

When progressives took power: the limited economic effects of municipal reform in US cities

Maria Carreri

*Department of Social and Political Sciences, Bocconi University,
Milano, Italy, and*

Centre for Economic Policy Research (CEPR), Paris, France, and

Julia Payson and Daniel M. Thompson

*Department of Political Science, University of California, Los Angeles,
California, USA*

Received 12 January 2026
Revised 25 February 2026
Accepted 9 March 2026

Abstract

How did the adoption of reform-style government affect socioeconomic inequality in US cities during the Progressive Era? The conventional wisdom describes the reforms of this time as having a large impact on inequality, benefiting elites and white business owners at the expense of less well-off city residents. However, more recent political science work on this period offers reasons to expect more nuanced redistributive effects. We study the impact of reform leveraging deanonymized census records, newly digitized municipal budgets and reform introduction dates across 455 US cities during 1900–1940. Using a doubly-robust difference-in-differences design, we document the impact of Progressive municipal government on the relative socioeconomic well-being of black, immigrant and working-class residents compared to whites, natives and business elites. We find that inequality increased only modestly in reformed cities, with no significant differences in public spending. The results challenge the dominant narrative that progressive reforms significantly widened socioeconomic inequality.

Keywords Municipal reform, Progressive era, Socioeconomic inequality, Urban politics, Council-manager government

Paper type Research paper

1. Introduction

How did the municipal reform movement of the Progressive era affect the social and economic lives of urban residents? This question is at the center of a long literature in urban politics that emphasizes the impact of the Progressive agenda on less advantaged communities, including immigrants, African Americans and the less affluent.

In the 1960s, historians tended to portray urban reforms as an attempt by white business elites to seize power from less affluent residents of US cities, including the largely immigrant working class (Buenker, 1973; Hays, 1964; Weinstein, 1969). Although reformers claimed they wanted to improve city governance, scholars concluded that racist and nativist streaks permeated the movement, which was comprised primarily of white, Protestant and highly educated middle- and upper-class Americans. According to this research tradition, the main reforms of this era – commission and council-manager forms of government, at-large elections and nonpartisan ballots – were not primarily designed to decrease corruption and improve service provision as reformers alleged [1]. Instead, reformers sought to shape policies to their own advantage, by placing policy in the hands of elite-backed “experts” and by making it difficult for poor, immigrant and minority voters to cast ballots and achieve



representation on city councils, with negative socioeconomic consequences for these less well-off groups of voters. Hays (1964) encapsulates this perspective, asserting that “the movement for reform in municipal government constituted an attempt by upper-class, advanced professional and large business groups to take formal political power from the previously dominant lower- and middle-class elements so that they might advance their own conceptions of desirable public policy” (p. 162).

The conventional wisdom on progressive municipal reforms, still prominent in urban politics textbooks (Liazos, 2020), derives from this early scholarship. However, more recent political science research offers theoretical and empirical reasons to question this view. In particular, models comparing elected and appointed officials (e.g. Alesina and Tabellini, 2007) suggest that reforms would only systematically harm lower-income residents if bureaucrats were captured by elites and unresponsive to broader electoral preferences. Reforms, however, often failed to produce meaningful shifts in political power, frequently preserving elite dominance through informal coalitions rather than disrupting it (Bridges, 1999; Rice, 1977), with political monopolies, rather than reform institutions, being the main drivers of unaccountable governance and inequitable outcomes (Trounstine, 2009). Together, these insights indicate that the effects of reform were conditional and context-dependent and that the most critical accounts may overstate their negative distributive impact.

Despite the centrality of the urban Progressive movement for American political development, providing a comprehensive assessment of the effects of urban reforms for less advantaged groups has been challenging, given the lack of systematic data on US cities in the decades when these reforms were introduced (Holman *et al.*, 2025). While rich case studies offer many valuable accounts of who benefited from Progressive policies across specific local contexts, they often focus on the experiences of particular communities in the largest US cities, which are not necessarily representative of municipalities more broadly in the first half of the 20th century (Kuipers and Sahn, 2022) [2].

We introduce new data to study how the urban reform movement affected the lives of more and less advantaged city residents. We draw from: (1) de-anonymized census records at the individual level; (3) newly digitized city financial statistics; (2) dates of municipal reform, which we hand collected from primary sources; and (4) estimates of electoral participation at the county level.

Collectively, our data covers the universe of 455 cities observed consistently during 1900–1940 in the US census. 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, changes to the structure of government have been one of the longest-lasting Progressive legacies (e.g. Bernard and Rice, 1975; Holli, 1969). This change was typically accompanied by the adoption of at-large elections and nonpartisan ballots (Banfield and Wilson, 1966; Bridges, 1999; Lee, 1960; Griffith, 1927) and serves as an effective proxy for when Progressives gained control of the city government and implemented the standard package of institutional reforms advocated by reformers across the country. We further discuss the theoretical and empirical rationale for focusing on this particular reform in section 2, and we validate it as an effective proxy of reform-style governance with both quantitative and qualitative evidence in section 7.

We study how socioeconomic inequality evolved in reformed versus nonreformed cities using a modern doubly-robust generalized difference-in-difference method that accounts for potential heterogeneous effects under staggered treatment (e.g. De Chaisemartin and d’Haultfoeuille, 2020; Goodman-Bacon, 2021) and for potential violations of the parallel trends assumption (Chiu *et al.*, 2023; Xu, 2017). Importantly, because the timing of reform is not random, this method uses trajectory balancing (Hazlett and Xu, 2018), which ensures that

outcomes in reform and nonreform cities follow similar trends in the prereform period. We study the impact of reform on the most comprehensive set of socioeconomic outcomes available in the Census. Because we are interested in the unequal impact of reform, we study whether reform differentially affected more affluent city residents with respect to less affluent ones. Specifically, for each measure of socioeconomic status, we compare immigrant residents with native residents, working-class residents with business elites, and black residents with white residents.

We show that the adoption of reform-style government led to at most very modest increases in socioeconomic inequality. We do not find any systematic difference between reformed and nonreformed cities in employment rates, probability of employment in local government jobs, literacy rates and labor market segregation. We show that the earnings gap between more and less advantaged residents increased in cities that adopted reform-style government, but the magnitude of the effect is small. The earnings of black and working-class residents decreased by 1.5 and 0.7%, respectively, while the earnings of business workers increased by 0.6% following the reform. The reform had no significant impact on the earnings of immigrant and native residents. These effects result in a statistically significant but relatively small widening of the white-black and business versus nonbusiness earnings gap. 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 use trajectory balancing would result in estimates that are twice as large as the true treatment effect on the native-immigrant gap.

One of the main mechanisms through which reform might affect socioeconomic inequality is changes in the allocation of government resources. We digitized detailed annual budget data and show that cities that reformed did not decrease total public spending or change the allocation of government funds across different types of public goods. These results are in line with the lack of large consequences for socioeconomic inequality and suggest that government priorities did not change significantly as a result of reform.

One possible concern is that the adoption of council-manager or commission government (what we call *reform-style government*) is an imperfect proxy for the standard package of institutional reforms of progressive municipal reformers, which often also included nonpartisan and at-large elections. If this concern is valid, noise in treatment measurement might bias our coefficient estimates downward, explaining our largely null results [3]. In section 8, we present an important test to assuage this concern. We show that the switch in form of government was accompanied by a significant decrease in turnout in presidential and congressional elections. This result is consistent with the consensus that one of the consequences of municipal reform was decreased political participation across the board for all voters, as the goal of the movement was to explicitly place city government in the hands of technocrats and limit popular participation in politics (Fox, 2012; Hajnal and Lewis, 2003; Holli, 1969; Martin, 1933; Schiesl, 1977; Schaffner *et al.*, 2001). This gives us confidence that the limited economic effects we observe are not the result of an underpowered design.

In conclusion, our findings challenge the conventional wisdom by suggesting that the impact of Progressive Era municipal reforms on urban socioeconomic inequality was, on average, negative but modest in magnitude. We document a more limited effect than that portrayed in earlier historical and qualitative accounts, contributing to a growing body of revisionist scholarship that reexamines the consequences of Progressive institutional reforms. In particular, our results complement recent work reassessing the effects of civil service reform, the other cornerstone of the Progressive agenda. Anzia and Trounstein (2025) argue that municipal employees actively supported, rather than resisted, civil service reform,

while [Kuipers and Sahn \(2022\)](#) find no evidence that such reforms decreased the share of local government jobs held by immigrants.

Our paper contributes to a number of studies that have examined *reform-style government* and evaluated its effects. Many of these works report mixed or context-dependent results. For instance, [Morgan and Pelissero \(1980\)](#) and [Ruhil \(2003\)](#) analyze changes in the form of municipal government over a similar historical period as our study and finds no significant impact on city expenditures, echoing [Amy Bridges's \(1999\)](#) case studies of Southwestern cities, where reform governments spent only “somewhat less than machine descendants” ([Bridges, 1999](#), p. 160). Our paper also contributes to a broader literature investigating specific elements of the reform agenda, particularly at-large and nonpartisan elections ([Abott and Magazinnik, 2020](#); [Adrian, 1952](#); [Davidson and Korbel, 1981](#); [John et al., 2018](#); [Marshall et al., 2010](#); [Ricca and Trebbi, 2022](#); [Sass and Pittman, 2000](#); [Welch, 1990](#)). This line of research explores how these institutions may diminish the political influence of racial minorities and economically disadvantaged citizens, though the findings are often mixed or conditional on local context. For instance, ([Welch and Bledsoe, 1988](#))’s pioneering research on nonpartisan and at-large elections finds that these institutions did not influence policy priorities of municipal officeholders. [Trounstone and Valdin \(2008\)](#) show that at-large elections significantly affect the representation of certain minority groups – specifically African American men and white women – but not Latinos or African American women.

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 explosive urbanization of the late 19th and early 20th century marked a period of increasing administrative challenges as city governments struggled to provide urban residents with adequate services ([Glaab and Brown, 1967](#); [Holman et al., 2025](#)). 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 ([Trounstone, 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 urban living conditions and eliminate the graft and corruption of machine-style politics ([Buenker, 1973](#); [Renner and DeSantis, 1993](#)). Part of the broader Progressive movement in American politics, municipal reformers sought to reorganize city administration. The reforms of this era centered around the introduction of the council-manager form of government, the adoption of at-large elections, and nonpartisan ballots, and reformers often claimed that their agenda was designed to broadly improve city governance and provide better services for all residents ([Bridges and Kronick, 1999](#); [Burnham, 2001](#); [Cantor, 2019](#); [Santucci, 2022](#)).

However, the first studies to critically consider the motivations of urban reformers argued that the movement was a “near conspiracy” ([Liazos, 2020](#), p.5). A body of work developed by historians in the 1960s highlighted that business interests and upper-middle class whites featured prominently in the ranks of urban reformers and suggested a sharp divide between the stated goals of the reform movement and its true objective ([Buenker, 1973](#); [Hays, 1964](#); [Weinstein, 1969](#)). According to this research tradition, the real motivation of adopting reform-style government institutions was to place policy in the hands of elite-backed “experts” while making it more difficult for poor, immigrant, and minority voters to cast

ballots and achieve representation on city councils. For example, Banfield and Wilson wrote of the council-manager form of government: “Making local government ‘businesslike’ meant ‘getting rid of politics’, which in turn meant curtailing the representation of low-status minorities. In its early years, the [council-manager] plan appealed to a good many people as a convenient means of putting the Catholics, the Irish, the Italians, the labor unions and all ‘underdogs’ in their places” (Banfield and Wilson, 1966, p. 171).

This body of research argues that reform governments, by reducing the political representation of working-class, immigrant, and minority voters, implemented policies that systematically advantaged white upper-class and business elites at the expense of less affluent groups. Hays (1964) encapsulates this perspective, asserting that “the movement for reform in municipal government constituted an attempt by upper-class, advanced professional and large business groups to take formal political power from the previously dominant lower- and middle-class elements so that they might advance their own conceptions of desirable public policy” (p. 162). How, then, did public policy shift under reform governments to achieve this end? This research tradition identifies at least three dimensions of policy change detrimental to marginalized communities.

First, reformers reallocated services and benefits to favor higher-income residents. As Rice (1977) argues, this reflected “capitalistic and business-oriented agendas” in which reformers “had little concern for promoting public welfare or ensuring equal opportunities for all citizens” (pp. 5, 40). Reform governments often neglected lower-income neighborhoods in favor of wealthier districts, prioritizing expenditures that served downtown businesses and corporations (Judd and Hinze, 2018, p. 75). Urban renewal projects, for instance, invested heavily in expressways while displacing low-income and minority residents to facilitate commercial development and attract affluent shoppers downtown (Banfield and Wilson, 1966, pp. 261–262). Consequently, many working-class and minority neighborhoods became underserved, lacking basic infrastructure such as paved roads and sewage systems (Pitti, 2018, p. 90).

Second, reformers restructured labor and employment policies, with disproportionately negative effects on working-class and minority residents. To reduce costs, many cities downsized their municipal workforces and adopted more precarious forms of employment (Weinstein, 1969, p. 111). In San Diego, for example, African Americans faced significant barriers to securing municipal employment (Bridges, 1999, p. 169).

Third, according to Martin (1990), minority-owned businesses were often disadvantaged when city services were privatized. The requirement for performance bonds discouraged newer firms – particularly minority-owned businesses without established reputations – from competing successfully for municipal contracts (p. 179).

2.1 *Challenging the conventional wisdom*

This early scholarship remains influential and continues to shape contemporary understandings of progressive municipal reforms, particularly in standard textbooks (Liazos, 2020) [4]. However, closer theoretical scrutiny suggests that the impact of reform would be substantial only under specific conditions – conditions that were likely uncommon across most reform cities in the US between 1900 and 1940. In line with this, a growing body of political science research presents a more nuanced account of the aims and consequences of the early twentieth-century urban progressive movement. These findings call into question the dominant narrative, suggesting that the most critical assessments of urban reform may overstate its uniformly negative effects.

The literature on elected versus appointed public officials offers a useful theoretical framework for evaluating the redistributive implications of adopting council-manager or

commission forms of municipal government. [Alesina and Tabellini \(2007\)](#) analyze the comparative performance of elected politicians and appointed bureaucrats in executing redistributive policies. In their model, elected officials – motivated by electoral incentives – seek to appeal to a numerical majority of voters, typically through redistribution that reflects the preferences of that majority. In contrast, appointed bureaucrats are primarily concerned with maintaining a reputation for competence among their professional peers, which enhances future career prospects. As a result, bureaucratic resource allocation may be more technocratic but also potentially arbitrary or misaligned with the preferences of the broader electorate. Importantly, any group that constitutes a meaningful share of the electorate – regardless of socioeconomic status – can expect more favorable treatment under an elected official than under an unaccountable bureaucrat. Consequently, we should only expect systematically negative distributive outcomes for less advantaged city residents under bureaucratic governance if the appointed official is effectively captured by a coalition that excludes these groups from influence.

However, research in political science offers several reasons to question this assumption – and, more broadly, to be skeptical of the conventional view that Progressive Era urban reforms uniformly exacerbated political and socioeconomic inequality. First, this research highlights that local reform coalitions were often ideologically and socially heterogeneous. Work such as [Bridges \(1999\)](#), [Liazos \(2020\)](#) and [Rice \(1977\)](#) describe how white business elites often pushed for reforms alongside labor unions, socialists, women and African American leaders, who wanted better working conditions and expanded social services. As [Bridges \(1999, p. 30\)](#) characterizes it, these coalitions embodied a “mixed assortment of motivations and goals, of efficiency and elitism, clean government and racism, the common good and exclusion.”

Second, reform did not necessarily entail a substantive shift in who held political power or how local government operated. In many cases, reform served as a rhetorical vehicle for existing elites to maintain their positions of influence. [Bridges \(1999\)](#) emphasizes that in many southwestern cities during the early twentieth century, reform governance often took the form of unstable and informal coalitions of “factions and personalities governing with little consistency.” These arrangements did not necessarily reduce opportunities for corruption, collusion between officials and real estate interests, or clientelism toward disadvantaged constituencies ([Bridges, 1999, p. 73](#)).

Third, [Trounstine \(2009\)](#) argues that the primary threat to democratic accountability and equitable service provision during this period stemmed not from reform *per se*, but from the emergence of political monopolies. These monopolies – defined as governing coalitions that faced limited electoral competition – undermined responsiveness and allowed entrenched interests to dominate policymaking. Crucially, such monopolies arose under both machine and reform governments, suggesting that institutional form alone does not explain variations in distributive outcomes.

2.2 *Measuring reform-style government*

The net effect of reform on the economic and social lives of more disadvantaged city residents remains an open empirical question. We shed light on this question by analyzing socioeconomic outcomes for various groups of residents around the time when cities switched 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 the broader package of urban reforms, which typically also included nonpartisan and at-large elections. 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, this reform 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 make it difficult for local politicians to engage in patronage (Judd and Hinze, 2018).

Existing historical work suggests that changing the form of government itself was the most extreme example of reform (e.g. Bernard and Rice, 1975; Holli, 1969), and the vast majority of council-manager systems also concurrently adopted other Progressive reforms (Banfield and Wilson, 1966). In 1914, Griffith (1927) reported that the nonpartisan ballot was "incorporated in the majority of new charters" of US cities (Griffith, 1927, p. 271). A report from 1929 discusses how "the commission plan is of such a nature that election at large is most practical" and indeed "only 5% of the cities elect their commissioners from wards or districts" (Detroit Bureau of Governmental Research, 1931). Similarly, Lee (1960) found that 81% of US cities with a commission or council-manager form of government had nonpartisan elections in 1929 (p. 25) and that 83% of at-large cities were nonpartisan by 1959 (Lee, 1960, p. 25, 27, 28).

Case studies consistently show that reform-style government was typically adopted following the first election of a progressive local administration. In Cincinnati, for example, the Republican machine led by Boss George Cox dominated for decades until reform-minded Democrats, Independents and Republicans formed the City Charter Committee in 1924. The 1925 municipal charter introduced a council-manager system, at-large elections and nonpartisan ballots. In 1926, six "Charterites" were elected to the city council, along with the first Democratic mayor in 40 years (Burnham, 1997). In Spokane, Washington, Progressive reformer Charles Marvin Fassett championed the commission-style government, became a commissioner in 1911 after the new charter's adoption, and was elected mayor in 1914 (Rice, 1977). In Wichita, Kansas, Progressive residents petitioned for a 1917 election to adopt the council-manager form, subsequently defeating the incumbent mayor and electing five reformers to the city commission (Stillman, 1974).

After presenting the main results, section 7 offers case studies and quantitative evidence supporting the idea that adoption of the council-manager system is a valid proxy for the classic package of local progressive reforms. The next section introduces our data sources and research design.

3. Data collection and variable definition

In this paper, we measure the adoption of reform-style government with the introduction of the council-manager or city commissioner forms 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. Drawing from the Municipal Yearbooks of 1934 and 1940 and archival records available in Rice (1977), we collected data on the year of adoption for the 1,100 largest cities in the USA.

Data on socioeconomic 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. After 72 years, the Census Bureau releases data at the individual level, which allows us to track a variety of socioeconomic outcomes for different groups of urban residents. Such comparisons are not possible with the more commonly used Census data aggregated at the location level, which does not allow researchers to construct socioeconomic variables that vary both at the city- and at the group-level.

We collected data on the date of adoption of reform-style government for the universe of cities that consistently appear in the US census during the 1900–1940 period. In total, 455 cities appeared in the census for all five decades – the universe for which we have a balanced sample – and 186 of them reformed during our period of study. Figure 1 shows the number of cities that adopted reform-style government in every year between 1900 and 1940. While the majority of reforms took place between 1910 and 1920, cities continued to change their form of government over the course of the sample. Online Appendix Table A.1 presents summary statistics for the reform indicator and for all other variables in the analysis.

Figure 2 depicts the geographic distribution of cities that reformed at some point during the 40-year period and cities that never reformed. Nonreformed 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 effects of reform on public spending, we digitized information on city financial spending from the yearly Financial Statistics of Cities Bulletins provided by the US 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 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.

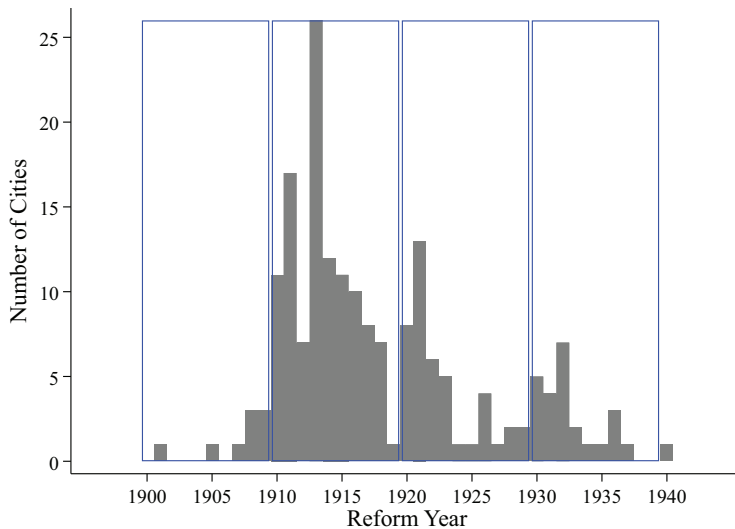


Figure 1. Number of reforms by year

Note(s): The plot shows the year of adoption for each of the 186 reformed cities in our sample of 455 cities in the USA between 1900 and 1940. Blue lines highlight census decades

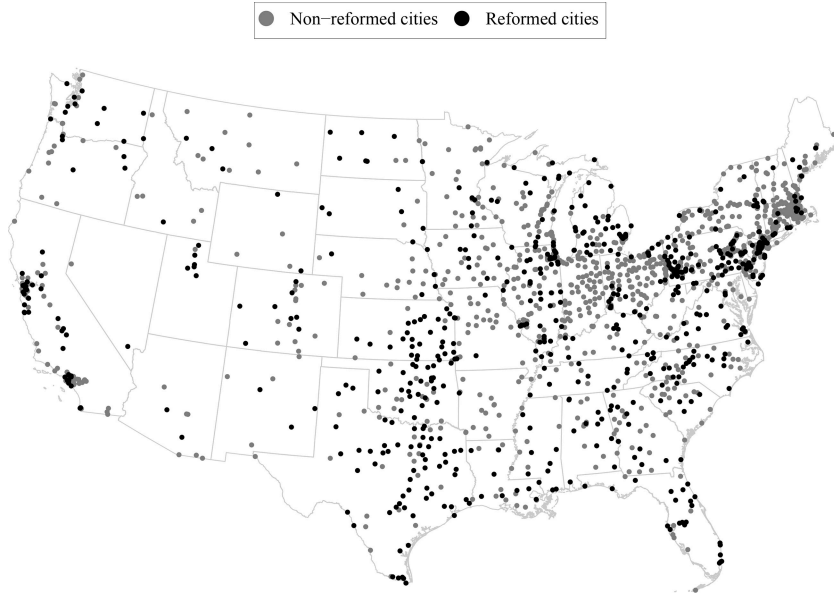


Figure 2. Geographic distribution of reformed cities

3.1 Variable definition

To assess how different groups fared in reformed vs nonreformed 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 US [5]. Second, we divide residents between African American and nonimmigrant 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 nonbusiness professions. Specifically, we follow existing literature (e.g. [Buchmann and McDaniel, 2016](#)) and define “business” to include occupations classified under the Managers, Officials and Proprietors category according to IPUMS [6].

To explore the socioeconomic impact of reform, we look at the most comprehensive set of socioeconomic outcomes that can be computed in the Census. We look at residents’ wages, whether they are employed, whether they have a job in the local government, and whether they are literate. Wage earnings are the first of a set of measures 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 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 [7]. 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. *Employment* is the share of each group that is employed. *Local Government Job* is

the share of each group that holds a job in “local government” as defined by industry classification in the census. *Literacy* is the share of each group that could read and write.

To explore possible mechanisms for the socioeconomic impact of reform, we look at two additional Census outcomes. *Group Population Share* is the share of each group among the residents of a city. *Occupational segregation* indicates the degree to which workers belonging to different groups are clustered in different occupations [8].

We compute all our measures both at the city-decade-group level – to study the impact of reform on specific demographic groups – and at the city-decade level, to study the aggregate impact of reform on the socioeconomic evolution of a city [9]. In addition, 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 nonbusiness workers.

4. Empirical approach

Our goal is to study the effect of the Progressive movement across US cities at the turn of the 20th century. Our identification strategy exploits the staggered introduction of reform-style government across cities (shown in Figure 1) to study the effect of progressive power on socioeconomic outcomes for different groups of residents and on city expenditures. Census outcomes are aggregated at the city-decade-group and are recorded every decade. 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:

$$y_{ct} = \gamma_c + \delta_t + \beta \text{Reformed}_{ct} + \varepsilon_{ct} \quad (1)$$

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 decade fixed effects are represented by γ_c and δ_t , respectively [10]. 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 nonbusiness 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 two-way fixed effects regressions, like in equation (1), are biased when the treatment goes into effect at different times for different units if treatment effects change over time (e.g. 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 (1900–1910, 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:

$$y_{cgt} = \gamma_{cg} + \delta_{ig} + \beta \text{Reformed}_{cgt} + \varepsilon_{cgt} \quad (2)$$

where g identifies the timing group, δ_{ig} represents period-by-timing group fixed effects, and γ_{cg} represents city-by-timing group fixed effects [11]. Standard errors are clustered at the city level. We can interpret β as the effect of reform under the assumption that reform and nonreform cities would have been on the same average trajectory had neither reformed.

Naturally, the timing of reform is not exogenous: cities may adopt a city manager or commission government in response to evolving socioeconomic conditions (Tebbi *et al.*, 2008). Table 1 indicates that reform cities systematically differ from nonreform cities in both baseline characteristics and pretreatment values of all outcome variables. For example, reform cities exhibited a lower proportion of immigrant residents in 1900, aligning with Banfield and Wilson (1966)’s portrayal of such cities as more ethnically homogeneous and less influenced by what they term the “immigrant ethos” (p. 46). Measures of inequality also vary meaningfully between the two groups. Specifically, reform cities exhibit lower pretreatment earnings inequality between immigrant and nonimmigrant residents but higher inequality between white and Black residents, relative to nonreform cities. While balance in levels across treated and control units is not a requirement for the validity of a difference-in-differences design, the observed pretreatment imbalances raise the possibility of underlying differences in time-varying confounders that may threaten identification. To address this issue, we use the trajectory balancing strategy proposed by Hazlett and Xu (2018). Intuitively, this approach ensures that the never-reformed cities match the average outcome for the reform cities in their timing group in each period before reform. Formally, we use entropy balancing to find weights that minimize the difference between the average reform

Table 1. City characteristics by selection into reform

Variable	Mean (1)	Coefficient (2)	Std. Error (3)
Immigrant share in 1990	0.538	-0.085	0.017
Predicted log earnings gap (Native – Immigrant)	0.077	-0.028	0.009
Employment gap (Native – Immigrant)	-0.016	0.005	0.004
Local government job gap (Native – Immigrant)	0.001	0.001	0.000
Literacy gap (Native – Immigrant)	0.053	-0.009	0.005
Black share in 1990	0.128	0.034	0.015
Predicted log earnings gap (White – Black)	0.428	0.022	0.010
Employment gap (White – Black)	-0.001	0.010	0.007
Local government job gap (White – Black)	0.003	0.000	0.001
Literacy gap (White – Black)	0.089	0.006	0.006
Nonbusiness share in 1990	0.898	-0.010	0.002
Predicted log earnings gap (Business – Nonbusiness)	0.535	0.017	0.007
Employment gap (Business – Nonbusiness)	0.220	-0.002	0.005
Literacy gap (Business – Nonbusiness)	0.048	-0.004	0.003

Note(s): The balance table above reports coefficients from separate regressions of the variable reported in the leftmost column on a treatment indicator and including decade fixed effects. In each decade, the sample includes all cities that reformed in that decade (for whom the treatment indicator takes value one) and all cities that never reform during 1900–1940 plus those that reform after that decade (for both of these groups the treatment indicator takes value zero). *Gap* is defined as the difference between the more and less privileged group (e.g. native – immigrant, white – black and business – nonbusiness). Column (1) reports the sample mean for each variable. Heteroskedasticity-robust standard errors clustered by city reported in parentheses

and never-reform cities on all prereform observations of the outcome while maintaining weights as close to 1 for all control units (Hainmueller, 2012; Hazlett and Xu, 2018; Imai *et al.*, 2018). The resulting method is a doubly robust generalized difference-in-differences strategy that is well-suited to cases with many treated units and few pretreatment periods, which is not the case with standard synthetic control methods for panel data.

To investigate pretrends and the dynamic evolution of the treatment effect, we also estimate a nonparametric event-study specification:

$$y_{cgt} = \gamma_{cg} + \delta_{ig} + \sum_{\tau=-3}^{+3} \beta_{\tau} \text{Reformed}_{cg} \times \mathbb{1}[t = \tau] + \varepsilon_{cgt} \quad (3)$$

where the coefficients of interest, β_{τ} , 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.

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. We investigate the impact of reform on four different measures of socioeconomic standing of groups of city residents: their earnings, whether they are employed, whether they have a local government job, and whether they are literate. Table 2 reports estimates from equation (2), which measures the impact of the reform on the evolution of each outcome for different groups of residents. Columns 1–3 focus on immigrants versus natives, columns 4–6 focus on blacks versus whites, while columns 7–9 focus on residents in nonbusiness versus business occupations.

Overall, we find that the reform had at most modest distributional effects. Panel A of Table 2 shows that reform led to a 1% 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). The remaining columns of Panel A of Table 2 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% drop in earnings among black residents. The earnings gap between residents in business versus nonbusiness occupations increased by 0.013, as a result of a 0.7% decrease in earnings for nonbusiness residents and a 0.6% increase for business residents. Online Appendix Figure A.1 presents event-study estimates from equation (3) and shows evidence of immediate increases in the earnings 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 nonbusiness residents is 0.506; thus, the reform increased the gaps by 4.1 and by 2.6%, respectively, relative to the average unreformed city [12].

Panel B of Table 2 does not reveal a significant effect of reform on employment gaps between more and less advantaged city residents. In Panel C, we also find no evidence that reform led to a relative decrease in the probability that less advantaged residents held a local government job, which often paid better wages than private employment (Trounstine, 2006). Notice that, while reform-style government was not associated with the adoption of civil service reform, we might still observe a lower probability of public employment for less advantaged residents if reformers were biased against these groups in employment decisions. The fact that we do not find such impact is instead inconsistent with this hypothesis. Interestingly, Kuipers and Sahn (2022) study the introduction of civil service reforms across

Table 2. The socioeconomic impact of reform across groups

Variable	Immigrant (1)	Native (2)	Gap (3)	Black (4)	White (5)	Gap (6)	Non -Business (7)	Business (8)	Gap (9)
<i>Panel A: predicted log earnings</i>									
Reform	-0.010 (0.006)	-0.002 (0.004)	0.007 (0.006)	-0.015 (0.008)	0.002 (0.005)	0.017 (0.008)	-0.007 (0.004)	0.006 (0.002)	0.013 (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	1182	1220	1,042	785.3	1217	1,576	1015	1707	1,704
Outcome stdv	156.6	137.4	0.116	140.7	143.2	0.195	150	166.9	0.197
<i>Panel B: employment</i>									
Reform	0.004 (0.008)	-0.001 (0.006)	-0.005 (0.006)	-0.001 (0.014)	-0.004 (0.006)	-0.003 (0.013)	-0.000 (0.005)	-0.002 (0.007)	0.002 (0.007)
Num obs	2,109	2,109	2,109	1,650	1,650	1,650	2,112	2,112	2,112
Num cities	434	434	434	349	349	349	435	435	435
Outcome mean	0.809	0.800	-0.009	0.765	0.802	0.036	0.970	0.787	0.183
Outcome stdv	0.085	0.082	0.047	0.129	0.083	0.130	0.038	0.079	0.082
<i>Panel C: local government job</i>									
Reform	0.001 (0.001)	-0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)	-0.000 (0.001)	-0.001 (0.001)	-0.000 (0.001)	-0.000 (0.003)	0.000 (0.003)
Observations	6,305	6,305	6,305	4,845	4,845	4,845	6,305	6,305	6,305
Num cities	454	454	454	366	366	366	455	455	455
Outcome mean	0.010	0.012	0.002	0.006	0.012	0.006	0.006	0.006	0.006
Outcome stdv	0.009	0.010	0.009	0.011	0.009	0.014	0.014	0.014	0.014
<i>Panel D: literacy</i>									
Reform	-0.001 (0.003)	-0.001 (0.001)	0.001 (0.003)	0.004 (0.007)	-0.000 (0.001)	-0.005 (0.007)	-0.000 (0.001)	-0.000 (0.003)	0.000 (0.003)
Observations	5,044	5,044	5,044	3,876	3,876	3,876	5,048	5,048	5,048
Num cities	454	454	454	366	366	366	455	455	455
Outcome mean	0.946	0.989	0.0430	0.901	0.988	0.0870	0.985	0.949	0.0360
Outcome stdv	0.0490	0.0140	0.0500	0.0890	0.0150	0.0810	0.0150	0.0460	0.0380

Note(s): Gap is defined as the difference between the more and less privileged group (e.g. native – immigrant, white – black and business – nonbusiness). Regressions estimated using all men aged 19–50 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 (unlogged for earnings) dependent variables are shown in the table. Note that individuals in business or nonbusiness occupations do not have a job in local government, hence missing coefficients in Panel C, Columns (7)–(9)

US cities and its effect on public employment probability for less advantaged city residents. Their results present a revisionist account similar to ours by showing that, in contrast to conventional wisdom on civil service, civil service reform was on average not associated with a lower probability of public employment for immigrant voters. Finally, in Panel D of Table 2, we similarly find that the literacy gaps between groups were not affected by reform.

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. Figure 3 exemplifies this point for the case of earnings. A regression that does not use trajectory balancing 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 the left panel of Figure 3, 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 (right panel of Figure 3). Online Appendix Figures A.2–A.4 present event-study estimates from equation (3) for all outcomes.

To further assess the socioeconomic effects of reform and explore potential mechanisms linking reform to inequality, we examine two additional outcomes from Census data. First, we analyze the population share of each demographic group to test whether earnings changes stem from shifts in group size. Economic deterioration could reduce a group’s population share through higher mortality, lower fertility or out-migration, which might in turn affect wages – for instance, reduced competition could increase wages for remaining workers. Consistent with our earlier findings of limited distributional effects, Online Appendix Figure A.5 shows no significant changes in group shares following reform.

Second, we examine whether reform is associated with occupational segregation. While ethnic clustering can sometimes foster employment opportunities by leveraging community-specific skills (Waldinger and Bozorgmehr, 1996), existing research finds little evidence that it improves minority earnings or educational outcomes (Model, 2018; Wilson, 1999). Online Appendix Figure A.5 shows suggestive, though imprecise, evidence that reformed cities exhibited higher occupational segregation [13].

Despite the limited distributional effects, reform-style government may have influenced aggregate welfare through differential economic growth relative to unreformed cities. While

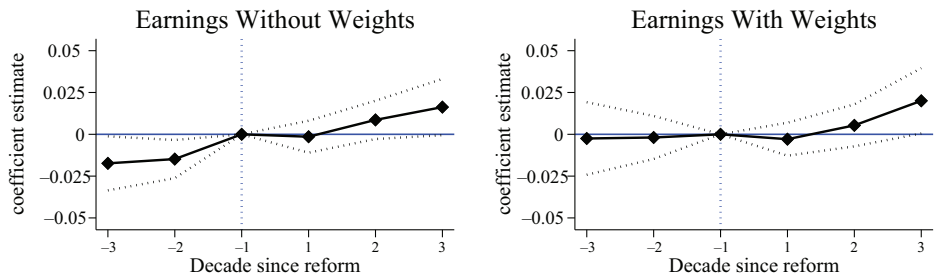


Figure 3. Event study estimates for the Native-Immigrant earnings gap

Note(s): Shows coefficient estimates from the model described in equation (3) 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 uses balancing weights as described in Hainmueller (2012). Dotted line shows the 95% confidence intervals

the lack of significant treatment effects across most outcomes and groups (Table 2 and Online Appendix Figure A.5) suggests otherwise, we test this directly by reestimating equation (2) using city–census decade-level data. Online Appendix Table A.3 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 and literacy rates did not change with reform.

Overall, our empirical analysis paints a picture that is inconsistent with large distributional or aggregate effects of the Progressive agenda. 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 nonbusiness residents) in the decades following the reform.

6. No change in public expenditures after reform

Allocating services and benefits has long been a core function of city government (Kaufmann, 2004). We therefore examine whether and how the adoption of reform-style government influenced public goods spending. If reformers significantly shaped local policy priorities and resource distribution, this should be evident in both the level and allocation of city spending. Policies favoring more advantaged residents over immigrant, Black and working-class citizens would likely result in reduced spending on redistributive categories like education and health, and increased spending on more regressive categories like parks and libraries.

Leveraging new yearly data on city-level spending broken down by fine-grained categories, we examine how spending evolved in reformed vs nonreformed cities [14]. 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 services: schools, fire, police, sanitation, public health, highways, recreation and hospitals. This newly collected data allows us to paint a comprehensive portrait of how municipal budgets were affected by the adoption of Progressive reforms.

We find few differences in the evolution of public goods spending between cities that reformed and those that did not. Table 3 shows the results. In Column 1, we find a modest and statistically insignificant increase in total spending of around 2% among cities that reformed. The upper bound of the 95% confidence interval is 5.8%. These results point in the same direction but are smaller in magnitude than Coate and Knight (2011), who find that per capita spending rose by just under 8% when cities adopted council-manager government in the 1980s and 1990s. Although Progressive Era reformers often claimed their proposals would cut costs and improve services (Bruere, 1913; Taylor, 1919), reformed cities did not reduce overall expenditures in the early 20th century. Importantly, the absence of an effect on total spending is not concealing distributional changes: Table 3 shows no impact on the composition of spending across categories.

These null results may help explain why we do not observe significant increases in socioeconomic gaps between more and less advantaged residents in reformed cities. While historical accounts suggest that Progressive reforms often served white, middle-class business interests, this may not have led to greater inequality if public goods spending remained stable. Admittedly, we cannot observe sub-city allocation, and case studies indicate reform governments often neglected poorer neighborhoods (e.g. Beach *et al.*, 2018; Judd and

Table 3. The impact of reform on public expenditures

Variable	Total (1)	School (2)	Police (3)	Highways (4)	Hospitals (5)	Fire (6)	Sanitation (7)	Recreation (8)	Health (9)
Reform	0.022 (0.018)	0.030 (0.018)	-0.010 (0.024)	0.013 (0.034)	-0.055 (0.142)	-0.000 (0.022)	0.026 (0.032)	0.055 (0.064)	-0.085 (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

Note(s): 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

Hinze, 2018). Nonetheless, aggregate differences in spending and policy priorities between reformed and nonreformed governments appear limited.

7. Validating our measure of reform-style government

We interpret the adoption of a council-manager or commission form of government as a proxy for reform-style governance – namely, the standard package of Progressive Era reforms. Historiographical evidence supports this interpretation, showing that institutional changes were typically accompanied by two additional reforms: nonpartisan ballots and at-large elections. While this bundling prevents us from isolating the effects of each component, our aim is to capture the broader reform agenda.

A potential concern is that the adoption of council-manager or commission government may not precisely mark the introduction of the broader Progressive Era reform package. If other reforms (i) occurred in different decades or (ii) had stronger economic effects, we may be underestimating the true impact of progressive reform. We address this concern using both quantitative and qualitative evidence.

Quantitatively, while precise dates are only available for changes in government form, we use data from the Municipal Yearbook (1940) to identify cities with nonpartisan ballots. Three findings support our approach. First, 76% of cities in our sample that adopted council-manager or commission government also had nonpartisan elections by 1940. Second, our results remain robust when we exclude control cities with nonpartisan ballots in Online Appendix Table A.4. Third, we find no heterogeneous effects when limiting the sample to cities that adopted both reforms (Online Appendix Table A.5). These results reinforce the validity of our proxy for reform-style governance.

Second, we collect qualitative evidence for a representative random subset of cities in our sample to show that reform-style government was introduced as one of the first acts of new progressive city governments and was accompanied by nonpartisan ballots and at-large elections. We carried out case studies for thirty treated cities, *i.e.* cities where a reform-style government or city commission was introduced during 1900–1940 [15]. We used different sources, including digitized newspaper archives, city charters, and historical scholarship on the topic. For each city, we searched for information on:

- the electoral dynamics surrounding the adoption of reform-style government;
- information about the mayors serving before and after the reform (especially partisanship, previous job and role in reform movement); and
- if and when nonpartisan ballots and at-large elections were introduced.

Qualitatively, we conducted case studies of 30 treated cities (those adopting reform-style government between 1900 and 1940). Using newspaper archives, city charters and historical studies, we examined:

- the political context of reform adoption;
- the profiles of pre- and post-reform mayors (e.g. partisanship, profession, reform involvement); and
- the timing of related reforms.

Our findings show that council-manager or commission government was typically introduced soon after the election of a reform-minded administration and accompanied by nonpartisan ballots and at-large elections – often within the same decade. We identified only four partial exceptions.

For example, Dayton, Ohio, implemented all three reforms together following a devastating 1913 flood. Under pressure from businessman John Patterson, the city introduced a five-member commission elected in nonpartisan, at-large elections, who then appointed a city manager ([The Boston Globe, 1914](#); [Dayton Daily News, 1914](#); [Sealander, 2014](#)). Patterson argued the city should be governed “not by partisans [...] but by men who are skilled in business management and social science” ([Weinstein, 1962](#)).

In Rochester, New York, a 1928 charter amendment introduced both the council-manager system and nonpartisan ballots after a campaign led by the City Manager League ([Story, 1926](#)). Vice Mayor Isaac Adler noted that the new charter eliminated party labels from municipal ballots and was “nearly identical to the model charter” used across New York State ([Democrat and Chronicle, 1926](#)).

Additional cases from Portland (OR), Bethlehem (PA) and San Jose (CA), discussed in Online Appendix section A.3, also support our interpretation. One partial exception is Toledo, Ohio, where progressive mayors Samuel M. Jones (1897–1904) and Brand Whitlock (elected in 1906) championed reform agendas decades before the city adopted council-manager government in 1936. While both mayors supported progressive causes, neither introduced the structural political reforms central to our study ([Holli, 1999](#); [Crunden, 1969](#)).

Finally, in East St. Louis, Illinois, voters elected a progressive mayor in 1913 who targeted vice and corruption. The council-manager form was adopted via ballot initiative four years later, in 1917 ([Lumpkins, 2006](#)). However, because both changes occurred within the same census decade, this case does not affect our empirical analysis because our unit of observation is a city-decade.

8. Validating treatment using turnout data

Finally, we examine how the adoption of reform-style government affected political participation. Existing empirical work typically finds that the reforms of the Progressive era reduced democratic participation across the board for city residents (e.g. [Hajnal and Trounstone, 2005](#); [Martin, 1933](#); [Schiesl, 1977](#)). By showing that the switch in form of government led to a significant shock to electoral participation, we can provide further evidence that the mostly insignificant socioeconomic effects that we uncovered in the previous section are not simply the result of a treatment measured with noise.

For this analysis, we construct 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). [Figure 4](#) shows how turnout evolved before and after the adopting of reform-style government, and we show the results formally with more discussion in the Online Appendix (Section A.5). We find consistent 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.

Our 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 6–8 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 specification not accounting for endogenous timing of reform, in Columns 1 and 3 of Table A.6 in the Online Appendix, are 65 and 48% larger than

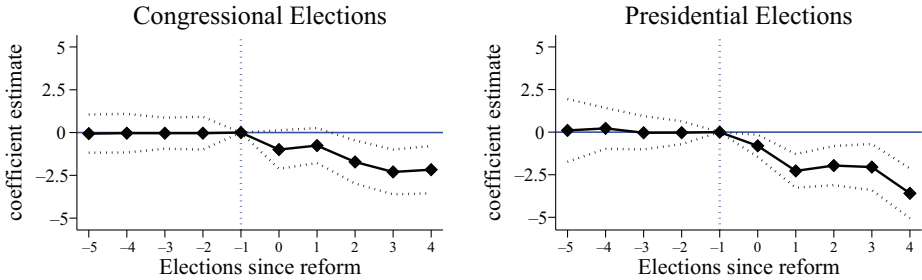


Figure 4. Event study estimates for voter turnout

Note(s): Shows coefficient estimates from the model described in [equation \(3\)](#) for congressional and presidential turnout with balancing weights. Dotted line shows the 95% confidence intervals

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 ([Banfield and Wilson, 1966](#); [Judd and Hinze, 2018](#); [Martin, 1933](#)). The observed turnout effect alleviates concerns that attenuation bias from treatment misclassification is driving our results.

These results indicate that reform governance had one clear and immediate consequence: a decline in political participation. While our data does not allow us to assess whether this decline was disproportionately concentrated among particular groups, existing literature suggests that lower-income and marginalized voters are often most likely to disengage under institutional changes that reduce electoral salience. If this pattern holds in reform cities, the resulting underrepresentation of less advantaged groups could have meaningful implications for policymaking and resource allocation. Over time, such shifts in political voice may contribute to persistent disparities in political influence and socioeconomic outcomes, potentially exacerbating inequality in the long run.

9. Discussion

We study whether the adoption of reform-style government in the early 20th century affected socioeconomic inequality between immigrants, African Americans, and the working class compared to native-born, white and business elites. We use de-anonymized census data to look at the most comprehensive set of measures of socioeconomic standing for these different groups of city residents. We find that the reform had at most modest distributional effects. We do not uncover any impact of reform on employment, the likelihood of having a job in the local government, nor on literacy. We find that the earnings gap increased only marginally in reformed relative to nonreformed cities. Crucially, a naive difference-in-differences approach would have uncovered a much larger effect of reform on earnings inequality. After accounting for the nonrandom adoption of reform institutions with trajectory balancing, we find that much of the difference in the earnings gaps can be explained by existing disparities in the wage dynamics of cities that reformed vs those that did not.

We also find no meaningful differences in public goods expenditures across reform and nonreformed cities, either in terms of total budget size or in terms of spending composition. This result suggests that the policy priorities of reformed and nonreformed cities may have been largely similar, which is in line with the fact that we fail to detect significant increases in

the socioeconomic gaps between different groups of more and less advantaged residents as a result of reform. 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.

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 using trajectory balancing and are in line with existing literature suggesting that the reforms of the Progressive era reduced political participation.

Together, these findings speak to important questions about the effects of Progressive-era reforms – particularly for poor and racial minorities. Most scholarly work on the effects of Progressive institutions has focused specifically on their 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 (Abott and Magazinnik, 2020; Davidson and Korbel, 1981; Engstrom and McDonald, 1981; Hajnal and Trounstine, 2007; Karnig and Welch, 1982; Marschall *et al.*, 2010; Trebbi *et al.*, 2008; Trounstine and Valdini, 2008). However, until this point, we have lacked systematic quantitative evidence on the economic impacts of these institutions, and different scholarly traditions offer competing predictions about the degree to which reformed cities may have exacerbated inequality.

Our findings call into question the perspective that the urban Progressive movement severely hampered the economic prospects of poor and immigrant city residents. Instead, our results are more consistent with work by Kuipers and Sahn (2022), Trounstine (2009), and Anzia and Trounstine (2025) that emphasize contradictions in the motivations behind reform adoption and unintended consequences of this movement. Although scholars tend to agree that the motives of urban reformers often reflected a desire to regain political power from the immigrant masses, the findings in this paper suggest that the economic and social consequences of these reforms may not have been as dramatic as the political effects.

Acknowledgements

Authors are listed in alphabetical order and contributed equally. Claudio Giambone and Lorenzo Pappalardo provided excellent research assistance. The authors would like to thank Sarah Anzia, Doug Cantor, Scott Gelbach, Asya Magazinnik, John Matsusaka, Nico Napolio, Pilar Sorribas, Jessica Trounstine and seminar participants at the University of Southern California, the European University Institute, Bocconi University, the Yale-UB Historical Political Economy Workshop, ITAM, the American Political Science Association annual meeting, the Stockholm Research Workshop on the Selection of Politicians and Bureaucrats, the Historical Political Economy Horizons Conference and the Virtual Workshop on Historical Political Economy.

Notes

- [1.] Note that civil service reform was also a common goal of Progressives, but this change was not typically introduced along commission and council-manager forms of government, at-large elections and nonpartisan ballots through city charter reform. Instead, cities began adopting civil service reform decades earlier than the period we study and continued to do so until late in the 20th century (Anzia and Trounstine 2025).
- [2.] For example, 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 incorporation 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) describe the opposition of working-class residents to reforms in nine urban cities. For a review of the historical and sociological literature, see Fox (2012) and Leonard (2016).

- [3.] We elaborate on this concern and explain how we address it both quantitatively and qualitatively with case studies in section 7.
- [4.] i.e. Dennis Judd and Todd Swanstrom, *City Politics: The Political Economy of Urban America* (New York: Pearson Education), 2009, 75-97. See (Liazos 2020) for a more comprehensive review.
- [5.] For this classification, we rely on the variable *NATIVITY* provided by IPUMS.
- [6.] For specific details on the various IPUMS variables used in each of our analyses, see Table A.2 in Online Appendix section A.2.
- [7.] We make predictions using only cities that had not reformed by 1940 to avoid projecting any consequences of reform into the past.
- [8.] We use 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; Gentzkow and Shapiro 2011; Iceland, Weinberg and Steinmetz 2002). 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 (Cutler, Glaeser and Vigdor 1999; Gentzkow and Shapiro 2011; White 1986). The Dissimilarity Index is defined for each city-year as $Dissimilarity = \frac{1}{2} \sum_{k \in K} \left| \frac{immigrants_k}{immigrants} - \frac{natives_k}{natives} \right|$, where k is one of the K occupations present in that city-year. The Isolation Index is defined as $Isolation = \sum_{k \in K} \frac{immigrants_k}{immigrants} \left| \frac{immigrants_k}{immigrants} - \frac{natives_k}{natives} \right|$. Both indices range between 0 (no segregation) and 1 (perfect segregation).
- [9.] An exception is our occupational segregation measures, which by construction can be computed only at the city-decade level.
- [10.] 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.
- [11.] Note that city-by-timing group fixed effects are effectively city fixed effects in our analysis using census data. While our analysis using political and fiscal outcomes 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).
- [12.] It is worth noting that our procedure to impute predicted earnings affects the interpretation of the estimates of the gap between business and nonbusiness residents. Because we rely on information on an individual's occupation, age, and region to predict their earnings, an important driver of a group's average earnings is the extent to which members of a group move across occupations. Because our analysis in Columns 7–9 compares the earnings of two groups defined on the basis of occupational categories, the changes in the average earnings of business (nonbusiness) residents in a city across decades will be mostly driven by shifts of residents between low and high paying business (nonbusiness) occupations. In other words, the treatment effects in Columns 7–9 abstract from possible movements of individuals from business to nonbusiness occupations, or vice versa. Importantly, as we show below, we do not observe any effect of the reform on the share of the population employed in business occupations, suggesting that the reform was not accompanied by significant changes in the composition of these groups.

- [13.] 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 nonbusiness occupations, which is why we omit this comparison from Figure A.5 in the Online Appendix. See the previous section for details.
- [14.] Given the higher frequency of city budget data with respect to census and elections data (yearly vs decennial and quadrennial/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 in the last three prereform years.
- [15.] We focused on the thirty largest treated cities in our sample by population in 1900.

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Supplementary material

The supplementary material for this article can be found online.

Corresponding author

Maria Carreri can be contacted at: maria.carreri@unibocconi.it