The Political and Economic Effects of Progressive Era Reforms in U.S. Cities: Evidence from Newly Digitized Data *

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August 29, 2022

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Abstract

How did Progressive era reforms affect the lives of urban residents across U.S. cities? The historical record is unclear. Some scholars argue that many of the progressive reforms were motivated by nativist and racist animus and explicitly designed to benefit white business elites at the expense of disadvantaged groups. Others point out that reformers often sought to improve urban living and working conditions and expand access to education, which generated new opportunities for social mobility. We inform this debate leveraging new data on 455 U.S. cities from 1900-1940 combining *i)* dates of adoption of reform-style government, *ii)* deanonymized census data, *iii)* data on political participation, and *iv)* detailed municipal budget data. Using a difference-in-differences design, we document the impact of Progressive reforms on political participation, public goods spending, and the relative socioeconomic well-being of black, immigrant, and working class residents vis-a-vis whites, natives, and business elites. Despite finding that voter turnout decreased in reformed cities, we uncover only a modest increase in earnings inequality across more and less advantaged groups and no significant differences in expenditure patterns as a consequence of reform. This approach provides a comprehensive portrait of the legacy of Progressive municipal institutions and suggests that, on average, the reforms of this era may have exacerbated political inequality more than economic inequality, at least in the first decades following their adoption.

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1 Introduction

How did the municipal reform movement of the Progressive era affect the social and economic lives of urban residents? A lively historical debate offers conflicting accounts as to whether the Progressive agenda helped or harmed less advantaged communities, including immigrants, African Americans, and the less affluent. Some researchers emphasize the fact that reformers of this era sought to clean up government corruption, address the poor living conditions of the working class, introduce basic forms of social insurance, and end exploitative labor practices (Bremner 1956; Davis 1984; Faulkner 1937). But other scholars have pointed out that the Progressive movement was comprised primarily of white, Protestant, and highly educated middle and upper-class Americans (Weinstein 1969; Hays 1964; Buenker 1973). This research tradition concludes that racist and nativist streaks permeated many of the movement's goals, and achieving reform often required disenfranchising poor, working class, and immigrant voters (Bridges 1999).

The question of how harmful or beneficial the Progressive agenda was for these groups remains unresolved in part because the historiography often focuses on the experiences of particular communities in particular cities. Scholars have documented the rise and fall of Irish machines in major cities like New York in the early 20th century (Erie 1990), described how Italians fared in Boston's West End (Gans et al. 1982), and analyzed the political dynamics of Germans, Irish, and Poles in Detroit (Zunz 2000). Bridges (1999) explores the ascent of municipal reformers in the Southwest with a focus on the political participation of the poor and racial minorities, while Stone, Price, and Stone (1940) describes the opposition of working class residents to reforms in nine urban cities. While these rich case studies offer nuanced accounts of who benefited from Progressive policies across specific local contexts, to date we largely lack systematic empirical research on the overall effects of the Progressive reform movement on the socioeconomic lives of urban residents across the U.S.[1]

[1]For a review of the historical and sociological literature, see Fox (2012) and Leonard (2016).

We bring new data to bear on these old questions, including (1) de-anonymized census records at the individual level, (2) newly digitized city financial statistics, (3) dates of municipal reform which we hand collected from primary sources, and (4) estimates of electoral participation at the county level. Collectively, our data cover 455 U.S. cities and span the period from 1900 to 1940. We focus specifically on reforms that led to the adoption of a council-manager or city commissioner form of government. Considered the most extreme example of municipal reform (e.g. Holli 1969; Bernard and Rice 1975), this change was often accompanied by the adoption of at-large elections and non-partisan ballots Banfield and Wilson (1966); Bridges (1999) and serves as an effective proxy for when Progressives gained control of the city government. In addition to being one of the most institutionally dramatic reforms of this era, changes to the structure of government have been one of the longest lasting Progressive legacies. We further discuss the theoretical and empirical rationale for focusing on this particular reform in section 2.

Our research design exploits the fact that U.S. cities varied in whether and when they reformed. This allows us to study how city life changed around the time of the reform by comparing changes in socioeconomic and political outcomes of various groups in reformed cities vis-a-vis in cities that did not reform. In order to avoid bias arising from heterogeneous treatment effects in staggered difference-in-differences designs (e.g. Xu 2017; Goodman-Bacon 2021; De Chaisemartin and d'Haultfoeuille 2020), we follow the approach proposed by Cengiz et al. (2019) and compare reform cities only to cities that never reform. Importantly, since the timing of reform cannot be considered random, we then employ a weighting strategy introduced by (Hazlett and Xu 2018), which ensures that outcomes in reform and non-reform cities follow similar trends in the pre-reform period.

We use individual-level census data covering the period 1900-1940 to construct measures of the socioeconomic standing of several groups of urban residents. In particular, we compare the outcomes of more advantaged groups—natives, whites, and members of the business elite—to those of immigrant, black, and non-business workers. As our primary measure of economic well-being we use the average predicted wage earnings of each group, which we compute following the procedure outlined in Abramitzky et al. (2021). We also analyze four additional socioeconomic outcomes: the employment rate within each group, the share holding a local government job, the group's literacy rate, and city-level occupational segregation across groups.

Overall, our results show that the adoption of Progressive era reforms modestly increased the earnings gap between more and less advantaged residents, but the magnitude of the effects is small. The reform had no significant impact on the earnings of immigrant and native residents. The earnings of business residents increased by 0.6 percent following the reform, while the earnings of black and non-business residents decreased by 1.5 percent and 0.7 percent, respectively. These effects result in a statistically significant but relatively small widening of the white-black and business-nonbusiness earnings gap. Consistent with these limited effects on earnings, we show that reforms did not lead to systematic changes across other socioeconomic outcomes. Crucially, we find that failing to account for the endogeneity in the timing of reform adoption would significantly affect our estimates: an estimation strategy that does not employ our weighting method would result in estimates that are twice as large as the true treatment effect on the native-immigrant gap. While we observe some evidence of distributional effects, we show that a wider earnings gap between more and less advantaged groups was not the price for more city growth: we document that reform cities did not experience higher aggregate growth in earnings, population, or employment.

We next examine the effect of reform on political participation. As we discuss in the next section, there is little disagreement in the historiography that one of the consequences of the municipal reform movement was to restrict popular participation in politics (Martin 1933; Banfield and Wilson 1966; Judd and Hinze 2018). Fox (2012) provides suggestive evidence that, in aggregate, voter turnout in presidential elections decreased more in regions with a large number of reform cities, and Hajnal and Lewis (2003) shows that turnout is lower today in California cities that maintain various Progressive-era reforms. We offer new evidence that the adoption of reform-style government decreased turnout. Using historical data from Clubb, Flanigan, and Zingale (2006), we show that turnout in presidential and congressional elections decreased significantly in counties where cities reformed. This finding is consistent with ample evidence suggesting that the reforms enacted during this period stripped political power from immigrants, minorities, and poorer residents (Holli 1969; Karnig and Walter 1983; Caren 2007).

Finally, we also show that the reduction in electoral participation did not go hand in hand with significant changes in public goods provision. Leveraging newly digitized data from the Financial Statistics of Cities Bulletins, we show that cities that reformed did not decrease public spending, nor significantly change the allocation of government funds across different types of public goods. These results suggest that, on average, the Progressive agenda neither helped nor harmed disadvantaged communities to the extent suggested by existing literature, at least economically. The well-documented reduction in terms of electoral participation and descriptive representation of immigrants and working class residents (Weinstein 1969; Davidson and Korbel 1981; Trounstine 2009) appears to have only modestly hurt their economic prospects. In the next section, we describe the historical background of the municipal reform movement and flesh out the theoretical debates at stake.

2 Historical Context and Theoretical Perspectives

The late 19th and early 20th century marked a period of rapid urban growth in the United States. Cities spread beyond their original boundaries, and suburban communities emerged thanks to the development of new methods of urban transportation in the second half of the 19th century (Jackson 1987). This process of rapid urban expansion led to increasing administrative challenges, and city government often struggled to provide urban residents with adequate services (Glaab and Brown 1967). Population growth and density were associated with a rise in diseases, fires, water pollution, and overcrowding, which tended to disproportionately impact the living conditions of working class and immigrant residents in urban cores (Trounstine 2018). In this period, political machines often emerged in American cities as important providers of services for immigrants as well as poor communities generally and facilitated economic and social integration in exchange for political support (Schiesl 1977).

By the late 1800s, a reform movement began to emerge whose stated goal was to improve city life and living conditions and eliminate the graft, corruption, and patronage associated with machine-style politics (Buenker 1973; Renner and DeSantis 1993). Part of the broader Progressive movement in American politics, these municipal reformers sought to reorganize city administration. As pointed out by (Schiesl 1977) "Political reform appeared to be a matter of running municipal government along the lines of the business corporation." The reforms of this era centered around the introduction of the council-manager form of government, the introduction of at-large elections and non-partisan ballots, and civil service rules for municipal employees. However, while reformers often claimed that their agenda was designed to broadly improve city governance and provide better services for all residents (Stewart 1950), the movement combined contradictory impulses.

On the one hand, some historians have pointed out that the Progressive reform movement was fueled primarily by business interests and upper-middle class whites who feared the political influence wielded by foreigners, racial minorities, and poor people (Merton 1968; Fox 2012; Lane 1962). Although urban poverty shocked the sensibilities of Progressives, many reformers believed the solution to such societal ills could only be achieved by reigning in the power of the lower classes (Weinstein 1969; Leonard 2016; Banfield and Wilson 1966). To achieve their goals, reformers called for various structural measures that would limit the ability of immigrants and ethnic minorities to exercise political power. While often enacted under the guise of reducing election fraud and streamlining governance, reforms such as voter registration, at-large elections, and non-partisan ballots made it more difficult for poor and minority voters to cast ballots and achieve representation on city councils (Buenker 1973). Additionally, early accounts portrayed the political machines of this era as sources of political power and vehicles of upward mobility for immigrants (DiGaetano 1988). Perhaps not surprisingly, the most active opposition to these reform efforts came from immigrant and working class voters (Bridges and Kronick 1999).

On the other hand, subsequent research has demonstrated that most machines relied on petty favors rather than encouraging immigrant communities to organize around their common economic and political interests. City machines often benefited only one ethnic group at the expense of others (usually the Irish), and machine politicians did little to improve the dangerous living and working conditions experienced by many immigrants (Judd and Hinze 2018; Erie 1990; Trounstine 2009). Moreover, even as Progressive Era reformers sought to weaken immigrant voting blocs politically, they also made important improvements to workplace conditions, passed child labor laws, advocated for safer housing, and expanded social services (Fox 2012). According to Kirschner (1975), early scholarship on the Progressive movement tended to sing the praises of urban reformers. “Their generous efforts to ease the burdens of the poor by limiting the working hours of women and children, improving factory, housing and health conditions, and introducing rudimentary forms of social insurance, according to this interpretation, mark[ed] them as path breakers to the New Deal and the modern welfare state” (Kirschner 1975).

Ultimately, while historians largely agree that the government reforms of this era resulted in a reduced political participation of disadvantaged residents in cities (Stone, Price, and Stone 1940; Cassel 1986; Bridges and Kronick 1999), it is unclear what the overall socioeconomic impact was. While Progressives were often blatantly suspicious of immigrants, racial minorities, and the urban poor, their social policies may have still improved the economic well-being of those communities both directly and indirectly. As Leonard (2016) summarizes, “The great contradiction at the heart of Progressive Era reform was its view of the poor as victims deserving state uplift and as threats requiring state restraint. The unstable amalgam of compassion and contempt helps explain why Progressive Era reform lent a helping hand to those it deemed worthy of citizenship and employment while simultaneously narrowing that privileged circle by excluding the many it judged unworthy." The net effect on the economic and social lives of more disadvantaged groups and, more generally, which groups benefited when reformers gained power in cities, remains an empirical question that is largely unresolved.

We shed light on this question by analyzing socioeconomic outcomes for various groups of residents around the time during which a city switches to a council-manager or city commissioner form of government. While the Progressive movement was characterized by a series of reforms, we use the adoption of this new form of government as a proxy for Progressives gaining control of the apparatus of city government. Beyond being one of the most dramatic and long-lasting structural reforms of this era (Chambers 2000), this is one of the few reforms for which the date of adoption was systematically collected for every municipality across the country via the City Managers' Association (now the International City Management Association). Broadly, these reforms sought to remove power from elected mayors and city council members and place policymaking authority with appointed city managers or city commissioners. The goal was to streamline decision-making, increase efficiency, and—importantly—make it difficult for machines and party bosses to engage in patronage (Judd and Hinze 2018). Existing historical work suggests that reforming the city charter itself was the most extreme example of reform (e.g. Holli 1969; Bernard and Rice 1975), and the vast majority of council-manager systems adopted other Progressive reforms at some point (Banfield and Wilson 1966).

We emphasize that adopting reform-style government represents a bundled treatment that was often accompanied by other reforms—such as non-partisan ballots and new voter registration rules—and marked a shift both culturally and politically (Bridges 1999). Some of these Progressive reforms and policies likely had direct effects on the jobs and social lives of less advantaged city residents. For example, we know that at-large elections and non-partisan ballots reduced turnout in immigrant neighborhoods and led to a higher proportion of occupational elites on city councils (Fox 2012; Cassel 1986). Civil service reforms also made it more difficult for immigrants to obtain municipal jobs via patronage, although recent research has demonstrated that these concerns are likely overstated (Kuipers and Sahn 2022). We view the adoption of reform-style government as a proxy for Progressives gaining power and being able to advance their agenda. Any of the effects we observe are almost certainly the result of the multiple reforms that were part of the Progressive agenda. The goal of this paper is not to isolate the impact of a specific reform, but to contribute to the debate about the overall effect of the Progressive legacy on the economic and social well-being of urban residents.

3 Data

3.1 Data Collection

In this paper, we study the effect of the adoption of reform-style government, which entailed a switch to a council-manager or city commissioner form of government. The City Managers' Association (now the International City Management Association) kept detailed historical records of the list of cities that adopted this reform, along with the date of adoption.[2] Drawing from the Municipal Yearbooks of 1934 and 1940 and archival records available in Rice (2014), we collected data on the year of adoption of reform-style government for the 1,100 largest cities in the U.S.

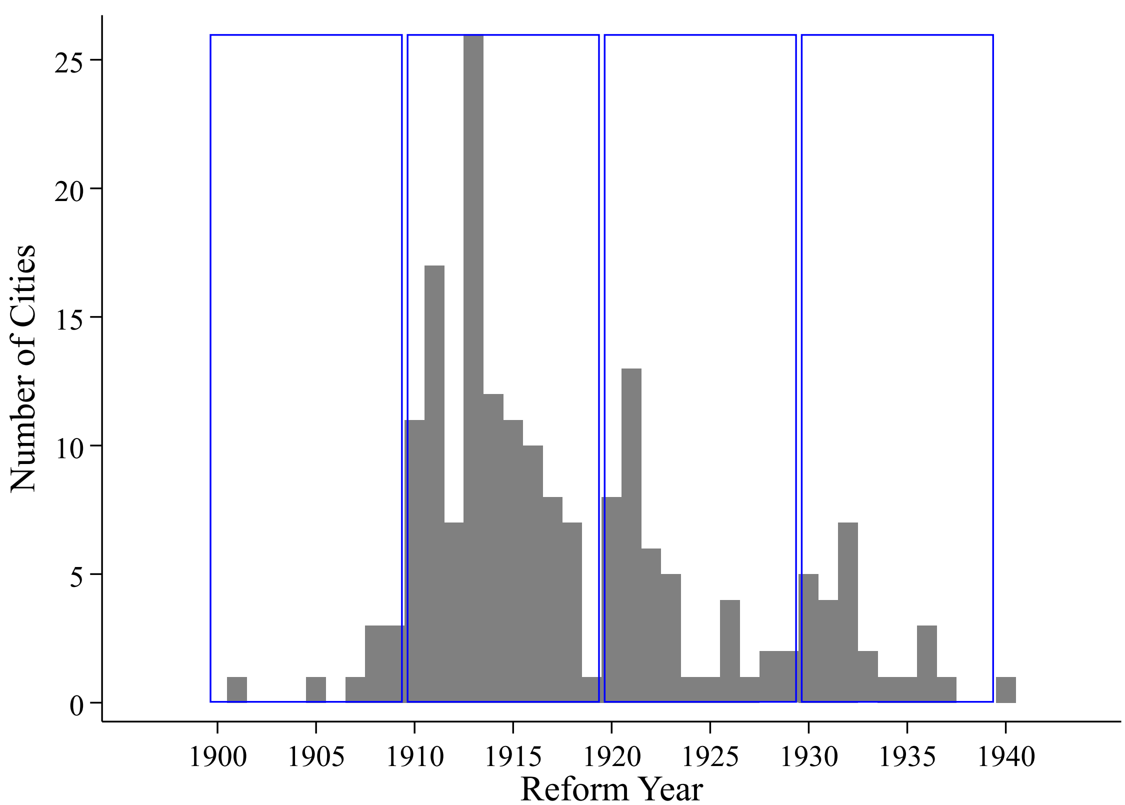
Data on socio-economic outcomes are constructed from individual-level census data available via the Integrated Public Use Microdata Sample (IPUMS) for the years 1900, 1910, 1920, 1930, and 1940. Every 72 years, the Census Bureau releases data at the individual level, which allows us to track a variety of outcomes for different groups of urban residents as cities experimented with new government institutions over the first half of the 20th century.

[2]Unfortunately, no systematic data exist on the year of adoption of at-large elections and non-partisan ballots, which were two of the other common reforms of this time. We attempted to hand collect this data from `newspapers.com` and by emailing local municipal archives but had little luck. However, as described in Section 2, historians describe how these reforms were usually introduced after a transition to the council-manager form of government.

Crucially, such comparisons are not possible with the more commonly used Census data aggregated at the location level, which does not allow one to construct socio-economic variables that vary both at the city- and at the group-level.

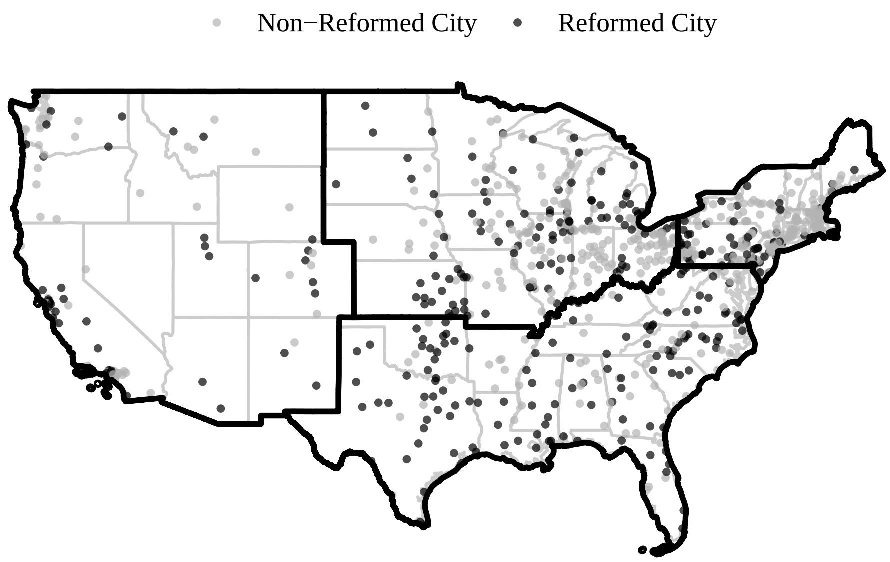
We combined data on the date of adoption of reform-style government with census data for the 1900-1940 period. In total, there were 455 cities that appeared in both the census data (for all five decades) and our dataset on municipal form of government, 186 of which reformed during our sample period. Figure 1 shows the number of cities that adopted a reform-style government in every year between 1900 and 1940. While the majority of reforms took place between 1910 and 1920, cities continued to reform their charters over the course of the sample.

Figure 1: Number of Reforms by Year



Notes: the plot above shows the year of adoption for each of the 186 reformed cities in our sample of 455 cities in the U.S. between 1900 and 1940. Blue lines highlight census decades.

Figure 2: Geographic Distribution of Reformed Cities



Notes: the plot above shows the geographic distribution of cities that reformed (in black) and did not reform (in gray) during the period 1900-1940.

Figure 2 depicts the geographic distribution of cities that reformed at some point during the 40 year period and cities that never reformed. Non-reformed cities were particularly common in the Northeast and Midwest. Examples of reformed cities can be found in every state, although they are particularly common in the South and Southwest.

To study the effect of the adoption of reform-style government on political participation, we rely on data on voter turnout in both congressional and presidential elections from 1900 to 1940 available from Clubb, Flanigan, and Zingale (2006). Unfortunately, the smallest administrative unit at which data on voter turnout is available is the county. We thus map each city in our sample to a county, and assign to the city the turnout in the county where the city is located.

Finally, to study the effects of reform on public spending, we digitized information on city financial spending from the yearly Financial Statistics of Cities Bulletins provided by the U.S.

Census Bureau between 1902 and 1940. Our efforts build on work by Trounstine (2018) and Janas (2022) who have also transcribed portions of these data. These reports were released by the Census Bureau yearly from 1902 to 1938 and contain detailed information on the revenues, expenditures, debts, and public service enterprises for all cities with a population above 30,000 (until 1931) and then for all cities with a population above 100,000 (from 1932 to 1938). Importantly, the data contain information not only on the aggregate amount of public expenditures, but also on the specific amount spent by the city for specific public services. Of the 455 cities in the sample that we used for our socioeconomic outcomes analyses, 136 appear in the Financial Statistics bulletins. For each available year, we digitized city expenditures on schools, fire and police services, sanitation, public health, highways, recreation, hospitals, as well as total municipal spending.

3.2 Variable Definition

To assess how different groups fared in reformed vs. non-reformed cities, we split the residents of each city along several dimensions. First, we divide residents between immigrants and natives: we define "immigrant" to include both foreign-born individuals and respondents whose parents were born outside the U.S.[3] Second, we divide residents between African American and non-immigrant white residents, relying on the *RACE* variable provided by IPUMS. Finally, we use census occupation codes to investigate whether Progressive reforms differentially affected business elites by dividing residents into those employed in business occupations and those employed in non-business professions. Specifically, we follow existing literature (Buchmann and McDaniel 2016: e.g.) and define "business" to include occupations classified under the Managers, Officials, and Proprietors category according to IPUMS.[4]

We use wage earnings as our measure of economic well-being of different groups. For each of these groups, we compute the variable *Predicted Log Earnings*, which reflects the predicted average wages earned by the members of the group. Specifically, while data on respondent

[3]For this classification, we rely on the variable *NATIVITY* provided by IPUMS.
[4]For specific details on the IPUMS variables used in each of our analyses, see the Appendix.

occupation exist over the course of the panel, the census only began collecting information on wages starting in 1940. Following the procedure outlined in Abramitzky et al. (2021), we first predict wages in 1940 based on occupation, age, and region. We then impute wages in previous census years based on the same characteristics.[5] While this measure cannot capture changes in earnings over time within an occupation or city, it reflects the local value of each resident's occupation had they performed it in 1940. Finally, we average predicted wages at the city-decade-group level, and we take its logarithm.

To further explore the socioeconomic impact of reform, as well as to explore possible mechanisms behind the relationship between reform and wages, we look at five additional outcomes available in the Census data. First, we calculate the variable *Employment*, which is the share of each group that is employed. This indicator is based on the IPUMS variables *labforce* and *classwrkr*, and additional details about variable construction can be found in the Appendix. Second, we construct the variable *Local Government Job*, which is an indicator that takes a value of one if an individual holds a job in "local government" as defined by industry classification in the census. Third, we use the variable *Literacy* as a measure of cultural assimilation and human capital. It is an indicator that takes a value of one if a respondent could read and write. Fourth, we construct the variable *Group Population Share*, which is the share of each group among the residents of a city.

Finally, we calculate the variable *Occupational segregation*, which indicates the degree to which workers belonging to different groups are clustered in different occupations. We employ two standard approaches to measure segregation: a dissimilarity index and an isolation index, both widely used measures in the literature (Cutler, Glaeser, and Vigdor 1999; Iceland, Weinberg, and Steinmetz 2002; Gentzkow and Shapiro 2011). The two indices are defined in a given city-year as:

$$Dissimilarity = \frac{1}{2}\sum_{k\in K}\left|\frac{immigrants_k}{immigrants} - \frac{natives_k}{natives}\right| \tag{1}$$

[5]We make predictions using only cities that had not reformed by 1940 to avoid projecting any consequences of reform into the past.

$$Isolation = \sum_{k \in K} \frac{immigrants_k}{immigrants} \frac{immigrants_k}{immigrants} - \frac{natives_k}{natives} \tag{2}$$

where k is one of the K occupations present in that city-year. Both indices range between 0 (no segregation) and 1 (perfect segregation). The dissimilarity index can be interpreted as the share of minority residents (or majority) that would need to switch occupations for the minority share to be uniform across the labor market. The isolation index measures the extent to which minority residents are only exposed to one another in their occupations (White 1986; Cutler, Glaeser, and Vigdor 1999; Gentzkow and Shapiro 2011).

With the exception of our occupational segregation measures, which by construction can be computed only at the city-year level, we compute all our measures both at the city-year-group level, to study the impact of reform on specific demographic groups, and at the city-year level, to study the aggregate impact of reform on the socio-economic evolution of a city. Additionally, in order to directly measure the distributional impacts of reform, we also calculate the gap in each measure between the more advantaged groups – natives, whites, and members of the business elites – and the more disadvantaged ones – immigrants, African Americans, and non-business workers.

4 Empirical Approach

Our goal is to study the effect of reform across U.S. cities at the turn of the 20th century. Our identification strategy exploits the staggered introduction of the reform across cities shown in Figure 1 to study the effect of progressive reforms on three sets of city-level outcomes: socioeconomic outcomes for different groups of residents, voter turnout, and public expenditures. Census outcomes are aggregated at the city-year-group and are recorded every decade. Turnout is measured at the county level and is available every two years for congressional elections and every four years for presidential elections. Finally, city budget outcomes are measured at the city-year level directly and are measured every year for the subset of cities for which this information is available. The standard difference-in-differences specification for our setting would be the following

$$y_{ct} = \gamma_c + \delta_t + \beta Reformed_{ct} + \varepsilon_{ct} \tag{3}$$

where y_{ct} is the outcome for city c and decade t. $Reformed_{ct}$ is an indicator variable that takes a value of 1 after city c reforms. City and year fixed effects are represented by γ_c and δ_t respectively.[6] For census outcomes, we are most interested in whether city reforms affect more and less advantaged residents differently. For every outcome, we always show results for less advantaged residents (immigrants, black people, and non-business workers) and more advantaged residents (natives, whites, and business people) separately, and we then show the effect on the gap in that outcome between the two groups. Standard errors are clustered by city.

Standard difference-in-differences regressions, like in Equation 3, are biased when the treatment goes into effect at different times for different units if treatment effects change over time (e.g. Xu 2017; Goodman-Bacon 2021; De Chaisemartin and d'Haultfoeuille 2020). This is likely to be the case in our setting if reform affects the fortunes of various groups differentially over the course of our panel. To avoid this source of bias, we follow the stacked approach proposed by Cengiz et al. (2019) and compare reform cities only to cities that never reform ("clean control" cities). As they propose, we create as many copies of each never-reformed city as treatment periods in our data. For instance, when looking at census data which is available for each decade during 1900-1940, we create four copies of never-reformed cities, one for each decade highlighted in Figure 1 in which treated cities reformed (1901-1910, 1911-1920, 1921-1930, 1931-1940). We refer to each set of reform cities and their corresponding never-reformed cities as a "timing group." We then compare reform cities only to the never-reformed cities in the same timing group by estimating:

[6] Across all analyses, we limit our data to cities for which we have data in all relevant years. This means that the exact number of treated and control units varies across analyses.

$$y_{cgt} = \gamma_{cg} + \delta_{tg} + \beta Reformed_{cgt} + \varepsilon_{cgt} \tag{4}$$

where g identifies the timing group, δ_{tg} represents period-by-timing group fixed effects, and γ_{cg} represents city-by-timing group fixed effects.[7] Standard errors are clustered at the city level. We can interpret β as the effect of reform under the assumption that reform and non-reform cities would have been on the same average trajectory had neither reformed.

Of course, the timing of reform is not random: cities may choose to adopt a city manager in response to changing socioeconomic conditions. For example, the reform movement gained strength in the west at the same time as many people were moving to the region. To address this issue, we re-weight our data to ensure that the never-reformed cities match the average outcome for the reform cities in their timing group before reform (Imai, Kim, and Wang 2018). This approach is similar to the strategy proposed in Hazlett and Xu (2018). Specifically, we use entropy balancing to find weights that minimize the difference between the average reform and never-reform cities on all pre-reform observations of the outcome while maintaining weights as close to 1 for all control units (Hainmueller 2012). This method is well-suited to cases with many treated units and few pre-treatment periods, which is not the case with standard synthetic control methods for panel data.

To investigate pre-trends and the dynamic evolution of the treatment effect, we also estimate a non-parametric event-study specification:

$$y_{cgt} = \gamma_{cg} + \delta_{tg} + \sum_{\tau=-3}^{+3} \beta_\tau Reformed_{cg} \times \mathbb{1}[t = \tau] + \varepsilon_{cgt} \tag{5}$$

where the coefficients of interest, β_τs, measure the change in outcomes of treated cities τ decades before or after treatment, relative to the decade preceding the introduction of reform in each city and compared to the change in outcomes of pure control cities.

[7]Note that city-by-timing group fixed effects are effectively city fixed effects in our analysis using census data. This is because, differently from our analysis using political and fiscal outcomes, which leverages more granular time variation, in our census analysis each pure control city enters each timing group for the same number of decades (all the decades in the 1900 to 1940 period).

5 Reforms Had Minimal Socioeconomic Impact

In this section, we begin by presenting our results on socioeconomic gaps between more vs less advantaged groups of city residents. Table 1 reports estimates from equation 4, which measures the impact of the reform on the evolution of earnings for different groups of residents. Columns 1-3 focus on immigrants versus natives, columns 4-5 focus on blacks versus whites, while columns 7-9 focus on residents in non-business versus business occupations. Overall, we find that the reform had at most moderate distributional effects. The reform led to a 1 percent reduction in earnings for immigrant residents (p-value 0.095), while it had a negligible impact for natives, resulting in an insignificant 0.007 increase in the native-immigrant earnings gap (i.e. the difference in log earnings between natives and immigrants).

Table 1: The Impact of Reform on Earnings Across Groups

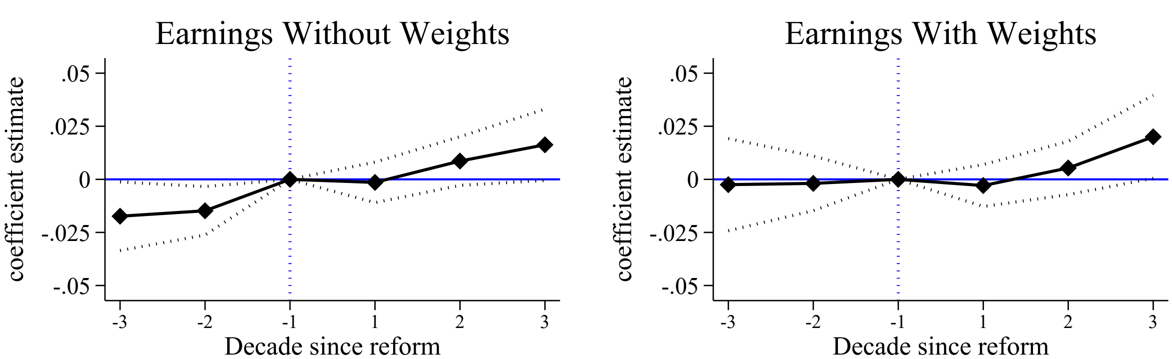
	Predicted Log Earnings								
	Immigrant (1)	Native (2)	Gap (3)	Black (4)	White (5)	Gap (6)	Non-Business (7)	Business (8)	Gap (9)
Reform	-0.010	-0.002	0.007	-0.015	0.002	0.017	-0.007	0.006	0.013
	(0.006)	(0.004)	(0.006)	(0.008)	(0.005)	(0.008)	(0.004)	(0.002)	(0.004)
Num Obs	6,305	6,305	6,305	4,845	4,845	4,845	6,310	6,310	6,310
Num Cities	454	454	454	366	366	366	455	455	455
Outcome Mean	7.065	7.100	0.035	6.650	7.097	0.447	6.911	7.438	0.527
Outcome Stdv	.137	.115	.114	.181	.12	.127	.157	.101	.112
City FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Gap is defined as the difference between the more and less privileged group (e.g. native - immigrant, white - black, and business - non-business). Regressions estimated using all men age 19 to 50 living in cities from 1900 to 1940. Heteroskedasticity-robust standard errors clustered by city reported in parentheses. The mean and standard deviation of the weighted dependent variable are shown in the table..

Importantly, we can show that a failure to account for the possible endogenous timing in the adoption of the reform would result in significantly inflated estimates. A regression that does not employ entropy balancing weights results in an estimated effect on the earnings gap that is twice as large (estimate of 0.014, p-value 0.005). Indeed, as we show in Figure 3, Panel A, cities that reformed were already experiencing an increase in the immigrant-native earnings gap, relative to unreformed cities, in the decades leading up to the reform. When we apply our weighting procedure, which ensures that reform and never-reformed cities are on similar trends before the reform, we estimate a significantly smaller effect of the reform (see Panel B of Figure 3).

The remaining columns of Table 1 provide some evidence that the reform increased the earnings gap between more and less advantaged residents, but the economic magnitude of these effects is modest. The white-black earnings gap increased by 0.017 following the reform, with the effect mainly resulting from a 1.5 percent drop in earnings among black residents. The earnings gap between residents in business versus non-business occupations increased by 0.013, as a result of a 0.7 percent decrease in earnings for non-business residents and a 0.6 percent increase for business residents. Appendix Figure A.1 presents event-study estimates from equation 5 and shows evidence of immediate increases in the gaps in the first decade after the adoption of the new form of government. To put these effects in perspective, the average gap in log earnings between white and black in unreformed cities over the sample period is 0.414, and the one between business and non-business residents is 0.506; thus, the reform increased the gaps by 4.1 and by 2.6 percent, respectively, relative to the average unreformed city.

Figure 3: Event Study Estimates for the Native-Immigrant Earnings Gap



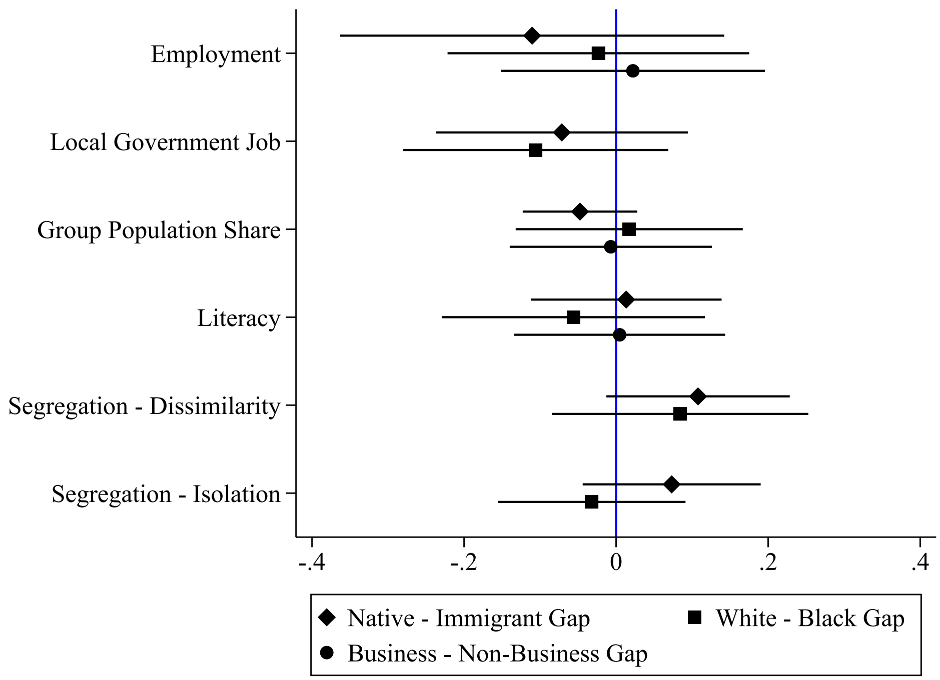
Notes: Shows coefficient estimates from the model described in equation 5 for the gap in earnings between native born and immigrants residents. The figure on the left uses raw data, and the figure on the right employs balancing weights as described in Hainmueller (2012). Dotted line shows the 95 percent confidence intervals.

In line with the small earnings effects uncovered in Table 1, we find small and mostly statistically insignificant effects on other measures of socioeconomic standing. Figure 4 reports the coefficients and 95 percent confidence intervals from estimating equation 4 for all our additional socioeconomic variables (where coefficients are expressed in standard deviation units of their respective dependent variable). For each outcome, we estimate the impact of the reform on the gap between more and less advantaged groups of residents.

If the adoption of reform-style government made it more difficult for machines to offer employment opportunities to disadvantaged residents, we would expect the effect of the reform on their probability of employment in general, and on their probability of having a public job more specifically, to be negative. As Trounstine (2006) observes, public sector jobs in the early 20th century often paid better wages than private employment. If instead less advantaged residents in cities that reformed were not particularly reliant on patronage for employment, or if reforms did not significantly reduce their likelihood of employment, we would observe no effect of the form of government on labor force participation. In line with this second possibility, we find no evidence that the reform widened employment gaps or gaps in the probability of holding a local government job. In addition, we find that the literacy gaps between groups were not affected by the adoption of reform.

We use our segregation measures to investigate whether cities that reformed had higher degrees of occupational segregation. Importantly, such clustering is not necessarily a negative thing for minorities. For instance, co-ethnic niches can capitalize on particular skill sets and human capital attributes to provide employment opportunities to members of the same communities (Waldinger and Bozorgmehr 1996). At the same time, existing empirical work finds little evidence that such occupational segregation benefits minorities in terms of their earnings and educational attainment (Wilson 1999; Model 2018). We do find some evidence that cities that reformed had higher degrees of occupational segregation, although the estimates are imprecise.[8]

Figure 4: The Impact of Reform on Other Socioeconomic Outcomes



Notes: Shows coefficient estimates and 95 percent confidence intervals from the model described in equation 4. See section 3.2 for a description of the dependent variables shown on the y axis. Results in table format are presented in Tables A.3, A.4, A.5.

Finally, we look at the overall share of each group in the population. We do this for two reasons. First, worsening economic conditions for a particular demographic group in the decades following the reform could lead to a decrease in that group's population share through a combination of increased mortality rates, lower birth rates, and negative net migration rates. Second, we can use this variable to investigate whether any changes to earnings are driven by changes in the relative size of the groups under study. If, for example, the reforms in a particular city led to a reduction in the white population, we might expect higher wages for this group because of lower competition for similar jobs. In line with the

[8]Note that our occupational segregation measure is constructed at the city level directly and it is equal to 1 by definition when the groups we look at are residents in business and non-business occupations, which is why we omit this comparison from Figure 4. See the previous section for details.

small to null distributional effects we uncover, we do not find significant changes in the shares of the city population belonging to any of the groups we study.

Despite the small distributional effects, the adoption of reform-style government may have had aggregate welfare effects, leading to differential economic growth relative to unreformed cities. While the absence of significant treatment effects for most of the outcomes and groups in Table 1 and Figure 4 already suggests this is not the case, we can provide direct evidence on the absence of significant aggregate welfare effects by re-estimating equation 4 on a sample at the city-census decade level. Table 2 shows that the adoption of reform-style government was not associated with differential changes to overall earnings, city population, or employment trajectories in the decades following the reform. Similarly, the share of employment in local government jobs and literacy rates did not change after the reform.

Table 2: The Impact of Reform on Aggregate Outcomes

	Log Predicted Earnings (1)	Log Total Population (2)	Employment (3)	Local Government Job (4)	Literacy (5)
Reform	-0.007	-0.007	-0.000	0.000	-0.001
	(0.004)	(0.046)	(0.006)	(0.000)	(0.003)
Num Obs	6,310	6,310	2,112	6,310	5,048
Num Cities	455	455	435	455	455
Outcome Mean	7.025	23.349	0.801	0.011	0.952
Outcome Stdv	.129	89.981	.074	.008	.043
City FEs	Yes	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes	Yes
Balancing Weights	Yes	Yes	Yes	Yes	Yes

Notes: Shows estimates of the aggregate effect of the reform. Heteroskedasticity-robust standard errors clustered by city reported in parentheses. The mean and standard deviation of the weighted dependent variable are shown in the table.

Overall, our empirical analysis paints a picture that is inconsistent with large distributional or aggregate effects of Progressive reforms. Our estimates show that, on average, the relative socioeconomic standing of less advantaged groups was either unaffected (for the case of immigrants) or only moderately worsened (for the case of African Americans and non-business residents) in the decades following the reform.

6 Political Participation Decreased in Reformed Cities

We next examine how the adoption of reform-style government affected political participation. Here, the theoretical predictions are more clear-cut, and existing empirical work tends to support the idea that the reforms of the Progressive era reduced democratic participation, consistent with them being partially designed to weaken popular participation in machine politics at the city-level (Banfield and Wilson 1966; Buenker 1973). Table 3 reports results from equation 4, estimated on a panel that includes all presidential and congressional elections from 1900 to 1940. While the treatment remains at the city-level, turnout data are not available below the level of the county. The dependent variable is thus turnout in the county where the city is located in each two or four year cycle (for congressional and presidential elections, respectively). Odd columns present unweighted coefficient estimates, while even columns present coefficient estimates from our preferred specification employing entropy balancing weights as described in section 4.[9] Standard errors are clustered at the city level and Appendix Tables A.6 shows robustness to clustering standard errors at the county level. Figure 5 presents event-study estimates from equation 5.

We find large negative effects for both congressional and presidential elections. When a city reforms, turnout in that city's county decreases by 2.045 percentage points in congressional elections and 2.242 percentage points in presidential elections. The event study estimates show that the drop in electoral participation is already visible in the first election post-reform, and it becomes larger over time. These results are in line with Fox (2012), which suggests that turnout decreased more quickly in the south and southwest than in the reform resistant north in the early 20th century. Today, turnout remains 6 to 8 percentage points higher in mayor-council cities compared to council-manager cities in California, according to estimates by Hajnal and Lewis (2003). It is worth noting that coefficients from the specifi-

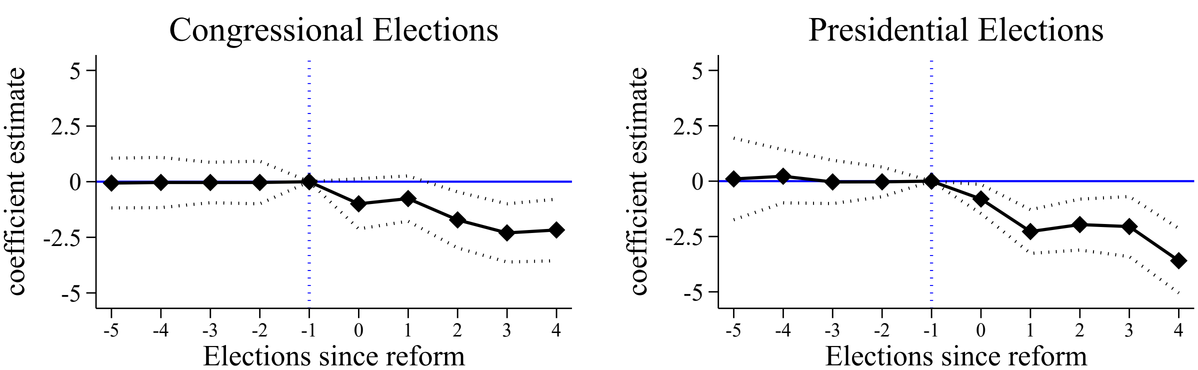
[9]Election results are available for a larger number of cities that the 455 included in the balanced sample of cities in our main analysis using census data and shown in Table 1. For consistency, Appendix Table A.7 shows robustness to restricting the electoral analysis to the sample of 455 cities present in census data. Note that 2 cities cannot be included because election results are available for one year only.

Table 3: The Impact of Reform on Voter Turnout

	Turnout Congressional Elections		Turnout Presidential Elections	
	(1)	(2)	(3)	(4)
Reform	-3.366	-2.045	-3.313	-2.242
	(0.584)	(0.588)	(0.595)	(0.635)
Num Obs	425,586	383,754	137,929	137,929
Num Cities	1342	1342	1649	1649
Outcome Mean	53.364	48.780	60.642	55.449
Outcome Stdv	17.617	18.932	18.356	20.409
City × Timing Group FEs	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes
Balancing Weights	No	Yes	No	Yes

Notes: Shows estimates of the effect of the reform on voter turnout. The dependent variable is turnout in the county where each city is located in each two year period (for Congressional elections) or four year period (for Presidential elections). Heteroskedasticity-robust standard errors clustered by city reported in parentheses. The mean and standard deviation of the unweighted dependent variable are shown in column (1) and column (3) of the table. The mean and standard deviation of the weighted dependent variable are shown in column (2) and column (4) of the table.

Figure 5: Event Study Estimates for Voter Turnout



Notes: Shows coefficient estimates from the model described in equation 5 for congressional and presidential turnout with balancing weights. Dotted line shows the 95 percent confidence intervals.

cation not accounting for endogenous timing of reform, in columns 1 and 3 of Table 3, are 65% and 48% larger than coefficients in columns 2 and 4 for Congressional and Presidential elections respectively. Our results provide some of the first historical evidence at the local level and using a before-and-after design that reform-style government decreased turnout—a widely assumed consequence of Progressive institutions (Martin 1933; Banfield and Wilson 1966; Judd and Hinze 2018). Moreover, by showing that the reform led to a significant shock to electoral participation, we can provide direct evidence that the mostly insignificant socio-economic effects that we uncovered in the previous section are not simply the result of a weakly specified treatment. The adoption of reform-style government mattered for the residents of American cities, but the consequences were more pronounced in the political rather than the economic sphere.

7 No Change in Public Expenditures After Reform

Finally, we examine whether and how the adoption of reform-style government influenced the allocation of spending on public goods. The theoretical predictions here are mixed. On the one hand, qualitative work suggests that the middle and upper-class supporters of the Progressive movement favored investment in amenities like parks, libraries, museums, and infrastructure improvements that would benefit downtown business districts (e.g. Hays 1964). But reformers also sought to expand access to education and social services (e.g. Buenker 1973). And while municipal reformers also claimed that their efforts would make city government more cost-effective and efficient (Schiesl 1977), existing empirical work shows that overall spending is no lower in council-manager cities (Lineberry and Fowler 1967; Ruhil 2003) and in fact is sometimes even higher (Coate and Knight 2011).

Here, we examine how spending across several key categories evolved in reformed vs. non-reformed cities.[10] Following Trounstine (2018) and Janas (2022), we draw from the Financial Statistics of Cities bulletins. Out of the 455 cities in our socioeconomic outcomes analysis, 136 cities also appear in the bulletins. For these cities, we digitized yearly information on aggregate municipal public spending, as well as on the amount spent on eight categories of

[10] Given the higher frequency of city budget data with respect to census and elections data (yearly *vs.* decennial and quadriennial/biennial respectively), and in order to be consistent with the weighting strategy used in the previous analysis and described in 4, we here use entropy balancing to find weights that minimize the difference between the average reform and never-reform cities only on the last three pre-reform years .

services: schools, fire, police, sanitation, public health, highways, recreation, and hospitals. This newly collected data allow us to paint a comprehensive portrait of how municipal budgets were affected by the adoption of Progressive reforms.

Interestingly, we find few differences in the evolution of public goods spending between cities that reformed and those that did not. Table 4 shows the results. In Column 1, we find a modest and statistically insignificant increase in total spending of around 2% among cities that reformed. These results point in the same direction but are substantively smaller than Coate and Knight (2011), who find that per capita spending increased by just under 8% when cities switched to council-manager government in the 1980s and 1990s. Although reformers of the Progressive Era frequently claimed that their proposals would cut costs and improve services (Bruere 1913; Taylor 1919), reformed cities did not actually reduce their overall expenditures in the early 20th century.

Table 4: The Impact of Reform on Public Expenditures

	Total (1)	School (2)	Police (3)	Highways (4)	Hospitals (5)	Fire (6)	Sanitation (7)	Recreation (8)	Health (9)
Reform	0.022	0.030	-0.010	0.013	-0.055	-0.000	0.026	0.055	-0.085
	(0.018)	(0.018)	(0.024)	(0.034)	(0.142)	(0.022)	(0.032)	(0.064)	(0.046)
Num Obs	5,106	8,435	8,268	8,436	5,437	8,268	8,268	7,930	8,268
Num Cities	121	136	136	136	122	136	136	135	136
Outcome Mean	5955.3	2000.9	534.2	475.7	572.7	409.3	383.4	160.7	125.6
Outcome Stdv	25459.6	9897.4	2877.6	1467.3	2705.4	1532.9	2108.6	651.9	512
City × Timing Group FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables are the natural log of spending in each budget category measured in thousands of dollars. The mean and standard deviation of the unlogged weighted dependent variable are shown in the table. Heteroskedasticity-robust standard errors clustered by city reported in parentheses.

Despite the fact that reform coalitions were widely perceived as supporting public goods like parks and libraries in the more affluent parts of their community, we uncover only a noisy (but positive) effect of the reform on spending on recreation. In general, across individual policy categories, we do not observe consistent patterns in the expenditure priorities of city governments after they reform. Spending did increase modestly on education—the largest expenditure category—but the estimates are not statistically distinguishable from zero.

The null results in this section offer some suggestive evidence as to why we fail to detect meaningful increases in the socioeconomic gaps between different groups of more and less advantaged residents in reformed cities. While the historical literature gives us reason to believe that the municipal Progressive agenda may have disproportionately catered to white and middle-class business elites, the actions of city leaders may not have translated into observable increases in inequality if spending on public goods remained fairly constant. Of course, we cannot observe how funding was allocated at the sub-city level, and case study evidence suggests that reform governments tended to neglect poorer neighborhoods (e.g. Judd and Hinze 2018; Beach et al. 2018). But the aggregate differences in the policy priorities and overall spending of reformed vs. non-reformed governments appear to be quite small overall.

8 Discussion

According to Judd and Hinze (2018), "The municipal reforms of the early nineteenth century were designed to undercut the electoral influence of working-class and immigrant voters" (77). What is less clear is whether these reforms reduced the economic power of these groups as well. In this paper, we study whether the adoption of reform-style government affected a variety of socioeconomic outcomes for immigrants, African Americans, and the working class compared to native-born, white, and business elites. Using de-anonymized census data to construct estimates of the wage earnings of city residents, we find that the earnings gap increased only marginally in reformed relative to non-reformed cities. We also find some modest evidence that occupational segregation increased, particularly for immigrants. Crucially, a naive difference-in-difference approach would have uncovered a much larger effect of reform on earnings inequality. After accounting for the non-random adoption of reform institutions with trajectory balancing, we find that much of the difference in earnings gaps can be explained by differences in the wage dynamics of cities that reformed vs. those that did not.

To show that these minimal effects are not simply the result of a weakly specified treatment, we use the same empirical setup to show that voter turnout did decrease in counties where more cities reformed. These results remain consistent even after employing trajectory balancing and are consistent with existing literature suggesting that the reforms of the Progressive era reduced political participation. While we cannot state with certainty whether this reduction in turnout disproportionately impacted working class and racial and ethnic minorities, one consistent explanation for this result is the fact that Progressive reformers tended to implement stricter voter registration and literacy requirements once they gained power.

Finally, we find no meaningful differences in public goods expenditures across reform and non-reformed cities. While we lacked clear ex ante theoretical predictions for these analyses, we include these results to paint the most comprehensive portrait possible about how municipal government evolved in cities before and after the adoption of Progressive institutions. We hope that these newly digitized data from the Financial Statistics of Cities Bulletins will be a resource for other scholars of historical political economy.

Together, these findings speak to important debates today about the contemporary legacy of Progressive-era reforms—particularly for poor and racial minorities. Many cities still rely on non-partisan ballots and at-large elections, and voter turnout remains much lower at the local level (Hajnal and Trounstine 2005). In some states, like California, a spate of recent lawsuits have aimed to overturn these electoral institutions with the goal of achieving more equitable representation for communities of color.[11]

Overall, this paper provides a comprehensive portrait of the economic, political, and public policy landscapes for a large sample of American cities in the early 20th century. Most scholarly work on the effects of Progressive institutions has focused specifically on their

[11] https://www.commoncause.org/california/resource/california-municipal-democracy-index/

consequences for participation and representation, and we have learned a great deal about the conditions under which local institutions are more or less responsive to the interests of racial minorities (Engstrom and McDonald 1981; Davidson and Korbel 1981; Karnig and Welch 1982; Hajnal and Trounstine 2007; Trebbi, Aghion, and Alesina 2008; Trounstine and Valdini 2008; Marschall, Ruhil, and Shah 2010; Abott and Magazinnik 2020). The findings in this paper suggest that the economic and social consequences of these reforms may also warrant additional scrutiny in the contemporary political environment. To the extent that the design of local political institutions exacerbates other types of socioeconomic inequalities, reforming these institutions becomes all the more urgent. On the other hand, if the consequences are largely confined to the political sphere, creating more equitable electoral institutions is no less urgent—but such efforts are likely insufficient to address broader socioeconomic disparities.

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Online Appendix

Intended for online publication only.

Contents

A.1 Data Appendix

Table A.1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Reformed City	0.409	0.492	0	1	455
Elections					
Turnout Congressional Elections	53.354	17.62	0.3	98.900	383754
Turnout Presidential Elections	60.642	18.356	0	99.2	137929
Census - Earnings					
Predicted Log Earnings (Immigrant)	7.057	0.132	6.5	7.509	6305
Predicted Log Earnings (Native)	7.124	0.105	6.607	7.468	6305
Predicted Log Earnings (Gap: Native - Immigrant)	0.067	0.11	-0.47	0.409	6305
Predicted Log Earnings (Black)	6.707	0.157	6.11	7.543	4845
Predicted Log Earnings (White)	7.127	0.103	6.642	7.468	4845
Predicted Log Earnings (Gap: White - Black)	0.42	0.12	-0.442	0.874	4845
Predicted Log Earnings (Non-Business)	6.944	0.139	6.377	7.291	6310
Predicted Log Earnings (Business)	7.453	0.087	7	7.675	6310
Predicted Log Earnings (Gap: Business - Non-Business)	0.509	0.11	0.2	0.924	6310
Log of Finance Spending					
Total	14.657	1.309	12.411	19.719	5106
School	13.565	1.287	11.155	18.812	8435
Police	12.249	1.406	9.384	17.861	8268
Highways	12.43	1.275	9.817	17.248	8436
Hospitals, Charities and Corrections	11.727	1.982	3.951	17.574	5437
Fire	12.328	1.197	9.832	17.03	8268
Sanitation	11.89	1.437	8.654	17.549	8268
Recreation	11.043	1.687	0	16.407	7930
Health Conservation	10.587	1.618	5.74	15.849	8268
Census - Other Outcomes					
Employment (Gap: Native - Immigrant)	-0.011	0.048	-0.225	0.295	2109
Employment (Gap: White - Black)	0.036	0.13	-0.37	0.883	1650
Employment (Gap: Business - Non-Business)	0.182	0.084	-0.237	0.532	2112
Local Government Job (Gap: Native - Immigrant)	0.002	0.01	-0.071	0.073	6305
Local Government Job (Gap: White - Black)	0.006	0.013	-0.149	0.083	4845
Group Population Share (Immigrants)	0.498	0.238	0.017	0.969	6305
Group Population Share (Black)	0.118	0.141	0.001	0.781	4845
Group Population Share (Non-Business)	0.93	0.02	0.823	0.987	6310
Segregation - Dissimilarity (Immigrant)	0.252	0.079	0.062	0.6	5044
Segregation - Dissimilarity (Black)	0.584	0.091	0.159	0.999	3876
Segregation - Isolation (Immigrant)	0.093	0.046	0.015	0.386	5044
Segregation - Isolation (Black)	0.26	0.119	0.007	0.653	3876
Literacy (Gap: Native - Immigrant)	0.055	0.057	-0.065	0.495	5044
Literacy (Gap: White - Black)	0.082	0.08	-0.021	0.639	3876
Literacy (Gap: Business - Non-Business)	0.039	0.041	-0.06	0.306	5048

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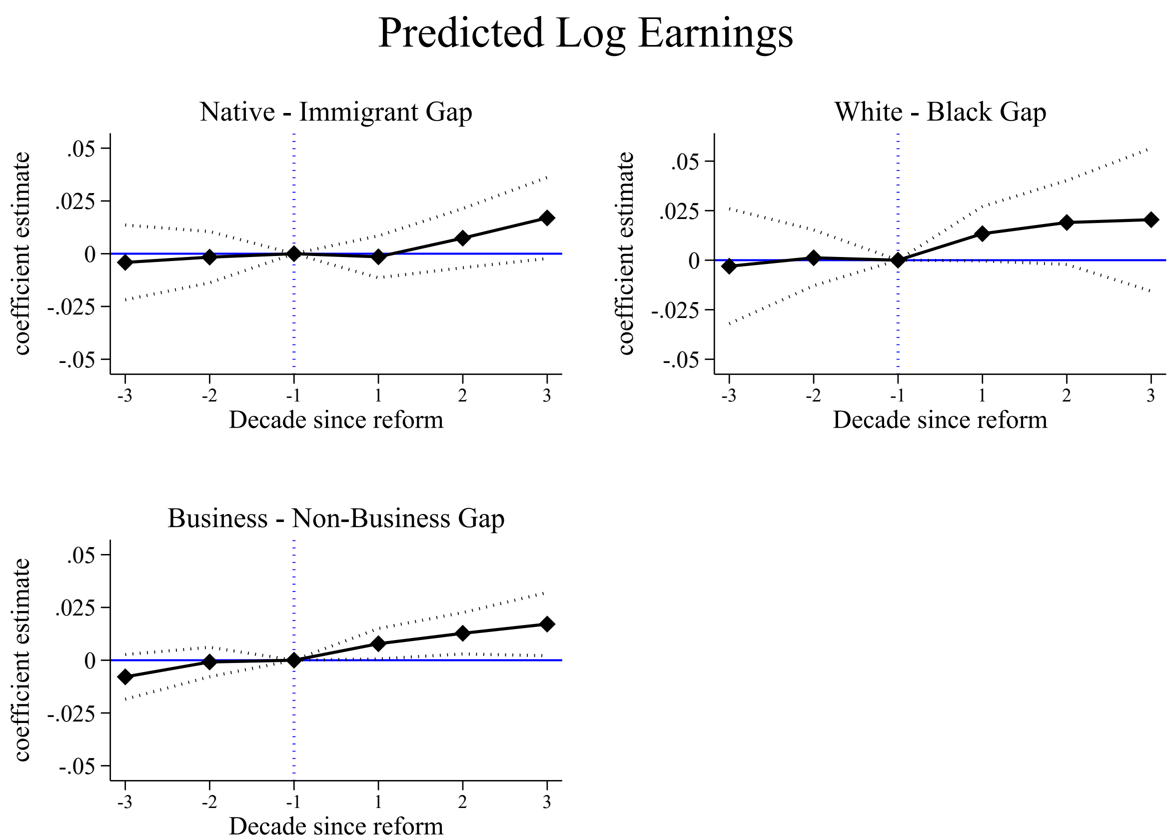
Table A.2: Description of IPUMS Variables Used in Analysis

IPUMS Variable	Description & Notes
LABFORCE	A dichotomous variable indicating whether a person participated in the labor force. See EMPSTAT for a non-dichotomous variable that indicates whether the respondent was part of the labor force – working or seeking work – and, if so, whether the person was currently unemployed. *Notes*: we combine this variable with CLASSWKR to identify if an individual is employed. Note that the variable EMPSTAT referenced in the IPUMS definition above is only available for the years 1910, 1930, and 1940. However, LABFORCE alone cannot distinguish between employed workers and unemployed individuals who are in the labor force but currently out of work. Our variable Employment takes a value of 1 if an individual is listed as being in the labor force AND has a current occupation listed for the CLASSWKR variable
CLASSWKR	Indicates whether respondents worked for their own enterprise(s) or for someone else as employees. *Notes*: in combination with LABFORCE, allows us to distinguish between employed and unemployed individuals in the labor force.
INCWAGE	Reports each respondent's total pre-tax wage and salary income - that is, money received as an employee - for the previous year. *Notes*: we use wages in 1940 to build a prediction model that allows us to impute wages to previous years based on an individuals' occupation, immigration status, age, and place of residence. Used to construct our variable Predicted Log Earnings
OCC1950	Applies the 1950 Census Bureau occupational classification system to occupational data, to enhance comparability across years. Note: used to predict Log Earnings
NATIVITY	Indicates whether respondents were native-born or foreign-born; for native-born respondents, it indicates whether their mothers and/or fathers were native-born or foreign-born. *Notes*: we define an individual as an immigrant if they are foreign-born or if either of their parents is foreign-born
RACE	Indicates whether respondents were white, African American, Native American, Chinese, Japanese, or classified as "other"

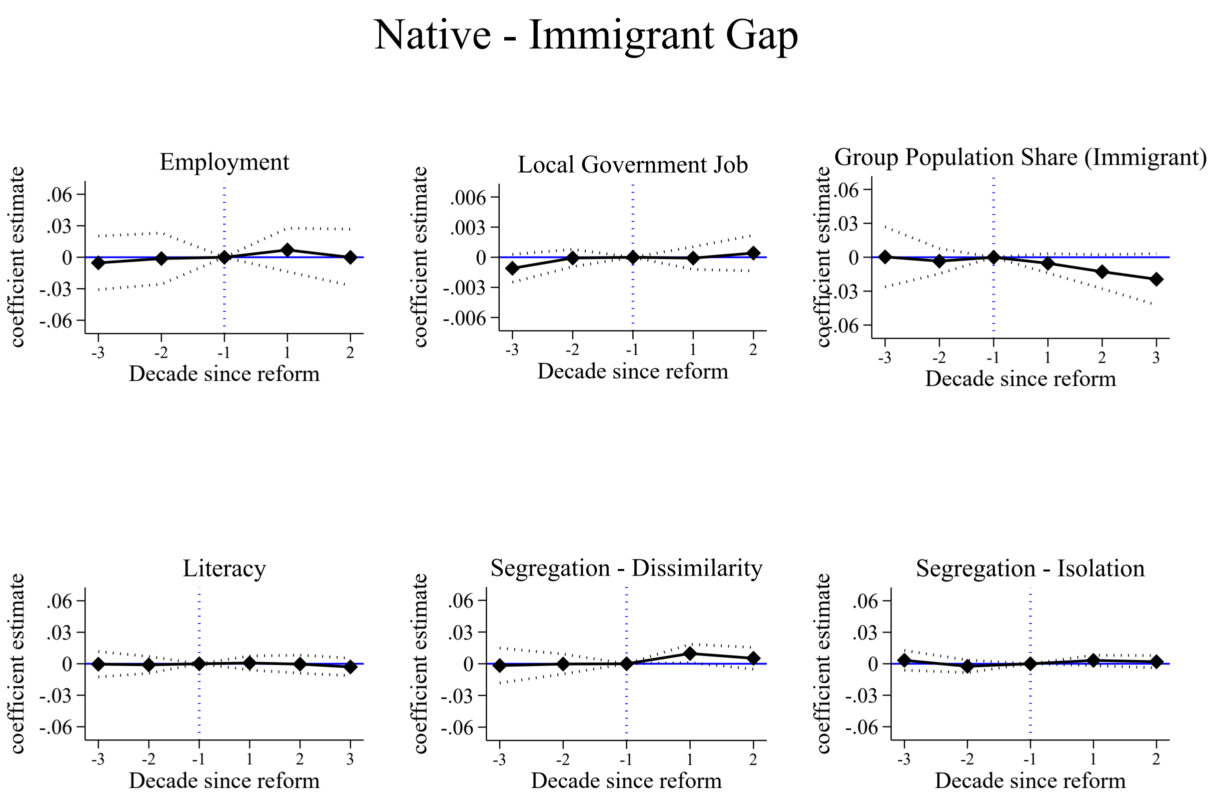
A.2 Additional Statistical Results

Figure A.1: The Impact of Reform on Earnings Gap



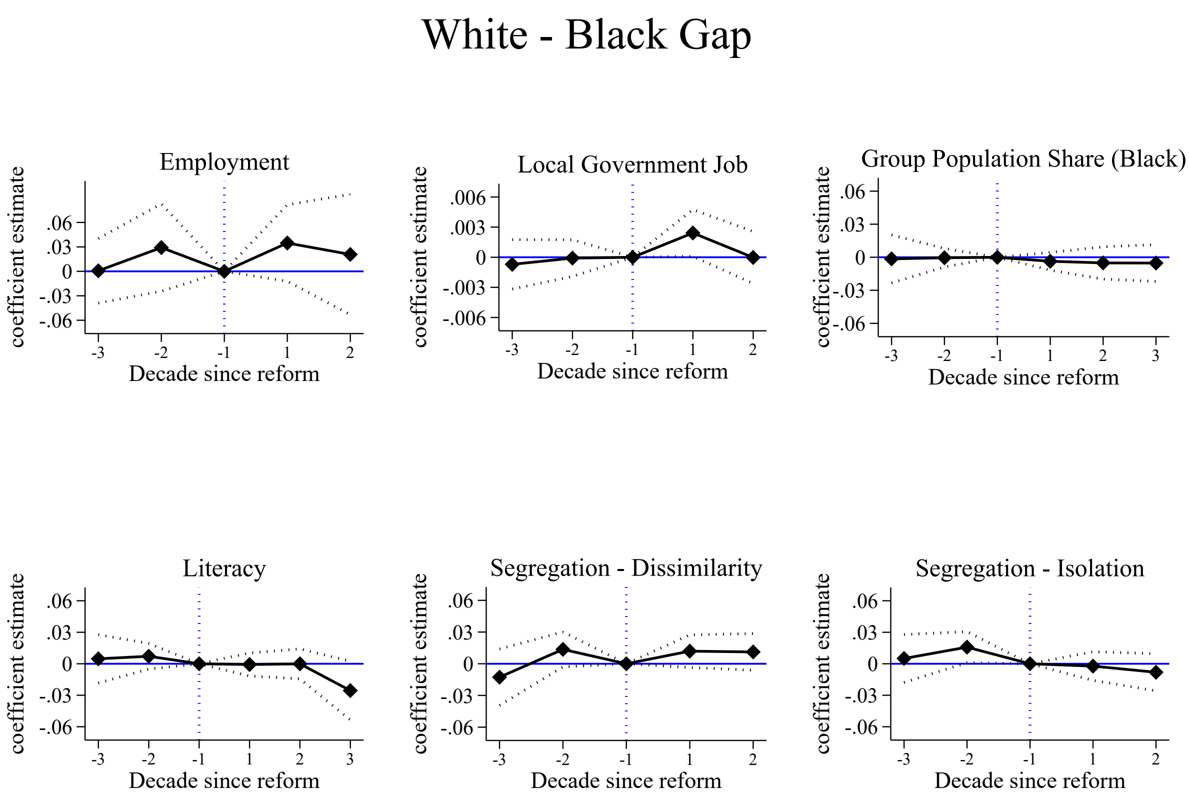
Notes: Shows coefficient estimates from the model described in equation 4 employing balancing weights for outcomes shown in Table 1. Dotted line shows the 95 percent confidence intervals.

Figure A.2: Event Study Estimates for the Native-Immigrant Gap in Other Socioeconomic Outcomes



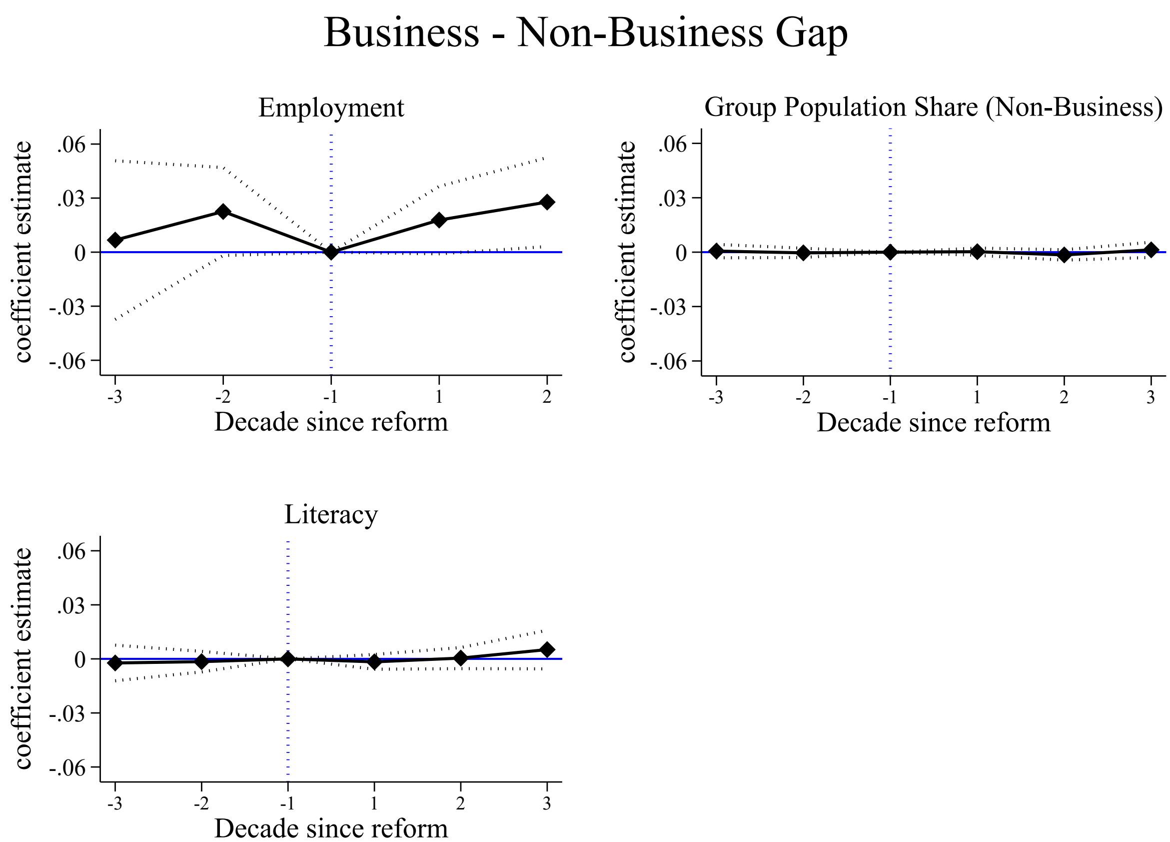
Notes: Shows coefficient estimates from the model described in equation 4 employing balancing weights for outcomes shown in Figure 4 for native and immigrant residents. Dotted line shows the 95 percent confidence intervals.

Figure A.3: Event Study Estimates for the White-Black Gap in Other Socioeconomic Outcomes



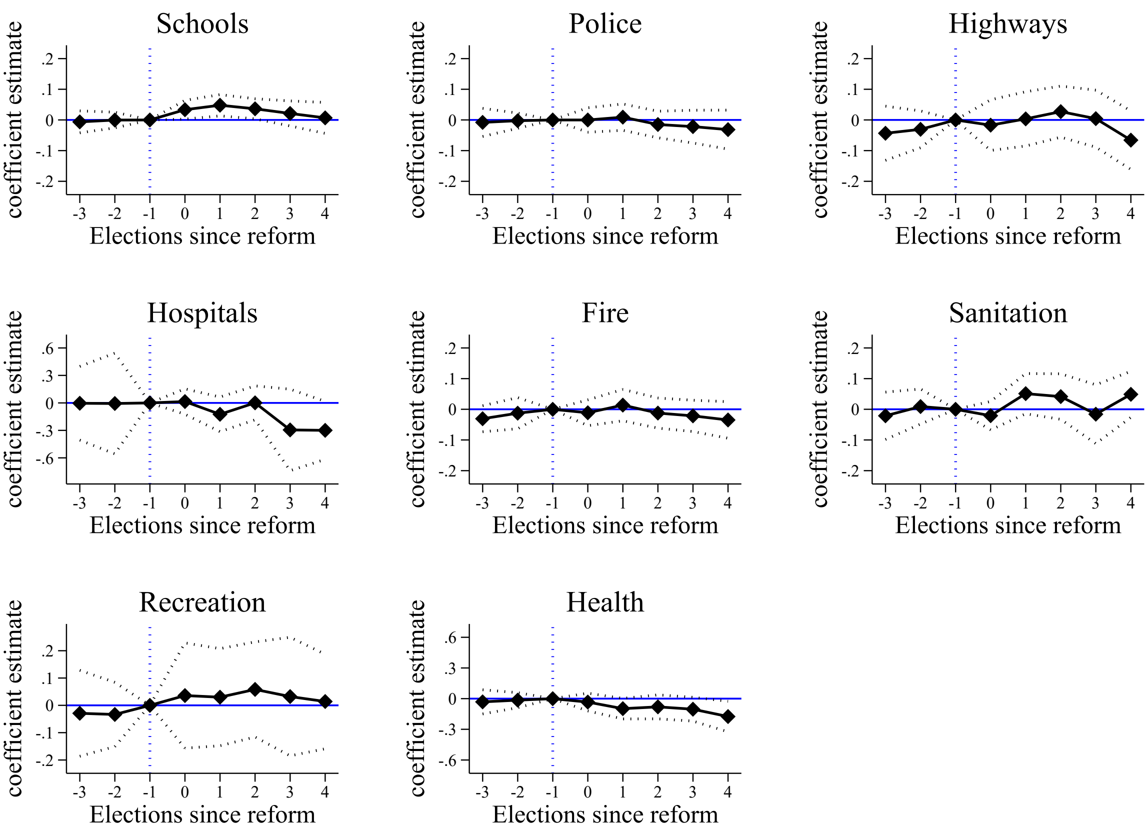
Notes: Shows coefficient estimates from the model described in equation 4 employing balancing weights for outcomes shown in Figure 4 for white and black residents. Dotted line shows the 95 percent confidence intervals.

Figure A.4: Event Study Estimates for the Business-Non-Business Gap in Other Socioeconomic Outcomes



Notes: Shows coefficient estimates from the model described in equation 4 employing balancing weights for outcomes shown in Figure 4 for residents in business and non-business occupations. Dotted line shows the 95 percent confidence intervals.

Figure A.5: Event Study Estimates for Public Expenditures



Notes: Shows coefficient estimates from the model described in equation 4 employing balancing weights for outcomes shown in Table 4. Dotted line shows the 95 percent confidence intervals.

Table A.3: The Impact of Reform on the Native-Immigrant Gap in Other Socioeconomic Outcomes

	Native - Immigrant Gap					
					Segregation	
	Employment	Local Government Job	Group Population Share	Literacy	Dissimilarity	Isolation
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	-0.005	-0.001	-0.011	0.001	0.008	0.003
	(0.006)	(0.001)	(0.009)	(0.003)	(0.004)	(0.002)
Num Obs	2,109	6,305	6,305	5,044	5,044	5,044
Num Cities	434	454	454	454	454	454
Outcome Mean	-0.009	0.002	0.401	0.043	0.235	0.084
Outcome Stdv	.047	.009	.232	.05	.072	.04
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes	Yes	Yes
Balancing Weights	Yes	Yes	Yes	Yes	Yes	Yes

Notes: the table above reproduces estimates shown in figure 4 for native and immigrant residents. The two indices of segregation do not refer to the gap because they are city-wide measures. The group population share refers to the share of immigrant men. The mean and standard deviation of the weighted dependent variable are shown in the table.

Table A.4: The Impact of Reform on the White-Black Gap in Other Socioeconomic Outcomes

	White - Black Gap					
					Segregation	
	Employment	Local Government Job	Group Population Share	Literacy	Dissimilarity	Isolation
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	-0.003	-0.001	0.000	-0.005	0.007	-0.004
	(0.013)	(0.001)	(0.001)	(0.007)	(0.007)	(0.008)
Num Obs	1,650	4,845	4,845	3,876	3,876	3,876
Num Cities	349	366	366	366	366	366
Outcome Mean	0.036	0.006	0.076	0.087	0.581	0.286
Outcome Stdv	.13	.014	.019	.081	.086	.123
City FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes	Yes	Yes
Balancing Weights	Yes	Yes	Yes	Yes	Yes	Yes

Notes: the table above reproduces estimates shown in figure 4 for white and black residents. The two indices of segregation do not refer to the gap because they are city-wide measures. The group population share refers to the share of black men. The mean and standard deviation of the weighted dependent variable are shown in the table.

Table A.5: The Impact of Reform on the Business-Non-business Gap in Other Socioeconomic Outcomes

	Business - Non-Business Gap		
	Employment	Group Population Share	Literacy
	(1)	(2)	(3)
Reform	0.002	-0.011	0.000
	(0.007)	(0.009)	(0.003)
Num Obs	2,112	6,305	5,048
Num Cities	435	454	455
Outcome Mean	0.183	0.401	0.036
Outcome Stdv	.082	.232	.038
City FEs	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes
Balancing Weights	Yes	Yes	Yes

Notes: the table above reproduces estimates shown in figure 4 for residents in business and non-business occupations. The group population share refers to the share of non-business men. The mean and standard deviation of the weighted dependent variable are shown in the table.

Table A.6: The Impact of Reform on Voter Turnout – Robustness to County Clustering

	Congressional Elections Turnout		Presidential Elections Turnout	
	(1)	(2)	(3)	(4)
Reform	-3.366	-2.045	-3.313	-2.242
	(0.753)	(0.611)	(0.674)	(0.641)
Num Obs	425,586	383,754	137,929	137,929
Num Cities	1342	1342	1649	1649
Outcome Mean	53.364	48.780	60.642	55.449
Outcome Stdv	17.617	18.932	18.356	20.409
City × Timing Group FEs	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes
Balancing Weights	No	Yes	No	Yes

Notes: the table above reproduces estimates shown in Table 3 with standard errors clustered at the county level. See Table 3 for additional table notes.

Table A.7: The Impact of Reform on Voter Turnout – Same Sample as Earnings Results

	Turnout Congressional Elections		Turnout Presidential Elections	
	(1)	(2)	(3)	(4)
Reform	-4.350	-1.946	-4.887	-3.145
	(0.922)	(0.958)	(1.006)	(0.948)
Num Obs	122,139	58,674	33,965	28,403
Num Cities	453	402	453	447
Outcome Mean	53.444	49.059	60.816	56.543
Outcome Stdv	18.322	18.8	18.213	19.984
City × Timing Group FEs	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes
Balancing Weights	No	Yes	No	Yes

Notes: Reproduces results shown in Table 3, restricting the sample to the cities also appearing in Table 1. The mean and standard deviation of the unweighted dependent variable are shown in column (1) and column (3) of the table above. The mean and standard deviation of the weighted dependent variable are shown in column (2) and (4) of the table above.