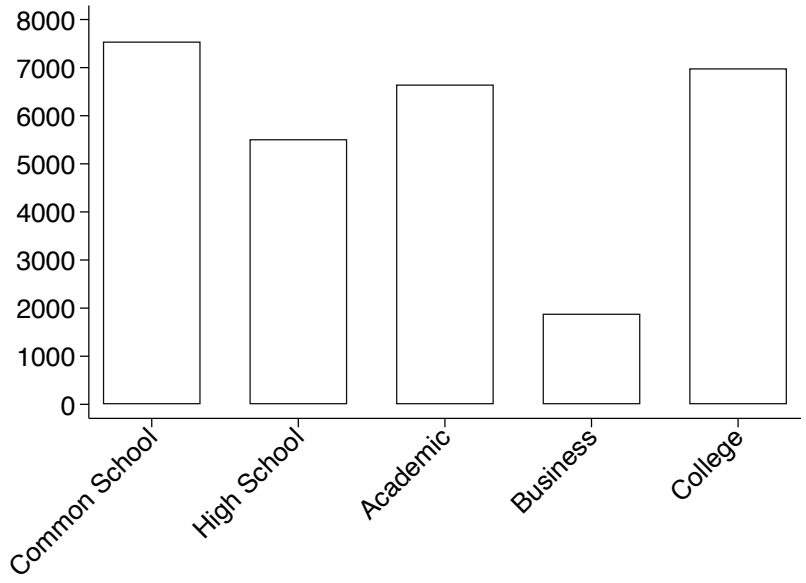
SIXTH SUBJECT.—*Meteorology.*

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?

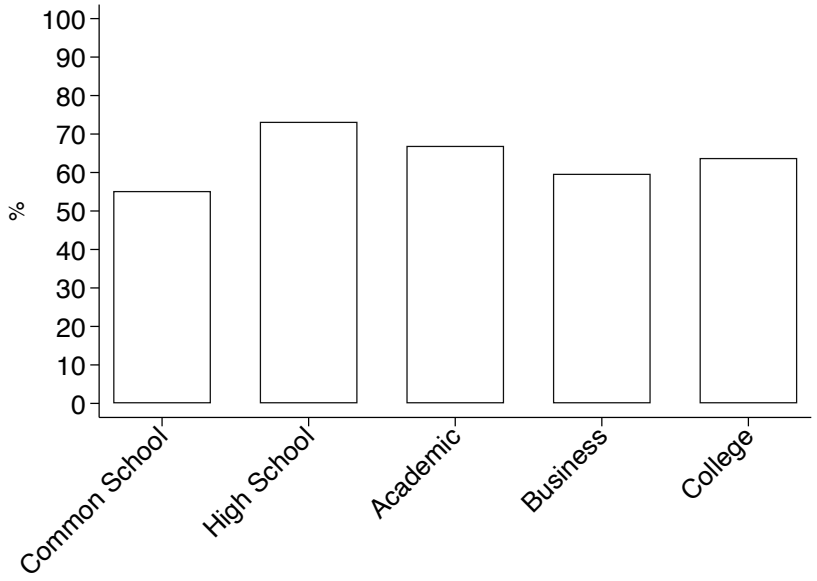
Notes: The two panels in this figure show example questions of the civil service exam. The figure in panel (a) shows an example question of the arithmetic exam. This exam was required for all employees taking either the "general" (for clerks) or "limited" (for copyists) examinations. The figure in panel (b) shows an example question for the special exam for "meteorological clerks" in the Department of Agriculture.

FIGURE A3: TOTAL NUMBER OF APPLICANTS AND EXAM PASSING RATES, BY EDUCATIONAL BACKGROUND 1887-1893

(A) NUMBER OF APPLICANTS



(B) PASSING RATE



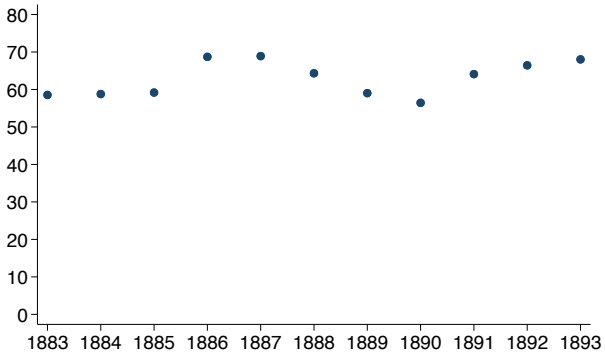
Notes: Panel (a) shows the number of applicants to the "Classified Departmental Service" in DC, by applicants' educational background. Panel (b) shows the fraction of such applicants who obtained a passing grade. These figures correspond to applicants who completed exams from 1886 to 1893, and are based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

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

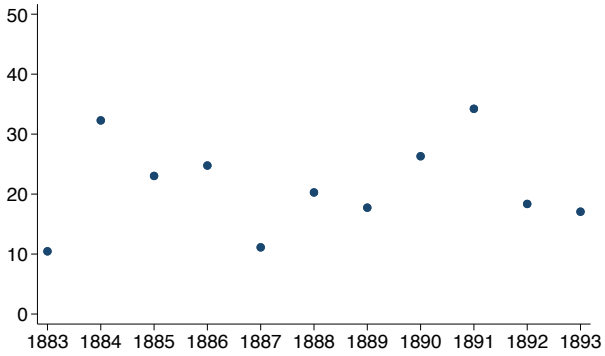
(A) NUMBER OF APPLICANTS



(B) PASSING RATE

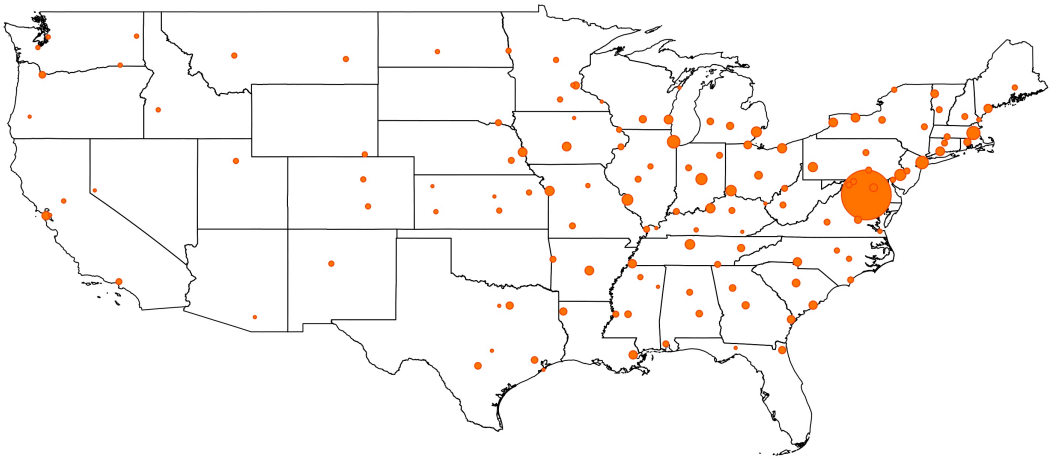


(C) APPOINTMENT RATE



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).

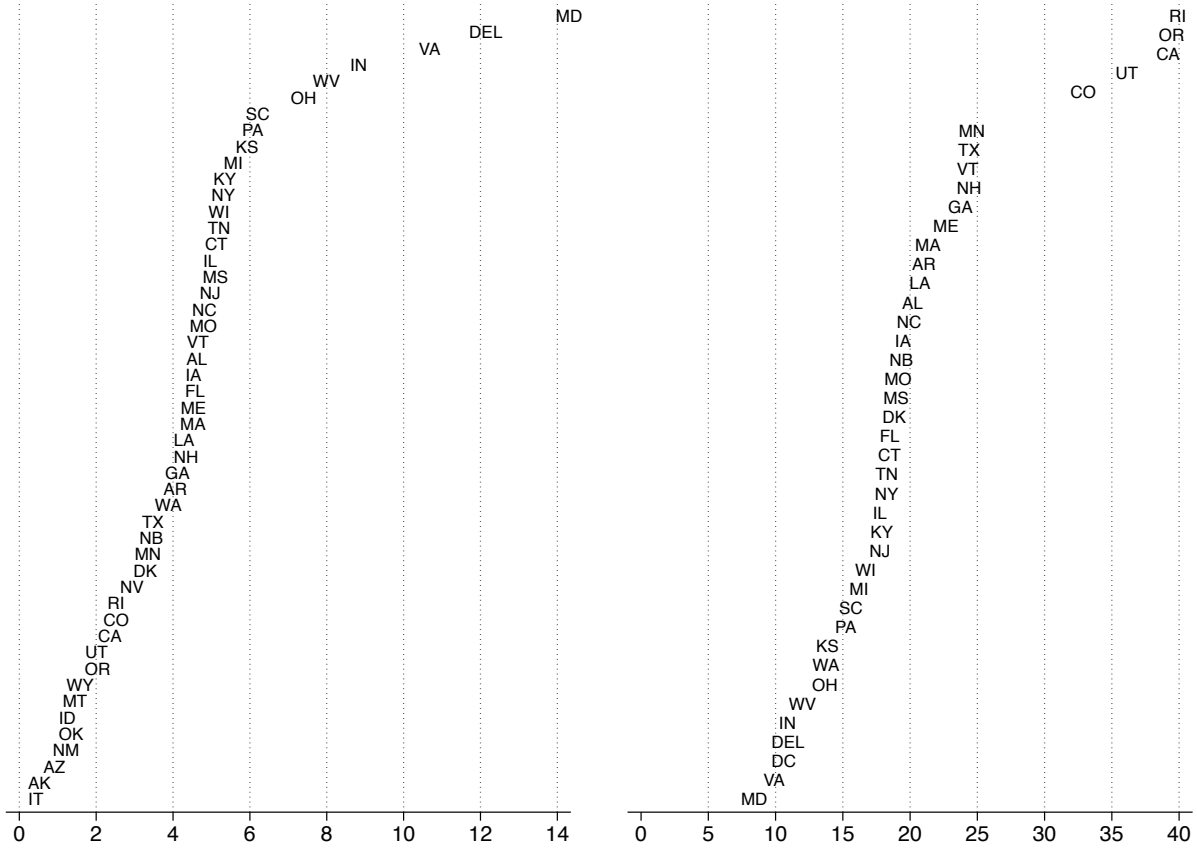
FIGURE A5: LOCATION OF CIVIL SERVICE EXAMS, 1886-1893



Notes: This map shows the location of all civil service exams that took place from 1886 to 1893. The 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.

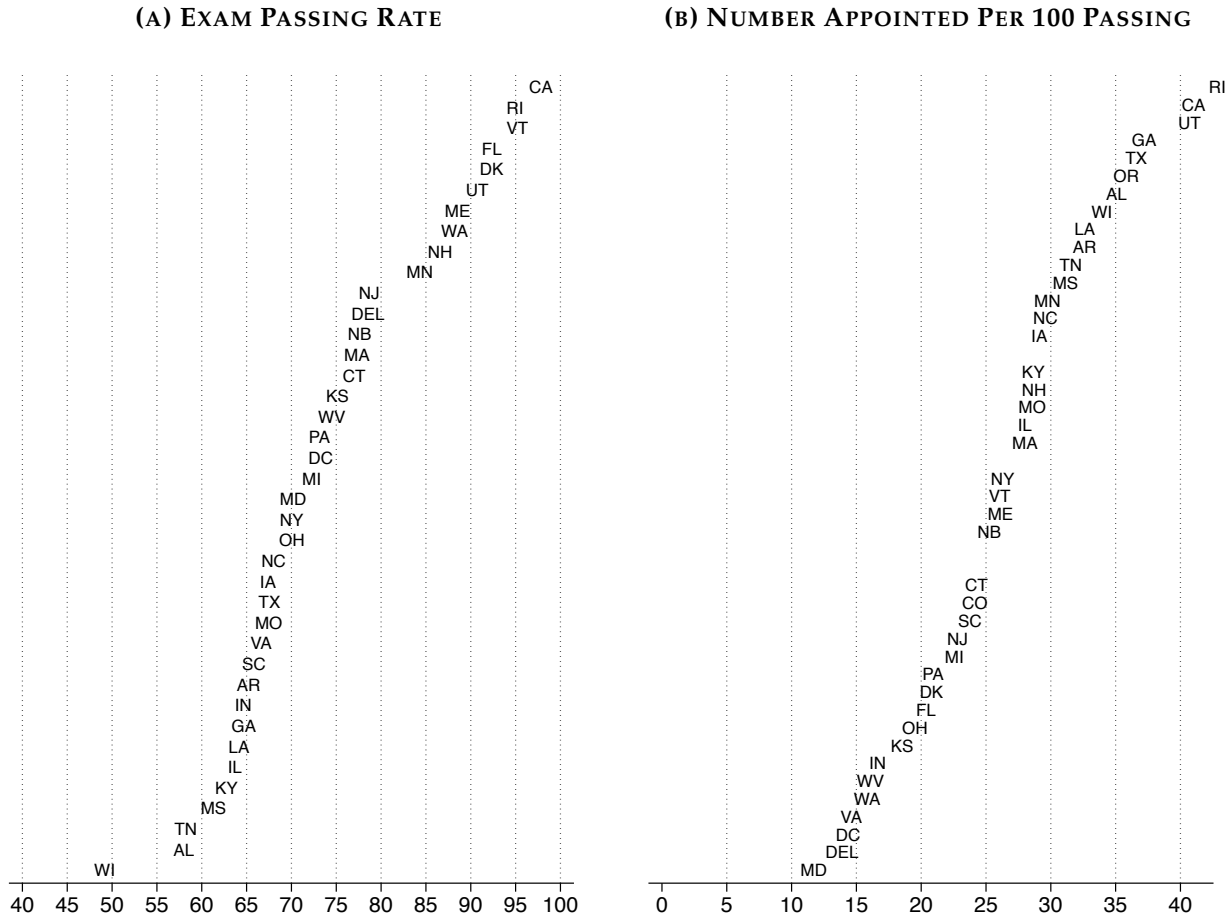
FIGURE A6: TOTAL NUMBER OF APPLICANTS AND SHARE APPOINTED, BY STATE

(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 A7: EXAM PASSING RATES AND SHARE APPOINTED, BY STATE



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 A8: 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 listing employees appointed to the classified departmental service.

FIGURE A9: 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

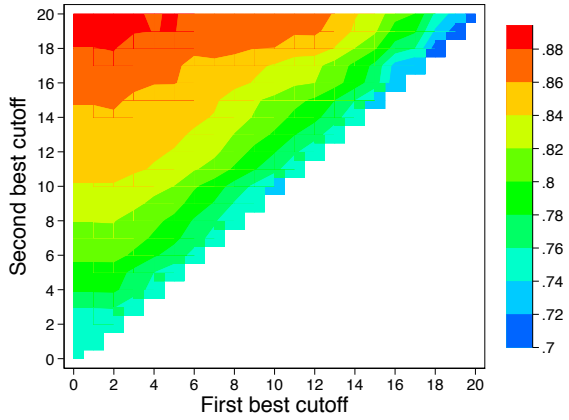
*The amount of compensation paid them during the fiscal year 1892.

Digitized by Google
Original from UNIVERSITY OF ILLINOIS AT URBANA-CHAMPAIGN
Generated for Santiago E Perez (University of California, Davis) on 2019-08-01 01:12 GMT / http://hdl.handle.net/2027/uiug.30112113390267
Public Domain, Google-digitized / http://www.hathitrust.org/access_use#pd-google

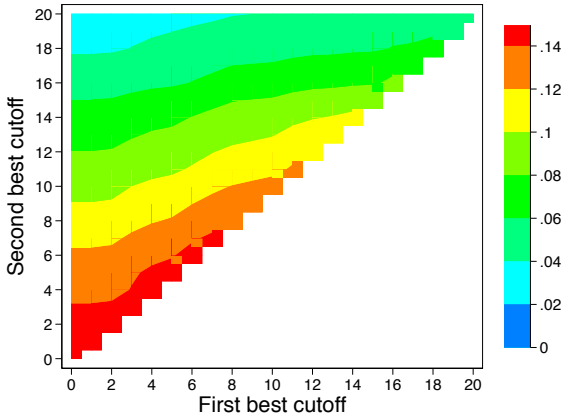
Notes: This figure shows an example page from the Civil Service Commission Reports listing the positions that were subject to exams in the Department of the Treasury.

FIGURE A10: POSITIVE PREDICTION VALUE AND TRUE POSITIVE RATE

(A) % MATCHES WITH MATCHING PLACE OF RESIDENCE



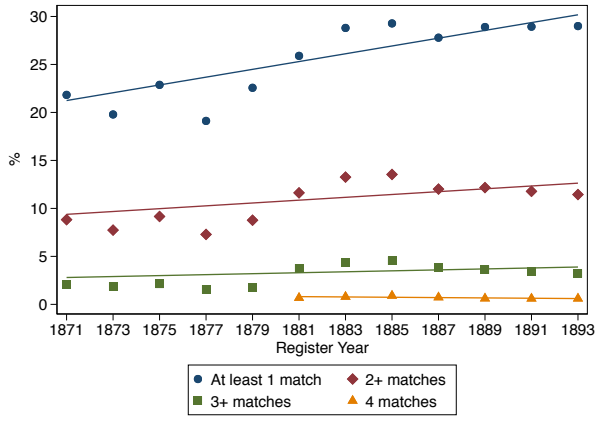
(B) % OBSERVATIONS WITH MATCHING PLACE OF RESIDENCE



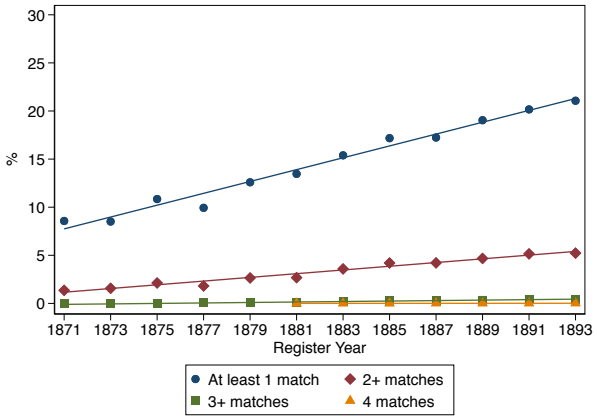
Notes: Panel (a) shows, out of all the individuals that we deem as a match, the proportion of individuals who are matched to someone living in a matching county of residence (based on their 1881 place of employment according to the Official Registers). Panel (b) shows, out all the individuals that we 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 A11: MATCHING RATES, BY REGISTER YEAR

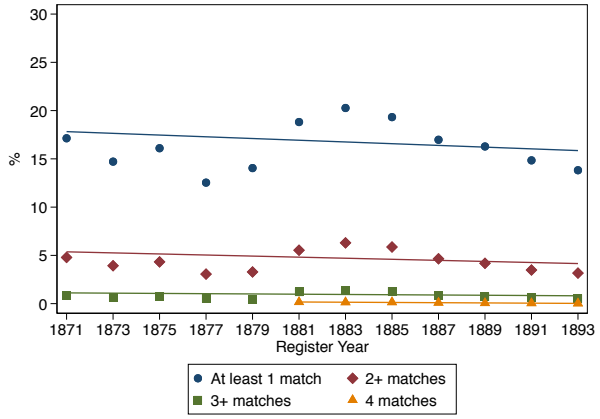
(A) % MATCHED



(B) % LINKED AS < 18 YEARS OLD



(C) % LINKED AS $18+$ YEARS OLD

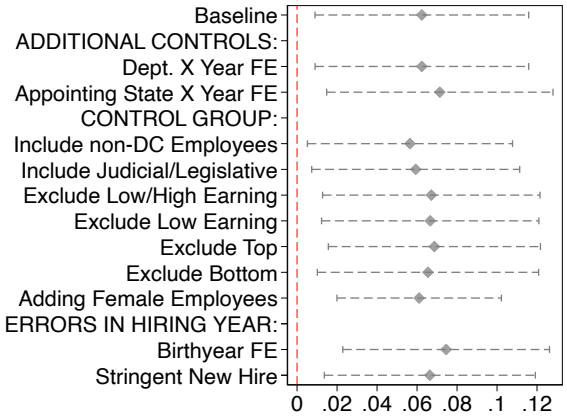


Notes: Panel (a) in this figure shows the proportion of individuals in each register that we match to at least one, at least two, at least three or exactly four censuses in our baseline sample. Panel (b) shows the corresponding proportion for individuals that we find when they are less than 18 years 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.

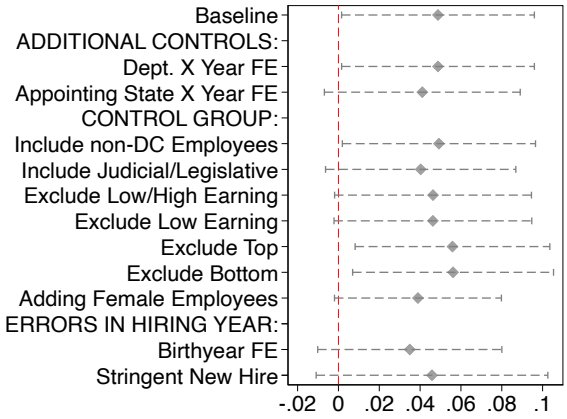
B Additional Results

FIGURE B1: ROBUSTNESS

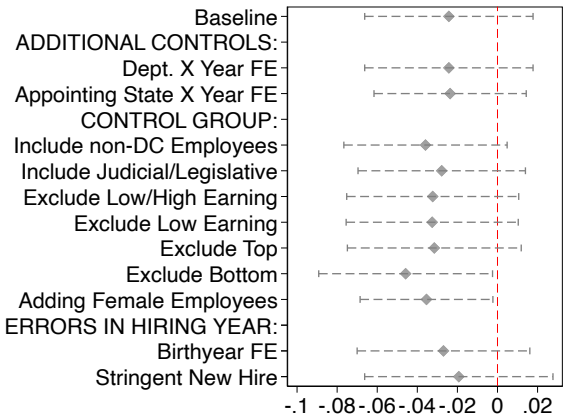
(A) FATHER WEALTH RANK



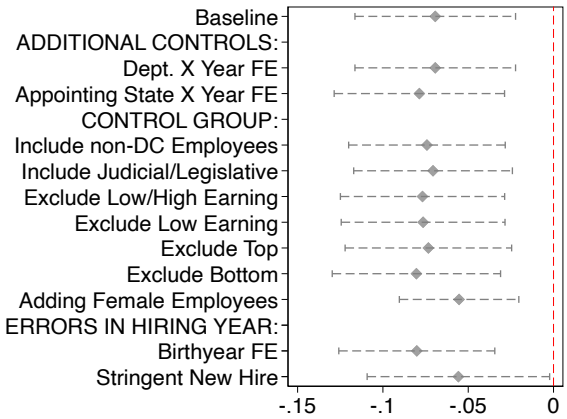
(B) FATHER PROFESSIONAL



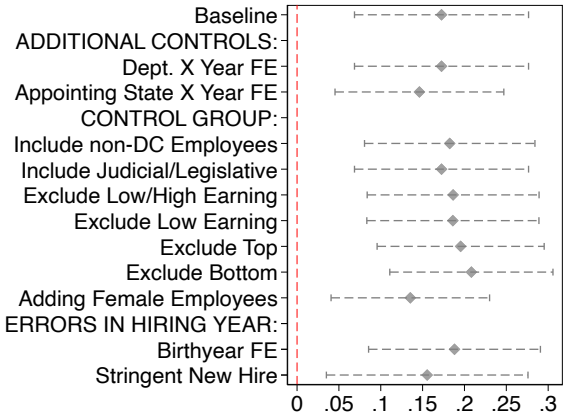
(C) FATHER UNSKILLED



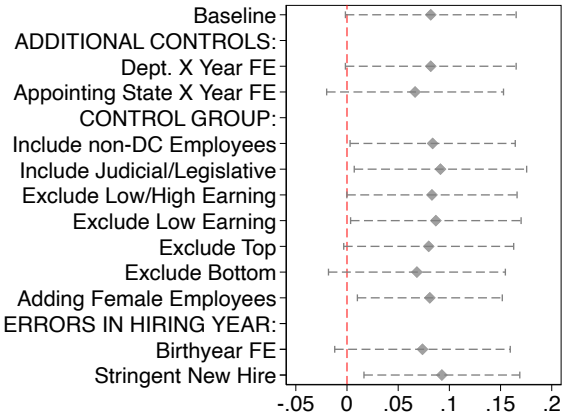
(D) IMMIGRANT PARENTS



(E) FATHER INDEX

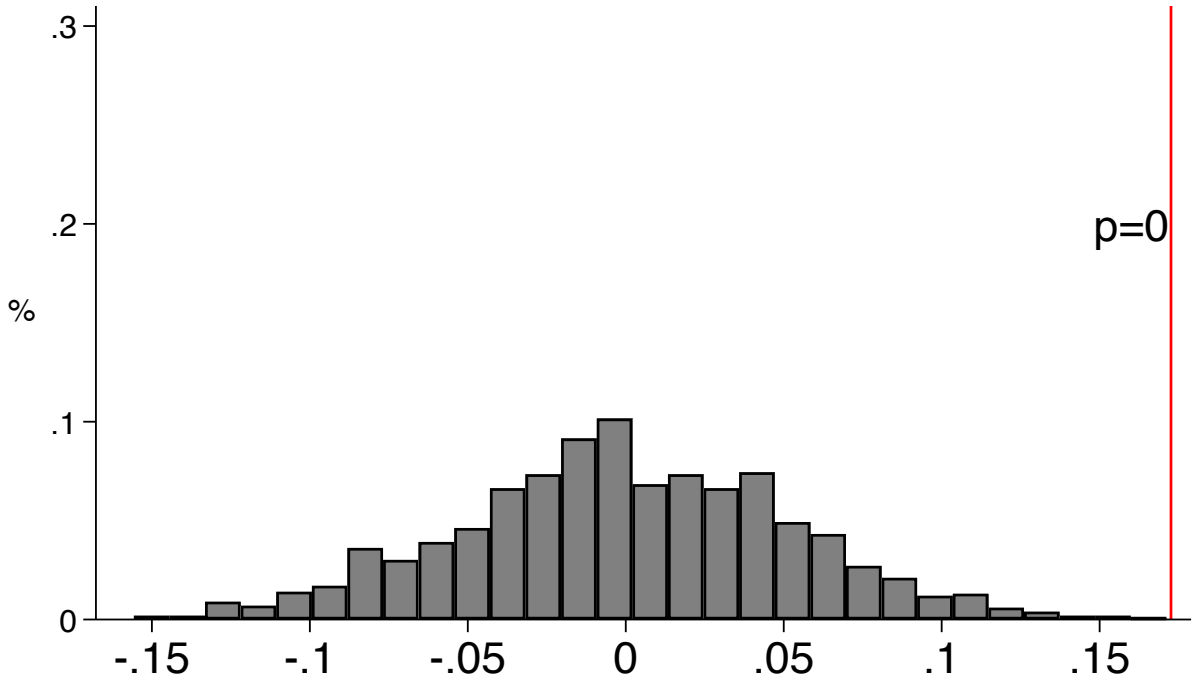


(F) OWN OCCUPATION PROFESSIONAL



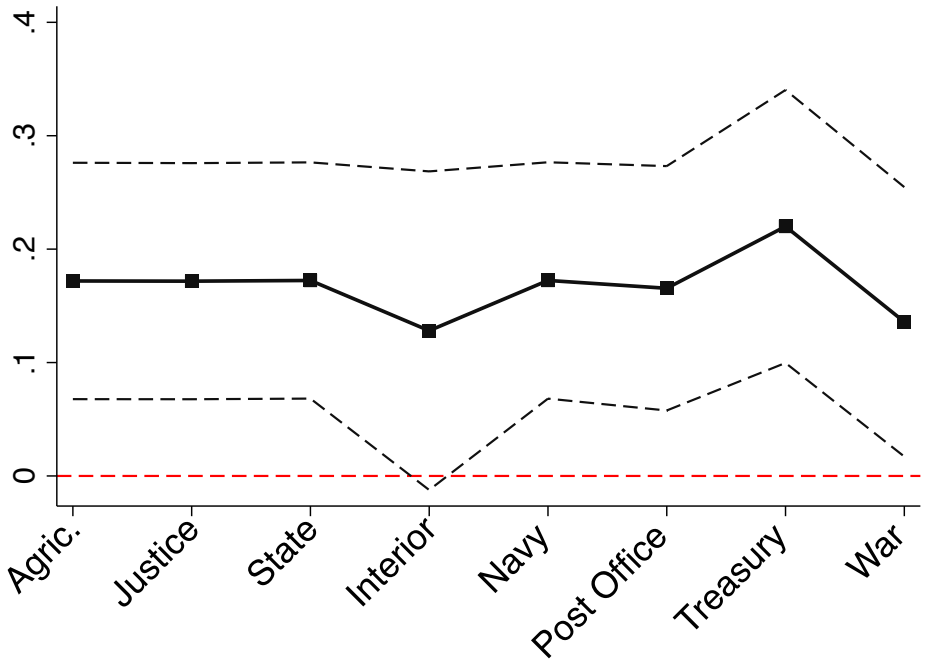
Notes: This figure presents the sensitivity of our difference-in-differences estimates to a number of alternative specifications and samples. The top row in each figure presents our baseline estimate. In the rows under "Additional controls" we add additional control variables to our baseline specification: (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 B2: 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).

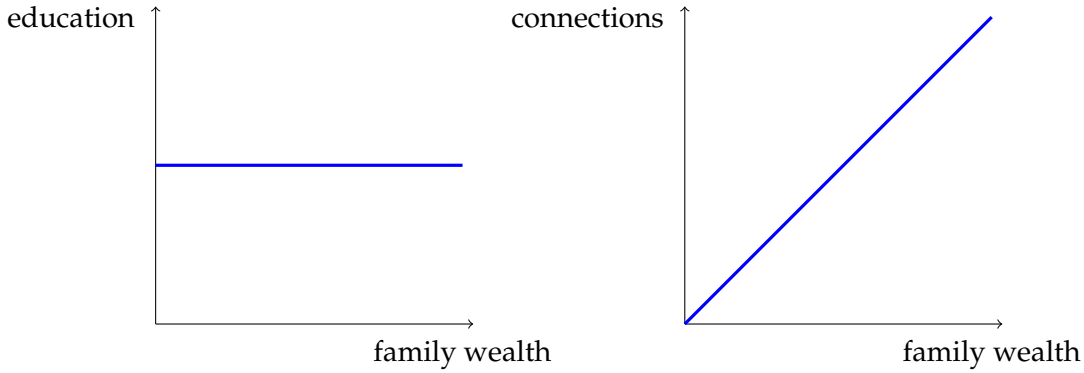
FIGURE B3: EXCLUDING ONE DEPARTMENT AT A TIME



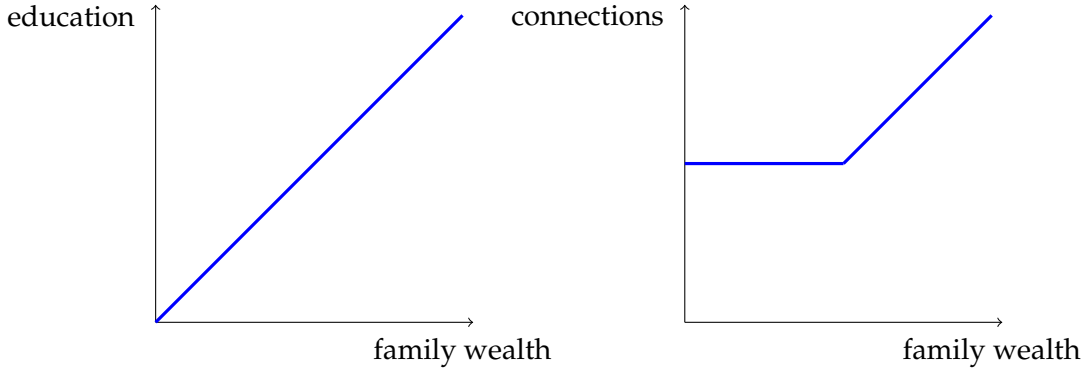
Notes: This figure shows the sensitivity of the effects of the reform on the summary index of social class (computed using the approach in Kling *et al.* (2007)) to excluding workers from one executive department at a time. The y-axis shows the estimated effect of exams on average parental wealth rank,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

FIGURE B4: AMBIGUOUS RELATIONSHIP BETWEEN EXAMS AND ECONOMIC MOBILITY

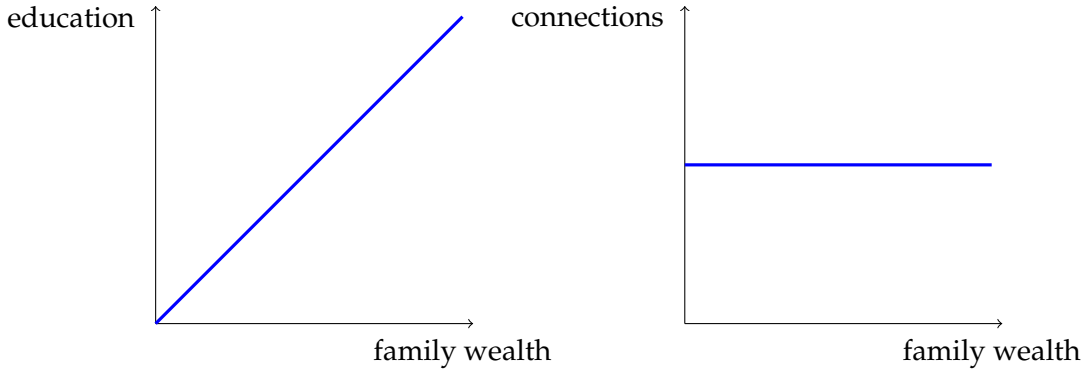
(A) EXAMS HELP THE "POOR"



(B) EXAMS HELP THE "MIDDLE"

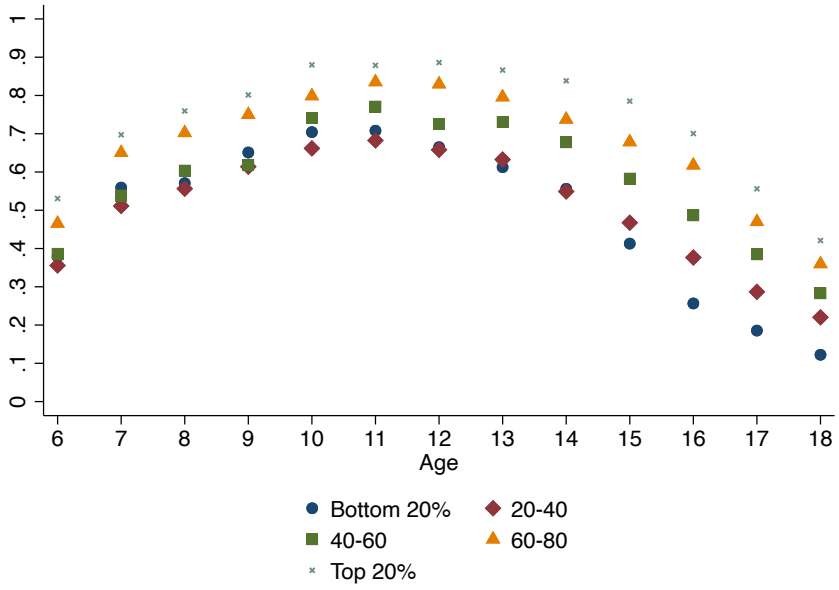


(C) EXAMS HELP THE "RICH"



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.

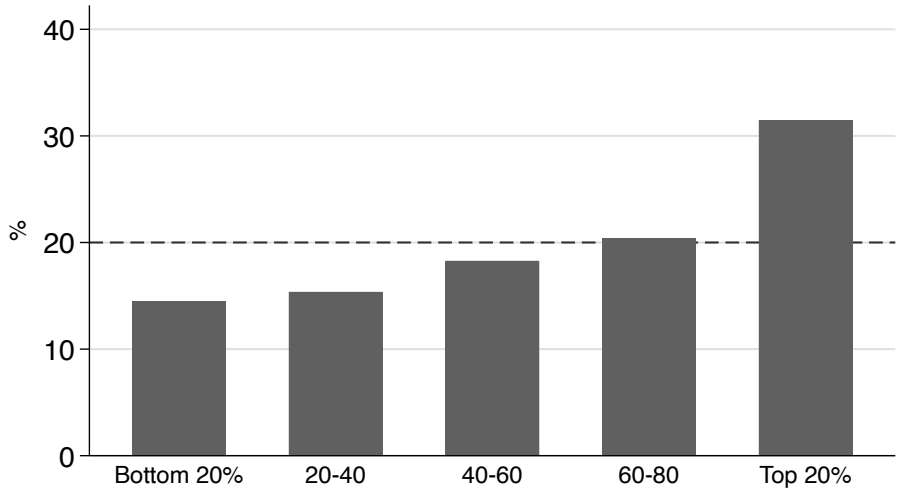
FIGURE B5: SCHOOL ATTENDANCE RATES BY AGE AND PARENTAL WEALTH QUINTILE



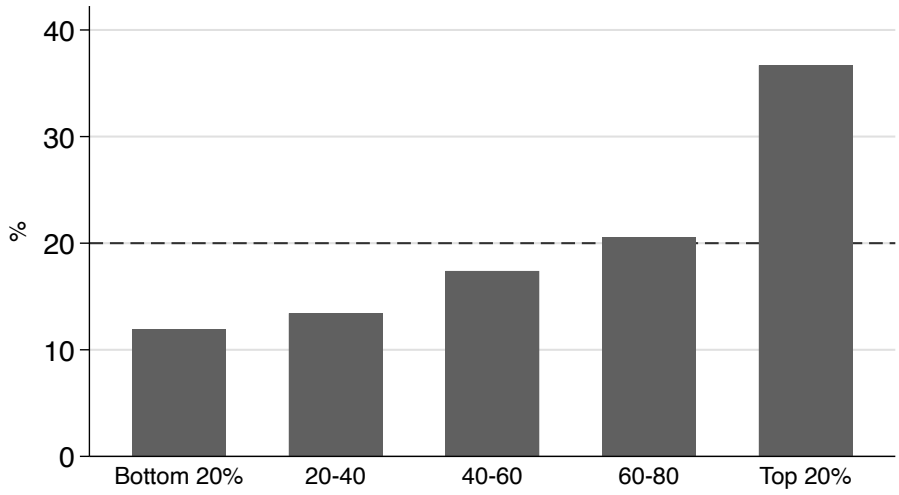
Notes: This figure shows school attendance rates for children of different ages, based on the wealth quintile of their parents. The figure is based on cross-sectional data from the 1870 population census.

FIGURE B6: 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.

TABLE B1: SUMMARY STATISTICS

	Non-Exam			Exam		
	Mean	Median	Observations	Mean	Median	Observations
	(1)	(2)	(3)	(4)	(5)	(6)
i. Parental Wealth Ranks						
Total	0.52	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 B2: PRE- AND POST-REFORM TRENDS IN MAIN OUTCOME VARIABLES

Outcome	Pre-1883		Post-1883	
	Mean	p-value	Mean	p-value
	(1)	(2)	(3)	(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 B3: 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 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 B4: EFFECTS OF EXAMS ON THE SHARE OF FOREIGN-BORN EMPLOYEES

	All Immigrants		Non-English-Speaking		English-Speaking		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 an immigrant 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 an immigrant 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 B5: 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	0.0951**	0.115	-0.196***	-0.186**	0.0623*	0.0801	-0.0126	0.0685	0.00304	-0.0221
	(0.0378)	(0.0867)	(0.0566)	(0.0848)	(0.0338)	(0.0706)	(0.0318)	(0.0586)	(0.0436)	(0.0745)
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	3883	1047	3883	1047	3883	1047	3883	1047	3883	1047
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 clustered at the position level. Standard errors are clustered at the position level.