Civil Service Reforms: Evidence from U.S. Police Departments

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Abstract

Merit systems reducing politicians' control over police officers' hiring and firing have been in effect in the United States beginning in the early 1900s. But did they succeed in improving police performance? To answer this question, I exploit population-based mandates for police department merit systems in a regression discontinuity design. Merit systems improved performance: in the first ten years after the reform, the property crime rate was lower and the violent crime clearance rate was higher in departments operating under a merit system than in departments operating under a spoils system. I explore three possible channels: resources, police officers' characteristics and police officers' incentive structure. Changes in resources or police officers' characteristics do not drive the effect: employment and expenditures were not affected and there is limited evidence of selection changing pre-1940. I provide indirect evidence that changes in the incentive structure faced by police officers are instead important: merit systems had no effect on performance when the ban on patronage dismissals, the component of the reform that most directly affects incentives, was not part of the treatment.

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1 Introduction

Bureaucracies are a key component of state capacity. As policy implementers, they translate policy choice into outcomes and affect a state's ability to provide public goods. We know both from expert surveys (e.g. La Porta et al., 1999; Hyden, Court, and Mease, 2003; Kaufmann, Kraay, and Zoido, 1999) and direct experiments (e.g. Chong et al., 2014) that there is a high degree of cross-country variation in bureaucratic performance. Why are some bureaucracies effective while others fail?

Whether politicians control public employees' hiring and firing has been identified as the first order determinant of bureaucratic effectiveness by a long tradition in the social sciences. There is no consensus, however, on whether politicians' control over these decisions improves performance. Historically, the entire American public administration was characterized by a spoils system in which politicians were free to hire and fire bureaucrats as they saw fit. In 1829, President Andrew Jackson justified the system on grounds of increased responsiveness: "The duties of all public officers are [...] so plain and simple that men of intelligence may readily qualify themselves for their performance. [...] More is lost by the long continuance of men in office that is generally to be gained by their experience" (as quoted in White, 1954, p. 347).

By the end of the 19th century, the opposite view that merit systems insulating bureaucrats from politics were necessary to give public employees long term incentives and foster expertise had become more prominent. Reforms professionalizing the bureaucracy were first introduced at the federal level in the 1880s and soon started diffusing at lower levels of government. Nevertheless, the debate on whether politicians' control improved performance was by no means closed. When the U.S. Supreme Court was called upon to discuss whether patronage dismissals violated the First Amendment in 1980, the decision of the court was in support of merit systems, but the dissenting opinion of Justice Stewart endorsed the spoils system again on grounds of increased responsiveness: "Patronage serves the public interest by facilitating the implementing of policies endorsed by the electorate."

Whether merit systems improve performance depends on the trade-off between expertise and responsiveness and is ultimately an empirical question. Evaluating the trade-off has, however, proven to be a difficult task. When bureaucratic organizations are defined at the country level, their effect is confounded by other country-specific factors. When within country variation exists, endogenous adoption makes the identification of causal effects challenging. In addition, finding direct measures of bureaucratic performance is not straightforward. The principal contribution of this paper is to provide well-identified causal evidence on the effect of bureaucracy professionalization on a credible set of performance measures.

The setting is that of municipal police departments in the United States. In particular, I contrast the performance of police departments operating under a spoils system with that of departments in which a merit system was exogenously introduced. Under a spoils system, politicians were free to hire and fire as they saw fit. Under a merit system, the authority to appoint, promote and dismiss police officers was taken from the mayor and given to a semi-independent civil service commission. Hiring and promotion decisions had to follow merit-based criteria and dismissals were only permitted for just cause.

The first cities to establish merit systems, Albany, Utica and Yonkers (NY), did so in 1884, just a year after the Pendleton Act had introduced meritocratic hiring for part of the federal bureaucracy.[1] However, it took a long time for the reform to diffuse at the local level, especially as far as smaller municipalities were concerned. As late as in the mid-1970s, only 20% of police departments in cities with fewer than 10,000 inhabitants had in place a merit system for hiring their police officers.[2]

There is a high degree of variation in how merit systems were introduced at the local level. This paper focuses on states with population-based mandates for police department merit systems. The mandates operated in the following way. When the state legislation was first passed, all municipalities with population above the threshold in the latest available census were mandated to introduce a merit system. At the following census, previously untreated municipalities that had grown above the lower limit also became subject to the mandate and were required to introduce a merit system for their police department. Municipalities below the threshold were allowed to introduce a merit system at any time.

Whenever a population census was taken, treatment was assigned to all previously untreated municipalities above the cutoff. Each census defines a separate experiment in which the effect of the mandate can be estimated using a standard cross-sectional RD design comparing municipalities just above and just below the threshold. The baseline specification estimates the average treatment effect pooling all experiments. For the causal effect of the mandate to be identified, municipalities just above the threshold must be comparable to municipalities just below. I validate the assumption by showing that the density of the running variable is smooth at the discontinuity and that municipality characteristics are balanced at baseline.

Using pre-1940 data, I find that being above the threshold increased the probability of having a civil service board by 43%. The effect is large, but smaller than one, both because of municipalities below the cutoff introducing a board and because of municipalities above the cutoff facing delays. However, the protections granted by the mandate were enforceable in court from the moment in which the official census counts were published and partial treatment was in place even before the creation of a civil service commission. The measure understates the extent to which police departments were covered by merit system provisions.

My main objective is to study how the introduction of merit systems affected the performance of police departments. I proxy for police performance using crime rates (crime per 100,000 people) and clearance rates (crimes cleared by arrest over total crimes). The data are from the Uniform Crime Reports (UCRs) published by the Federal Bureau of Investigation. UCRs are available at

[1] Merit systems covered all employees in the largest cities but were restricted to members of police departments in the vast majority of municipalities.

[2] Author's calculations based on data from Ostrom, Parks, and Whitaker (1977).

the individual department level only starting from 1960. As there exists no data on merit system adoption for this period, the main analysis estimates intention to treat effects. At the end of the 1970s two U.S. Supreme Court decisions, Eldor v. Burns (1976) and Branti v. Finkel (1980), extended protection from political dismissals to all public employees regardless of municipality size, thus substantially altering the content of the reform. The main analysis ends in 1980 but I look at the later period to explore the role of patronage dismissals in explaining the results.

My evidence indicates that merit systems improved police performance. In the first ten years after a municipality became subject to the mandate, the property crime rate was 46% lower and the violent crime clearance rate was 12% higher in municipalities just above the threshold relative to municipalities just below. The results are not explained by pre-existing differences: there is no discontinuity in the outcomes before the introduction of merit systems. Studying the effect as a function of years since treatment shows that it took two to three years for merit systems to first affect the property crime rate but that, after the first adjustment period, the effect was constant.

I test whether the results depend on the choice of sample, specification and estimation technique. The effect of merit systems on the property crime rate is not driven by any of the choices made in the estimation. The effect of merit systems on the violent crime clearance rate, however, is less robust. In addition, I argue that it is improbable that the results are driven by other state-specific policies changing at the same threshold. Finally, the greatest challenge for the interpretation of the results as improved police performance is the concern that only crime statistics, and not actual crimes, changed at the threshold. I discuss in detail in the paper why my results are unlikely to be an artifact of differential reporting.

The effect of merit systems on performance may be explained by changes in the resources available to the police department, by changes in police officers' characteristics or by changes in the incentive structure that police officers face on the job. First, merit systems may influence the amount of resources available. I find no effect on expenditures or employment at the discontinuity, which suggests that departments operating under a merit system used similar inputs as departments operating under a spoils system.

Second, police departments under merit systems may select and retain officers with different characteristics. I study the demographic composition of the departments using a novel dataset with individual-level information on police officers that I construct from the full count microdata from the population censuses 1910 to 1940. I use this dataset to look at ethnicity, ethnic patronage and human capital.[3]

I find scant evidence that the ethnic composition of police departments changed in municipalities covered by the mandates. I test whether ethnic patronage was affected by looking at the fraction of police officers who were of the same ethnicity as the mayor or from the dominant ethnic group. To define the first proxy, I collected names and years of service of these municipalities' mayors

[3]The occupational question for the 1900 census is still being digitized. I have applied and am waiting for access to the modern censuses from 1950-2000 that are not publicly available.

and assigned them an ethnicity by linking them in the census. Ethnic patronage was not different in municipalities just above and just below the threshold. Finally, I turn to the human capital of police officers. I find lower educational attainment under a merit system, although the effect is driven by cities just below the discontinuity having a particularly high realization of the outcome.

The effect of merit systems on performance cannot be explained by changes in resources or police officers' characteristics. This suggests that the remaining channel, changes in the incentive structure faced by police officers, is likely important. At the end of the 1970s, the U.S. Supreme Court issued two decisions that made patronage dismissals, the component of the reform that most directly affects incentives, unavailable both in places with and without a merit system. Studying the effect of the mandates after 1980 therefore provides indirect evidence on the role played by the patronage dismissals' ban in explaining the effect on police performance. Merit system mandates have no effect on crime or clearance rates after 1980, which is consistent with the hypothesis that police officers' incentive are important to explain the main results, in particular to the extent that they are susceptible to pressure from politicians.

My finding that merit systems have a positive effect on performance is consistent both with cross-country comparisons (e.g. Evans and Rauch, 1999; Rauch and Evans, 2000) and with papers focusing on specific aspects of the reform (e.g. Akhtari, Moreira, and Trucco, 2016; Iyer and Mani, 2012; Rasul and Rogger, 2016). The closest contribution is that of Rauch (1995), who studied the effect of U.S. municipal merit systems on infrastructure investment and growth using a differences-in-differences design. What differentiates my study from the existing literature is that the novel setting allows me to both improve on identification and examine a direct measure of performance. In addition, the paper provides complementary evidence to existing work on the effect of U.S. federal and state merit systems on outcomes other than performance (e.g. Folke, Hirano, and Snyder, 2011; Johnson and Libecap, 1994; Ujhelyi, 2014). Finally, the paper relates to studies looking at determinants of police performance by providing evidence on the role played by police organization (e.g. Chalfin and McCrary, Forthcoming; Evans and Owens, 2007; Levitt, 1997; Mas, 2006).

The remainder of the paper is organized as follows. Section 2 presents the background, section 3 presents the data, and section 4 discusses the empirical strategy. The main results are presented in section 5 and potential mechanisms are presented in section 6. Section 7 concludes. Additional tables and details are available in an online appendix.[4]

[4]The online appendix is available at the following link.

2 Background

Historical background

The Wickersham Commission reports, published in 1931, offer a dismal picture of the state of American policing at the beginning of the 20th century.[5] Police departments across the nation were described as tainted by corruption and misconduct and as incapable of controlling crime. The main culprit was identified to be excessive political influence in policing, which made the tenure of executive chiefs and officers alike too short and the selection of personnel with adequate qualifications impossible. As J. Edgar Hoover (1938) wrote just a few years later: "the real "Public Enemy Number One" against law and order is corrupt politics." The solution proposed was police professionalization through the development of effective merit systems.[6]

Police forces were just one of many public organizations not operating under a professionalized model. Jefferson was the first president during whose term "party service was recognized as a reason for appointment to office, and party dissent as a cause for removal" (Fish, 1905, p. 51). It is Jackson, however, who is credited with introducing a full-fledged spoils system under which newly elected presidents could substitute office holders nominated in previous administrations for party loyals (Freedman, 1994). At the height of the spoils system (1845-1865), wholesale dismissal and replacement of federal employees was the norm (United States Civil Service Commission, 1973) but even during the first Cleveland administration (1885-1889) more than 43,000 fourth-class postmasters were removed and substituted (Fowler, 1943).

By the mid 19th century, however, the discussion on whether the spoils system was the best way to organize the bureaucracy had begun. The proponents of professionalization saw it as a response to widespread inefficiencies; those opposing reform were afraid of losing not only political power, but also the support of an aligned bureaucracy. The assassination of President Garfield in 1881 by a disappointed office seeker, Charles Guiteau, precipitated change: the first civil service reform aimed at professionalizing public employees, the Pendleton Act, was adopted in 1883. The act created a bipartisan Civil Service Commission under the control of the President and introduced meritocratic hiring for around 10% of federal employees, mostly those working in large post offices and custom houses. Job tenure and protection from partisan dismissals (the other two defining characteristics of a merit system) were established a few years later at the end of the 1890s (Lewis, 2010).

The Pendleton Act allowed presidents to extend the merit-based system to other categories of workers. Expansion was swift: by 1920, only 20% of all federal employees were still under a spoils system. At the state and at the local level, a first wave of reforms coincided with the passage of the

[5] The National Commission on Law Observance and Enforcement, also known as the Wickersham Commission after its chairman former attorney general George W. Wickersham, was created by President Hoover in 1929 with the objective of studying the state of crime and policing and identifying possible solutions.

[6] By the 1930s, early merit systems had been established in the largest US cities, but were found inadequate.

Pendleton Act. New York and Massachusetts were the first two states to adopt a merit system in 1883 and 1885, and Albany, Utica and Yonkers (NY) were the first cities in 1884. Adoption picked up again during the Progressive Era, when reformers identified professionalization as the remedy for the inefficiency of city hall. The diffusion of the reform was slower than at the federal level and by 1920 fewer than 40% of cities with more than 25,000 inhabitants had a merit system.

Police departments were central to the debate for municipal civil service reform. Originally an offshoot of the Progressive movement (Fogelson, 1977, p. 44), the professionalization of the police force was at the center of police reform long after the original impetus has subsided. In 1954, O. W. Wilson was still supporting the ideal: "sound personnel management operates on the merit principle that to the best-qualified goes the job - not to the victor belongs the spoils."

Merit system mandates

There was wide variation in the legislative basis of municipal merit systems. In the majority of the cases, the reform was adopted independently by municipalities through ordinance or referendum.[7] This makes studying the effect of merit systems challenging: because introducing the reform was a political decision taken by those who had to gain (or lose) from it, the timing was likely endogenous. In some cases, however, merit systems were introduced by state legislatures. In particular, the paper focuses on states with population-based mandates for merit systems for police departments.

I collected information on state legislation related to police merit systems from a combination of primary and secondary sources (Appendix D reports in detail how the information was collected). As Table 1 shows, I identify eight states with mandates based on population thresholds. Because Wisconsin had two different cutoffs based on whether a municipality was incorporated as a village or as a city, I consider Wisconsin villages and Wisconsin cities separately. When the legislation explicitly excluded municipalities under specific forms of government (for example, municipalities organized under a commission form of government), I omit them from the analysis.[8]

Whereas there were differences in the details of the legislation across states, the fundamental features of the reform were the same. When a merit system was introduced for a police department, the authority over hiring, promotions and dismissals was removed from the mayor and given to a semi-independent civil service commission. Hiring and promotion decisions, not regulated under a spoils system, had to be based on merit following competitive examinations.[9] Police officers, who could be dismissed by the mayor at will under a spoils system, could only be fired for just cause and had access to a formal grievance procedure administered by the commission.

[7] Since 1939, the federal government has at times included a merit system requirement for employees receiving certain federal grants-in-aid. However, most of the programs were geared towards state and county governments and not municipalities (Aronson, 1974).

[8] In Wisconsin, the mandate does not apply to cities under a city manager form of government before 1933.

[9] I discuss the history of police officers examinations in the mechanisms sections and in particular in footnote 28.

Civil service commissions were usually nominated by the mayor or by the governing body of the city. They were composed of three to six members with overlapping terms. In five out of nine cases (Arizona, Illinois, West Virginia, Wisconsin cities and Wisconsin villages), the commission was bipartisan and in two additional states (Iowa and Louisiana) members were required to be non-political. In Montana and Nebraska, members were only required to be citizens of good standing supporting the merit system principle for public administration. The provisions covered all police officers of lower ranks, but were sometimes extended to the police chief.[10]

When the state legislation was first passed, all municipalities above the population threshold according to the latest available census had to introduce a merit system for their police department. In all subsequent censuses, municipalities that had grown above the cutoff also became subject to the mandate and had to introduce a merit system. Only a few states had penalties in case a municipality failed to comply fully with the mandate, but the protections given to police officers became binding in the moment in which the official counts from the census were released and could be challenged in court. Municipalities below the threshold were allowed to introduce a merit system through ordinance or referendum at any time.

The years of introduction of the reform at the state level cover a wide span, with Montana (1907) being the earliest adopter and Arizona (1969) the latest. At the end of the 1970s, two U.S. Supreme Court decisions, Elrod v. Burns (1976) and Branti v. Finkel (1980), made patronage dismissals illegal for all municipal employees on grounds of violation of the First Amendment. This substantially alters the treatment by limiting political influence even in municipalities not under a merit system. The main analysis focuses on the period before 1980, but I use later census experiments to investigate potential mechanisms.

The thresholds are between 4,000 and 15,000: the legislation focuses on police departments of small municipalities. To think about the effect of merit systems in this context, it is helpful to have some information on how police departments in small municipalities operated. A survey conducted by Elinor Ostrom in 1974 (Ostrom, Parks, and Whitaker, 1977) is one of the few data sources reporting information on small police departments for the relevant period.[11] The survey provides information on all police departments in a random sample of standard metropolitan areas. Departments in municipalities of fewer than 10,000 people employed on average six full-time sworn officers and one civilian. Out of the six full-time officers, four had grade of patrolman. This highlights that career concerns may be less relevant in this case than in other, bigger, organizations. Finally, the principal police functions that the departments engaged in internally were patrolling, traffic control and criminal investigation.

[10] In Arizona, Louisiana and West Virginia the police chief was not under a merit system. In Illinois, the commission nominated the chief by default, but the provision could be changed by ordinance. In Iowa the chief did not receive protections but could be nominated only from an eligibility list.

[11] For example, the first Law Enforcement Management and Administrative Statistics Survey was published in 1987.

3 Data

To study the effect of merit systems on the performance of police departments and explore potential mechanisms, I combine data from four different sources.

Reform adoption. I predict the year in which a municipality became subject to the mandate using population counts digitized from the official publications of the Census Bureau. Information on actual reform adoption is available from three surveys conducted by the Civil Service Assembly of the United States in 1938, 1940 and 1943.[12] The surveys were collected as part of an effort to track the development of merit systems by contacting a wide range of organizations and experts in the field. They report the year of introduction of the civil service commission, the structure of the commission itself and what departments it covered. There may be civil service agencies not reported in the census but they are likely to be the exception and not the rule, as the data collection process seems to have been fairly comprehensive. No information on reform adoption is available for more recent years.

Crime. The crime data are from the Uniform Crime Reports (UCRs) published by the Federal Bureau of Investigation. UCRs are compiled from returns voluntarily submitted to the FBI by police departments. They report monthly counts of offenses known to the police and of offenses cleared by arrest for seven crimes (burglary, larceny-theft, motor vehicle theft, murder and negligent manslaughter, rape, robbery, and assault).[13] Whereas the first UCRs were published in 1930, data at the individual department level are only available after 1960.

I use UCRs to define two sets of outcomes. The first set of outcomes are property (burglary, larceny and vehicle theft) and violent (robbery, assault, rape, murder and negligent manslaughter) crime rates. Crime rates are crime per 100,000 people. To calculate crime rates in intercensal years, I linearly interpolate municipal population from the official publications of the Census Bureau. I analyze separately the property and the violent crime rate to allow for the possibility that they have different determinants and are thus differentially affected by police actions. The second set of outcomes are property and violent crime clearance rates. Clearance rates are defined as the number of crimes cleared by arrest over total crimes.[14] The property and the violent crime clearance rate

[12]Some of the first studies to use these data were Tolbert and Zucker (1983) and Rauch (1995).

[13]Assault includes both simple and aggravated assault. As noted by Evans and Owens (2007), "the UCR data are essentially unedited by the FBI, and there is tremendous heterogeneity across cities in the quality of the reporting. As a result, the data requires thorough cleaning before use." I clean the data following the indications reported by Maltz (2006) but do not use his data imputation procedure. Appendix E included in the Online Appendix discusses in detail the data cleaning procedure.

[14]The FBI website states: "for a crime to be cleared by arrest it must be the case that at least one person has been: (1) arrested; (2) charged with the commission of the offense; (3) turned over to the court for prosecution." There is no perfect correspondence between the crimes that are reported as being cleared in a certain month and the offenses taking place in that month. I ignore the issue when defining the outcome as I find a large effect on crimes, which suggests that in order to use clearance rates to proxy for performance normalizing by volume is important. In addition, to avoid results being driven by outlier months in which the number of crimes cleared by arrest is higher than the number of crimes and support the interpretation of the outcome as fraction of crimes cleared by arrest, I windsorize the outcome at 1. Clearance rates have been defined in this way and used as proxy for performance in the economics of crime literature, in particular in McCrary (2007).

are also analyzed separately to take into account compositional effects in type of crime, as violent crimes have higher clearance rates on average.

Table 2 presents the descriptive statistics. All statistics are for municipalities in the control group within a 1,250 population bandwidth from the threshold. I restrict the sample to the one used in the main analysis, which includes outcome data 1960 to 1980 and exploits variation in treatment status from 1970. The pre-treatment sample covers 1960 to 1969, while the post-treatment sample covers 1970 to 1979. In the pre-treatment sample, there are around 84 property crimes and 11 violent crimes per 100,000 people per month. Consistently with a general trend toward higher crime in the 1970s, crime rates are higher in the post-treatment sample. There are 228 property crimes and 30 violent crimes per 100,000 people per month. Clearance rates are around 22% for property crimes and significantly higher, at 70%, for violent crimes in the pre-period. Average clearance rates are 19% and 66% for the post-period. The increase in sample size from the pre- to the post-treatment period is driven by improved coverage over time.

Expenditures and employment. Data on expenditures and employment for police departments are from the Census Bureau. The data on expenditures are available at the municipality level starting from 1970. The data on employment and payroll expenditures are available starting from 1972, although data specifically for police officers (as opposed to everyone employed by the department) are only available starting from 1977. Both datasets cover the universe of municipalities in 1972 and 1977 (from the Census of Government) and a sample of local governments in all other years (from the Annual Survey of State and Local Government Finances). The specific outcomes I look at are total and payroll expenditures per 1,000 people and total and sworn officers employment per 1,000 people. I linearly interpolate population for intercensal years.

Police officer characteristics. I construct a dataset of police officers using the full count microdata of the 1910 to 1940 population censuses available through the Minnesota Population Center and ancestry.com.[15] I identify police officers using reported occupation, industry and class of worker.[16] I assign them to the police department of the municipality in which they were enumerated, as residency requirements were widespread before World War II.[17] I validate the procedure for 1940 by comparing the number of police officers I find in the census and the number reported in a survey of police departments of municipalities with more than 2,000 inhabitants published by the League of Wisconsin Municipalities in 1939. I am able to match the size of most departments and mismeasure by more than two police officers in a single case. Finally, the questions included in the census vary across years. Using the historical data, I study the ethnic composition, ethnic patronage and human capital.[18]

[15]The micro data for the 1900 census is available, but it lacks information on occupation (currently in digitization).

[16]Appendix F included in the Online Appendix discusses how I identify police officers in detail.

[17]See the Wickersham Commission report on the police, 1931, p. 64 and Fosdick (1920, p. 277).

[18]The microdata for the population censuses after 1960 are available through the Research Data Centers of the Census Bureau. I applied for access to the modern census data in December 2015 and am currently in the process of obtaining application approval. In the modern census data, I plan to use reported place of work to assign police offers to the correct department. In addition to human capital, I will use the modern data to explore the racial and gender composition of the departments.

4 Empirical strategy

The empirical strategy to identify the impact of merit systems exploits population-based mandates in a regression discontinuity design. The key feature of the setting is that each population census defines a separate experiment: whenever a new census is taken, treatment is assigned to all previously untreated ("at risk") municipalities. As a result, the effect of the mandate can be estimated using a separate cross-sectional regression discontinuity design for each census experiment. To maximize power, the baseline specification pools all experiments and estimates the average treatment effect. The baseline specification is:

$$y_{mtc} = \beta \mathbb{1}(dist_{mc} \geqslant 0) + f_c(dist_{mc}) + \delta_{stc} + \varepsilon_{mtc} \; for \; m \in RS^c \tag{1}$$

y_{mtc} is outcome y for municipality m, month (or year) t and census experiment c, $dist_{mc}$ is the population distance to the threshold (i.e. the running variable), $\mathbb{1}(dist_{mc} \geqslant 0)$ is an indicator for being above the threshold, $f_c(dist_{mc})$ are a set of census experiment specific flexible functions of the running variable, δ_{stc} are state, month (or year) and census experiment fixed effects and RS^c is the set of "at risk" municipalities for census experiment c. β estimates the effect of having a mandated merit system and is the coefficient of interest. The controls in the running variable vary by census to allow for additional flexibility, while the fixed effects increase precision. Standard errors are clustered at the municipality level to correct for the correlation induced by including the same municipality multiple times in the estimation. To take into account the possibility that there are too few clusters, I also compute wild bootstrap p-values following Cameron, Gelbach, and Miller (2008) and Cameron and Miller (2015).

The specification is estimated for the set of "at risk" municipalities: all municipalities in the last census before the introduction of the state legislation and previously untreated municipalities in each census experiment thereafter. The main effect is estimated pooling all post-treatment observations. The post-treatment period starts either in the year of introduction of the mandate at the state level or, for all the following census experiments, in the year of the population census itself.[19] The post-treatment period ends in the year of the following census. I focus on the short-term effect of the mandate because the long-term effect would be confounded by the control municipalities growing above the threshold and being treated in following census experiments.[20] In addition, I estimate the same specification on the sample of pre-treatment observations to test for pre-existing discontinuities in the outcomes.

[19]Preliminary counts for the population census were published between May and October, which makes the year when the census is taken a transition year. In the baseline estimation, I consider it a post-treatment year, but I show that my results are robust to treating it as a pre-treatment year in Table 6a.

[20]In principle, I could estimate medium-term effects of the reform by comparing medium-term outcomes for places that were just above and just below the threshold in a certain census and are below the threshold in the following one. However, given that most cities experience population growth, I do not have enough data to estimate such treatment effects.

I observe each census experiment's outcomes for different years since treatment. I study heterogeneous effects along this dimension using the following RD event study specification:

$$y_{mtc} = \sum_{\sigma \in \{-5,+10\}} \beta_\sigma \mathbb{1}(dist_{mc} \geqslant 0)\mathbb{1}(t - \tilde{c} = \sigma) + f_{ct}(dist_{mc}) + \delta_{stc} + \varepsilon_{mtc} \; for \; m \in RS^c \tag{2}$$

y_{mtc} is outcome y for municipality m, month (or year) t and census experiment c, $dist_{mc}$ is the population distance to the threshold (i.e. the running variable), $\mathbb{1}(dist_{mc} \geqslant 0)$ is an indicator for being above the threshold, $\mathbb{1}(t - \tilde{c} = \sigma)$ is an indicator equal to 1 if σ years have elapsed since treatment ($\tilde{c}$ is treatment year for census experiment c), $f_{ct}(dist_{mc})$ is a set of census experiment and year specific flexible functions of the running variable, δ_{stc} are state, month (or year) and census experiment fixed effects and RS^c is the set of "at risk" municipalities for census experiment c. β_σ estimates the effect of having a mandated merit system for σ years and is equivalent to the RD estimate from a cross-sectional RD that pools all observations measured σ years since treatment.[21] The specification is estimated pooling both pre- and post-treatment observations. Standard errors are clustered at the municipality level.

The identification assumption is that all factors other than treatment vary continuously at the threshold. First, municipalities must not sort around the cutoff according to their characteristics. I validate the design by testing for discontinuities in the density of the running variable (McCrary, 2008) and in baseline covariates. Second, it must be the case that no other policies change at the same threshold, a particularly common issue for RD designs based on population cutoffs (Eggers et al., Forthcoming). Almost every state had other policies that changed at the same threshold but no single legislative provision was the same across states. I therefore argue that my results are not driven by other state-specific provisions by showing robustness to estimating the main specification excluding one state at the time.

I estimate the results using locally linear regression (Gelman and Imbens, 2016) and a uniform kernel, which is equivalent to estimating a linear regression on observations within the bandwidth separately on both sides on the discontinuity. I show results for three fixed bandwidths (750, 1,000, 1,250) and for an outcome and sample specific MSE-optimal bandwidth calculated using the procedure suggested by Calonico, Cattaneo, and Titiunik (2014). The optimal bandwidth is calculated separately for each outcome and sample after partialling out the fixed effects and allowing for clustering of the standard errors following Bartalotti and Brummet (2016).

Specification checks

To provide evidence supporting the validity of the identification assumption, I test for discontinuities in the density of the running variable and in baseline covariates separately for each census experiment used in the analysis. First, I use a McCrary test to show that municipalities did not

[21] The crime outcomes are measured monthly. I estimate heterogenous effects at the year level to increase power.

sort around the threshold. Figure 1 presents the McCrary test for the 1970 census experiment (the census experiments used in the main analysis). The McCrary test does not show a discontinuity in the density of the running variable. Appendix Figure 1a and Appendix Figure 1b present the McCrary test for the other census experiments. There is no discontinuity in the density of the running variable for ten out of the eleven census experiments, but the McCrary test barely fails for 1980.

Second, I show that there are no discontinuities in baseline characteristics at the threshold. I estimate equation (1) using as outcomes municipality characteristics measured in the population census in which treatment was assigned. Table 3 shows the results of the covariate balance test for the 1970 census experiment. It reports the coefficient on the dummy for being above the threshold for three fixed bandwidths (750, 1,000, 1,250) and an outcome specific MSE-optimal bandwidth. None of the coefficients is statistically significant: the places just below the threshold appear to be a good control group for those just above. Appendix Tables B-2a and B-2b show covariate balance for the other census experiments used in the analysis (1910 to 1940 and 1980 to 2000). Some of the coefficients are statistically significant for some of the bandwidths, in line with what one would expect based on testing error and given the number of equations being estimated.

5 Results

Merit system adoption

I begin by examining pre-1940 merit system adoption. I proxy for merit system adoption using year of introduction of a civil service board. This measure captures the presence of a full-fledged merit system but the protections granted to police department employees were valid and violations could be challenged in court from the moment in which an official population census was published. A partial treatment was therefore in place even without a board: the measure understates the extent to which police departments were covered by merit system provisions.

Figure 2a shows the RD graphs for merit system adoption for the pre-treatment sample (graph to the left) and for the post-treatment sample (graph to the right). The outcome is a dummy equal to one if the municipality has a civil service board and zero otherwise. The dots show the average value of the outcome for different bins of the running variable. The line plots the fit from a locally linear regression estimated separately on each side of the discontinuity. Since the mean of the outcome may be different across experiments, I partial out state-year-census experiment fixed effects. The pre-treatment sample includes the ten years before the mandate becomes effective (either the year of introduction of the state legislation or the census year). The post-treatment sample includes the years between then and the following population census. Given that the outcome data are available until 1940, the first stage exploits variation in treatment status from the 1900, 1910, 1920 and 1930 census experiments (Appendix Table 3 shows the years included in the sample for each census experiment).[22]

The graphs show no jump at the discontinuity in the pre-period, although some municipalities both above and below the threshold already have a merit system. In the post-period, there is a large jump in the probability of having a merit system right at the threshold. Table 4 shows the coefficient on the dummy for being above the threshold for four different bandwidths for the pre-treatment sample (columns 1 to 4) and for the post-treatment sample (columns 5 to 8). As evidenced by the RD graphs, there is no discontinuity at the threshold in the probability of having a civil service board before the mandate is introduced. In the post-period, however, places above the threshold are 33% to 43% more likely (depending on the bandwidth) to have a civil service board than the places below. The coefficient is statistically significant at the 5% level.

The effect is large but less than one, both because some places below the threshold introduced a civil service board and because some places above the threshold failed to. In particular, there may have been some delays between when treatment was assigned and when a civil service board was created. To explore the possibility, I estimate the event study specification (equation (2)) and show the β_σ coefficients together with 95% confidence intervals in Figure 2b.[23] In the 5 years before the reform, municipalities just above the thresholds are not more likely to have a civil service board than the municipalities just below. In the year in which treatment is assigned, there is a large increase in the coefficient: municipalities covered by the mandate are significantly more likely to have a merit system. The effect of the mandate then becomes larger over time, suggesting that there were indeed delays in implementation.

Effects on performance

I study the effect of police professionalization on performance by estimating the impact of merit system mandates on crime and clearance rates. Crime data are available at the department level starting from 1960. U.S. Supreme Court decisions banning patronage dismissals substantially altered the content of the reform at the end of the 1970s. The analysis of the effect of merit systems on performance uses outcomes for the 1960 to 1980 period and variation in treatment status from the 1970 census experiment.[24] As there exists no data on merit system adoption for this period, the

[22]More precisely, the outcome data are available until 1943. In the baseline results I exclude the 1940 census experiment as I do not have the full post-period. Appendix Table B-4 shows the first stage including the 1940 census experiment

[23]Differently from differences-in-differences event study specifications, there is no omitted category because the model never gets fully saturated and the omitted category that serves as control is constituted by the controls municipalities in each experiment.

[24]The 1970 census experiment is the only one for which outcome data are available for both the pre- and for the post-period. The 1960 census experiment has outcome data for the post-period. However, as shown in Appendix Table B-5 panel (a), police departments in municipalities just above the threshold are more likely to submit data to the FBI. This is a potentially interesting outcome as it suggests that police departments under a merit system have better record keeping practices. However, it makes it impossible to interpret the results on crime rates. Municipalities just above the threshold appear to have higher property crime rates, which is consistent with police departments under a merit system submitting their crime data independently of what the crime rate is and places not under a merit system submitting their data only when the crime rate is low.

analysis estimates intention to treat effects.

The first set of outcomes that I examine are crime rates, defined as crimes per 100,000 people. I use log crime rates to make the coefficients comparable across experiments as the period used in the analysis is characterized by a substantial increase in crime. The pre-treatment sample covers 1960 to 1969; the post-treatment sample covers 1970 to 1979. In five out of the nine states in the sample, the mandate was explicitly based on population measured in the federal census. In the remaining four states (Illinois, Montana, Nebraska and West Virginia), the mandate was based on population measured in any official municipal, state or federal census. In these states, it is likely that the mandate became effective before the federal census was released, as the actual population of a municipality grew above the threshold and an official census was taken. The analysis of the pre-treatment sample aims to show that there are no systematic differences in the outcomes before the mandate becomes effective. A difference in the outcomes driven by early treatment, a "true anticipation effect," does not invalidate the design. I therefore exclude from the pre-treatment sample state and year combinations in which early treatment is likely (in this case, the last three years before a federal population census is taken).[25]

Figure 3a presents the RD graphs for the property and violent crime rate for the pre-treatment sample (graph to the left) and for the post-treatment sample (graph to the right). There is no difference in the property crime rate at the discontinuity in the pre-treatment sample. However, after the mandate becomes effective, municipalities just above the threshold have a lower property crime rate than those just below. The violent crime rate does not present a discontinuity either in the pre- or in the post-treatment sample. Table 5 panel (a) shows the effect of having a mandated merit system for four different bandwidths for the pre-treatment sample (columns 1 to 4) and for the post-treatment sample (columns 5 to 8). The table reflects the results suggested by the raw data in the graphs: there is no difference in the property crime rate in the pre-period, but municipalities above the threshold have a lower property crime rate in the post-period with respect to those below. The coefficients are statistically significant at the 5% level, and the result is robust to different bandwidths. There is no difference in the violent crime rate.

The magnitude of the effect is large: looking at the estimates for places within a 1,000 bandwidth from the threshold, the coefficient shows a 46% reduction in the property crime rate for treated places in the first ten years after the reform was introduced. This is equivalent to 4.6 fewer property crimes per month for a municipality of 5,000 inhabitants. Crime rates are noisy and standard errors are large: the 95% confidence interval is always negative but contains effect of very different magnitudes.[26]

[25]It is not surprising that the anticipation effect does not appear in the pre-1940 merit system adoption analysis. First, the majority of the sample is composed of municipalities from states in which the mandates are explicitly based on the federal population census. Second, the anticipation effect is not present when the mandate becomes effective based on the introduction of new statewide reforms, as is the case in many of the experiments included in the sample.

[26]The results presented in this section are intention to treat effects. Since there is imperfect compliance, it is possible that the true effect of the reform is larger. The estimates on merit system adoption, however, are too small and do not provide the correct factor by which to rescale the effects. First, as discussed in footnote 22, the pre-1940 sample does

Merit systems affect property but not violent crime rates. It is possible that property and violent crimes have different determinants. Moreover, violent crimes are rarer, and the sample may not be large enough to see an effect. However, the differential effect may also be interpreted as potentially indicating differential reporting at the threshold, a possibility that I discuss in detail below. In addition, police performance is only one of the many determinants of crime rates, but to the extent that unobservables vary continuously at the threshold and that there are no pre-treatment differences in the socio-economic composition of control and treated municipalities, it seems unlikely that the decline is driven by external factors.

The results presented thus far show the average treatment effect in the pre- and post-treatment sample. The effect of the mandate, however, may change as a function of time since treatment. I estimate the event study specification (equation (2)) and show the β_σ coefficients together with 95% confidence intervals in Figure 3b panel (a). The graph shows that the effect is gradual over time and is statistically significant starting five years after treatment is assigned according to the 1970 population census. None of the coefficients in the pre-period is statistically significant, but the point estimates start being negative two to three years before treatment. This is not surprising given that the specification is estimated on the full set of states, including those for which a "true anticipation effect" is likely. Figure 3b panel (b) shows the event study graph separately for states where the mandate was explicitly based on the federal population census and for states where official municipal and state censuses were also valid. As expected, there is an effect of the mandate before 1970 only for the latter group of states. When states likely to experience anticipation effects are excluded, there is no difference in crime rates until 1972. The decline is gradual at first but remains constant in magnitude in the following years. Whereas none of the coefficients in the event study restricted to states with mandates based on federal population census is statistically significant, the magnitudes are similar as in the full sample.

The second set of outcomes that I examine are clearance rates, defined as number of crimes cleared by arrest over total crimes. Figure 4a presents the RD graphs for property and violent crime clearance rates for the pre-treatment sample (graph to the left) and for the post-treatment sample (graph to the right). The RD graphs show that there is no difference in the property crime clearance rate, either pre- or post-treatment. However, even if there is no difference in the pre-period, the violent crime clearance rate is higher in places above the threshold with respect to places below after the mandate becomes effective. Table 5 panel (b) shows the treatment effects from the estimation of equation (1). Columns 1-4 confirm that there is no difference in clearance rates in the pre-treatment sample. In the case of violent crime, the coefficient is negative and has the same order of magnitude as the main effect, but it is generally not significant. In the post-period, the coefficient is positive and statistically significant at the 5% level. In particular, police departments in municipalities just above the threshold are 12% more likely to clear a violent crime by arrest

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not measure the "true anticipation effect" in reform adoption. Second, the proxy used to study merit system adoption (whether the municipality has a civil service board) is an imperfect measure of merit system adoption. Since protections against patronage hiring and dismissals can be challenged in court even without a commission being, a partial treatment is in place even without the institution of a full-fledged merit system.

than those just below. Figure 4b shows the event study graph for the violent crime clearance rate. The event study graph, although noisier, shows a similar time pattern in the treatment effect: a gradual increase in police performance starting two years after the introduction of the reform and a constant effect thereafter.

I interpret the result on the property crime rate as evidence that merit systems improved police performance. The greatest challenge to this interpretation is the concern that the result may be confounded by differential crime reporting at the threshold. Differential reporting may arise in three different stages. First, citizens who experience a crime may not report it or, even if the crime is reported, the police may fail to create a record for it. Misreporting at this stage is less likely for crimes that involve insured goods such as burglaries and vehicle thefts, as insurance companies often would not honor theft claims without a police report. Appendix Table B-6a shows that merit systems had a negative effect both on the burglary and vehicle theft rate and on the larceny rate. Second, after a record is created, it can be altered to distort crime incidents reported to the FBI. In particular, as discussed in Mosher, Miethe, and Hart (2010), an offense can be downgraded to a non-index crime or it can be reported as unfounded. The fact that I find similar effects across crime types is reassuring as not all crimes can be downgraded as easily (for example, larcenies below $50 are not an index crime, which makes them particularly susceptible to the issue). Unfortunately, counts of unfounded offenses are not reported before 1978 so I cannot test directly whether this dimension is affected. Third, the department may decide to simply not submit a report to the FBI as participation in the UCR program is voluntary. I can exclude the possibility since, as Appendix Table B-7 shows, there is no discontinuity at the threshold in the probability of submitting crime data for any given month. The discussion, together with the fact that I also find a positive effect on the violent crime clearance rate, suggests that it is unlikely that the effects are driven by differential crime reporting.

Robustness checks

Table 6a tests whether the results are robust to different samples and specifications. I only report estimates for a 1,000 bandwidth. Tables C-1 to C-5 in the Online Appendix show the estimates for the full set of bandwidths. The results are robust to the choices made in defining the sample. Table 6a column 1 shows the pre-period estimates including all years. As one would expect when including states likely to have anticipation effects, the coefficient on the property crime rate is larger in absolute value, although it is not significant. The other coefficients are similarly not significant. Columns 2 and 5 restrict the sample to states with mandates explicitly based on federal population censuses for the pre- and post-treatment sample. In the pre-treatment sample, the coefficient on the property crime rate is large and positive and not statistically different from 0. The coefficient on the violent crime clearance rate is positive and significant. The sample restriction implies dropping around 47% of the observations, which is particularly problematic for the violent crime clearance rate as the outcome is defined for fewer observations: it is not surprising that the coefficient is noisy and sensitive to the choice of sample. Columns 3 and 6 show the estimation assigning 1970 to the pre-treatment period. In the pre-period, both the coefficient on the violent crime rate and the coefficient on the violent crime clearance rate are significant. Looking at the full set of bandwidths however shows that this is not the case for all of them. The results in the post-treatment period are robust to excluding 1970, although the effect on the violent crime clearance rate is not statistically significant for the optimal bandwidth.

The remaining columns show robustness to different specifications. Columns 4 and 7 show robustness to controlling for a full set of baseline municipality characteristics.[27] The property crime rate coefficient is negative and significant at the 5% level in the pre-period and larger and significant at the 1% level in the post-period. Online Appendix Table C-4a shows that this is the case for all but the largest bandwidth, but Online Appendix Table C-4b suggests that the control that is driving the result in the median household income. Finally, column 8 shows that the results are robust to a specification in which I control for the baseline value of the outcome.[28]

Table 6b shows robustness to re-estimating the main specification excluding one state at the time for the post-treatment period. This is an important exercise because it shows that the results are not driven by state-specific policies also changing at the same threshold. Almost all of the states in my sample have at least one legislative provision that implies a policy discontinuity at the same cutoff, but no single provision is the same across states. Were the effects driven by any of the other policy discontinuities, they should disappear once the state is dropped. Also, the provision would have to be strong enough to influence the overall treatment effect in its direction. As the table shows, the treatment effect in the post-period survives the sequential exclusion of each state. The magnitude of the coefficients is generally similar, with the exception of the coefficient on the violent crime clearance rate that is almost double in magnitude when Illinois is dropped. Table C-6 in the Online Appendix shows that in the pre-treatment period the coefficients are sensitive to dropping Illinois and Iowa and are statistically significant for the violent crime clearance rate. This is likely due to the fact that I have fewer observations for the pre-treatment period which, as mentioned above, is especially problematic for the violent crime clearance rate.

Locally linear regression is the preferred estimation technique following Gelman and Imbens (2016) but I test whether the results are robust to different methods in Appendix Table B-8. First, I show robustness to using different kernels. Columns 1 and 5 show the estimation using a triangular kernel, while columns 2 and 6 show the estimation using an Epanechnikov kernel. The results in the post-treatment period are unchanged but the coefficient on the violent crime clearance rate is negative and statistically significant in the pre-treatment period. Second, I estimate the main specification using locally quadratic regression (columns 3 and 7) and locally cubic regression (columns 4 and 8) with a uniform kernel. The results on the property crime rate is robust to using poly-

[27]The controls included are percentage male, percentage non-white, percentage with high school degree, percentage unemployed, percentage below poverty line and median income according to the 1970 census.

[28]I include the average value of the outcome in the pre-treatment period after having partialled out state and month fixed effects.

nomials of different orders but the result on the violent crime clearance rate is not. In particular, although the magnitude and sign of the coefficient are similar, it is not significant in the post-treatment period. Table C-12 in the Online Appendix shows that the results are robust to allowing the running variable to vary flexibly both by census and by outcome year as in the event study specification. Table C-13 in the Online Appendix shows the results allowing for clustering both at the municipality and at the running variable level as suggested by Lee and Card (2008) in cases in which the running variable is discrete. Two-way clustering does not make a difference, as one would expect given that few municipalities have the same value of the running variable (e.g., the number of clusters for the post-treatment sample in a 1,000 bandwidth increases only by four).

Finally, the result that merit systems reduced property crimes does not depend on how the outcome is defined. As reported in Appendix Table B-9, crime rates expressed in levels, crime counts and log of crime counts all show no difference in the pre-period and a large decline in the post-period. However, when the outcome is defined as the log of crime rate + 1 (a common transformation in the literature to include all observations that are 0), some of the bandwidths show pre-trends.

Overall, the effect of merit systems on the property crime rate is robust to different estimation techniques, sample definitions and specifications. The effect on the violent crime clearance rate, however, is less robust and there appears to be a negative and statistically significant difference in places just above and just below the threshold in the pre-treatment period. This is likely due to the fact that the outcome is defined for a significantly smaller sample, as a municipality must have experienced at least one violent crime for it to be defined. Moreover, the event study shows that one single year, 1966, is likely driving the negative coefficients in the pre-treatment period, whereas the coefficients for other years do not appear to be negative.

6 Mechanisms

The results presented thus far establish that mandates for adoption of merit systems improved the performance of police departments. Merit systems may impact performance through three main channels: resources, police officers' characteristics and police officers' incentive structure.

Resources

Merit systems may impact performance by increasing the resources available to the department. For example, professionalized police departments may have greater bargaining power in budget allocation decisions or may be able to attract more funding from outside sources. Alternatively, resources may be negatively affected as the political authority may have an incentive to decrease funding allocation upon losing control of the police department (Ujhelyi, 2014). I explore the channel by studying expenditures and employment.

Figure 5 shows no difference in total or payroll expenditures and on total or police officers' employment in the post-treatment period. Places above and below the threshold have similar expenditure and employment levels. The result is confirmed by both panels of Table 7, and Appendix Table B-11 shows that it is true no matter how the outcomes are defined.

The result that departments operating under a merit system and under a spoils system has access to the same amount of resources suggests that they used similar inputs. There was no adjustment in labor supply along extensive margins. Changes in equipment or in the labor supply of police officers on the intensive margin (for example through overtime hours) would likely be reflected in total or payroll expenditures.[29] The effect must be explained by something that the police was doing keeping inputs constant.

Police officers' characteristics

Merit systems departments may select and retain more productive police officers. First, police officers in departments under a merit system may receive more training. According to the Olmstrom survey (1974), almost all police departments of municipalities with population below 10,000 people required training but almost none provided training in house. To the extent that the departments would have covered training costs, the fact that expenditures did not change suggests that large adjustments along the training margin are unlikely.

Second, merit system departments may be composed of police officers with different characteristics, in particular through an effect on selection. Merit systems may affect selection both directly and indirectly. First, merit systems require competitive examinations and for hiring and promotion decisions to be based exclusively on merit, thus directly affecting the selection process. In addition, as they change the incentive structure faced by police officers, merit systems may influence selection indirectly by attracting different applicants.

I test whether the demographic composition of police departments changed using the microdata from the population censuses 1910 to 1940. It is especially interesting to study the historical context as the direct channel is likely to have been particularly relevant. Over time, even municipalities without merit systems developed procedures to screen potential police officers, but it was less common before 1940.[30] Even in places with a merit system, selection methods were still being developed: it is unclear that any of the tests selected on relevant characteristics.[31]

[29] It is possible that police officers have the same labor supply but the fraction of time spent actively policing (for example the fraction of time spent patrolling) increases. This would not be picked up by payroll expenditures. I interpret these adjustments as changes in effort.

[30] For example, an article published in the League of Wisconsin municipalities in 1940 that provided a model ordinance for the organization of police departments of cities below 4,000 inhabitants as "requested by officers of numerous small Wisconsin cities" suggests unmet demand for such systems. In 1970, a book designed as a manual for the officers in charge of smaller police departments (Leonard, 1970) shows a much more advanced personnel system in place in smaller departments.

[31] Selection tests were comprised of a physical examination and an "aptitude test". The earliest tests often included spelling and arithmetic questions and tested the applicant's ability to provide directions to city landmarks (Fuld, 1909).

I examine three sets of outcomes: ethnic composition of police departments, ethnic patronage and human capital. Overall, I do not find evidence of selection being affected by merit systems in the pre-1940 period: merit systems did not impact the probability that foreigners were hired, they did not change the degree of ethnic patronage and they did not improve human capital.

Figure 6a shows the RD graphs for the ethnicity outcomes for the post-period. Overall, places with and without a merit system appear to have police departments with comparable ethnic composition. There is no difference in the fraction of police officers who are a first- or second-generation immigrant or who have a distinctively foreign sounding first name.[32] Table 8 panel (a) confirms the results and shows that this is true even when the sample is restricted to police officers more likely to be affected by the reform: those who are low-ranked, young or recent hires.[33] It is interesting to note however that the coefficients are generally negative. The fact that merit systems did not make it more difficult for foreigners to get access to these jobs is surprising as early police reforms had a strong anti-immigrant component (Fogelson, 1977, p. 42). Moreover, the tests often demanded a good command of English and merit systems, by formalizing applications, required documents such as birth certificates that were harder for foreigners to access.

Ethnic networks had a primary role in the distribution of public jobs (Walker, 1977, p. 11) and the reduction of ethnic patronage was one of the principal motivations behind the reforms in the Progressive Era. I test whether merit systems were successful in reaching this objective by looking at whether the fraction of police officers who were co-ethnic with the mayor or from the dominant ethnic group was different in places above and below the threshold. To define the outcomes, I collected the names and years of service of the mayors of the municipalities in the sample and assigned them to an ethnic group by matching them into the census microdata.[34] I define police officers as co-ethnic if they come from the same ethnic group as one of the mayors serving in the ten years prior to the census. Figure 6b shows the RD graphs for the ethnic patronage outcomes for the post-period. There appears to be no discontinuity at the threshold. This is confirmed by the regression estimates reported in Table 8 panel (b). There is no effect of being under a merit system

From the Report of the Crime Commission (1927, p. 251) as quoted in the Wickersham Commission Report on Police (1931, p. 65): "It is of small moment that the applicant can locate the Tropic of Capricorn or compute the number of rolls of wall paper required to paper a room of given dimension. The police administration seeks neither navigators nor interior designers." By the 1930s, the focus had shifted to tests designed to screen on aptitude, intelligence and adaptability (Stone, 1938), although there were still wide differences in the actual design of the test. A common choice was to use the Army Alpha test but a vast majority of the departments were still using unstandardized tests (O'Rourke, 1929).

[32] I use the 1930 5% sample to compute a group name index for each name occurring more than ten times following Fryer and Levitt (2004) and Fouka (2015):

$$GNI_{name} = \frac{Pr(name|group)}{Pr(name|group) + Pr(name|non-group)} \tag{3}$$

I then define a name to be from ethnic group G is the name is more than twice more likely in group G than in others.

[33] A police officer is considered "low ranked" if he does not specify a higher position in the department. Young police officers are those with an age below the median. I identify recently hired police officers by linking police officers across censuses and excluding police officers who were previously employed by the department. Appendix F included in the Online Appendix discusses the linking procedure.

[34] Appendix G included in the Online Appendix discusses the data collection and the linking procedure.

on the proxies for patronage hiring, although the result must be interpreted with caution as data limitations bias the result towards zero.

Finally, I study the human capital of police officers, specifically age and education. Figure 6c shows the RD graphs for the human capital outcomes. There is no discontinuity in average age or in the fraction of police officers who finished primary school. The RD graph for fraction of police officers who finished high school suggests that police officers hired in places just above the threshold are less likely to have achieved this education level. The regression table confirms the result: the fraction of police officers who finished high school is significantly lower in places under a merit system. Appendix Table B-14 validates the result by showing that the effect is not driven by overall changes in the education level of municipal employees: there is no discontinuity at the threshold for local workers not covered by the merit system. Moreover, a placebo test comparing places that were just treated in 1940 also does not show any difference between locations at the threshold. However, it is clear by looking at the RD graph that the effect, albeit robust, is driven by cities just below the discontinuity having an especially high educational attainment. I interpret the result on education as showing that, if anything, merit systems did not have a positive effect on this dimension.

Overall, selection on observable characteristics does not seem to have been impacted by the introduction of merit systems in the historical period. While it is possible that the unobserved characteristics of police officers changed, the fact that I find no clear breaks in any of these salient dimensions suggests a limited role for selection in explaining the performance improvement.

Incentive structure

The discussion thus far shows that the effect of merit systems on police performance cannot be explained by changes in resources or in police officers' characteristics. This suggests that the remaining channel, changes in the incentive structure faced by police officers on the job, is important to explain the result.

Merit systems may affect the incentive structure faced by police officers' in different ways. First, by limiting dismissals for reasons other than just cause, merit systems may decrease turnover and increase average tenure. I study turnover in the pre-1940 period using the dataset described in the previous section. In particular, I link police officers across censuses and define them as new hires if they were not employed by the department ten years prior.[35] Appendix Table B-15 panel (a) shows that I do not find effects on turnover in the pre-1940 period. This is likely due to the fact that turnover was extremely high: 95% of the police officers I find in each census were new hires (Appendix Table B-12). This implies that even large increases in average tenure may not appear in the data. The result, however, does not necessarily generalize to the more modern period. In particular, turnover was significantly lower after World War II and average seniority was around

[35] Appendix E included in the Online Appendix discusses the linking procedure.

ten years (Aamodt, 2004). I will test the hypothesis directly using the microdata from the modern population censuses.

Second, it is possible that merit systems affect how police officers are compensated, for example by changing the wage-experience profile. Using the pre-1940 data and proxying for experience using age, I find some suggestive evidence that income per age was lower under a merit system (Appendix Table B-15 panel (b)). However, the results must be interpreted with caution given the small sample size, a consequence of the fact that income was only recorded in the 1940 census. Again, I will test the hypothesis more directly using the microdata from the modern population censuses.

Third, merit systems may affect police officers' effort allocation and motivation. While I cannot provide direct evidence for this hypothesis, the explanation that motivation is important to explain police officers' performance is consistent, for example, with previous work on police departments by Mas (2006), who showed that final offer arbitration decisions against the wage required by the police officers have a negative effect on performance. Finally, merit systems may affect other aspects of the incentive structure such as the overall culture of the department or the matching of police officers' to tasks.

The component of the reform that is more likely to directly affect the incentive structure faced by police officers is the one that relates to patronage dismissals. It is unclear what the true extent of patronage was in the later period, especially as far as small municipalities are concerned. Overall, the excessive corruption that had characterized police employment under political machines was a thing of the past. Banfield and Wilson (1963) argue that "the more common practice among small cities without a civil service system is a rather informal but at the same time highly nonpolitical personnel system." However, they also reckon that many appointments were indeed political. Consistently with this interpretation, Freedman (1994) states: "there are probably thousands of small pockets of patronage lodged in the 80,000 plus units of local government in the United States."[36] Independently of this, merit systems still limited the power of the political authority to interfere with the department.

Whereas in the status quo the political authority was able to fire police officers as they saw fit, under a merit system dismissals were possible only for just cause. At the end of the 1970s, a series of U.S. Supreme Court decisions made patronage dismissals illegal for all municipal employees while statewide legislation related to merit systems stayed in place. When municipalities grew above the threshold, they were still mandated to create independent civil service commissions, but there was no discontinuity in whether political dismissals could be used to influence police officers' behavior: they could not - neither in the treatment nor in the control group. By studying the effect of merit system mandates for the census experiments after 1980, I can thus provide indirect evidence on the role of patronage dismissals provisions in explaining the results.

[36]For patronage dismissals to influence police officers' behavior, they do not need to be happening frequently as they may be an out-of-the-equilibrium-path outcome.

Figure 7a shows the RD graphs for treated and control municipalities in the 1980, 1990 and 2000 census experiment for the baseline pre- and post-treatment sample. The figure shows that there is no discontinuity at the threshold in the property crime rate, either in the pre- or post-treatment period. The RD graph for the violent crime rate shows no difference in the pre-treatment period, but suggests that there may be a difference at the threshold after the mandate becomes effective. Table 9 panel (a) shows the results from the corresponding regressions. The result of no change in the property crime rate is confirmed. For the violent crime rate, the coefficient for being above the threshold is indeed negative in the post-treatment, but it is only statistically significant for the largest threshold. In addition, Figure 7b and Table 9 panel (b) suggests no difference in clearance rates.

The results show that there was no difference in police performance when protection from patronage dismissals was not part of the treatment. The evidence is consistent with protection from political interference being important to explain the results, in particular to the extent that it affects the incentive structure faced by police officers. However, the analysis presented in this section exploits time variation and therefore the null results post-1980 may be caused by other changes impacting policing during the 1970s such as unionization, changes in sentencing or the start of the war on drugs.

7 Conclusion

Merit systems reducing politicians' control over bureaucrats' hiring and firing foster expertise and create a long-term incentive structure, but come at the cost of decreased responsiveness to the executive and the electorate. Whether they improve performance is unclear a priori and must be ascertained empirically.

I address the question by looking at the introduction of merit systems for U.S. municipal police departments in the 20th century. To address potential endogeneity concerns in reform adoption, I exploit statewide merit system mandates based on population thresholds to implement a regression discontinuity design. I find that merit systems increased performance. In the first ten years after the reform, the property crime rate was 46% higher and the violent crime clearance rate was 12% higher in municipalities just above the threshold with respect to municipalities just below.

Providing well-identified empirical evidence on the effect of merit system on performance is the principal contribution of the paper. The finding that professionalizing a public organization improves performance is consistent with cross-country correlations (e.g. Evans and Rauch, 1999; Rauch and Evans, 2000), evidence from large U.S. cities (Rauch, 1995) and recent work on perceived determinants of bureaucrats' effectiveness (Oliveros and Schuster, 2016) and on management practices and public service delivery (Rasul and Rogger, 2016).

Looking at the mechanisms suggests that merit systems' positive effect on performance is likely explained by the fact that they reduce a politicians' ability to influence the incentive structure that police officers face on the job. Whereas it is no surprise that political influence may distort public employees' behavior (e.g., among others, Eynde, Moradi, and Kuhn, 2016), what makes this result especially interesting is the fact that it holds in what appears to be an informal but relatively low corruption setting. Understanding the mechanisms behind this particular result is a fascinating question that I hope to address in future research.

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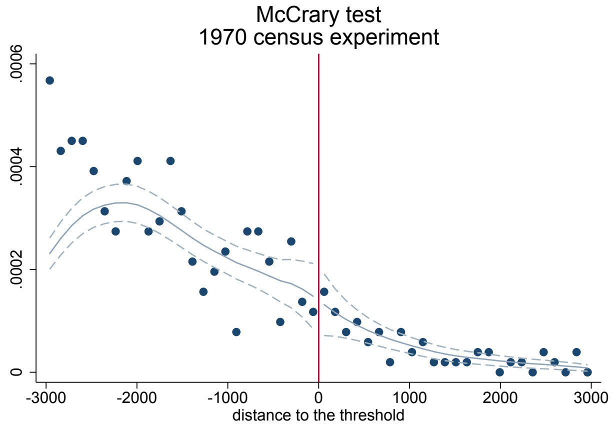
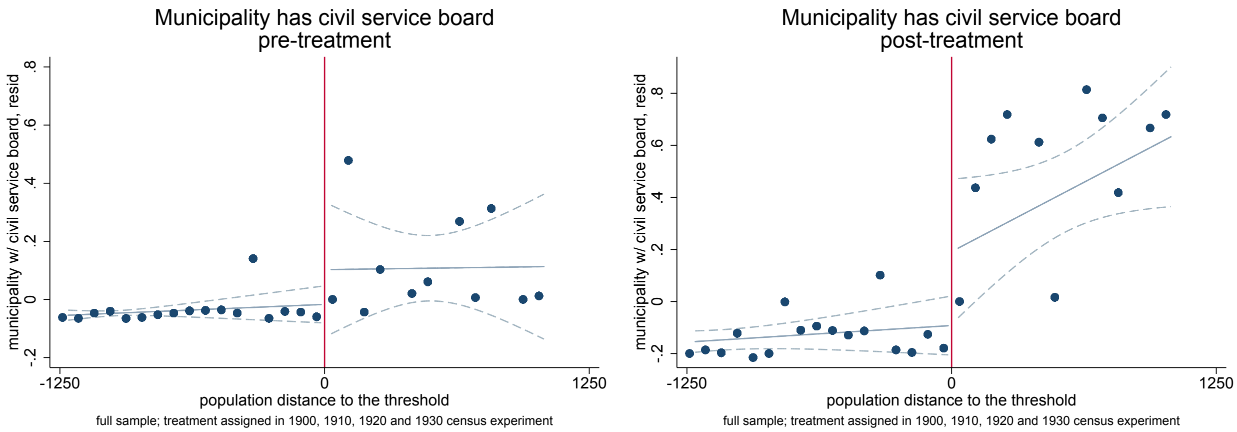
Figure 1: The McCrary test for 1970 shows no discontinuity in the running variable density



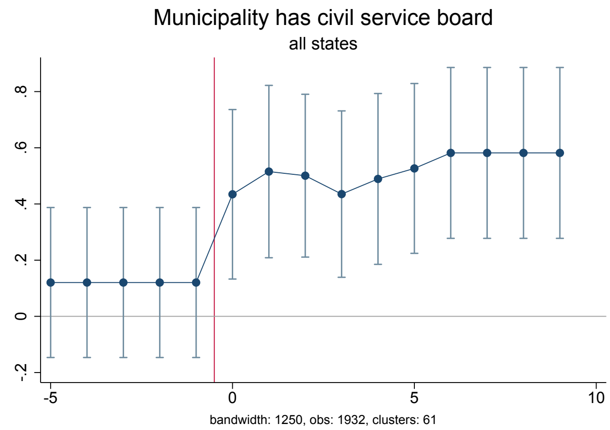
Notes: the graphs shows the McCrary (2008) test for the 1970 census experiment.

Figure 2a: Merit system mandates increase reform adoption pre-1940, RD graphs



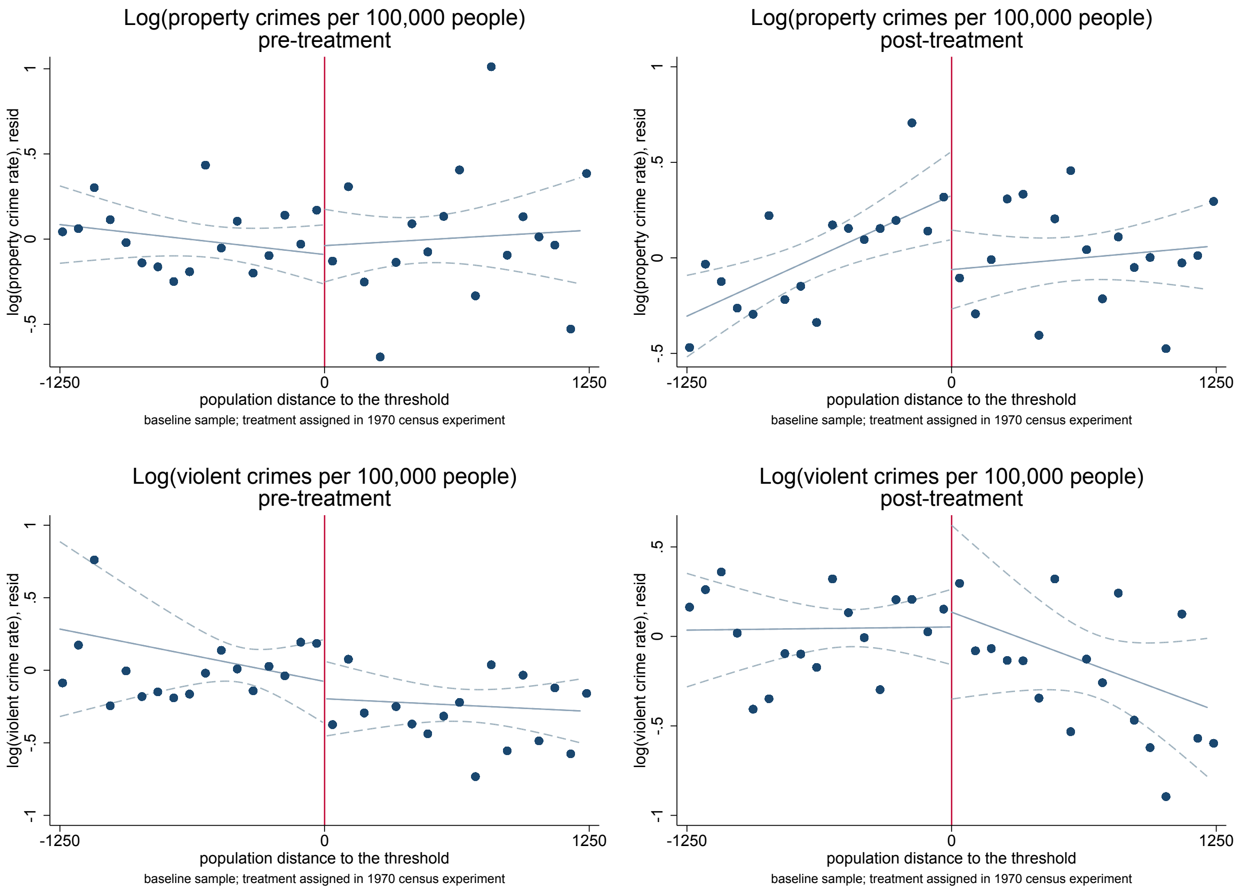
Notes: the graphs show the effect of merit system mandates on pre-1940 reform adoption for the sample of pre-treatment years (on the left) and post-treatment years (on the right). The sample exploits variation in treatment status from the 1900, 1910, 1920 and 1930 census experiments. Pre-treatment years span from the year of the previous census to the year in which treatment is assigned. Post-treatment years span from the year in which treatment is assigned to the year before the following census. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-year-census experiments fixed effects are partialled out.

Figure 2b: Merit system mandates increase reform adoption pre-1940, event study graph



Notes: the graph shows the effect of merit system mandates on pre-1940 reform adoption estimated using the event study specification (equation (2)). The sample exploits variation in treatment status from the 1900, 1910, 1920 and 1930 census experiments. The sample includes both pre-treatment and post-treatment years. Pre-treatment years span from the year of the previous census to the year in which treatment is assigned. Post-treatment years span from the year in which treatment is assigned to the year before the following census. The points are the point estimates β_σ from the event study specification with 95% confidence intervals. The coefficients are estimated using locally linear regression and a uniform kernel for a 1250 bandwidth. Standard errors are clustered at the municipality level. State-month fixed effects are included in all columns.

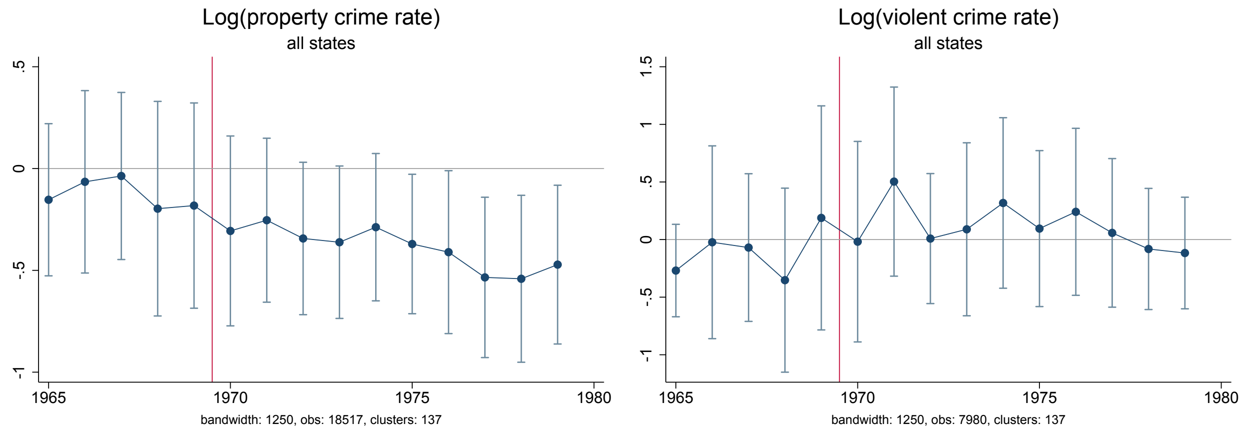
Figure 3a: Merit systems lower property crime rates, RD graphs



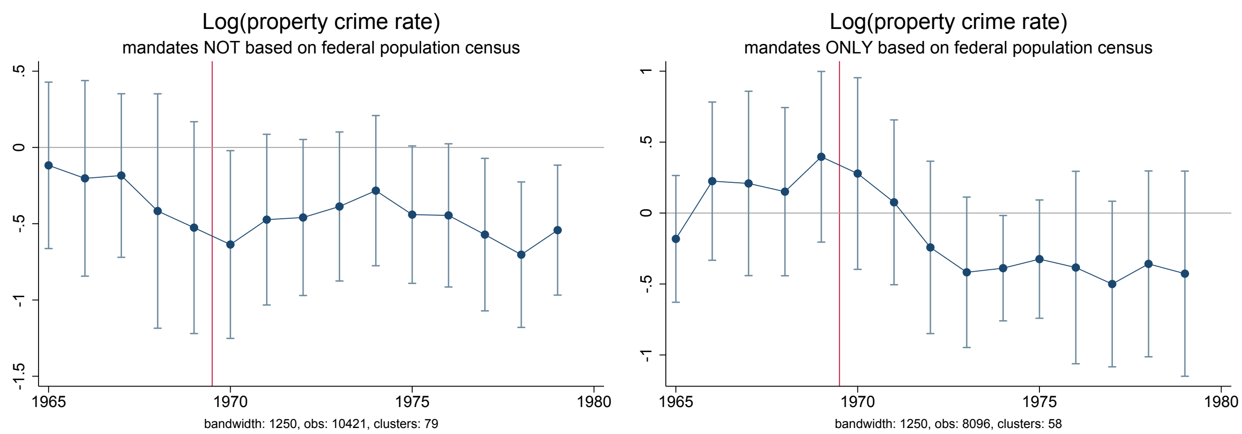
Notes: the graphs show the effect of merit system mandates on property and violent crime rates for the sample of pre-treatment years (on the left) and post-treatment years (on the right). Crime rates are crimes per 100,000 people. The sample exploits variation in treatment status from the 1970 census experiment. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-month fixed effects are partialled out.

Figure 3b: Merit systems lower property crime rates, event study graphs

Panel (a): effect on property and violent crime rate, all states

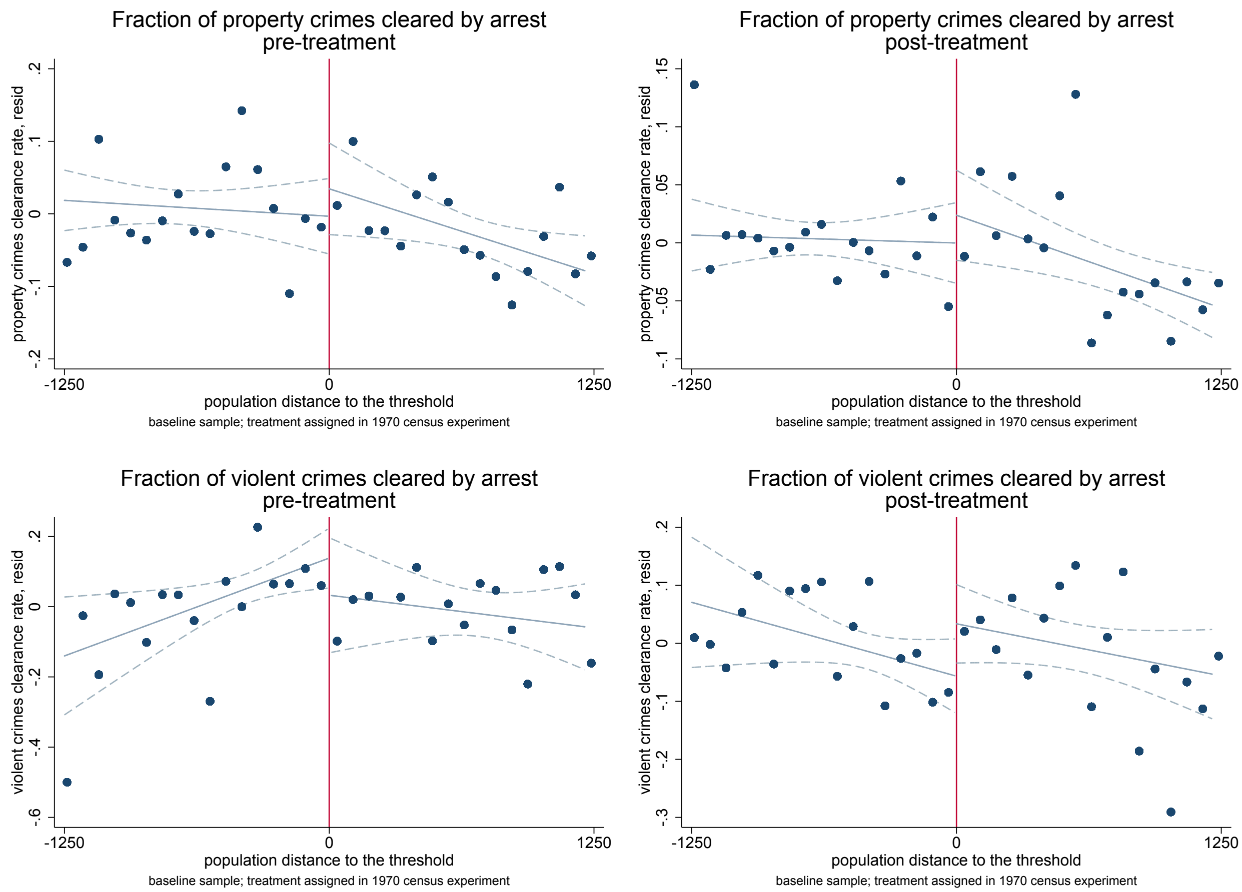


Panel (b): effect on property crime rate, separately for states with and without mandates based on federal population census



Notes: the graph shows the effect of merit system mandates estimated using the event study specification (equation (2)) on property and violent crime rates for the full sample of states (panel (a)) and on property crime rates separately for states with and without mandates explicitly based on federal population census (panel (b)). Crime rates are crimes per 100,000 people. The sample exploits variation in treatment status from the 1970 census experiments. The sample includes both pre-treatment and post-treatment years and spans 1965 to 1979. The points are the point estimates β_σ from the event study specification with 95% confidence intervals. The coefficients are estimated using locally linear regression and a uniform kernel for a 1250 bandwidth. Standard errors are clustered at the municipality level. State-month fixed effects are included in all columns.

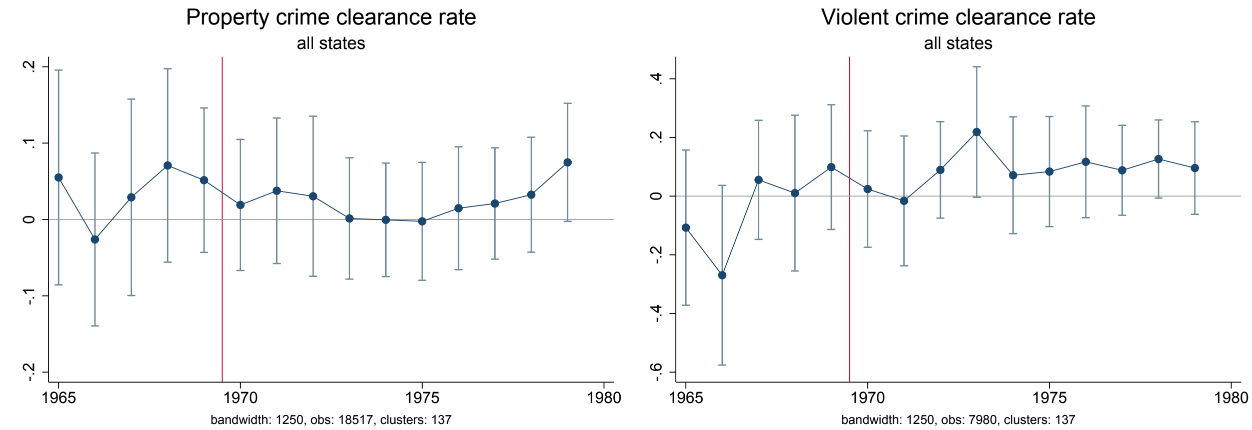
Figure 4a: Merit systems increase violent crime clearance rate, RD graphs



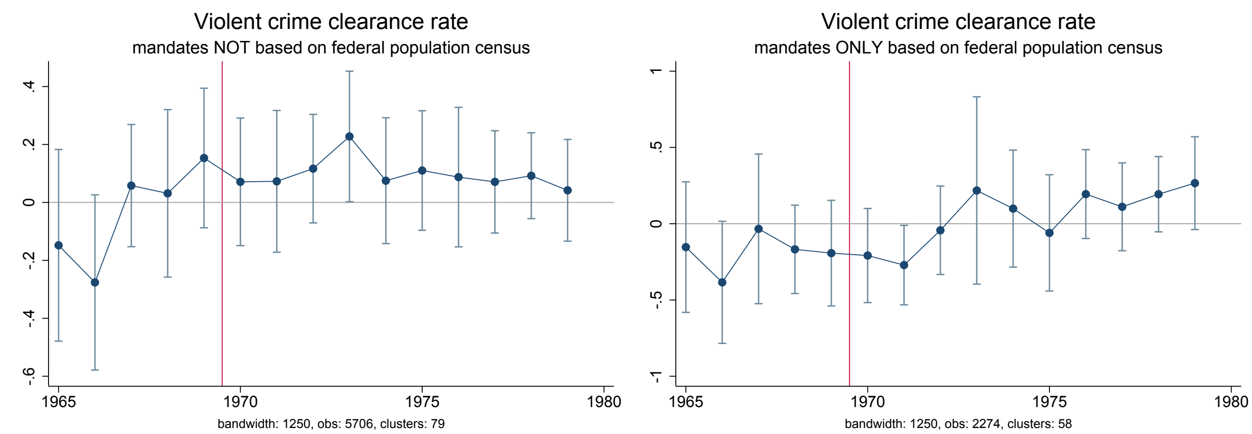
Notes: the graphs show the effect of merit system mandates on property and violent crime clearance rates for the sample of pre-treatment years (on the left) and post-treatment years (on the right). Clearance rates are number of crimes cleared by arrest over total number of crimes. The sample exploits variation in treatment status from the 1970 census experiment. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-month fixed effects are partialled out.

Figure 4b: Merit systems lower property crime rates, event study graphs

Panel (a): effect on property and violent crime clearance rate, all states

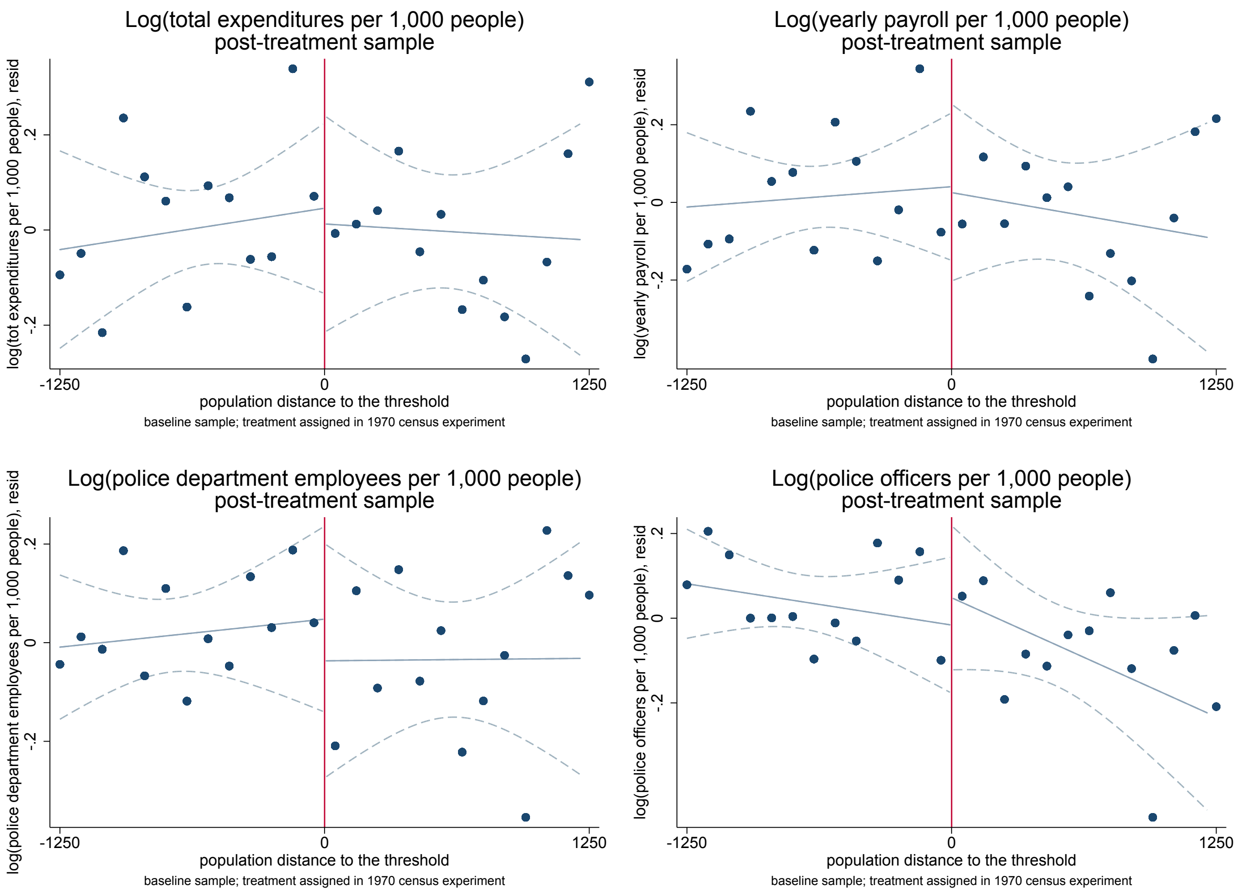


Panel (b): effect on violent crime clearance rate, separately for states with and without mandates based on federal population census



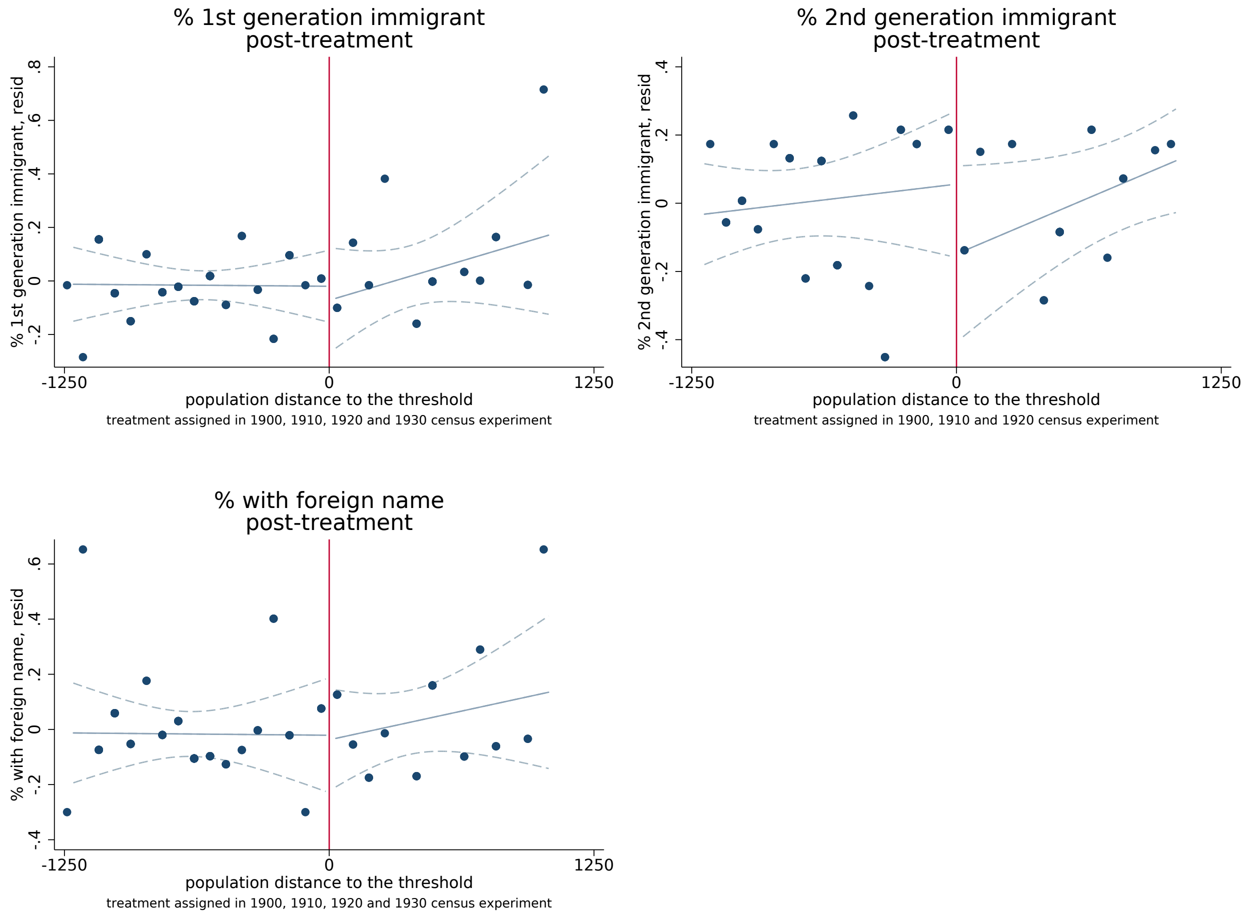
Notes: the graph shows the effect of merit system mandates estimated using the event study specification (equation (2)) on property and violent crime clearance rates for the full sample of states (panel (a)) and on violent crime clearance rates separately for states with and without mandates explicitly based on federal population census (panel (b)). Clearance rates are number of crimes cleared by arrest over total number of crimes. The sample exploits variation in treatment status from the 1970 census experiments. The sample includes both pre-treatment and post-treatment years and spans 1965 to 1979. The points are the point estimates β_σ from the event study specification with 95% confidence intervals. The coefficients are estimated using locally linear regression and a uniform kernel for a 1250 bandwidth. Standard errors are clustered at the municipality level. State-month fixed effects are included in all columns.

Figure 5: Merit systems do not affect expenditures or employment, RD graphs



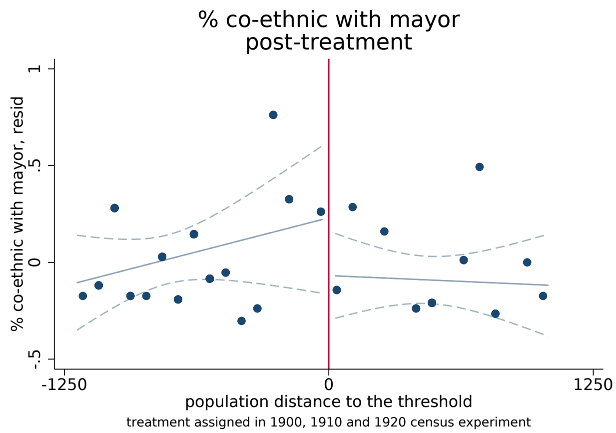
Notes: the graphs show the effect of merit system mandates on expenditures and employment for the sample of post-treatment years. The sample exploits variation in treatment status from the 1970 census experiment. Post-treatment years are 1970 to 1979 for expenditures, 1972 to 1979 for payroll expenditures and employment and 1977 to 1979 for officers. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-year fixed effects are partialled out.

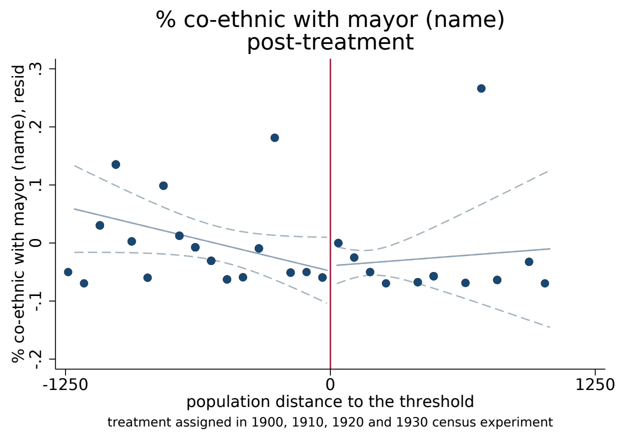
Figure 6a: Merit systems do not affect the ethnic composition of police departments, RD graphs

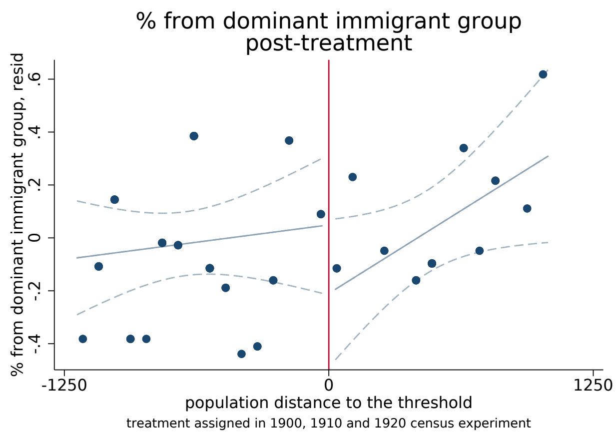


Notes: the graphs show the effect of merit system mandates on the ethnic composition of police departments for the sample of post-treatment years. The outcomes (and the census years for which they are available) are fraction first generation immigrant (1910-1940), fraction second generation immigrant (1920-1940), fraction with foreign name (1910-1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-year fixed effects are partialled out.

Figure 6b: Merit systems do not affect patronage, RD graphs

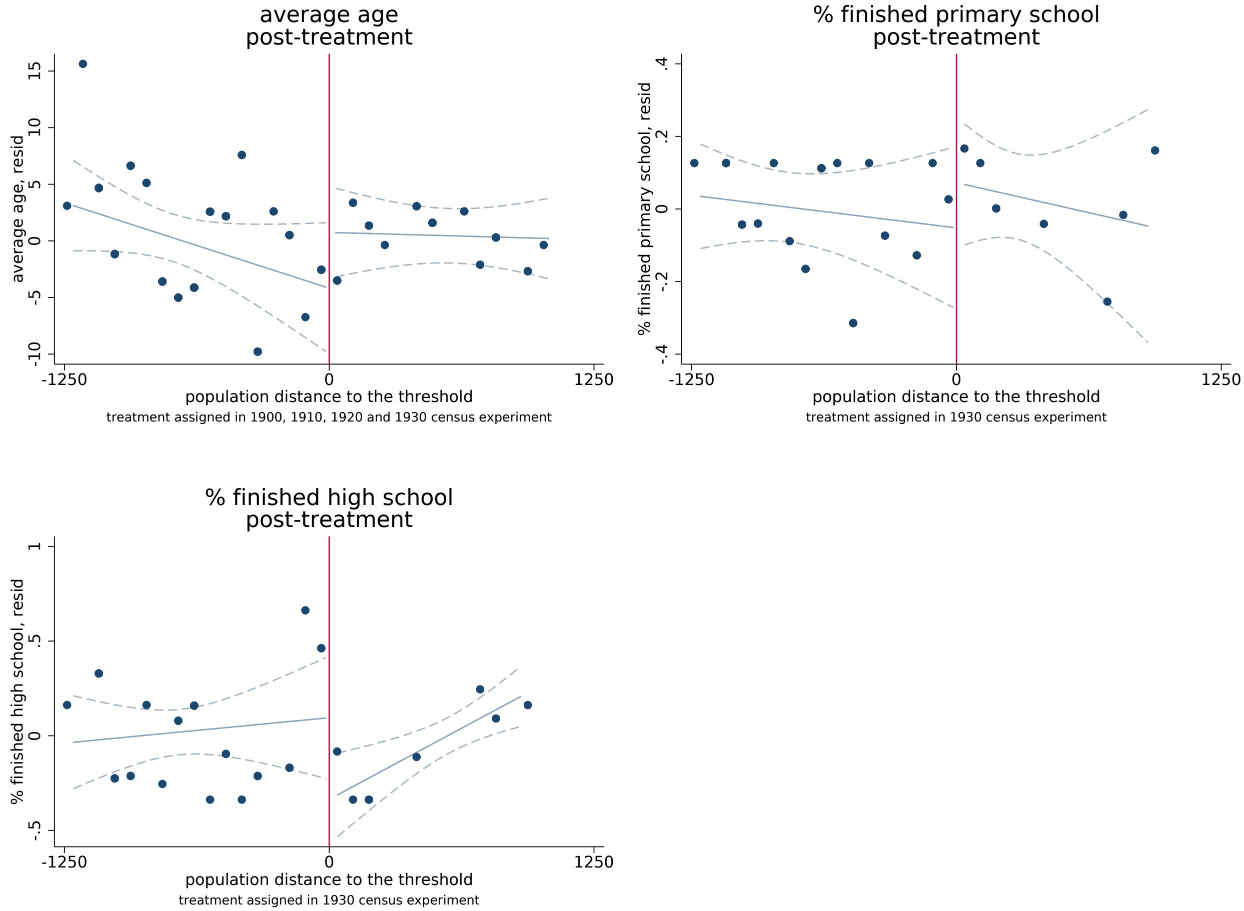






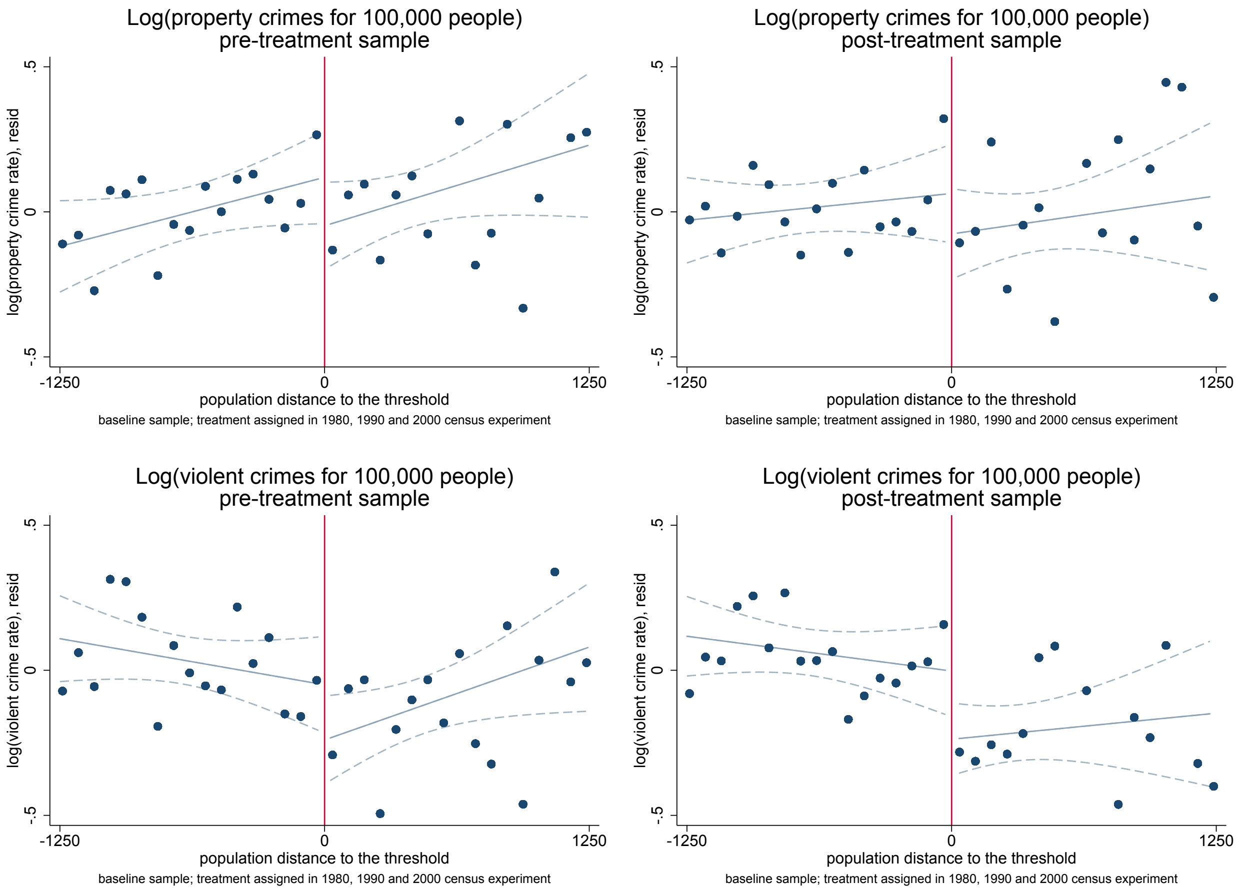
Notes: the graphs show the effect of merit system mandates on patronage for the sample of post-treatment years. The outcomes (and the census years for which they are available) are fraction co-ethnic with the mayor (1910-1930), fraction co-ethnic with the mayor based on their first names (1910-1940) and fraction belonging to the dominant ethnic group (1910-1930). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-year fixed effects are partialled out.

Figure 6c: Merit systems do not increase human capital, RD graphs



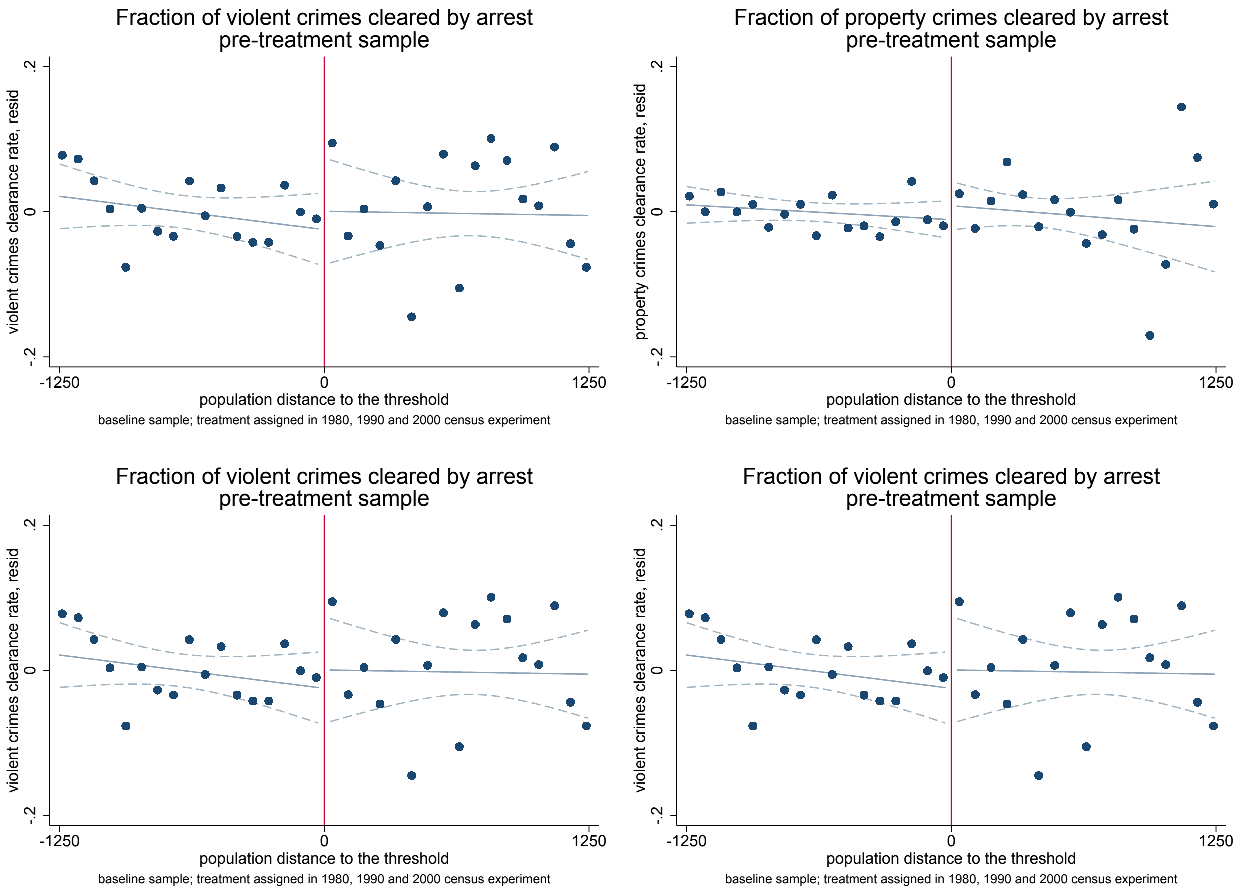
Notes: the graphs show the effect of merit system mandates on patronage for the sample of post-treatment years. The outcomes are average age (1910-1940), fraction with primary school education (1940) and fraction with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-year fixed effects are partialled out.

Figure 7a: Merit systems do not affect crime rates post-1980, RD graphs



Notes: the graphs show the post-1980 effect of merit system mandates on crime rates for the sample of pre-treatment years (on the left) and post-treatment years (on the right). Crime rates are crimes per 100,000 people. The sample exploits variation in treatment status from the 1980, 1990 and 2000 census experiment. Pre-treatment years span from the year of the previous census to the year before the census experiment for states with mandates based on the federal population census only and from the year of the previous census to three years before the census experiment for states with mandates based on federal, state or municipal census. Post-treatment years span from the year of the census experiment to the year before the following census for all states. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-month-census experiments fixed effects are partialled out.

Figure 7b: Merit systems do not affect clearance rates post-1980, RD graphs



Notes: the graphs show the post-1980 effect of merit system mandates on clearance rates for the sample of pre-treatment years (on the left) and post-treatment years (on the right). Clearance rates are number of crimes cleared by arrest over total number of crimes. The sample exploits variation in treatment status from the 1980, 1990 and 2000 census experiment. Pre-treatment years span from the year of the previous census to the year before the census experiment for states with mandates based on the federal population census only and from the year of the previous census to three years before the census experiment for states with mandates based on federal, state or municipal census. Post-treatment years span from the year of the census experiment to the year before the following census for all states. The points show the average value of the outcome within a 75 population distance bin; the line plots a linear fit estimated separately on each side of the discontinuity and prediction intervals that allow for clustering at the municipality level. State-month-census experiments fixed effects are partialled out.

Table 1: Population-based merit system mandates for police departments

state	reform year(s)	threshold	details
Arizona	1969	15,000	Mandate for cities and towns 15,000+ with more than 15 full-time employees in the police department introduced in 1969.
Illinois	1949 & 1951 & 1957	15,000 (13,000 & 5,000)	Possibility for cities and villages 7,000+ and 100'000- in 1903. Mandate for cities and villages 15,000+ introduced in 1949. Threshold lowered to 13,000 in 1951 and 5,000+ in 1957.
Iowa	1917	8,000	Mandate for cities 8,000+ introduced in 1917.
Louisiana	1944 & 1964	13,000 & 7,000	Mandate for cities 13,000+ introduced in 1944. Threshold lowered to 7,000 in 1964.
Montana	1907, 1947 & 1975	10,000 & 5,000 & 0	Mandate for cities of the first class (10,000+) introduced in 1907. Mandate extended to all cities of the second class (5,000+) in 1947. Mandate extended to all cities in 1975.
Nebraska	1957	5,000	Mandate for cities 5,000+ introduced in 1957.
West Virginia	1937 & 1969	5,000 & 10,000	Mandate for cities 5,000+ introduced in 1937. Threshold increased to 10'000 in 1969.
Wisconsin (cities)	1917	4,000	Mandate for cities 4,000+ introduced in 1917. Cities under city manager form of government not included in mandate before 1933.
Wisconsin (villages)	1941	5,500	Mandate for villages 5,500+ introduced in 1941.

Notes: the table summarized the information on legislation mandating merit systems state by state. When more than one year or more than one threshold is reported, the legislation was modified over time.

Table 2: Descriptive statistics for crime and clearance rates

Statistics	N	Mean	Sd
Panel (a): pre-treatment sample			
Property crime rate	6320	83.958	96.089
Violent crime rate	3369	11.295	28.585
Property crime clearance rate	3304	0.216	0.325
Violent crime clearance rate	822	0.706	0.410
Panel (b): post-treatment sample			
Property crime rate	9947	255.559	238.156
Violent crime rate	9947	29.811	52.020
Property crime clearance rate	9470	0.192	0.235
Violent crime clearance rate	4507	0.662	0.398

Notes: the table reports descriptive statistics for crime and clearance rates. Panel (a) reports summary statistics (number of observations, mean and standard deviation) for property and violent crime and clearance rates for the sample of pre-treatment year (1960-1969). Panel (b) reports summary statistics (number of observations, mean and standard deviation) for property and violent crime and clearance rates for the sample of post-treatment years (1970-1979). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes.

Table 3: Covariate balance test for 1970

Census year	1970			
	(1)	(2)	(3)	(4)
Population growth	0.047	0.201	-0.042	0.180
	(0.356)	(0.390)	(0.295)	(0.395)
Observations	90	114	138	68
Bandwidth	750	1000	1250	602
Male	-0.004	-0.001	-0.002	-0.005
	(0.007)	(0.006)	(0.005)	(0.007)
Observations	90	114	138	95
Bandwidth	750	1000	1250	794
Non-white	0.003	0.001	-0.001	0.004
	(0.035)	(0.031)	(0.026)	(0.037)
Observations	90	114	138	86
Bandwidth	750	1000	1250	725
Male 15 to 30	0.000	-0.007	-0.003	-0.010
	(0.023)	(0.020)	(0.017)	(0.026)
Observations	90	114	138	59
Bandwidth	750	1000	1250	537
Finished college	0.052	0.049	0.029	0.053
	(0.053)	(0.044)	(0.039)	(0.053)
Observations	90	114	138	88
Bandwidth	750	1000	1250	731
Unemployed	0.010	0.008	0.006	0.011
	(0.013)	(0.011)	(0.009)	(0.013)
Observations	90	114	138	83
Bandwidth	750	1000	1250	705
Below poverty line	0.038	0.032	0.037*	0.038
	(0.025)	(0.022)	(0.020)	(0.025)
Observations	90	114	138	90
Bandwidth	750	1000	1250	746
Median hh income	1,009.377	1,321.762	566.862	1,446.674
	(1,442.223)	(1,216.533)	(1,091.865)	(1,800.494)
Observations	90	114	138	62
Bandwidth	750	1000	1250	563
State FE	x	x	x	x

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the results of a covariate balance test. The table presents RD estimates on municipality characteristics at baseline for the samples of places to which treatment is assigned in the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. State fixed effects are included in all columns. Robust standard errors are shown in parentheses.

Table 4: Effect of merit system mandates on pre-1940 reform adoption

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Civil service board	0.185	0.096	0.183	0.190	0.334**	0.430**	0.437**	0.337*
	(0.151)	(0.159)	(0.138)	(0.183)	(0.168)	(0.177)	(0.171)	(0.198)
Observations	646	863	1060	595	572	747	902	481
Clusters	42	52	61	39	42	52	61	37
Bandwidth	750	1000	1250	713	750	1000	1250	651
Wild bootstrap p-value	[0.320]	[0.692]	[0.280]	[0.414]	[0.088]	[0.052]	[0.038]	[0.138]
State-year-census FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the pre-1940 first stage. It presents RD estimates on an indicator variable for whether a municipality has a civil service board for the sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Pre-treatment years span from the year of the previous census to the year before treatment is assigned. Post-treatment years span from the year in which treatment is assigned to the year before the following census. Variation in treatment status is from the 1900, 1910, 1920 and 1930 census experiments. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. Wild bootstrap p-values are shown in brackets.State-year-census experiment fixed effects are included in all columns.

Table 5: Effect of merit system mandates on crime and clearance rates

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.149	-0.056	0.059	-0.232	-0.587***	-0.461**	-0.394**	-0.628***
	(0.178)	(0.148)	(0.145)	(0.193)	(0.213)	(0.180)	(0.160)	(0.222)
Clusters	76	96	118	55	89	113	137	77
Observations	4476	5738	6994	3024	8891	11215	13589	7822
Bandwidth	750	1000	1250	557	750	1000	1250	666
Wild bootstrap p-value	[0.406]	[0.724]	[0.674]	[0.246]	[0.012]	[0.020]	[0.018]	[0.016]
Log(violent crime rate)	-0.251	-0.307	-0.107	-0.308	-0.030	0.027	0.091	-0.053
	(0.252)	(0.214)	(0.209)	(0.319)	(0.429)	(0.333)	(0.296)	(0.378)
Clusters	60	78	95	33	89	113	137	102
Observations	577	745	946	335	4402	5540	6542	5048
Bandwidth	750	1000	1250	464	750	1000	1250	858
Wild bootstrap p-value	[0.368]	[0.256]	[0.593]	[0.388]	[0.970]	[0.960]	[0.862]	[0.938]
Panel (b): clearance rates								
Property crime clearance rate	0.043	0.032	0.036	0.026	0.013	0.020	0.023	0.005
	(0.049)	(0.044)	(0.042)	(0.050)	(0.034)	(0.029)	(0.026)	(0.035)
Clusters	76	96	117	67	89	113	137	77
Observations	3090	4006	4852	2702	8891	11215	13589	7822
Bandwidth	750	1000	1250	672	750	1000	1250	672
Wild bootstrap p-value	[0.390]	[0.482]	[0.414]	[0.646]	[0.700]	[0.484]	[0.372]	[0.852]
Violent crime clearance rate	-0.193*	-0.152	-0.142	-0.171	0.123**	0.125***	0.098**	0.126**
	(0.108)	(0.096)	(0.095)	(0.124)	(0.052)	(0.047)	(0.048)	(0.055)
Clusters	60	78	95	38	89	113	137	79
Observations	577	745	946	385	4402	5540	6542	3971
Bandwidth	750	1000	1250	558	750	1000	1250	680
Wild bootstrap p-value	[0.076]	[0.122]	[0.146]	[0.180]	[0.038]	[0.018]	[0.066]	[0.040]
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the reduced form effect of merit systems on police performance. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. Wild bootstrap p-values are shown in brackets. State-month fixed effects are included in all columns.

Table 6a: Effect on crime and clearance rates, robustness to sample definitions and specifications

Sample	pre-treatment				post-treatment			
Specification	All years	Mandates based on federal census	1970 in pre-treatment	Controls	Mandates based on federal census	1970 in pre-treatment	Controls	Controls for baseline outcome
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.179	0.252	-0.056	-0.333**	-0.407	-0.463**	-0.668***	-0.438***
	(0.162)	(0.196)	(0.150)	(0.147)	(0.251)	(0.182)	(0.190)	(0.138)
Clusters	101	38	96	96	47	113	113	101
Observations	7302	3024	6127	5738	4798	10227	11215	10355
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Wild bootstrap p-value	[0.266]	[0.228]	[0.748]	[0.048]	[0.210]	[0.026]	[0.004]	[0.010]
Log(violent crime rate)	-0.300	-0.154	-0.420**	-0.172	-0.161	0.038	0.008	0.154
	(0.291)	(0.110)	(0.199)	(0.280)	(0.318)	(0.324)	(0.181)	(0.200)
Clusters	88	31	80	78	47	113	113	92
Observations	1325	356	847	745	1506	5202	5540	4874
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Wild bootstrap p-value	[0.336]	[0.220]	[0.064]	[0.596]	[0.730]	[0.954]	[0.932]	[0.570]
Panel (b): clearance rates								
Property crime clearance rate	0.031	0.101	0.034	0.044	0.031	0.022	0.019	0.022
	(0.039)	(0.064)	(0.043)	(0.047)	(0.057)	(0.030)	(0.032)	(0.030)
Clusters	101	38	96	96	47	113	113	101
Observations	5570	2289	4395	4006	4798	10227	11215	10355
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Wild bootstrap p-value	[0.420]	[0.150]	[0.464]	[0.302]	[0.644]	[0.478]	[0.514]	[0.498]
Violent crime clearance rate	-0.031	0.152**	-0.152*	-0.193	0.201*	0.133***	0.146***	0.159***
	(0.069)	(0.063)	(0.086)	(0.126)	(0.111)	(0.048)	(0.053)	(0.048)
Clusters	88	31	80	78	47	113	113	92
Observations	1325	356	847	745	1506	5202	5540	4874
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Wild bootstrap p-value	[0.740]	[0.062]	[0.064]	[0.182]	[0.144]	[0.008]	[0.026]	[0.004]
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows robustness of the main results to different sample definitions and inclusion of controls. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. Column 1 shows robustness to including 1967 to 1969 for states with mandates based on federal, state or municipal census. Columns 2 and 5 show robustness to restricting the sample to states with mandates based on the federal population census only for pre-treatment and post-treatment years respectively. Columns 3 and 6 show robustness to the inclusion of controls for pre-treatment and post-treatment years respectively. The controls included in the regression are percentage male, percentage non-white, percentage with high school degree, percentage unemployed, percentage below poverty line and median income according to the 1970 census. Column 8 additionally controls for the average value in the pre-period of the residuals from a regression of the outcome on state-month fixed effects for pre-treatment years. The coefficients are estimated using locally linear regression and a uniform kernel for a 1000 bandwidth. Standard errors clustered at the municipality level are shown in parentheses. Wild bootstrap p-values are shown in brackets. State-month fixed effects are included in all columns.

Table 6b: Effect on crime and clearance rates, robustness to other policies changing at the same threshold

Sample	post-treatment							
State being excluded	AZ	IL	IA	LA	MT	NE	WI CITY	WI VILL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.461**	-0.442**	-0.505***	-0.465**	-0.467**	-0.453**	-0.439**	-0.476***
	(0.180)	(0.225)	(0.191)	(0.182)	(0.182)	(0.192)	(0.214)	(0.184)
Clusters	113	60	101	103	108	105	91	110
Observations	11215	5957	9896	10552	10968	10303	8758	10856
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Wild bootstrap p-value	[0.024]	[0.046]	[0.010]	[0.014]	[0.022]	[0.020]	[0.058]	[0.026]
Log(violent crime rate)	0.027	-0.063	0.032	0.094	0.009	0.025	0.000	0.023
	(0.333)	(0.282)	(0.354)	(0.357)	(0.332)	(0.341)	(0.341)	(0.333)
Clusters	113	60	101	103	108	105	91	110
Observations	5540	1928	5028	5030	5474	5184	5096	5500
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Wild bootstrap p-value	[0.928]	[0.928]	[0.950]	[0.948]	[0.984]	[0.994]	[0.984]	[0.984]
Panel (b): clearance rates								
Property crime clearance rate	0.020	0.061	0.020	0.024	0.018	0.008	0.014	0.019
	(0.029)	(0.052)	(0.031)	(0.030)	(0.029)	(0.030)	(0.030)	(0.030)
Clusters	113	60	101	103	108	105	91	110
Observations	11215	5957	9896	10552	10968	10303	8758	10856
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Wild bootstrap p-value	[0.492]	[0.252]	[0.528]	[0.428]	[0.552]	[0.828]	[0.608]	[0.532]
Violent crime clearance rate	0.125***	0.247***	0.120**	0.125**	0.122***	0.118**	0.114**	0.124***
	(0.047)	(0.090)	(0.047)	(0.049)	(0.047)	(0.048)	(0.048)	(0.047)
Clusters	113	60	101	103	108	105	91	110
Observations	5540	1928	5028	5030	5474	5184	5096	5500
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Wild bootstrap p-value	[0.026]	[0.012]	[0.012]	[0.020]	[0.018]	[0.016]	[0.028]	[0.014]
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows that the results are not driven by any single state and thus do not depend on other state-specific laws also changing at the same threshold. The table presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of post-treatment years (columns 1 to 8), excluding one state at the time. Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for a 1000 bandwidth. Standard errors clustered at the municipality level are shown in parentheses. Wild bootstrap p-values are shown in brackets. State-month fixed effects are included in all columns.

Table 7: Effect of merit system mandates on expenditures and employment

Sample	post-treatment			
	(1)	(2)	(3)	(4)
Panel (a): expenditures				
Log(expenditures per 1,000 people)	-0.030	0.131	-0.034	0.020
	(0.208)	(0.186)	(0.163)	(0.202)
Clusters	89	113	137	95
Observations	492	632	753	531
Bandwidth	750	1000	1250	805
Wild bootstrap p-value	[0.878]	[0.510]	[0.892]	[0.952]
Log(payroll per 1,000 people)	0.062	0.167	-0.018	0.061
	(0.218)	(0.199)	(0.164)	(0.232)
Clusters	88	112	136	75
Observations	372	483	572	303
Bandwidth	750	1000	1250	649
Wild bootstrap p-value	[0.756]	[0.432]	[0.914]	[0.804]
Panel (b): employment				
Log(employment per 1,000 people)	-0.112	-0.018	-0.092	-0.028
	(0.231)	(0.204)	(0.169)	(0.212)
Clusters	88	112	136	107
Observations	372	483	572	460
Bandwidth	750	1000	1250	940
Wild bootstrap p-value	[0.712]	[0.990]	[0.608]	[0.920]
Log(officers per 1,000 people)	-0.044	0.016	0.061	-0.048
	(0.154)	(0.146)	(0.124)	(0.152)
Clusters	84	107	131	88
Observations	150	195	232	156
Bandwidth	750	1000	1250	771
Wild bootstrap p-value	[0.764]	[0.898]	[0.626]	[0.784]
State-month FE	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the effect of the merit system mandate on the resources available to the police department. The table presents RD estimates on expenditures (panel (a)) and employment (panel (b)) for the sample of post-treatment years (columns 1 to 4). Post-treatment years are 1970 to 1979 for expenditures, 1972 to 1979 for payroll expenditures and employment and 1977 to 1979 for officers. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. Wild bootstrap p-values are shown in brackets. State-month fixed effects are included in all columns.

Table 8: Effect of merit system mandates on demographic composition of police departments

Sample	post-treatment						
Individuals sample	All police officers				Low ranked	Young	New hires
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): ethnicity							
1st generation immigrant	-0.014	-0.086	-0.060	-0.061	-0.111	-0.102	-0.092
	(0.105)	(0.113)	(0.123)	(0.117)	(0.113)	(0.088)	(0.104)
Clusters	42	52	60	47	50	44	50
Observations	62	82	96	74	75	56	77
Bandwidth	750	1000	1250	915	1000	1000	1000
Wild bootstrap p-value	[0.902]	[0.480]	[0.656]	[0.618]	[0.306]	[0.192]	[0.382]
2nd generation immigrant	-0.454	-0.406	-0.389	-0.404	-0.377	0.091	-0.444
	(0.334)	(0.318)	(0.275)	(0.331)	(0.375)	(0.342)	(0.334)
Clusters	32	40	44	37	37	26	37
Observations	39	52	60	49	45	29	47
Bandwidth	750	1000	1250	981	1000	1000	1000
Wild bootstrap p-value	[0.166]	[0.180]	[0.142]	[0.188]	[0.374]	[0.828]	[0.174]
Foreign name	-0.124	0.082	-0.016	-0.296*	0.076	0.153	0.096
	(0.153)	(0.177)	(0.158)	(0.154)	(0.189)	(0.229)	(0.182)
Clusters	42	52	60	30	50	44	50
Observations	62	82	96	40	75	56	77
Bandwidth	750	1000	1250	572	1000	1000	1000
Wild bootstrap p-value	[0.372]	[0.714]	[0.878]	[0.048]	[0.672]	[0.564]	[0.620]
Panel (b): patronage							
Coethnic with mayor	-0.715**	-0.403	-0.220	-0.587*	-0.409	-0.007	-0.397
	(0.294)	(0.289)	(0.233)	(0.301)	(0.332)	(0.310)	(0.299)
Clusters	32	40	44	33	37	26	37
Observations	39	52	60	40	45	29	47
Bandwidth	750	1000	1250	763	1000	1000	1000
Wild bootstrap p-value	[0.040]	[0.186]	[0.360]	[0.096]	[0.286]	[0.960]	[0.220]
Coethnic with mayor (name)	-0.075	0.001	0.006	-0.026	0.044	0.086	0.004
	(0.072)	(0.075)	(0.046)	(0.067)	(0.066)	(0.097)	(0.072)
Clusters	42	52	60	44	50	44	50
Observations	62	82	96	69	75	56	77
Bandwidth	750	1000	1250	803	1000	1000	1000
Wild bootstrap p-value	[0.458]	[0.992]	[0.876]	[0.746]	[0.580]	[0.344]	[0.998]
Belongs to dominant immigrant	-0.149	-0.457	-0.533*	-0.373	-0.591	-0.135	-0.367
group	(0.270)	(0.338)	(0.294)	(0.323)	(0.377)	(0.427)	(0.318)
Clusters	32	40	44	36	37	26	37
Observations	39	52	60	46	45	29	47
Bandwidth	750	1000	1250	823	1000	1000	1000
Wild bootstrap p-value	[0.634]	[0.166]	[0.086]	[0.260]	[0.128]	[0.734]	[0.250]
Panel (c): human capital							
Age	0.385	2.170	4.145	1.810	5.057	5.067***	1.791
	(5.109)	(4.588)	(4.089)	(4.693)	(4.235)	(1.922)	(4.799)
Clusters	42	52	60	48	50	44	50
Observations	62	82	96	77	75	56	77
Bandwidth	750	1000	1250	946	1000	1000	1000
Wild bootstrap p-value	[0.912]	[0.686]	[0.322]	[0.692]	[0.232]	[0.022]	[0.728]

Finished primary school	0.260*	0.135	0.130	0.347*	0.173	0.204	0.117
	(0.154)	(0.160)	(0.151)	(0.204)	(0.221)	(0.194)	(0.169)
Clusters	23	30	36	19	30	27	30
Observations	23	30	36	19	30	27	30
Bandwidth	750	1000	1250	640	1000	1000	1000
Wild bootstrap p-value	[0.140]	[0.486]	[0.394]	[0.180]	[0.564]	[0.374]	[0.560]
Finished high school	-0.566**	-0.545***	-0.456**	-0.566**	-0.526***	-0.587**	-0.560***
	(0.241)	(0.195)	(0.186)	(0.241)	(0.202)	(0.241)	(0.197)
Clusters	23	30	36	23	30	27	30
Observations	23	30	36	23	30	27	30
Bandwidth	750	1000	1250	738	1000	1000	1000
Wild bootstrap p-value	[0.066]	[0.032]	[0.028]	[0.058]	[0.036]	[0.036]	[0.014]
State-census FE	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on demographic composition of police departments. It presents RD estimates on outcomes related to ethnicity (panel (a)), ethnic patronage (panel (b)) and the human capital of police officers (panel (c)) for the sample of post-treatment years (columns 1 to 7), specifically for all police officers (columns 1 to 4), low ranked police officers (column 5), young police officers (column 6) and newly hired police officers (column 7). The outcomes related to ethnicity (and the census years for which they are available) are fraction first generation immigrant (1910-1940), fraction second generation immigrant (1910-1930), fraction with foreign name (1910-1940). The outcomes related to ethnic patronage (and the census years for which they are available) are fraction co-ethnic with the mayor (1910-1930), fraction co-ethnic with the mayor based on their first names (1910-1940) and fraction belonging to the dominant ethnic group (1910-1930). The outcomes related to the human capital of police officers are average age (1910-1940), fraction with primary school education (1940) and fraction with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. Wild bootstrap p-values are shown in brackets. State-census year fixed effects are included in all columns.

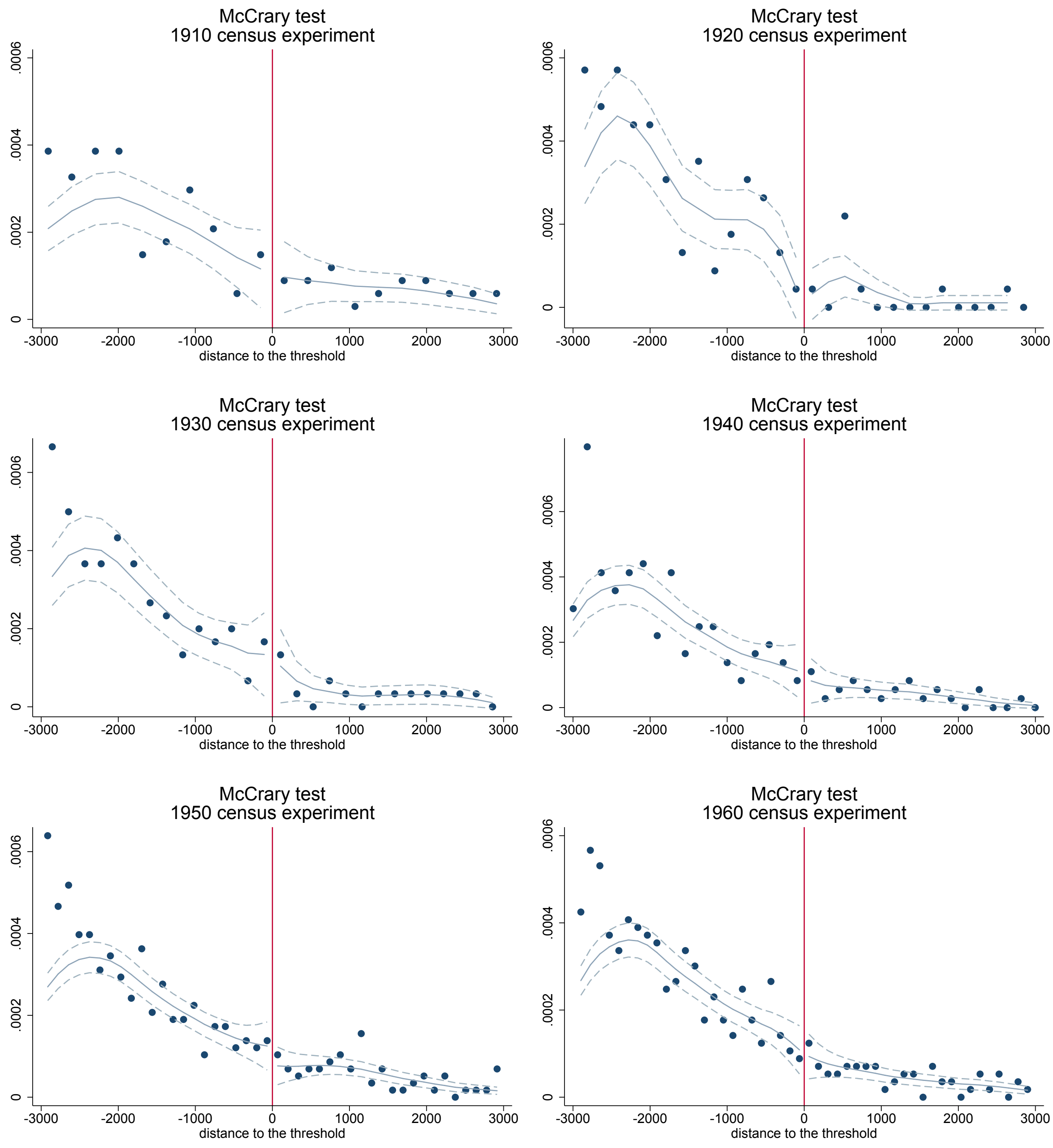
Table 9: Effect of merit system mandates on crime and clearance rates post-1980

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.175	-0.161	-0.224**	-0.465*	-0.199	-0.090	-0.133	-0.374*
	(0.136)	(0.131)	(0.114)	(0.241)	(0.167)	(0.154)	(0.124)	(0.193)
Clusters	123	154	190	56	112	143	174	58
Observations	13128	18222	22910	5627	13848	19531	24964	6933
Bandwidth	750	1000	1250	370	750	1000	1250	424
Wild bootstrap p-value	[0.208]	[0.220]	[0.046]	[0.078]	[0.276]	[0.542]	[0.320]	[0.114]
Log(violent crime rate)	-0.082	-0.100	-0.202*	0.064	-0.195	-0.130	-0.248**	-0.203
	(0.151)	(0.125)	(0.114)	(0.158)	(0.131)	(0.115)	(0.108)	(0.124)
Clusters	122	153	189	85	111	142	173	120
Observations	7292	10006	12760	4340	9913	13977	17873	10361
Bandwidth	750	1000	1250	544	750	1000	1250	781
Wild bootstrap p-value	[0.558]	[0.452]	[0.112]	[0.656]	[0.146]	[0.272]	[0.020]	[0.100]
Panel (b): clearance rates								
Property crime clearance rate	0.013	0.018	0.014	0.016	0.036	-0.001	0.001	0.018
	(0.029)	(0.026)	(0.022)	(0.028)	(0.034)	(0.030)	(0.024)	(0.045)
Clusters	123	154	190	138	112	143	174	57
Observations	13128	18222	22910	14808	13848	19531	24964	6816
Bandwidth	750	1000	1250	825	750	1000	1250	417
Wild bootstrap p-value	[0.626]	[0.520]	[0.570]	[0.658]	[0.338]	[0.996]	[0.964]	[0.724]
Violent crime clearance rate	-0.002	-0.039	0.021	0.035	-0.024	-0.022	-0.002	-0.014
	(0.081)	(0.067)	(0.056)	(0.093)	(0.070)	(0.057)	(0.045)	(0.090)
Clusters	122	153	189	101	111	142	173	73
Observations	7292	10006	12760	5394	9913	13977	17873	6075
Bandwidth	750	1000	1250	620	750	1000	1250	530
Wild bootstrap p-value	[0.950]	[0.562]	[0.730]	[0.804]	[0.754]	[0.686]	[0.988]	[0.840]
Census year-state-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the effect of the merit system mandate on police performance when there is no discontinuity in whether police officers are protected from patronage dismissals. The table presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years span from the year of the previous census to the year before the census experiment for states with mandates based on the federal population census only and from the year of the previous census to three years before the census experiment for states with mandates based on federal, state or municipal census. Post-treatment years span from the year of the census experiment to the year before the following census for all states. Variation in treatment status is from the 1980, 1990 and 2000 census experiments. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. Wild bootstrap p-values are shown in brackets. Census year-state-month fixed effects are included in all columns.

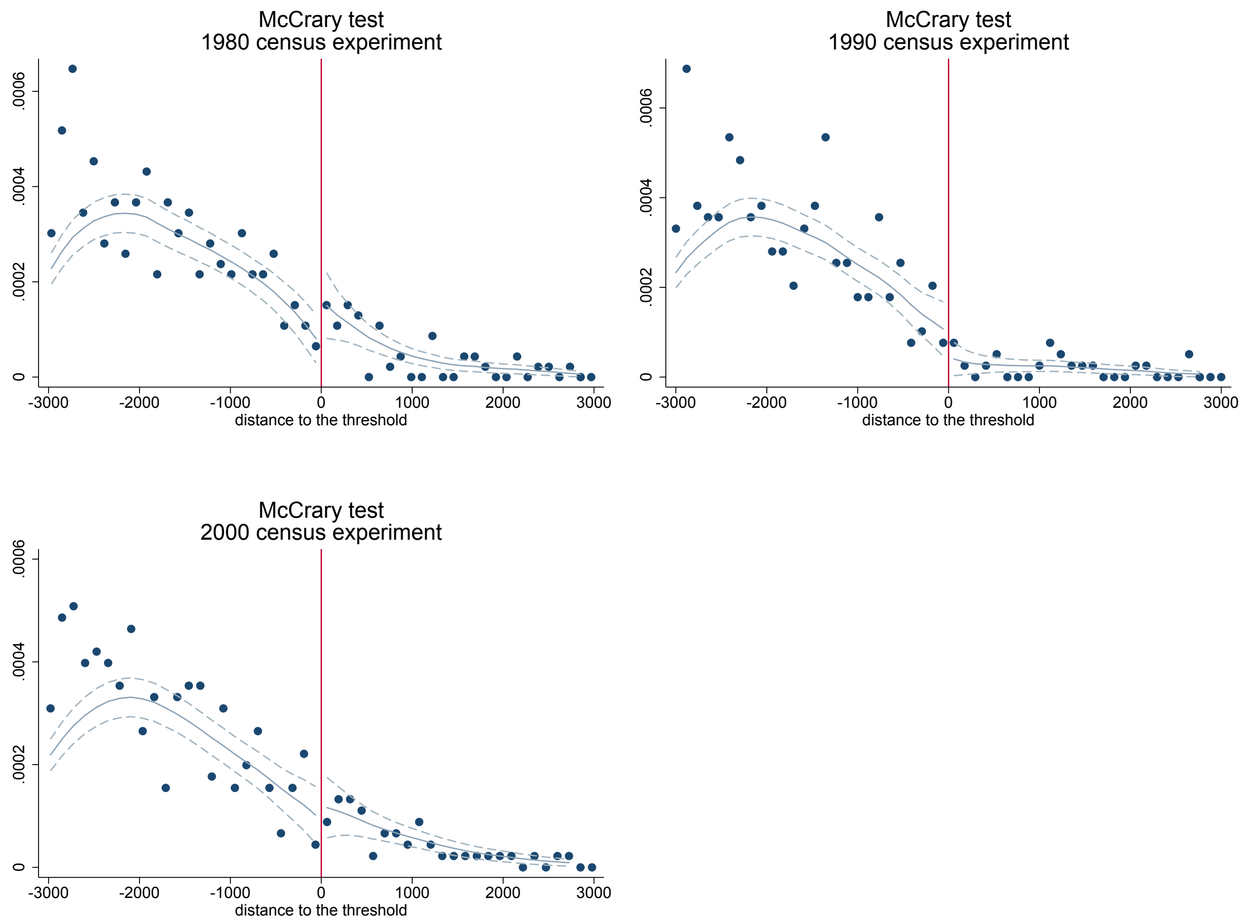
Appendix A - Figures

Figure A-1a: McCrary tests 1910-1960



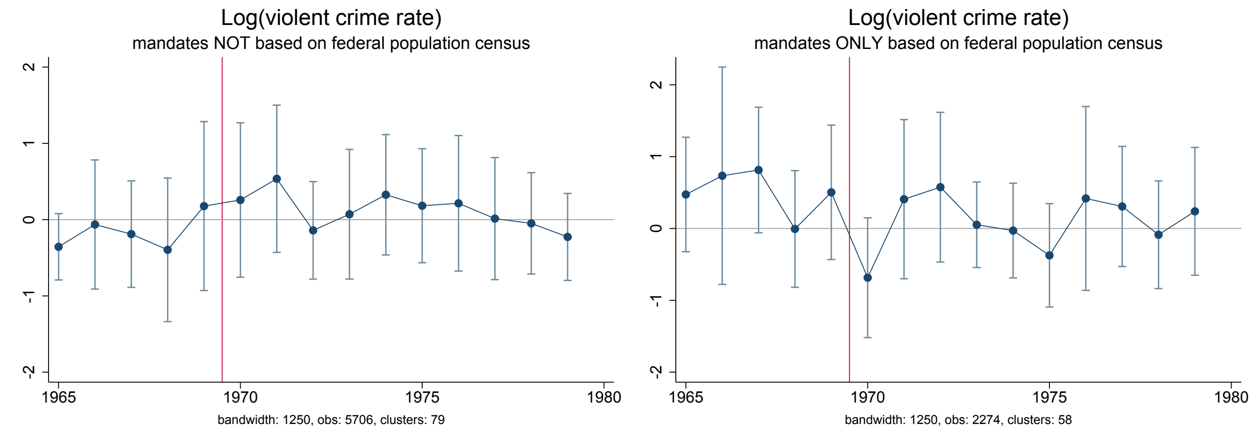
Notes: the graphs shows the McCrary (2008) test for the 1910, 1920, 1930, 1940, 1950 and 1960 census experiments.

Figure A-1b: McCrary tests 1980-2000



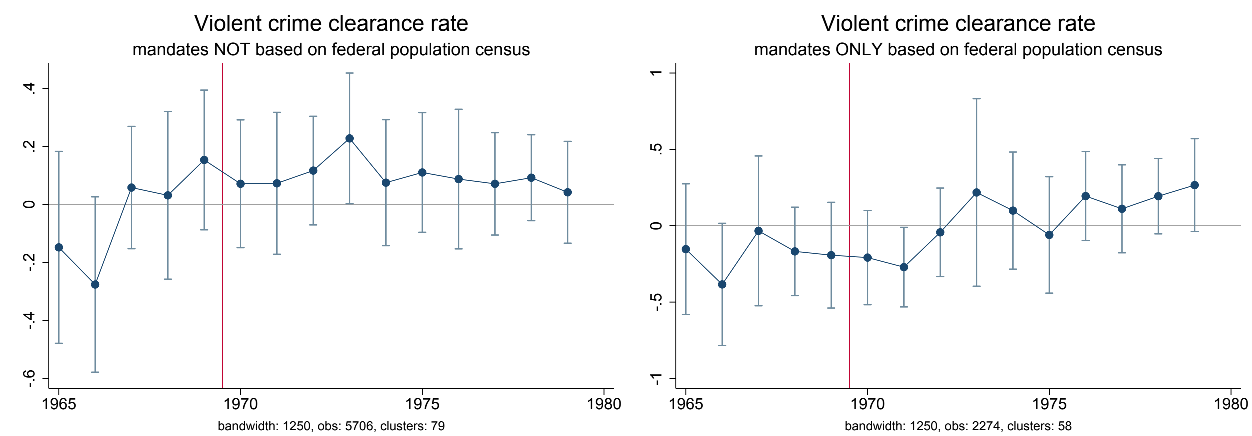
Notes: the graphs shows the McCrary (2008) test for the 1980, 1990 and 2000 census experiments.

Figure A-2: Effect on violent crime rates, event study graphs, separately for states with and without mandates explicitly based on federal population census



Notes: the graph shows the effect of merit system mandates on violent crime rates estimated using the event study specification (equation (2)) separately for states with and without mandates explicitly based on federal population census. Crime rates are crimes per 100,000 people. The sample exploits variation in treatment status from the 1970 census experiments. The sample includes both pre-treatment and post-treatment years and spans 1965 to 1979. The points are the point estimates β_σ from the event study specification with 95% confidence intervals. The coefficients are estimated using locally linear regression and a uniform kernel for a 1250 bandwidth. Standard errors are clustered at the municipality level. State-month fixed effects are included in all columns.

Figure A-3: Effect on property clearance rates, event study graphs, separately for states with and without mandates explicitly based on federal population census



Notes: the graph shows the effect of merit system mandates on property crime clearance rates estimated using the event study specification (equation (2)) separately for states with and without mandates explicitly based on federal population census. Clearance rates are number of crimes cleared by arrest over total number of crimes. The sample exploits variation in treatment status from the 1970 census experiments. The sample includes both pre-treatment and post-treatment years and spans 1965 to 1979. The points are the point estimates β_σ from the event study specification with 95% confidence intervals. The coefficients are estimated using locally linear regression and a uniform kernel for a 1250 bandwidth. Standard errors are clustered at the municipality level. State-month fixed effects are included in all columns.

Appendix B - Tables

Table B-1: Legislative provisions implying policy discontinuities at the same threshold

state	overlap with municipality classification	overlap with police legislation	details
Arizona	no	no	Other legislation: procedure to publish notice of bonds emission.
Illinois	no	yes	Police legislation: mimum salary. Other legislation: community nurses, parks, strong mayor form of government, arbitration procedure for firemen, pension fund for city employees (overlaps only for 2 years).
Iowa	no	no	Other legislation: appropriation of special funds on part of county to fund construction in certain cities.
Louisiana	no	no	-
Montana	yes	no	-
Nebraska	yes	yes	Police legislation: possibility to introduce pension funds for policemen. Other legislation: way of setting up a new charter.
West Virginia	yes	yes	Police legislation: pension and relief fund for policemen and firemen (after 1969 only). Other legislation: number of councilmen, incorporation procedure, bonds.
Wisconsin (cities)	no	no	-
Wisconsin (villages)	no	no	-

Table B-2a: Covariate balance tests for 1910, 1920, 1930 and 1940

Census year	1910				1920				1930				1940			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Male	-0.024	-0.055*	-0.044*	-0.044*	0.004	-0.001	-0.003	-0.001	0.005	0.006	0.006	0.006	0.009	0.002	-0.005	0.009
	(0.035)	(0.032)	(0.024)	(0.025)	(0.030)	(0.024)	(0.022)	(0.024)	(0.016)	(0.013)	(0.012)	(0.013)	(0.015)	(0.013)	(0.011)	(0.018)
Observations	18	25	35	29	22	27	30	27	23	30	36	30	32	36	50	16
Bandwidth	750	1000	1250	1071	750	1000	1250	999	750	1000	1250	1021	750	1000	1250	508
Age	5.805	5.492	2.688	6.440	4.927	5.009*	4.953**	5.009*	-0.438	-1.184	-0.905	-0.528	-2.402	-1.228	-0.819	-2.138
	(4.938)	(4.098)	(2.839)	(4.377)	(4.007)	(2.822)	(2.328)	(2.822)	(1.733)	(1.484)	(1.345)	(1.572)	(1.751)	(1.475)	(1.108)	(1.733)
Observations	18	25	35	20	22	27	30	27	23	30	36	16	32	36	50	30
Bandwidth	750	1000	1250	783	750	1000	1250	999	750	1000	1250	586	750	1000	1250	684
White	-0.002	-0.001	0.000	-0.001	0.004	0.000	0.001	0.000	-0.031	-0.038	-0.035	0.090**	-0.067	-0.040	-0.009	-0.071
	(0.005)	(0.004)	(0.005)	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)	(0.035)	(0.038)	(0.038)	(0.035)	(0.054)	(0.035)	(0.017)	(0.057)
Observations	18	25	35	23	22	27	30	27	23	30	36	12	32	36	50	34
Bandwidth	750	1000	1250	915	750	1000	1250	999	750	1000	1250	376	750	1000	1250	916
1st generation immigrant	-0.137*	-0.199**	-0.126**	-0.201**	-0.062	-0.006	-0.004	-0.006	-0.010	-0.019	-0.009	-0.033	-0.038	-0.045	-0.060**	-0.041
	(0.077)	(0.085)	(0.056)	(0.086)	(0.044)	(0.048)	(0.036)	(0.048)	(0.030)	(0.025)	(0.022)	(0.023)	(0.040)	(0.032)	(0.025)	(0.041)
Observations	18	25	35	24	22	27	30	27	23	30	36	43	32	36	50	31
Bandwidth	750	1000	1250	939	750	1000	1250	999	750	1000	1250	1404	750	1000	1250	746
2nd generation immigrant	-0.243	-0.285*	-0.190*	-0.196*	-0.220**	-0.108	-0.103	-0.108	0.007	-0.019	0.016	-0.017	-	-	-	-
	(0.174)	(0.158)	(0.114)	(0.116)	(0.088)	(0.091)	(0.069)	(0.091)	(0.071)	(0.061)	(0.059)	(0.080)	-	-	-	-
Observations	18	25	35	33	22	27	30	27	23	30	36	24	-	-	-	-
Bandwidth	750	1000	1250	1134	750	1000	1250	999	750	1000	1250	760	-	-	-	-
Fishined primary school	-	-	-	-	-	-	-	-	-	-	-	-	0.010	0.039	0.015	0.003
	-	-	-	-	-	-	-	-	-	-	-	-	(0.046)	(0.043)	(0.031)	(0.056)
Observations	-	-	-	-	-	-	-	-	-	-	-	-	32	36	50	25
Bandwidth	-	-	-	-	-	-	-	-	-	-	-	-	750	1000	1250	620
Finished high school	-	-	-	-	-	-	-	-	-	-	-	-	0.025	0.058	0.026	-0.007
	-	-	-	-	-	-	-	-	-	-	-	-	(0.047)	(0.048)	(0.033)	(0.064)
Observations	-	-	-	-	-	-	-	-	-	-	-	-	32	36	50	14
Bandwidth	-	-	-	-	-	-	-	-	-	-	-	-	750	1000	1250	446
State FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the results of a covariate balance test. The table presents RD estimates on municipality characteristics at baseline for the samples of places to which treatment is assigned in the 1910, 1920, 1930 and 1940 census experiments. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. State fixed effects are included in all columns. Robust standard errors are shown in parentheses.

Table B-2b: Covariate balance tests for 1980, 1990 and 2000

Census year	1980				1990				2000			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Population growth	-0.315	-0.095	-0.231	-0.314	-0.149	-0.147	-0.069	-0.147	-0.064	-0.008	0.057	-0.127
	(0.311)	(0.249)	(0.216)	(0.383)	(0.134)	(0.108)	(0.095)	(0.108)	(0.150)	(0.131)	(0.158)	(0.194)
Observations	75	104	132	58	45	65	88	65	61	83	112	26
Bandwidth	750	1000	1250	636	750	1000	1250	988	750	1000	1250	340
Male	0.002	0.004	0.002	-0.001	-0.003	0.000	0.005	0.001	-0.003	0.004	0.009	-0.006
	(0.010)	(0.008)	(0.007)	(0.009)	(0.015)	(0.014)	(0.011)	(0.015)	(0.012)	(0.012)	(0.011)	(0.020)
Observations	77	106	134	85	47	67	91	69	63	85	114	36
Bandwidth	750	1000	1250	832	750	1000	1250	1040	750	1000	1250	409
Non-white	-0.015	-0.004	-0.056*	-0.064***	-0.023	-0.087	-0.136	-0.058	-0.015	0.027	0.022	0.035
	(0.022)	(0.023)	(0.031)	(0.020)	(0.089)	(0.089)	(0.085)	(0.070)	(0.060)	(0.056)	(0.046)	(0.066)
Observations	77	106	134	37	47	67	91	34	63	85	114	35
Bandwidth	750	1000	1250	412	750	1000	1250	591	750	1000	1250	391
Male 15 to 30	0.009	0.009	0.006	0.007	-0.004	-0.007	-0.012*	-0.004	-0.006	-0.002	0.000	-0.018
	(0.010)	(0.008)	(0.007)	(0.010)	(0.008)	(0.008)	(0.006)	(0.008)	(0.008)	(0.007)	(0.006)	(0.012)
Observations	77	106	134	83	47	67	91	52	63	85	114	36
Bandwidth	750	1000	1250	817	750	1000	1250	791	750	1000	1250	417
Finished college	-0.038	-0.012	-0.025	-0.035	-0.038	-0.008	-0.010	0.018	-0.119**	-0.110**	-0.081	-0.087
	(0.045)	(0.041)	(0.031)	(0.052)	(0.092)	(0.086)	(0.062)	(0.096)	(0.056)	(0.054)	(0.050)	(0.070)
Observations	77	106	134	63	47	67	91	59	63	85	114	36
Bandwidth	750	1000	1250	641	750	1000	1250	860	750	1000	1250	400
Unemployed	0.014	0.000	-0.001	0.015	-0.015	-0.028**	-0.029**	-0.021	0.014	0.015*	0.011	0.024*
	(0.018)	(0.015)	(0.013)	(0.022)	(0.017)	(0.012)	(0.010)	(0.015)	(0.010)	(0.009)	(0.009)	(0.014)
Observations	77	106	134	52	47	67	91	50	63	85	114	41
Bandwidth	750	1000	1250	548	750	1000	1250	767	750	1000	1250	514
Below poverty line	-0.009	-0.015	-0.017	-0.009	-0.049	-0.076*	-0.072**	-0.077	0.005	0.013	0.004	0.016
	(0.019)	(0.016)	(0.015)	(0.016)	(0.046)	(0.044)	(0.035)	(0.047)	(0.018)	(0.016)	(0.014)	(0.025)
Observations	77	106	134	98	47	67	91	55	63	85	114	40
Bandwidth	750	1000	1250	922	750	1000	1250	806	750	1000	1250	488
Median hh income	11	2,319	341	-695	-7,325	-3,293	-2,521	-1,214	-12,311	-10,770*	-3,869	221
	(2,406)	(2,640)	(1,885)	(3,479)	(11,297)	(9,144)	(6,806)	(11,608)	(7,502)	(6,531)	(7,754)	(10,907)
Observations	77	106	134	48	47	67	91	54	63	85	114	29
Bandwidth	750	1000	1250	491	750	1000	1250	799	750	1000	1250	356
State FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the results of a covariate balance test. The table presents RD estimates on municipality characteristics at baseline for the samples of places to which treatment is assigned in the 1980, 1990 and 2000 census experiments. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. State fixed effects are included in all columns. Robust standard errors are shown in parentheses.

Table B-3: Years in pre-treatment and post-treatment samples for pre-1940 reform adoption analysis

census	state	reform	pre	post
1900	Montana	1907	1897 to 1906	1907 to 1909
	Ohio	1902	1892 to 1901	1902 to 1909
1910	Montana	census	1900 to 1909	1910 to 1919
	Ohio	census		
	Iowa	1917	1907 to 1916	1917 to 1919
	Wisconsin cities	1911	1901 to 1910	1911 to 1919
1920	Montana	census	1910 to 1919	1920 to 1929
	Ohio	census		
	Iowa	census		
	Wisconsin cities	census		
1930	Montana	census	1920 to 1929	1930 to 1939
	Ohio	census		
	Iowa	census		
	Wisconsin cities	census		
	West Virginia	1937	1927 to 1936	1937 to 1939
1940	Montana	census	1930 to 1939	1940 to 1943
	Ohio	census		
	Iowa	census		
	Wisconsin cities	census		
	West Virginia	census		
	Illinois	1949	1939 to 1948	1949 to 1949
	Louisiana	1947	1937 to 1946	1947 to 1949
	Wisconsin villages	1941	1931 to 1940	1941 to 1949

Table B-4: Effect of merit system mandates on pre-1943 reform adoption (includes the 1940 census experiment)

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Civil service board	0.128	0.068	0.098	0.120	0.288**	0.367**	0.299**	0.295*
	(0.107)	(0.115)	(0.087)	(0.119)	(0.142)	(0.152)	(0.136)	(0.159)
Clusters	51	59	72	47	51	59	72	46
Observations	886	1123	1440	815	668	851	1054	569
Bandwidth	750	1000	1250	713	750	1000	1250	651
State-year-census FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the pre-1943 first stage including variation in treatment status from the 1940 census experiment. The table presents RD estimates on an indicator variable for whether a municipality has a civil service board for the sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Pre-treatment years span from the year of the previous census to the year before treatment is assigned. Post-treatment years span from the year in which treatment is assigned to the year before the following census. Variation in treatment status is from the 1900, 1910, 1920, 1930 and 1940 census experiments. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. Census year-state-month fixed effects are included in all columns.

Table B-5: Effect of merit system mandates on reporting, crime and clearance rates for the 1960 census experiment

Sample	post-treatment				post-treatment (without 1960)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): reporting								
Monthly crime report missing	-0.040	-0.175**	-0.146**	-0.143	-0.061	-0.211**	-0.181**	-0.188**
	(0.101)	(0.157)	(0.156)	(0.201)	(0.221)	(0.192)	(0.197)	(0.193)
Clusters	77	107	136	91	77	107	136	98
Observations	8760	12300	15600	10440	7932	11124	14112	10152
Bandwidth	750	1000	1250	840	750	1000	1250	918
Panel (b): crime rates								
Log(property crime rate)	0.243	0.244	0.361**	0.551**	0.274	0.295	0.401**	0.541*
	(0.242)	(0.248)	(0.239)	(0.305)	(0.381)	(0.312)	(0.425)	(0.388)
Clusters	71	101	128	55	71	101	127	56
Observations	5595	8183	10069	4477	5146	7564	9295	4178
Bandwidth	750	1000	1250	569	750	1000	1250	591
Log(violent crime rate)	-0.225	-0.091	-0.016	0.041	-0.225	-0.091	-0.016	0.041
	(0.228)	(0.041)	(0.043)	(0.060)	(0.063)	(0.032)	(0.031)	(0.031)
Clusters	63	97	119	30	63	97	119	30
Observations	987	1502	1841	327	987	1502	1841	327
Bandwidth	750	1000	1250	397	750	1000	1250	397
Panel (c): clearance rates								
Property crime clearance rate	0.063	0.029	0.032	0.002	0.063	0.029	0.032	0.002
	(0.046)	(0.102)	(0.101)	(0.136)	(0.162)	(0.055)	(0.049)	(0.048)
Clusters	71	101	127	58	71	101	127	58
Observations	4223	6189	7573	3522	4223	6189	7573	3522
Bandwidth	750	1000	1250	627	750	1000	1250	627
Violent crime clearance rate	0.266	0.151	0.135	0.297	0.266	0.151	0.135	0.297
	(0.184)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Clusters	63	97	119	55	63	97	119	55
Observations	987	1502	1841	768	987	1502	1841	768
Bandwidth	750	1000	1250	636	750	1000	1250	636
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the effect of the merit system mandate on crime reporting and police performance for the 1960 census experiment. The table presents RD estimates on crime reporting (panel (a)), crime rates (panel (b)) and clearance rates (panel (c)) for the sample of post-treatment years including 1960 (columns 1 to 4) and post-treatment years excluding 1960 (columns 5 to 8). Monthly crime report missing is a dummy equal to one if the department did not submit a report for the month, crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Post-treatment years are 1960/1961 to 1969 for all states. Variation in treatment status is from the 1960 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table B-6a: Crime-by-crime effect of merit system mandates on property crime and clearance rates

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(burglary and vehicle theft rate)	0.035	0.053	0.073	0.068	-0.410*	-0.265	-0.220	-0.432**
	(0.122)	(0.100)	(0.093)	(0.146)	(0.218)	(0.181)	(0.158)	(0.206)
Clusters	76	96	118	52	89	113	137	95
Observations	2880	3754	4718	1769	7673	9615	11472	8167
Bandwidth	750	1000	1250	510	750	1000	1250	802
Log(larceny rate)	0.016	0.075	0.138	-0.251**	-0.570***	-0.457**	-0.380**	-0.627**
	(0.176)	(0.123)	(0.120)	(0.121)	(0.212)	(0.180)	(0.159)	(0.217)
Clusters	74	94	116	41	89	113	137	76
Observations	3724	4823	5847	1848	8640	10897	13148	7542
Bandwidth	750	1000	1250	443	750	1000	1250	644
Panel (b): clearance rates								
Burglary and vehicle theft clearance rate	0.022	0.051	0.068	0.054	0.055*	0.049**	0.061**	0.041
	(0.061)	(0.053)	(0.048)	(0.055)	(0.029)	(0.025)	(0.023)	(0.029)
Clusters	75	95	116	90	89	113	137	76
Observations	2065	2677	3333	2455	7673	9615	11472	6636
Bandwidth	750	1000	1250	952	750	1000	1250	655
Larceny clearance rate	0.055	0.030	0.037	0.015	-0.003	0.007	0.006	-0.007
	(0.056)	(0.052)	(0.048)	(0.066)	(0.041)	(0.034)	(0.031)	(0.044)
Clusters	73	93	114	53	89	113	137	65
Observations	2609	3438	4146	1863	8640	10897	13148	6459
Bandwidth	750	1000	1250	573	750	1000	1250	572
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the reduced form effect of merit systems on crime rates by crime type. It presents RD estimates on crime rates (panel (a)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table B-6b: Crime-by-crime effect of merit system mandates on violent crime and clearance rates

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): clearance rates								
Log(robbery rate)	0.053	-0.016	-0.032	0.040	0.144	0.158	0.269*	0.185
	(0.116)	(0.135)	(0.114)	(0.143)	(0.189)	(0.171)	(0.162)	(0.163)
Clusters	37	46	59	18	70	91	111	39
Observations	107	133	173	57	747	908	1004	505
Bandwidth	750	1000	1250	411	750	1000	1250	470
Log(assault rate)	-0.256	-0.339	-0.134	-0.212	0.047	0.077	0.131	0.047
	(0.276)	(0.240)	(0.223)	(0.339)	(0.430)	(0.334)	(0.297)	(0.430)
Clusters	54	73	88	31	88	112	136	87
Observations	494	637	805	331	4073	5142	6067	4066
Bandwidth	750	1000	1250	527	750	1000	1250	742
Panel (b): clearance rates								
Robbery clearance rate	-0.219	-0.023	0.036	-0.010	0.062	0.040	0.001	0.026
	(0.214)	(0.195)	(0.183)	(0.177)	(0.058)	(0.052)	(0.051)	(0.063)
Clusters	28	37	50	41	70	91	111	51
Observations	62	83	108	89	747	908	1004	658
Bandwidth	750	1000	1250	1052	750	1000	1250	602
Assault clearance rate	-0.163	-0.148	-0.130	-0.172	0.102*	0.102**	0.092*	0.059
	(0.136)	(0.120)	(0.114)	(0.134)	(0.053)	(0.048)	(0.050)	(0.055)
Clusters	54	73	88	48	88	112	136	68
Observations	494	637	805	458	4073	5142	6067	3207
Bandwidth	750	1000	1250	707	750	1000	1250	611
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the reduced form effect of merit systems on clearance rates by crime type. It presents RD estimates on clearance rates (panel (a)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table B-7: Effect of merit system mandates on reporting

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Monthly crime report missing	-0.018	-0.021	-0.039	-0.051	0.043	0.022	-0.001	0.031
	(0.130)	(0.113)	(0.104)	(0.107)	(0.055)	(0.042)	(0.040)	(0.045)
Clusters	90	114	138	126	90	114	138	103
Observations	8928	11304	13716	12528	10560	13380	16260	12120
Bandwidth	750	1000	1250	1105	750	1000	1250	858
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table presents RD estimates on crime reporting for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Monthly crime report missing is a dummy equal to one if the department did not submit a report for the month. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table B-8: Effect on crime and clearance rates, robustness to estimation

Sample	pre-treatment				post-treatment			
Polynomial	Linear	Linear	Quadratic	Cubic	Linear	Linear	Quadratic	Cubic
Kernel	Triangular	Epanech-nikov	Uniform	Uniform	Triangular	Epanech-nikov	Uniform	Uniform
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.166	-0.156	-0.293	-0.076	-0.558***	-0.548***	-0.640**	-0.451*
	(0.157)	(0.156)	(0.201)	(0.221)	(0.197)	(0.193)	(0.259)	(0.269)
Clusters	96	96	96	96	113	113	113	113
Observations	5738	5738	5738	5738	11215	11215	11215	11215
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Log(violent crime rate)	-0.293	-0.297	-0.290	-0.309	-0.004	-0.024	0.015	0.308
	(0.248)	(0.239)	(0.305)	(0.381)	(0.425)	(0.388)	(0.531)	(0.622)
Clusters	78	78	78	78	113	113	113	113
Observations	745	745	745	745	5540	5540	5540	5540
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Panel (b): clearance rates								
Property crime clearance rate	0.034	0.038	0.064	0.052	0.010	0.013	-0.002	0.002
	(0.041)	(0.043)	(0.060)	(0.063)	(0.031)	(0.031)	(0.037)	(0.038)
Clusters	96	96	96	96	113	113	113	113
Observations	4006	4006	4006	4006	11215	11215	11215	11215
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
Violent crime clearance rate	-0.202**	-0.190*	-0.256*	-0.228	0.104**	0.108**	0.096	0.098
	(0.102)	(0.101)	(0.136)	(0.162)	(0.049)	(0.048)	(0.066)	(0.081)
Clusters	78	78	78	78	113	113	113	113
Observations	745	745	745	745	5540	5540	5540	5540
Bandwidth	1000	1000	1000	1000	1000	1000	1000	1000
State-month FE	no	yes	yes	yes	no	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows robustness to different estimation techniques. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. Columns 1 and 5 and columns 2 and 6 are estimated using locally linear regression and a triangular kernel and an Epachnikov kernel respectively. They include state-month fixed effects. Columns 3 and 7 are estimated using locally quadratic regression and a uniform kernel and include state-month fixed effects. Columns 4 and 8 are estimated using locally cubic regression and a uniform kernel and include state-month fixed effects. All columns present estimates restricting to a 1000 bandwidth. Standard errors clustered at the municipality level are shown in parentheses.

Table B-9: Effect on crime rates, robustness to different outcome definitions

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates, levels								
Property crime rate	-24.617	-11.950	0.575	-20.928	-191.402**	-148.522**	-122.177**	-148.522**
	(19.743)	(16.089)	(16.089)	(20.861)	(78.338)	(64.077)	(52.614)	(64.077)
Clusters	76	96	118	55	89	113	137	113
Observations	5723	7349	9079	3874	9106	11576	14128	11576
Bandwidth	750	1000	1250	565	750	1000	1250	1002
Violent crime rate	-11.302*	-9.854*	-6.501	-11.823	4.666	8.270	9.186	3.139
	(6.534)	(5.297)	(4.780)	(9.203)	(33.055)	(26.591)	(23.719)	(16.793)
Clusters	71	91	112	50	89	113	137	265
Observations	3021	3949	4874	2051	9106	11576	14128	24782
Bandwidth	750	1000	1250	566	750	1000	1250	2270
Property crime rate, 0 is missing	-14.042	-3.825	8.423	6.326	-189.110**	-144.593**	-119.622**	-155.773**
	(20.214)	(16.185)	(15.791)	(24.485)	(79.309)	(64.527)	(53.187)	(70.203)
Clusters	76	96	118	59	89	113	137	108
Observations	5306	6783	8276	4001	8923	11269	13660	10756
Bandwidth	750	1000	1250	604	750	1000	1250	966
Violent crime rate, 0 is missing	-11.457	-9.905*	-6.259	-13.143	5.152	9.115	9.611	3.363
	(7.174)	(5.752)	(5.287)	(11.031)	(33.833)	(27.178)	(24.147)	(18.562)
Clusters	71	91	112	46	89	113	137	226
Observations	2604	3383	4071	1654	8923	11269	13660	20531
Bandwidth	750	1000	1250	502	750	1000	1250	1996
Panel (b): crime rates, log plus 1								
Log(property crime rate +1)	-0.857**	-0.642**	-0.438	-0.611	-0.751***	-0.645***	-0.545***	-0.684***
	(0.355)	(0.325)	(0.325)	(0.494)	(0.250)	(0.232)	(0.202)	(0.241)
Clusters	76	96	118	52	89	113	137	100
Observations	5723	7349	9079	3678	9106	11576	14128	10209
Bandwidth	750	1000	1250	521	750	1000	1250	836
Log(violent crime rate +1)	-0.631*	-0.562**	-0.440*	-0.645	-0.607	-0.368	-0.396	-0.406
	(0.322)	(0.265)	(0.244)	(0.398)	(0.449)	(0.386)	(0.350)	(0.375)
Clusters	71	91	112	51	89	113	137	127
Observations	3021	3949	4874	2087	9106	11576	14128	12995
Bandwidth	750	1000	1250	571	750	1000	1250	1144
Panel (c): crime counts, levels								
Property crimes	-0.795	-0.119	0.662	-0.036	-10.150**	-8.005**	-7.590**	-7.293**
	(1.088)	(0.845)	(0.868)	(1.265)	(4.082)	(3.515)	(3.042)	(3.100)
Clusters	76	96	118	56	89	113	137	133
Observations	5723	7349	9079	3946	9106	11576	14128	13678
Bandwidth	750	1000	1250	580	750	1000	1250	1182
Violent crimes	-0.505*	-0.460*	-0.284	-0.551	0.186	0.343	0.373	0.220
	(0.307)	(0.249)	(0.240)	(0.426)	(1.720)	(1.393)	(1.246)	(1.021)
Clusters	71	91	112	50	89	113	137	210
Observations	3021	3949	4874	2051	9106	11576	14128	20140
Bandwidth	750	1000	1250	558	750	1000	1250	1885
Property crimes, 0 is missing	-0.366	0.219	0.964	-1.009	-10.080**	-7.866**	-7.550**	-7.140**
	(1.127)	(0.869)	(0.863)	(1.178)	(4.141)	(3.550)	(3.087)	(3.267)
Clusters	76	96	118	66	89	113	137	127

Observations	5306	6783	8276	4514	8923	11269	13660	12593
Bandwidth	750	1000	1250	657	750	1000	1250	1137
Violent crimes, 0 is missing	-0.522	-0.471*	-0.281	-0.708	0.210	0.382	0.389	-0.352
	(0.338)	(0.272)	(0.267)	(0.515)	(1.760)	(1.423)	(1.268)	(1.006)
Clusters	71	91	112	43	89	113	137	233
Observations	2604	3383	4071	1511	8923	11269	13660	21129
Bandwidth	750	1000	1250	481	750	1000	1250	2037
Panel (d): crime counts, logs								
Log(property crimes)	-0.166	-0.083	0.054	-0.087	-0.626***	-0.530***	-0.490***	-0.696***
	(0.182)	(0.150)	(0.152)	(0.242)	(0.215)	(0.187)	(0.168)	(0.228)
Clusters	76	96	118	56	89	113	137	79
Observations	4476	5738	6994	3096	8891	11215	13589	7948
Bandwidth	750	1000	1250	572	750	1000	1250	690
Log(violent crimes)	-0.234	-0.280	-0.089	-0.342	-0.049	-0.027	0.005	-0.127
	(0.250)	(0.211)	(0.210)	(0.309)	(0.428)	(0.336)	(0.297)	(0.382)
Clusters	60	78	95	33	89	113	137	104
Observations	577	745	946	335	4402	5540	6542	5110
Bandwidth	750	1000	1250	481	750	1000	1250	891
Panel (e): crime counts, logs + 1								
Log(property crimes +1)	-0.333*	-0.224	-0.089	-0.194	-0.615***	-0.534***	-0.478***	-0.554***
	(0.174)	(0.152)	(0.157)	(0.201)	(0.203)	(0.182)	(0.161)	(0.190)
Clusters	76	96	118	60	89	113	137	103
Observations	5723	7349	9079	4314	9106	11576	14128	10503
Bandwidth	750	1000	1250	618	750	1000	1250	876
Log(violent crimes +1)	-0.200*	-0.181**	-0.130	-0.206	-0.190	-0.103	-0.102	-0.129
	(0.112)	(0.091)	(0.084)	(0.156)	(0.244)	(0.205)	(0.185)	(0.166)
Clusters	71	91	112	47	89	113	137	187
Observations	3021	3949	4874	1990	9106	11576	14128	18063
Bandwidth	750	1000	1250	526	750	1000	1250	1681
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows that results are robust to different ways of defining the crime outcomes. It presents RD estimates on crime rates in levels (panel (a)), crime rates plus 1 in logs (panel (b)), crime counts in levels (panel (c)), crime counts in logs (panel (d)) and crime counts in log plus 1 (panel (e)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table B-10: Descriptive statistics for employment and expenditures

Statistics	N	Mean	Sd
Panel (a): post-treatment sample			
Employment per 1,000 people	507	26.645	25.577
Payroll per 1,000 people	381	22.353	15.838
Police employees per 1,000 people	381	2.681	1.235
Officers per 1,000 people	157	2.067	0.840

Notes: This table reports descriptive statistics for expenditures and employment. Panel (a) reports summary statistics (number of observations, mean and standard deviation) for exployment, payroll, police employees and officers per 1,000 people for the baseline sample of post-treatment. Post-treatment years are 1970 to 1979 for expenditures, 1972 to 1979 for payroll expenditures and employment and 1977 to 1979 for officers.

Table B-11: Effect on expenditures and employment, robustness to different outcome definitions

Sample	post-treatment			
	(1)	(2)	(3)	(4)
Panel (a): expenditures and employment, levels				
Expenditures per 1,000 people	-3.978	1.865	-1.182	1.427
	(9.786)	(7.904)	(6.419)	(7.895)
Clusters	89	113	137	114
Observations	492	632	753	635
Bandwidth	750	1000	1250	1021
Payroll per 1,000 people	-1.167	2.151	-0.834	-0.479
	(6.409)	(5.467)	(4.386)	(6.188)
Clusters	88	112	136	93
Observations	372	483	572	394
Bandwidth	750	1000	1250	793
Police employees per 1,000 people	-0.330	-0.050	-0.192	-0.142
	(0.597)	(0.515)	(0.430)	(0.525)
Clusters	88	112	136	109
Observations	372	483	572	470
Bandwidth	750	1000	1250	982
Officers per 1,000 people	-0.161	-0.029	0.102	-0.084
	(0.390)	(0.343)	(0.289)	(0.379)
Clusters	84	107	131	95
Observations	150	195	232	173
Bandwidth	750	1000	1250	847
Panel (b): expenditures and employment totals, level				
Expenditures	-24.422	3.757	-28.987	11.267
	(48.033)	(40.147)	(34.849)	(61.150)
Clusters	89	113	137	64
Observations	492	632	753	333
Bandwidth	750	1000	1250	577
Payroll expenditures	-6.291	9.119	-21.536	21.623
	(32.913)	(29.580)	(25.809)	(40.761)
Clusters	88	112	136	68
Observations	372	483	572	271
Bandwidth	750	1000	1250	608
Police employees	-1.631	-0.468	-2.645	-1.978
	(3.178)	(2.767)	(2.440)	(3.460)
Clusters	88	112	136	81
Observations	372	483	572	334
Bandwidth	750	1000	1250	706
Officers	-0.974	-0.380	-0.730	-0.826
	(1.963)	(1.818)	(1.592)	(2.127)
Clusters	84	107	131	77
Observations	150	195	232	135
Bandwidth	750	1000	1250	701
Panel (c): expenditures and employment totals, log				
Log (expenditures)	-0.080	0.041	-0.155	-0.004
	(0.217)	(0.193)	(0.175)	(0.260)

Clusters	89	113	137	68
Observations	492	632	753	353
Bandwidth	750	1000	1250	605
Log (payroll expenditures)	0.005	0.071	-0.151	0.038
	(0.229)	(0.206)	(0.177)	(0.247)
Clusters	88	112	136	75
Observations	372	483	572	303
Bandwidth	750	1000	1250	659
Log(employment)	-0.168	-0.114	-0.224	-0.151
	(0.240)	(0.209)	(0.178)	(0.233)
Clusters	88	112	136	99
Observations	372	483	572	429
Bandwidth	750	1000	1250	841
Log(officers employment)	-0.114	-0.100	-0.104	-0.063
	(0.160)	(0.149)	(0.131)	(0.170)
Clusters	84	107	131	73
Observations	150	195	232	126
Bandwidth	750	1000	1250	665
State-month FE	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows that results on resources available to the police department are robust to different ways of defining the expenditure and employment outcomes. The table presents RD estimates onexpenditures and employment per 1000 people in levels (panel (a)), expenditures and employment totals in levels (panel (b)) and expenditures and employment totals in logs (panel (c)) for the sample of post-treatment years (columns 1 to 4). Post-treatment years are 1970 to 1979 for expenditures, 1972 to 1979 for payroll expenditures and employment and 1977 to 1979 for officers. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table B-12: Descriptive statistics for police officers 1910-1940

Census year	1910	1920	1930	1940
	(1)	(2)	(3)	(4)
Panel (a): information on sample				
Experiment year	1900	1910	1920	1930
States	MT	IA, MT, WI	IA, MT, WI	IA, MT, WI, WV
Municipalities	2	74	88	127
Policemen	17	186	201	377
New hires	-	156	192	354
Panel (b): descriptive statistics				
Policemen	7	81	161	288
White	1.000	1.000	1.000	1.000
	(.)	(.)	(.)	(.)
Age	38.714	47.741	47.596	44.573
	(6.873)	(11.822)	(11.86)	(9.844)
1st generation immigrant	0.714	0.136	0.087	0.052
	(.488)	(.345)	(.283)	(.223)
2nd generation immigrant	0.857	0.630	0.571	.
	(.378)	(.486)	(.496)	.
Belongs to dominant ethnic group	0.571	0.309	0.236	.
	(.535)	(.465)	(.426)	.
Foreign name	0.429	0.420	0.373	0.278
	(.535)	(.497)	(.485)	(.449)
Coethnic with the mayor	0.714	0.148	0.143	.
	(.488)	(.357)	(.351)	.
Coethnic with the mayor (name)	0.143	0.037	0.043	0.031
	(.378)	(.19)	(.205)	(.174)
Finished primary school	.	.	.	0.813
	.	.	.	(.39)
Finished high school	.	.	.	0.225
	.	.	.	(.419)
Highest grade	.	.	.	8.947
	.	.	.	(2.612)

Notes: This table reports descriptive statistics for policemen characteristcs. Each column reporting information for a specific census. The census year reported at the top of the column refers to when the outcomes are measured; variation in treatment status is from the census experiment ten year prior. Panel (a) reports the states in the sample, the number of municipalities, the number of police officers and the number of newly hired police officers. Panel (b) reports mean and standard deviation for the police officers in municipalities in the control groups and within a 3000 population bandwidth.

Table B-13: Effect on demographic composition of police departments, additional outcomes

Sample	post-treatment						
Individuals sample	All police officers				Low ranked	Young	New hires
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): ethnicity							
1st generation immigrant, non-English speaking	-0.028	-0.112	-0.130	-0.024	-0.134	-0.088	-0.121
	(0.065)	(0.086)	(0.087)	(0.065)	(0.090)	(0.086)	(0.080)
Clusters	42	52	60	42	50	44	50
Observations	62	82	96	61	75	56	77
Bandwidth	750	1000	1250	741	1000	1000	1000
2nd generation immigrant, non-English speaking	-0.236	-0.406	-0.398	-0.353	-0.552	-0.351	-0.423
	(0.292)	(0.317)	(0.291)	(0.329)	(0.356)	(0.467)	(0.306)
Clusters	32	40	44	36	37	26	37
Observations	39	52	60	47	45	29	47
Bandwidth	750	1000	1250	882	1000	1000	1000
Index (1st generation, ethnic name)	-0.276	-0.081	-0.196	-0.275	-0.140	-0.125	-0.069
	(0.419)	(0.432)	(0.494)	(0.464)	(0.451)	(0.610)	(0.434)
Clusters	42	52	60	30	50	44	50
Observations	62	82	96	40	75	56	77
Bandwidth	750	1000	1250	547	1000	1000	1000
Index (1st generation, 2ns generation, ethnic name)	-0.863	-0.545	-0.678	-0.741	-0.692	-0.663	-0.679
	(0.956)	(0.919)	(0.963)	(1.049)	(1.007)	(1.457)	(1.023)
Clusters	32	40	44	30	37	26	37
Observations	39	52	60	36	45	29	47
Bandwidth	750	1000	1250	700	1000	1000	1000
Panel (b): patronage							
Index (co-ethnic with mayor, co-ethnic name, dominant group)	-0.579	-1.110	-1.124*	-0.831	-1.373	-0.479	-0.950
	(0.691)	(0.796)	(0.605)	(0.879)	(0.963)	(1.532)	(0.736)
Clusters	32	40	44	29	37	26	37
Observations	39	52	60	35	45	29	47
Bandwidth	750	1000	1250	683	1000	1000	1000
State-census FE	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on additional outcomes related to the demographic composition of police departments. It presents RD estimates on outcomes related to ethnicity (panel (a)) and ethnic patronage (panel (b)) for the sample of post-treatment years (columns 1 to 7), specifically for all policemen (columns 1 to 4), low ranked police officers (column 5), young police officers (column 6) and newly hired police officers (column 7). The outcomes related to ethnicity (and the census years for which they are available) are fraction first generation immigrant from non-English speaking countries (1910-1940), fraction second generation immigrant from non-English speaking countries (1910-1930), a standardized index based on fraction first generation immigrant and fraction with foreign name (1910-1940) and a standardized index based on fraction first generation immigrant, second generation immigrant and fraction with foreign name (1910-1930). The outcome related to ethnic patronage (and the census years for which they are available) is a standardized index based on co-ethnicity with the mayor, co-ethnicity with the mayor based on first name and fraction belonging to the dominant ethnic group (1910-1930). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table B-14: Effect on demographic composition of police departments, placebo tests

Sample	pre-treatment				post-treatment			
Individuals sample	All police officers				Other municipal workers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): ethnicity								
1st generation immigrant	-0.064	-0.067	-0.011	-0.178	-0.018	0.034	0.014	0.003
	(0.094)	(0.079)	(0.071)	(0.162)	(0.052)	(0.057)	(0.059)	(0.055)
Clusters	57	66	83	37	41	52	60	46
Observations	90	110	142	50	62	83	100	73
Bandwidth	750	1000	1250	493	750	1000	1250	894
2nd generation immigrant	-0.259	-0.315	-0.397**	-0.346	0.006	0.040	-0.016	-0.074
	(0.241)	(0.215)	(0.198)	(0.216)	(0.135)	(0.137)	(0.117)	(0.145)
Clusters	40	48	59	50	32	40	46	29
Observations	58	74	92	76	40	54	67	36
Bandwidth	750	1000	1250	1018	750	1000	1250	688
Foreign name	-0.077	-0.068	-0.147*	-0.103	-0.011	0.002	-0.001	0.077
	(0.111)	(0.093)	(0.078)	(0.097)	(0.096)	(0.085)	(0.085)	(0.119)
Clusters	57	66	83	61	41	52	60	29
Observations	90	110	142	100	62	83	100	40
Bandwidth	750	1000	1250	882	750	1000	1250	564
Panel (b): patronage								
Coethnic with mayor	-0.310	-0.296	-0.174	-0.302	-0.303*	-0.136	-0.076	-0.303*
	(0.278)	(0.236)	(0.206)	(0.260)	(0.170)	(0.169)	(0.141)	(0.170)
Clusters	40	48	59	43	32	40	46	32
Observations	58	74	92	66	40	54	67	40
Bandwidth	750	1000	1250	885	750	1000	1250	755
Coethnic with mayor (name)	-0.061	-0.036	-0.050	-0.072	-0.014	0.002	-0.008	-0.030
	(0.059)	(0.056)	(0.048)	(0.051)	(0.039)	(0.033)	(0.034)	(0.053)
Clusters	57	66	83	74	41	52	60	30
Observations	90	110	142	126	62	83	100	42
Bandwidth	750	1000	1250	1084	750	1000	1250	585
Belongs to dominant immigrant	-0.070	-0.237	-0.114	-0.198	-0.060	-0.117	-0.072	-0.090
group	(0.220)	(0.201)	(0.202)	(0.209)	(0.094)	(0.108)	(0.092)	(0.110)
Clusters	40	48	59	43	32	40	46	29
Observations	58	74	92	66	40	54	67	36
Bandwidth	750	1000	1250	888	750	1000	1250	669
Panel (c): human capital								
Age	-5.478	-3.786	-3.216	-6.180	-1.070	2.352	1.926	-2.346
	(3.863)	(3.374)	(2.487)	(4.540)	(2.932)	(2.736)	(2.162)	(3.896)
Clusters	57	66	83	47	41	52	60	31
Observations	90	110	142	70	62	83	100	44
Bandwidth	750	1000	1250	630	750	1000	1250	605
Finished primary school	-0.082	0.010	-0.032	-0.069	0.051	0.001	0.007	0.030
	(0.240)	(0.217)	(0.152)	(0.239)	(0.071)	(0.068)	(0.060)	(0.050)
Clusters	32	36	50	33	22	29	33	40
Observations	32	36	50	33	22	29	33	40
Bandwidth	750	1000	1250	794	750	1000	1250	1425
Finished high school	0.078	0.172	-0.036	0.120	0.087	0.025	0.061	-0.169
	(0.274)	(0.271)	(0.200)	(0.275)	(0.143)	(0.124)	(0.117)	(0.241)

Clusters	32	36	50	28	22	29	33	14
Observations	32	36	50	28	22	29	33	14
Bandwidth	750	1000	1250	667	750	1000	1250	511
State-census FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on demographic composition of police departments. It presents RD estimates on outcomes related to ethnicity (panel (a)), ethnic patronage (panel (b)) and the human capital of police officers (panel (c)) for pre-treatment years and all police officers (columns 1 to 4) and for the sample of post-treatment years and other municipal workers (columns 5 to 8). The outcomes related to ethnicity (and the census years for which they are available) are fraction first generation immigrant (1910-1940), fraction second generation immigrant (1910-1930), fraction with foreign name (1910-1940). The outcomes related to ethnic patronage (and the census years for which they are available) are fraction co-ethnic with the mayor (1910-1930), fraction co-ethnic with the mayor based on their first names (1910-1940) and fraction belonging to the dominant ethnic group (1910-1930). The outcomes related to the human capital of police officers are average age (1910-1940), fraction with primary school education (1940) and fraction with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome (1910 to 1940 for the pre-treatment analysis). The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table B-15: Effect on organizational structure of police departments 1910-1940

Sample	post-treatment			
Individuals sample	All police officers			
	(1)	(2)	(3)	(4)
<u>Panel (a): turnover</u>				
Fraction employed at next census	-0.140	-0.012	0.214	0.108
	(0.197)	(0.201)	(0.163)	(0.157)
Clusters	32	40	44	19
Observations	39	52	60	22
Bandwidth	750	1000	1250	514
Fraction new hire	-0.042	-0.035	-0.169	0.084
	(0.117)	(0.103)	(0.117)	(0.141)
Clusters	42	51	59	27
Observations	62	80	94	35
Bandwidth	750	1000	1250	498
<u>Panel (b): income and wages</u>				
Income	-43.325	-124.031	-130.232	136.067
	(192.780)	(162.270)	(154.025)	(204.192)
Clusters	23	30	36	17
Observations	23	30	36	17
Bandwidth	750	1000	1250	608
Week wage	-1.044	-2.660	-3.217	-1.090
	(3.423)	(2.947)	(2.811)	(3.393)
Clusters	23	30	36	18
Observations	23	30	36	18
Bandwidth	750	1000	1250	628
Wage/age	-8.299	-10.107**	-9.951**	-6.777
	(5.703)	(4.650)	(4.295)	(5.650)
Clusters	23	30	36	18
Observations	23	30	36	18
Bandwidth	750	1000	1250	628
State-census FE	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on outcomes related to the organization of police departments. It presents RD estimates on outcomes related to turnover (panel (a)) and income (panel (b)) for the sample of post-treatment years for all police officers (columns 1 to 4). The outcomes related to income (and the census years for which they are available) are income, weekly wage and wage over age (1940). The outcomes related to turnover (and the census years for which they are available) are fraction of police officers still employed by the police department at the next census (1910-1930) and fraction of police officers who are new hires (1920 to 1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table B-16: Effect of merit system mandates on reporting post-1980

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Monthly crime report missing	0.064	0.076	0.068	0.017	0.019	0.022	0.031	0.022
	(0.057)	(0.052)	(0.045)	(0.060)	(0.029)	(0.031)	(0.030)	(0.033)
Clusters	125	158	195	101	125	158	195	82
Observations	17592	24672	32232	13344	21120	29640	39000	12600
Bandwidth	750	1000	1250	618	750	1000	1250	506
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table presents RD estimates on crime reporting for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Monthly crime report missing is a dummy equal to one if the department did not submit a report for the month. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Civil Service Reforms:
Evidence from U.S. Police Departments

Arianna Ornaghi

November 10, 2016

ONLINE APPENDIX

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Appendix C - Additional tables

Table C-1: Effect on crime and clearance rates, robustness to full pre-treatment sample, all bandwidths

Sample	pre-treatment			
	(1)	(2)	(3)	(4)
Panel (a): crime rates				
Log(property crime rate)	-0.293	-0.179	-0.034	-0.098
	(0.189)	(0.162)	(0.157)	(0.232)
Clusters	80	101	123	59
Observations	5715	7302	8790	4113
Bandwidth	750	1000	1250	583
Log(violent crime rate)	-0.254	-0.300	-0.106	-0.256
	(0.350)	(0.291)	(0.271)	(0.356)
Clusters	67	88	108	55
Observations	1059	1325	1624	892
Bandwidth	750	1000	1250	660
Panel (b): clearance rates				
Property crime clearance rate	0.036	0.031	0.034	0.037
	(0.041)	(0.039)	(0.038)	(0.041)
Clusters	80	101	122	56
Observations	4329	5570	6648	2989
Bandwidth	750	1000	1250	556
Violent crime clearance rate	-0.024	-0.031	-0.030	-0.012
	(0.077)	(0.069)	(0.067)	(0.077)
Clusters	67	88	108	38
Observations	1059	1325	1624	658
Bandwidth	750	1000	1250	493
State-month FE	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to to including 1967 to 1969 for states with mandates based on federal, state or municipal census. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-2: Effect on crime and clearance rates, robustness to restring to mandates explicitly based on the federal population census, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	0.207	0.252	0.315*	-0.103	-0.501*	-0.407	-0.288	-0.907***
	(0.232)	(0.196)	(0.183)	(0.361)	(0.302)	(0.251)	(0.220)	(0.265)
Clusters	30	38	48	10	37	47	58	17
Observations	2323	3024	3707	690	3735	4798	6013	1609
Bandwidth	750	1000	1250	260	750	1000	1250	332
Log(violent crime rate)	-0.147	-0.154	0.364	.	-0.264	-0.161	0.134	-0.355
	(0.130)	(0.110)	(0.338)	.	(0.377)	(0.318)	(0.325)	(0.427)
Clusters	23	31	39	.	37	47	58	29
Observations	260	356	474	.	1164	1506	1847	903
Bandwidth	750	1000	1250	.	750	1000	1250	617
Panel (b): clearance rates								
Property crime clearance rate	0.125*	0.101	0.107*	0.198*	0.021	0.031	0.032	0.005
	(0.070)	(0.064)	(0.059)	(0.108)	(0.069)	(0.057)	(0.049)	(0.067)
Clusters	30	38	48	20	37	47	58	29
Observations	1737	2289	2815	1151	3735	4798	6013	2950
Bandwidth	750	1000	1250	507	750	1000	1250	605
Violent crime clearance rate	0.141*	0.152**	-0.077	0.314***	0.215**	0.201*	0.099	0.391***
	(0.077)	(0.069)	(0.067)	(0.077)	(0.000)	(0.000)	(0.000)	(0.000)
Clusters	23	31	39	5	37	47	58	19
Observations	260	356	474	47	1164	1506	1847	551
Bandwidth	750	1000	1250	264	750	1000	1250	378
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables robustness to restricting the sample to states with mandates based on the federal population census only. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-3: Effect on crime and clearance rates, robustness to including 1970 in the pre-treatment sample, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.152	-0.056	0.069	-0.120	-0.592***	-0.463**	-0.402**	-0.670***
	(0.180)	(0.150)	(0.147)	(0.203)	(0.214)	(0.182)	(0.161)	(0.226)
Clusters	76	96	118	60	89	113	137	79
Observations	4783	6127	7466	3649	8091	10227	12424	7249
Bandwidth	750	1000	1250	613	750	1000	1250	682
Log(violent crime rate)	-0.338	-0.420**	-0.188	-0.325	-0.027	0.038	0.099	-0.039
	(0.227)	(0.199)	(0.202)	(0.305)	(0.420)	(0.324)	(0.289)	(0.368)
Clusters	62	80	97	38	89	113	137	102
Observations	660	847	1074	437	4122	5202	6134	4746
Bandwidth	750	1000	1250	504	750	1000	1250	859
Panel (b): clearance rates								
Property crime clearance rate	0.038	0.034	0.036	0.040	0.018	0.022	0.024	0.013
	(0.048)	(0.043)	(0.040)	(0.049)	(0.035)	(0.030)	(0.027)	(0.036)
Clusters	76	96	117	74	89	113	137	79
Observations	3397	4395	5324	3338	8091	10227	12424	7249
Bandwidth	750	1000	1250	728	750	1000	1250	690
Violent crime clearance rate	-0.192**	-0.152*	-0.144	-0.173	0.131**	0.133***	0.103**	0.072
	(0.098)	(0.086)	(0.087)	(0.115)	(0.054)	(0.048)	(0.049)	(0.056)
Clusters	62	80	97	41	89	113	137	67
Observations	660	847	1074	441	4122	5202	6134	3202
Bandwidth	750	1000	1250	554	750	1000	1250	594
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustenss to including 1970 in the pre-period. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1970 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1971 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-4a: Effect on crime and clearance rates, robustness to including municipality controls (including median HH income), all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.360**	-0.333**	-0.064	-0.571***	-0.658***	-0.668***	-0.465***	-0.636***
	(0.165)	(0.147)	(0.158)	(0.170)	(0.212)	(0.190)	(0.179)	(0.223)
Clusters	76	96	118	55	89	113	137	73
Observations	4476	5738	6994	3024	8891	11215	13589	7387
Bandwidth	750	1000	1250	557	750	1000	1250	632
Log(violent crime rate)	-0.138	-0.172	-0.047	-0.152	-0.026	0.008	0.029	-0.078
	(0.314)	(0.280)	(0.254)	(0.358)	(0.207)	(0.181)	(0.162)	(0.184)
Clusters	60	78	95	33	89	113	137	102
Observations	577	745	946	335	4402	5540	6542	5048
Bandwidth	750	1000	1250	475	750	1000	1250	858
Panel (b): clearance rates								
Property crime clearance rate	0.050	0.044	0.046	0.050	0.015	0.019	0.021	0.005
	(0.050)	(0.047)	(0.041)	(0.050)	(0.039)	(0.032)	(0.026)	(0.039)
Clusters	76	96	117	76	89	113	137	82
Observations	3090	4006	4852	3090	8891	11215	13589	8179
Bandwidth	750	1000	1250	752	750	1000	1250	703
Violent crime clearance rate	-0.254*	-0.193	-0.182*	-0.359***	0.135**	0.146***	0.099*	0.134*
	(0.136)	(0.126)	(0.106)	(0.125)	(0.068)	(0.053)	(0.050)	(0.072)
Clusters	60	78	95	38	89	113	137	79
Observations	577	745	946	385	4402	5540	6542	3971
Bandwidth	750	1000	1250	558	750	1000	1250	680
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to the inclusion of controls. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. The controls included in the regression are percentage male, percentage non-white, percentage with high school degree, percentage unemployed, percentage below poverty line and median income according to the 1970 census. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-4b: Effect on crime and clearance rates, robustness to including municipality controls (excluding median HH income), all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.151	-0.042	0.095	-0.347*	-0.559***	-0.436**	-0.331**	-0.582***
	(0.172)	(0.153)	(0.152)	(0.184)	(0.190)	(0.173)	(0.162)	(0.215)
Clusters	76	96	118	55	89	113	137	73
Observations	4476	5738	6994	3024	8891	11215	13589	7387
Bandwidth	750	1000	1250	557	750	1000	1250	632
Log(violent crime rate)	-0.183	-0.213	0.001	-0.127	-0.054	0.023	0.061	-0.075
	(0.267)	(0.232)	(0.230)	(0.312)	(0.192)	(0.179)	(0.167)	(0.183)
Clusters	60	78	95	33	89	113	137	102
Observations	577	745	946	335	4402	5540	6542	5048
Bandwidth	750	1000	1250	475	750	1000	1250	858
Panel (b): clearance rates								
Property crime clearance rate	0.030	0.025	0.036	0.030	0.004	0.020	0.020	-0.005
	(0.048)	(0.042)	(0.040)	(0.048)	(0.036)	(0.030)	(0.026)	(0.037)
Clusters	76	96	117	76	89	113	137	82
Observations	3090	4006	4852	3090	8891	11215	13589	8179
Bandwidth	750	1000	1250	752	750	1000	1250	703
Violent crime clearance rate	-0.268**	-0.195*	-0.198**	-0.317**	0.108*	0.103**	0.066	0.108*
	(0.120)	(0.110)	(0.098)	(0.129)	(0.059)	(0.048)	(0.048)	(0.063)
Clusters	60	78	95	38	89	113	137	79
Observations	577	745	946	385	4402	5540	6542	3971
Bandwidth	750	1000	1250	558	750	1000	1250	680
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to the inclusion of controls. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. The controls included in the regression are percentage male, percentage non-white, percentage with high school degree, percentage unemployed and percentage below poverty line according to the 1970 census. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-5: Effect on crime and clearance rates, robustness to controlling for baseline value of the outcome, all bandwidths

Sample	post-treatment		
	(1)	(2)	(3)
Panel (a): crime rates			
Log(property crime rate)	-0.485***	-0.438***	-0.416***
	(0.155)	(0.138)	(0.128)
Clusters	80	101	123
Observations	8258	10355	12545
Bandwidth	750	1000	1250
Log(violent crime rate)	0.037	0.154	0.140
	(0.238)	(0.200)	(0.170)
Clusters	71	92	110
Observations	3846	4874	5786
Bandwidth	750	1000	1250
Panel (b): clearance rates			
Property crime clearance rate	0.010	0.022	0.020
	(0.035)	(0.030)	(0.027)
Clusters	80	101	122
Observations	8258	10355	12477
Bandwidth	750	1000	1250
Violent crime clearance rate	0.162***	0.159***	0.141***
	(0.053)	(0.048)	(0.048)
Clusters	71	92	110
Observations	3846	4874	5786
Bandwidth	750	1000	1250
State-month FE	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to controlling for the average value in the pre-period of the residuals from a regression of the outcome on state-month fixed effects for pre-treatment years. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of post-treatment years (columns 1 to 4). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for three different bandwidths: 750, 1000, 1250. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-6: Effect on crime and clearance rates, robustness to other policies changing at the same threshold, all bandwidths

Sample	pre-treatment											
State being excluded	AZ				IL				IA			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel (a): crime rates												
Log(property crime rate)	-0.149	-0.056	0.059	-0.232	0.112	0.248	0.302*	0.135	-0.378**	-0.240	-0.126	-0.296
	(0.178)	(0.148)	(0.145)	(0.193)	(0.247)	(0.190)	(0.176)	(0.264)	(0.192)	(0.150)	(0.145)	(0.193)
Clusters	76	96	118	55	39	49	61	31	68	84	104	48
Observations	4476	5738	6994	3024	2774	3621	4455	2084	3603	4422	5473	2497
Bandwidth	750	1000	1250	568	750	1000	1250	613	750	1000	1250	516
Log(violent crime rate)	-0.251	-0.307	-0.107	-0.308	-0.147	-0.191*	0.288	-0.089	-0.243	-0.377	-0.253	-0.243
	(0.252)	(0.214)	(0.209)	(0.319)	(0.128)	(0.113)	(0.307)	(0.363)	(0.363)	(0.285)	(0.248)	(0.363)
Clusters	60	78	95	33	28	38	47	5	52	66	81	52
Observations	577	745	946	335	268	384	505	47	408	491	606	408
Bandwidth	750	1000	1250	481	750	1000	1250	286	750	1000	1250	747
Panel (b): clearance rates												
Property crime clearance rate	0.043	0.032	0.036	0.043	0.107	0.062	0.062	0.189*	0.005	-0.008	-0.010	0.010
	(0.049)	(0.044)	(0.042)	(0.048)	(0.068)	(0.066)	(0.060)	(0.106)	(0.047)	(0.040)	(0.039)	(0.049)
Clusters	76	96	117	78	39	49	61	26	68	84	103	51
Observations	3090	4006	4852	3186	2004	2630	3244	1323	2466	3058	3759	1816
Bandwidth	750	1000	1250	763	750	1000	1250	513	750	1000	1250	559
Violent crime clearance rate	-0.193*	-0.152	-0.142	-0.171	0.136*	0.158***	-0.051	0.314***	-0.339**	-0.274**	-0.223**	-0.355**
	(0.108)	(0.096)	(0.095)	(0.124)	(0.076)	(0.061)	(0.113)	(0.043)	(0.133)	(0.107)	(0.098)	(0.145)
Clusters	60	78	95	38	28	38	47	5	52	66	81	39
Observations	577	745	946	385	268	384	505	47	408	491	606	325
Bandwidth	750	1000	1250	546	750	1000	1250	291	750	1000	1250	605
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows that the results are not driven by any single state and thus do not depend on other state-specific laws also changing at the same threshold. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8), excluding one state at the time. Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

pre-treatment															
LA				MT				NE				WI CITY			
(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)
-0.172	-0.079	0.037	-0.263	-0.144	-0.080	0.048	-0.313*	-0.103	-0.051	0.054	-0.233	-0.167	-0.033	0.155	-0.290
(0.178)	(0.150)	(0.148)	(0.197)	(0.177)	(0.148)	(0.145)	(0.185)	(0.170)	(0.148)	(0.148)	(0.195)	(0.225)	(0.191)	(0.183)	(0.252)
72	92	114	48	73	92	114	41	70	89	109	48	61	77	93	42
4277	5539	6795	2734	4278	5458	6714	2217	4223	5421	6526	2808	3509	4513	5493	2242
750	1000	1250	517	750	1000	1250	407	750	1000	1250	535	750	1000	1250	519
-0.233	-0.279	-0.089	-0.238	-0.251	-0.297	-0.094	-0.308	-0.251	-0.309	-0.108	-0.307	-0.261	-0.323	-0.105	-0.327
(0.252)	(0.218)	(0.213)	(0.330)	(0.252)	(0.215)	(0.210)	(0.319)	(0.253)	(0.215)	(0.210)	(0.319)	(0.253)	(0.215)	(0.212)	(0.315)
57	75	92	32	57	74	91	33	58	75	91	31	50	64	78	28
527	695	896	331	573	727	928	335	573	735	933	331	546	703	895	327
750	1000	1250	539	750	1000	1250	481	750	1000	1250	467	750	1000	1250	457
0.047	0.042	0.047	0.047	0.043	0.041	0.042	0.047	0.048	0.043	0.053	0.048	0.017	0.021	0.030	0.037
(0.049)	(0.044)	(0.042)	(0.049)	(0.049)	(0.043)	(0.041)	(0.049)	(0.050)	(0.044)	(0.041)	(0.050)	(0.059)	(0.052)	(0.050)	(0.056)
72	92	113	72	73	92	113	70	70	89	108	71	61	77	92	71
2944	3860	4706	2944	2981	3847	4693	2874	2932	3824	4582	2949	2362	3050	3678	2764
750	1000	1250	753	750	1000	1250	727	750	1000	1250	759	750	1000	1250	888
-0.192*	-0.147	-0.135	-0.231*	-0.192*	-0.155	-0.145	-0.169	-0.194*	-0.155	-0.148	-0.169	-0.205*	-0.160*	-0.141	-0.177
(0.108)	(0.098)	(0.098)	(0.128)	(0.108)	(0.096)	(0.095)	(0.124)	(0.108)	(0.096)	(0.095)	(0.124)	(0.109)	(0.096)	(0.095)	(0.130)
57	75	92	40	57	74	91	35	58	75	91	33	50	64	78	29
527	695	896	385	573	727	928	381	573	735	933	377	546	703	895	331
750	1000	1250	600	750	1000	1250	566	750	1000	1250	511	750	1000	1250	487
yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

pre-treatment				post-treatment											
WI VILL				AZ				IL				IA			
(29)	(30)	(31)	(32)	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)	(41)	(42)	(43)	(44)
-0.135	-0.045	0.049	-0.224	-0.587***	-0.461**	-0.394**	-0.620***	-0.529*	-0.442**	-0.380*	-1.100***	-0.667***	-0.505***	-0.435**	-0.671***
(0.176)	(0.152)	(0.150)	(0.191)	(0.213)	(0.180)	(0.160)	(0.230)	(0.272)	(0.225)	(0.201)	(0.167)	(0.231)	(0.191)	(0.172)	(0.234)
73	93	113	53	89	113	137	73	48	60	74	9	81	101	123	71
4192	5454	6508	2852	8891	11215	13589	7387	4729	5957	7479	801	8033	9896	12031	7137
750	1000	1250	565	750	1000	1250	635	750	1000	1250	224	750	1000	1250	652
-0.250	-0.307	-0.102	-0.238	-0.030	0.027	0.091	-0.032	-0.261	-0.063	0.013	-0.488	-0.045	0.032	0.063	-0.080
(0.252)	(0.215)	(0.211)	(0.331)	(0.429)	(0.333)	(0.296)	(0.449)	(0.320)	(0.282)	(0.254)	(0.405)	(0.472)	(0.354)	(0.317)	(0.419)
58	76	90	34	89	113	137	79	48	60	74	30	81	101	123	85
567	735	913	339	4402	5540	6542	3971	1507	1928	2435	939	4068	5028	5863	4296
750	1000	1250	489	750	1000	1250	689	750	1000	1250	504	750	1000	1250	785
0.035	0.019	0.021	0.037	0.013	0.020	0.023	0.011	0.058	0.061	0.065	0.075	0.016	0.020	0.019	0.021
(0.048)	(0.045)	(0.042)	(0.049)	(0.034)	(0.029)	(0.026)	(0.035)	(0.064)	(0.052)	(0.044)	(0.086)	(0.037)	(0.031)	(0.028)	(0.034)
73	93	112	72	89	113	137	85	48	60	74	22	81	101	123	86
2851	3767	4450	2819	8891	11215	13589	8479	4729	5957	7479	2168	8033	9896	12031	8524
750	1000	1250	736	750	1000	1250	721	750	1000	1250	372	750	1000	1250	816
-0.191*	-0.158	-0.146	-0.171	0.123**	0.125***	0.098**	0.063	0.248***	0.247***	0.132	0.466***	0.118**	0.120**	0.094**	0.074
(0.107)	(0.096)	(0.095)	(0.124)	(0.052)	(0.047)	(0.048)	(0.055)	(0.086)	(0.090)	(0.098)	(0.157)	(0.056)	(0.047)	(0.044)	(0.058)
58	76	90	38	89	113	137	66	48	60	74	21	81	101	123	58
567	735	913	385	4402	5540	6542	3363	1507	1928	2435	590	4068	5028	5863	2996
750	1000	1250	566	750	1000	1250	581	750	1000	1250	361	750	1000	1250	556
yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

post-treatment															
MT				LA				NE				WI CITY			
(45)	(46)	(47)	(48)	(49)	(50)	(51)	(52)	(53)	(54)	(55)	(56)	(57)	(58)	(59)	(60)
-0.562***	-0.465**	-0.410**	-0.616***	-0.586***	-0.467**	-0.396**	-0.635***	-0.591***	-0.453**	-0.356**	-0.651***	-0.575**	-0.439**	-0.391**	-0.501**
(0.213)	(0.182)	(0.163)	(0.222)	(0.213)	(0.182)	(0.160)	(0.223)	(0.224)	(0.192)	(0.171)	(0.238)	(0.248)	(0.214)	(0.187)	(0.221)
80	103	127	73	85	108	132	73	82	105	126	69	72	91	108	88
8280	10552	12926	7568	8699	10968	13342	7539	8089	10303	12370	6917	6984	8758	10391	8468
750	1000	1250	709	750	1000	1250	660	750	1000	1250	646	750	1000	1250	929
0.040	0.094	0.140	0.077	-0.030	0.009	0.081	-0.065	-0.023	0.025	0.118	-0.067	-0.049	0.000	0.083	-0.051
(0.452)	(0.357)	(0.320)	(0.363)	(0.429)	(0.332)	(0.295)	(0.387)	(0.442)	(0.341)	(0.310)	(0.400)	(0.434)	(0.341)	(0.303)	(0.437)
80	103	127	97	85	108	132	96	82	105	126	86	72	91	108	70
3939	5030	6032	4803	4382	5474	6476	4988	4079	5184	6020	4359	4075	5096	6018	4015
750	1000	1250	917	750	1000	1250	848	750	1000	1250	791	750	1000	1250	726
0.012	0.024	0.028	0.001	0.012	0.018	0.022	-0.011	-0.002	0.008	0.006	-0.014	0.011	0.014	0.021	0.011
(0.034)	(0.030)	(0.027)	(0.039)	(0.034)	(0.029)	(0.026)	(0.035)	(0.035)	(0.030)	(0.027)	(0.036)	(0.036)	(0.030)	(0.028)	(0.033)
80	103	127	51	85	108	132	64	82	105	126	64	72	91	108	81
8280	10552	12926	5454	8699	10968	13342	6661	8089	10303	12370	6415	6984	8758	10391	7859
750	1000	1250	517	750	1000	1250	594	750	1000	1250	615	750	1000	1250	828
0.120**	0.125**	0.103**	0.118**	0.124**	0.122***	0.096**	0.107**	0.112**	0.118**	0.093*	0.107*	0.115**	0.114**	0.093*	0.065
(0.053)	(0.049)	(0.050)	(0.054)	(0.052)	(0.047)	(0.048)	(0.054)	(0.053)	(0.048)	(0.050)	(0.056)	(0.053)	(0.048)	(0.050)	(0.060)
80	103	127	76	85	108	132	73	82	105	126	76	72	91	108	47
3939	5030	6032	3818	4382	5474	6476	3771	4079	5184	6020	3855	4075	5096	6018	2873
750	1000	1250	723	750	1000	1250	652	750	1000	1250	713	750	1000	1250	519
yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

post-treatment			
WI VILL			
(61)	(62)	(63)	(64)
-0.591***	-0.476***	-0.417**	-0.457***
(0.214)	(0.184)	(0.164)	(0.172)
86	110	132	122
8532	10856	12995	11933
750	1000	1250	1140
-0.029	0.023	0.080	-0.031
(0.428)	(0.333)	(0.295)	(0.448)
86	110	132	76
4362	5500	6408	3931
750	1000	1250	688
0.010	0.019	0.024	0.020
(0.034)	(0.030)	(0.027)	(0.028)
86	110	132	121
8532	10856	12995	11836
750	1000	1250	1113
0.122**	0.124***	0.099**	0.086
(0.052)	(0.047)	(0.048)	(0.055)
86	110	132	68
4362	5500	6408	3492
750	1000	1250	616
yes	yes	yes	yes

Table C-7: Effect on crime and clearance rates, robustness to using a triangular kernel, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.196	-0.166	-0.064	-0.193	-0.589***	-0.558***	-0.485***	-0.591***
	(0.174)	(0.157)	(0.143)	(0.182)	(0.222)	(0.197)	(0.174)	(0.210)
Clusters	76	96	117	66	89	113	136	101
Observations	4476	5738	6973	3801	8891	11215	13485	10061
Bandwidth	750	1000	1250	656	750	1000	1250	857
Log(violent crime rate)	-0.277	-0.293	-0.196	-0.290	0.028	-0.004	0.025	0.008
	(0.285)	(0.248)	(0.234)	(0.306)	(0.502)	(0.425)	(0.371)	(0.476)
Clusters	60	78	94	50	89	113	136	99
Observations	577	745	942	479	4402	5540	6526	4936
Bandwidth	750	1000	1250	660	750	1000	1250	833
Panel (b): clearance rates								
Property crime clearance rate	0.028	0.034	0.038	0.028	0.005	0.010	0.014	0.013
	(0.043)	(0.041)	(0.039)	(0.043)	(0.033)	(0.031)	(0.028)	(0.035)
Clusters	76	96	116	74	89	113	136	58
Observations	3090	4006	4837	3041	8891	11215	13485	5959
Bandwidth	750	1000	1250	733	750	1000	1250	528
Violent crime clearance rate	-0.218*	-0.202**	-0.187**	-0.217*	0.096*	0.104**	0.105**	0.089
	(0.112)	(0.102)	(0.094)	(0.120)	(0.054)	(0.049)	(0.045)	(0.056)
Clusters	60	78	94	49	89	113	136	82
Observations	577	745	942	463	4402	5540	6526	4083
Bandwidth	750	1000	1250	643	750	1000	1250	701
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to using a triangular kernel. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1970 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1971 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a triangular kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-8: Effect on crime and clearance rates, robustness to using a Epanechnikov kernel, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.191	-0.156	-0.033	-0.195	-0.605***	-0.548***	-0.459***	-0.606***
	(0.177)	(0.156)	(0.141)	(0.190)	(0.221)	(0.193)	(0.170)	(0.211)
Clusters	76	96	117	58	89	113	136	98
Observations	4476	5738	6973	3306	8891	11215	13485	9772
Bandwidth	750	1000	1250	601	750	1000	1250	819
Log(violent crime rate)	-0.265	-0.297	-0.171	-0.269	-0.008	-0.024	0.024	-0.030
	(0.278)	(0.239)	(0.228)	(0.318)	(0.470)	(0.388)	(0.337)	(0.438)
Clusters	60	78	94	43	89	113	136	100
Observations	577	745	942	435	4402	5540	6526	5008
Bandwidth	750	1000	1250	601	750	1000	1250	848
Panel (b): clearance rates								
Property crime clearance rate	0.031	0.038	0.041	0.031	0.005	0.013	0.017	0.011
	(0.045)	(0.043)	(0.041)	(0.045)	(0.034)	(0.031)	(0.028)	(0.037)
Clusters	76	96	116	75	89	113	136	58
Observations	3090	4006	4837	3058	8891	11215	13485	5959
Bandwidth	750	1000	1250	734	750	1000	1250	516
Violent crime clearance rate	-0.212*	-0.190*	-0.175*	-0.203	0.102*	0.108**	0.108**	0.100*
	(0.112)	(0.101)	(0.094)	(0.125)	(0.054)	(0.048)	(0.045)	(0.054)
Clusters	60	78	94	39	89	113	136	88
Observations	577	745	942	401	4402	5540	6526	4392
Bandwidth	750	1000	1250	581	750	1000	1250	737
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to using a Epanechnikov kernel. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1970 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1971 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a Epanechnikov kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-9: Effect on crime and clearance rates, robustness to using locally quadratic regression, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.160	-0.293	-0.264	-0.100	-0.533*	-0.640**	-0.602**	-0.620**
	(0.228)	(0.201)	(0.194)	(0.228)	(0.274)	(0.259)	(0.243)	(0.267)
Clusters	76	96	118	84	89	113	137	108
Observations	4476	5738	6994	4883	8891	11215	13589	10706
Bandwidth	750	1000	1250	839	750	1000	1250	959
Log(violent crime rate)	-0.284	-0.290	-0.538*	-0.284	0.147	0.015	-0.108	-0.090
	(0.344)	(0.305)	(0.285)	(0.344)	(0.587)	(0.531)	(0.472)	(0.482)
Clusters	60	78	95	60	89	113	137	134
Observations	577	745	946	577	4402	5540	6542	6411
Bandwidth	750	1000	1250	733	750	1000	1250	1204
Panel (b): clearance rates								
Property crime clearance rate	0.053	0.064	0.061	0.063	-0.010	-0.002	0.006	-0.008
	(0.063)	(0.060)	(0.056)	(0.061)	(0.038)	(0.037)	(0.036)	(0.038)
Clusters	76	96	117	93	89	113	137	94
Observations	3090	4006	4852	3824	8891	11215	13589	9399
Bandwidth	750	1000	1250	982	750	1000	1250	785
Violent crime clearance rate	-0.237	-0.256*	-0.150	-0.248	0.063	0.096	0.160**	0.046
	(0.149)	(0.136)	(0.133)	(0.156)	(0.072)	(0.066)	(0.068)	(0.072)
Clusters	60	78	95	54	89	113	137	82
Observations	577	745	946	534	4402	5540	6542	4083
Bandwidth	750	1000	1250	700	750	1000	1250	703
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to using locally quadratic regressions. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1970 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1971 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally quadratic regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-10: Effect on crime and clearance rates, robustness to using locally cubic regression, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.113	-0.076	-0.171	-0.127	-0.410	-0.451*	-0.562**	-0.492*
	(0.262)	(0.221)	(0.219)	(0.246)	(0.280)	(0.269)	(0.280)	(0.278)
Clusters	76	96	118	86	89	113	137	133
Observations	4476	5738	6994	5069	8891	11215	13589	13178
Bandwidth	750	1000	1250	882	750	1000	1250	1184
Log(violent crime rate)	-0.466	-0.309	-0.343	-0.474	0.323	0.308	0.201	0.163
	(0.353)	(0.381)	(0.339)	(0.353)	(0.720)	(0.622)	(0.591)	(0.590)
Clusters	60	78	95	61	89	113	137	139
Observations	577	745	946	580	4402	5540	6542	6657
Bandwidth	750	1000	1250	759	750	1000	1250	1274
Panel (b): clearance rates								
Property crime clearance rate	0.052	0.052	0.061	0.042	0.028	0.002	0.015	-0.002
	(0.067)	(0.063)	(0.065)	(0.065)	(0.039)	(0.038)	(0.040)	(0.039)
Clusters	76	96	117	124	89	113	137	108
Observations	3090	4006	4852	5013	8891	11215	13589	10706
Bandwidth	750	1000	1250	1357	750	1000	1250	953
Violent crime clearance rate	-0.260	-0.228	-0.190	-0.196	0.106	0.098	0.083	0.067
	(0.210)	(0.162)	(0.155)	(0.156)	(0.088)	(0.081)	(0.079)	(0.080)
Clusters	60	78	95	74	89	113	137	127
Observations	577	745	946	701	4402	5540	6542	6173
Bandwidth	750	1000	1250	971	750	1000	1250	1133
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to using locally cubic regressions. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1970 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1971 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally cubic regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-11: Effect on crime and clearance rates, robustness to not including state-month fixed effects, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.139	-0.022	0.135	-0.073	-0.492**	-0.422**	-0.357**	-0.508**
	(0.160)	(0.147)	(0.153)	(0.172)	(0.215)	(0.192)	(0.174)	(0.208)
Clusters	76	96	119	54	89	113	138	95
Observations	4516	5770	7140	3063	8892	11216	13709	9515
Bandwidth	750	1000	1250	554	750	1000	1250	798
Log(violent crime rate)	-0.352	-0.517**	-0.383*	-0.318	0.163	0.071	0.149	0.136
	(0.275)	(0.247)	(0.227)	(0.328)	(0.373)	(0.312)	(0.281)	(0.373)
Clusters	66	84	102	43	89	113	138	92
Observations	698	862	1137	460	4541	5664	6761	4696
Bandwidth	750	1000	1250	492	750	1000	1250	767
Panel (b): clearance rates								
Property crime clearance rate	0.057	0.046	0.047	0.087	0.015	0.028	0.032	-0.001
	(0.049)	(0.044)	(0.041)	(0.056)	(0.035)	(0.032)	(0.030)	(0.037)
Clusters	76	96	118	51	89	113	138	48
Observations	3110	4022	4952	2051	8892	11216	13709	4949
Bandwidth	750	1000	1250	502	750	1000	1250	409
Violent crime clearance rate	-0.133	-0.063	-0.125	-0.120	0.140**	0.170***	0.101*	0.141**
	(0.118)	(0.102)	(0.093)	(0.136)	(0.057)	(0.055)	(0.058)	(0.069)
Clusters	66	84	102	46	89	113	138	58
Observations	698	862	1137	514	4541	5664	6761	3230
Bandwidth	750	1000	1250	546	750	1000	1250	528
State-month FE	no	no	no	no	no	no	no	no

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to excluding the state-month fixed effects. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1970 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1971 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses.

Table C-12: Effect on crime and clearance rates, robustness to more flexible running variable, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.152	-0.058	0.062	-0.247	-0.588***	-0.462**	-0.395**	-0.618***
	(0.177)	(0.149)	(0.144)	(0.191)	(0.213)	(0.180)	(0.160)	(0.229)
Clusters	76	96	118	55	89	113	137	73
Observations	4476	5738	6994	3024	8891	11215	13589	7387
Bandwidth	750	1000	1250	557	750	1000	1250	632
Log(violent crime rate)	-0.234	-0.309	-0.062	-0.258	-0.026	0.028	0.091	-0.053
	(0.252)	(0.206)	(0.209)	(0.351)	(0.429)	(0.335)	(0.295)	(0.379)
Clusters	60	78	95	33	89	113	137	102
Observations	577	745	946	335	4402	5540	6542	5048
Bandwidth	750	1000	1250	475	750	1000	1250	858
Panel (b): clearance rates								
Property crime clearance rate	0.043	0.032	0.038	0.043	0.013	0.020	0.023	0.006
	(0.049)	(0.044)	(0.041)	(0.049)	(0.034)	(0.029)	(0.027)	(0.036)
Clusters	76	96	117	76	89	113	137	82
Observations	3090	4006	4852	3090	8891	11215	13589	8179
Bandwidth	750	1000	1250	752	750	1000	1250	703
Violent crime clearance rate	-0.185*	-0.134	-0.160*	-0.159	0.122**	0.119***	0.097**	0.125**
	(0.098)	(0.090)	(0.086)	(0.124)	(0.052)	(0.046)	(0.046)	(0.055)
Clusters	60	78	95	38	89	113	137	79
Observations	577	745	946	385	4402	5540	6542	3971
Bandwidth	750	1000	1250	558	750	1000	1250	680
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustenss to allowing the running variable to vary by census and outcome year. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-13: Effect on crime and clearance rates, robustness to two-way clustering, all bandwidths

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): crime rates								
Log(property crime rate)	-0.149	-0.056	0.059	-0.232	-0.587***	-0.461**	-0.394**	-0.620***
	(0.178)	(0.149)	(0.145)	(0.192)	(0.213)	(0.181)	(0.160)	(0.230)
Clusters	74	93	115	53	86	109	133	71
Observations	4476	5738	6994	3024	8891	11215	13589	7387
Bandwidth	750	1000	1250	557	750	1000	1250	632
Log(violent crime rate)	-0.251	-0.307	-0.107	-0.308	-0.030	0.027	0.091	-0.053
	(0.252)	(0.214)	(0.209)	(0.319)	(0.429)	(0.333)	(0.296)	(0.378)
Clusters	60	77	94	33	86	109	133	99
Observations	577	745	946	335	4402	5540	6542	5048
Bandwidth	750	1000	1250	475	750	1000	1250	858
Panel (b): clearance rates								
Property crime clearance rate	0.043	0.032	0.036	0.043	0.013	0.020	0.023	0.005
	(0.049)	(0.044)	(0.042)	(0.049)	(0.034)	(0.029)	(0.026)	(0.036)
Clusters	74	93	114	74	86	109	133	79
Observations	3090	4006	4852	3090	8891	11215	13589	8179
Bandwidth	750	1000	1250	752	750	1000	1250	703
Violent crime clearance rate	-0.193*	-0.152	-0.142	-0.171	0.123**	0.125***	0.098**	0.126**
	(0.108)	(0.096)	(0.095)	(0.124)	(0.052)	(0.047)	(0.048)	(0.055)
Clusters	60	77	94	38	86	109	133	77
Observations	577	745	946	385	4402	5540	6542	3971
Bandwidth	750	1000	1250	558	750	1000	1250	680
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows robustness to clustering the standard errors at the municipality and running variable level. It presents RD estimates on crime rates (panel (a)) and clearance rates (panel (b)) for the baseline sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1960 to 1969 for states with mandates based on the federal population census only and 1960 to 1967 for states with mandates based on federal, state or municipal census. Post-treatment years are 1970 to 1979 for all states. Variation in treatment status is from the 1970 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality and running variable level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-14: Effect on demographic composition, all bandwidths

Sample	post-treatment											
Individuals sample	Low ranked				Young				New Hires			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel (a): ethnicity												
1st generation immigrant	-0.049	-0.111	-0.067	0.113	-0.128	-0.102	-0.078	-0.131	-0.050	-0.092	-0.053	-0.090
	(0.113)	(0.113)	(0.125)	(0.176)	(0.108)	(0.088)	(0.085)	(0.112)	(0.106)	(0.104)	(0.111)	(0.118)
Clusters	40	50	58	28	36	44	50	35	41	50	58	42
Observations	55	75	89	37	44	56	64	42	60	77	89	63
Bandwidth	750	1000	1250	593	750	1000	1250	730	750	1000	1250	781
2nd generation immigrant	-0.516	-0.377	-0.350	-0.383	0.250	0.091	0.152	0.150	-0.488	-0.444	-0.458	-0.485
	(0.405)	(0.375)	(0.331)	(0.336)	(0.351)	(0.342)	(0.256)	(0.260)	(0.353)	(0.334)	(0.305)	(0.341)
Clusters	28	37	41	40	22	26	29	29	31	37	41	36
Observations	32	45	53	50	23	29	33	32	37	47	53	46
Bandwidth	750	1000	1250	1088	750	1000	1250	1066	750	1000	1250	987
Foreign name	-0.149	0.076	0.019	0.011	-0.084	0.153	0.077	0.033	-0.137	0.096	0.031	-0.267
	(0.168)	(0.189)	(0.162)	(0.171)	(0.228)	(0.229)	(0.203)	(0.225)	(0.159)	(0.182)	(0.158)	(0.166)
Clusters	40	50	58	46	36	44	50	40	41	50	58	32
Observations	55	75	89	68	44	56	64	52	60	77	89	44
Bandwidth	750	1000	1250	923	750	1000	1250	911	750	1000	1250	619
Panel (b): patronage												
Coethnic with mayor	-0.735**	-0.409	-0.124	-0.503	-0.198	-0.007	0.160	-0.072	-0.761**	-0.397	-0.114	-0.766**
	(0.350)	(0.332)	(0.275)	(0.366)	(0.232)	(0.310)	(0.272)	(0.250)	(0.303)	(0.299)	(0.267)	(0.384)
Clusters	28	37	41	32	22	26	29	24	31	37	41	28
Observations	32	45	53	39	23	29	33	27	37	47	53	33
Bandwidth	750	1000	1250	828	750	1000	1250	844	750	1000	1250	698
Coethnic with mayor (name)	-0.044	0.044	0.049	0.000	0.000	0.086	0.065	0.041	-0.071	0.004	0.024	-0.011
	(0.060)	(0.066)	(0.036)	(0.054)	(0.060)	(0.097)	(0.067)	(0.086)	(0.069)	(0.072)	(0.053)	(0.064)
Clusters	40	50	58	42	36	44	50	39	41	50	58	43
Observations	55	75	89	62	44	56	64	51	60	77	89	67
Bandwidth	750	1000	1250	797	750	1000	1250	879	750	1000	1250	844
Belongs to dominant immigrant	-0.400	-0.591	-0.541*	-0.592	-0.011	-0.135	-0.172	-0.135	-0.150	-0.367	-0.308	-0.198
group	(0.380)	(0.377)	(0.325)	(0.382)	(0.484)	(0.427)	(0.403)	(0.427)	(0.279)	(0.318)	(0.292)	(0.388)
Clusters	28	37	41	33	22	26	29	26	31	37	41	28
Observations	32	45	53	40	23	29	33	29	37	47	53	33
Bandwidth	750	1000	1250	882	750	1000	1250	998	750	1000	1250	685
Panel (c): human capital												
Age	2.870	5.057	6.376	5.057	4.982***	5.067***	6.632***	10.596**	0.464	1.791	3.022	1.431
	(4.681)	(4.235)	(3.925)	(4.235)	(1.928)	(1.922)	(1.736)	(2.282)	(5.328)	(4.799)	(4.405)	(4.850)
Clusters	40	50	58	50	36	44	50	23	41	50	58	47
Observations	55	75	89	75	44	56	64	25	60	77	89	73
Bandwidth	750	1000	1250	989	750	1000	1250	547	750	1000	1250	968
Finished primary school	0.338	0.173	0.163	0.617***	0.406**	0.204	0.169	0.426**	0.239	0.117	0.112	0.347*
	(0.223)	(0.221)	(0.201)	(0.236)	(0.203)	(0.194)	(0.183)	(0.207)	(0.164)	(0.169)	(0.160)	(0.202)
Clusters	23	30	36	17	21	27	31	20	23	30	36	20
Observations	23	30	36	17	21	27	31	20	23	30	36	20
Bandwidth	750	1000	1250	622	750	1000	1250	721	750	1000	1250	648
Finished high school	-0.532**	-0.526***	-0.449**	-0.536**	-0.498*	-0.587**	-0.616***	-0.500	-0.566**	-0.560***	-0.473**	-0.507*
	(0.241)	(0.202)	(0.187)	(0.234)	(0.287)	(0.241)	(0.214)	(0.351)	(0.245)	(0.197)	(0.186)	(0.274)
Clusters	23	30	36	24	21	27	31	18	23	30	36	20
Observations	23	30	36	24	21	27	31	18	23	30	36	20
Bandwidth	750	1000	1250	788	750	1000	1250	669	750	1000	1250	681
State-census FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on demographic composition of police departments. It presents RD estimates on outcomes related to ethnicity (panel (a)), ethnic patronage (panel (b)) and the human capital of police officers (panel (c)) for the sample of post-treatment years (columns 1 to 12), specifically for low ranked police officers (columns 1 to 4), young police officers (columns 5 to 8) and newly hired police officers (columns 9 to 12). The outcomes related to ethnicity (and the census years for which they are available) are fraction first generation immigrant (1910-1940), fraction second generation immigrant (1910-1930), fraction with foreign name (1910-1940). The outcomes related to ethnic patronage (and the census years for which they are available) are fraction co-ethnic with the mayor (1910-1930), fraction co-ethnic with the mayor based on their first names (1910-1940) and fraction belonging to the dominant ethnic group (1910-1930). The outcomes related to the human capital of police officers are average age (1910-1940), fraction with primary school education (1940) and fraction with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table C-15: Effect on demographic composition, robustness to including controls, all bandwidths

Sample	post-treatment						
Individuals sample	All police officers				Low ranked	Young	New hires
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): ethnicity							
1st generation immigrant	0.063	-0.023	0.001	0.013	-0.049	-0.073	-0.023
	(0.097)	(0.099)	(0.110)	(0.105)	(0.099)	(0.075)	(0.085)
Clusters	42	52	60	47	50	44	50
Observations	62	82	96	74	75	56	77
Bandwidth	750	1000	1250	915	1000	1000	1000
2nd generation immigrant	-0.166	-0.250	-0.242	-0.198	-0.175	0.159	-0.264
	(0.275)	(0.279)	(0.239)	(0.275)	(0.323)	(0.364)	(0.293)
Clusters	32	40	44	37	37	26	37
Observations	39	52	60	49	45	29	47
Bandwidth	750	1000	1250	981	1000	1000	1000
Foreign name	-0.126	0.065	-0.016	-0.249	0.054	0.134	0.074
	(0.156)	(0.174)	(0.155)	(0.169)	(0.190)	(0.235)	(0.181)
Clusters	42	52	60	30	50	44	50
Observations	62	82	96	40	75	56	77
Bandwidth	750	1000	1250	572	1000	1000	1000
Panel (b): patronage							
Coethnic with mayor	-0.571	-0.273	-0.109	-0.451	-0.250	0.296	-0.224
	(0.357)	(0.311)	(0.227)	(0.356)	(0.347)	(0.406)	(0.301)
Clusters	32	40	44	33	37	26	37
Observations	39	52	60	40	45	29	47
Bandwidth	750	1000	1250	763	1000	1000	1000
Coethnic with mayor (name)	-0.072	-0.001	0.006	-0.035	0.039	0.075	0.002
	(0.072)	(0.079)	(0.047)	(0.069)	(0.073)	(0.096)	(0.076)
Clusters	42	52	60	44	50	44	50
Observations	62	82	96	69	75	56	77
Bandwidth	750	1000	1250	803	1000	1000	1000
Belongs to dominant immigrant group	-0.055	-0.388	-0.459	-0.299	-0.506	-0.002	-0.244
	(0.312)	(0.381)	(0.328)	(0.355)	(0.385)	(0.463)	(0.322)
Clusters	32	40	44	36	37	26	37
Observations	39	52	60	46	45	29	47
Bandwidth	750	1000	1250	823	1000	1000	1000
Panel (c): human capital							
Age	1.072	1.688	4.077	1.645	5.176	4.873**	1.251
	(4.675)	(4.624)	(4.072)	(4.583)	(3.954)	(1.930)	(4.887)
Clusters	42	52	60	48	50	44	50
Observations	62	82	96	77	75	56	77
Bandwidth	750	1000	1250	946	1000	1000	1000
Finished primary school	0.339	0.211	0.237	-0.109	0.267	0.250	0.192
	(0.324)	(0.225)	(0.176)	(0.547)	(0.297)	(0.218)	(0.236)
Clusters	23	30	36	19	30	27	30
Observations	23	30	36	19	30	27	30
Bandwidth	750	1000	1250	640	1000	1000	1000

Finished high school	-0.661	-0.617**	-0.515**	-0.661	-0.673**	-0.631*	-0.636**
	(0.450)	(0.277)	(0.247)	(0.450)	(0.291)	(0.353)	(0.280)
Clusters	23	30	36	23	30	27	30
Observations	23	30	36	23	30	27	30
Bandwidth	750	1000	1250	738	1000	1000	1000
State-census FE	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on demographic composition of police departments. Regressions include city level controls. It presents RD estimates on outcomes related to ethnicity (panel (a)), ethnic patronage (panel (b)) and the human capital of police officers (panel (c)) for the sample of post-treatment years (columns 1 to 7), specifically for all police officers (columns 1 to 4), low ranked police officers (column 5), young police officers (column 6) and newly hired police officers (column 7). The outcomes related to ethnicity (and the census years for which they are available) are fraction first generation immigrant (1910-1940), fraction second generation immigrant (1910-1930), fraction with foreign name (1910-1940). The outcomes related to ethnic patronage (and the census years for which they are available) are fraction co-ethnic with the mayor (1910-1930), fraction co-ethnic with the mayor based on their first names (1910-1940) and fraction belonging to the dominant ethnic group (1910-1930). The outcomes related to the human capital of police officers are average age (1910-1940), fraction with primary school education (1940) and fraction with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The controls included are fraction male, fraction white, fraction 1st generation immigrants for the outcomes from the 1910, 1920, 1930 and 1940 census; fraction male, fraction white, fraction 1st generation immigrant, fraction 2nd generation immigrants for the outcomes from the 1910, 1920 and 1930 census; fraction male, fraction white, fraction 1st generation immigrant, fraction with primary school, fraction with high school for the outcomes from the 1940 census. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table C-16: Effect on demographic composition, robustness to dropping places in metropolitan areas, all bandwidths

Sample	post-treatment						
Individuals sample	All police officers				Low ranked	Young	New hires
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): ethnicity							
1st generation immigrant	-0.012	-0.081	-0.070	-0.052	-0.119	-0.105	-0.087
	(0.106)	(0.119)	(0.131)	(0.124)	(0.122)	(0.099)	(0.111)
Clusters	39	47	54	42	45	39	45
Observations	59	77	90	69	70	51	72
Bandwidth	750	1000	1250	889	1000	1000	1000
2nd generation immigrant	-0.488	-0.427	-0.407	-0.418	-0.395	0.058	-0.463
	(0.333)	(0.317)	(0.274)	(0.329)	(0.376)	(0.357)	(0.334)
Clusters	30	38	42	34	35	24	35
Observations	37	50	58	45	43	27	45
Bandwidth	750	1000	1250	891	1000	1000	1000
Foreign name	-0.162	0.033	-0.061	-0.279*	0.012	0.017	0.052
	(0.147)	(0.180)	(0.164)	(0.166)	(0.192)	(0.210)	(0.188)
Clusters	39	47	54	28	45	39	45
Observations	59	77	90	37	70	51	72
Bandwidth	750	1000	1250	534	1000	1000	1000
Panel (b): patronage							
Coethnic with mayor	-0.735**	-0.433	-0.261	-0.610*	-0.441	-0.002	-0.427
	(0.299)	(0.295)	(0.244)	(0.312)	(0.340)	(0.321)	(0.306)
Clusters	30	38	42	33	35	24	35
Observations	37	50	58	41	43	27	45
Bandwidth	750	1000	1250	783	1000	1000	1000
Coethnic with mayor (name)	-0.079	-0.013	0.002	-0.030	0.024	0.035	-0.010
	(0.073)	(0.078)	(0.049)	(0.068)	(0.065)	(0.082)	(0.076)
Clusters	39	47	54	41	45	39	45
Observations	59	77	90	66	70	51	72
Bandwidth	750	1000	1250	789	1000	1000	1000
Belongs to dominant immigrant group	-0.218	-0.490	-0.552*	-0.453	-0.635	-0.206	-0.402
	(0.283)	(0.349)	(0.301)	(0.339)	(0.395)	(0.474)	(0.330)
Clusters	30	38	42	34	35	24	35
Observations	37	50	58	45	43	27	45
Bandwidth	750	1000	1250	855	1000	1000	1000
Panel (c): human capital							
Age	-0.292	1.349	3.167	-2.959	4.471	4.759**	1.016
	(5.081)	(4.600)	(4.073)	(7.064)	(4.220)	(1.904)	(4.817)
Clusters	39	47	54	30	45	39	45
Observations	59	77	90	42	70	51	72
Bandwidth	750	1000	1250	592	1000	1000	1000
Finished primary school	0.295**	0.219	0.256*	0.242	0.283	0.382**	0.202
	(0.138)	(0.163)	(0.153)	(0.153)	(0.227)	(0.183)	(0.173)
Clusters	22	27	32	25	27	24	27
Observations	22	27	32	25	27	24	27
Bandwidth	750	1000	1250	949	1000	1000	1000

Finished high school	-0.547**	-0.523**	-0.415**	-0.505**	-0.507**	-0.497**	-0.536***
	(0.234)	(0.205)	(0.202)	(0.223)	(0.214)	(0.238)	(0.206)
Clusters	22	27	32	24	27	24	27
Observations	22	27	32	24	27	24	27
Bandwidth	750	1000	1250	896	1000	1000	1000
State-census FE	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on demographic composition of police departments dropping places in metropolitan areas. It presents RD estimates on outcomes related to ethnicity (panel (a)), ethnic patronage (panel (b)) and the human capital of police officers (panel (c)) for the sample of post-treatment years (columns 1 to 7), specifically for all police officers (columns 1 to 4), low ranked police officers (column 5), young police officers (column 6) and newly hired police officers (column 7). The outcomes related to ethnicity (and the census years for which they are available) are fraction first generation immigrant (1910-1940), fraction second generation immigrant (1910-1930), fraction with foreign name (1910-1940). The outcomes related to ethnic patronage (and the census years for which they are available) are fraction co-ethnic with the mayor (1910-1930), fraction co-ethnic with the mayor based on their first names (1910-1940) and fraction belonging to the dominant ethnic group (1910-1930). The outcomes related to the human capital of police officers are average age (1910-1940), fraction with primary school education (1940) and fraction with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table C-17: Effect on demographic composition, robustness to individual level regressions, all bandwidths

Sample	post-treatment						
Individuals sample	All police officers				Low ranked	Young	New hires
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): ethnicity							
1st generation immigrant	-0.034	-0.065	-0.003	-0.039	-0.049	-0.074	-0.042
	(0.076)	(0.066)	(0.069)	(0.070)	(0.065)	(0.066)	(0.059)
Clusters	42	53	60	48	51	45	51
Observations	207	281	315	262	224	131	245
Bandwidth	750	1000	1250	904	1000	1000	1000
2nd generation immigrant	-0.371	-0.251	-0.249	-0.438	-0.021	0.229	-0.201
	(0.318)	(0.286)	(0.255)	(0.345)	(0.278)	(0.334)	(0.271)
Clusters	32	41	44	30	38	27	38
Observations	107	148	164	99	113	70	120
Bandwidth	750	1000	1250	710	1000	1000	1000
Foreign name	-0.135	0.050	-0.008	-0.187	0.086	0.202	0.090
	(0.136)	(0.154)	(0.125)	(0.128)	(0.155)	(0.188)	(0.150)
Clusters	42	53	60	37	51	45	51
Observations	207	281	315	166	224	131	245
Bandwidth	750	1000	1250	643	1000	1000	1000
Panel (b): patronage							
Coethnic with mayor	-0.682**	-0.322	-0.114	-0.671**	-0.190	0.086	-0.327
	(0.299)	(0.286)	(0.242)	(0.307)	(0.270)	(0.297)	(0.276)
Clusters	32	41	44	32	38	27	38
Observations	107	148	164	105	113	70	120
Bandwidth	750	1000	1250	727	1000	1000	1000
Coethnic with mayor (name)	-0.042	0.007	-0.004	-0.024	0.032	0.057	0.005
	(0.051)	(0.054)	(0.037)	(0.042)	(0.050)	(0.072)	(0.057)
Clusters	42	53	60	44	51	45	51
Observations	207	281	315	226	224	131	245
Bandwidth	750	1000	1250	771	1000	1000	1000
Belongs to dominant immigrant	-0.042	-0.245	-0.254	-0.042	-0.203	0.097	-0.148
group	(0.203)	(0.229)	(0.219)	(0.271)	(0.259)	(0.326)	(0.202)
Clusters	32	41	44	29	38	27	38
Observations	107	148	164	94	113	70	120
Bandwidth	750	1000	1250	670	1000	1000	1000
Panel (c): human capital							
Age	-0.533	1.745	3.479	0.032	4.384	4.502**	0.983
	(3.379)	(3.062)	(2.857)	(3.286)	(3.228)	(1.755)	(3.295)
Clusters	42	53	60	46	51	45	51
Observations	207	281	315	244	224	131	245
Bandwidth	750	1000	1250	880	1000	1000	1000
Finished primary school	0.290**	0.124	0.087	0.496***	0.156	0.107	0.123
	(0.144)	(0.138)	(0.134)	(0.110)	(0.183)	(0.157)	(0.147)
Clusters	23	30	36	16	30	27	30
Observations	98	131	149	65	110	59	123
Bandwidth	750	1000	1250	596	1000	1000	1000

Finished high school	-0.460**	-0.485***	-0.443***	-0.408*	-0.474***	-0.593***	-0.459***
	(0.201)	(0.159)	(0.144)	(0.211)	(0.149)	(0.210)	(0.154)
Clusters	23	30	36	21	30	27	30
Observations	98	131	149	90	110	59	123
Bandwidth	750	1000	1250	712	1000	1000	1000
State-census FE	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on demographic composition of police departments. Regressions are run at the individual level (this is equivalent to municipal level regressions weighted by size of the police department). It presents RD estimates on outcomes related to ethnicity (panel (a)), ethnic patronage (panel (b)) and the human capital of police officers (panel (c)) for the sample of post-treatment years (columns 1 to 7), specifically for all police officers (columns 1 to 4), low ranked police officers (column 5), young police officers (column 6) and newly hired police officers (column 7). The outcomes related to ethnicity (and the census years for which they are available) are first generation immigrant (1910-1940), second generation immigrant (1910-1930), with foreign name (1910-1940). The outcomes related to ethnic patronage (and the census years for which they are available) are co-ethnic with the mayor (1910-1930), co-ethnic with the mayor based on their first names (1910-1940) and belonging to the dominant ethnic group (1910-1930). The outcomes related to the human capital of police officers are age (1910-1940), with primary school education (1940) and with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table C-18: Effect on demographic composition, robustness to defining outcomes as differences, all bandwidths

Sample	post-treatment						
Individuals sample	All police officers				Low ranked	Young	New hires
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): ethnicity							
1st generation immigrant	0.001	-0.043	-0.038	-0.050	-0.059	-0.055	-0.060
	(0.076)	(0.079)	(0.084)	(0.087)	(0.076)	(0.051)	(0.071)
Clusters	42	52	60	45	50	44	50
Observations	62	82	96	71	75	56	77
Bandwidth	750	1000	1250	846	1000	1000	1000
2nd generation immigrant	-0.034	0.077	0.045	0.062	0.191*	0.303*	0.057
	(0.148)	(0.110)	(0.099)	(0.140)	(0.113)	(0.167)	(0.113)
Clusters	32	40	44	21	37	26	37
Observations	39	52	60	24	45	29	47
Bandwidth	750	1000	1250	578	1000	1000	1000
Panel (b): human capital							
Age	-2.539	-0.311	2.344	-1.444	2.109	-0.201	-0.633
	(4.011)	(3.917)	(3.679)	(3.919)	(3.513)	(1.451)	(4.128)
Clusters	42	52	60	46	50	44	50
Observations	62	82	96	73	75	56	77
Bandwidth	750	1000	1250	875	1000	1000	1000
Finished primary school	-0.028	-0.114	-0.070	0.111	-0.192	-0.053	-0.115
	(0.098)	(0.096)	(0.090)	(0.125)	(0.119)	(0.086)	(0.098)
Clusters	23	30	36	13	30	27	30
Observations	23	30	36	13	30	27	30
Bandwidth	750	1000	1250	501	1000	1000	1000
Finished high school	-0.249*	-0.149	-0.074	-0.189	-0.102	-0.166	-0.155
	(0.134)	(0.115)	(0.109)	(0.137)	(0.128)	(0.146)	(0.114)
Clusters	23	30	36	17	30	27	30
Observations	23	30	36	17	30	27	30
Bandwidth	750	1000	1250	623	1000	1000	1000
State-census FE	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on demographic composition of police departments. It presents RD estimates on outcomes related to ethnicity (panel (a)) and the human capital of police officers (panel (b)) for the sample of post-treatment years (columns 1 to 7), specifically for all police officers (columns 1 to 4), low ranked police officers (column 5), young police officers (column 6) and newly hired police officers (column 7). The outcomes are defines as the absolute value of the difference between the fraction of the police officers and the fraction of the population having a characteristics. The outcomes related to ethnicity (and the census years for which they are available) are fraction first generation immigrant (1910-1940) and fraction second generation immigrant (1910-1930). The outcomes related to the human capital of police officers are average age (1910-1940), fraction with primary school education (1940) and fraction with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table C-19: Effect on demographic composition, robustness to defining outcomes as standard deviations, all bandwidths

Sample	post-treatment						
Individuals sample	All police officers				Low ranked	Young	New hires
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel (a): ethnicity							
1st generation immigrant	-0.055	-0.060	-0.017	0.003	-0.066	0.061	-0.051
	(0.119)	(0.106)	(0.104)	(0.112)	(0.092)	(0.089)	(0.086)
Clusters	39	50	56	45	41	30	46
Observations	51	69	79	62	54	37	64
Bandwidth	750	1000	1250	910	1000	1000	1000
2nd generation immigrant	-0.053	-0.188	-0.272	-0.084	-0.460	-0.130	-0.266
	(0.218)	(0.260)	(0.256)	(0.245)	(0.309)	(0.688)	(0.229)
Clusters	27	35	38	18	24	16	31
Observations	29	40	45	19	27	19	35
Bandwidth	750	1000	1250	602	1000	1000	1000
Foreign name	-0.138	-0.040	-0.026	-0.101	0.220	0.422	0.018
	(0.185)	(0.181)	(0.164)	(0.176)	(0.210)	(0.301)	(0.178)
Clusters	39	50	56	44	41	30	46
Observations	51	69	79	61	54	37	64
Bandwidth	750	1000	1250	885	1000	1000	1000
Panel (b): patronage							
Coethnic with mayor	-0.012	0.155	0.190	0.212	0.316	0.706*	0.141
	(0.306)	(0.276)	(0.240)	(0.301)	(0.289)	(0.417)	(0.254)
Clusters	27	35	38	31	24	16	31
Observations	29	40	45	35	27	19	35
Bandwidth	750	1000	1250	818	1000	1000	1000
Coethnic with mayor (name)	-0.058	-0.036	-0.061	-0.007	0.029	0.021	-0.051
	(0.098)	(0.083)	(0.064)	(0.098)	(0.082)	(0.077)	(0.086)
Clusters	39	50	56	44	41	30	46
Observations	51	69	79	61	54	37	64
Bandwidth	750	1000	1250	886	1000	1000	1000
Belongs to dominant immigrant	-0.297	-0.203	-0.237	-0.190	-0.021	0.949***	-0.176
group	(0.318)	(0.275)	(0.263)	(0.307)	(0.332)	(0.216)	(0.250)
Clusters	27	35	38	28	24	16	31
Observations	29	40	45	30	27	19	35
Bandwidth	750	1000	1250	762	1000	1000	1000
Panel (c): human capital							
Age	-0.735	-2.853	-3.091*	-6.236***	-4.678*	-0.023	-3.045
	(2.268)	(1.788)	(1.645)	(2.373)	(2.587)	(1.727)	(1.964)
Clusters	39	50	56	24	41	30	46
Observations	51	69	79	28	54	37	64
Bandwidth	750	1000	1250	500	1000	1000	1000
Finished primary school	-0.255	-0.074	-0.098	-0.292	0.068	-0.090	-0.058
	(0.218)	(0.212)	(0.185)	(0.218)	(0.221)	(0.234)	(0.223)
Clusters	22	29	34	21	27	18	29
Observations	22	29	34	21	27	18	29
Bandwidth	750	1000	1250	733	1000	1000	1000

Finished high school	-0.125	-0.201	-0.220	-0.104	-0.355**	-0.316	-0.206
	(0.224)	(0.167)	(0.149)	(0.231)	(0.170)	(0.322)	(0.167)
Clusters	22	29	34	20	27	18	29
Observations	22	29	34	20	27	18	29
Bandwidth	750	1000	1250	718	1000	1000	1000
State-census FE	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table shows the reduced form effect of merit systems on the variance of demographic composition of police departments. It presents RD estimates on outcomes related to ethnicity (panel (a)), ethnic patronage (panel (b)) and the human capital of police officers (panel (c)) for the sample of post-treatment years (columns 1 to 7), specifically for all policemen (columns 1 to 4), low ranked police officers (column 5), young police officers (column 6) and newly hired police officers (column 7). All outcomes are defined as the standard deviation of the characteristic at the municipality level. The outcomes related to ethnicity (and the census years for which they are available) are first generation immigrant (1910-1940), second generation immigrant (1910-1930), police officer with foreign name (1910-1940). The outcomes related to ethnic patronage (and the census years for which they are available) are police officer co-ethnic with the mayor (1910-1930), police officer co-ethnic with the mayor based on their first names (1910-1940) and police officer belonging to the dominant ethnic group (1910-1930). The outcomes related to human capital are age (1910-1940), police officer with primary school education (1940) and police officer with secondary school education (1940). Variation in treatment is from the 1900 to 1930 census experiments depending on the availability of the outcome. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-census year fixed effects are included in all columns.

Table C-20: Effect on reporting, crime and clearance rates for the 1980 census experiment

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): reporting								
Monthly crime report missing	-0.004	0.045	0.068	0.049	0.021	0.052	0.067	0.065
	(0.074)	(0.075)	(0.070)	(0.108)	(0.045)	(0.055)	(0.050)	(0.047)
Clusters	74	103	130	28	74	103	130	49
Observations	6552	9036	11376	2484	8880	12360	15600	5880
Bandwidth	750	1000	1250	345	750	1000	1250	548
Panel (b): crime rates								
Log(property crime rate)	-0.089	-0.066	-0.144	-0.469	-0.167	-0.016	-0.027	-1.304***
	(0.221)	(0.198)	(0.165)	(0.388)	(0.210)	(0.192)	(0.150)	(0.357)
Clusters	72	100	126	29	74	102	127	22
Observations	5219	7081	8615	2300	8360	11464	14102	2470
Bandwidth	750	1000	1250	391	750	1000	1250	266
Log(violent crime rate)	-0.301*	-0.137	-0.420**	-0.301*	-0.228	-0.061	-0.182	-0.346
	(0.179)	(0.155)	(0.170)	(0.179)	(0.184)	(0.154)	(0.143)	(0.319)
Clusters	71	99	125	71	74	102	127	24
Observations	2158	3005	3603	2158	6067	8330	10229	1830
Bandwidth	750	1000	1250	742	750	1000	1250	285
Panel (c): clearance rates								
Property crime clearance rate	-0.005	0.019	0.019	0.007	0.016	-0.021	-0.026	-0.023
	(0.043)	(0.039)	(0.031)	(0.050)	(0.029)	(0.026)	(0.024)	(0.042)
Clusters	72	100	126	62	74	102	127	32
Observations	5219	7081	8615	4479	8360	11464	14102	3617
Bandwidth	750	1000	1250	676	750	1000	1250	407
Violent crime clearance rate	0.043	0.003	0.021	0.024	-0.013	-0.007	-0.017	-0.039
	(0.119)	(0.099)	(0.078)	(0.127)	(0.104)	(0.082)	(0.064)	(0.139)
Clusters	71	99	125	32	74	102	127	45
Observations	2158	3005	3603	856	6067	8330	10229	3648
Bandwidth	750	1000	1250	415	750	1000	1250	504
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The tables shows the effect of the merit system mandate on crime reporting and police performance for the 1980 census experiment. The table presents RD estimates on crime reporting (panel (a)), crime rates (panel (b)) and clearance rates (panel (c)) for the sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Monthly crime report missing is a dummy equal to one if the department did not submit a report for the month, crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1970 to 1979 for states with mandates based on the federal population census only and 1970 to 1977 for states with mandates based on federal, state or municipal census. Post-treatment years are 1980 to 1989 for all states. Variation in treatment status is from the 1980 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-21: Effect on reporting, crime and clearance rates for the 1990 census experiment

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): reporting								
Monthly crime report missing	-0.133	-0.114	0.011	-0.011	-0.088	-0.045	-0.024	-0.049
	(0.103)	(0.104)	(0.100)	(0.091)	(0.061)	(0.059)	(0.072)	(0.088)
Clusters	43	63	85	79	43	63	85	141
Observations	4260	6264	8436	7788	5160	7560	10200	16920
Bandwidth	750	1000	1250	1189	750	1000	1250	1664
Panel (b): crime rates								
Log(property crime rate)	-0.101	-0.007	-0.202	-0.179	0.372	0.288	-0.079	0.313
	(0.348)	(0.337)	(0.243)	(0.243)	(0.462)	(0.343)	(0.259)	(0.384)
Clusters	43	63	82	79	42	62	83	50
Observations	3893	5694	7308	7117	2569	3962	5412	3073
Bandwidth	750	1000	1250	1221	750	1000	1250	803
Log(violent crime rate)	-0.129	-0.164	-0.266	-0.265	0.289	-0.203	-0.298	0.400
	(0.297)	(0.252)	(0.204)	(0.209)	(0.297)	(0.324)	(0.244)	(0.357)
Clusters	43	63	82	76	43	63	84	25
Observations	2371	3281	4317	4085	2007	2985	3997	1131
Bandwidth	750	1000	1250	1164	750	1000	1250	536
Panel (c): clearance rates								
Property crime clearance rate	-0.071	-0.071*	-0.051	-0.071	-0.042	-0.043	-0.021	-0.033
	(0.048)	(0.039)	(0.037)	(0.048)	(0.071)	(0.060)	(0.040)	(0.060)
Clusters	43	63	82	43	42	62	83	55
Observations	3893	5694	7308	3893	2569	3962	5412	3483
Bandwidth	750	1000	1250	741	750	1000	1250	896
Violent crime clearance rate	-0.201	-0.207	-0.091	-0.086	-0.121*	-0.102	0.023	-0.051
	(0.182)	(0.172)	(0.143)	(0.144)	(0.074)	(0.079)	(0.058)	(0.095)
Clusters	43	63	82	74	43	63	84	24
Observations	2371	3281	4317	4011	2007	2985	3997	1099
Bandwidth	750	1000	1250	1134	750	1000	1250	521
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the effect of the merit system mandate on crime reporting and police performance for the 1990 census experiment. The table presents RD estimates on crime reporting (panel (a)), crime rates (panel (b)) and clearance rates (panel (c)) for the sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Monthly crime report missing is a dummy equal to one if the department did not submit a report for the month, crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1980 to 1989 for states with mandates based on the federal population census only and 1980 to 1987 for states with mandates based on federal, state or municipal census. Post-treatment years are 1990 to 1999 for all states. Variation in treatment status is from the 1990 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Table C-22: Effect on reporting, crime and clearance rates for the 2000 census experiment

Sample	pre-treatment				post-treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (a): reporting								
Monthly crime report missing	0.224**	0.154**	0.072	0.168	0.068	0.018	0.017	-0.013
	(0.089)	(0.075)	(0.065)	(0.106)	(0.049)	(0.043)	(0.047)	(0.033)
Clusters	59	81	109	39	59	81	109	24
Observations	5892	8136	10860	3816	7080	9720	13200	2880
Bandwidth	750	1000	1250	545	750	1000	1250	316
Panel (b): crime rates								
Log(property crime rate)	-0.372*	-0.415**	-0.382**	-0.386*	-0.597**	-0.470*	-0.465**	-0.577
	(0.201)	(0.188)	(0.165)	(0.217)	(0.296)	(0.252)	(0.232)	(0.353)
Clusters	57	78	105	48	32	45	62	23
Observations	3893	5694	7308	7117	2569	3962	5412	3073
Bandwidth	750	1000	1250	1221	750	1000	1250	803
Log(violent crime rate)	0.076	-0.070	-0.027	0.097	-0.504***	-0.274**	-0.406***	.
	(0.254)	(0.232)	(0.203)	(0.273)	(0.141)	(0.129)	(0.151)	.
Clusters	58	79	106	44	30	43	60	.
Observations	2371	3281	4317	4085	2007	2985	3997	.
Bandwidth	750	1000	1250	1164	750	1000	1250	.
Panel (c): clearance rates								
Property crime clearance rate	0.081	0.055	0.047	0.047	0.131	0.073	0.091	0.131
	(0.065)	(0.057)	(0.048)	(0.053)	(0.102)	(0.086)	(0.074)	(0.102)
Clusters	57	78	105	83	32	45	62	32
Observations	3893	5694	7308	3893	2569	3962	5412	3483
Bandwidth	750	1000	1250	741	750	1000	1250	896
Violent crime clearance rate	0.073	0.009	0.080	0.253**	0.024	-0.007	0.020	-0.055
	(0.087)	(0.071)	(0.060)	(0.106)	(0.130)	(0.100)	(0.087)	(0.085)
Clusters	58	79	106	35	30	43	60	9
Observations	2371	3281	4317	4011	2007	2985	3997	1099
Bandwidth	750	1000	1250	1134	750	1000	1250	521
State-month FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The tables shows the effect of the merit system mandate on crime reporting and police performance for the 2000 census experiment. The table presents RD estimates on crime reporting (panel (a)), crime rates (panel (b)) and clearance rates (panel (c)) for the sample of pre-treatment years (columns 1 to 4) and post-treatment years (columns 5 to 8). Monthly crime report missing is a dummy equal to one if the department did not submit a report for the month, crime rates are crimes per 100,000 people and clearance rates are number of crimes cleared by arrest over total number of crimes. Pre-treatment years are 1990 to 1999 for states with mandates based on the federal population census only and 1990 to 1997 for states with mandates based on federal, state or municipal census. Post-treatment years are 2000 to 2009 for all states. Variation in treatment status is from the 2000 census experiment. The coefficients are estimated using locally linear regression and a uniform kernel for four different bandwidths: 750, 1000, 1250 and an outcome and sample specific MSE-optimal bandwidth. Standard errors clustered at the municipality level are shown in parentheses. State-month fixed effects are included in all columns.

Appendix D - Municipal merit system legislation

Procedure followed to identify merit system mandates

This section documents the procedure followed to identify the legislation on municipal merit systems and choices made in the final definition of the sample. The procedure was conducted separately for two time periods because of different primary source availability. First, I performed a search until 1940. Second, I extended the legislative review from 1940 to 2000.

Legislation until 1940

1. I exclude states which according to the Civil Service Agencies census of 1940 either did not have municipal civil service boards or did not have municipal civil service boards with legal base in State statues or constitutions. This excludes 13 states.

2. For the remaining states, I search through legislative records (in particular Session Laws and Statutes on HeinOnLine) to identify the specifics of civil service reforms and use this information to classify the reform. The search is conducted as follows: I first identify any legislation introducing merit systems by searching all session laws 1900-1940 for keywords such as "civil service commission", "merit system", "board of police and fire commissioners". Once I identify the specific wording on the reform for the state, I proceed searching session laws with the appropriate wording. When the legislation changes over a few years (in particular, if the threshold is changed over a 1 to 3 years periods), I consider the final legislation. Utah and Wisconsin villages are the ones affected.

3. I classify whether:

   - The reform was introduced by the legislature but was city specific;
   - The reform imposed a mandate;
   - The reform took a population threshold form. If yes, I also classify whether the legislation directly imposes the reform for municipalities above a particular threshold or it imposes the reform for classes/types of municipalities that in turn are defined based on population thresholds;
   - The population thresholds were higher than 15,000, which suggests that they were targeted to specific cities (and also that there are not going to be cities around the discontinuity).

Legislation from 1940 to 1990

1. I take a snapshot of the legislation at three different points in time: 1940, 1978 and 1993. The information for 1940 is based on my previous 50 state survey; the information for 1978 and

1990 is based on Hill (1978, 1990).

2. To ensure that there is no state missing, if a state is not reported having a mandate for a municipal merit system in 1978 or 1993, I perform an additional check looking at state statutes. For each of these states:

   - I access the oldest statute available through the Historical Statutes on WestLaw (or 1990 or closest year to 1990 available) and/or the current statute and perform the following keyword searches in the statutory text:
     - (Board /s police) /p (municipal! or cit! or town!) % "state police"
     - "merit system" /p (municipal! or cit! or town!)
   - Also, I search through the Index of the statute and skim through the following entries to identify whether there is specific legislation on merit systems for cities and if so, what is the content of the legislation:
     - Municipalities;
     - Civil service;
     - Police.

3. For the states that are reported in 1940 to have legislation of the relevant form or to have a mandate for a merit system to be instituted in cities (not restricted to legislation for cities above certain population) in 1978 or 1993:

   - First, I identify the text of the legislation. I proceed as follows:
     - I use West Law to identify the wording of the legislation and references in the State Session Laws. This covers 1990 and current statutes. I mainly use the references in the secondary source.
     - For the states for which I cannot find a reference, I use the reference given in the secondary source and look up the historical state statutes around 1980 on microfiches (since these are non-searchable I only check the specific reference reported in the secondary source).
   - If I identify that at any of these points in time the legislation the form I am interested in I use Session Laws to get the details.

Final state sample selection

This identifies a set of states potentially in the sample. From these, I select the final set of states in the sample based on the following considerations separately for the main analysis and the historic census analysis (this is because they are based on different census experiments).

1. Historic census analysis (1900, 1910, 1920 and 1930 census experiments).

- Potential sample: Arkansas, Iowa, Montana, Ohio, Utah, West Virginia, Wisconsin cities.
- Final sample: Iowa, Montana, Ohio, West Virginia, Wisconsin cities.
  - Arkansas is excluded as there is no first stage in the 1930 census experiment.
  - Ohio is excluded as tenure is granted both to places above and below the threshold.
  - Utah is excluded as there is no first stage in the 1920 and 1930 census experiments (and there are no cities around the threshold).

2. Crime analysis

   - Potential sample: Alabama, Arizona, Illinois, Louisiana, Montana, Nebraska, Ohio, West Virginia, Wisconsin cities and Wisconsin villages.
   - Final sample: Arizona, Illinois, Louisiana, Montana (only for outcomes measured before 1975), Nebraska, Ohio, West Virginia, Wisconsin cities and Wisconsin villages.
     - Alabama is excluded as the legislation does not specify what is required for a city to institute a merit system and, in particular, it does not require the removal of the power to appoint law enforcement officers from the political authority of the city.
     - Ohio is excluded as tenure is granted both to places above and below the threshold.

Legislation for states included in the sample

Arizona

History of the reform

- Mandate for civil service merit system for municipal law enforcement officers introduced for municipalities 15,000+ which have a full-time police department of more than 15 men in 1969.[1] Current statutes include amendments post-2000 but are otherwise the same.

Content of the reform

- Merit system council:
  - 5 members;
  - Appointed by the governing body of the city;
  - Overlapping 5 years terms;
  - No more than three members shall belong to the same political party. All members shall be persons having recognized knowledgeable interest in the merit principles of personnel administration. Members cannot be elected or appointed to public office.

[1] More precisely, the 1969 act mandates the law enforcement merit system for all cities and towns, with the exception of cities and towns with population of less than 15,000 inhabitants or with a full-time police department of less than 15 men. According to UCRs cities at the discontinuity have more than 15 policemen.

- Provisions:
  - Duties of the merit system council:
    * Classifying all positions in the police department and fixing standards and qualifications for classified positions;
    * Providing a plan for selection, appointment, retention and separation or removal from service by resignation or dismissal of all classified law enforcement officers.[2];
    * Providing a plan for promotion of law enforcement officers (promotions should be based on competitive examinations);
    * Hearing and reviewing appeals from any order of the department head in connection with suspension, demotion, or dismissal of a classified law enforcement officer.
  - Previous employees are grandfathered into the reform without examinations.
- Chief of police:
  - Whether the chief of police is covered by the provisions depends on the classification of the council.
- Additional notes:
  - Each municipality subject to the act can either institute its own council or use the services of the county merit system council.

References

- Merit system: Laws 1969, Ch. 102 and A.R.S. T. 38-1001 et seq.;
- Non-civil service appointments: A.R.S. T. 9-240 and A.R.S. T. 9-274.

Illinois

History of the reform

- Possibility to institute board of fire and police commissioners introduced for cities 7,000+ and 100,000- in 1903. Mandate for cities 15,000+ instituted in 1949. Threshold lowered to 13,000+ in 1951 and 5,000+ in 1957.

Content of the reform

[2] Even though the act does not directly institute competitive examinations, in Taylor vs. McSwain (1939), as cited in Hamilton vs. City of Mesa (1995): "A merit system is defined to include the following: the appointment of all employees who come under the system is made on the basis, and as the result, of open and competitive examinations arranged to determine which of the applicants for the position is best fitted to perform its duties, regardless of political affiliations, or past record, and that once an appointment is made, removal from the position should be based only on unfitness for the work for one reason or another, and not upon personal considerations."

- Board of Fire and Police Commissioners:
  - 3 members;
  - Appointed by the mayor of the city with the consent of the city council or by the president of the village or incorporated town with the consent of the board of trustees;
  - Overlapping 3 years terms;
  - No nominations by the mayor or president in the last 30 days of his mandate;
  - One shall be a representative citizen of the employee class, one shall be a representative citizen of the employing class, one shall be a representative citizen not identified with either the employing of the employee class;
  - No more than two members of the board may belong to the same political party.
- Provisions:
  - Duties of the board:
    * Appoint all officers and members of the department;
    * Hold examinations.
  - All applications for a position in the police department are subject to an examination that is public, competitive and open to all applicants. Appointments should be made in order of relative excellence as determined by the examination.
  - Promotions should be made from members of the department through a competitive examination. Promotions should be from the top three applications.
  - Dismissals are only permitted for just cause and after an opportunity to appeal has been granted.
  - Previous employees are grandfathered into the reform without examinations.
  - Publicity is required for all rules made by the board and examinations.
- Chief of police:
  - By default the commission also nominates the chief (but can be changed by ordinance).
- Other notes:
  - In municipalities not under the act, the power to appoint a city police officer is vested in the mayor with the approval of the council. Policemen can be discharged with or without cause.

References

- Merit system: Laws 1961, p. 576, § 10-2.1 et seq. and 65 ILCS 5/10-2.1-1 formerly cited as IL ST CH 24 § 10-2.1-1;
- Non-civil service appointments: 8 Ill. Law and Prac. Cities, Villages, Etc. § 139.

Iowa

History of the reform

- Mandate to institute a board of fire and police commissioners for cities of the first class introduced in 1907. Threshold lowered in 1909. Mandate to introduce a board of fire and police commissioners introduced for all cities 8,000+ with a paid fire or police department in 1917.

Content of the reform

- Civil Service Commission:
  - 3 members;
  - Appointed by the mayor of the city with the consent of the city council;
  - Overlapping 3 years terms;
  - The commissioners must be citizens of Iowa and residents of the city for more than five years next preceding their appointment, and shall serve without compensation. No person while on said commission, shall hold or be a candidate for any office of public trust.
- Provisions:
  - Duties of the board:
    * Hold examinations yearly and when necessary for appointments and promotions.
  - Appointments are conditional upon probation. The ultimate power of appointment in in the fire and police chiefs with approval from city council.
  - Examinations are to be used to determine eligibility lists to be used for appointment.
  - Promotions should be made from members of the department.
  - Dismissals are only permitted for just cause and after an opportunity to appeal has been granted.
  - Previous employees are grandfathered into the reform without examinations (the only exception is the chief of police).
  - Employees are prohibited from campaign contributions.
  - Political activities (taking advantage of civil service position) are prohibited to employees.
- Chief of police:
  - The chief of police is not covered by the provisions. The appointment is made by the political authority but can only be made from the chief of police eligibility list.

- Other notes:
  - The current version of the act mandates using the federal census of 1980.
  - In municipalities not under the act, the mayor has the power to appoint policemen (though this has to be provided for by an ordinance of the city council).

References

- Merit system: I.C.A. T. IX, Subt. 4, Ch. 400;
- Non-civil service appointments: IA ST § 363.40 and 1973 WL 324501 (Iowa A.G.).

Louisiana

History of the reform

- Mandate for fire and police departments introduced for municipalities 50,000+ in 1920 and for cities 13,000+ in 44. Threshold lowered to 7,000 in 1964.

Content of the reform

- Fire and Police Civil Service Board:
  - 3 members;
  - Appointed by governing body of the municipality;
  - Overlapping 3 years terms;
  - Members must be residents of the municipality and not be member of political organizations. One is nominated by the governing body upon its own nomination, one is appointed from a list of two nominees from an institution of higher education, one should be elected by the members of the police and fire department.
- Provisions:
  - Duties of the board:
    * Create eligible lists;
    * Conduct investigations in case of wrongdoing and make decisions on eventual disciplinary actions upon request of the appointing authority;
    * Grant and administer appeals procedures.
  - Appointments and promotion are to be made upon certification based on competitive examinations. The appointing authority (check) makes the appointment from the list provided by the commission.

  - Dismissals are permitted for just cause.
  - Political activities are prohibited to employees.

- Chief of police:
  - The chief of police is not under civil service.

- Other notes:
  - In municipalities not under the act, the mayor is in control of the department and has the power to appoint and remove policemen. The current version of the code includes the possibility for municipalities to have an elected chief of police (in which case he makes suggestions for hiring and promotions). In the historic version of the code, the marshal his elected and has control over the policy of the department while the mayor is in charge of appointments.

References

- Merit system: Acts 1964, No. 282, § 1 and LSA-R.S. 33:2531 et seq.;
- Non-civil service appointments: LSA-R.S. 33:404 and General Statutes of the State of Louisiana 1939 2:5365 and 2:5422.

Montana

History of the reform

- Mandate for police commission introduced for all cities of the first class (10,000+) in 1907. Mandate extended to all cities of the second class (5,000+) in 1947 and to all cities in 1975. Civil service commission mandated for all municipalities under the municipal commission-manager form of government in 1911 and 1917 respectively.

Content of the reform

- Police Commission:
  - 3 members;
  - Appointed by mayor or city manager;
  - Overlapping 3 years terms;
  - Members shall have the qualifications required by law to hold a municipal officer therein.

- Provisions:

  - Duties of the commission:
    * Hold examinations and certify eligibility of applicants.
  - The power of appointment is in the mayor but in cities where a police commission exists the mayor may appoint only individuals who have passed the examination provided by the commission.
  - There is no inherent right to indefinite tenure given to policemen but policemen can be removed for cause (when they are remiss in their duties).
  - Political activities (participating in political conventions and soliciting votes) are prohibited to employees.

- Chief of police:
  - The chief of police is covered by the provisions. The appointment is made by the political authority but can only be made from the chief of police eligibility list.

- Other notes:
  - Important for the classification: municipalities 2,500+ are cities, 1,000 to 2,500 can be either cities or towns and 1,000- are towns (in 1947).
  - In cities without a police board, the mayor (or corresponding governing authority) has power over the police department.

References

- Merit system: Laws 1907, Ch. 136 and Mont. Code Ann. 1947 § 11-1801 et seq. and Mont. Code Ann. 1978 § 7-32-4151.
- Non-civil service appointments: Mont. Code Ann. 1947 § 11-1801 et seq.

Nebraska

History of the reform

- Mandate for all members of fire departments of municipalities 5,000+ and 40,000- introduced in 1943. Expanded to police departments of the same municipalities in 1957.

Content of the reform

- Civil Service Commission:
  - 3 members;
  - Appointed by mayor or authority who previously appointed the chief of police;

- Overlapping 6 years terms;
- No person shall be appointed a member of such commission who is not a citizen of the United States, a resident of such city for at least three years immediately preceding such appointment, and an elector of the county wherein such person resides.

• Provisions:
  - Duties of the commission:
    * Hold tests and create eligible lists. Appointments should be made following the eligible list.
  - All appointments to and promotions in such departments shall be made solely on merit, efficiency, and fitness, which shall be ascertained by open competitive examination and impartial investigation.
  - Dismissals are only possible for just cause after an opportunity to appeal has been granted.
  - Previous employees are grandfathered into the reform without examinations.

• Chief of police:
  - The commission also nominates the chief.

• Other notes:
  - In cities which do not adopt the act, all police officers are appointed by the mayor and council and can be removed anytime by the mayor.
  - The threshold corresponds to the threshold classifying cities of the first class.

References

- Merit system: Laws 1957, LB 305, Neb.Rev.St. § 19-1825;
- Non-civil service appointments: Neb.Rev.St. § 19-1825.

West Virginia

History of the reform

- Before 1937 civil service for specific (generally large) cities, provided in charters approved by state legislation. In 1937, civil service mandated for paid police departments of municipalities with population 5,000+. In 1969, mandate only for cities of first and second class (10,000+). Cities that already have a civil service commissions are to keep it. Cities of the third class which do not have civil service already may introduce it with an election. Current legislation has the same form.

Content of the reform

- Civil Service Commission:
  - 3 members;
  - One appointed by the governor, one appointed by the local fraternal order of the police, one appointed by the local chamber of commerce or if there is not one by a business man's association;
  - Overlapping 5 years terms;
  - Commissioners should be residents of the city, no more than two of them shall be from the same political party and no commissioner should hold an office.
- Provisions:
  - Duties of the commission
    * Make rules and regulations providing for examinations for positions in police departments and for appointments and promotions;
    * Hold examinations and create eligible list;
    * Hear and review appeals for dismissals or disciplinary actions.
  - Appointments are made by the appointing officer from three names certified by the civil service commission from the eligible list.
  - Promotions should be made internally whenever possible.
  - Dismissals are not acceptable for just cause ("which shall not be religious or political").
  - Political activities (taking advantage of civil service position) are prohibited to employees.
- Previous employees are grandfathered into the reform without examinations.
- Chief of police:
  - The chief is not covered by these provisions.
- Other notes:
  - In cities which are not under the provisions of the reform, the police department is under the authority of the mayor.

References

- Merit system: Acts 1937, c. 57, W. Va. Code, § 8-10-14;
- Non-civil service appointments: W. Va. Code, § 8-10-1.

Wisconsin, cities

History of the reform

- In 1897, civil service introduced for all cities of the second and third class. In 1909, extended to cities of the fourth class. Shortly after, in 1911, civil service mandated for cities of the fourth class with population 4,000+. Cities of the fourth class with population 4,000- may introduce a civil service board with an election (later: by ordinance). In 1933, introduced for cities under city manager form of government. The legislation survived in essentially the same form until today, with the exception of the inclusion of provisions providing for the possibility for contracting law enforcement services across local governments introduced starting from 1980.[3]

Content of the reform

- Board of Police and Fire Commissioners:
  - 5 members;
  - Appointed by mayor;
  - Overlapping 5 years terms;
  - No more than 3 members of the board can belong to the same political party.
- Provisions:
  - The board of police and fire is the only mechanism for the appointment, removal or disciplining of policemen.
  - Duties of the commission:
    - Appoints the chief;
    - Approves all appointments and promotions (which have to be made from eligible lists provided by examinations);
    - Recommends salary decreases to the common council;
    - Suspend or dismiss members of the police force for cause;
    - Receive charges and holds disciplinary hearings.
  - Under optional provisions the board also has the power to supervise the police force, prescribes rule for its management and contracts for police department purchases.
  - The chief appoints policemen subject to the board approval. Appointments are from eligibility lists. Promotions are to be made from within the department.

[3] The amendments allowing to contract protective services with a city, another village or the county does not matter for my design. If a local government unit were to contract out their law enforcement services they would not appear in the UCR data.

  - Previous employees are grandfathered into the reform without examinations.

- Chief of police:
  - The chief of police is covered by the provisions and appointed by the board. The appointment is made by the political authority but can only be made from the chief of police eligibility list.

- Other notes:
  - In cities without a police board, the mayor has power over the police department.
  - In 1979 protection from political dismissals was granted to all law enforcement employees.
  - The legislation continues to today in a very similar form, with the exception of provisions allowing for out-contracting of police services starting from 1985.

References

- Merit system: W.S.A. 62.13;
- Non-civil service appointments: W.S.A. 62.09 § 8;
- Other: W.S.A. 164.

Wisconsin, villages

History of the reform

- Mandate for villages with population 5,000+ introduced in 1937. In 1941, threshold increased to 5,500. In 1979, the possibility to contract protective services with a city, another village or the county is introduced. In 1981, the possibility to create a joint police department with another city, village, town or county is introduced. If a village 5,500+ creates a joint police department it should appoint a joint board of police and fire (threshold lowered to 5,000 in 1981).

Content of the reform

- The content of the legislation is the same as the one for cities.

References

- Merit system and non-civil service appointments: W.S.A. 61.65.

Procedure followed to identify provisions implying policy discontinuity at the same threshold

To check whether there is overlapping legislation for the states in my sample:

1. I search through the oldest statute available on WestLaw using the threshold in the form appearing in the civil service legislation (e.g. for Iowa "eight thousand").
    - When the threshold is expressed as a number I perform the search excluding the number + dollars (e.g. for Iowa "eight thousand" % "eight thousand dollars").
    - If there is overlap with city classification, I search for the classification.
2. I search through State Session Laws using the threshold in the form appearing in the civil service legislation.

Appendix E - Uniform Crime Reports

Data cleaning

The source I use for the crime data are the Uniform Crime Reports Return A data files distributed by the FBI. As noted by Evans and Owens (2007), "the UCR data are essentially unedited by the FBI. As a result, the data requires thorough cleaning before use." In this Appendix, I discuss the steps I take to clean the data and, in particular, how I identify missing data.

The main issue with the data files is that a zero observation can be either a true zero or missing. As noted by Maltz (2006), zeros can mean that no crimes occurred in that month or that:

1. The department had not yet begun reporting data to the FBI;
2. The department reported its crime data through another agency;
3. The data were aggregated and reported on lower frequencies (e.g. quarterly, annualy);
4. The department did not report data for one month and compensated for the omission by reporting in the next month;
5. The department did not submit data for that month.

The original files contain indicators flagging these issues, but they are not always accurate. First, I use these indicators and set to missing all observations that are flagged to be indeed missing. Since I am interested in monthly data, I also set as missing observations that include information for more than one month. Second, I also include the following additional corrections:

1. I set a zero observation to missing for all months before the first non-zero non-missing report is submitted;
2. I set a zero observation to missing if the department only reports zeros for that year;
3. I set a zero observation to missing if the department only reports zeros or missing for that year.

Appendix F - Police Officers

Finding police officers in the census

I identify police officers based on their occupation, industry and class of worker. I assign them to the police department of the municipality they are enumerated in. Identifying police officers is conceptually straightforward but requires using numerous uncleaned string variables.

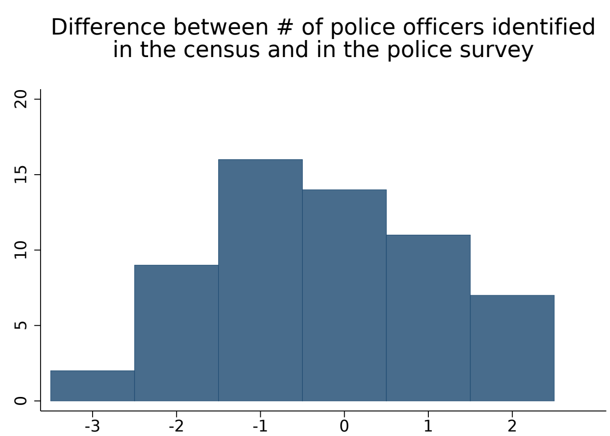
I clean the data and identify police officers using the following procedure:

- Place identifiers are often reported wrong in the data, either because they include non incorporated areas / other municipalities or because they fail to include significant fractions of the population. I carefully clean the place identifiers to match the number of inhabitants in each municipality as reported by the official census reports. I especially rely on enumeration district descriptions from http://stevemorse.org/ and on actual census schedules available through https://familysearch.org). I clean (up to a 100 inhabitants errors) the place identifier for the vast majority of municipalities.
- I find police officers by first defining the largest possible group of potential police officers based on occupation and then refine the matches based on industry and workers' class.
- For the 1910 to 1930 census:
  - An individual is a potential police officer is his/her occupation contains the following strings: polic, patrol, traffic officer, detective, officer, marshall, captain, watch, sergeant, chief.
  - I exclude potential police officers whose industry does not contain the following strings: city, polic, villag, town, municipal. Among these potential police officers, I further exclude those whose industry includes the following strings: merchan, shop, state, fire, plant.
  - I check industry and occupation strings for the set of potential police officers and exclude individuals with occupation or industry clearly not corresponding to a police officer.
  - I exclude from the sample of police officers individuals who report being employed in the private sector or being self-employed.
- For the 1940 census:
  - An individual is a potential police officer is his/her occupation contains the strings "police", his/her occupation code refers to "Policemen and detectives, government" or "Policemen and detectives, except government" and his/her industry code refers to "State and local government".

- I then check occupation strings occurring more then once for individuals whose occupation contains the strings "police" or whose occupation code refers to "Policemen and detectives, government" but whose industry code refers to industries other than "State and local government". I include in the set of potential police officers individuals with a relevant occupation string.
- I check occupation strings occurring more then once for the set of potential police officers and exclude individuals with occupation or industry clearly not corresponding to a police officer. I exclude all those with occupation strings occurring only once as they were not hand checked.

I validate the procedure for 1940 by comparing the number of police officers I find in the census and the number reported in a survey of police departments of municipalities with more than 2,000 inhabitants published by the League of Wisconsin Municipalities in 1939. I am able to match the size of most departments and mismeasure by more than two police officers only in one case. The following figure shows a histogram of the difference:

Figure F-1: Validation of the procedure using survey of police departments



Cross census linking

To identify new hires I link police officers across censuses using the following matching procedure (similar to Nix and Qian, 2015):

1. I define potential matches as police officers who are a first and last name Phonex match for the police officer, live in the same municipality and are within a 5 year window from the age predicted based on age at the census in which I identify them as police officers.
2. If there is at least one potential match, I check for perfect matches. A potential match is perfect if the spelling of the first and last name corresponds exactly.

3. If there is one perfect match only, I keep that as the final match.

4. If there is more than one match, I keep the one with the closest age. If there are multiple matches with the same age, I randomly pick one of the perfect matches as the final match.

5. If there is no perfect match, I search for Soundex and Jaro-Winkler (JW) matches. A potential match is a Soundex match if the Soundex translation of the first and last name both correspond. A potential match is a JW match if the JW distance of the first name plus the JW distance of the surname is larger than 1.6.

6. If there is one perfect match only, I keep that as the final match.

7. If there is more than one match, I keep the one with the closest age. If there are multiple matches with the same age, I randomly pick one of the matches as the final match.

Appendix G - Mayors

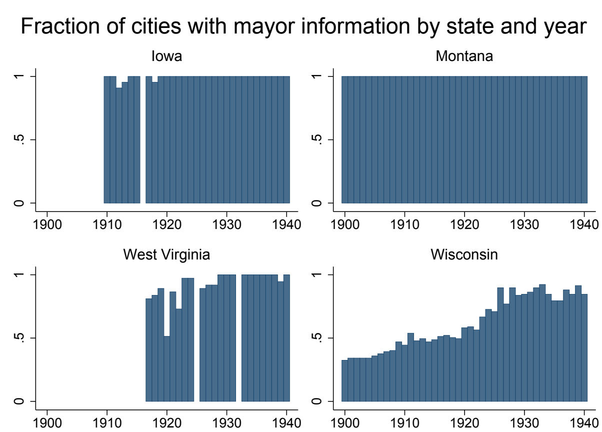
Data collection

I collected information on mayors who served in municipalities within a 3,000 bandwidth in the 1900, 1910, 1920 or 1930 census experiment for the 1900 to 1940 period using the following data sources:

- Data for Iowa and West Virginia were collected from official directories of local government officials published by the state government.
- Data for Montana were collected from the internet and searching through historical newspapers on newspaperarchive.org.
- Data for Wisconsin were collected from the internet, through a phone survey of city clerks and from county directories.

The following figure reports the fraction of municipalities for which I have information on the mayors' name by state and year:

Figure G-1: Data availability by state and year



Matching

I match the mayors in the 1910, 1920, 1930 and 1940 census using the following matching procedure (similar to Nix and Qian, 2015):

1. I define potential matches as white males who are a first name and last name Phonex match for the mayor and live in the municipality.

2. If there is at least one potential match, I check for perfect matches. A potential match is perfect if the spelling of the first and last name corresponds exactly.
3. If there is one perfect match only, I keep it as the final match.
4. If there is more than one perfect match, I check whether the individuals matched have the same ethnicity.
5. If they do, I randomly pick one of the perfect matches as the final match. If they do not, there is no match.
6. If there is no perfect match, I search for Soundex and Jaro-Winkler (JW) matches. A potential match is a Soundex match if the Soundex translation of the first and last name both correspond. A potential match is a JW match if the JW distance of the first name plus the JW distance of the surname is larger than 1.6 if I have both the full first name or the JW distance of the last name only is larger than 0.8 if I only have first name initial.
7. If there is one match only, I keep it as the final match.
8. If there is more than one match, I check whether the individuals matched have the same ethnicity.
9. If they do, I randomly pick one of the perfect matches as the final match. If they do not, there is no match.
10. Finally, I exclude individuals who are either too young or too old to be a match. An individual is too young to be a match if the implied age at the beginning of the term as mayor is less than 20; an individual is too old to be a match if the implied age at the end of the term as mayor is more than 70.

The procedure gives me at most one perfect match or at most one Soundex and JW match for each mayor. Each procedure gives a unique match, but Soundex and JW match are potentially different. This is a problem only if the Soundex and JW matches have different ethnicity, which is never the case in my sample. I assign the ethnicity of the perfect match if there is one, and of either the Soundex or the JW match if there is not. Finally, if the matches in the different censuses yield different nationalities, I keep them all.

Table G-1: Descriptive statistics of the mayors' matching procedure by state

	# mayors	# mayors matched	% matched	% foreign
Total	3121	1275	0.409	0.405
Iowa	262	59	0.225	0.373
Montana	70	37	0.529	0.622
West Virginia	359	103	0.287	0.126
Wisconsin cities	855	401	0.469	0.666

I am able to match around 40% of the mayors. The fraction of mayors matched is lower for Iowa and West Virginia, two states for which the source does not report the full first name but only the first name initial.