When Progressives Took Power: The Limited Economic Effects of Municipal Reform in U.S. Cities *

Maria Carreri,† *UC Berkeley*

Julia Payson,‡ *UCLA*

Daniel M. Thompson,§ *UCLA*

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Abstract

How did the adoption of reform-style government affect socioeconomic inequality in U.S. cities during the Progressive Era? The conventional wisdom describes the reforms of this time as reflecting racist and nativist impulses and primarily benefiting white business owners. However, political science work offers theoretical reasons to expect more nuanced redistributive effects of reform. We study the impact of reform leveraging deanonymized census records, newly digitized municipal budgets, and reform adoption dates across 455 U.S. cities during 1900-1940. Using a two-way fixed effects 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. Our results challenge the dominant narrative that the reforms of this era produced large increases in socioeconomic inequality.

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†Assistant Professor, Goldman School of Public Policy. Email:mariacarreri@berkeley.edu.

‡Assistant Professor, Department of Political Science. Email: jpayson@ucla.edu.

§Assistant Professor, Department of Political Science. Email: dthompson@polisci.ucla.edu.

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 U.S. 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, the goal was to make it more difficult for poor, immigrant and minority voters to cast ballots and achieve representation on city councils and to place policy in the hand of elite-backed "experts".

The conventional wisdom on progressive municipal reforms is based on this early scholarship, which continues to be widely cited and appears prominently in urban politics textbooks today (Liazos 2020). However, more recent work in political economy and political science offers both theoretical reasons and empirical observations that should lead us to expect more nuanced effects of these reforms on economic inequality. In particular, models of redistribution under elected vs. appointed public officials posit that the economic effects of these reforms should depend on the nature and size of the reform coalition and whether the newly adopted city institutions actually led to a change in political power (e.g. Alesina and

[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 2022).

Tabellini 2007). As we discuss in detail in the next section, the diversity of the urban progressive movement and lack of real political change in cities that adopted reform institutions may have mitigated the downstream consequences of reform on public service delivery and socioeconomic outcomes (Bridges 1999; Liazos 2020; Rice 1977; Trounstine 2009).

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 U.S. cities in the decades when these reforms were introduced. 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 U.S. 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, (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 covers the universe of 455 cities observed consistently during 1900-1940 in the U.S. 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 non-partisan ballots (Banfield and Wilson 1966; Bridges 1999; Lee 1960; Griffith 1927) and serves as an effective

[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) describes 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).

proxy for when Progressives gained control of the city government. We further discuss the theoretical and empirical rationale for focusing on this particular reform in section 2, and we present both quantitative and qualitative evidence in section 7 to validate the idea that the adoption of reform-style government reflected the key moment when Progressives assumed power.

Using a two-way fixed effects design that accounts for potential heterogeneous effects under staggered treatment (e.g. De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021; Xu 2017), we study how socioeconomic inequality evolved in reformed versus non-reformed cities. Importantly, since the timing of reform is not random, we also 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 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 percent and 0.7 percent, respectively, while the earnings of business workers increased by 0.6 percent 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 non-business earnings gap. Consistent with these limited effects on earnings, we show that other socioeconomic outcomes did not change systematically in reformed vs. non-reformed cities. 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.

Moreover, we document no 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 total public spending, nor significantly changed 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 progressive power and that noise in treatment measurement is biasing 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, Wright, and Streb 2001). This gives us confidence that the limited economic effects we observe are not the result of an underpowered design.

In conclusion, our results challenge the conventional wisdom and suggest that, on average, the impact of Progressive reform on urban socioeconomic inequality was negative but modest. We document a more limited impact than what suggested by early historical and qualitative accounts, complementing more recent work that revises our understanding of the effects of this movement (e.g. Kuipers and Sahn 2022; Anzia and Trounstine 2022). 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

[3] We elaborate on this concern and explain how we address it both quantitatively and qualitatively in section 7.

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 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 non-partisan 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 hand 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).

2.1 Challenging The Conventional Wisdom

This early scholarship on progressive municipal reforms continues to be influential and is predominant in urban politics textbooks (Liazos 2020).[4] However, a growing body of work in political science has challenged this stark negative view of urban reform by painting a more nuanced picture of the goals and effects of the urban progressive movement of the early twentieth century. There are also theoretical reasons to believe that the reforms of this era may have had more limited impacts on socioeconomic inequality than the conventional wisdom suggests.

The literature on elected vs. appointed public officials provides theoretical predictions for thinking about the redistributive impact of adopting the council-manager or commission form of government. Alesina and Tabellini (2007) study the desirability of a bureaucrat vis-a-vis an elected politician in implementing a redistributive task. The politician—who is accountable to voters at the polls—will always try to appease a numerical majority of voters through redistribution. The bureaucrat instead only cares about appearing competent in the eyes of their professional peers because this improves their future job prospects and can therefore split the pie in a potentially arbitrary and unfair way. Therefore, as long as a group is electorally of some value, i.e. its size does not make it inconsequential to politicians, its members are in expectation better off with a politician than with an unfair bureaucrat who arbitrarily allocates resources. Note that this is true for any group of non-negligible size, including both more and less advantaged city residents. Therefore, we should expect a disparate negative impact on less advantaged city residents only under the assumption that the appointed bureaucrat is captured by a coalition that excludes them.

[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.

Research in political science offers several reasons why the assumption above might not hold in practice and, in general, why we might be skeptical of the conventional wisdom portraying a large negative impact of urban reforms on political and socioeconomic inequality. First, this research highlights that the local coalitions backing the reform agenda were often very 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. Amy Bridges describes these coalitions as a "mixed assortment of motivations and goals, of efficiency and elitism, clean government and racism, the common good and exclusion" (Bridges 1999, p.).

Second, urban reform also did not always translate into real change in who held political power and how cities were run. Bridges (1999) highlights that in the first half of the 20th century reform often did not imply a change in local politicians as local elites campaigned on the reform platform to persist in power. In fact, the most common form of local politics in southwestern reformed cities in the first half of the twentieth century consisted of an array of "factions and personalities governing with little consistency" that did not in any way decrease "opportunities for petty corruption, for collusion among city officials and real estate interests [...] and for courting the disadvantaged" (Bridges 1999, p. 73). Trounstine (2009) makes the argument that what really negatively impacted local outcomes in this historical period was the presence of a political monopoly which was orthogonal to reform. Political monopolies arose when the governing coalition successfully limited the probability of being defeated in the future, with negative consequences for accountability and service provision. Importantly, local political monopolies arose under machine and reform governments alike.

Based on this existing literature, we should expect the impact of reform on the socioeconomic gap between more and less advantaged city residents to vary depending on (i) who was in the reform coalition, and (ii) whether this led to a change in who held political power at the city level. Textbook accounts suggest that reform coalitions were predominantly made-up of native, white and business-class elites, who replaced politicians on city council who were more responsive to the needs of immigrant, black and working-class residents. In this case, we should expect a large negative impact of reform on city-level socioeconomic inequalities. On the other side, the ideas that reform coalitions were often very heterogeneous and that reform did not often coincide with a relevant transition in political power at the local level suggest that we should expect a more moderate effect of reform.

The other two reforms that were typically introduced together with reform-style government, namely at-large and non-partisan elections, have typically been theorized to reduce the political power of racial minorities and poorer citizens, leading to worse descriptive representation and potentially lower levels of public goods provision and redistribution.[5] However, similarly to the case of council-manager or commission form of government, these theoretical considerations rest on the assumption that these reforms led to significant changes to the patterns of political selection. As discussed above, recent work in political science casts doubt on this assumption.

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 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

[5]For example, a long literature on at-large elections shows that, in the presence of geographical segregation, this institution worsens descriptive representation for racial minorities and poorer citizens with respect to a districted system (Abott and Magazinnik 2020; Davidson and Korbel 1981; John, Smith, and Zack 2018; Marschall, Ruhil, and Shah 2010; Ricca and Trebbi 2022; Sass and Pittman Jr 2000; Trounstine and Valdini 2008; Welch 1990) and can translate into large negative downstream economic consequences for the underrepresented citizens (Aneja and Avenancio-León 2019; Cascio and Washington 2014; Fujiwara 2015; Kose, Kuka, and Shenhav 2021; Naidu 2012).

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 non-partisan ballot was "incorporated in the majority of new charters" of U.S. 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 U.S. cities with a commission or council-manager form of government had non-partisan elections in 1929 (p.25) and that 83% of at-large cities were non-partisan by 1959 (Lee 1960 p25, 27, 28).

Case studies confirm that adopting reform-style government was typically associated with Progressives gaining power. In Cincinnati, the Republican Machine established by Boss George Cox had governed virtually unopposed for decades, until a group of reform-minded Democrats, Independents, and Republicans formed the City Charter Committee or Charter Party in 1924. The new municipal charter of 1925 established a council-manager form of government, at-large elections, and non-partisan ballots, and the next city council election in 1926 ushered in the election of six "Charterites" to the city council and the first Democratic mayor in 40 years (Burnham 1997).

In Spokane, Washington, the Progressive Era reformer Charles Marvin Fassett was the city's lead advocate for the commission-style government. He was appointed as one of the first city commissioners in 1911 following the adoption of the new charter and was subsequently elected mayor in 1914 (Rice 1977). In Wichita, Kansas, Progressive residents in the city petitioned for an election in 1917 and voted to adopt the council-manager form of government. The new voting bloc ousted the incumbent mayor and elected five new self-professed reformers to the city commission (Stillman 1974). After presenting the main results, we present several additional case studies in section 7 and quantitative evidence to validate the idea that charter reform serves as a valid proxy for Progressive power. In the next section, we introduce our data sources and research design.

3 Data

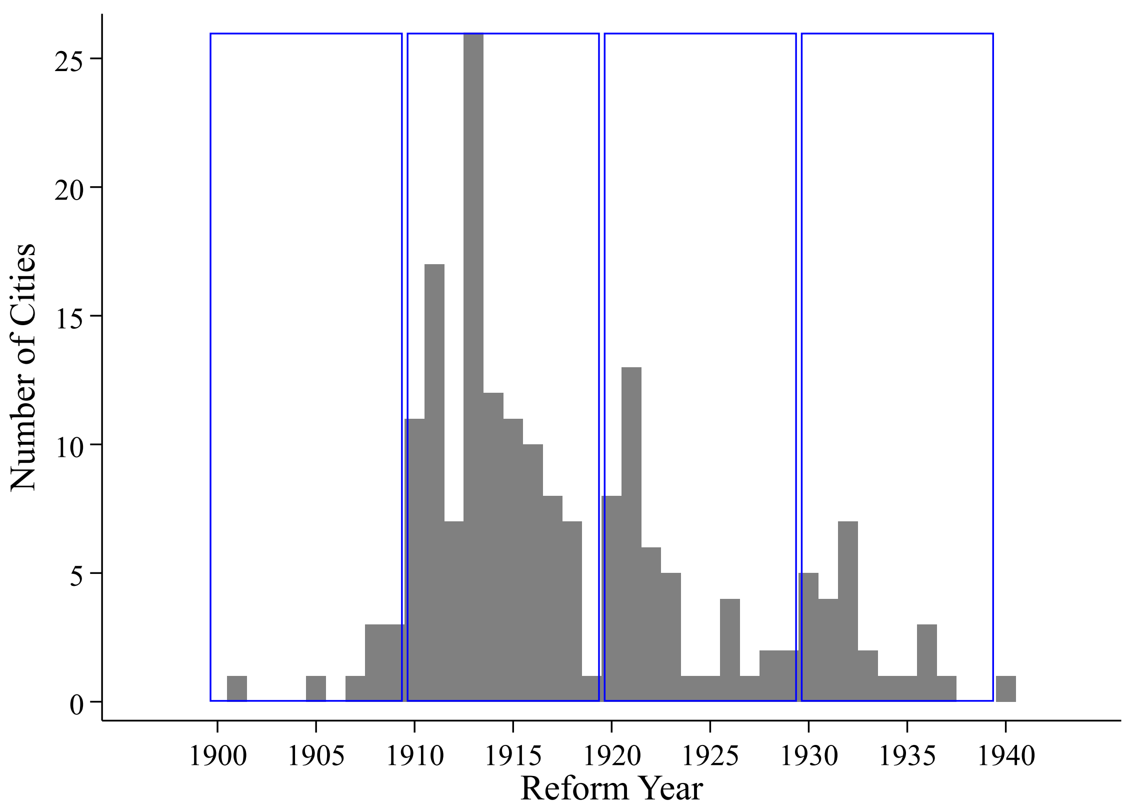
3.1 Data Collection

In this paper, we use the adoption of reform-style government (either council-manager or city commissioner form of government) to proxy for progressive power. 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 of reform-style government for the 1,100 largest cities in the U.S.

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 U.S. 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.

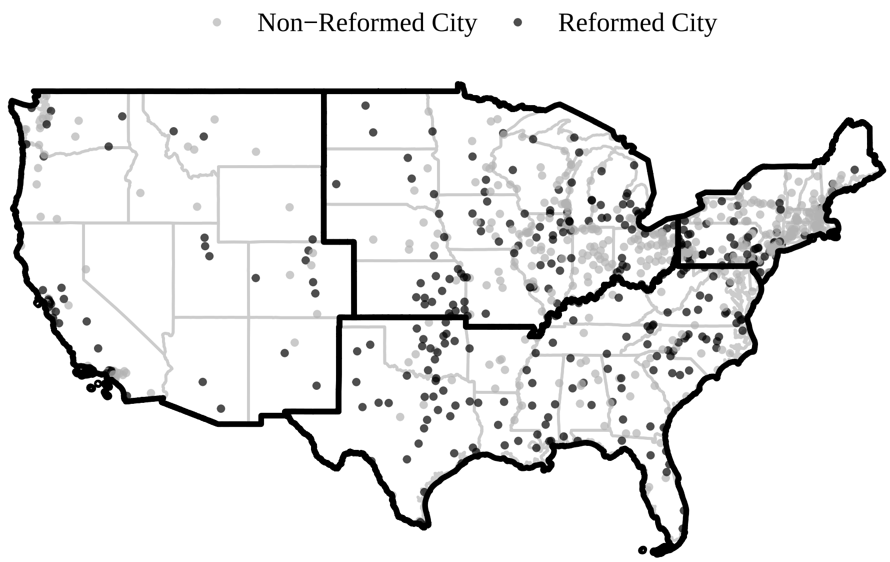
Figure 1: Number of Reforms by Year



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 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

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.

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 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 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.[6] 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 (e.g. Buchmann and McDaniel 2016) and define "business" to include occupations classified under the Managers, Officials, and Proprietors category according to IPUMS.[7]

We use wage earnings as our primary 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 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.[8] 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.

[6]For this classification, we rely on the variable *NATIVITY* provided by IPUMS.
[7]For specific details on the various IPUMS variables used in each of our analyses, see the Appendix.
[8]We make predictions using only cities that had not reformed by 1940 to avoid projecting any consequences of reform into the past.

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. *Employment* is the share of each group that is employed. *Local Government Job* 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. *Literacy* is an indicator that takes a value of one if a respondent could read and write. *Group Population Share* is the share of each group among the residents of a city.

Finally, we calculate *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; 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).[9]

With the exception of our occupational segregation measures, which by construction can be computed only at the city-decade level, 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. 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.

[9]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 $Isolation = \sum_{k\in K}\frac{immigrants_k}{immigrants}\frac{immigrants_k}{immigrants} - \frac{natives_k}{natives}$. Both indices range between 0 (no segregation) and 1 (perfect segregation).

4 Empirical Approach

Our goal is to study the effect of the Progressive movement across U.S. 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 two-way fixed effects specification for our setting would be the following

$$y_{ct} = \gamma_c + \delta_t + \beta Reformed_{ct} + \varepsilon_{ct} \tag{1}$$

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 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 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. De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021; Xu 2017). 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

[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.

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:

$$y_{cgt} = \gamma_{cg} + \delta_{tg} + \beta Reformed_{cgt} + \varepsilon_{cgt} \tag{2}$$

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.[11] 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 (or commission) 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.

[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).

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

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

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. Table 1 reports estimates from equation 2, 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 modest 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).

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 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

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	1181.529	1219.991	1.042	785.284	1216.965	1.576	1015.153	1707.21	1.704
Outcome Stdv	156.596	137.448	.116	140.737	143.177	.195	149.996	166.856	.197
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

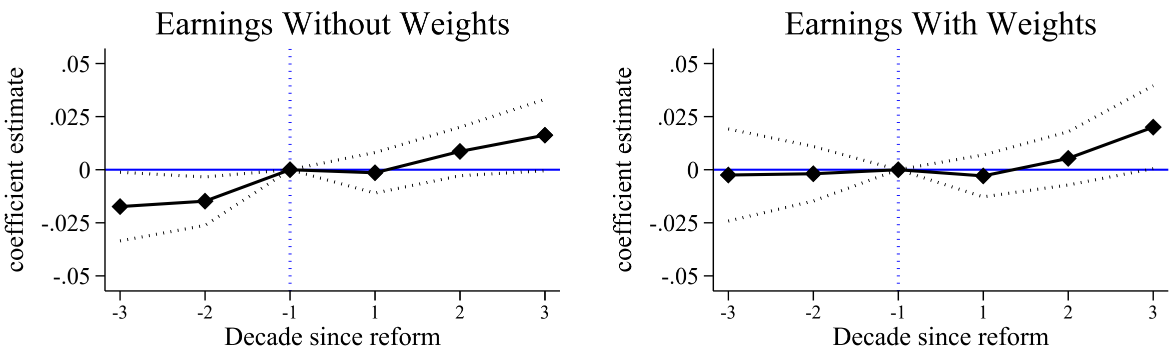
Notes: Gap is defined as the difference between the more and less privileged group (e.g. native - immigrant, white - black, and business - non-business). Regressions estimated using all men age 19 to 50 living in cities from 1900 to 1940. Heteroskedasticity-robust standard errors clustered by city reported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

on similar trends before the reform, we estimate a significantly smaller effect of the reform (see the right panel 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 3 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.

It is worth noting that our procedure to impute predicted earnings affects the interpretation of the estimates of the gap between business and non-business residents. Since we rely

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



Notes: 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 employs balancing weights as described in Hainmueller (2012). Dotted line shows the 95 percent confidence intervals.

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. Since 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 (non-business) residents in a city across decades will be mostly driven by shifts of residents between low and high paying business (non-business) occupations. In other words, the treatment effects in columns 7-9 abstract from possible movements of individuals from business to non-business 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.

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 2 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.

As we show in Figure 4, we do not find a significant effect of reform on employment gaps between more and less advantaged city residents. 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.[12] The fact that we do not find such impact is instead inconsistent with this hypothesis. We similarly find that the literacy gaps between groups were not affected by the adoption of reform.

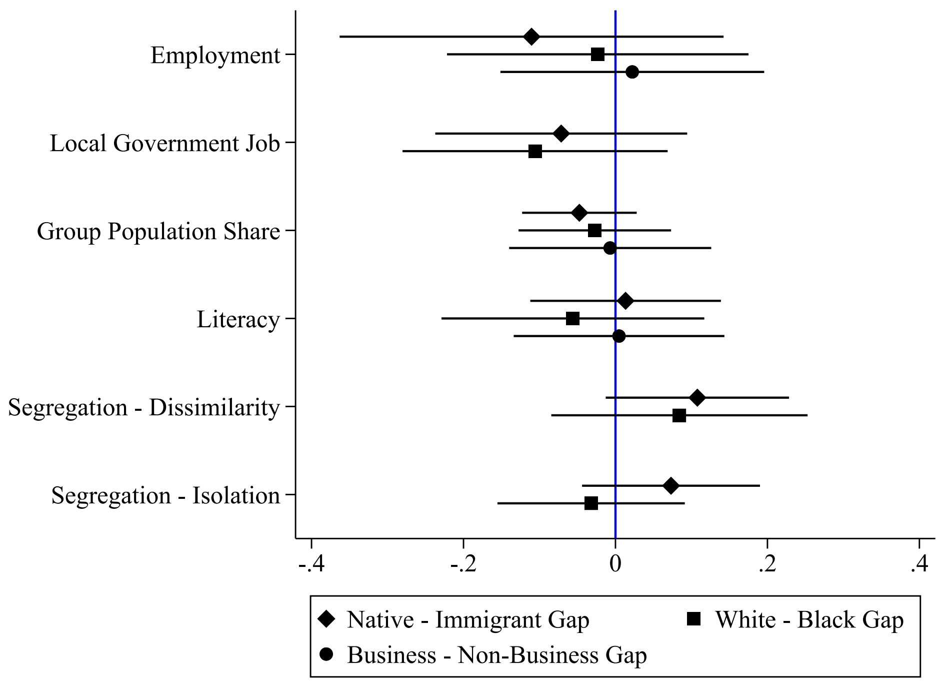
Next, we look at the overall share of each group in the population. We do this for two reasons. First, worsening economic conditions for a particular demographic group in the decades following the reform could lead to a decrease in that group's population share through a combination of increased mortality 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 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.

Finally, we use our segregation measures to investigate whether cities that reformed had higher degrees of occupational segregation. Importantly, such clustering is not necessarily detrimental for minorities. For instance, co-ethnic niches can capitalize on particular skill

[12] Interestingly, Kuipers and Sahn (2022) study the introduction of civil service reforms across U.S. 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 less immigrant voters.

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 (Model 2018; Wilson 1999). We do find some evidence that cities that reformed had higher degrees of occupational segregation, although the estimates are imprecise.[13]

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 2. 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.

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

[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 non-business occupations, which is why we omit this comparison from Figure 4. See the previous section for details.

the absence of significant aggregate welfare effects by re-estimating equation 2 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	1154.498	23.349	0.801	0.011	0.952
Outcome Stdv	142.017	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 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 non-business residents) in the decades following the reform.

6 No Change in Public Expenditures After Reform

Allocating services and benefits was and still is one of the key functions of city government (Kaufmann 2004). Therefore, we next examine whether and how the adoption of reform-style government influenced the allocation of spending on public goods. If reformers had a sizeable impact on the policy priorities of their local government and significantly affected the distribution of resources across communities, we would expect this to be reflected in the level of city spending as well as its distribution. We would expect policies aimed at favoring more advantaged city residents at the expense of immigrant, black and working-class citizens to materialize in a sizeable decrease in redistributive spending categories, such as education and health, and a sizeable increase in more regressive spending categories, such as 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. non-reformed 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 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. The upper bound of the 95% confidence interval is 5.8%. 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.

[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 pre-reform years.

Importantly, the lack of effect on total budget size is not masking distributional effects. Table 3 shows no impact on the composition of spending across the different spending categories.

Table 3: 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.

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. Beach et al. 2018; Judd and Hinze 2018). But the aggregate differences in the policy priorities and overall spending of reformed vs. non-reformed governments appear to be modest overall.

7 Validating Reform-style Government as a Proxy of Progressive Power

So far, we have interpreted the adoption of reform-style government as a proxy for Progressives gaining power. The historiographical literature strongly suggests that adopting this new form of government was often bundled with the two other main progressive reforms: nonpartisan ballots and at-large elections. While this means we cannot isolate the effects of council-manager or commission form of government from these other reforms, our argument is precisely that the switch in form of government proxies for Progressive power broadly. However, a different concern is that perhaps the switch in form of government does not effectively represent the moment when Progressives took control of city government, and perhaps by not observing the adoption of other reforms we are underestimating our main effects. In other words, if these other reforms (i) were not adopted in the same decade as the council-manager or city-commission form of government, and (ii) had stronger economic impacts than the switch in form of government, this might explain the largely null results we uncover. We address this concern drawing from both quantitative and qualitative evidence.

First, while the precise date of adoption is only available for changes in the form of government, we leverage information in the Municipal Yearbook of 1940 documenting which cities had non-partisan ballots by that year. We use this information to present three pieces of empirical evidence. First, we document that 76% of cities that adopted council-manager or commission form of government in our sample also had introduced non-partisan elections by 1940. Second, in appendix Table A.6 we demonstrate that our results are robust to excluding from the sample control cities that had non-partisan ballots in 1940. Third, in appendix Table A.7, we show that there are no heterogeneous effects when we zoom in on cities that had also adopted non-partisan ballots by 1940. Taken together, theses additional analyses lend support to the idea that reform-style government is an effective proxy for overall Progressive power.

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 typically accompanied by non-partisan ballots and at-large elections. We carried out case studies for thirty treated cities in our sample, *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 i) the electoral dynamics surrounding the adoption of reform style government, ii) information about the mayors serving before and after the reform (especially partisanship, previous job, and role in reform movement), and iii) if and when non-partisan ballots and at-large elections were introduced.

We found strong support for city-manager/commission form of government being a valid proxy of Progressive power in city government. City charters were amended to introduce city manager or city commission form of government shortly after the election of a new reform minded city government in the vast majority of cases analyzed. Moreover, the reform we study was typically accompanied by other reforms introduced at the same time or in the same decade. Out of thirty cities, we identified only four "counter examples,'' i.e. cities where either non-partisan ballots or at-large elections were introduced in a different year with respect to the council-manager or commission form of government, or where progressive governments had been in power before the introduction of the reform we study.

For example, Dayton, Ohio, was one of the first cities to reform its form of government and it concurrently introduced at-large elections and non-partisan ballots. Dayton adopted these policies under the pressure of John Patterson, an influential businessman and the largest employer in Dayton in the early 1900s, who took charge of relief efforts after a flood in 1913 killed many citizens and caused large destruction. Dayton introduced a new form of government featuring five commissioners elected in nonpartisan, citywide elections who appointed a city manager (The Boston Globe 1914; Dayton Daily News 1914; Sealander 2014). Patterson sponsored the idea that Dayton should be governed "not by partisans [...] but by men who are skilled in business management and social science; who would treat our

[15]We focused on the thirty largest treated cities in our sample by population in 1900.

money as a trust fund, to be expended wisely and economically, without waste, and for the benefit of all citizens" (Weinstein 1962).

In Rochester, New York, the Council-Manager form of government was introduced in 1928 when voters approved an amendment to the city charter, after a long campaign that saw the active involvement of the City Manager League (Story 1926). The amendment also introduced non-partisan ballots. As stated by the vice mayor Isaac Adler to a local newspaper, the *Rochester Democrat and Chronicle*, in the municipal elections of 1928, "the principal election change made by the new Charter is the provision for a non-partisan ballot without emblems of any kind, designed to do away with party voting in municipal elections" (Democrat and Chronicle 1926). Interestingly, the vice mayor also points out that the charter resulting from the amendment was "nearly identical to the model charter" and, importantly for us, similar to several other new charters in New York state introducing the council-manager form of government as well as non-partisan elections. (Democrat and Chronicle 1926).

We present further case studies confirming our claims in Appendix section A.3, including evidence from Portland (OR), Bethlehem (PA), and San Jose (CA). One of the few cases we found that constitutes a partial counterexample was Toledo, Ohio, where two mayors who were considered progressive politicians took office long before the introduction of the Council-Manager form of government in 1936. Samuel M. Jones, whose term lasted from 1897 until his death in 1904, is the most famous mayor of Toledo (OH) and was voted in a 1993 survey as the fifth-best among all mayors who ever held office in the United States. Jones made history by campaigning for municipal ownership of the utilities, public ownership of national trusts, fair pay for labor, and for striving to improve conditions for the working class in Toledo (Holli 1999). Later, the progressive journalist and member of the Democratic party Brand Whitlock, after being elected mayor in 1906, continued Jones' reform efforts (Crunden 1969). However, these mayors did not introduce any of the political reforms at the center of our study, and their tenure took place more than 30 years before the city adopted council-manager government.

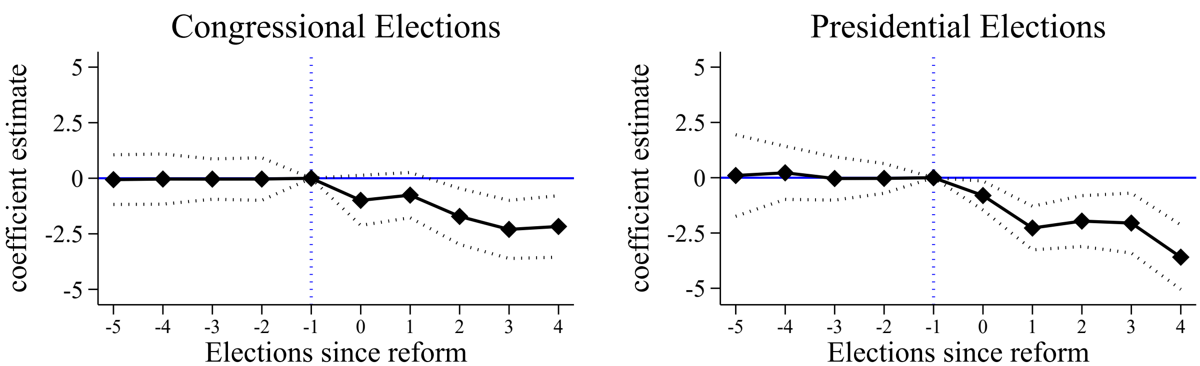
Finally, Voters in East St. Louis, Illinois, elected a progressive mayoral candidate in 1913 who promoted suppression of prostitution, gambling and illegal saloons. The Council-Manager form of government was voted via ballot initiative four years later, in November 1917 (Lumpkins 2006). It is worth noting that this case does not affect our empirical analysis. Because census data are available every ten years, the unit of observation is a city-decade, and both the progressive governments described above fall in the same 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 Trounstine 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 5 shows how turnout evolved before and after the adopting of reform-style government, and we show the results formally with more discussion in the appendix (Section A.4). 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.

Figure 5: Event Study Estimates for Voter Turnout



Notes: Shows coefficient estimates from the model described in equation 3 for congressional and presidential turnout with balancing weights. Dotted line shows the 95 percent confidence intervals.

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 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 specification not accounting for endogenous timing of reform, in columns 1 and 3 of Table A.10, 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 (Banfield and Wilson 1966; Judd and Hinze 2018; Martin 1933). These results further assuage concerns around noise in our treatment biasing our coefficient estimated downwards.

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. 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. Crucially, a naive difference-in-differences 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 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 non-reformed cities, either in terms of total budget size or in terms of spending composition. This result suggests that the policy priorities of reformed and non-reformed 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 employing 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, Ruhil, and Shah 2010; Trebbi, Aghion, and Alesina 2008; Trounstine and Valdini 2008). However, until this point we have lacked systematic 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) and Trounstine (2009) that emphasizes the contradictions and unintended consequences of this movement. Although scholars tend to agree that the motives of urban reformers often reflected a desire to re-gain 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.

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Online Appendix: When Progressives Took Power. The Political and Economic Effects of Municipal Reform in U.S. Cities

Intended for online publication only.

A.2 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	4429
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
Aggregate Outcomes					
Log Predicted Earnnings	6.996	0.127	6.535	7.222	455
Log Total Population	9.092	1.107	6.021	14.518	6310
Employment	0.801	0.076	0.446	0.950	2112
Local Government Job	0.01	0.007	0	0.077	6310
Literacy	0.946	0.049	0.625	1	5048

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.3 Case Study Evidence: Validating Reform-Style Government as a Proxy for Progressive Power

Here we present several additional case studies validating our argument that i) the adoption of city-manager or commissioner form of government reflected a moment when Progressive seized power and ii) this reform was typically accompanied by other Progressive measures. Other case studies are presented in the main paper in section 7.

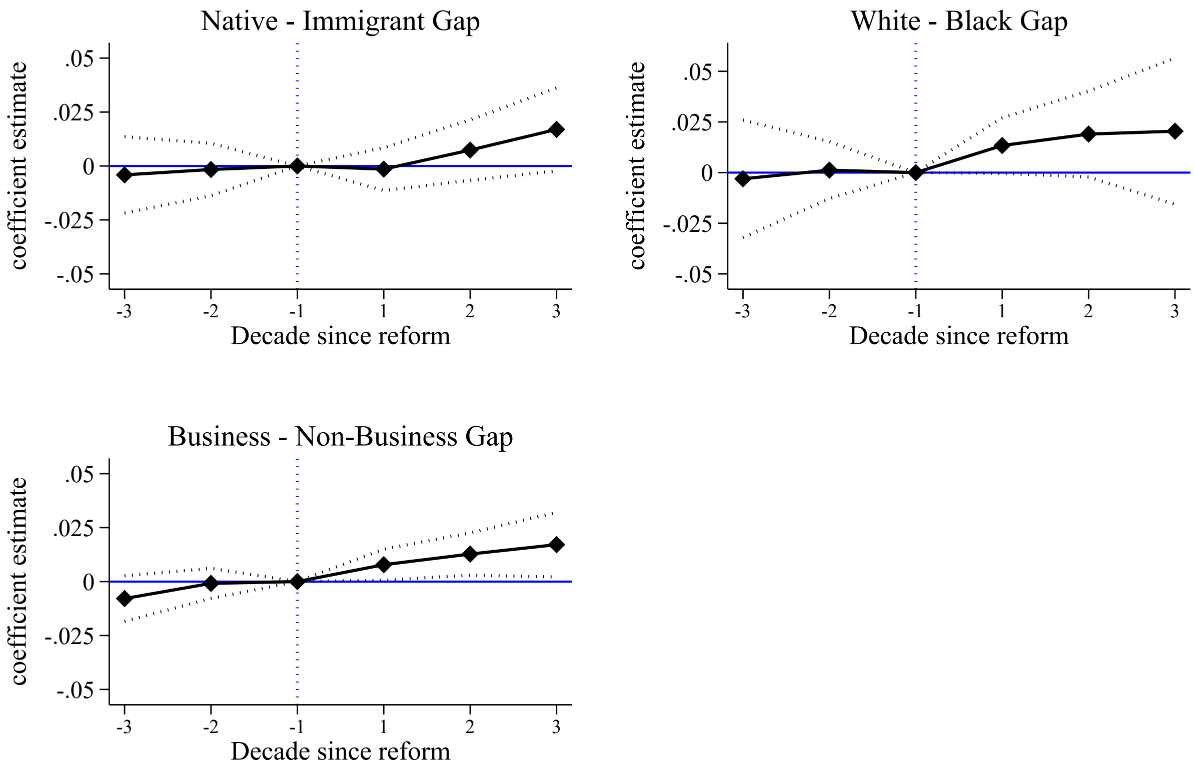
In Portland, Oregon, scandals surrounding the administration of Allen G. Rushlight—mayor between 1911 and 1913—were the breeding ground for the fast rise to power of the local progressive movement. After the New York Bureau of Municipal Research offered a detailed and jarring critique of Portland's local government, in 1913 Portland voters supported a new city charter mandating a commission government and elected the progressive mayor Russel H. Albee (Bureau of Municipal Research 1913). The new charter passed by a small margin, with opposition coming from the local Republican Party machine, and support coming from Roosevelt Progressives, downtown businesses, and the middle class homeowners and professionals (Johnston 2003; Lansing 2005).

The local business elites of Bethlehem, Pennsylvania, had long advocated for the consolidation of the municipality of Bethlehem and South Bethlehem into the city of Bethlehem and for a new city charter establishing a commission form of government (Vadasz 1975). This plan was realized thanks to the backing of the Bethlehem Steel Company in 1918, when its executive Archibald Johnston was elected mayor in the first election for the city of Bethlehem which took place with an at-large non-partisan race (The Morning Call 1918).

Finally, in San Jose, California, a Good Government League was formed as early as 1900 by the brothers E.A. and J.O. Hayes, and a mayor backed by the League served one term starting in 1902 (Lukes 1994; Bridges 1999). Interestingly, these early San Jose reformers were characterised by nativist rhetoric and their particular brand of progressivism prioritising "urban development, cheap labor, and electoral reform" influenced the large progressive movement in California (Lee 1960). Lee (1960) reports a new charter passed in 1906, but we could not find any information regarding the provisions of this new charter. Finally, in 1914 the progressives took control of City Hall again. In 1915, as reported in our data, they established the City Manager form of government, accompanied by at-large council elections and non-partisan ballots (Lee 1960).

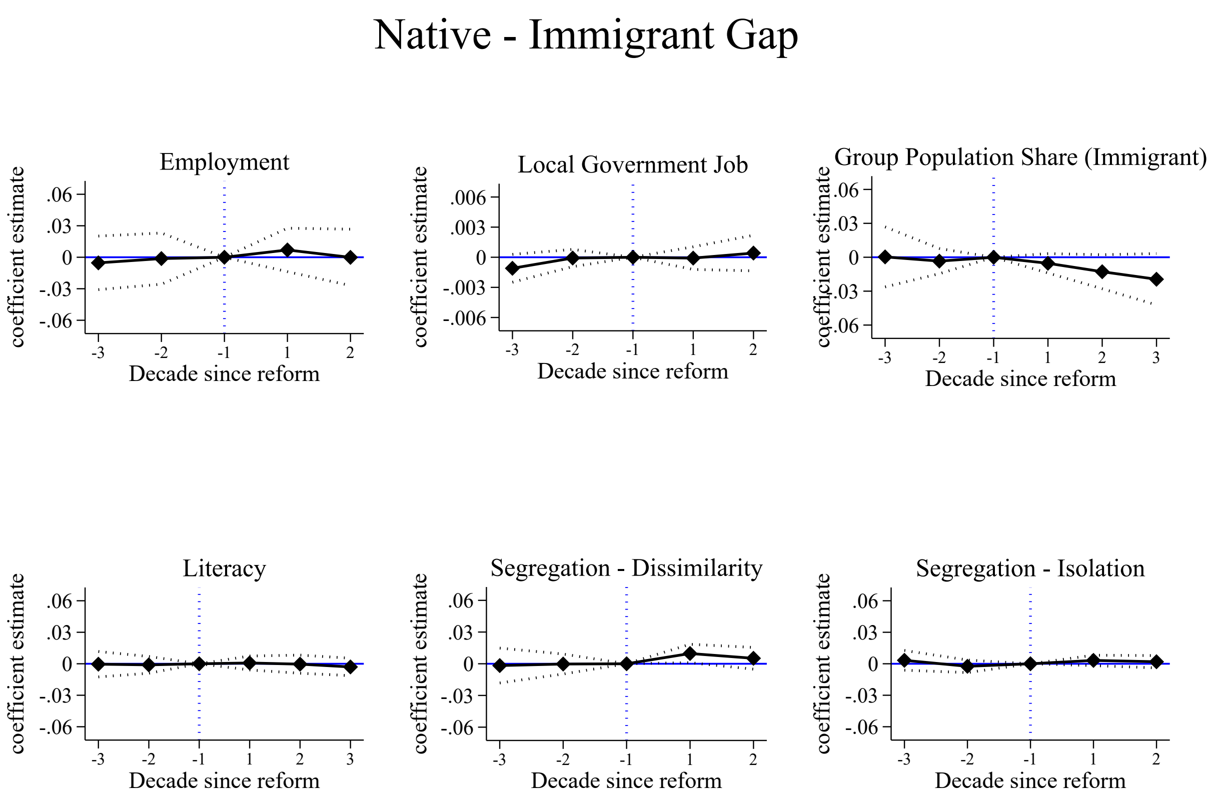
A.4 Additional Statistical Results

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



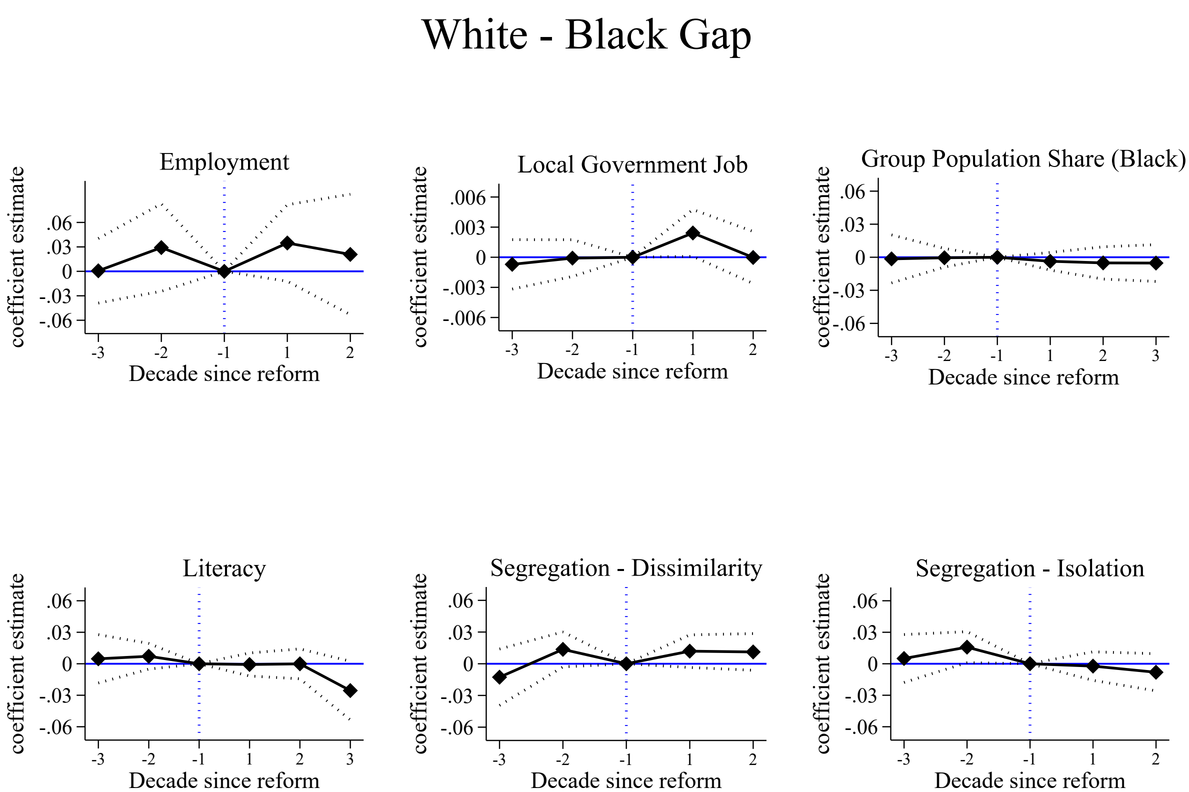
Notes: Shows coefficient estimates from the model described in equation 2 employing balancing weights for outcomes shown in Table 1. Dotted line shows the 95 percent confidence intervals.

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



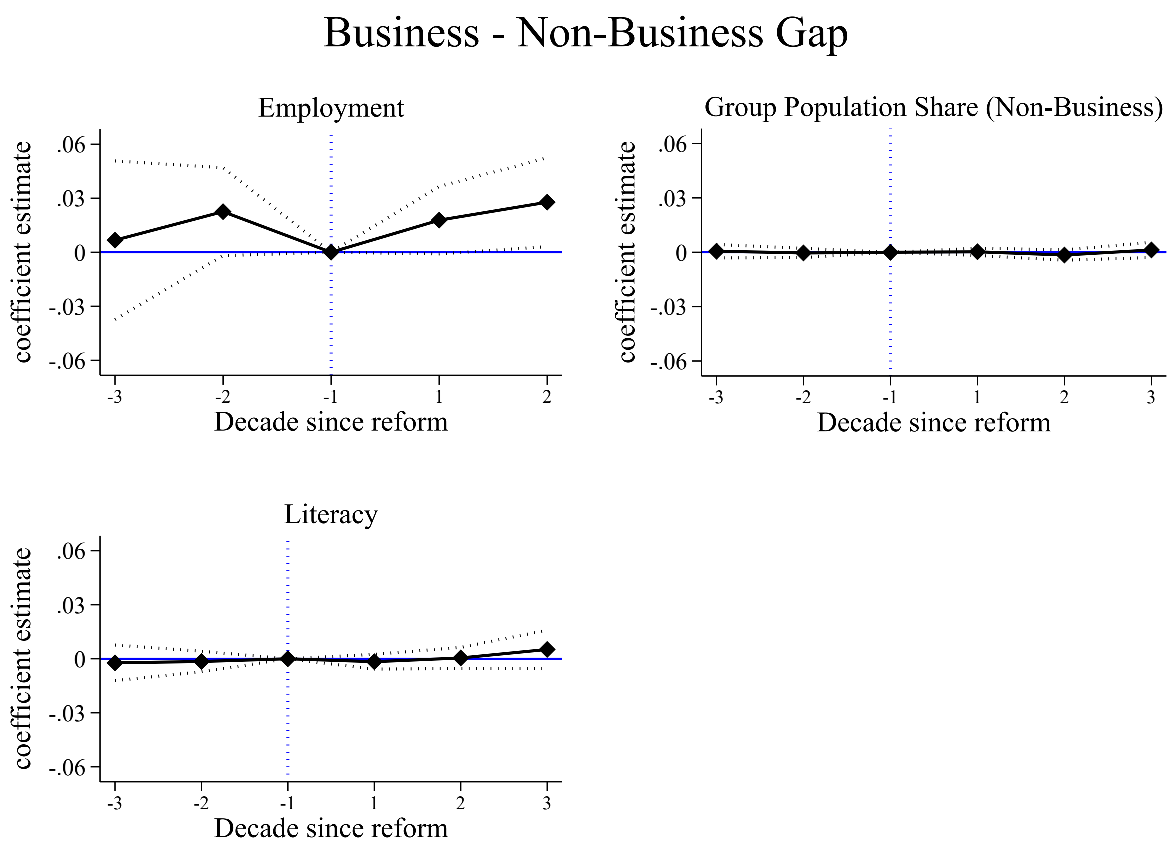
Notes: Shows coefficient estimates from the model described in equation 2 employing balancing weights for outcomes shown in Figure 4 for native and immigrant residents. Dotted line shows the 95 percent confidence intervals.

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



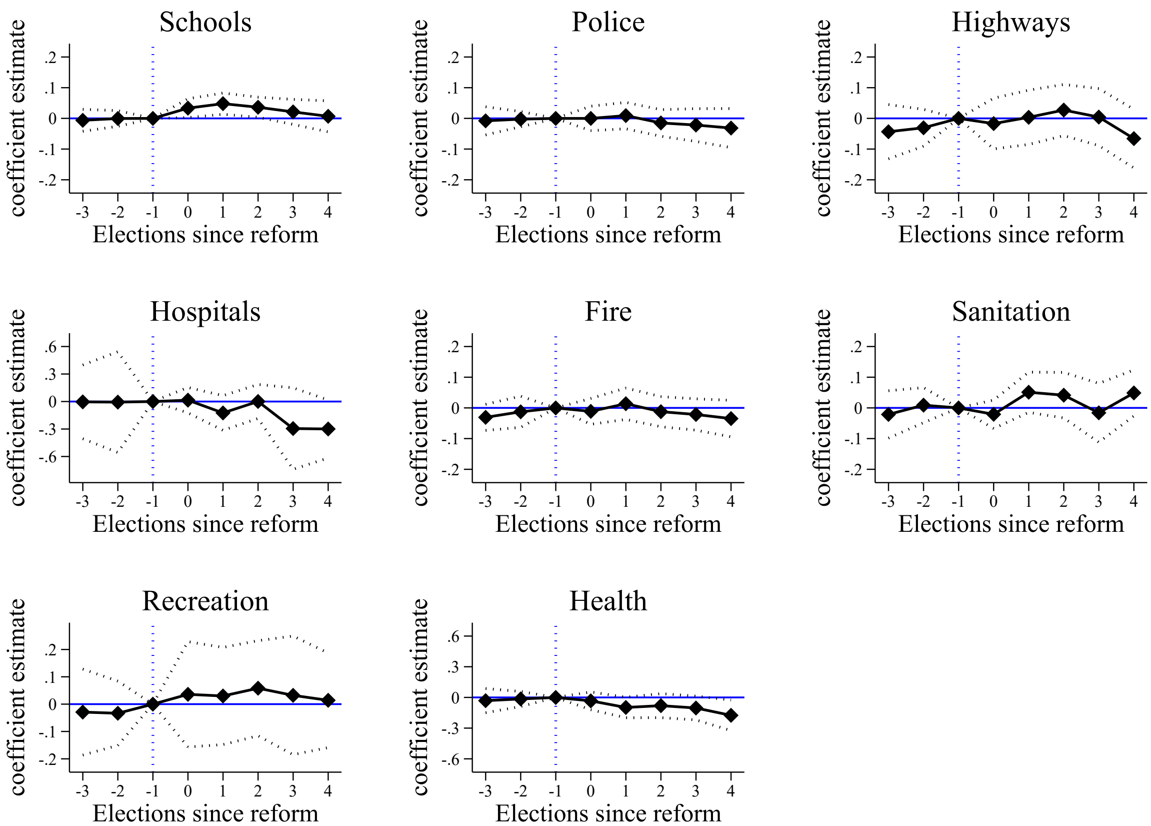
Notes: Shows coefficient estimates from the model described in equation 2 employing balancing weights for outcomes shown in Figure 4 for white and black residents. Dotted line shows the 95 percent confidence intervals.

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 2 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 2 employing balancing weights for outcomes shown in Table 3. Dotted line shows the 95 percent confidence intervals.

A.5 Political Participation Results

Table A.10 reports results from equation 2, 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

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

	Native - Immigrant Gap					
		Local	Group		Segregation	
	Employment	Government Job	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					
		Local	Group		Segregation	
	Employment	Government Job	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.

specification employing entropy balancing weights as described in section 4.[16] Standard er-

[16]Election results are available for a larger number of cities than 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.9 shows robustness to restricting the electoral analysis to the sample of 455 cities present in census data and Figure A.6 presents the corresponding event-study estimates. Note that 2 cities cannot be included because election results are available for one year only.

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: Robustness to excluding control cities with non-partisan elections by 1940

	Predicted Log Earnings								
	Immigrant	Native	Gap	Black	White	Gap	Non-Business	Business	Gap
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Reform	-0.017	-0.008	0.008	-0.014	0.002	0.016	-0.009	0.006	0.016
	(0.007)	(0.006)	(0.007)	(0.011)	(0.008)	(0.012)	(0.005)	(0.003)	(0.005)
Num Obs	4,265	4,265	4,265	3,385	3,385	3,385	4,270	4,270	4,270
Num Cities	352	352	352	293	293	293	353	353	353
Outcome Mean	1183.201	1223.397	40.196	785.422	1217.772	432.35	1015.874	1708.912	693.039
Outcome Stdv	152.906	147.3	134.583	147.672	148.489	119.928	149.34	184.095	130.344
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: Reproduces results shown in Table 1 excluding from the sample those control cities that had non-partisan elections in 1940. The mean and standard deviation of the unweighted dependent variable are shown above.

rors are clustered at the city level and Appendix Tables A.8 shows robustness to clustering standard errors at the county level. Figure 5 presents event-study estimates from equation 3.

As described in section 3.1, since electoral data are only available at the county level, we assign to each city the turnout in the county where the city is located. This procedure assigns the same turnout values to cities located in the same county. In our sample, 62% of

Table A.7: Heterogeneity based on non-partisan elections in 1940

	Predicted Log Earnings								
	Immigrant	Native	Gap	Black	White	Gap	Non-Business	Business	Gap
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Reform × Non-partisan	0.021	0.015	-0.006	0.020	0.009	-0.012	0.008	-0.005	-0.013
	(0.013)	(0.009)	(0.012)	(0.017)	(0.010)	(0.020)	(0.009)	(0.005)	(0.011)
Reform	-0.024***	-0.010	0.014	-0.015	-0.001	0.014	-0.013*	0.007*	0.019**
	(0.009)	(0.007)	(0.009)	(0.014)	(0.008)	(0.016)	(0.007)	(0.004)	(0.008)
Non-partisan	-0.005	-0.012*	-0.007	-0.017	-0.012	0.005	-0.003	0.006**	0.010
	(0.009)	(0.006)	(0.007)	(0.011)	(0.008)	(0.013)	(0.005)	(0.003)	(0.006)
Num Obs	6,285	6,285	6,285	4,825	4,825	4,825	6,290	6,290	6,290
Num Cities	453.000	453.000	453.000	365.000	365.000	365.000	454.000	454.000	454.000
Outcome Mean	1180.934	1221.088	40.15428	783.681	1215.863	432.182	1014.377	1707.941	693.564
Outcome Stdv	152.5885	148.382	139.054	148.007	147.498	116.179	150.252	1707.941	127.971
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: Shows heterogeneity of results shown in Table 1 for cities that had non-partisan elections in 1940. The mean and standard deviation of the unweighted dependent variable are shown above.

Table A.8: 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 A.10 with standard errors clustered at the county level. See Table A.10 for additional table notes.

city-year observations belong to a county shared with at least another city in our sample. In Appendix Table A.11 and Figure A.7, we show robustness to restricting our sample to cities that do not share the county with any other city in the sample. Despite the substantial reduction in sample size, which shrinks to approximately one third of the full sample, results are qualitatively similar.

Table A.9: The Impact of Reform on Voter Turnout – Restricting Sample to counties in Earnings Results Sample

	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 A.10, 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.

Table A.10: 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

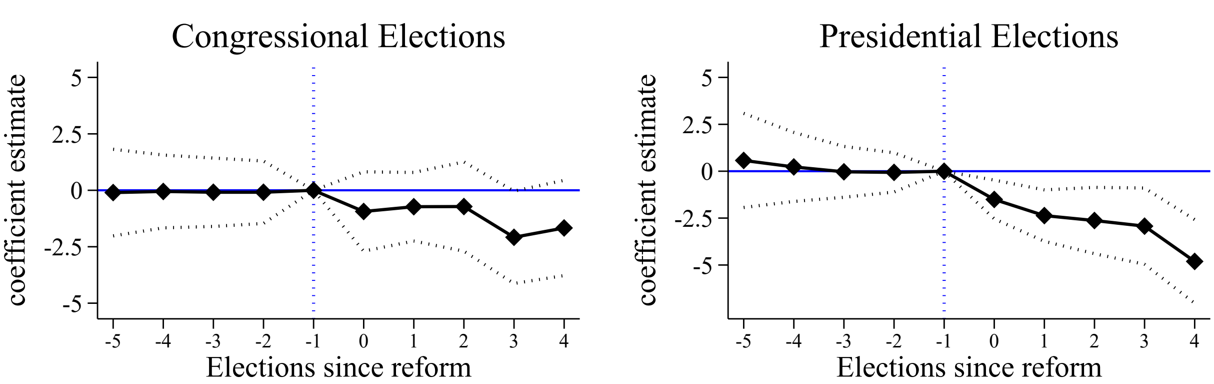
Notes: Shows estimates of the effect of the reform on voter turnout. The dependent variable is turnout in the county where each city is located in each two year period (for Congressional elections) or four year period (for Presidential elections). Heteroskedasticity-robust standard errors clustered by 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.

Table A.11: The Impact of Reform on Voter Turnout – Restricting Sample to One-City Counties

	Turnout Congressional Elections		Turnout Presidential Elections	
	(1)	(2)	(3)	(4)
Reform	-0.536	-1.173	0.157	-0.239
	(0.861)	(0.884)	(0.840)	(0.887)
Num Obs	87,297	74,319	47,278	47,278
Num Cities	449	449	626	626
Outcome Mean	56.029	47.412	59.243	50.789
Outcome Stdv	21.367	21.86	23.779	24.259
City × Timing Group FEs	Yes	Yes	Yes	Yes
Year × Timing Group FEs	Yes	Yes	Yes	Yes
Balancing Weights	No	Yes	No	Yes

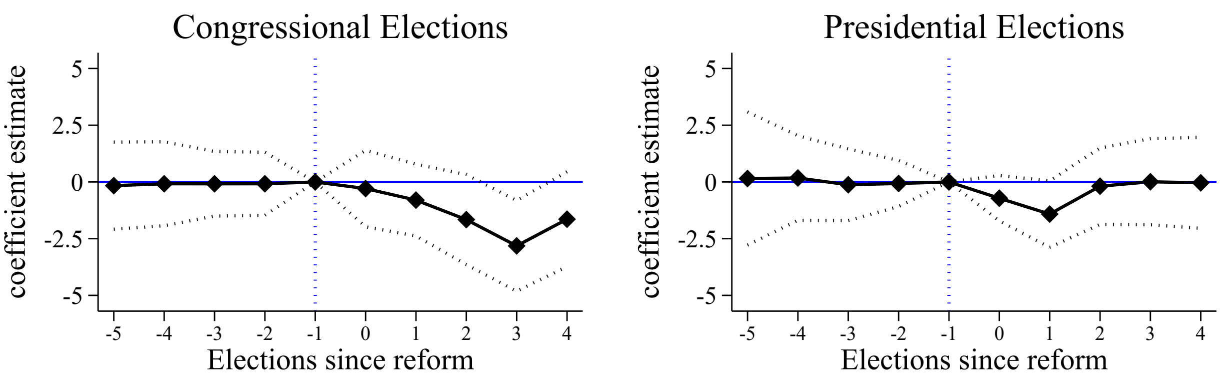
Notes: Reproduces results shown in Table A.10restricting the sample to cities that do not share the county with any other city in our sample. The mean and standard deviation of the unweighted dependent variable are shown in column (1) and column (3) of the table above. The mean and standard deviation of the weighted dependent variable are shown in column (2) and (4) of the table above.

Figure A.6: Event Study Estimates Restricting Sample to counties in Earnings Results Sample



Notes: Reproduces estimates shown in Figure 5 restricting the sample to counties included in our main results shown in Table 1 and Figure A.1. See Figure 5 for additional figure notes.

Figure A.7: Event Study Estimates Restricting Sample to One-City Counties



Notes: Reproduces estimates shown in Figure 5 restricting the sample to cities that do not share the county with any other city in our sample. See Figure 5 for additional figure notes.