

THE REPRESENTATIONAL CONSEQUENCES OF MUNICIPAL CIVIL SERVICE REFORM

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Abstract

A prominent argument holds that the chief aim of municipal civil service reform in the United States was to dislodge the overrepresentation of recent immigrants in city government. Using new data on all municipal employees from 1850–1940, and employing three research designs, we detect no evidence that the share of local government jobs held by foreign-born whites decreased following the introduction of reforms. Instead, we show that foreign-born whites—Irish immigrants in particular—experienced substantial gains in local government employment, concentrated in blue-collar occupations in small and medium sized municipalities. Our results call for a revisionist interpretation of Progressive Era reforms by questioning generalizations drawn from the experience of the largest cities in the United States. For most municipalities, instead, civil service reform in fact opened avenues to representation for members of foreign-born constituencies who had previously been locked out of government jobs.

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Questions of representation lie at the heart of debates over how governments ought to staff their bureaucracies. In theory, the merit system—and its emphasis on standardized recruitment through examinations—enables members from all groups and classes to win coveted employment. Theodore Roosevelt, a prominent supporter of municipal civil service reform, wrote, for instance, that “[t]he system of competitive examinations” for local government jobs ought to be adopted because of the end “it puts to discrimination for or against a man because of his religious convictions” (Roosevelt 1895). In contexts with a history of favoritism for certain groups, the merit system might therefore represent a major step towards achieving representational parity.

In practice, however, the merit system can also lead to an unrepresentative public sector, as privileged groups outperform marginalized groups on entrance examinations and go on to staff government jobs at disproportionately high rates. Some scholars have argued that the unstated intent of civil service reform was to purposefully advantage middle- and upper-class, native-born whites in the competition for local government jobs (?, ?, p. 41; Hofstadter, 1955, p. 9; Shefter, 1993, p. 77-81). According to this view, reforms were championed by middle- and upper-class whites who chafed against the spoils system, complaining of corruption and inefficiency while also objecting to the rising political influence of immigrant groups. Pushing for reform in the name of identity-neutral concerns over “good governance,” these comparatively well-educated constituencies are thought to have advocated for reforms that relied on written examinations to select government employees in the hopes that they would also see representational gains under this system.¹

In this article, we draw a distinction between the intent of civil service reform and its representational effects. We challenge the assumption that municipal civil service reform in the United States did, in effect, benefit native-born whites at the expense of foreign-born whites, as its advocates had surreptitiously anticipated. We argue that this view stems from a narrow focus on the largest cities—settings in which foreign-born whites were numerous enough to forge electorally dominant coalitions that captured local public administration. But many immigrants settled in smaller municipalities in which they constituted a smaller share of the local population, and in which native-born constituencies guarded their comparatively strong position by wielding the discretionary recruitment of public servants. As the diffusion of civil service reforms reached these

¹This view is recounted in several introductory textbooks in political science, which we reference in Supplementary Material in Section A. Particularly emblematic is Kernell et al. (2017, 501) who write, “[a]lthough ostensibly aimed at rooting out corruption and cleaning up electoral politics, progressive reforms also were designed to enhance the political clout of the ‘right’ kind of people—educated middle- and upper-middle-class folks like the reformers themselves—at the expense of poor urban immigrants of their leaders ‘of slender social distinction’” See, *inter alia*: (Judd and Swanstrom, 1998, p. 85-86; Lowi et al., 2017, p. 526.)

smaller municipalities—often under pressure from state governments for whom the impetus was the experience of larger municipalities—it opened opportunities for the representation of foreign-born whites who had previously been locked out of government jobs. In other words, we argue that, at least during the Progressive Era, the effect of civil service reform on descriptive representation was critically conditioned by the size of the municipality in which it was adopted.

Our argument hinges on a broader theoretical claim about the relationship between city size, bureaucratic selection, and representation. Owing to the negative externalities of life in large cities (crime, traffic, etc.), these municipalities tend to be expected to perform a wider range of functions, meaning there exists a larger number of jobs to distribute. When these jobs are distributed via the spoils system, marginalized groups thus face a strong incentive to engage in a coordinated effort to capture these wages by trading votes for wholesale employment. These dynamics prevail less in smaller cities, where marginalized groups often comprise smaller shares of the population, meaning that these groups find themselves unlikely to secure unemployment under the spoils system. In sum, we theorize that enclaves of marginalized groups in small and midsize cities are—at least compared to their peers in large cities—poised to benefit more from a shift to the merit system.

To evaluate this argument, we pair data on the race and birthplace of municipal government employees from the full-count census from 1850 to 1940 with data on the timing of municipal civil service reforms to examine changes in descriptive representation in municipal bureaucracy. Our primary analysis uses an event study design to assess the effects of municipal civil service reform on descriptive representation. Here, we find that the share of local government jobs held by foreign-born whites relative to their population share increased in the three decades following the introduction of reforms, with the effects concentrated in blue-collar employment and driven largely by gains among Irish immigrants. We detect no analogous effect of municipal civil service reform on foreign-born whites' descriptive representation in white-collar jobs.

Next, we leverage variation in statewide legislation that mandated the adoption of civil service reform. The timing of municipal reforms was far from random, and may have been associated with changing bureaucratic demographics. By using state-level assignment, we circumvent the particular circumstances of a specific city. Crucially, many of these statutes required that only municipalities above a given population threshold implement changes to the manner in which government employees were recruited. Our second research design thus uses statewide assignment as an instrument to estimate the effect of civil service reform on descriptive representation on an unrestricted sample. The findings of this analysis are directionally identical to those obtained from

the event study: looking at municipalities assigned to adopt civil service reform, the share of foreign-born whites in blue-collar government jobs increased relative to their share in the population. Our third research design exploits the population thresholds in the statewide legislation by implementing a regression discontinuity that compares the effects of reform on a restricted sample of municipalities that are within 15,000 residents of the population thresholds. Here, we detect no evidence that civil service reform had any impact on descriptive representation.

All three analyses show that, contrary to the purported aims of civil service reform, the share of blue-collar local government jobs held by foreign-born whites did not decrease following reforms. What explains these findings, in which municipal civil service reforms evidently failed to achieve their stated aim? Our interpretation is revisionist. We show that, on the eve of civil service reform, many municipal jobs were not held by appointees of immigrant-led machines. Instead, in small-to-medium-sized cities across the United States, we demonstrate that foreign-born whites tended to be underrepresented in municipal government, likely facing discrimination similar to what they experienced in private labor markets. Historical cases studies and existing empirical work have largely overlooked this tendency by focusing on a handful of the very largest cities. Far from dislodging the hypothesized domination of immigrants, the introduction of civil service reform actually bolstered the representation of members from foreign-born constituencies who had previously been locked out of government jobs. Consistent with this argument, we show that our main results are predominantly driven by increases in foreign-born white representation in municipal employment in small-to-medium-sized cities.

Our findings make several contributions. First, and despite the importance of representational considerations in explaining support for the civil service reform movement, scholars have yet to systematically evaluate the consequences of meritocratic recruitment procedures on the composition of those employed in municipal public service.² Instead, studies of municipal civil service commissions have focused primarily on assessing their effects on public services (Lineberry and Fowler 1967; Ornaghi 2019; Rauch 1994). This oversight stands in contrast to work on other Progressive Era municipal reforms and where descriptive representation has been studied as an outcome (e.g., Trounstein and Valdinì 2008) and the descriptive consequences of racial segregation in the federal bureaucracy (?).

Second, this research highlights the extent to which the conventional wisdom drawn from case

²Generally, scholars have examined representation in municipal government (Eisinger 1982; ?), but have not traced a relationship to municipal civil service reform in particular.

studies and quantitative work on the largest cities in the United States may not describe the modal American city. To be clear, our argument does not negate these accounts: in the largest cities, where these studies were often situated, we find that representation of native-born whites in municipal governance increased—and the share of foreign-born whites decreased relative to their population—following civil service reforms. But importantly, by broadening the range of cases under consideration, our findings reveal the conditional nature of the existing theoretical accounts of the causes and consequences of municipal civil service reform.

Theoretical Perspectives on Reform and Representation

A rich body of research spanning political science and public administration emphasizes the importance of a representative bureaucracy (Kingsley 1944). The core theoretical claim uniting these accounts is the idea that, especially for under-served groups, better *descriptive* representation in government institutions enables forms of *active* representation that yield better outcomes for the constituencies that bureaucracies are intended to serve (Meier and Stewart Jr 1992; Selden 2015; Sowa and Selden 2003). The connection between descriptive and active representation is thought to operate through several channels. For one, government institutions that mirror the constituencies they are intended to serve are often perceived as more legitimate and thus better able to engage in the co-production of governance with a diverse body of citizens (Ricucci, Van Ryzin and Li 2016). Second, having a representative bureaucracy appears to enable the full range of diverse attitudes and preferences held by citizens to be reflected in the halls of power, thereby promoting more accountable and responsive policy (Bradbury and Kellough 2008).

Much of the research investigating representation in bureaucracies has focused on race and gender, although not exclusively (e.g. Waldinger 1999). Scholars working in this tradition have documented the extent to which women and racial minorities are critically underrepresented in the public sector institutions (Ricucci and Saidel 1997). A smaller body of research has emerged to examine the ways in which immigrant populations—the members of which may not necessarily be racial minorities—face hurdles to adequate representation, particularly in government service. Chief among the obstacles that undermine adequate representation of recent immigrants in government service are both onerous citizenship requirements and language barriers enacted through competitive examinations, which, in the United States, are almost always held in English (Lewis, Liu and Edwards 2014).

The theory evaluated in this paper builds on the literature on representative bureaucracy by

evaluating how variation in the mechanism by which governments recruit employees affects broader trends in the representation of privileged and marginalized groups in local government employment. In theory, civil service reform—and its emphasis on examinations—enables members from all groups and classes to win coveted employment in government. An early generation of scholarship on civil service reform held that it delivered on its promise to end *de jure* discrimination (Hoogenboom 1968; Van Riper 1976). Van Riper (1963), in a review of mid-century advances in municipal reform, argued that “[i]n most jurisdictions we have gone far to eliminate discrimination not only on the grounds of prior partisan politics but also on the grounds of race, religion, national origin, and sex.” In other words, this first wave of scholarship argued that municipal civil service reform had a salutary effect on achieving the equitable representation of minoritized groups in local government employment.

But a second wave of scholarship has suggested that civil service reform can also lead to an unrepresentative public sector, as privileged groups outperform marginalized groups on entrance examinations and go on to staff government jobs at disproportionately high rates. This *de facto* discrimination is theorized to occur through several channels; here we underscore those that relate to the properties of an examination-based system.³ The first concerns the potentially biased nature of examination questions designed to capture applicant “merit.” The physical examination for the position of bridge keeper in New York City in 1886, for instance, asked examiners whether “the applicant [is] apparently a sound man in all respects,” a gendered and also open-ended inquiry that offers an opportunity for undue biases to enter into the evaluation (Bowker 1886, 18). But, second, civil service examinations call on applicants to take written assessments that often contain questions which individuals from educated backgrounds are better positioned to answer correctly—such as arithmetic and spelling—but which might not necessarily bear directly on the work to be completed, as in the case of blue-collar employment. This tendency—for examination success to reflect existing inequalities—has been shown to be true over and over again in academic settings, and seems likely to be operating in the context of public sector recruitment, as well (e.g., Lewis and Diamond 2015).

We examine these competing expectations in the context of the Progressive Era movement to introduce civil service reform in U.S. municipalities during the late-nineteenth and early-twentieth centuries. At the time, a growing number of foreign-born whites were emigrating to American

³As we discuss below, other features of civil service reform—such as changes to the manner in which employees could be terminated—may have also had impacts on the representation of certain groups in local government bureaucracies.

cities, which created new opportunities for municipal politicians, who harnessed these newcomers as electoral constituencies by offering them jobs in municipal government. In his account of city politics, Dahl (2005, 33–34) explains, “[a]ny political leader who could help members of an ethnic group to overcome the handicaps and humiliations associated with their identity, who could increase the power, prestige, and income of an ethnic or religious out-group, automatically had an effective strategy for earning support and loyalty.” To this end, municipal politicians distributed city jobs to these constituencies: “[t]o obtain and hold the votes, the political leaders rewarded them with city jobs.” The spoils system thus created an ironclad electoral commitment on the part of working-class voters and politicians, which made the outcomes of municipal elections difficult to contest by the typically less numerous middle- and upper-class, native-born white constituencies.

According to this argument, reformers were motivated to press the merit system by the changing demographics of local government offices and the attendant shifts in political power in American cities in the mid-to-late nineteenth century. The case of Boston is illustrative: Miller (2009) argues that “[t]he reformers believed that Irish laborers voted exclusively to secure employment from Boston ward bosses. By eliminating the ignorant laborer’s sole reason to vote, reformers believed he would be rendered impotent and would have no reason to frequent the voting booth.” To reformers, whose leaders were primarily native-born, Protestant whites, the introduction of meritocratic procedures for recruiting municipal employees was both a move towards efficiency and professionalism, but also a tool to reassert their waning political strength. Consistent with this argument, several studies have presented evidence that the municipalities most likely to implement civil service reform were those in which the share of foreign-born whites holding public sector jobs increased the most (Ruhil 2003; Ruhil and Camões 2003; Tolbert and Zucker 1983).

In this article, we develop and evaluate an argument that challenges the prevailing view of civil service reform’s representational effects in Progressive Era municipalities. Our argument hinges on a theoretical shift in perspective that distinguishes the distinct representational consequences of civil service reform in small and large cities. We show that the prevailing view of civil service reform—i.e., that it achieved its intention of dislodging the overrepresentation of foreign-born whites—suffers from a case selection focusing on only the largest cities in the United States. This focus is understandable, as the largest cities in absolute terms tended to be those with the largest proportion of foreign-born whites (see Figure 1). In these settings, foreign-born whites were indeed able to forge electorally dominant coalitions in which they could capture local public administration through the spoils system. The introduction of the merit system, in turn, expanded the likelihood of securing

work in public service to those who had been comparatively locked out—here, native-born whites, among other groups.

But many foreign-born whites settled in hundreds of smaller municipalities in which they constituted a smaller share of the local population. In these cities, the hegemony of native-born whites often remained unchallenged by the influx of foreign-born whites who, in comparative terms, did not pose an electoral threat. Under the spoils system, then, the consequence for foreign-born whites was underrepresentation in local government jobs, as native-born whites guarded their comparatively strong position by wielding the discretionary recruitment of public servants to reward in-group members. Yet, the diffusion of civil service reforms nonetheless reached these smaller municipalities, often under pressure from state governments for whom the impetus was the experience of larger municipalities. Here, the introduction of meritocratic recruitment procedures for local government jobs opened (rather than closed) the door to foreign-born whites, who had, in these cases, been previously locked out.

In addition to the tendency for city size to positively correlate with the proportion of foreign-born whites during the era we study, our theory depends on another more generalizable feature of city size. Namely, compared to smaller cities, larger cities have a larger number of jobs to distribute, as these contexts have both larger budgets and face problems not encountered in smaller municipalities—such as unwieldy public transit networks, traffic congestion, and crime (Post and Kuipers 2018). When these jobs are distributed via the spoils system, then, groups face a stronger incentive to engage in a coordinated effort to capture these wages, at least as compared to smaller cities.⁴

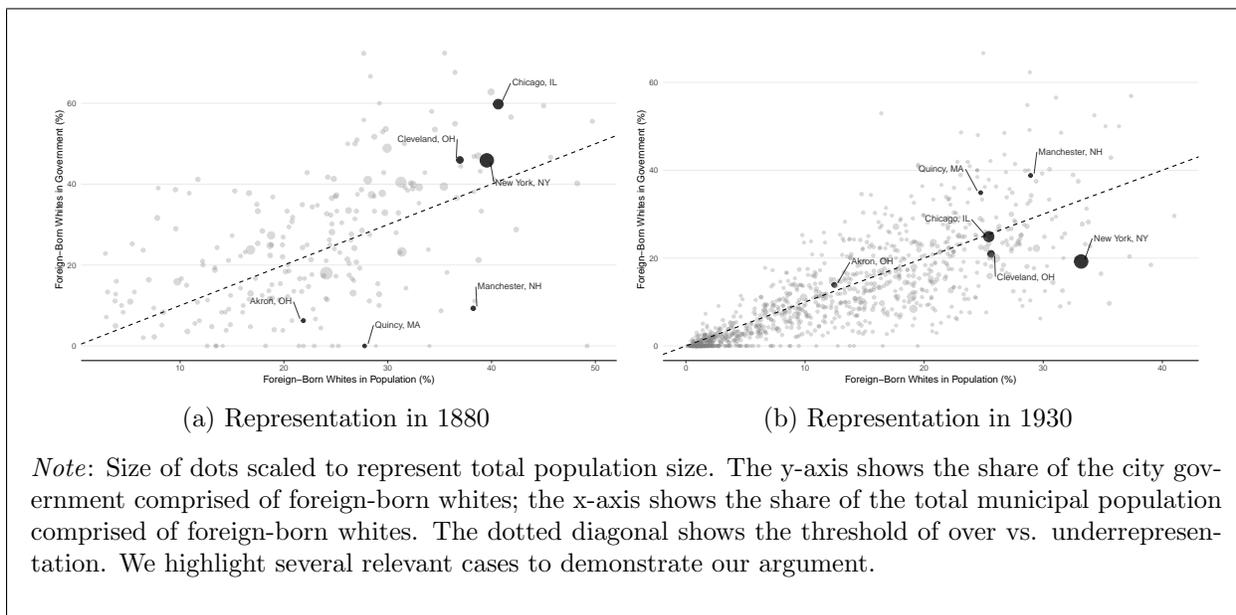
To summarise, we argue that the effect of civil service reform on descriptive representation in Progressive Era United States was conditioned by the size of the municipality. In the largest cities—consistent with the existing literature—we expect that civil service reform led to a decrease in the share of government jobs held by foreign-born whites relative to their share of the population. However, in the far more numerous other cases, we expect the reverse: that civil service reform actually improved the representational standing of foreign-born whites.

As we will argue, Irish-Americans were particularly well-positioned to benefit the most from these reforms in small-to-medium-sized cities—above and beyond other foreign-born whites. As we show in the Supplementary Material in Figure A2, Irish-Americans settled in and made up

⁴One such coordinated effort took the form of Irish “ethnic associations” through which workers banded together to secure employment in spite of a hostile population of native-born whites (Emmons 1989, 7-8).

a significant share of the population of cities outside the major urban centers commonly known to have been captured by immigrant-led machines (c.f., Doyle 1990). In these contexts, Irish-Americans often encountered hostile native-born populations, such that they had little success in securing public sector employment (Emmons 1989, 6). Upon the introduction of municipal civil service reforms, though, two factors set the Irish immigrants apart and enabled them to secure higher rates of public sector employment than other foreign-born whites upon the introduction of civil service reforms. First, the Irish had higher rates of proficiency in English: 84% of Irish spoke English as their primary language, the language in which examinations took place. This was ten percentage points higher than the next immigrant group, giving the Irish a considerable advantage. Moreover, second, Irish immigrants were more likely to be literate (70%), as compared with other immigrant groups such as Germans (60%) and Italians (44%)—again, offering a distinct advantage when it came to obtaining employment through written examinations.

Figure 1—Representation of Foreign-Born Whites in Municipal Government, 1880 and 1930



The case-study trends highlighted in Figure 1 typify our argument. In 1880, before the introduction of municipal civil service reform, some of the largest cities in the United States—Chicago, Cleveland, and New York—reported an overrepresentation of foreign-born whites in local government jobs, vis-à-vis their share of the local population. Meanwhile, in smaller municipalities—such as Akron, Manchester, and Quincy—foreign-born whites were underrepresented. By 1930, all six municipalities had adopted civil service reform and the smaller cities now reported an overrepre-

sentation of foreign-born whites in local government, while the larger cities witnessed the reverse.

We argue that the divergent fortunes of Chicago and Quincy are consequential for our understanding of the effects of civil service reform and how reform is conditioned by city and immigrant group size. Our argument thus rests on a subtle distinction: since we are interested in the effects of reform on governments' composition—rather than individuals' outcomes—our theoretical interest is in the city-level unit of analysis, rather than the individual-level unit of analysis. Our emphasis is motivated by both substantive and methodological concerns. On the first count, our perspective is institutionalist: we wish to understanding the impact of institutional reforms, conditional on various characteristics. On the second count, we follow the advice of Dunning (2012) wherein we treat municipalities as the unit of analysis given that they are the level at which the treatment variable is assigned.

Background and Context

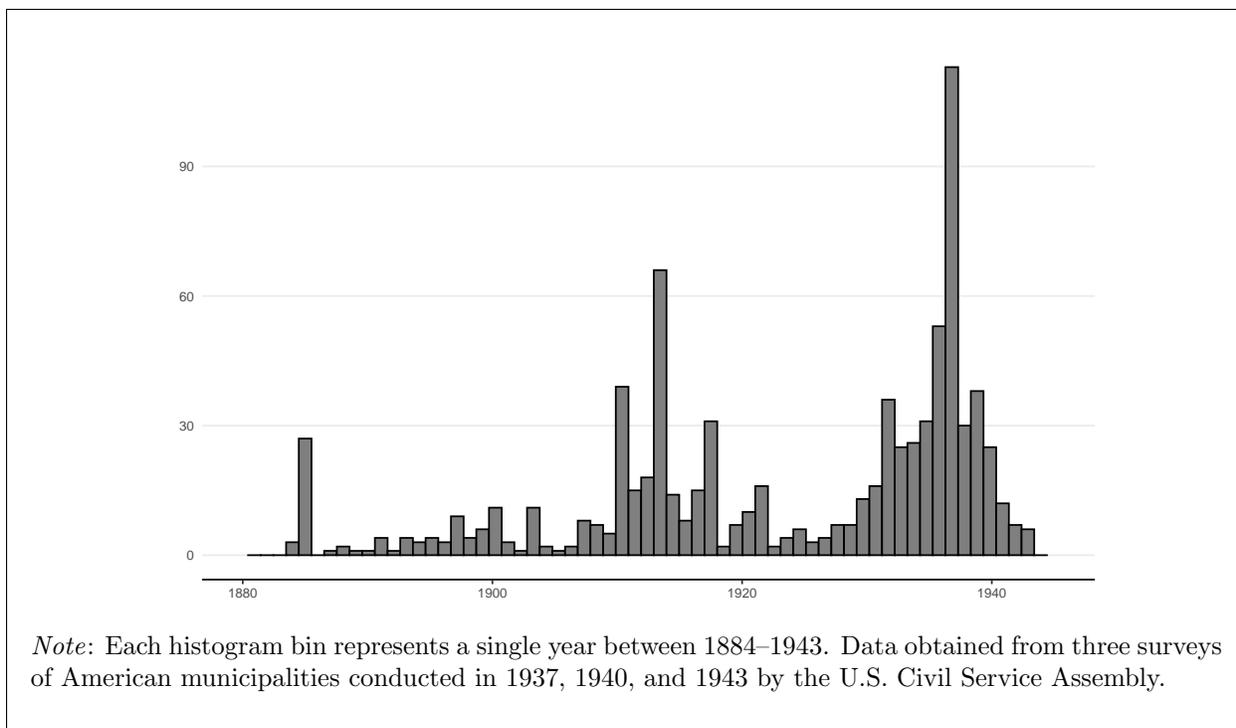
Throughout the early nineteenth century, civil service positions in the federal government were allocated through patronage—doled out to party members and supporters who had contributed to victorious electoral campaigns. By the early 1880s, nearly 130,000 jobs were distributed in this fashion. In 1881, the impetus for reform came when President James Garfield was assassinated by an aggrieved job-seeker who believed he had been wrongly passed over for an appointment despite having contributed to Garfield's presidential campaign. Reformers seized on the swing in public opinion to push for the adoption of the Pendleton Act, passed in 1883, which outlawed the use of patronage in the allocation of some federal civil service jobs. Initially applied to customs officials and post offices, the roll out of merit-based recruitment took effect over the next half century.

The diffusion of civil service reforms at lower levels of government was staggered. The first wave of reforms occurred in the early 1880s, with statewide legislation in New York and Massachusetts. Municipalities slowly followed suit (see Figure 2). Diffusion was not random; larger cities, those with home rule, and cities with at-large councils were more likely to adopt civil service earlier (Ruhil 2003), pointing to elected officials' incentives to resist the end of the spoils system. The diffusion to municipalities was not complete until the 1970s with the intervention of the U.S. Supreme Court. Even then, as late as 1976, arguments in favor of political patronage received considerable support.⁵ The diffusion of civil service reforms to the municipal level occurred through two paths.

⁵Justice Powell, in a dissenting opinion, argued that the use of political patronage was central to the functioning of the American political system: “unless the candidates for these offices are able to dispense the traditional patronage that has accrued to the offices, they also are unlikely to attract donations of time or money from voluntary groups.”

Some municipalities adopted reforms under pressure of local interest groups comprised of reform-minded activists, including under pressure from current local government employees themselves interested in greater job security (Anzia and Trounstein 2021). But others adopted civil service reforms under statutory requirement from state legislatures. In the latter case, reforms were often mandated to be adopted in certain cities according to arbitrary population thresholds.

Figure 2—Timing of Civil Service Reforms in American Municipalities, 1884-1943



Municipal civil service reform typically established commissions that were tasked with recruiting, transferring, and terminating public sector employees. There were significant variations in the structure of these commissions, but the mandate was broadly similar: to remove political considerations from hiring and firing decisions. Appointment to civil service commissions was mostly done by city councils or mayors, although some municipalities made the positions elected. Terms for commissioners typically ranged from 3–5 years. Many municipal civil service commissions included partisan restrictions. The New York State law legislating municipal civil service reform, for instance, includes a provision that commissions not be composed of members from the same political party.⁶

⁶“All appointments or designations of municipal civil service commissioners shall be made in such manner that not more than two-thirds of such commissioners in any city shall at any time be adherents of the same political party.”

The extent of authority for municipal civil service commissions varied considerably, as well. Civil service reform involved distinguishing between “classified” and “unclassified” services—with the merit system applying to the former. Civil service commissions oversaw the recruitment, transfer, and promotion for positions in the classified services—with postings ranging from blue-collar, white-collar, police departments, and fire departments. Unclassified services included political appointees, department heads, and temporary staff. The final category—temporary staff—was particularly abused by politicians seeking to bend the merit system towards patronage.

For classified services, hiring usually proceeded as follows. Managers notified the commissioners of a vacancy in their department. The commissioners then crafted an exam and openly advertised the position. In Ohio, statewide legislation in 1902 mandated that commissioners give “[n]otice of the time and place and general scope of every examination ... once each week for two weeks preceding such examination, in at least two daily newspapers of opposite politics published in such city.” As indicated, the workhorse of recruitment under the merit-system was the written examination. For white-collar work, police postings, and employment in fire departments, the written examination typically consisted in essays. For blue-collar work, the examination was occasionally substituted with alternative systems. In early twentieth century New York City, applicants were asked to present character references from “at least two citizens in good moral standing.” In Ohio, the above-mentioned legislation indicated that laborers had to demonstrate their sobriety. After scoring exams, the civil service commission provided a list of the top three scoring candidates to hiring managers for selection—the final stage, known as the “rule of three.”

In general, municipal civil service reform was faithfully implemented following its passage. A 1912 meeting of the National Assembly of Civil Service Commissions reported on the findings of a policy review, finding that “civil service laws [have been] faithfully and honestly executed” (Doyle 1912).⁷ Yet, some politicians resisted the genuine implementation of civil service reform. We conducted a review of newspaper accounts, the results of which indicate that this took place through two principal channels. First, municipal politicians antagonistic to civil service reform often created delays in the process of the classifying, examining, and appointing of new employees. Following the passage of municipal civil service reform in Phoenix, Arizona in 1933, disagreements between the civil service board and the city commission over rules and regulations continued through 1935, thus delaying its introduction.⁸ In New York City in 1927, thousands of current employees

⁷Throughout much of the twentieth century, strong public sector unions further championed and deepened local commitments to the merit-system (Holzer 1977).

⁸“Civil Service Delay is Hit,” *Arizona Republic*. 1935.

had yet to take civil service examinations, despite requests for more examiners three years prior. Even delays in grading completed examinations led to confusion over the announcement of selected candidates for the police and fire departments in Pittsburgh in 1903.⁹

Second, the favored tool of patronage-inclined municipal politicians was the reclassification of positions as outside the domain of civil service commissions. Richard Daley, the longtime Mayor of Chicago, reportedly had 5,500 constituents on payroll working temporary jobs in 1960s—approximately 20% of the municipal government payroll.¹⁰ Parks and recreation, sanitation, sewage, and municipal utilities were frequently classified beyond the reach of civil service commissions, with a leading expert in March 1969 estimating that, across the country, 663,000 out of the 1,434,000 full time municipal employees were in this category (Tolchin and Tolchin 1971, 72). Alternatively, many positions were simply declared exempt from commission oversight, a practice lamented as a “concession to the exponents of the spoils system” at the 1935 meeting of the National Civil Service Assembly.¹¹ Civil service commissions had few reliable mechanisms to challenge these moves. When the mayor of Buffalo exempted market clerks from the civil services, local news coverage appeared to accept the move as a *fait accompli*.¹²

Research design

Data

We join two groups of data: records on the ethnic, nativity, and racial composition of the population and municipal government employees and the timing of civil service reform adoption. For our outcome variables, we leverage the full-count decennial censuses from 1850–1940,¹³ which enumerate all residents and city government employees on the basis of race and country of birth. Because the census enumerates the statistical population, there is no sampling error in estimates of the size of racial, ethnic, or employment groups. For race, we consider two principal categories, “white” and “Black,” grouping all other observations into a residual category that we do not analyze. For nativity, we rely on census questions in which respondents stated their birthplace. We code those respondents born outside the U.S. as “foreign-born,” although it is possible some of these individuals were born U.S. citizens. We further disaggregate “foreign-born” individuals into five

⁹“Civil Service Delay Appointments,” *Pittsburgh Daily Post*. 1903.

¹⁰Some of his advisors also reportedly held 180-day temporary jobs for “twenty years, after getting appointed and reappointed.”

¹¹“Curb Asked on Civil Service Exempt Jobs,” *The Ruston Daily Leader*. 1935.

¹²“Mayor Knight Shows His Prehensile Hand,” *The Buffalo Enquirer*. 1903.

¹³With the exception of the 1890 census, which was destroyed in a fire.

categories of European-born immigrants: German-, Italian-, Irish-, Polish-, and Russian-born.

To identify municipal government employees, we use industry and occupational codes which were asked of all adult-age respondents. We make the assumption that a city resident whose stated industry is as a local government employee also works in the city of their residence, since the census does not ask for the name of employers. Given the lack of rapid transportation options allowing for inter-city commutes during the majority of our sample, we believe this to be a fair assumption. We specifically rely on the census industry code which indicates those working in “local public administration,” distinguishing between those working in “white-collar” and “blue-collar” occupations. We exclude policemen and firefighters, who often have distinct recruitment processes and civil service protections, and whose early labor organizations played a role in pushing for civil service reform for their employees (Anzia and Trounstein 2021).¹⁴ Moreover, as we show in Figure A3, public sector employees in police and fire departments never comprised each more than ten percent of the local government payroll for the decades under analysis.

Our key independent variable is the timing of municipal civil service reforms. We assemble data from two different sources. For our event study analysis, we obtain city-level data from three municipal surveys carried out by the U.S. Civil Service Assembly in 1937, 1940, and 1943.¹⁵ These surveys contain information on the year in which U.S. municipalities adopted civil service reforms, specifically establishing a civil service commission. The survey also includes further variables on the structure of the commission, the manner in which its officials are appointed, and the departments to which it applies. These surveys do not cover all municipalities in the United States. Of the 1,051 cities enumerated in the census between 1880 and 1940, 408 have reform adoption dates. Importantly, our key independent variable, the adoption of civil service reforms, encapsulates the bundle of changes to personnel management included within a commission’s remit: hiring, firing, and promotion. We are chiefly interested in the representational effects of civil service reform stemming from changes to hiring practices. But it is worth underscoring that the changes made to the regulations around firing public sector employees have important considerations for our analysis; namely, by making it more difficult to fire public sector employees, these reforms locked in the demographic composition of the municipal civil service at the moment of reform. As we discuss below, this means that the representational consequences of new recruitment practices may only

¹⁴The coding schema for classifying occupation can be found in Table A1. White-collar employment categories include technical professionals, managers, clerical workers. Blue-collar worker categories include craftsmen, mechanics, service workers, and laborers.

¹⁵We would like to thank James Hollyer for sharing the digitized data with us.

be observed over longer time horizons.

For the instrumental variables and regression discontinuity frameworks, we draw on state legislation mandating the introduction of civil service commissions in municipalities. A team of research assistants reviewed all state legislation between 1880-1940 pertaining to municipal civil service commissions to construct a comprehensive panel of civil service mandates. Using a keyword search of session laws on HeinOnline, a legal database, we identified 853 laws that pertained to requirements for municipal civil service positions. In many states, the legislation stipulated that municipalities adopt civil service commissions conditional on population levels. In the late nineteenth-century, many statutes relied on states’ prior categorization of cities according to population “class,” with mandates applying only to cities of the first or second class, but not to those of the third or fourth classes. By the twentieth century, many of the mandates directly specified numerical population cutoffs. Using this information, we construct an auxiliary dataset that indicates, for each city and decade in our sample, whether a state mandate was in effect. In the instrumental variable framework, this allows us to use the presence of a state mandate as an instrument for municipal adoption; for the regression discontinuity framework, we use distance from the population threshold as the running variable.

Empirical Strategy 1: Event Study

To estimate the effect of the civil service reforms on descriptive representation in local public administration, we first adopt an event study framework. Broadly, our estimation strategy compares trends in representation within municipalities in the decades after the adoption of civil service laws, using levels of representation in the prior decade as a counterfactual benchmark. More specifically, we implement the following specification:

$$Y_{mt} = \sum_{\tau=-T}^T \beta_{\tau} M_{m,t+\tau} + \alpha X_{mt} + \delta_m + \zeta_t + \epsilon_m \quad (1)$$

where our unit of analysis here is the municipality–census decade–occupation. The dependent variable in equation 1, Y_{mt} , representation of a given group in a given occupation in municipality m in census decade t : i.e., the proportion of government jobs held by a given group in municipality m . The key independent variable is M_{mt} , which is an indicator variable that captures whether municipality m implemented civil service reform in the decade preceding census decade t . Our independent variable M_{mt} is benchmarked to the period M_{m-1} , i.e., the decade prior to reform, such that the estimated coefficients are the effect of civil service reform relative to levels of representation

at that point.

As we possess eight waves of the census, we fit our models for $\tau \in [-4, 4]$, where, again, we benchmark against the decade prior to the introduction of civil service reform ($\tau = -1$). Our decision to examine representational trends by decade is motivated by data-availability concerns; nonetheless, we believe that one feature of municipal civil service reform makes this a reasonable decision. Namely, civil service reforms typically included provisions preventing the firing of employees, thus locking in the demographic composition of the public sector at the moment of reform, only allowing its effects to appear over a longer time horizon—in this case, decades. Next, as we are interested in the over- or underrepresentation of groups in local government jobs, we include X_{mt} , which is a vector of values capturing a given group’s share of the total population in decade t in municipality m . We also include city fixed effects (δ) and decade fixed effects (ζ), which control for unobserved municipality and decade-specific factors. Finally, we cluster the standard errors in equation 1 at the municipality level.

Although it is our preferred specification, we caution that this research design relies on assumptions in order to attach a causal interpretation to the estimates. For one, identification requires that trends in representation look equivalent prior to the introduction of reforms—an expectation that is visually demonstrable in most but not all the analyses. We also make the so-called “parallel trends” assumption: municipalities that do and do not adopt civil service reform ought to report similar trends in the share of various groups’ representation in local administration in the absence of civil service reform adoption. This assumption would be violated, for instance, if there were unobserved factors affecting both the adoption of civil service reforms and representation in municipal government. The most likely candidate for such confounding is the overall proportion of a given ethnic group in any given municipality, which we control for in our models.

Empirical Strategy 2: Instrumental Variables

Next, we estimate the effect of municipal civil service reform on descriptive representation using an instrumental variable analysis that uses state-level mandates as an instrument for municipal civil service reform. Leveraging state level mandates to estimate the effect of municipal civil service reform on descriptive representation allows us to assess the evidence without making the onerous assumptions found in the event study framework. The first stage of this analysis is the effect of the state-level mandate S_{mt} , an indicator variable that captures whether municipality m is under the state mandate in time t , on M_{mt} whether the municipality has adopted civil service reform in

that time period. We include city fixed effects (δ) and decade fixed effects (ζ) and cluster standard errors at the city level. θ , the coefficient on the state mandate, captures the compliance rate.

$$M_{mt} = \eta + \theta S_{mt} + \delta_m + \zeta_t + \epsilon_{mt} \quad (2)$$

We use the estimates obtained from equation 2 to generate the fitted values \tilde{M}_{mt} , which we plug into the second stage equation:

$$Y_{mt} = \beta_{IV} \tilde{M}_{mt} + \alpha X_{mt} + \tilde{\delta}_m + \tilde{\zeta}_t + \tilde{\epsilon}_{mt} \quad (3)$$

Similarly to the event study framework, the size of the β coefficient can be interpreted as the effect of adopting civil service on group representation, relative to their rate in the population as a whole.¹⁶

The instrumental variable analysis relies on several assumptions, not all of which are testable. First, we assume that state-level mandates are a strong instrument for city adoption; we verify the strength of the instrument in the main presentation of the results. The exclusion restriction, which is untestable, seems a reasonable assumption in our case. State-level mandates are unlikely to affect representation in municipal bureaucracies other than through municipal civil service hiring practices. We might also be concerned about sorting around the cutpoint, or in this case, deliberate placement of the cutpoint to include or exclude specific cities. While we believe it would be unlikely and infeasible for city leaders to limit their population growth in order to avoid civil service requirements, state legislatures may have initially placed the thresholds to not affect certain cities. However, given our multiple decades of data, we can capture cities that are forced to adopt reforms in later periods following population growth, alleviating some concerns about the initial placement of the population threshold.

¹⁶We also estimate a standard two-way fixed effects model, shown in equation 4, and which is effectively a pooling of the estimates obtained from the event study in equation 1.

$$Y_{mt} = \beta_{OLS} M_{mt} + \alpha X_{mt} + \delta_m + \zeta_t + \epsilon_{mt} \quad (4)$$

We also emphasize the reduced-form analysis, shown in equation 5, which takes a similar form to the endogenous equation, with the state mandate S_{mt} as the key independent variable instead of municipal adoption.

$$Y_{mt} = \beta_{RF} S_{mt} + \alpha X_{mt} + \delta_m + \zeta_t + \epsilon_{mt} \quad (5)$$

Empirical Strategy 3: Regression Discontinuity

We also estimate the effect of municipal civil service reform on ethnic representation using a regression discontinuity framework—an approach that relies on weaker assumptions for identification but whose results may be less generalizable to other contexts. As discussed earlier, we rely on state legislation that required municipalities over a certain population threshold to implement civil service reform. The regression discontinuity strategy assumes that, within a narrow bandwidth, cities just above or just below this population threshold are statistically exchangeable, allowing us to identify the local average treatment effect (LATE) of civil service reform at the cutoff. We arbitrarily select a bandwidth of 15,000 residents, although our results are unchanged when using smaller or larger bandwidths. To recover the LATE, we use a simple linear regression model, which takes the following form:

$$Y_m = \alpha + \beta_{RD}M_m + \epsilon_m \tag{6}$$

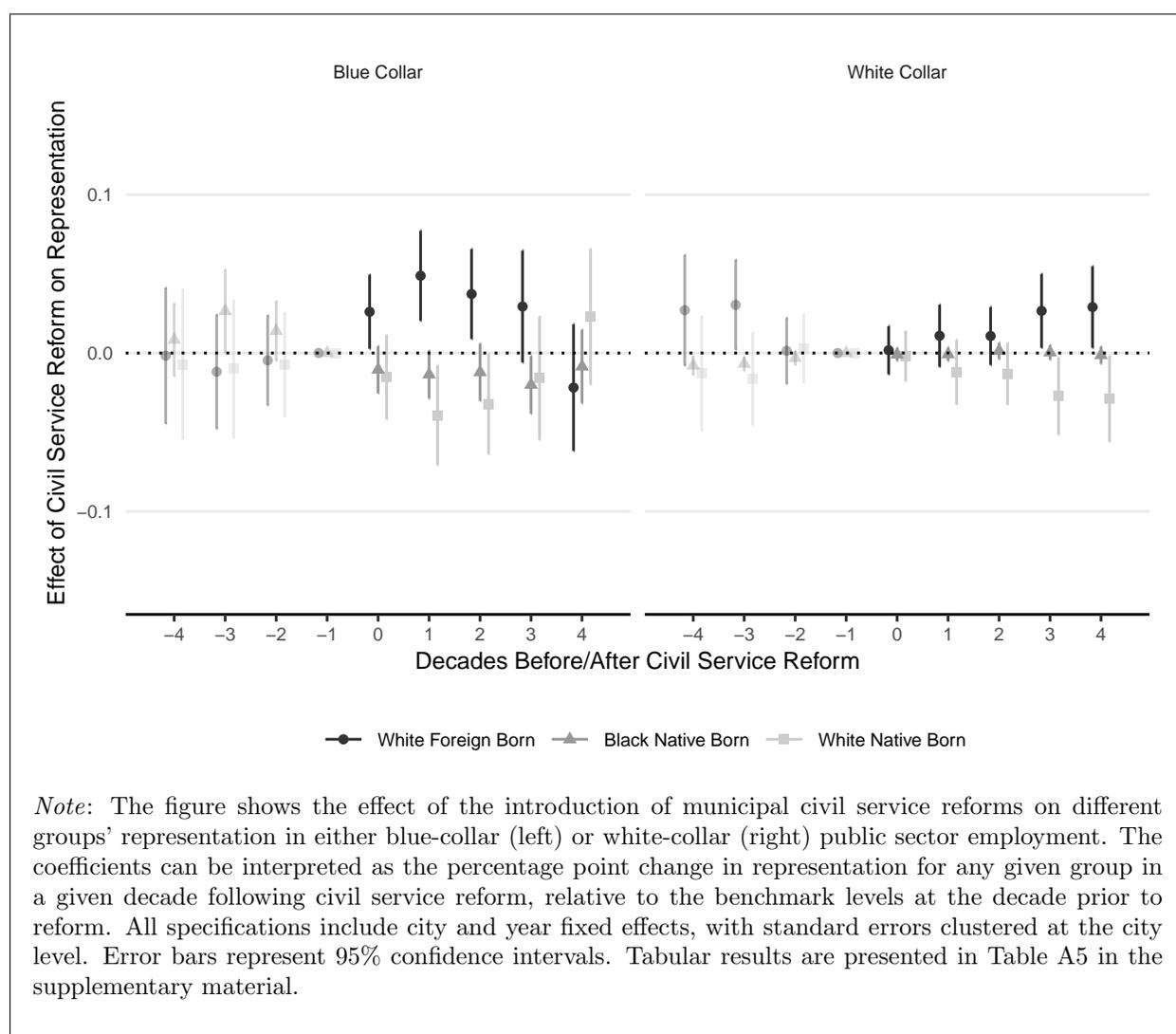
where, similar to the event study, the unit of analysis is the municipality–census decade–occupation, but where we subset observations to those where the census decade comes after the year in which the legislation was passed. Again, Y_m is a stand-in for the over or underrepresentation of a given ethnic group in a given occupation in municipality m . The main independent variable, M_m is an indicator that takes a ‘1’ in the event that municipality m had a population value in excess of the statewide threshold *and* had a population within 15,000 residents of the threshold. Meanwhile, M_m takes a ‘0’ if municipality m had a population under the threshold but within 15,000 residents of the threshold. Finally, β_{RD} is the parameter in which we are interested, and which we interpret as the effect of having been assigned to adopt civil service reform on the outcomes of interest. To attach a causal interpretation to β_1 we argue that whether any given municipality falls on one side of the threshold or another is as good as random within a narrow bandwidth. It is worth underscoring that all three research designs make onerous and ultimately untestable assumptions; nonetheless, it is our belief that in tandem these approaches provide a stronger signal of the effect of municipal civil service reform on representation in local government.

Results

Analysis 1: Event Study

How did the introduction of civil service reforms affect the share of municipal jobs held by different ethnic groups? We present our benchmark event study analysis in Figure 4, which depicts the results from equation 1. Recall that we subset our analysis according to different types of jobs—blue-collar or white-collar. For ease of interpretation, we present the main analysis graphically by plotting the beta coefficients from our main specification.

Figure 3—Effect of Civil Service Reforms on Representation



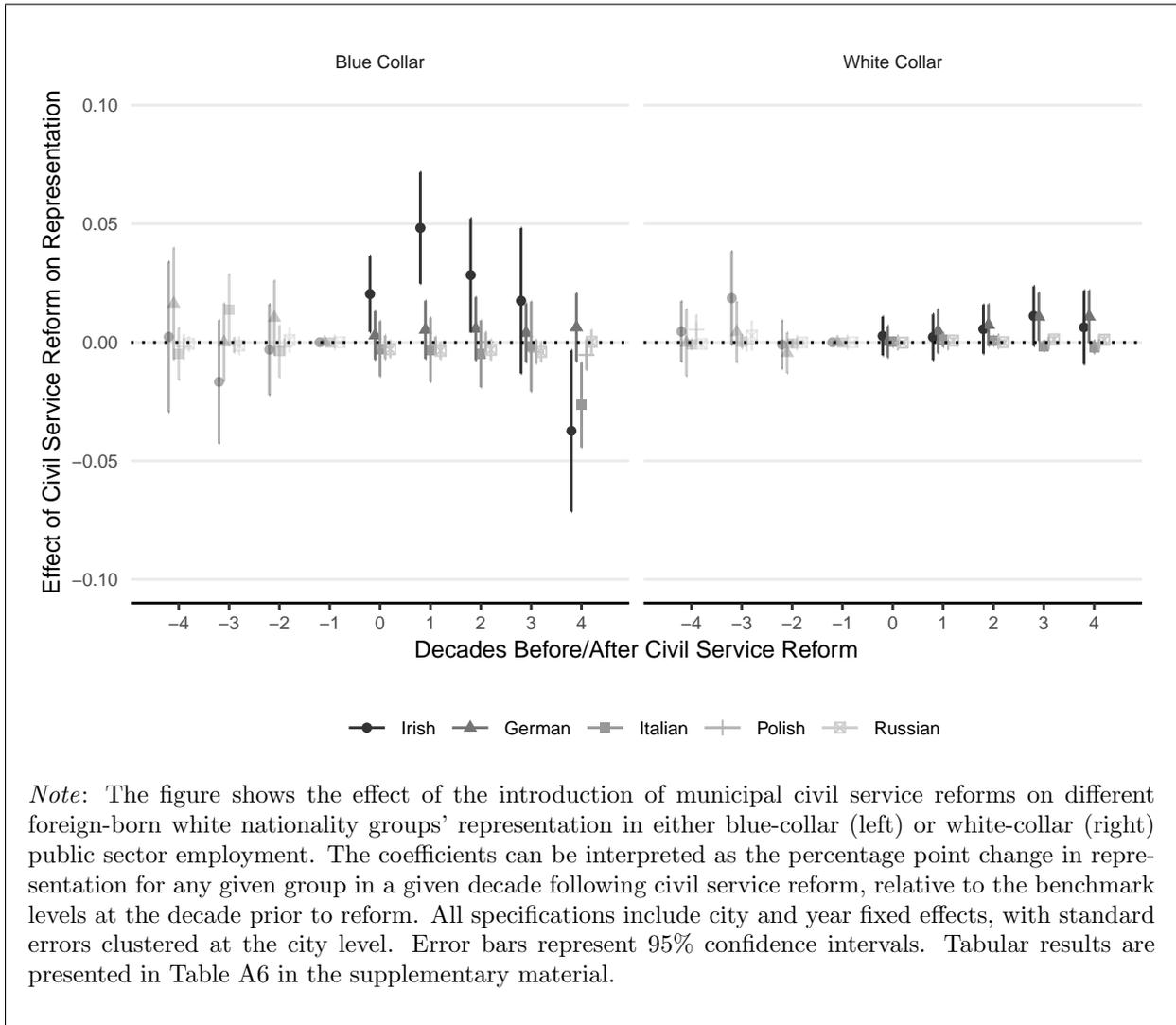
Interpreting the results, we focus first on group representation in blue-collar jobs. Here, we find

that foreign-born whites see their representation in blue-collar work significantly increase following the introduction of civil service reforms. In particular, relative to the decade before, the representation of foreign-born whites increased by 2.7 and 5.2 percentage points in the first and second decades following the introduction of civil service reforms. In general, it appears these gains in representation for foreign-born whites came at the expense of both Black and White native-born constituencies. In the second decade following the introduction of civil service reforms, native-born Blacks and whites saw their share of government jobs decrease by 1.4 and 3.7 percentage points respectively. While it seems likely that the divergent findings are a result of displacement—foreign-born whites capturing jobs that would, absent reform, have been given to native-born whites or Blacks—we caution that we cannot evaluate this possibility with our data.

The short-term representational consequences of civil service reforms appear more pronounced in blue-collar jobs. We detect no statistically significant effects on group representation among white-collar jobs within the first few decades of their introduction. This finding highlights the ironic possibility that the distributional consequences of “meritocratic” reforms might be most pronounced for positions in which written exams are most irrelevant in gauging applicants’ capacity. Given the limited labor pool for white-collar municipal jobs at the time, it may have been more difficult for governments to discriminate on the basis of nativity, putting a ceiling on the size of the reform effect. We similarly find muted effects looking at representation in police and fire jobs, shown in Figure A4. This could be because these were among the choicest municipal jobs and therefore a priority for the existing elite to retain even while they allowed immigrants to occupy other municipal jobs. Alternatively, the weak representational impact of reform may be attributable to the influence of police and fire departments’ professional organizations in preventing the dismissal of unqualified incumbents.

The findings presented in Figure 4 represent a significant reversal from the existing literature, which has argued that foreign-born whites were the biggest losers from municipal civil service reform. In Figure 3 we interrogate this finding further by examining the effect of civil service reform on foreign-born white representation, disaggregated by different countries of origin. We break down our analysis according immigrants from the five most common origin countries: Germany, Ireland, Italy, Poland, and Russia. By separating out foreign-born whites according to place of origin, we observe that our findings are entirely driven by increases in representation in blue-collar jobs for recent Irish immigrants. Specifically, we show that, compared to the decade before reforms were introduced, Irish born immigrants saw their share of government jobs increase by 2.0 and 4.8

Figure 4—Effect of Civil Service Reforms on Foreign-born White Representation



percentage points in the first and second decades afterwards, respectively. We find no increases in representation for foreign-born whites from other major origins across Europe at the time, and again find no effects on white-collar employment.

Analysis 2: Instrumental Variables

We further evaluate our hypothesis that civil service reform helped instead of hurt foreign-born whites obtain bureaucratic representation using state-level mandates as a source of exogeneity. In Table 3, we present results of a two-stage least squares estimation of municipal civil service reform, using the statewide mandate as an instrument. Recall that the application of the mandate depends not just on the timing of the state law, which could plausibly be related to trends in

immigrant representation in municipal civil service, but on population thresholds, which are less easily manipulable.

Table 1—Effect of Civil Service Reform on Representation

	<i>Dependent variable:</i>								
	Reform	Race by Nativity			Foreign-Born Whites				
Panel: Blue Collar	—	N. White	F. White	N. Black	Irish	Italian	German	Russian	Polish
Mandate	0.13** (0.03)	-0.03* (0.01)	0.06** (0.01)	-0.03** (0.01)	0.05** (0.01)	-0.002 (0.01)	0.005 (0.01)	0.001 (0.002)	-0.0004 (0.001)
$\widehat{\text{Reform}}$		-0.18* (0.10)	0.41** (0.09)	-0.24** (0.05)	0.37** (0.06)	-0.01 (0.05)	0.03 (0.04)	0.01 (0.01)	-0.003 (0.01)
Population Share		0.84** (0.08)	1.24** (0.08)	0.81** (0.07)	0.50** (0.12)	0.91** (0.08)	1.91** (0.16)	0.97** (0.06)	0.52** (0.04)
Observations	1,710	1,686	1,686	1,686	1,686	1,686	1,686	1,686	1,686

Note: *p<0.05; **p<0.01. Table shows the effect of the introduction of municipal civil service reform on group representation in different classes of employment in city government. Points represent regression estimates, standard errors calculated with robust standard errors. Regressions control for the share of group population in a given municipality. Outcome data were derived from IPUMS census data.

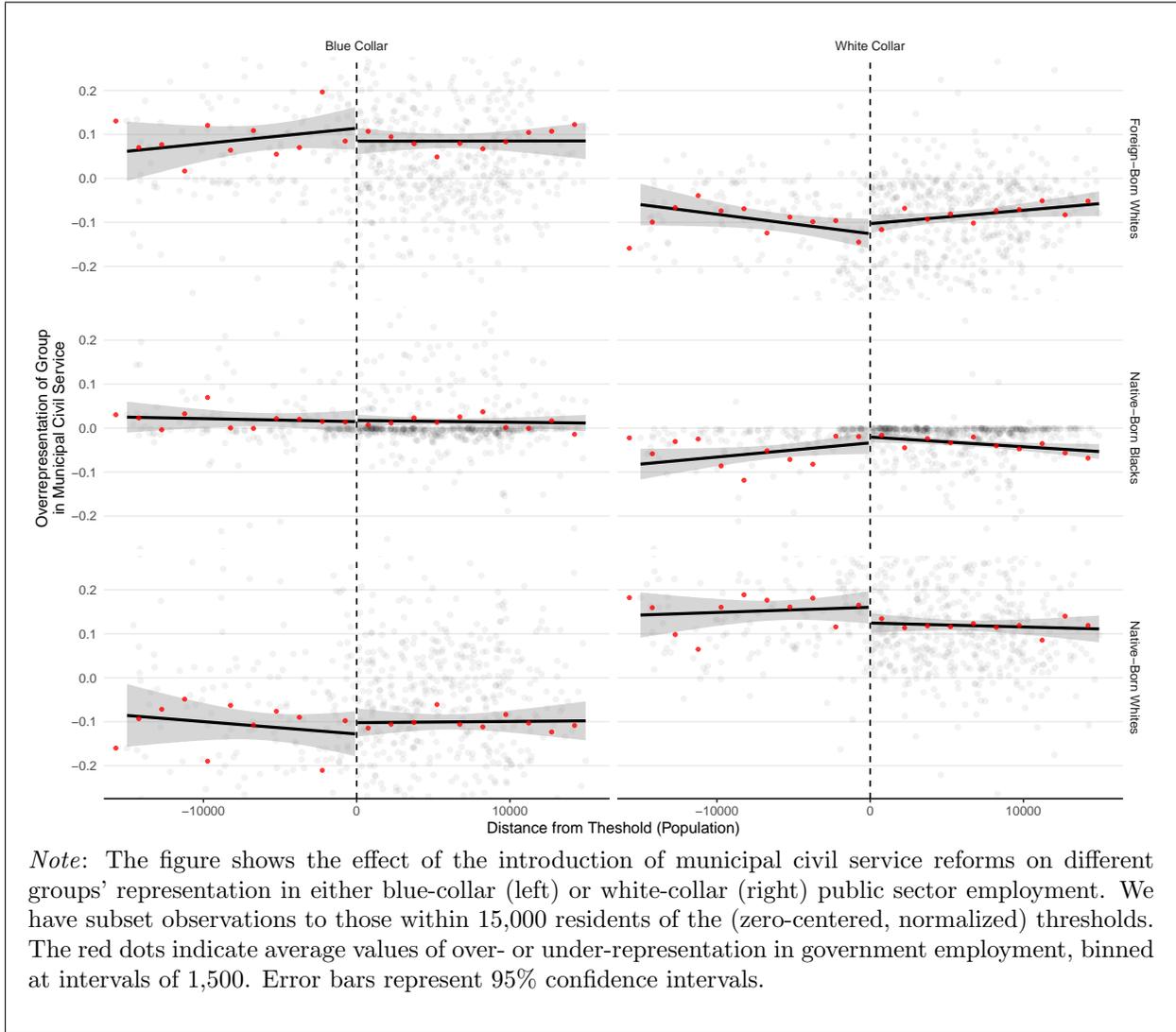
The first column of Table 1 shows the first stage, in which we regress an indicator variable measuring the presence of a state mandate applying to that municipality in the previous decade on the uptake of municipal civil service reform. The first stage indicates partial compliance with state mandates (recall that some cities may have adopted reforms before the state mandate) and is a valid instrument ($F = 10.7$). In all other columns we present reduced form and two-stage least squares results. The first row shows the reduced-form effect of a state mandate on group representation, while the $\widehat{\text{Reform}}$ row shows the instrumented estimate of municipal reform. The “Race by Nativity” columns show results that mirror the event study analysis: native-born whites and Blacks saw their representation in local government decrease by three percentage points while foreign-born whites increased by 6 percentage points. Breaking down foreign-born whites by country of birth, we again find the majority of gains captured by Irish immigrants. The two-stage least squares estimates, presented in the second row, depict a similar picture, albeit with much larger effects—roughly ten times the size of the event study, OLS, and reduced form estimates. We are thus hesitant to attach a substantive interpretation to these coefficients, although they confirm the general increase in foreign-born white representation due to reform, with offsetting decreases in native-born white and native-born Black representation.

Analysis 3: Regression Discontinuity

Finally, we leverage population cutoffs in the state mandates to conduct a regression discontinuity analysis to estimate the effects of reform. As discussed previously, the regression discontinuity

analysis requires fewer identifying assumptions than the event study framework, but suffers from concerns over external validity. In this case, it relies on fewer observations, which may limit the power to detect statistically significant but substantively small effects. Our preferred presentation is graphical, but in which we include linear lines of best fit. Figure 5 presents the results of this analysis, although we also include a tabular presentation of the estimates in Table A2. The figure shows the forcing variable on the x-axis, the distance from the population threshold designated in the state law. On the y-axis we show the overrepresentation of each group, calculated as the group's percentage of government employees less the percentage in population. Light grey points are individual observations and red points are binned averages. Black lines indicate the linear best fit. The left panel shows results for blue-collar workers, while the right panels show results for white-collar workers.

Figure 5—Effect of Civil Service Reforms on Representation



Although our preferred method is graphical, we also include a tabular presentation of the estimates of the LATE in Table A2. Here, we recover the LATE using the method proposed by Calonico, Cattaneo and Titiunik (2014). We present point estimates along with robust standard errors, the optimal bandwidth selected, and the N of the full sample and estimation sample. The dependent variable is the percentage of each group in blue and white-collar jobs, controlling for the percentage of each group in that municipality as a covariate. Because we have repeated observations per city, we cluster standard errors at the city level.

The results of the regression discontinuity analyses—both the graphical and tabular results—partially support our earlier findings, showing that foreign-born whites' representation in city gov-

ernment jobs was not reduced by civil service reform. Unlike the event study and instrumental variables analyses, however, we do not find any positive effects on the share of foreign-born whites who hold government jobs following the introduction of reforms, especially Irish immigrants. Looking at the main categories of foreign-born whites, native-born whites, and native-born Blacks, we find null and imprecisely estimated local average treatment effects. We find similar results looking at the five countries of origin; even though they are imprecisely estimated, all but one of the coefficients are of less than a single percentage point in magnitude.

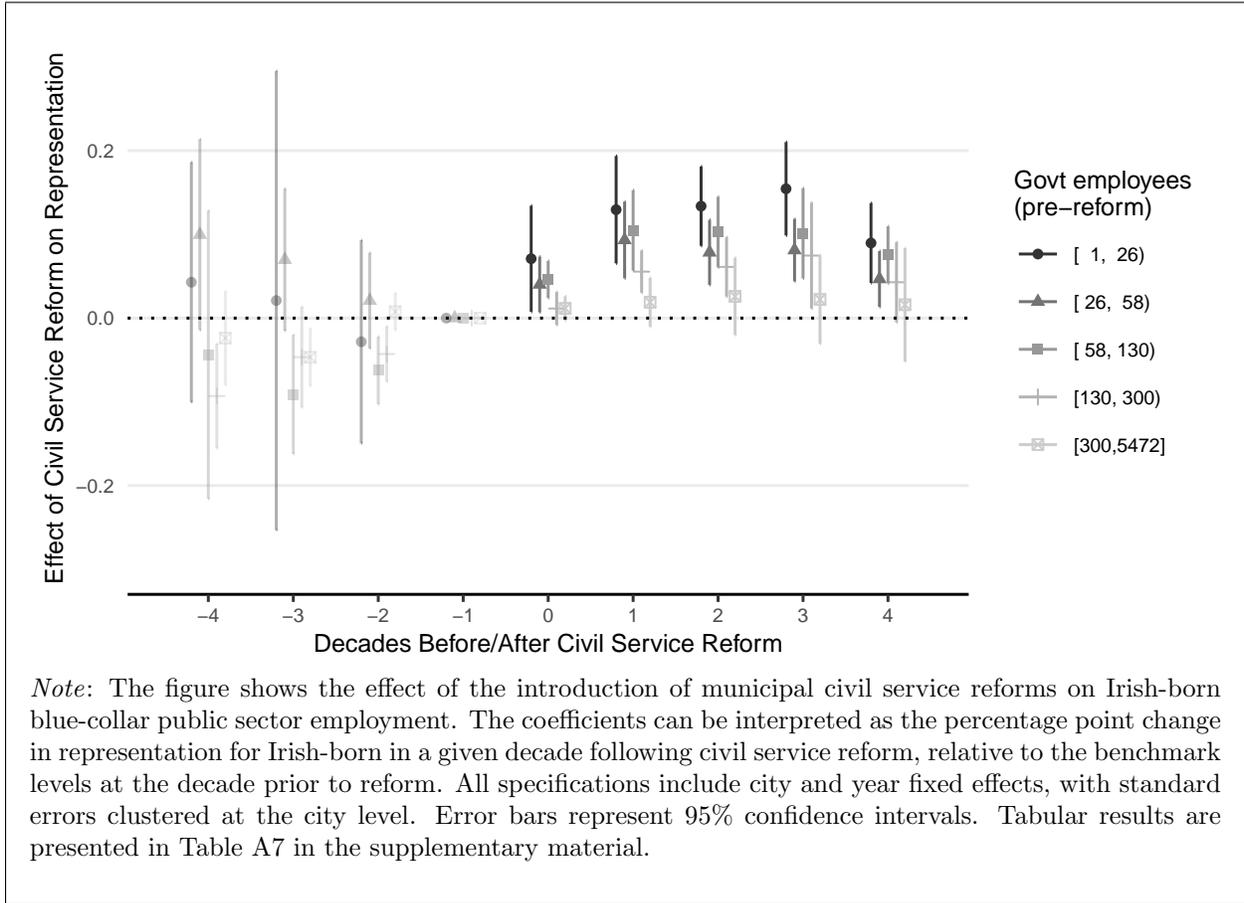
Discussion

What explains these findings, which cut against scholarly expectations and the documented intent of civil service reform? Our preferred interpretation is revisionist: foreign-born whites, Irish immigrants in particular, suffered under the spoils system in small-and-medium-sized cities where they did not represent a formidable political constituency. We argue that popular accounts of their dominance have been extrapolated from the experience of major urban centers. Instead, discrimination against Irish immigrants—widespread in the private sector—was mirrored in municipal hiring in small-to-medium size cities across the United States. The introduction of civil service reforms removed such barriers to entry in municipal government employment.

Qualitative historical accounts tracing the arc of the municipal civil service reform movement illustrate the manner in which it diffused from the largest cities. After the introduction of the Pendleton Act in 1883, in which civil service protections were introduced to the federal bureaucracy, the events surrounding the 1884 presidential election motivated the push for reform at lower levels of government. The Republican party nominated James Blaine as their presidential candidate, a champion of the spoils system who vowed to not enforce the Pendleton Act if victorious. Reform-minded Republicans, particularly those in New York, whose electoral votes were ultimately pivotal, defected to Grover Cleveland, a Democrat sympathetic to reform, and governor of New York. When these converted Republicans did not end up with a place in the Cleveland presidency, they instead turned to municipal reform, with local civil reform organizations quadrupling from 20 to 80 between 1890 and 1894 (Fox 1977). These organizations were inspired by reformers' success in harnessing public support for civil service reform to shape New York's electoral votes in the presidential election.

We substantiate this argument centering city size with further analyses. We create “size quintiles” of cities in which we split our sample in five equal subsets according to the number of people

Figure 6—Effect of Civil Service Reforms on Irish Representation by Size of Bureaucracy



working in municipal government in the decade prior to reform being implemented. Our choice to rely on number of government employees as opposed to, say, population size, is motivated by our expectation that the representational effect of civil service reform is conditioned by the size of the spoils—i.e., public sector jobs—available for capture.¹⁷ For ease of discussion, we nonetheless refer to these values as capturing “larger” and “smaller” cities. Our analyses in this section are primarily focused on the representation of recent Irish immigrants in blue-collar employment, as we have seen that it is these constituency and occupational categories driving the results in the main analysis.

Figure 6 examines the effects of reform on Irish blue-collar workers by size of bureaucracy. We find that the largest cities, where the Irish were overrepresented in municipal government pre-reform, see little effect of reform on representation. For all but this largest quintile of cities, we see

¹⁷In any case, in Figure A7, we show that the results are robust to binning by total population in the year before civil service reform.

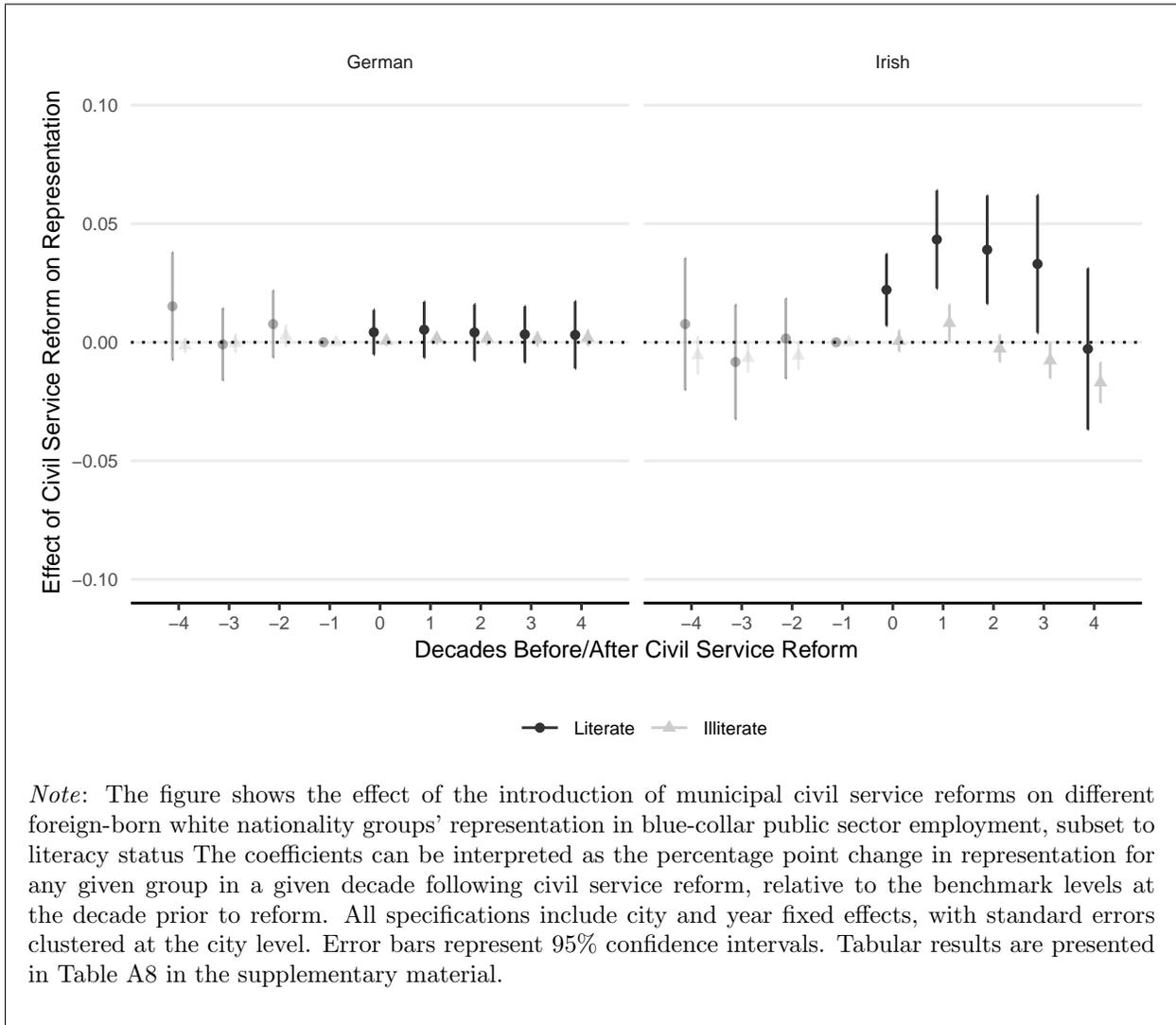
the positive and significant effects driving the overall effect on Irish immigrants. Although we do not find negative effects of reform in the largest cities, as historical accounts would suggest, the null effects on the largest quintile still distinguish the cities from which historical accounts derive from the remainder. The largest quintile of cities contained the plurality of immigrants, meaning that many immigrants did not in fact benefit from reform. However, our argument and empirical test is not one centered on individuals' employment fortunes but rather cities' policy changes. To make general claims about the effect of civil service reform on cities, we therefore focus on the city as a unit of analysis, regardless of the difference in the number of immigrants affected between cities.

Two factors set the Irish apart from other groups that may explain the benefit they witnessed from civil service reform: a shared native tongue, the number of migrants, and the timing of their migration. First, and perhaps most importantly, Irish immigrants benefited from sharing a common tongue with their new home: 84% of Irish immigrants spoke English as their primary language, 10 percentage points more than any other group. On top of being more likely to speak language in which examinations were conducted, they were also more likely to be literate: nearly 70% of Irish immigrants were literate English-speakers. German immigrants were the only group to have a majority literate in English at 60%, Italian immigrants were the least literate in English at 44%.

We provide evidence consistent with English-language literacy being the factor that distinguished the Irish from other groups in Figure 7. Here, we replicate the main event study analysis separating literate and non-literate Irish and German immigrants. We find that the positive effects of civil service reform for blue-collar Irish workers are concentrated among the literate. Looking at German immigrants, the next most literate group, we find that the introduction of civil service reform had no effect on both literate and illiterate recent immigrants.

We additionally examine heterogeneity in our main effects according to underlying regional variation in the distribution of reforms. Our primary findings—that native-born whites see their share of blue-collar civil service jobs decrease relative to their population share while foreign-born whites, notably Irish, see their representation increase—are primarily concentrated in the North and Midwest (see Figure A9). In the South, we find that civil service reforms' biggest beneficiaries were African Americans. Some Black Southerners, previously totally locked out of government jobs under a discretionary system, were able to obtain government employment for the first time, even under Jim Crow. In the West, where machines were less entrenched (Gimpel 1993), we find the opposite effects—native-born whites benefited from civil service reform at the expense of foreign-born whites.

Figure 7—Effect of Civil Service Reforms on Irish and German Representation, by Literacy



We also conduct an additional analysis to directly investigate the the role that the presence of a political machine may have played in mediating the effects of civil service reform. On the one hand, it may be that the introduction of civil service reform had particularly large representational effects in places where machines previously controlled the allocation of government jobs. On the other hand, the presence of a political machine may have hamstrung the implementation of civil service reform, thereby muting its representational effects. To evaluate these possibilities, we draw on the most comprehensive data available on municipal political machines, which tracks their presence in the 100 largest cities from 1900 onward (Trounstine 2009). Using the event study framework, Figure A8 estimates the effect of civil service reform on the representation of foreign-born whites in cities

under machine domination when they reformed and cities not under any domination. We detect no difference in the effect of civil service reform on the representation of foreign-born whites across machine and non-machine dominated cities. We caution, however, that these null effects may be a function of the small sample size: only 7 of the 42 machine cities in both datasets reformed during machine domination and we were forced to truncate the analysis to cities that reformed since 1900 owing to data limitations.

As a final extension, we examine the effects of municipal civil service reform on the representation of foreign-born whites, according to the share of the population that was foreign-born white in the decade before the introduction of reforms. We present the results in Figure A6. The results indicate that the positive effect of civil service reform on the representation of foreign-born whites was concentrated in municipalities where, in the decade prior to reform, they comprised a moderate share of the population—the effects are concentrated in second and third quintiles (8.0-19.1% foreign-born whites). By contrast, we observe no positive effect of civil service reform on the representation of foreign-born whites in cities where foreign-born whites comprised a larger share of the population in the fourth and fifth quintiles (19.1-49.7%)—i.e., the largest municipalities in terms of absolute population. The first quintile—cities in which foreign-born whites comprised the smallest share of the total population—observed a decline in the share of positions held by foreign-born whites started two decades after the introduction of municipal civil service reform.

Conclusion

Staffing government bureaucracies in pluralistic societies invites tension between privileged and marginalized groups. In this article we examine the effects of introducing meritocratic recruitment procedures on the representation of different groups. The evidence we have presented revises a widely-held belief that American municipalities' introduction of the merit system benefited native-born whites at the expense of foreign-born whites. Instead, we show that in certain settings these reforms benefited immigrants who were in a position to take advantage of more rigid hiring processes. Irish immigrants, who were native English speakers and more likely to be literate, reaped the benefits of reforms.

The evidence we have presented leverages the full-count censuses for the period 1850–1940, which enumerate all local government employees, as well as their race and place of birth. Combining these uniquely fine-grained data with the adoption dates of municipal civil service reforms, we implement three different research designs—an event study analysis, an instrumental variables

approach, and a regression discontinuity design. Using this new source of data, we establish two core results. First, descriptively, we show that foreign-born whites were underrepresented in the modal municipal government prior to reform. Second, we find that reform increased foreign-born whites' representation in blue-collar government jobs, possibly at the expense of native-born whites and Blacks—findings that contradict previous work that has mainly used less complete and representative data.

The interpretation we have advanced is historiographical. We have argued that the existing literature has rightly identified concerns over descriptive representation as crucial in motivating municipal civil service reform; yet, we have shown that the introduction of these reforms, on average, led to an increase in the representation of foreign-born white immigrants, in contrast with widely held expectations. In other words, we draw a distinction between the intent of the reforms and their average effect. We suggest that the existing literature has drawn these conclusions based on the experience of the largest cities. We suspect that this is not the only stylized fact where this is the case. With new data availability, many of the conclusions from urban history and urban politics can be re-examined and contextualized.

Conflicts of Interest

The authors declare no ethical issues or conflicts of interest associated with this research.

Ethical Standards

The authors affirm this research did not involve human subjects.

Data Transparency

Research documentation and data that support the findings of this study are openly available in the APSR Dataverse at <https://doi.org/10.7910/DVN/G7J6QJ>. Limitations on data availability are discussed in the text.

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Supplementary Materials

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A Textbook Quotes

In this section, we itemize several depictions of the motivation for municipal civil service reform during the Progressive era. These descriptions draw from sources widely used in undergraduate syllabuses in the study of American politics—including both prominent scholarly accounts, as well as textbooks. The thrust of these analyses—now often presented as fact—is that the introduction of municipal civil service reform was design to dislodge the representation of foreign-born whites in local government, and that these reforms succeeded in achieving this aim. Consider several examples:

- “Because of the nature of their political ethos, Protestants and Jews have been in the vanguard of every fight for municipal reform.” (?)
- “Hays argued that urban reformers tended to be from the upper-middle and upper-class business and professional elite, a group which endeavored through “reform” to reshape the city into a form congruent with its own economic social, cultural, and ethical interests and beliefs.” (Allswang 1986)
- “[Reformers] believed that government should serve not the interests of the “people.” but the “right” people, respectable people—the middle and patrician classes—who would substitute business for political practices.” (Plunkitt, ed. Callow 1976, pp. 178)
- “Reformers were least successful in the big cities with complex electorates, where middle- and upper-class voters did not constitute an electoral majority. Smaller cities, however, especially those in the Midwest and the West, almost always adopted reform items in their new charters. In these communities, lower-class ethnic voters were invariably outnumbered, and therefore when reform charter proposals were put before the voters, they usually passed.” (Judd and Swanstrom 1994, pp. 86)

B Occupational Coding Scheme

Table A1—Occupational Classification

Police	Fire	White Collar	Blue collar
Policemen and detectives;	Locomotive firemen; Stationary firemen; Firemen, fire protection;	Accountants and auditors; Actors and actresses; Airplane pilots and navigators; Architects; Athletes; Authors; Chemists; Chiropractors; Clergymen; College presidents and deans; Agricultural sciences-Professors and instructors; Biological sciences-Professors and instructors; Chemistry-Professors and instructors; Economics-Professors and instructors; Engineering-Professors and instructors; Geology and geophysics-Professors and instructors; Mathematics-Professors and instructors; Medical Sciences-Professors and instructors; Physics-Professors and instructors; Psychology-Professors and instructors; Statistics-Professors and instructors; Natural science (nec)-Professors and instructors; Social sciences (nec)-Professors and instructors; Non-scientific subjects-Professors and instructors; Subject not specified-Professors and instructors; Dentists; Designers; Dietitians and nutritionists; Draftsmen; Editors and reporters; Aeronautical-Engineers; Chemical-Engineers; Civil-Engineers; Electrical-Engineers; Industrial-Engineers; Mechanical-Engineers; Metallurgical, metallurgists-Engineers; Mining-Engineers; Engineers (nec); Entertainers (nec); Farm and home management advisors; Foresters and conservationists; Funeral directors and embalmers; Lawyers and judges; Librarians; Nurses, professional; Nurses, student professional; Agricultural scientists; Biological scientists; Geologists and geophysicists; Mathematicians; Physicists; Misc. natural scientists; Optometrists; Osteopaths; Personnel and labor relations workers; Pharmacists; Photographers; Physicians and surgeons; Radio operators; Recreation and group workers; Religious workers; Social and welfare workers, except group; Economists; Psychologists; Statisticians and actuaries; Misc social scientists; Sports instructors and officials; Surveyors; Medical and dental-technicians; Testing-technicians; Technicians (nec); Therapists and healers (nec); Veterinarians; Professional, technical and kindred workers (nec); Farmers (owners and tenants); Farm managers; Buyers and dept heads, store; Buyers and shippers, farm products; Conductors, railroad; Credit men; Floormen and floor managers, store; Inspectors, public administration; Managers and superintendants, building; Officers, pilots, pursers and engineers, ship; Officials and administrators (nec), public administration; Officials, lodge, society, union, etc.; Postmasters; Purchasing agents and buyers (nec); Managers, officials, and proprietors (nec); Agents (nec); Attendants and assistants, library; Attendants, physicians and dentists office; Baggage men, transportation; Bank tellers; Bookkeepers; Cashiers; Collectors, bill and account; Dispatchers and starters, vehicle; Express messengers and railway mail clerks; Mail carriers; Messengers and office boys; Office machine operators; Shipping and receiving clerks; Stenographers, typists, and secretaries; Telegraph messengers; Telegraph operators; Telephone operators; Ticket, station, and express agents; Clerical and kindred workers (n.e.c.); Advertising agents and salesmen; Auctioneers; Demonstrators; Hucksters and peddlers; Insurance agents and brokers; Newsboys; Real estate agents and brokers; Stock and bond salesmen; Salesmen and sales clerks (nec);	Bakers; Blacksmiths; Bookbinders; Boilermakers; Brickmasons,stonemasons, and tile setters; Cabinetmakers; Carpenters; Cement and concrete finishers; Compositors and typesetters; Cranemen,derrickmen, and hoistmen; Decorators and window dressers; Electricians; Electrotypers and stereotypers; Engravers, except photoengravers; Excavating, grading, and road machinery operators; Foremen (nec); Forgemen and hammermen; Furriers; Glaziers; Heat treaters, annealers, temperers; Inspectors, scalers, and graders log and lumber; Inspectors (nec); Jewelers, watchmakers, goldsmiths, and silversmiths; Job setters, metal; Linemen and servicemen, telegraph, telephone, and power; Locomotive engineers; Loom fixers; Machinists; Airplane-mechanics and repairmen; Automobile-mechanics and repairmen; Office machine-mechanics and repairmen; Radio and television-mechanics and repairmen; Railroad and car shop-mechanics and repairmen; Mechanics and repairmen (nec); Millers, grain, flour, feed, etc; Millwrights; Molders, metal; Motion picture projectionists; Opticians and lens grinders and polishers; Painters, construction and maintenance; Paperhangers; Pattern and model makers, except paper; Photoengravers and lithographers; Piano and organ tuners and repairmen; Plasterers; Plumbers and pipe fitters; Pressmen and plate printers, printing; Rollers and roll hands, metal; Roofers and slaters; Shoemakers and repairers, except factory; Stationary engineers; Stone cutters and stone carvers; Structural metal workers; Tailors and tailoresses; Tinsmiths, coppersmiths, and sheet metal workers; Tool makers, and die makers and setters; Upholsterers; Craftsmen and kindred workers (nec); Members of the armed services; Auto mechanics apprentice; Bricklayers and masons apprentice; Carpenters apprentice; Electricians apprentice; Machinists and toolmakers apprentice; Mechanics, except auto apprentice; Plumbers and pipe fitters apprentice; Apprentices, building trades (nec); Apprentices, metalworking trades (nec); Apprentices, printing trades; Apprentices, other specified trades; Apprentices, trade not specified; Asbestos and insulation workers; Attendants, auto service and parking; Blasters and powdermen; Boatmen, canalmen, and lock keepers; Brakemen, railroad; Bus drivers; Chainmen, rodmen, and axmen, surveying; Conductors, bus and street railway; Deliverymen and routemen; Dressmakers and seamstresses, except factory; Dyers; Filers, grinders, and polishers, metal; Fruit, nut, and vegetable graders, and packers, except fact; Furnacemen, smeltermen and pourers; Heaters, metal; Laundry and dry cleaning Operatives; Meat cutters, except slaughter and packing house; Milliners; Mine operatives and laborers; Motormen, mine, factory, logging camp, etc; Motormen, street, subway, and elevated railway; Oilers and greaser, except auto; Painters, except construction or maintenance; Photographic process workers; Power station operators; Sailors and deck hands; Sawyers; Spinners, textile; Switchmen, railroad; Taxicab drivers and chauffeurs; Truck and tractor drivers; Weavers, textile; Welders and flame cutters; Operative and kindred workers (nec); Housekeepers, private household; Laundresses, private household; Private household workers (nec); Attendants, hospital and other institution; Attendants, professional and personal service (nec); Attendants, recreation and amusement; Barbers, beauticians, and manicurists; Bartenders; Bootblacks; Boarding and lodging house keepers; Charwomen and cleaners; Cooks, except private household; Counter and fountain workers; Elevator operators; Guards, watchmen, and doorkeepers; Housekeepers and stewards, except private household; Janitors and sextons; Marshals and constables; Midwives; Porters; Practical nurses; Sheriffs and bailiffs; Ushers, recreation and amusement; Waiters and waitresses; Watchmen (crossing) and bridge tenders; Service workers, except private household (nec); Farm foremen; Farm laborers, wage workers; Farm laborers, unpaid family workers; Farm service laborers, self-employed; Fishermen and oystermen; Garage laborers and car washers and greasers; Gardeners, except farm and groundskeepers; Long-shoremen and stevedores; Lumbermen, raftsmen, and woodchoppers; Teamsters; Laborers (nec);

Note: Data and coding scheme were derived from IPUMS census data, and the OCC1950 variable in particular.

C Supplementary Analyses

C.1 Descriptive Statistics

Figure A1—Foreign-Born Whites Are Underrepresented in Many Cities Prior to Civil Service Reform

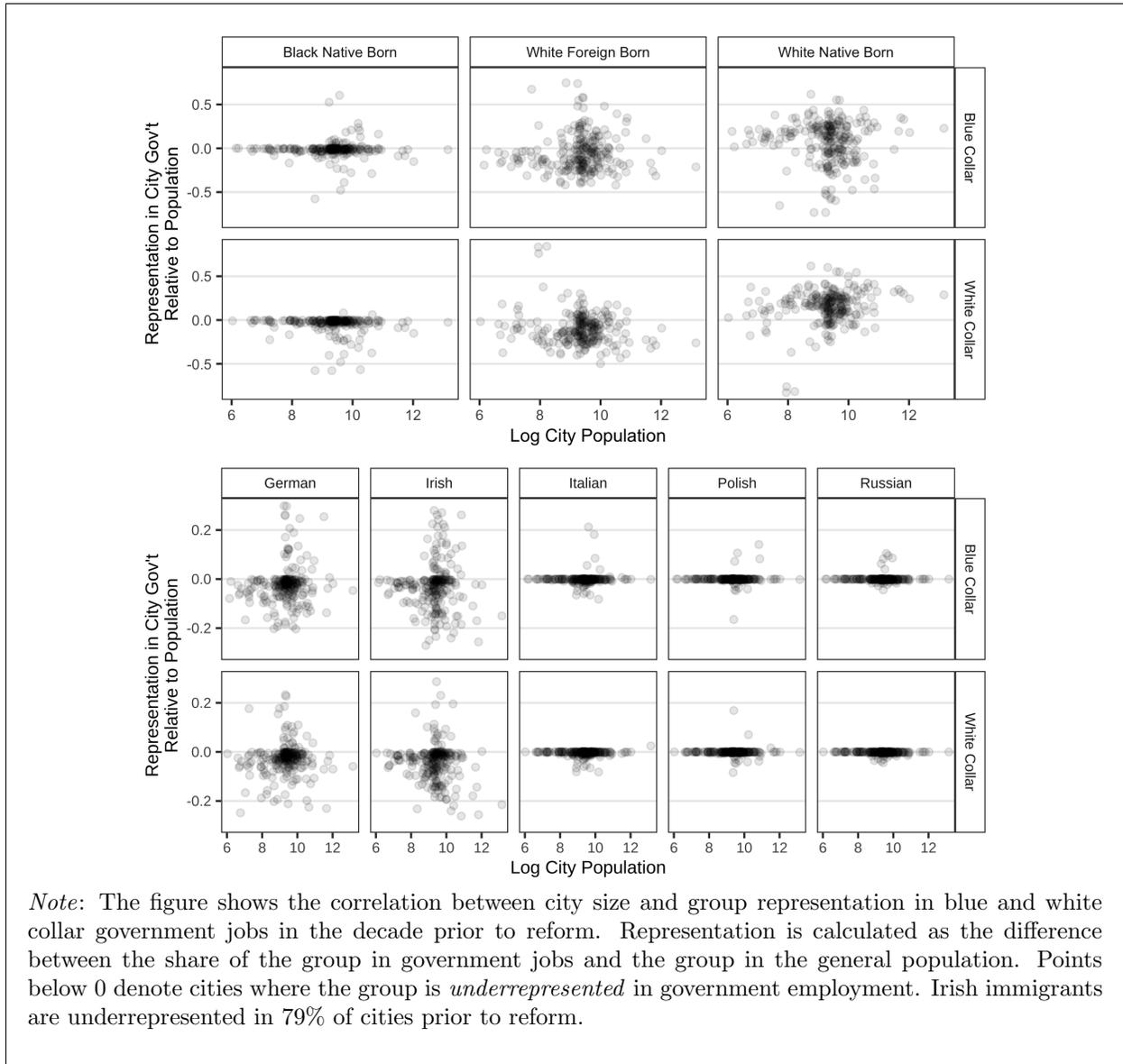


Figure A2—Irish Immigrants Make Up Similar Shares of Population in Small and Large Cities

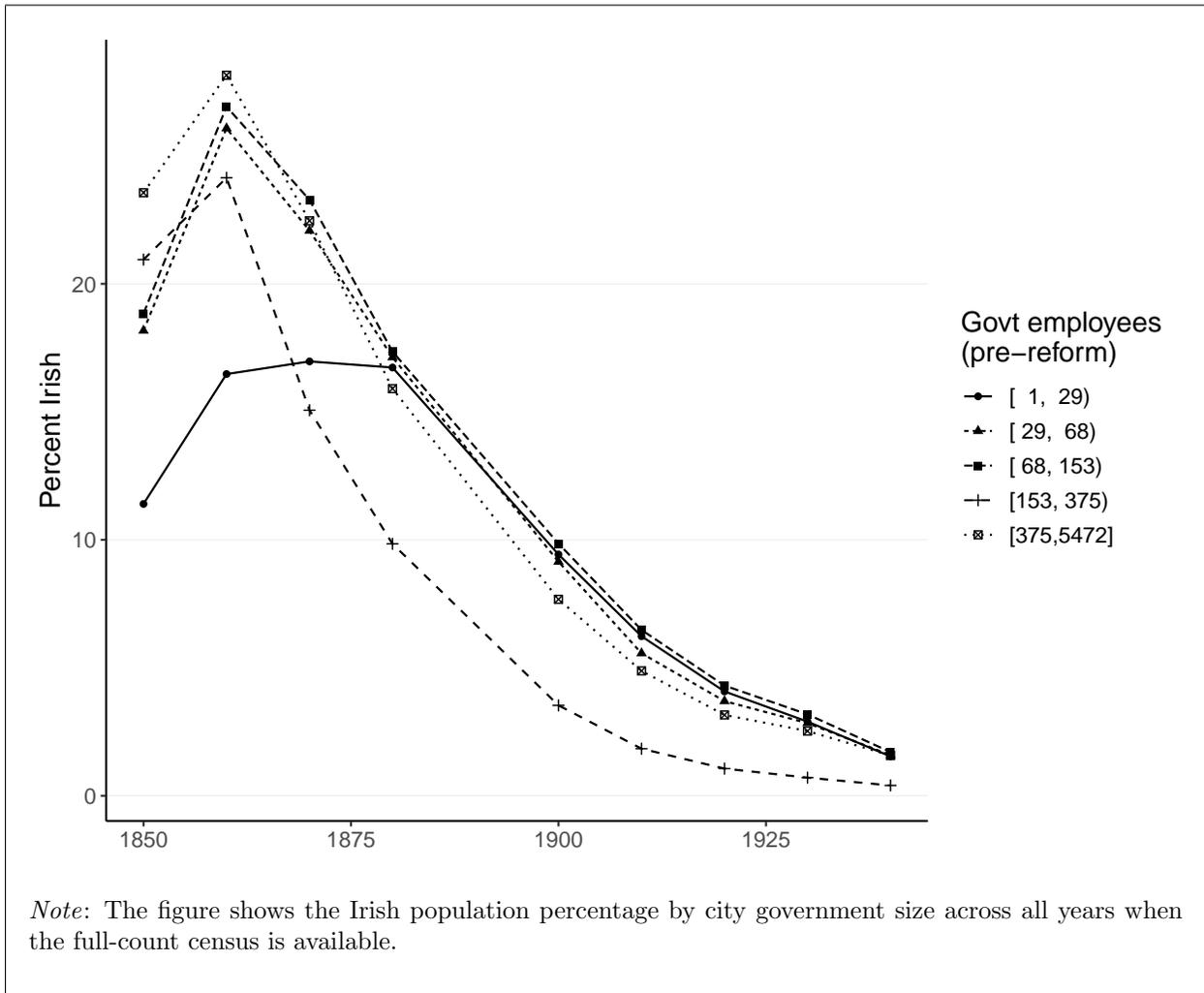
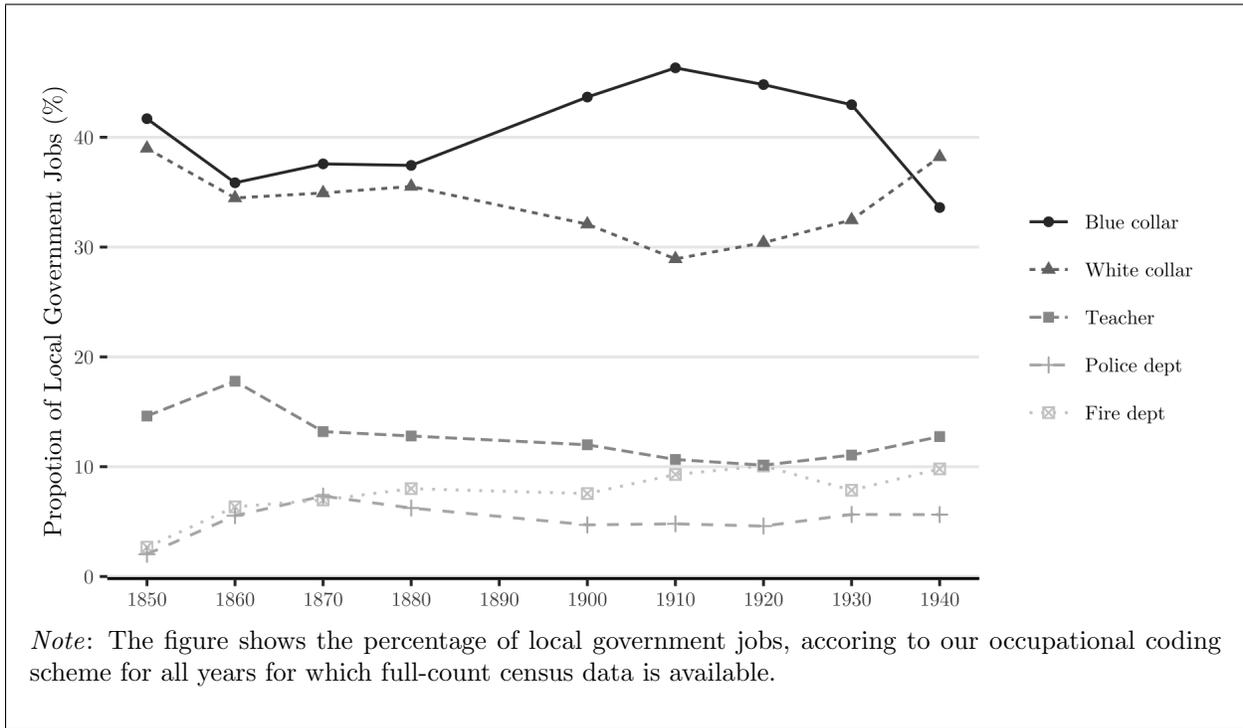


Figure A3—Composition of Local Government Jobs by Type



C.2 Tabular Presentation of Regression Discontinuity Results

Table A2—Effect of Civil Service Reform on Representation, RDD Results

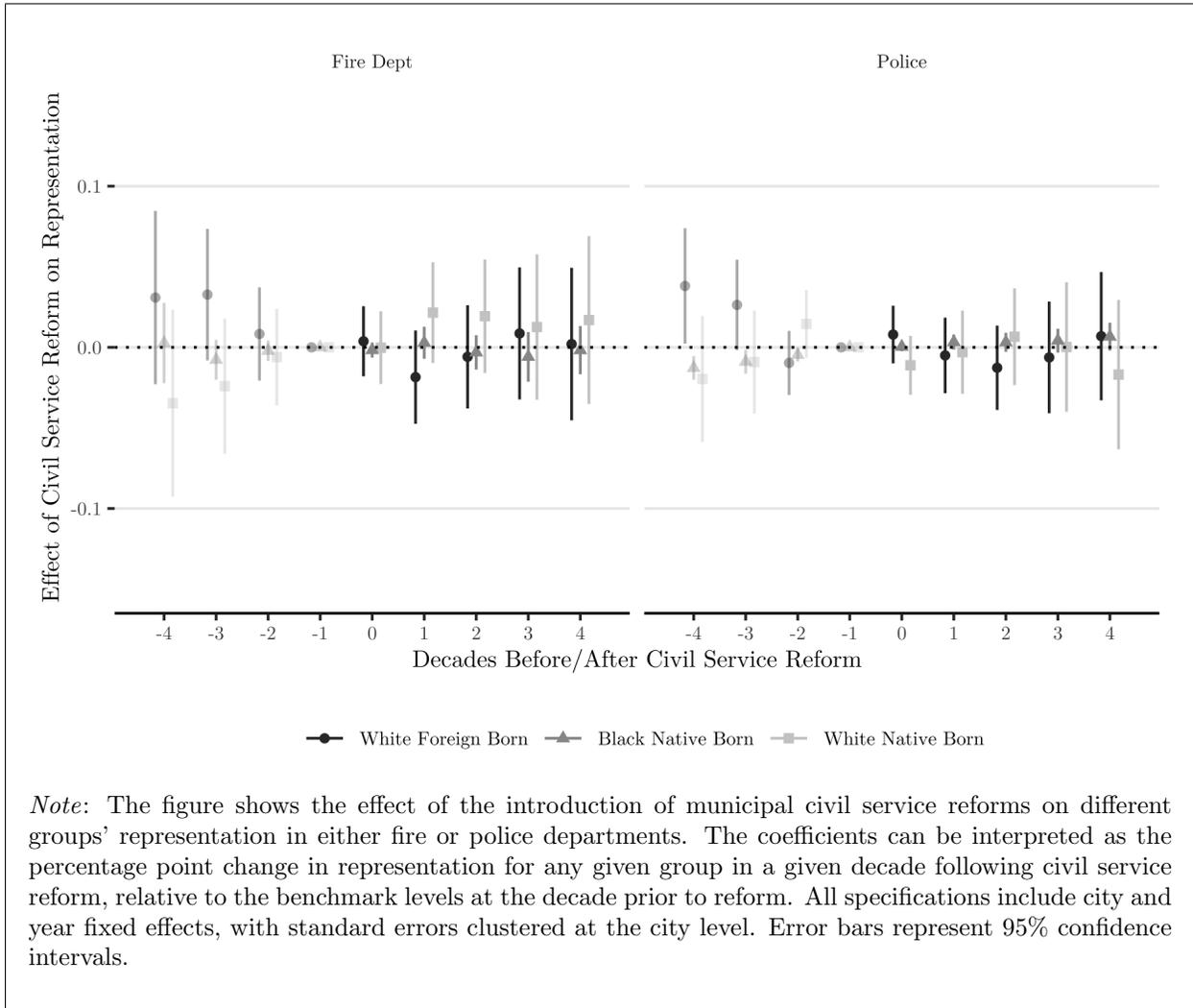
Group	Occupation	Estimate	SE (Conv.)	SE (Robust)	BW	N	Eff. N
Foreign W.	Blue Collar	-0.011	0.021	0.022	61,602.290	1,754	1,186
Native W.	Blue Collar	0.005	0.021	0.023	67,195.310	1,754	1,217
Native B.	Blue Collar	0.002	0.009	0.010	60,031.560	1,754	1,179
Foreign W.	White Collar	0.001	0.012	0.013	109,302.800	1,731	1,271
Native W.	White Collar	-0.001	0.011	0.012	127,234.500	1,731	1,284
Native B.	White Collar	0.001	0.001	0.001	53,308.360	1,731	1,144

Table A3—Effect of Civil Service Reform on Representation, RDD Results

Group	Occupation	Estimate	SE (Conv.)	SE (Robust)	BW	N	Eff. N
German	Blue Collar	0.005	0.008	0.008	71,577.870	1,754	1,219
Irish	Blue Collar	-0.003	0.014	0.016	64,756.590	1,754	1,211
Italian	Blue Collar	-0.011	0.009	0.010	99,711.240	1,754	1,269
Polish	Blue Collar	-0.0004	0.002	0.003	116,865.600	1,754	1,291
Russian	Blue Collar	0.001	0.002	0.002	56,542.110	1,754	1,163
German	White Collar	-0.0002	0.003	0.003	72,527.120	1,731	1,208
Irish	White Collar	-0.002	0.004	0.004	165,103.700	1,731	1,318
Italian	White Collar	-0.003	0.002	0.002	64,845.670	1,731	1,200
Polish	White Collar	0.001	0.001	0.001	97,267.950	1,731	1,251
Russian	White Collar	0.001	0.001	0.001	60,542.850	1,731	1,168

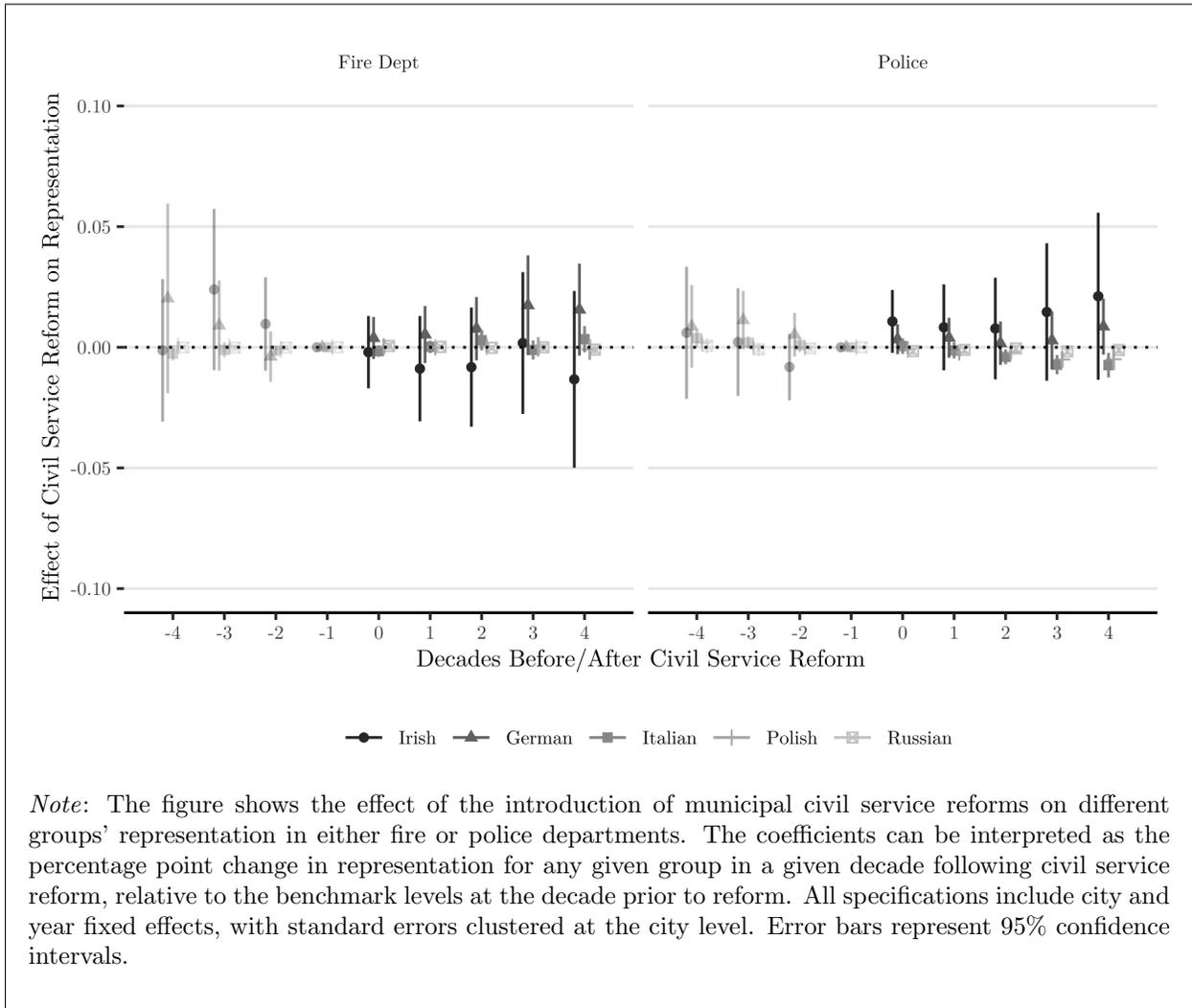
C.3 Event Study Results—Police and Fire Departments

Figure A4—Effect of Civil Service Reforms on Representation, Police and Fire



Note: The figure shows the effect of the introduction of municipal civil service reforms on different groups' representation in either fire or police departments. The coefficients can be interpreted as the percentage point change in representation for any given group in a given decade following civil service reform, relative to the benchmark levels at the decade prior to reform. All specifications include city and year fixed effects, with standard errors clustered at the city level. Error bars represent 95% confidence intervals.

Figure A5—Effect of Civil Service Reforms on Representation, Police and Fire



Note: The figure shows the effect of the introduction of municipal civil service reforms on different groups' representation in either fire or police departments. The coefficients can be interpreted as the percentage point change in representation for any given group in a given decade following civil service reform, relative to the benchmark levels at the decade prior to reform. All specifications include city and year fixed effects, with standard errors clustered at the city level. Error bars represent 95% confidence intervals.

C.4 Event Study Heterogeneity Results

Figure A6—Effect of Civil Service Reforms, by Share of Population Foreign Born White

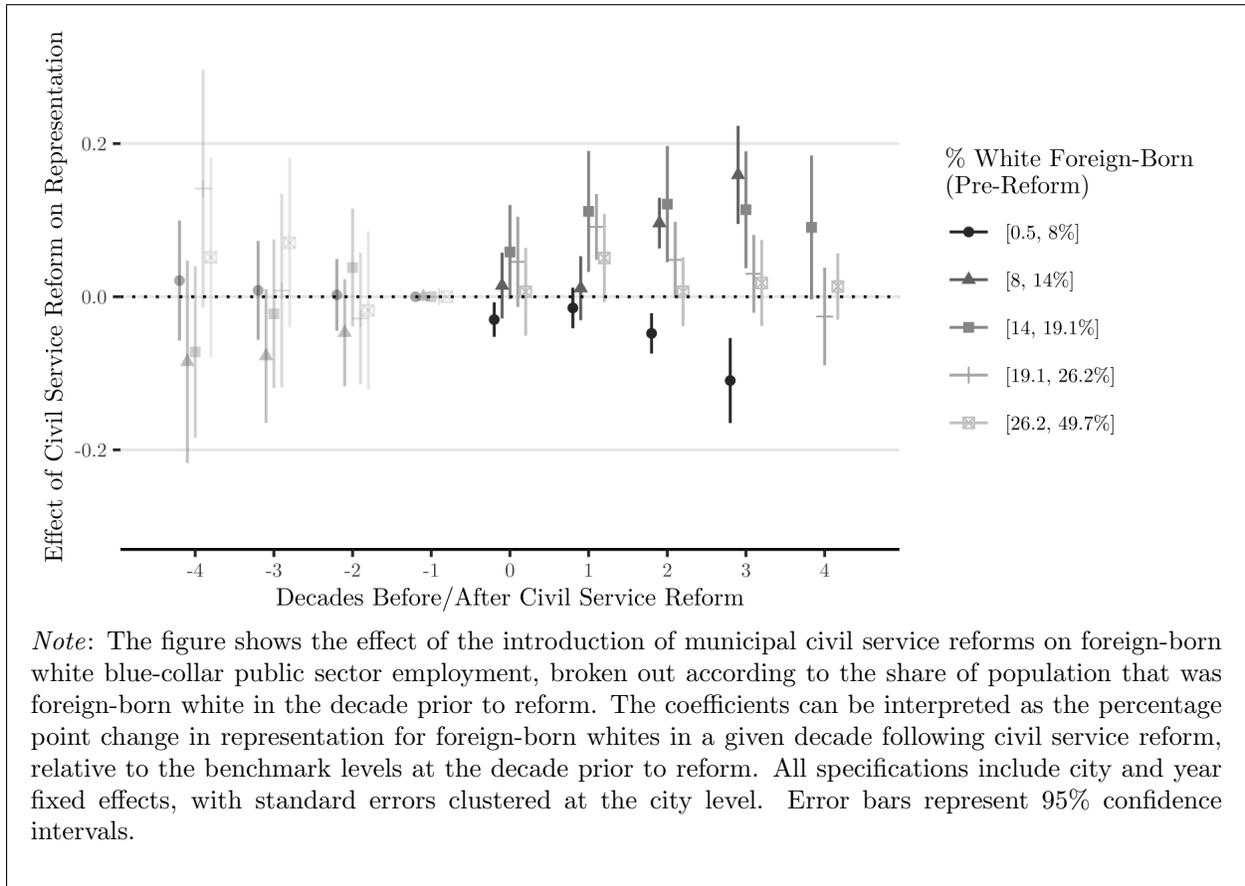


Figure A7—Effect of Civil Service Reforms, by City Size Population

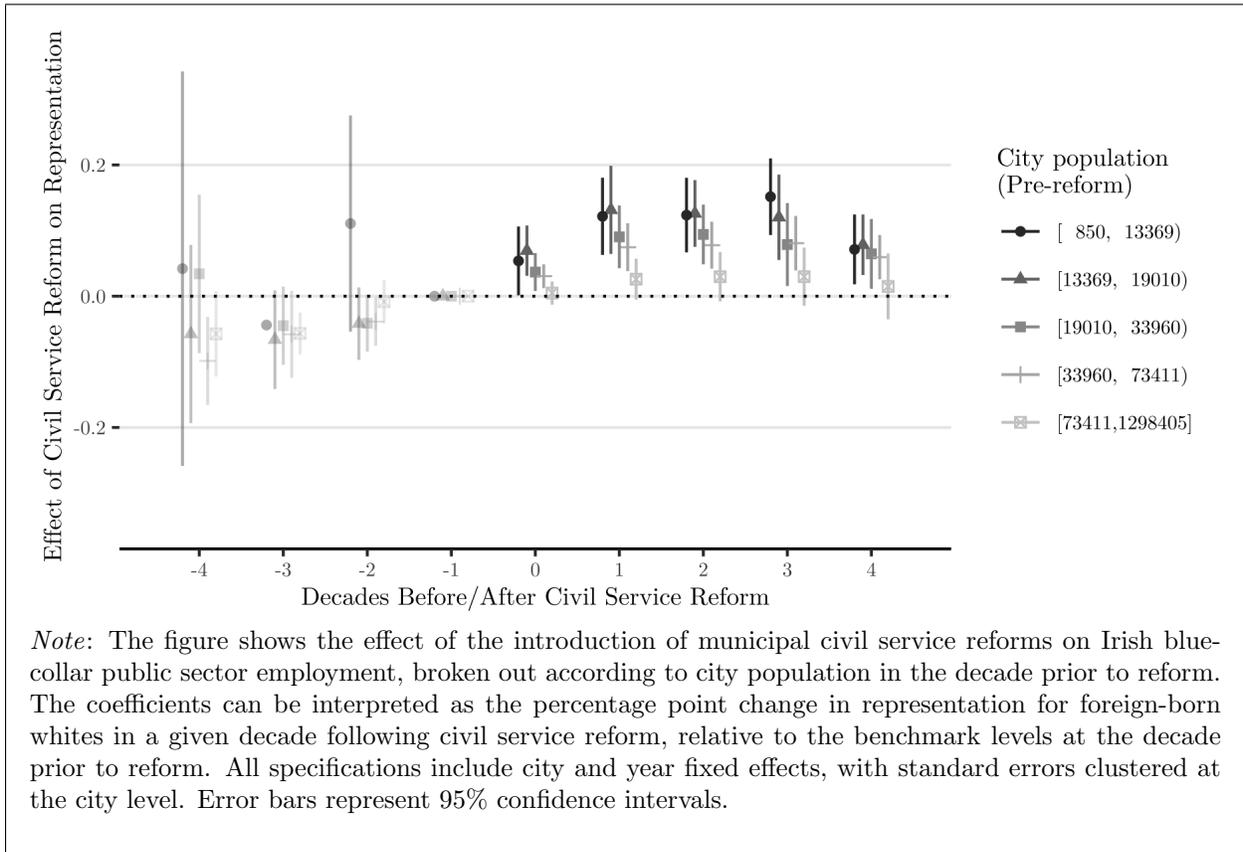


Figure A8—Effect of Civil Service Reforms, by Machine Dominance

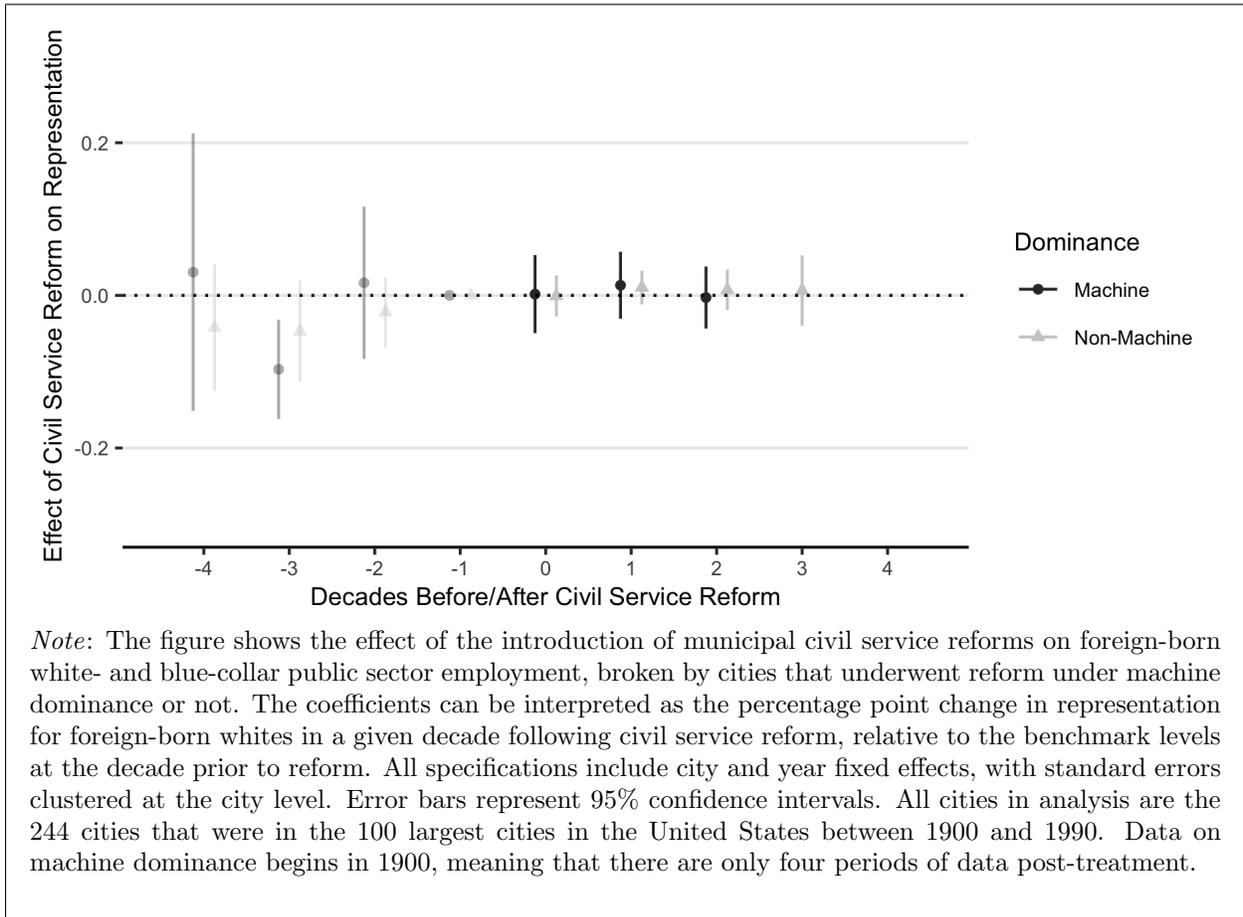


Figure A9—Effect of Civil Service Reforms on Representation in Blue Collar Jobs, by Region

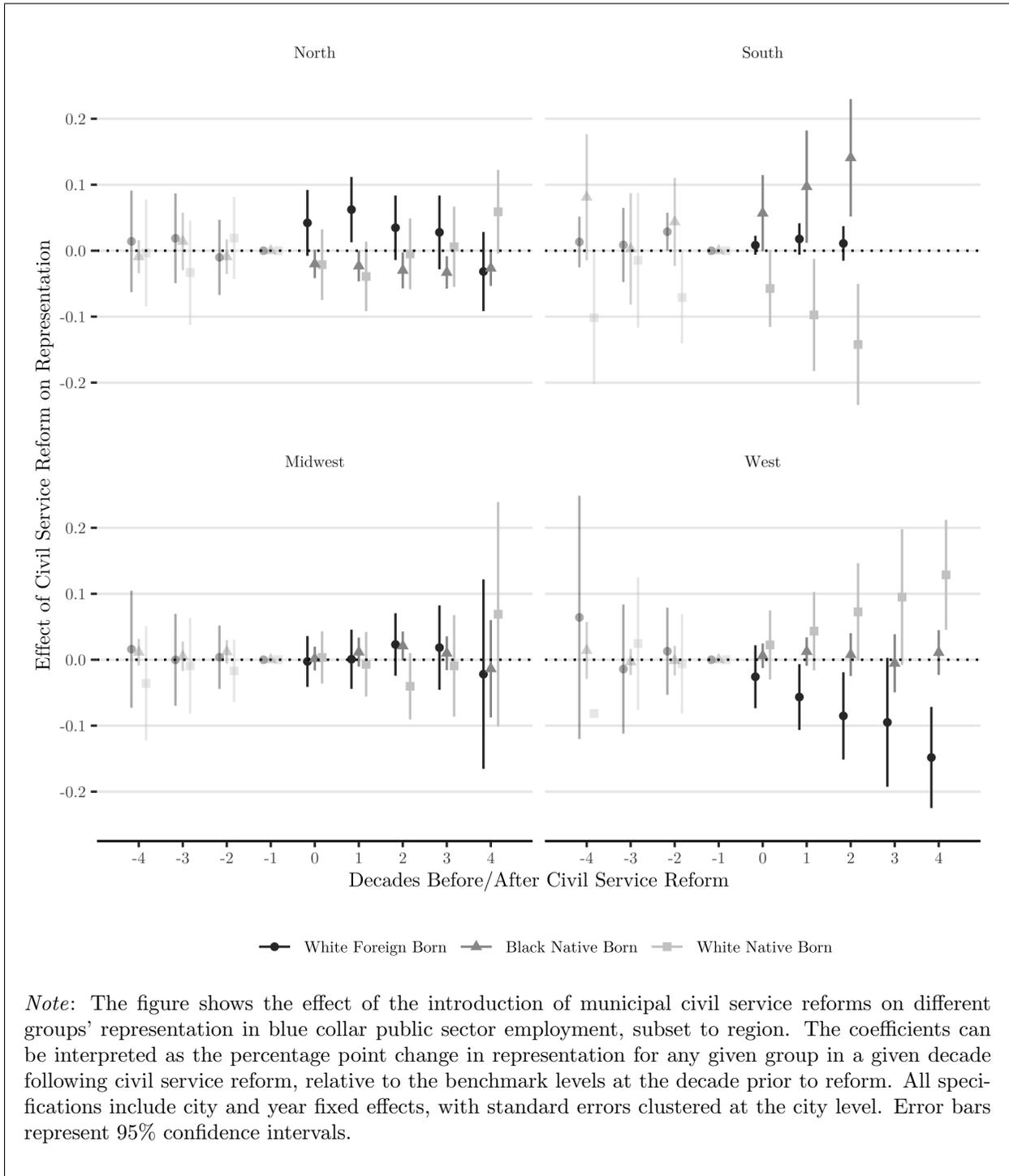


Figure A10—Effect of Civil Service Reforms on Representation in White Collar Jobs, by Region

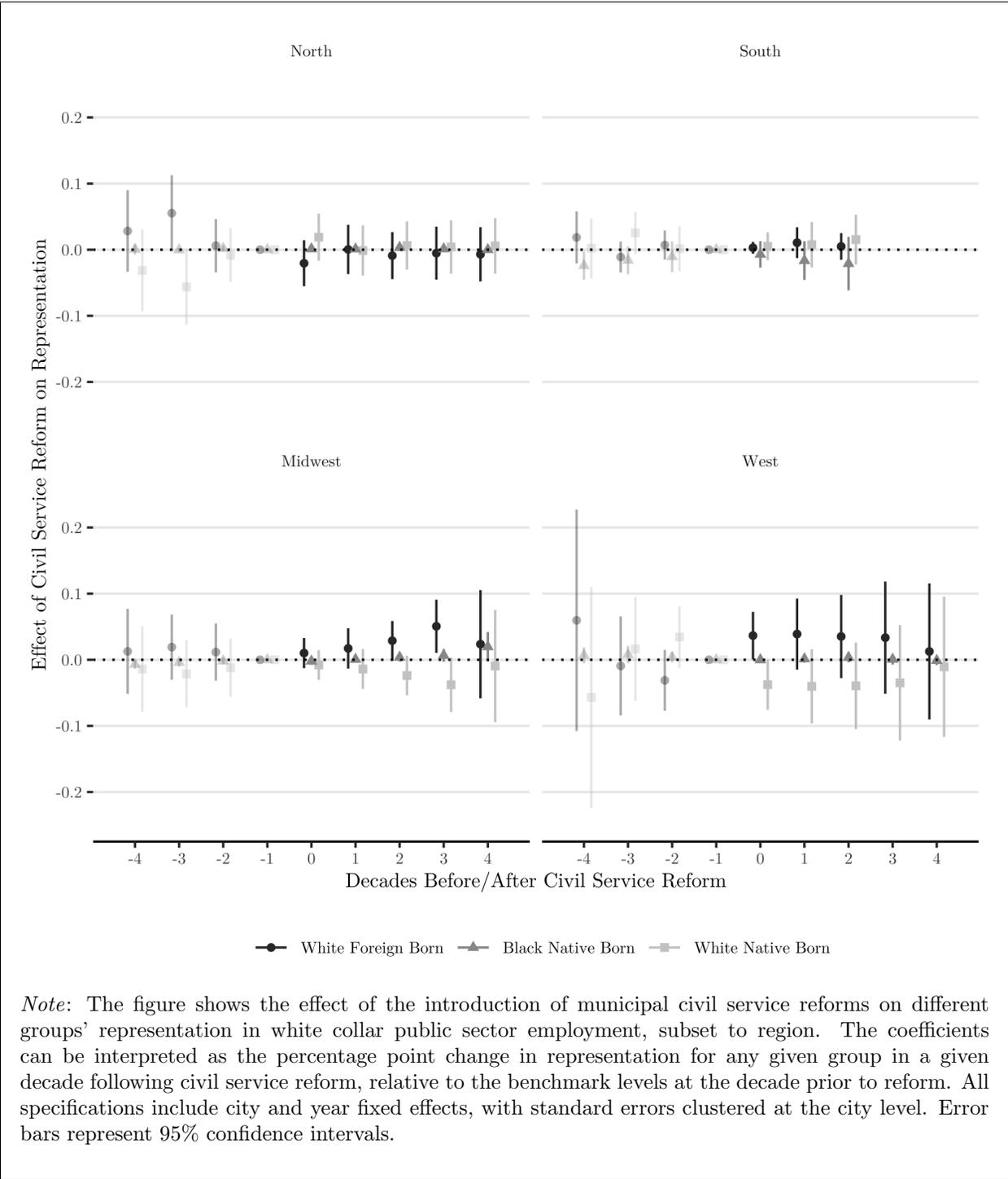
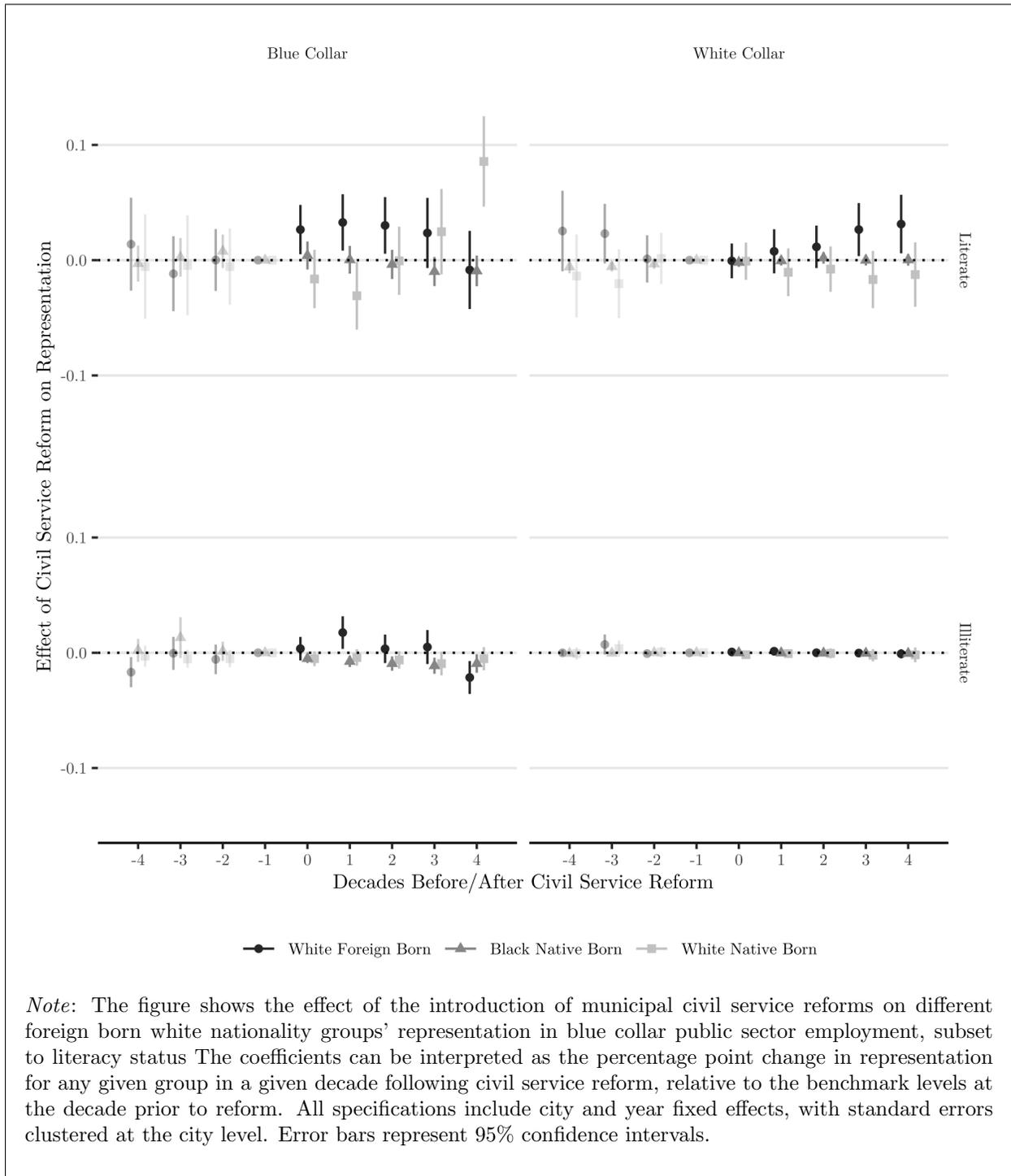


Figure A11—Effect of Civil Service Reforms on Representation, by Race and Nativity, Occupation, and Literacy



C.5 Main analysis – lagged dependent variable specification

We present alternative estimates of the effect of civil service on group representation using a lagged dependent variable specification, formally:

$$Y_{it} = \alpha + \beta reform_{it} + \lambda prop_{it} + \gamma Y_{i,t-1} + \epsilon_{it}$$

Where Y_{it} is the share of (native born whites) in (blue collar) municipal civil service jobs in city i in year t . $reform$ is a binary indicator whether city i has adopted civil service reforms in year t . We include a time varying control of the underlying proportion of (native born whites) in city i in year t , which we include to obtain estimates interpretable as the over or underrepresentation of specific groups. $Y_{i,t-1}$ is the lagged dependent variable, the share of (native born whites) in (blue collar) municipal civil service jobs in city i in year $t - 1$, which is the previous decennial census year.

Table A4—Effect of Civil Service Reform, Lagged-DV Models

Group	Occupation	Estimate	SE	P-Value
Native B.	White Collar	0.002	0.001	0.030
Native B.	Blue Collar	-0.010	0.005	0.042
Foreign W.	White Collar	-0.042	0.005	0
Foreign W.	Blue Collar	0.014	0.008	0.068
German	White Collar	-0.004	0.003	0.135
German	Blue Collar	0.003	0.004	0.461
Irish	White Collar	-0.011	0.003	0
Irish	Blue Collar	0.034	0.006	0
Italian	White Collar	-0.0003	0.001	0.756
Italian	Blue Collar	-0.003	0.004	0.449
Native W.	White Collar	0.028	0.006	0
Native W.	Blue Collar	-0.007	0.010	0.508
Polish	White Collar	-0.0003	0.0004	0.353
Polish	Blue Collar	-0.002	0.001	0.009
Russian	White Collar	-0.0004	0.001	0.449
Russian	Blue Collar	-0.001	0.001	0.435

C.6 Tabular Presentation of Main Event Study Figures

Table A5—Effect of Civil Service Reforms on Representation

	Blue Collar			White Collar		
	F. Born White	N. Born White	N. Born Black	F. Born White	N. Born White	N. Born Black
Decade = (-4, -3]	-0.002 (0.022)	-0.007 (0.024)	0.008 (0.012)	0.027 (0.018)	-0.013 (0.018)	-0.008* (0.003)
Decade = (-3, -2]	-0.012 (0.018)	-0.010 (0.022)	0.026* (0.013)	0.030* (0.015)	-0.016 (0.015)	-0.007** (0.002)
Decade = (-2, -1]	-0.005 (0.015)	-0.007 (0.017)	0.014 (0.010)	0.001 (0.011)	0.003 (0.011)	-0.003 (0.002)
Decade = (0, 1]	0.026* (0.012)	-0.015 (0.014)	-0.011 (0.008)	0.002 (0.008)	-0.002 (0.008)	-0.001 (0.002)
Decade = (1, 2]	0.049*** (0.014)	-0.039* (0.016)	-0.014+ (0.008)	0.011 (0.010)	-0.012 (0.010)	-0.001 (0.002)
Decade = (2, 3]	0.037* (0.014)	-0.032* (0.016)	-0.012 (0.009)	0.011 (0.009)	-0.013 (0.010)	0.001 (0.003)
Decade = (3, 4]	0.029 (0.018)	-0.016 (0.020)	-0.020* (0.009)	0.027* (0.012)	-0.027* (0.012)	0.000 (0.002)
Decade = (4, 5]	-0.022 (0.020)	0.023 (0.022)	-0.009 (0.012)	0.029* (0.013)	-0.029* (0.014)	-0.001 (0.003)
Population Share (%)	1.199*** (0.074)	0.802*** (0.066)	0.828*** (0.064)	0.627*** (0.064)	0.456*** (0.055)	0.104*** (0.018)
Num.Obs.	4408	4408	4408	4392	4392	4392
R2	0.672	0.616	0.734	0.519	0.496	0.288

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A6—Effect of Civil Service Reforms on Representation

	Blue Collar					White Collar				
	Irish	German	Italian	Polish	Russian	Irish	German	Italian	Polish	Russian
Decade = (-4, -3]	0.002 (0.016)	0.016 (0.012)	-0.005 (0.006)	-0.002 (0.003)	-0.001 (0.002)	0.005 (0.007)	0.000 (0.007)	-0.001 (0.001)	0.005+ (0.003)	-0.001 (0.001)
Decade = (-3, -2]	-0.017 (0.013)	0.000 (0.008)	0.014+ (0.008)	-0.001 (0.002)	-0.001 (0.002)	0.019+ (0.010)	0.004 (0.007)	0.000 (0.001)	-0.001 (0.000)	0.003 (0.003)
Decade = (-2, -1]	-0.003 (0.010)	0.010 (0.008)	-0.004 (0.006)	-0.002 (0.002)	0.001 (0.003)	-0.001 (0.005)	-0.004 (0.004)	-0.001 (0.001)	0.000 (0.001)	0.000 (0.001)
Decade = (0, 1]	0.020* (0.008)	0.003 (0.005)	-0.003 (0.006)	-0.002 (0.003)	-0.003 (0.002)	0.003 (0.004)	0.000 (0.003)	0.000 (0.001)	0.000 (0.000)	0.000 (0.000)
Decade = (1, 2]	0.048*** (0.012)	0.005 (0.006)	-0.003 (0.007)	-0.001 (0.002)	-0.004+ (0.002)	0.002 (0.005)	0.005 (0.005)	0.001 (0.002)	0.000 (0.000)	0.001 (0.001)
Decade = (2, 3]	0.028* (0.012)	0.006 (0.007)	-0.005 (0.007)	0.000 (0.002)	-0.003 (0.002)	0.006 (0.005)	0.007 (0.004)	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)
Decade = (3, 4]	0.018 (0.016)	0.004 (0.006)	-0.002 (0.010)	-0.004 (0.003)	-0.004+ (0.002)	0.011+ (0.006)	0.011* (0.005)	-0.002 (0.001)	0.000 (0.001)	0.001 (0.001)
Decade = (4, 5]	-0.037* (0.017)	0.006 (0.007)	-0.026** (0.009)	-0.005 (0.003)	0.000 (0.003)	0.006 (0.008)	0.011+ (0.006)	-0.002 (0.001)	-0.001 (0.001)	0.001 (0.001)
Population Share (%)	0.311** (0.118)	0.931*** (0.118)	1.909*** (0.209)	0.808*** (0.129)	0.320** (0.114)	0.219** (0.076)	0.673*** (0.083)	0.132*** (0.028)	0.172* (0.084)	0.197+ (0.105)
Num.Obs.	4408	4408	4408	4408	4408	4392	4392	4392	4392	4392
R2	0.554	0.508	0.585	0.563	0.438	0.448	0.443	0.370	0.345	0.291

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A7—Effect of Civil Service Reforms on Representation, by Size of Bureaucracy

	Size of Bureaucracy, Pre-Reform				
	[1, 26)	[26, 58)	[58, 130)	[130, 300)	[300,5472]
Decade = (-4, -3]	0.043 (0.073)	0.100+ (0.058)	-0.044 (0.088)	-0.093** (0.032)	-0.024 (0.029)
Decade = (-3, -2]	0.021 (0.140)	0.070 (0.043)	-0.091* (0.036)	-0.047 (0.031)	-0.047* (0.018)
Decade = (-2, -1]	-0.028 (0.062)	0.021 (0.029)	-0.062** (0.021)	-0.043* (0.017)	0.008 (0.011)
Decade = (0, 1]	0.071* (0.032)	0.040* (0.017)	0.046*** (0.011)	0.011 (0.010)	0.012 (0.007)
Decade = (1, 2]	0.130*** (0.033)	0.093*** (0.023)	0.105*** (0.025)	0.055*** (0.013)	0.019 (0.015)
Decade = (2, 3]	0.134*** (0.024)	0.078*** (0.020)	0.103*** (0.022)	0.061** (0.018)	0.026 (0.024)
Decade = (3, 4]	0.154*** (0.028)	0.081*** (0.019)	0.101*** (0.028)	0.075* (0.032)	0.022 (0.027)
Decade = (4, 5]	0.090*** (0.024)	0.047** (0.017)	0.076*** (0.017)	0.043+ (0.024)	0.016 (0.035)
Population Share (%)	1.832** (0.545)	1.637* (0.702)	0.086 (0.582)	0.435 (0.323)	0.366+ (0.197)
Num.Obs.	254	273	253	320	364
R2	0.681	0.671	0.715	0.552	0.597

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A8—Effect of Civil Service Reforms on Representation, by Literacy

	Literate		Illiterate	
	Irish	German	Irish	German
Decade = (-4, -3]	0.008 (0.014)	0.015 (0.012)	-0.006 (0.004)	-0.001 (0.002)
Decade = (-3, -2]	-0.008 (0.012)	-0.001 (0.008)	-0.007* (0.003)	0.000 (0.002)
Decade = (-2, -1]	0.002 (0.009)	0.008 (0.007)	-0.006+ (0.003)	0.003 (0.002)
Decade = (0, 1]	0.022** (0.008)	0.004 (0.005)	0.001 (0.002)	0.001 (0.001)
Decade = (1, 2]	0.043*** (0.011)	0.005 (0.006)	0.008* (0.004)	0.002 (0.001)
Decade = (2, 3]	0.039*** (0.012)	0.004 (0.006)	-0.003 (0.003)	0.002 (0.001)
Decade = (3, 4]	0.033* (0.015)	0.003 (0.006)	-0.008* (0.004)	0.001 (0.001)
Decade = (4, 5]	-0.003 (0.017)	0.003 (0.007)	-0.017*** (0.004)	0.002 (0.002)
Population Share (%)	1.336*** (0.179)	1.169*** (0.106)	0.131*** (0.039)	0.066** (0.026)
Num.Obs.	4408	4408	4408	4408
R2	0.556	0.510	0.291	0.373

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

C.7 Tabular Presentation of Appendix Event Study Figures

Table A9—Effect of Civil Service Reforms on Representation

	Police Departments			Fire Departments		
	F. Born White	N. Born White	N. Born Black	F. Born White	N. Born White	N. Born Black
Decade = (-4, -3]	0.038* (0.018)	-0.020 (0.020)	-0.013*** (0.004)	0.031 (0.027)	-0.035 (0.030)	0.003 (0.013)
Decade = (-3, -2]	0.026+ (0.014)	-0.009 (0.016)	-0.009* (0.004)	0.033 (0.021)	-0.024 (0.021)	-0.008 (0.006)
Decade = (-2, -1]	-0.010 (0.010)	0.014 (0.011)	-0.005* (0.002)	0.008 (0.015)	-0.006 (0.015)	-0.002 (0.003)
Decade = (0, 1]	0.008 (0.009)	-0.011 (0.009)	0.000 (0.001)	0.004 (0.011)	0.000 (0.012)	-0.002 (0.002)
Decade = (1, 2]	-0.005 (0.012)	-0.003 (0.013)	0.003 (0.002)	-0.018 (0.015)	0.022 (0.016)	0.003 (0.005)
Decade = (2, 3]	-0.013 (0.013)	0.007 (0.015)	0.003 (0.003)	-0.006 (0.016)	0.019 (0.018)	-0.003 (0.005)
Decade = (3, 4]	-0.006 (0.018)	0.000 (0.020)	0.004 (0.004)	0.009 (0.021)	0.013 (0.023)	-0.006 (0.008)
Decade = (4, 5]	0.007 (0.020)	-0.017 (0.024)	0.007 (0.004)	0.002 (0.024)	0.017 (0.027)	-0.002 (0.008)
Population Share (%)	1.140*** (0.081)	0.740*** (0.090)	0.102** (0.038)	0.857*** (0.122)	0.732*** (0.124)	0.139+ (0.079)
Num.Obs.	3470	3470	3470	3500	3500	3500
R2	0.674	0.643	0.534	0.589	0.564	0.470

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A10—Effect of Civil Service Reforms on Representation

	Police Departments					Fire Departments				
	Irish	German	Italian	Polish	Russian	Irish	German	Italian	Polish	Russian
Decade = (-4, -3]	0.006 (0.014)	0.009 (0.009)	0.004* (0.002)	0.002+ (0.001)	0.001 (0.002)	-0.001 (0.015)	0.020 (0.020)	-0.003+ (0.002)	0.001 (0.001)	0.000 (0.001)
Decade = (-3, -2]	0.002 (0.011)	0.011+ (0.006)	0.002 (0.001)	0.000 (0.001)	-0.001 (0.001)	0.024 (0.017)	0.009 (0.010)	-0.001 (0.002)	0.000 (0.001)	0.000 (0.001)
Decade = (-2, -1]	-0.008 (0.007)	0.005 (0.005)	0.001 (0.002)	0.000 (0.001)	0.000 (0.001)	0.010 (0.010)	-0.004 (0.005)	-0.001 (0.001)	-0.001 (0.001)	0.000 (0.001)
Decade = (0, 1]	0.011 (0.007)	0.003 (0.003)	0.000 (0.002)	-0.001 (0.001)	-0.002* (0.001)	-0.002 (0.008)	0.004 (0.004)	-0.002 (0.001)	0.000 (0.002)	0.001 (0.001)
Decade = (1, 2]	0.008 (0.009)	0.004 (0.004)	-0.001 (0.002)	-0.002* (0.001)	-0.001+ (0.001)	-0.009 (0.011)	0.005 (0.006)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
Decade = (2, 3]	0.008 (0.011)	0.002 (0.005)	-0.004* (0.002)	-0.002+ (0.001)	0.000 (0.001)	-0.008 (0.013)	0.008 (0.007)	0.003 (0.002)	0.000 (0.001)	0.000 (0.001)
Decade = (3, 4]	0.015 (0.015)	0.003 (0.006)	-0.007*** (0.002)	-0.005** (0.002)	-0.002* (0.001)	0.002 (0.015)	0.017+ (0.011)	-0.001 (0.002)	0.000 (0.002)	0.000 (0.001)
Decade = (4, 5]	0.021 (0.018)	0.009 (0.006)	-0.007** (0.003)	-0.005* (0.002)	-0.001 (0.001)	-0.013 (0.019)	0.016 (0.010)	0.003 (0.003)	-0.001 (0.002)	-0.001 (0.001)
Population Share (%)	0.649*** (0.178)	0.889*** (0.107)	0.343*** (0.058)	0.492*** (0.132)	0.117* (0.055)	0.668* (0.291)	0.684*** (0.127)	0.187 (0.131)	0.389* (0.172)	-0.022 (0.021)
Num.Obs.	3470	3470	3470	3470	3470	3500	3500	3500	3500	3500
R2	0.585	0.548	0.484	0.530	0.349	0.441	0.424	0.608	0.520	0.481

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A11—Effect of Civil Service Reforms on Representation

	Foreign Born White (% Pre Reform)				
	[0.5, 8%]	[8, 14%]	[14, 19.1%]	[19.1, 26.2%]	[26.2, 49.7%]
Decade = (-4, -3]	0.021 (0.040)	-0.085 (0.067)	-0.072 (0.057)	0.141+ (0.079)	0.051 (0.067)
Decade = (-3, -2]	0.008 (0.033)	-0.078+ (0.045)	-0.022 (0.050)	0.008 (0.065)	0.071 (0.057)
Decade = (-2, -1]	0.002 (0.024)	-0.047 (0.036)	0.038 (0.039)	-0.028 (0.044)	-0.018 (0.053)
Decade = (0, 1]	-0.030* (0.011)	0.015 (0.022)	0.058+ (0.031)	0.046 (0.030)	0.006 (0.029)
Decade = (1, 2]	-0.015 (0.013)	0.011 (0.021)	0.111** (0.040)	0.091*** (0.022)	0.050+ (0.029)
Decade = (2, 3]	-0.048*** (0.013)	0.096*** (0.017)	0.121** (0.039)	0.048+ (0.025)	0.006 (0.023)
Decade = (3, 4]	-0.110*** (0.028)	0.159*** (0.033)	0.114** (0.039)	0.030 (0.026)	0.018 (0.029)
Population Share (%)	0.633* (0.275)	0.953+ (0.481)	1.323*** (0.274)	0.983*** (0.268)	1.322*** (0.166)
Decade = (4, 5]			0.091+ (0.048)	-0.026 (0.033)	0.013 (0.022)
Num.Obs.	290	241	267	307	359
R2	0.531	0.530	0.612	0.676	0.659

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A12—Effect of Civil Service Reforms on Representation

	City Population (Pre Reform)				
	[850, 13369)	[13369, 19010)	[19010, 33960)	[33960, 73411)	[73411, 1298405]
Decade = (-4, -3]	0.042 (0.153)	-0.058 (0.069)	0.034 (0.062)	-0.099** (0.034)	-0.057+ (0.033)
Decade = (-3, -2]	-0.044 (0.194)	-0.066+ (0.038)	-0.045 (0.030)	-0.058+ (0.034)	-0.057*** (0.016)
Decade = (-2, -1]	0.111 (0.084)	-0.042 (0.028)	-0.041+ (0.022)	-0.039* (0.019)	-0.008 (0.017)
Decade = (0, 1]	0.054* (0.027)	0.069*** (0.020)	0.037* (0.015)	0.031** (0.009)	0.005 (0.009)
Decade = (1, 2]	0.122*** (0.030)	0.132*** (0.034)	0.090*** (0.024)	0.075*** (0.019)	0.026 (0.016)
Decade = (2, 3]	0.124*** (0.029)	0.126*** (0.026)	0.094*** (0.023)	0.078*** (0.018)	0.030 (0.019)
Decade = (3, 4]	0.152*** (0.030)	0.120*** (0.033)	0.079* (0.032)	0.081*** (0.021)	0.030 (0.023)
Decade = (4, 5]	0.071* (0.027)	0.078** (0.024)	0.065* (0.027)	0.060** (0.017)	0.015 (0.026)
Population Share (%)	1.998+ (1.047)	1.762+ (1.037)	1.206+ (0.687)	0.555 (0.333)	0.227 (0.166)
Num.Obs.	223	234	287	329	391
R2	0.683	0.699	0.642	0.677	0.577

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A13—Effect of Civil Service Reforms on Representation

	Machine Dominance	
	Machine	non-Machine
Decade = (-4, -3]	-0.042 (0.042)	0.030 (0.093)
Decade = (-3, -2]	-0.047 (0.034)	-0.097* (0.033)
Decade = (-2, -1]	-0.022 (0.023)	0.016 (0.051)
Decade = (0, 1]	-0.001 (0.014)	0.002 (0.026)
Decade = (1, 2]	0.010 (0.011)	0.013 (0.022)
Decade = (2, 3]	0.007 (0.013)	-0.003 (0.021)
Decade = (3, 4]	0.006 (0.023)	
Population Share (%)	0.966*** (0.226)	1.078* (0.299)
Num.Obs.	472	61
R2	0.664	0.777

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A14—Effect of Civil Service Reforms on Representation, Region

	F. Born White				N. Born White				N. Born Black			
	North	South	Midwest	West	North	South	Midwest	West	North	South	Midwest	West
Decade = (-4, -3]	0.014 (0.039)	0.013 (0.020)	0.016 (0.045)	0.064 (0.094)	-0.003 (0.041)	-0.102 (0.051)	-0.036 (0.044)	-0.082 (0.094)	-0.009 (0.013)	0.081 (0.049)	0.011 (0.010)	0.014 (0.022)
Decade = (-3, -2]	0.019 (0.035)	0.009 (0.029)	0.000 (0.035)	-0.014 (0.050)	-0.033 (0.040)	-0.015 (0.052)	-0.009 (0.037)	0.025 (0.051)	0.014 (0.022)	0.003 (0.043)	0.005 (0.011)	-0.003 (0.010)
Decade = (-2, -1]	-0.010 (0.029)	0.029 (0.015)	0.004 (0.025)	0.013 (0.034)	0.019 (0.032)	-0.071 (0.036)	-0.017 (0.024)	-0.006 (0.039)	-0.009 (0.013)	0.044 (0.034)	0.012 (0.009)	-0.001 (0.011)
Decade = (0, 1]	0.042 (0.025)	0.008 (0.007)	-0.003 (0.020)	-0.026 (0.024)	-0.021 (0.027)	-0.057 (0.030)	0.004 (0.020)	0.022 (0.027)	-0.020 (0.011)	0.057 (0.029)	0.002 (0.009)	0.006 (0.010)
Decade = (1, 2]	0.062 (0.025)	0.018 (0.012)	0.001 (0.023)	-0.057 (0.025)	-0.039 (0.027)	-0.097 (0.043)	-0.007 (0.025)	0.043 (0.030)	-0.023 (0.012)	0.097 (0.043)	0.012 (0.011)	0.012 (0.011)
Decade = (2, 3]	0.035 (0.025)	0.011 (0.013)	0.023 (0.024)	-0.085 (0.034)	-0.005 (0.027)	-0.142 (0.047)	-0.040 (0.026)	0.072 (0.038)	-0.030 (0.014)	0.141 (0.045)	0.021 (0.011)	0.008 (0.017)
Decade = (3, 4]	0.028 (0.029)		0.018 (0.033)	-0.095 (0.050)	0.006 (0.031)		-0.009 (0.039)	0.095 (0.053)	-0.033 (0.013)		0.010 (0.013)	-0.005 (0.023)
Decade = (4, 5]	-0.032 (0.031)		-0.022 (0.073)	-0.148 (0.039)	0.059 (0.032)		0.069 (0.087)	0.129 (0.043)	-0.027 (0.014)		-0.014 (0.038)	0.011 (0.017)
Population Share (%)	1.019 (0.126)	1.011 (0.202)	1.127 (0.132)	0.982 (0.444)	1.011 (0.134)	0.718 (0.117)	1.017 (0.139)	1.196 (0.447)	1.247 (0.236)	0.696 (0.103)	0.354 (0.130)	1.450 (0.500)
Num.Obs.	1738	874	1409	364	1738	874	1409	364	1738	874	1409	364
R2	0.602	0.636	0.690	0.635	0.579	0.686	0.658	0.631	0.508	0.727	0.645	0.549

Table A15—Effect of Civil Service Reforms on Representation, Literacy

	Literate			Illiterate		
	F. Born White	N. Born White	N. Born Black	F. Born White	N. Born White	N. Born Black
Decade = (-4, -3]	0.014 (0.021)	-0.006 (0.023)	-0.003 (0.008)	-0.017* (0.007)	-0.003 (0.005)	0.002 (0.005)
Decade = (-3, -2]	-0.012 (0.017)	-0.004 (0.022)	0.002 (0.009)	0.000 (0.007)	-0.005 (0.004)	0.013 (0.009)
Decade = (-2, -1]	0.000 (0.014)	-0.006 (0.017)	0.008 (0.007)	-0.006 (0.007)	-0.005 (0.004)	0.001 (0.004)
Decade = (0, 1]	0.027* (0.011)	-0.016 (0.013)	0.004 (0.006)	0.004 (0.005)	-0.005 (0.003)	-0.005* (0.002)
Decade = (1, 2]	0.033** (0.012)	-0.031* (0.015)	0.000 (0.006)	0.018* (0.007)	-0.004 (0.004)	-0.008** (0.002)
Decade = (2, 3]	0.030* (0.013)	-0.001 (0.015)	-0.004 (0.006)	0.003 (0.006)	-0.006 (0.004)	-0.009** (0.003)
Decade = (3, 4]	0.024 (0.016)	0.025 (0.019)	-0.010 (0.006)	0.005 (0.008)	-0.009+ (0.005)	-0.011** (0.004)
Decade = (4, 5]	-0.008 (0.017)	0.086*** (0.020)	-0.009 (0.007)	-0.021** (0.007)	-0.005 (0.005)	-0.009* (0.004)
Population Share (%)	1.466*** (0.049)	1.107*** (0.047)	1.399*** (0.047)	0.459*** (0.083)	0.249+ (0.149)	0.612*** (0.066)
Num.Obs.	4408	4408	4408	4408	4408	4408
R2	0.683	0.833	0.754	0.416	0.312	0.617

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A16—Effect of Civil Service Reforms on Representation, Literacy

	Literate			Illiterate		
	F. Born White	N. Born White	N. Born Black	F. Born White	N. Born White	N. Born Black
Decade = (-4, -3]	0.025 (0.018)	-0.014 (0.018)	-0.006* (0.003)	0.000 (0.001)	-0.001 (0.003)	-0.001 (0.001)
Decade = (-3, -2]	0.023+ (0.013)	-0.020 (0.015)	-0.006** (0.002)	0.007 (0.004)	0.004 (0.004)	0.000 (0.001)
Decade = (-2, -1]	0.001 (0.010)	0.001 (0.011)	-0.003 (0.002)	-0.001 (0.001)	0.001 (0.002)	0.000 (0.001)
Decade = (0, 1]	-0.001 (0.008)	-0.001 (0.008)	-0.002 (0.002)	0.001* (0.000)	-0.002 (0.001)	0.000 (0.000)
Decade = (1, 2]	0.008 (0.010)	-0.011 (0.011)	-0.001 (0.002)	0.001 (0.001)	-0.001 (0.002)	0.000 (0.000)
Decade = (2, 3]	0.012 (0.009)	-0.008 (0.010)	0.002 (0.003)	0.000 (0.001)	-0.001 (0.002)	0.000 (0.000)
Decade = (3, 4]	0.027* (0.012)	-0.017 (0.013)	0.000 (0.002)	0.000 (0.001)	-0.002 (0.003)	0.000 (0.000)
Decade = (4, 5]	0.031* (0.013)	-0.012 (0.014)	0.000 (0.003)	-0.001 (0.002)	-0.002 (0.003)	-0.001 (0.001)
Population Share (%)	0.716*** (0.045)	0.423*** (0.043)	0.054*** (0.012)	0.044 (0.035)	0.341* (0.144)	0.038* (0.015)
Num.Obs.	4392	4392	4392	4392	4392	4392
R2	0.540	0.933	0.230	0.243	0.266	0.225

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001