

Online Appendix
Civil Service Exams and Organizational Performance:
Evidence from the Pendleton Act*

Diana Moreira
UC Davis, BREAD and NBER

Santiago Pérez
UC Davis and NBER

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*dsmoreira@ucdavis.edu, seperez@ucdavis.edu.

A Data Appendix

A.1 Linking the Official Registers to the Census: Linking Algorithm and Robustness

Linking algorithm. 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). This step uses the dictionary of nicknames that is available from [?.](#)
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 no birthplace information (about 1.5% of all observations).¹
 - (b) Have a reported age in the census such that they would have been between 18 and 65 years old at the time they are observed in the Official Registers (for instance, when linking the 1881 Register to the 1850 Census we only look for individuals 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 censuses and for OCR errors in our digitization of the Official Registers. Intuitively, the lower the value of c_1 the more conservative our linking approach (i.e. 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 $c_2 \geq c_1$. For a given value of c_1 , a higher value of c_2 represents a more conservative choice.

In our baseline analysis, we chose $c_1 = 0.07$ and $c_2 = 0.07$. In other words, we deem an observation as a match provided that it is the *unique* observation within a Jaro-Winkler distance of 0.07 with respect to both first and last names. For reference, the Jaro-Winkler distance between "Smith" and "Smih" is 0.046. However, Figure A7 shows that our results on the likelihood that an employee would have had a professional occupation prior to joining the customs service (our only

¹Importantly, there is no correlation between the likelihood of a missing birthplace and the reform.

result that relies on the linked data) are very similar when we implement alternative cutoffs for c_1 (including just using exact matches, i.e. $c_1 = 0$).

Figure A5 shows the proportion of individuals that we match to at least one working-age (i.e. when the individual was 18 to 65) observation in the census (and to at least two, three and four, respectively) when using our baseline choice of parameters, by Register year. In this figure, we focus on matches to censuses conducted prior to each register year (that is, when we focus on the 1871 Register we ask whether we are able link an individual to the 1850, 1860 or 1870 censuses). On average, we are able to find at least one match for about 20% of Customs Service employees. We expect a lower proportion of individuals in later years to be matched to at least one adult observation, as the latest census we include is 1880 and some employees would have been less than 18 years old by this year (particularly those employed in later years). Overall, these matching rates are similar to those in other studies using historical data (?).

Representativeness of Linked Data. In our analysis using linked data, we assess how the backgrounds of Customs Service employees changed with the passing of the Pendleton Act. Our sample in this analysis includes *only* employees of the Customs Service who were successfully linked to at least one observation in the census. Specifically, we compare the characteristics of bureaucrats in classified districts to those in non-classified districts, 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 *differently* for individuals in classified districts after the reform. This is unlikely because our linking procedure is exactly the same throughout all sample years and across districts.

We implement several empirical exercises to further alleviate the concern that our results could be driven by differential matching into the linked sample:

First, we estimate our main difference-in-differences specification using as outcome variables: (1) the total number of censuses to which we link an employee, or (2) an indicator that takes a value of one if the employee is linked to at least one adult census. Figure A6 shows that, while employees in non-classified districts are overall more likely to be matched throughout the period of analysis, there is no evidence that such difference became larger or smaller after the reform. Indeed, Table A1 in the Online Appendix shows that there is no correlation between the reform and the likelihood of matching an individual to a census. Hence, it is very unlikely that the change in the background of employees that we document is due to biases in linking.

Second, in Table A2 we investigate the correlation between workers' individual-level characteristics and the likelihood of matching. Specifically, we show the relationship between the matching probability and the following individual-level characteristics: whether the person is foreign born, their (log) annual compensation, their likelihood of working in an exempted occupation, and their turnover. Foreign-born individuals appear to be slightly overrepresented in the sample (likely due to their more unique names). However there is only a weak relationship between the matching probability and the other characteristics: compensation, working in an occupation below the classification cutoff, and employee turnover.

With these small differences in observable characteristics in mind, Table A4 shows that our results are also similar when we reweight the data to account for differences in the matching probability across individuals with different observable characteristics. To implement this exercise, we follow a standard approach in papers using linked historical data (see, for instance, ? and ?). The approach has the following two steps:

1. We estimate a probit model of the probability of matching using the following set of characteristics: birthplace dummies, collection district dummies, register year dummies and compensation dummies (in \$100 intervals).
2. We reweight the data using the inverse matching probability based on the estimated probabilities in the probit model.

Finally, Table A3 shows that our main results on employee turnover and the likelihood that an employee would work in an exempted position (which *do not* require the linked data) are very similar when we estimate them on the smaller linked sample.

TABLE A1: NO EFFECTS OF THE REFORM ON THE PROBABILITY OF MATCHING, DIFFERENCE-IN-DIFFERENCES ESTIMATES

	(1) At least 1 match	(2) N. of matches
Classified X After	-0.0131 (0.0247)	-0.0122 (0.0334)
District FE	Yes	Yes
Year FE	Yes	Yes
Observations	45323	45323
Clusters	46	46
Mean of dep. var.	0.204	0.273

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 Customs Service employee is successfully matched to at least one adult observation in the census. The dependent variables in column 2 is instead the total number of censuses to which a Customs Service employee is matched to an adult observation. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE A2: RELATIONSHIP BETWEEN MATCHING PROBABILITY AND INDIVIDUAL-LEVEL CHARACTERISTICS

	(1)	(2)	(3)	(4)
Foreign born	0.0403*** (0.0117)			
log (Annual Compensation)		0.00829 (0.0134)		
Below Cutoff			-0.0195* (0.0115)	
Turnover				-0.00201 (0.00550)
Observations	40781	40781	40781	40781
Clusters	46	46	46	46
Mean of dep. var	0.208	0.208	0.208	0.208

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is an indicator that takes a value of one if a Customs Service employee is successfully matched to at least one adult observation in the census. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

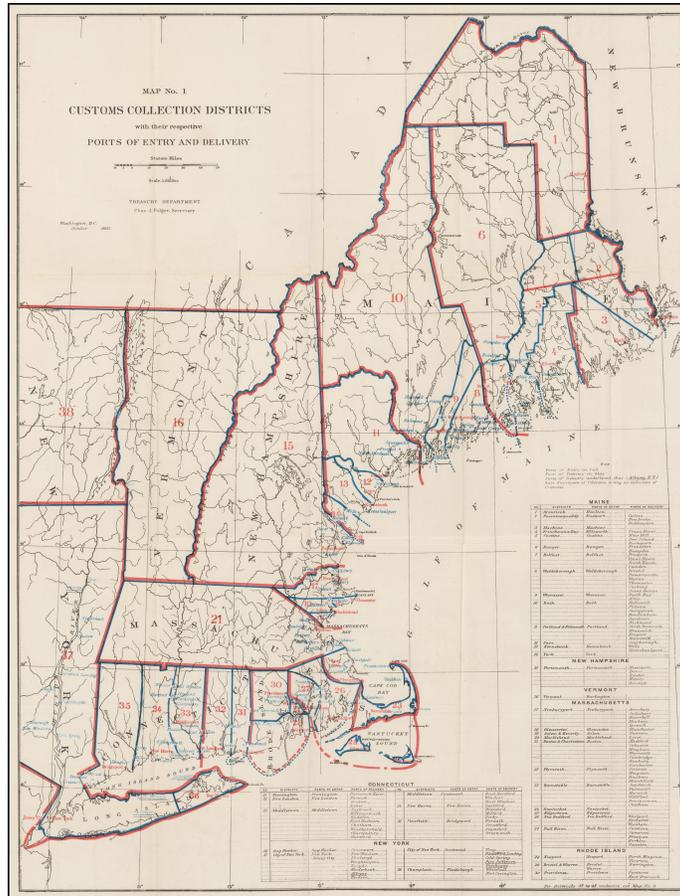
TABLE A3: ALL PERSONNEL OUTCOMES, LINKED SAMPLE

	Turnover		Below Cutoff	
	(1)	(2)	(3)	(4)
Classified X After	-0.116*** (0.0273)	-0.105** (0.0402)	0.169*** (0.0297)	0.132*** (0.0318)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	41680	8572	45323	9225
Clusters	46	46	46	46
Sample	All	Linked	All	Linked

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table shows the robustness of our personnel results that *do not rely* on linked register-to-census data to using the same linked sample that we use when studying the effects of the reform on employees' occupational background. Columns 1 and 3 use the full sample of personnel records. Columns 2 and 4 use only the observations that we can match to at least one observation in the census. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

FIGURE A1: CUSTOMS COLLECTION DISTRICTS IN 1883

(A) NEW ENGLAND



(B) MIDDLE ATLANTIC

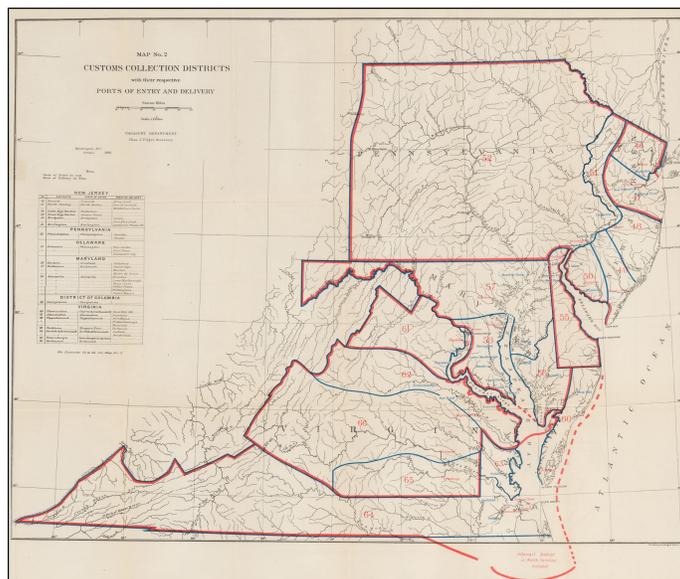


FIGURE A2: EXAM FOR APPLICANTS TO THE CUSTOMS SERVICE, EXAMPLE QUESTIONS

(A) ARITHMETIC

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

5,673,911,987 87
 44,376,013,705 90
 32,673,231,695 25
 7,736,910,286 16
 6,444,642,155 14
 44,297,763,429 39
 26,105,321,266 57
 9,708,132,873 63
 8,856,764,397 49
 42,231,001,161 86
 63,497,476,084 03
 123,435,602,002 90

(B) BOOKKEEPING

SIXTH SUBJECT.—*Elements of book-keeping and of accounts.*

Exercise.—Samuel Adams, a contractor, had the following dealings with the Treasury Department: He furnished, January 3, 1883, 2,575 lb. of twine, at 12 cents a lb.; April 4, '83, 25 doz. gold pens, at \$25 a doz.; May 7, '83, 645 reams letter-paper, at \$2 a ream; July 9, '83, 45 doz. qt. Arnold's ink, at \$3 a doz.; October 30, '83, 1,000,000 envelopes, at \$2 a thousand; and December 5, '83, 8 doz. inkstands, at \$1.97 a doz. He was paid cash as follows: February 4, 1883, \$175; April 30, \$350; July 15, \$700; November 5, \$2,300; and December 31, 1883, he was allowed on settlement \$45 for cartage, and charged \$75 for breakage and \$60 for shortage on envelopes. State his account in the blank below, with proper heading, and show the balance, if any, due him.

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Notes: This figure shows example questions for applicants to the classified Customs Service. Panel (a) shows a question corresponding to the arithmetic exam, whereas Panel (b) shows a question corresponding to the bookkeeping exam.

FIGURE A3: EXAMPLE PAGE, OFFICIAL REGISTERS OF THE UNITED STATES

1, 1883.] TREASURY DEPARTMENT. 201					
Customs Service.					
Name.	Office.	Where born.	Whence appointed.	Where employed.	Compen- sation.
Joseph Jewett	Clerk	Massachusetts	New York	New York	\$1, 600 00
George H. Keim	do	New York	do	do	1, 600 00
Berrien Keyser	do	do	do	do	1, 600 00
Louis Oppenheim	do	do	do	do	1, 600 00
Samuel P. Putnam	do	New Hampshire.	do	do	1, 600 00
James H. Thayer	do	Massachusetts.	do	do	1, 600 00
Theodore D. Wilson	do	Pennsylvania	do	do	1, 600 00
Edward E. Worl	do	do	do	do	1, 600 00
George W. Marston	do	New Hampshire.	do	do	1, 550 00
Michael Carey	do	Ireland	do	do	1, 400 00
Herman G. Carter	do	New York	do	do	1, 400 00
Calvin C. Church	do	do	do	do	1, 400 00
William B. Crawford	do	do	do	do	1, 400 00
Alfred Eaton	do	do	do	do	1, 400 00
Stephen B. Gregory	do	do	do	do	1, 400 00
Charles B. Jenney*	do	do	do	do	1, 400 00
Oliver W. Marvin	do	do	do	do	1, 400 00
John H. Walsh	do	do	do	do	1, 400 00
John Welch, jr	do	Massachusetts	do	do	1, 400 00
Thomas S. Woodcock	do	New York	do	do	1, 400 00
George P. Babcock	do	Connecticut	do	do	1, 200 00
Theodore Babcock, jr	do	New York	do	do	1, 200 00
John J. Barnicle	do	do	do	do	1, 200 00
Thomas H. Bryden	do	do	do	do	1, 200 00
Ogden D. Budd	do	do	do	do	1, 200 00
Samuel G. Burns*	do	do	do	do	1, 200 00
Frederick S. Cooke*	do	do	do	do	1, 200 00
George W. Cooney*	do	do	do	do	1, 200 00
Anthony Gross	do	Austria	do	do	1, 200 00
Edward H. Jones	do	England	do	do	1, 200 00
George Kleine	do	New York	do	do	1, 200 00
James B. Martine*	do	North Carolina	do	do	1, 200 00
John O'Shea	do	New York	do	do	1, 200 00
Charles E. Parsons	do	do	do	do	1, 200 00
James M. Smith	do	do	do	do	1, 200 00
Lewis A. Strahan*	do	do	do	do	1, 200 00
William P. Thomson	do	do	New Jersey	do	1, 200 00
Benson Van Voast*	do	do	New York	do	1, 200 00
Leonard Wightman	do	do	do	do	1, 200 00
Stephen B. Goszler	Clerk and messenger	Dist. Columbia	New Jersey	do	1, 000 00

Notes: This figure shows an example page of the "Official Registers of the United States" corresponding to employees of New York's collector's office in 1883. Source: ?.

FIGURE A4: EXAMPLE OF A COLLECTOR WHO DIED WHILE IN OFFICE

DEC. 19, 1889.] EXECUTIVE JOURNAL. 245

To the Senate of the United States:

I nominate T. Jefferson Jarrett, of Virginia, to be collector of customs for the district of Petersburg, in the State of Virginia, to succeed Peter F. Cogbill, deceased.

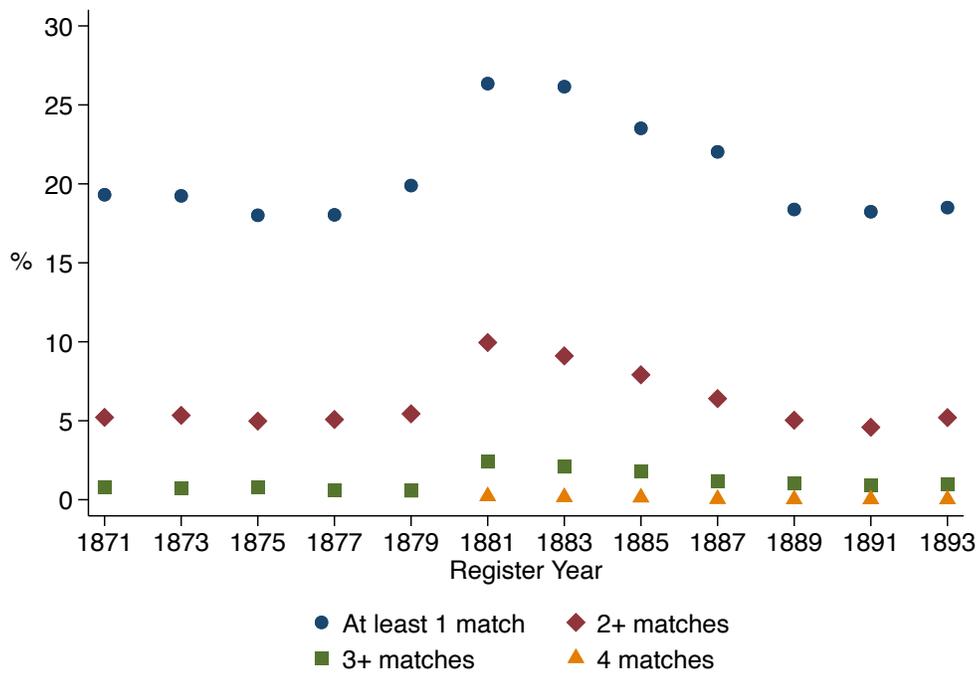
Mr. Jarrett was temporarily commissioned during the recess of the Senate, June 13, 1889.

BENJ. HARRISON.

EXECUTIVE MANSION, *December 19, 1889.*

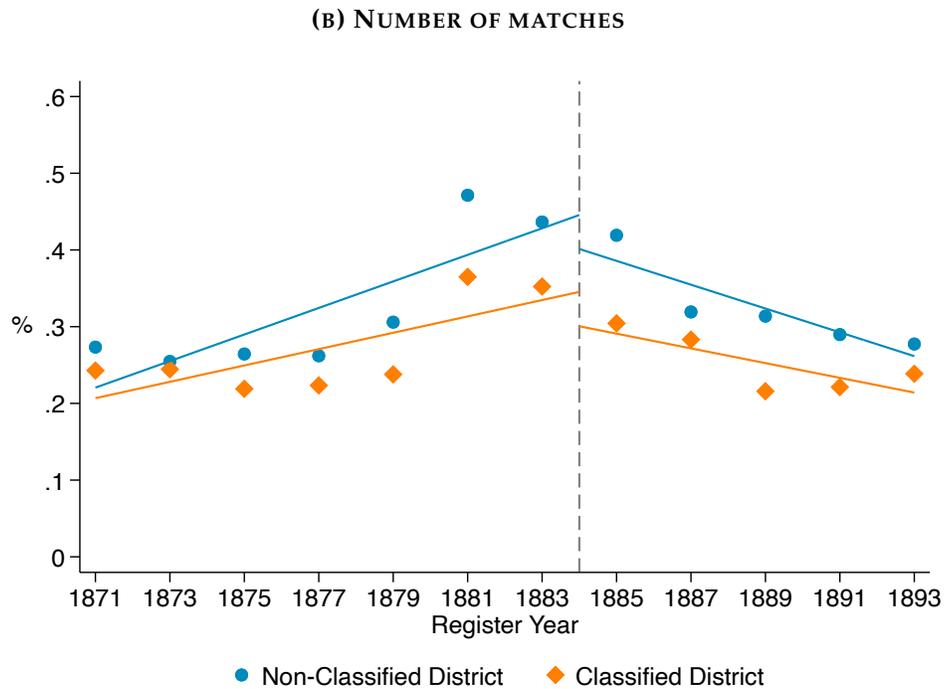
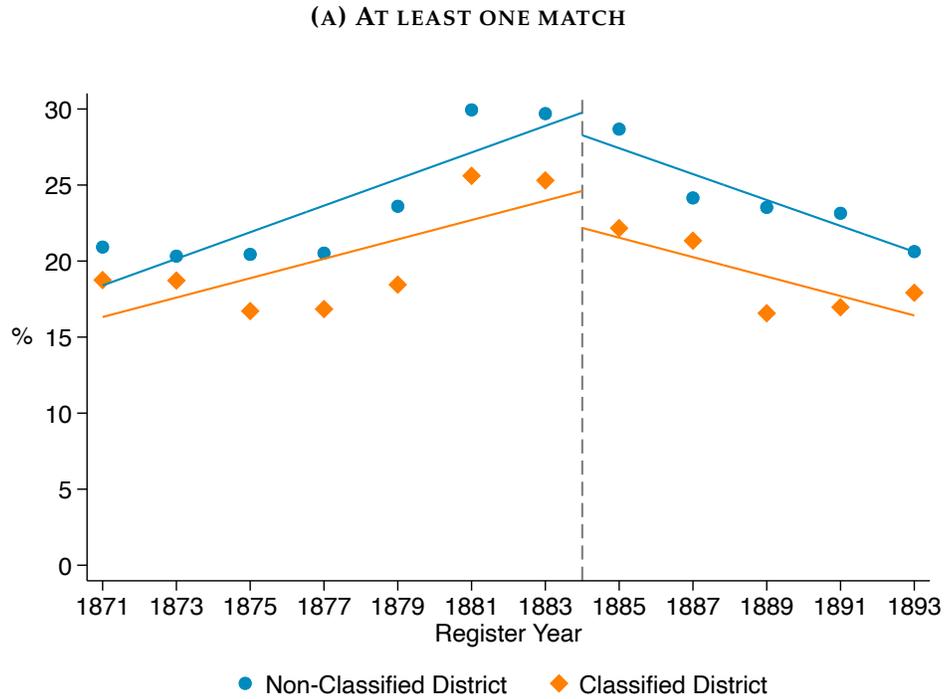
Notes: This figure shows an example page of the *Journal of the Executive Proceedings of the United States Senate* (?). This page lists the nomination of a new collector who would replace a collector who died while in office.

FIGURE A5: REGISTER-TO-CENSUS MATCH RATES, BY REGISTER YEAR



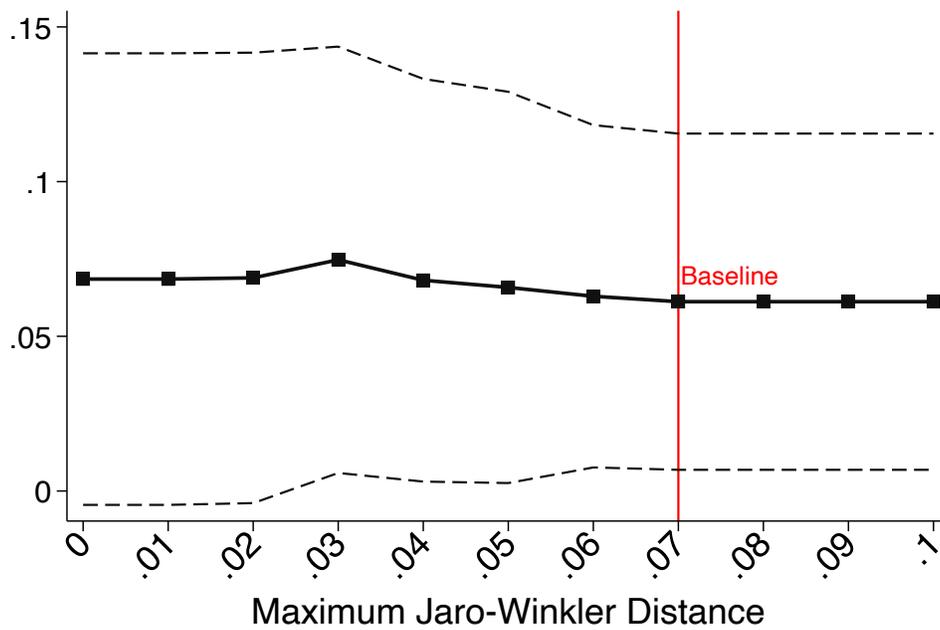
Notes: This figure shows the percent of Customs Service employees that are matched to at least one, two, three or four working-age (aged 18 to 65) observations in the population census, by register year. The sample is restricted to districts with at least 10 employees by 1883.

FIGURE A6: REGISTER-TO-CENSUS MATCH RATES, BY REGISTER YEAR AND CLASSIFICATION STATUS



Notes: Panel (a) shows the percent of Customs Service employees that are matched to at least one observation in the census, by register year and depending on whether the individual worked in a classified or a non-classified district. Panel (b) shows instead the average number of censuses to which an individual is linked. The sample is restricted to districts with at least 10 employees by 1883.

FIGURE A7: ROBUSTNESS TO ALTERNATIVE JARO-WINKLER CUTOFFS



Notes: This figure shows the estimated effect of the reform on the likelihood that a Customs Service employee would have held a professional occupation (y-axis), as a function of the minimum Jaro-Winkler string distance above which an observation would no longer be considered a match (x-axis). Lower values of the Jaro-Winkler distance represent more conservative matches: A Jaro-Winkler distance of zero correspond to two identical strings, whereas a distance of one correspond to two strings with no common characters. The red vertical bar corresponds to the cutoff used in the baseline approach. The sample is restricted to districts with at least 10 employees by 1883.

TABLE A4: ROBUSTNESS TO REWEIGHTING LINKED SAMPLE

	Turnover		Below Cutoff		Professional Occ.	
	(1)	(2)	(3)	(4)	(5)	(6)
Classified X After	-0.105** (0.0402)	-0.109** (0.0446)	0.132*** (0.0318)	0.141*** (0.0366)	0.0740** (0.0283)	0.0883*** (0.0282)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8572	8572	9225	9225	2033	2033
Clusters	46	46	46	46	45	45
Mean of dep. var.	0.362	0.362	0.167	0.167	0.0821	0.0821
Sample	Linked	Linked, reweighted	Linked	Linked, reweighted	Linked	Linked, reweighted

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table shows the robustness of our personnel results to reweighting the data to account for differences in the matching probability of different employees. To compute the weights, we first estimate a probit model of the probability of matching using the following set of characteristics: birthplace dummies, collection district dummies, register year dummies, and compensation dummies (in \$100 intervals). We then reweight the data using the inverse matching probability estimated in the probit model. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

B Additional Results

B.1 Robustness of Personnel Results

Table B3 shows that the effects of the “reform” on personnel outcomes are all small and statistically insignificant when we use placebo cutoffs of 20, 30 or 40 employees (instead of 50) for the minimum number of employees above which a district would have been subject to the reform. In this table, we focus on districts with less than 50 employees and estimate our main difference-in-differences specification using these placebo cutoffs. We restrict the sample to districts with fewer than 50 employees because otherwise the “placebo” treatment group would mechanically include the actual set of reformed districts (i.e. those with 50 or more employees).

In Figure B1, we implement a randomization inference approach for computing p-values. Specifically, we estimate the effects of 1,000 placebo “reforms” in which we randomly choose 11 districts as being “classified”. We then compare the estimated effects of these placebo reforms to the effects that we obtain when using the actual set of reformed districts in the estimation. Our estimated effects are always significantly larger in absolute value than the ones corresponding to the placebo reforms.

Since we have a relatively small number of classified districts, a concern is that the effects of the reform might have been driven by changes taking place in one specific classified district. In Figure B2, however, we show that the results are similar when we estimate our baseline difference-in-differences specification while excluding one classified district at a time. The x-axis in this figure indicates the district that we exclude from the regression, and the y-axis shows the estimated effect of the reform on each personnel outcome. The figure shows that the results are very stable regardless of which district we exclude. Hence, our findings are unlikely to be driven by concurrent changes unrelated to the reform that took place in a specific district.

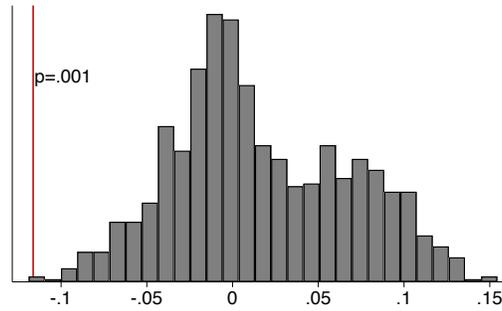
In our baseline specification, our control group is comprised of districts with 10 or more employees by 1883. We use this control group since it has similar pre-trends than the classified districts with respect to all of our main personnel and financial outcomes. However, Table B2 shows that we continue to find very similar results if we use alternative control groups with fewer (where we do not impose any restrictions on the minimum number of 1883 employees), or more (where we use a cutoff of 20+ employees) employees by 1883.

In Table B6, we show that our main results on personnel outcomes are similar when we estimate regressions at the district level (rather than at the employee level). In this table, the data are collapsed at the district-year level.

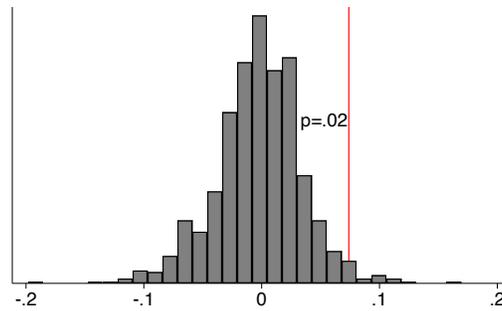
Finally, Table B5 shows that our personnel results are also robust to controlling for: (1) census region-year fixed effects, (2) interactions between a district number of employees in 1883 and year fixed effects, or (3) both at the same time. These results make it unlikely that our findings would be driven by pre-existing differential trends between districts of different size, or by differential trends across broad US regions.

FIGURE B1: PERSONNEL OUTCOMES, RANDOMIZATION INFERENCE

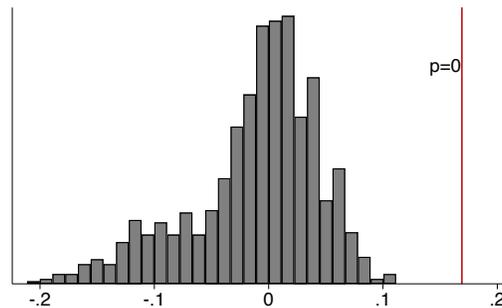
(A) TURNOVER



(B) PROFESSIONAL OCCUPATION



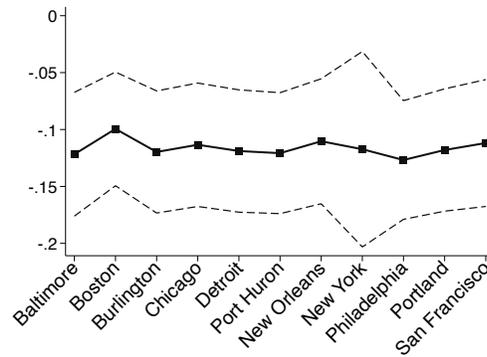
(C) BELOW CLASSIFICATION CUTOFF



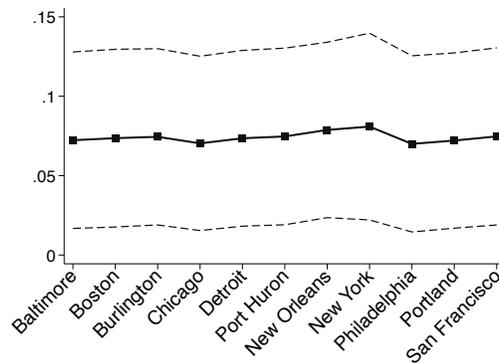
Notes: These figures show the empirical distribution of estimated effects when we implement a randomization inference approach. In this exercise, we randomly select eleven districts as being classified and estimate the “effects” of the reform using our baseline difference-in-differences model. We repeat 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 classified districts. The specification and outcome in panel (a) correspond to those in column 1 of Table ???. The specification and outcome in panel (b) correspond to those in panel (a), column 7 of Table ???. The specification and outcome in Panel (c) correspond to those in column 5 of Table ??.

FIGURE B2: PERSONNEL OUTCOMES, EXCLUDING ONE DISTRICT AT A TIME

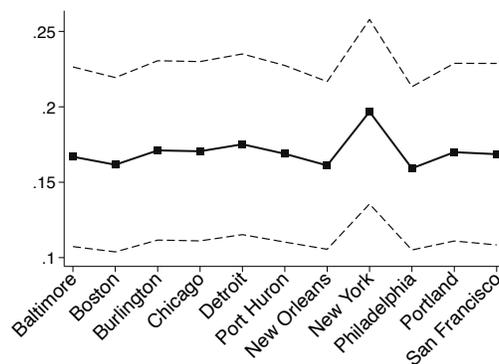
(A) TURNOVER



(B) PROFESSIONAL OCCUPATION

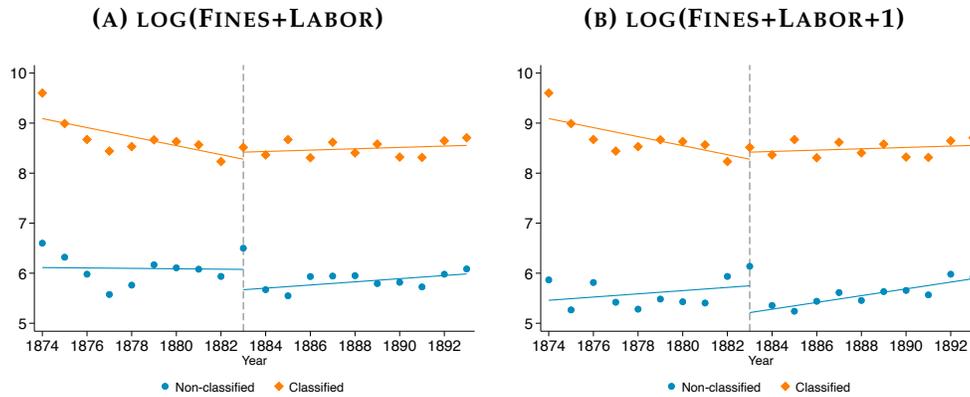


(C) BELOW CLASSIFICATION CUTOFF



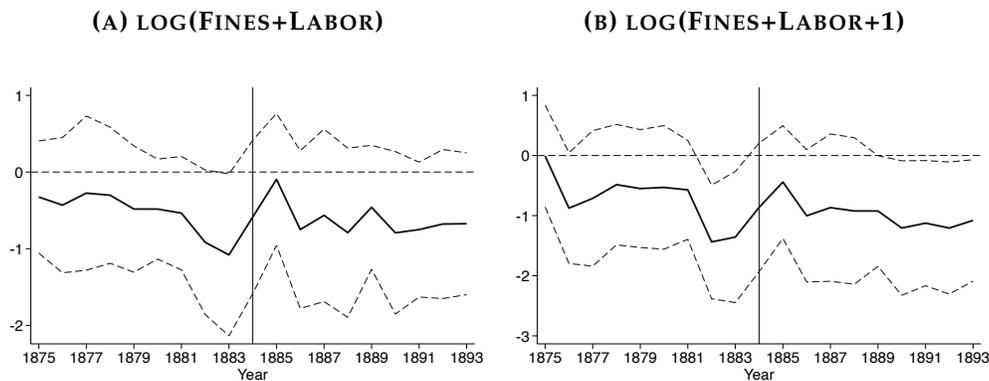
Notes: These figures show the sensitivity of the personnel results to excluding one classified district at a time. The y-axis shows our baseline difference-in-differences estimates around a 95% confidence interval when estimated excluding each of the classified districts indicated in the x-axis. The specification and outcome in panel (a) correspond to those in column 1 of Table ?? . The specification and outcome in panel (b) correspond to those in panel (a), column 7 of Table ?? . The specification and outcome in panel (c) correspond to those in column 1 of Table ?? .

FIGURE B3: AVERAGE REVENUE FROM FINES AND LABOR, 1874-1893



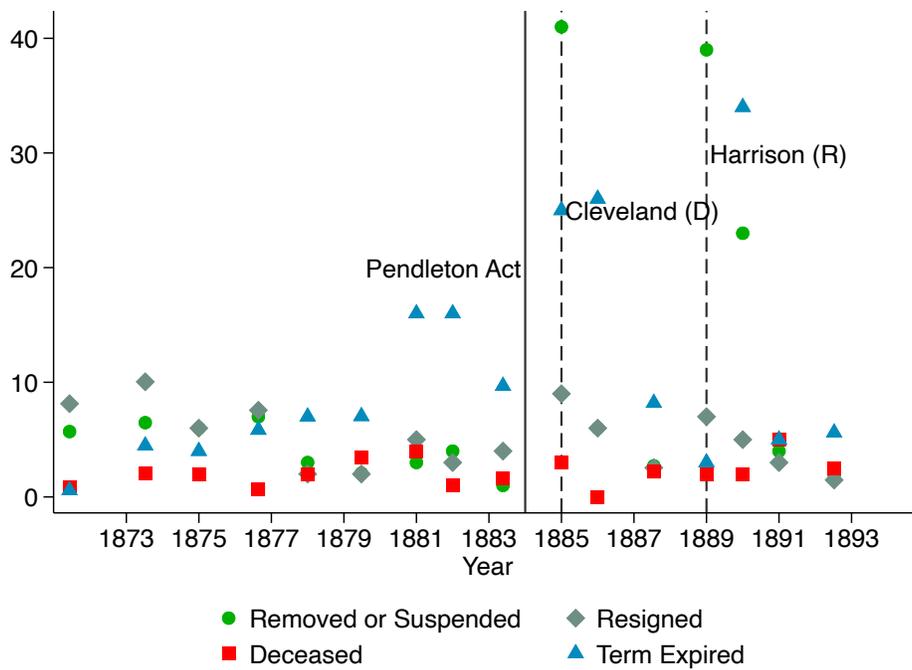
Notes: This figure uses data on the amount of revenue collected from “fines, penalties and forfeitures” and “labor, drayage and storage” from the Annual Reports of the Secretary of the Treasury (?). The figure shows yearly average log fines, separately for classified and non-classified districts from 1874 to 1893. The outcome in panel (a) is the log of fines, whereas in panel (b) it is the log of fines plus one.

FIGURE B4: EFFECTS OF REFORM ON REVENUE FROM FINES AND LABOR, EVENT-STUDY REGRESSIONS



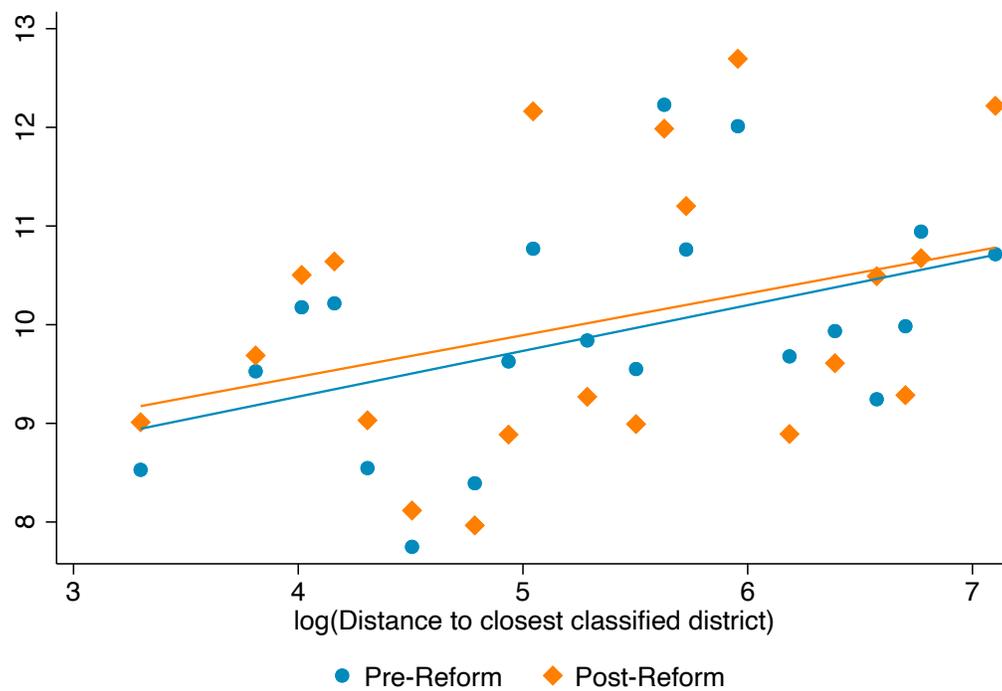
Notes: This figure uses data on the amount of revenue collected from “fines, penalties and forfeitures” and “labor, drayage and storage” from the Annual Reports of the Secretary of the Treasury (?). The figure shows event-study coefficients corresponding to estimating equation ?? in the main body of the paper. The omitted category is 1874. The outcome in panel (a) is the log of fines, whereas in panel (b) it is the log of fines plus one.

FIGURE B5: MOTIVES FOR COLLECTOR TURNOVER, 1871-1893



Notes: The y-axis shows the yearly number of nominations for the position of collector of customs, separately based on the motive for which a new collector had to be nominated. The dashed vertical lines correspond to years in which the Presidency went from a Republican to a Democrat or vice versa.

FIGURE B6: PROXIMITY TO CLASSIFIED DISTRICTS AND REVENUE



Notes: This figure shows the correlation between the distance to the nearest classified district (based on a district's port of entry) and total receipts, before and after 1883. The sample is restricted to non-classified districts.

TABLE B1: SUMMARY STATISTICS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All Non-Classified			Non-Classified (10+ emp.)			Classified		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
<i>A. District-level statistics</i>									
Total Expenses (000s)	14.66	8.55	15.16	24.47	20.02	16.72	458.58	239.60	718.29
Receipts (000s)	115.08	23.79	231.35	204.33	68.87	302.89	16172.67	2800.06	36472.89
Employees	14.10	10.00	14.96	22.53	19.00	17.99	331.21	194.00	468.05
# Observations	1520	.	.	720	.	.	220	.	.
# Districts	76	.	.	36	.	.	11	.	.
<i>B. Employee-level statistics</i>									
Turnover	0.48	0.00	0.50	0.47	0.00	0.50	0.34	0.00	0.47
Professional	0.08	0.00	0.27	0.07	0.00	0.26	0.07	0.00	0.25
Below cutoff	0.31	0.00	0.46	0.26	0.00	0.44	0.16	0.00	0.37
# Observations	12432	.	.	8525	.	.	37206	.	.

Notes: Panel (a) presents district-level summary statistics based on data from the “Annual report of the Secretary of the Treasury on the state of the finances” (?). An observation in this panel corresponds to a district-year. These data cover the 1874-1893 period and are annual. Panel (b) is based on Customs Service personnel records collected from the “Official Registers of the United States” (?). An observation in this panel corresponds to an employee-year. These data cover the 1871-1893 period and are biennial. Columns 1 to 3 show statistics corresponding to the full set of non-classified districts (i.e. those that were not required to hire through competitive exams after 1883) . Columns 4 to 6 show statistics for non-classified districts that had 10 or more employees by 1883. Columns 7 to 9 show statistics for the 11 classified districts.

TABLE B2: EFFECTS OF THE REFORM ON PERSONNEL OUTCOMES, DIFFERENCE-IN-DIFFERENCES, ROBUSTNESS TO ALTERNATIVE CONTROL GROUPS

	Turnover			Professional Occ.			Below Cutoff		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Classified X After	-0.0962*** (0.0251)	-0.116*** (0.0273)	-0.123*** (0.0286)	0.0755*** (0.0268)	0.0740** (0.0283)	0.0699** (0.0338)	0.161*** (0.0265)	0.169*** (0.0297)	0.151*** (0.0335)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45305	41680	38961	2146	2033	1890	48944	45323	42460
Clusters	109	46	30	85	45	29	109	46	30
Mean of dep. var.	0.375	0.366	0.358	0.0829	0.0821	0.0815	0.198	0.179	0.171
Comparison group	0+	10+	20+	0+	10+	20+	0+	10+	20+

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table shows the robustness of our personnel results to using alternative control groups. In columns 1, 4 and 7, the control group is comprised of all non-reformed districts (regardless of their number of employees in 1883). Columns 2, 5 and 8 correspond to our baseline sample (using districts with 10+ employees in 1883 as the control group). In columns 3, 6 and 9, the control group is restricted to districts with 20+ employees by 1883. The specification and outcome in columns 1 to 3 correspond to those in column 1 of Table ???. The specification and outcome in columns 4 to 6 correspond to those in panel (a), column 7 of Table ???. The specification and outcome in columns 7 to 9 correspond to those in column 5 of Table ???. Standard errors are clustered at the district level.

TABLE B3: PERSONNEL OUTCOMES, NO EFFECTS OF PLACEBO REFORMS

(A) 20+ EMPLOYEES			
	(1) Turnover	(2) Professional Occ.	(3) Below Cutoff
Placebo Classified X After	0.0262 (0.0378)	0.00331 (0.0564)	0.00214 (0.0468)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	7455	477	7903
Clusters	34	33	34
(B) 30+ EMPLOYEES			
	(1) Turnover	(2) Professional Occ.	(3) Below Cutoff
Placebo Classified X After	-0.0193 (0.0357)	-0.0614 (0.0555)	-0.0163 (0.0609)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	7455	477	7903
Clusters	34	33	34
(C) 40+ EMPLOYEES			
	(1) Turnover	(2) Professional Occ.	(3) Below Cutoff
Placebo Classified X After	-0.0224 (0.0277)	-0.0379 (0.0886)	-0.0351 (0.0502)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	7455	477	7903
Clusters	34	33	34

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table show the results of estimating our baseline difference-in-differences model using placebo cutoffs for the number of employees above which a district would have been classified. Panel (a) uses a placebo cutoff of 20 employees, panel (b) a cutoff of 30, and panel (c) a cutoff of 40. The specification and outcome in column 2 correspond to those in column 1 of Table ???. The specification and outcome in column 3 correspond to those in panel (a), column 7 of Table ???. The specification and outcome in column 3 correspond to those in column 5 of Table ???. The sample is restricted to employees who worked in districts with less than 50 employees (so as to exclude the actual set of “treated” districts from the sample). Standard errors are clustered at the district level.

TABLE B4: EFFECTS OF THE REFORM ON EMPLOYEES' PROFESSIONAL BACKGROUND, DIFFERENCE-IN-DIFFERENCES, ADJUSTING FOR BIRTHPLACE FIXED EFFECTS

	Professional Occ.		Unskilled Occ.	
	(1)	(2)	(3)	(4)
Classified X After	0.0740** (0.0283)	0.0738*** (0.0260)	-0.0778* (0.0429)	-0.0668 (0.0422)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Birthplace FE	No	Yes	No	Yes
Observations	2033	2033	2033	2033
Clusters	45	45	45	45
Mean of dep. var.	0.0821	0.0821	0.216	0.216

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In this table, we estimate our baseline effects of the reform on employees' professional background while adjusting for birthplace fixed effects. The specification and outcome in columns 1 to 2 correspond to those in panel (a), column 7 of Table ???. The specification and outcome in columns 5 to 8 correspond to those in panel (b), column 7 of Table ???. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B5: EFFECTS OF THE REFORM ON PERSONNEL OUTCOMES, DIFFERENCE-IN-DIFFERENCES, ADDITIONAL CONTROL VARIABLES

	Turnover			Professional Occ.			Below Cutoff		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Classified X After	-0.116*** (0.0273)	-0.110** (0.0479)	-0.107** (0.0497)	0.0740** (0.0283)	0.0814** (0.0316)	0.0799** (0.0314)	0.169*** (0.0297)	0.201*** (0.0366)	0.205*** (0.0329)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region X Year FE	No	No	Yes	No	No	Yes	No	No	Yes
1883 Employees X Year	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	41680	41680	41680	2033	2033	2033	45323	45323	45323
Clusters	46	46	46	45	45	45	46	46	46
Mean of dep. var.	0.366	0.366	0.366	0.0821	0.0821	0.0821	0.179	0.179	0.179

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table shows the robustness of our personnel results to controlling for interactions between a district number of employees in 1883 and year dummies. The specification and outcome in columns 1 to 3 correspond to those in column 1 of Table ???. The specification and outcome in columns 4 to 6 correspond to those in panel (a), column 7 of Table ???. The specification and outcome in columns 7 to 9 correspond to those in column 5 of Table ???. The sample is restricted to districts with at least 10 employees by 1883. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B6: EFFECTS OF THE REFORM ON PERSONNEL OUTCOMES, DIFFERENCE-IN-DIFFERENCES, ESTIMATED AT THE DISTRICT LEVEL

	Turnover		Professional Occ.		Below Cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Classified X After	-0.116*** (0.0273)	-0.0749* (0.0411)	0.0740** (0.0283)	0.0537 (0.0379)	0.169*** (0.0297)	0.143*** (0.0392)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41680	506	2033	340	45323	552
Clusters	46	46	45	45	46	46
Mean of dep. var.	0.366	0.447	0.0821	0.0774	0.179	0.271
Unit of analysis	Individual	District	Individual	District	Individual	District

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In this table, we aggregate the employee-level data at the district-year level. The specification and outcome in columns 1 and 2 correspond to those in column 1 of Table ???. The specification and outcome in columns 3 and 4 correspond to those in panel (a), column 7 of Table ???. The specification and outcome in columns 5 and 6 correspond to those in column 5 of Table ???. The sample is restricted to districts with at least 10 employees by 1883. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B7: EFFECTS OF THE REFORM ON PERSONNEL OUTCOMES, DIFFERENCE-IN-DIFFERENCES, INCLUDING PORTS OF DELIVERY IN CLASSIFIED DISTRICTS IN THE TREATMENT GROUP

	Turnover		Professional Occ.		Below Cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Classified X After	-0.116*** (0.0273)	-0.117*** (0.0274)	0.0740** (0.0283)	0.0658** (0.0304)	0.185*** (0.0296)	0.170*** (0.0296)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41680	41680	2033	2033	45323	45323
Clusters	46	46	45	45	46	46
Mean of dep. var.	0.366	0.366	0.0821	0.0821	0.179	0.179
Ports of delivery included in treatment group	No	Yes	No	Yes	No	Yes

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In this table, "ports of delivery" within classified districts are included in the treatment group. The specification and outcome in columns 1 and 2 correspond to those in column 1 of Table ???. The specification and outcome in columns 3 and 4 correspond to those in panel (a), column 7 of Table ???. The specification and outcome in columns 5 and 6 correspond to those in column 5 of Table ???. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B8: EFFECTS OF THE REFORM ON PERSONNEL OUTCOMES, DIFFERENCE-IN-DIFFERENCES, EXCLUDING PORTS OF DELIVERY

	Turnover		Professional Occ.		Below Cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Classified X After	-0.116*** (0.0273)	-0.0952*** (0.0315)	0.0740** (0.0283)	0.0755** (0.0299)	0.169*** (0.0297)	0.171*** (0.0308)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41680	40926	2033	1985	45323	44507
Clusters	46	43	45	42	46	43
Mean of dep. var.	0.366	0.365	0.311	0.0826	0.179	0.179
Ports of delivery included in sample	Yes	No	Yes	No	Yes	No

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In this table, we exclude from the sample those observations that correspond to employees who worked in “ports of delivery”. The specification and outcome in columns 1 and 2 correspond to those in column 1 of Table ???. The specification and outcome in columns 3 and 4 correspond to those in panel (a), column 7 of Table ???. The specification and outcome in columns 5 and 6 correspond to those in column 5 of Table ???. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B9: EFFECTS OF THE REFORM ON RECEIPTS AND EXPENSES, DIFFERENCE-IN-DIFFERENCES, EXCLUDING FIRST FIVE POST-REFORM YEARS

	log(Expenses)		log(Receipts)		log(Receipts/Expenses)	
	(1)	(2)	(3)	(4)	(5)	(6)
Classified X After	-0.0150 (0.111)	-0.115 (0.0866)	0.0232 (0.244)	-0.186 (0.229)	0.0382 (0.171)	-0.0713 (0.173)
Customhouse FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region X After FE	No	Yes	No	Yes	No	Yes
Observations	705	705	705	705	705	705
Clusters	47	47	47	47	47	47

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In this table, we exclude the first five post-reform years from the sample. The dependent variable in columns 1 and 2 is the log of total expenses, in columns 3 and 4 it is the log of total receipts, and in column 5 and 6 it is the natural log of the ratio between the total receipts and expenses. *Classified* \times *After* takes a value of one for districts that were part of the classified Customs Service after 1883. All columns include year and district fixed effects. Even columns also include *Region* \times *After* fixed effects. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B10: EFFECTS OF THE REFORM ON RECEIPTS AND EXPENSES, DIFFERENCE-IN-DIFFERENCES, BASIC DIFF-IN-DIFF SPECIFICATION

	log(Expenses)		log(Receipts)		log(Receipts/Expenses)	
	(1)	(2)	(3)	(4)	(5)	(6)
Classified X After	-0.0108 (0.0792)	-0.0827 (0.0596)	0.0251 (0.183)	-0.137 (0.172)	0.0359 (0.135)	-0.0541 (0.137)
Classified indicator	Yes	Yes	Yes	Yes	Yes	Yes
After indicator	Yes	Yes	Yes	Yes	Yes	Yes
Region X After FE	No	Yes	No	Yes	No	Yes
Observations	940	940	940	940	940	940
Clusters	47	47	47	47	47	47

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. The dependent variable in columns 1 and 2 is the log of total expenses, in columns 3 and 4 it is the log of total receipts, and in column 5 and 6 it is the log of the ratio between total receipts and expenses. *Classified* \times *After* takes a value of one for districts that were part of the classified Customs Service after 1883. Odd columns include year and a “Classified” indicator. Even columns also include *Region* \times *After* fixed effects. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B11: ASSOCIATION BETWEEN THE SHARE OF WORKERS WITH A PROFESSIONAL BACKGROUND AND DISTRICTS’ FINANCIAL OUTCOMES

	log(Expenses)	log(Receipts)	log(Receipts/Expenses)
	(1)	(2)	(3)
Share with Professional Occ.	-0.147 (0.115)	0.474** (0.217)	0.621*** (0.217)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	383	383	383
Clusters	88	88	88

Notes: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. An observation corresponds to a district-year. The dependent variable in column 1 is the log of expenses in district c in year t . The dependent variable in column 2 is the log receipts. The dependent variable in column 3 is the log of receipts over expenses. The sample is restricted to the pre-reform period. Standard errors are clustered at the district level.

TABLE B12: EFFECTS OF THE REFORM ON THE (LOG) NUMBER OF EMPLOYEES, DIFFERENCE-IN-DIFFERENCES

	Total		Non-Exempted		Exempted	
	(1)	(2)	(3)	(4)	(5)	(6)
Classified X After	0.0932 (0.0889)	0.0533 (0.0761)	-0.195 (0.125)	-0.285*** (0.0907)	0.619*** (0.142)	0.643*** (0.146)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region X After FE	No	Yes	No	Yes	No	Yes
Observations	552	552	552	552	552	552
Clusters	46	46	46	46	46	46

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. An observation corresponds to a district-year. The dependent variable in columns 1 and 2 is the log number of employees in district c in year t . The dependent variable in column 3 and 4 is the log number of employees in non-exempted positions and in columns 5 and 6 is the log number of employees in exempted positions. *Classified* \times *After* takes a value of one for districts that were made part of the classified system after 1883. All columns include year and district fixed effects. Even columns also include *Region* \times *After* fixed effects. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B13: IN THE PRE-REFORM PERIOD, EMPLOYEES PAID BELOW THE EXAM CUTOFF HAD WEAKER PROFESSIONAL BACKGROUNDS

	Professional Occ.	Unskilled	Literate
	(1)	(2)	(3)
Below Exam Cutoff	-0.0131 (0.0158)	0.102*** (0.0293)	-0.0284 (0.0202)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	1525	1525	1525
Clusters	46	46	46
Mean of dep. var.	0.0702	0.237	0.916

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table uses the data linking the Official Registers to earlier population censuses. An observation corresponds to an employee-year. "Below Exam Cutoff" is an indicator that takes a value of one if an employee made less than \$900 a year. The sample is restricted to employees in the *pre-reform* period and to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B14: ACCOUNTING FOR COMPOSITIONAL CHANGES

	Turnover		Professional Occ.	
	(1)	(2)	(3)	(4)
Classified X After	-0.116*** (0.0273)	-0.142*** (0.0269)	-0.00434 (0.0196)	0.0102 (0.0207)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Position FE	No	Yes	No	Yes
Observations	41680	41680	8614	8614
Clusters	46	46	46	46
p-value		0.0447		0.0991

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table shows how the estimated effects of the reform on employee turnover and the likelihood that an employee would have held a professional occupation change as we include position fixed effects to the regression. *Classified* \times *After* takes a value of one for districts that were made part of the classified system after 1883. All columns include year and district fixed effects. The odd columns further include position fixed effects. A position is defined as a combination between an occupation (for instance, “clerk”) and a compensation. Adding position fixed effects shuts down the effects of the reform that stem from position-based compositional changes in a district’s workforce. The last row of the table shows the p-value corresponding to the null hypothesis that the estimate from the model using position fixed effects is equal to the baseline model for each of the outcomes (that is, comparing the coefficients in column 1 to the coefficient in column 2 and the coefficient in column 3 to the coefficient in column 4). The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B15: MOTIVES FOR COLLECTOR TURNOVER, 1871-1893

	(1) Number	(2) %
Deceased	47	8.4
Removed or Suspended	183	32.6
Resigned	116	20.7
Term Expired	215	38.3
Total	561	100

Notes: This table shows the motives why a new collector had to be nominated based on data from the “Journals of Executive Proceedings of the Senate” (?).

TABLE B16: COLLECTORS TRANSITION AND (LAGGED) DISTRICT PERFORMANCE

	Transition					
	(1)	(2)	(3)	(4)	(5)	(6)
log(Expenses) at t-1	0.0148 (0.0319)	0.00594 (0.0328)				
log(Receipts) at t-1			-0.00237 (0.0196)	-0.0112 (0.0209)		
log(Receipts/Expenses) at t-1					-0.00878 (0.0198)	-0.0141 (0.0205)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region X Time FE	No	Yes	No	Yes	No	Yes
Observations	935	935	935	935	935	935

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table shows the results of estimating specification ???. The outcome variable *Transition* measures whether there is a new collector at year t . The independent variables are measures of district financial performance assessed at time $t - 1$. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B17: COLLECTOR FIXED EFFECTS, PRE-AND-POST REFORM PERIODS

(A) PRE-REFORM SAMPLE (1874 - 1882)			
	(1)	(2)	(3)
	F-stat	p-value	Observations
log(Expenses)	2.295	0.000	283
log(Receipts)	2.160	0.000	283
log(Receipts/Expenses)	2.836	0.000	283
(B) POST-REFORM SAMPLE (1883 - 1893)			
	(1)	(2)	(3)
	F-stat	p-value	Observations
log(Expenses)	3.472	0.000	382
log(Receipts)	4.626	0.000	382
log(Receipts/Expenses)	3.494	0.000	382

Notes: We estimate equation ?? for the pre-reform period (1874-1882, panel (a)) and the post-reform period (1883-1893, panel(b)). We use an empirical Bayesian shrinkage correction to estimate the collector fixed effects and present results of a F-test, testing the null hypotheses that (corrected) collector fixed effects are jointly equal to zero. The sample is restricted to district-years in which there is only one collector—so as to be able to associate a performance metric to a single collector. Therefore, it excludes district-years where multiple collectors were in charge of the district for different months of the year (295 out of a total of 960 district years).

TABLE B18: COLLECTOR’S DEATH WALD TEST, 1874-1893

	(1)	(2)	(3)
	J	p-value	$N_z J$
log(Expenses)	1.965	0.005	41.266
log(Receipts)	1.642	0.032	34.474
log(Receipts/Expenses)	1.602	0.039	33.650

Notes: The table presents the Wald test estimate defined in equation ??, testing whether there is excess variability in districts’ financial outcomes around the collector’s death. In contrast to Table ??, where we use the 1874-1906 time period, in this table we use a similar time horizon to the one we use in the remaining sections of the paper (1874-1893). This shorter time horizon includes 21 deaths of collectors while in office.

TABLE B19: COLLECTORS AND DISTRICT PERFORMANCE, NON-PARAMETRIC TEST BASED ON COLLECTORS' DEATHS

(A) 1974-1906	
Outcome	p-value
log(Expenses)	0.15
log(Receipts)	0.62
log(Receipts/Expenses)	0.88
(B) 1974-1893	
Outcome	p-value
log(Expenses)	0.63
log(Receipts)	0.69
log(Receipts/Expenses)	0.72

Notes: We present a non-parametric test of excess variability in districts' financial outcomes around the collector's death. This test is an alternative to the Wald test reported in Table ???. Both this test and the Wald test are used in ?. Panel (a) uses a longer time period, 1874-1906, including 33 deaths of collectors. Panel (b) uses a shorter time period, 1874-1893 including 21 deaths of collectors.

TABLE B20: EFFECTS OF THE REFORM ON REVENUE FROM FINES AND LABOR, DIFFERENCE-IN-DIFFERENCES

	log(Fines+Labor)		log(Fines+Labor+1)	
	(1)	(2)	(3)	(4)
Classified X After	-0.00539 (0.256)	-0.129 (0.199)	-0.173 (0.262)	-0.311 (0.211)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region X After FE	No	Yes	No	Yes
Observations	895	895	940	940
Clusters	47	47	47	47

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table uses data on the amount of revenue collected from "fines, penalties and forfeitures" and "labor, drayage and storage" from the Annual Reports of the Secretary of the Treasury (?). An observation corresponds to a district-year. *Classified* \times *After* takes a value of one for districts that were part of the classified Customs Service after 1883. All columns include year and district fixed effects. Even columns also include *Region* \times *After* fixed effects. The sample is restricted to districts with at least 10 employees by 1883. Standard errors are clustered at the district level.

TABLE B21: SPILLOVERS TO NON-CLASSIFIED DISTRICTS, RECEIPTS

	(1)	(2)	(3)	(4)
Distance Closest Classified X After	-0.0409 (0.126)	-0.0507 (0.137)	0.152 (0.143)	-0.00159 (0.215)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1500	1340	720	420
Clusters	75	67	36	21
Sample	0+	5+ employees	10+ employees	20+ employees

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. An observation corresponds to a district-year. This table shows the correlation between distance to the closest classified district and a district's total receipts. The sample is restricted to non-classified districts. The last row indicates the minimum number of employees in 1883 above which a district is included in the sample. Standard errors are clustered at the district level.

TABLE B22: SPILLOVERS TO NON-CLASSIFIED DISTRICTS, PERSONNEL OUTCOMES

	(1) Turnover	(2) Professional Occ.	(3) Below Cutoff
Distance to Closest Classified X After	-0.0155 (0.0183)	-0.00803 (0.0232)	0.0327 (0.0223)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	7633	495	8108
Clusters	34	33	34
Mean of dep. var.	0.470	0.0795	0.267

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. An observation corresponds to an employee-year. This table shows the correlation between distance to the closest classified district and district's personnel outcomes. The sample is restricted to non-classified districts.

TABLE B23: SPILLOVERS TO NON-CLASSIFIED DISTRICTS, EXCLUDING NON-CLASSIFIED DISTRICTS IN CLOSE PROXIMITY TO A CLASSIFIED DISTRICT, RECEIPTS

	(1)	(2)	(3)	(4)
Classified X After	0.0251 (0.185)	0.00700 (0.194)	-0.00458 (0.206)	0.0281 (0.208)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	940	880	820	700
Clusters	47	44	41	35
Comparison group	All	50+ miles	100+ miles	200+ miles

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table shows the estimated effects of the reform on total receipts when we restrict the control group to districts whose port of entry was at least 50, 100 or 200 miles away from the closest classified district's port of entry (as indicated by the last row of the table). All columns include year and district fixed effects. Standard errors are clustered at the district level.

TABLE B24: EFFECTS OF THE REFORM ON RECEIPTS AND EXPENSES OVER THE LONGER-TERM

	1874-1893		1874-1906	
	(1)	(2)	(3)	(4)
Classified X After	0.0359 (0.137)	-0.0541 (0.138)	0.00323 (0.162)	-0.0709 (0.169)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Region X After FE	No	Yes	No	Yes
Observations	940	940	1551	1551
Clusters	47	47	47	47

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the log of the ratio between total receipts and expenses. *Classified* \times *After* takes a value of one for districts that were part of the classified Customs Service starting in 1883. All columns include year and district fixed effects. Even columns also include *Region* \times *After* fixed effects. Columns (1) and (2) report the results for the dependent variable in the first 10 years of the reform (up to 1893), while columns (3) and (4) report the results for the dependent variable in the first 22 years (up to 1906). The sample is restricted to districts with at least 10 employees by 1883. Standard errors clustered at the district level.

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