Cities without Suburbs? The Effects of Municipal Scale on Local Public Finance *

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Abstract

Between 1945 and 1990, the average central city in an American metropolitan area grew its land area four-fold. We exploit sharp annexation events in a difference-in-differences design to study how local expenditures and public good provision evolved with these increases in municipal scale. Annexations increased the municipal population served by 37% and lowered the average costs of service provision for the central city. While cities spent to accommodate new constituents, spending increased proportionally less than the increase in population, resulting in a 9% decrease in per capita expenditures. The average cost of service provision decreased because of lower costs in outlying suburban areas and capital deepening, not decreases in the quality of service provision. Property tax rates lowered for incumbent residents. Changes in the structure of local public finance attracted economic activity: counties with annexing central cities see higher populations and establishments. By 1990, metro areas that centralized via annexation had lower aggregate expenditures per capita across all jurisdictions. Centralization also resulted in lower levels of segregation by race across jurisdictions.

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I. Introduction

A pressing challenge in post-war American local public finance was "the Metropolitan problem": how to organize local governance, taxation, and public services across increasingly sprawling metropolitan areas (Nolting and Arnold (1952)).[1] For city planners, an attractive solution to the threat of ineffective jurisdictional fragmentation was to create a single central authority by expanding the boundaries of the central municipality (Bollens (1956), Rusk (2013)).

City planners initiated a spree of municipal annexations between 1945 and 1990 where the average central city of a metro area grew its land area four-fold. However, because state laws differed, the ability to annex varied widely across the country. Two cities with similar economic incentives to expand could diverge sharply in eventual size: despite Cincinnati and Oklahoma City both residing in counties of roughly 800,000 people today, Cincinnati's city limits cover just 310,000 residents while Oklahoma City's covers 681,000.[2]

Whether municipal services are more effectively provided by large central municipalities like Oklahoma City's or compact fragmented ones like Cincinatti's is theoretically ambiguous: centralization may be more efficient if there are significant economies of scale or spillovers in public good provision, while decentralization may be preferable if there is significant heterogeneity in preferences for public goods (Alesina and Spolaore 1997, Tiebout 1956, Besley and Coate 2003, Ostrom, Tiebout, and Warren (1961)). Because it is difficult to find variation in jurisdictional scale for large urban areas, there have been relatively few empirical estimates of the consequences of metropolitan jurisdictional scale on public good provision.[3]

This paper studies the effects of large increases in municipal scale on the operation of city

[1]The "Metropolitan problem" was the subject of reports commissioned by the Governor's conference, and of specific interest to Congress in the Kestenbaum commission of 1955 and the newly created Advisory Commission on Intergovernmental Relations in 1959.

[2]Populations from the 2020 Census. The difference comes from land area, not density: Cincinnati covers 72 square miles, Oklahoma City 621. The Cincinnati MSA is slightly larger at 2.2 million people, to Oklahoma City's 1.5 million.

[3]Recent evidence on jurisdictional scale has come from villages and towns in France and India (Tricaud 2021, Narasimhan and Weaver 2024) or counties in Brazil (Dahis and Szerman 2024).

governments in large metropolitan areas. We examine how scale affects cities' populations, budgets, and tax rates, as well as their provision of key services (i.e., fire protection, policing, sewerage, and road maintenance). Doing so requires observing the operations of city governments annually over four decades. From surveys of city managers, Moody's municipal manuals, and a variety of Census publications, we assemble an annual panel that follows 237 large US municipalities through the annexation wave. To study how cities changed within their original 1950 borders, we also reconstruct historical city limits and link them to decennial Census outcomes between 1950 to 1980. To our knowledge, the result is the most comprehensive dataset covering the structure and operations of US municipalities in the post-War period.

These data allow us to document three new facts about the post-war annexation wave. First, the decline in urban cores was near universal in post-War period: between 1950 and 1980, annexing and non-annexing cities alike lost population, had real income growth far below the national average, and became substantially less white within their original borders. Second, most annexations absorbed whiter, wealthier areas, significantly altering the demographic composition of jurisdictions; and third, state-laws substantially reduced the feasibility of annexations in certain regions. The *average* Southern city added more than the entire land area of Detroit to its boundaries, while growth in Northeastern cities was virtually non-existent. As a result, the share of metropolitan residents living in the central city halved for non-annexing cities in the North and Midwest but stayed constant in places with large annexations.

To estimate the effects of annexations, we exploit the sharp, episodic nature of the expansions in a staggered difference-in-difference design. We compare cities that annexed in short, concentrated bursts to similar-sized cities in the same region that did not annex or had not yet annexed. Matching on size and region means the two groups faced comparable economic conditions. The main threat to this comparison is that annexation is a choice: cities may annex precisely when their metropolitan areas are suburbanizing or their economies are

booming. Two features of the setting limit this concern. First, whether a city could annex depended on the permissiveness of state law and the availability of unincorporated land at its borders, so our comparison cities include many that likely wanted to expand but could not.[4] Second, cities had weak control over the exact timing of annexation: assembling a plan and the political support to execute it took years, and even in permissive states, ordinances were often delayed, challenged in court, or rejected.[5] Because our data are annual, we can separate the sharp changes that follow these discrete jumps in city size from slow-moving differences between annexing and non-annexing cities.

The average annexation in our sample increases a city's population by 37% and decreases spending per resident by 10%. This decline in the average cost of service provision arises because total spending increases after annexation, but proportionally less than population. Cities run balanced budgets so revenues per resident also fall 10%, and because assessed property values per resident stay steady, the smaller budget translates to lower property tax rates. Changes in average costs differ sharply across types of expenditure: we observe 16% and 13% declines in fire and policing expenditures per capita, while sanitation and sewerage increase in per capita terms.

Using detailed annual surveys of police and fire department operations, we find evidence that annexing cities lowered average costs by substituting equipment for personnel and by adding territory that was cheaper to serve, not by cutting service provision. After annexing, cities operated more fire apparatus (i.e., engines, ladder trucks) per firefighter, and within their fleets shifted towards engines, the larger more capital-intensive of the two.[6] Annexed territory itself also lowered average costs: less dense suburbs were cheaper to serve than the urban core, so expanding cities added residents whose service costs fell below what

[4]In practice, almost all annexations in the 20th century in the US involved the absorption of unincorporated land. The reasons and implications of this are discussed in full in Section II.

[5]For example, Norfolk, VA passed an annexation ordinance in 1952 that did not clear the courts until 1958. Atlanta tried to expand unsuccessfully three times between 1947 and 1951, until the state legislature intervened.

[6]The National Board of Fire Underwriters declares engines preferable to trucks in almost every case for Urban municipalities.

they paid in. Service quality, by contrast, did not fall: cities exhibit no increase in total deficiency points recorded by fire insurance underwriters. [7] Taken together, these patterns reflect capital intensification and cross-subsidization from lower-cost areas, suggesting that annexation significantly altered the costs municipal residents faced.

Advocates for annexations argued that they would attract economic activity by improving local service provision. We find evidence for these claims: while treated counties are on similar trajectories until the year of the annexation, they see a 5% increase in total establishment counts after an annexation event. We also see an increase in births, which we consider as a suggestive proxy for population under the strong assumptions that annexations did not produce effects on birth rates. Because annexations are bundled treatments, both increasing service coverage and changing average costs in fixed areas, we cannot disentangle the exact mechanisms of annexations effects on economic activity: however, we can conclude that jurisdictional districting played a role in allocating economic activity in the post-War. Finally, because a large share of annexing municipalities were in the Sun-Belt, we posit that annexations may have played a role in the rise of the Sun-Belt.

While annexations result in more centralized municipalities, it is difficult to speak to the implications of this centralization by comparing to non-annexing counties at a given point time. To do so, we would require comparable counties that develop the same amount of land through new incorporation instead of annexation.

To overcome the limitation, we construct an instrument for central city area growth: the share of land within a five-kilometer radius of a city's 1950 boundaries that a city could possibly expand into. The intuition for this instrument is that even within similar policy regimes, certain cities were limited by their relative position to obstacles in the form of geographic features and other municipalities.[8] Importantly, the instrument retains predic-

[7] We do observe modest increases in the sub-category of fire department deficiency points that are compensated by gains in other categories. These other categories include fire department services like alarm systems and inspections.

[8] The spirit of this variation—the relative position of geographic features—is similar to the instrument in Harari (2020), although we do not follow exactly the same process.

tive power conditional on the presence of other municipalities and geographic features (i.e. coasts), suggesting that the variation we exploit is not whether a city has obstacles around it, but *where* the obstacles are located relative to the cities 1950 borders. This instrument complements our difference-in-difference estimates by identifying variation in annexations that could not be explained by endogenous timing.

This instrument identifies long-run variation in central city annexation between 1950 and 1980 and by proxy the extent of centralization in local governance: within Census regions, cities that were more hemmed in by the relative position of obstacles in 1950 ultimately expand less. In turn, counties where central cities expand less see more new incorporations, such that *total* municipal land area does not differ. In other words, we isolate variation in municipal fragmentation via variation in the relative position of obstacles that blocked certain central cities from expanding at the start of the annexation wave.

Our instrument confirms our city-level results, in particular, for fire protection. Central cities that are freer to expand report lower growth in per capita fire spending between 1957 and 1977. However, these effects attenuate when looking at total spending in the fixed area, reinforcing that annexations lower costs for the central city through composition. We also find aggregate decreases for total general expenditures per capita across the fixed county area, suggesting that while fire-protection may not directly benefit from centralization, there were returns to centralization across all services.

Our results speak to several strands of literature that examine the effects of jurisdictional boundaries on public sector operations and economic activity. Recent estimates have found local gains from political decentralization in developing countries (e.g. Narasimhan and Weaver 2024; Dahis and Szerman 2024). Others have considered the efficiency effects of decentralized public goods provision (Bordeu 2023; Hoxby 2000; Lipscomb and Mobarak (2017)). Empirical estimates have found mixed evidence on returns to scale when several adjacent municipalities co-ordinate service provision (Tricaud (2021); Reingewertz (2012); Blesse and Baskaran (2016); Cobban (2019); Frere, Leprince, and Paty (2014); Blom-Hansen

et al. (2016); Allers and Greef (2018); Luca and Modrego (2021)). Our estimates suggest that centralization resulted in efficiency gains for cities. This result differs from existing estimates for several reasons: first, we study the central cities of metropolitan areas, who may provide services at a different scale than jurisdictions, like villages or towns (Narasimhan and Weaver 2024; Tricaud 2021) or counties (Dahis and Szerman 2024). Second, our results are partly driven by the context of local public finance in the United States: land that was annexed often did not belong to any municipality and did not receive many of the services provided by cities. In this respect, the treatment effect that we estimate is not necessarily the converse of splitting a larger municipality into separate municipalities (Dahis and Szerman 2024; Narasimhan and Weaver 2024).

This paper also extends work on the historical development of cities and municipal services in the United States by focusing on the response of cities to the post-War suburbanization wave. Prior work has documented the productivity advantages of cities driven by agglomeration forces (Bleakley and Lin 2012, Hall, Matti, and Zhou 2020, Rosenthal and Strange 2008), and the municipal services necessary to resolve the urban externalities that arise from this agglomeration (Glaeser 2013, Geddes and Troesken 2003, Ferrie and Troesken 2008, Cutler and G. Miller 2005 Anderson, Charles, and Rees 2022). This paper extends this work by examining a regional divergence in municipal scope that took place after city governments had already introduced municipal police, fire protection, water, sewage, and waste management, and asks how this divergence in municipal scale affected the provision of these services.

This paper proceeds as follows: in Section II., we describe in further detail the institutions and rules that governed annexation in this period and present some stylized descriptives; Section III. explains our data sources; Section V. describes the empirical design; Section VI. provides our main estimates; and Section VIII. concludes and poses future avenues to expand the work in this paper.

II. Background

> "*All the rights, privileges and equal consideration under the city government automatically become yours with the New Year.*
>
> [. . .]
>
> *Your soon-to-come role as city residents is the result of many natural factors and a judicial process prescribed by the annexation laws of our State. Experience shows that cities must be allowed to grow. Economy factors and costs of services essential to urban living all are involved in such essential moves.*"[9]

A central challenge of post-war municipal finance was "the Metropolitan problem": the question of how to structure local governance, taxation, and public service provision in increasingly sprawling and potentially fragmented metropolitan areas (Nolting and Arnold (1952)). The fear of a metropolitan area composed of many small municipalities was one of duplicated costs, limited returns to scale, coordination issues and inequality induced by strategic sorting across jurisdictions. One solution to such a problem is to liberally allow for the expansion of municipal boundaries through annexation or consolidation. Annexation had been a popular policy in the late 19th and early 20th centuries: Philadelphia consolidated to its county borders in 1854, Manhattan and the Bronx joined with the three remaining boroughs of Brooklyn, Queens, and Staten Island in 1898, Los Angeles became the largest city in the United States in land area through annexation in the 1920s. However, cities, at various points, encountered resistance to annexations, halting their expansions outward: Boston was rebuffed by Brookline in 1874, Chicago by Evanston in 1894, and Los Angeles by Beverly Hills in 1923.

As the Supreme Court would affirm in its decision in *Hunter v. Pittsburgh (1907)*, a city's ability to annex places without their approval was entirely a decision of the state legislature (Shoked (2023)). These state-level rules were rules regarding two factors: (1) the legal ability of existing municipalities to expand their boundaries into surrounding territory; and (2) the ease by which surrounding areas could incorporate into municipalities of their own and thereby protect them-

[9]Excerpted from a letter to annexed residents by Norfolk City Council President, W. Fred Duckworth [tk] citation.

selves from annexation. While states had initially been willing to allow unilateral annexation (i.e. annexation without the approval of a majority in the *annexed* area), the string of wealthy suburbs resisting annexation by central cities that began with Brookline in 1874 slowly shifted states, particularly in the Northeast and Midwest, towards severely restricting the annexation powers of cities (Shoked (2023)).

In their review of state annexation laws, Dixon and Kerstetter (1959) survey the variety of annexation rules that had been set in place by the mid-20th century: in Texas, municipalities had the right to *unilaterally* annex territory into the city; in six states, cities had no legal annexation method; in most other states, municipalities were only allowed to expand into territory with approval from a majority of residents in the *annexed* territory. The necessity for approval in the annexed territory in particular drew the ire of urban and municipal planners who viewed the policy as privileging the welfare of a select population over potential gains for the entire population (Rusk (2013)). As an example of these skewed incentives, we can look at Boston's failed 1874 annexation of Brookline: 6,291 of 7,775 voters in Boston approved Brookline's annexation, only 292 of 1,006 voters in Brookline approved the annexation ("Annexation: Result of Yesterday's Voting" (1873)). Annexation's proponents argued that Brookline's vote reflected the preferences of a much smaller population than the city of Boston, whose population would have stood to gain from the annexation.

When annexation resurged as a policy tool, after a lull during the Great Depression and Second World War, it emerged into the policy context that had been shaped by annexation debates earlier in the 20th century. Therefore, only city leadership in certain states found themselves capable of annexing. Nevertheless, there was a resurgence in the popularity of annexation. In 1948, 145 square miles were annexed into American cities; by 1960, roughly 1,000 square miles were being annexed annually (Nolting and Arnold (1965)). This "boom" was however, not evenly distributed across the country, although there were several possible pathways to expansion: Texan cities, with their legal right to unilateral annexation, expanded swiftly and liberally; cities elsewhere triumphed in the face of more restrictive laws, as was the case with Columbus, whose pro-annexation mayor leveraged the politics of access to the water to convince surrounding territory to fold into the city; and finally certain cities benefited from legal intervention from the state legislatures—as was the

case with Atlanta's 1952 annexation.

This paper focuses on the specific post-war period in the history of annexation in the United States for two reasons. The first is practical: data on municipal finance and boundary changes become readily available only in the post-War period. The second is that, as argued above, this period reflects an important period for annexation as a policy in the United States: as most cities faced large suburbanization shocks, only certain cities were able to expand outward to continue to incorporate the suburban tax base. In fact, while annexation laws remained liberal in certain states until the 2010s, annexations never achieved a similar popularity: in terms of magnitude, the annexation shocks of the immediate post-war period, with cities adding dozens and sometimes hundreds of square miles in the span of a couple years, would remain significantly larger than most later annexations.

III. Data

To evaluate the impact of the annexation wave on the operation of local governments, we construct a newly digitized panel of land area, population, municipal expenditures, service provision, taxes, and economic activity for 237 large US municipalities. We digitize these data from a range of historical sources, each described below. To track municipal characteristics over fixed geographies, we construct a second dataset that links geospatial data on time-varying city boundaries to Census demographics, 1950–1980. To our knowledge, these data form the most comprehensive dataset covering the structure and operations of U.S. municipalities in the post-War period.

A. Annual Municipal Annexation and Population City Panel

Annexations Our annexation data come primarily from the Municipal Year Book.[10] From 1948 to 1968, during the postwar resurgence of annexations, the Municipal Year Book surveyed cities on their annexation activity, recording the square mileage and estimated population of each annexed area.[11]

From 1970 onward, we use the annual Boundary and Annexation Survey (BAS), which records

[10]Published annually since 1934 by the International City/County Management Association.

[11]In most years nearly all cities responded; the Municipal Year Book notes any incomplete information.

annexed square mileage but not population. We recover an estimate of annexed populations from the Census Bureau's Current Population Reports for 1975 and 1977.[12]

Finally, using data from Municipal Yearbooks, Decennial Censuses, and the BAS, we have cross-sectional data on total land area of cities in 1934, 1944, 1950, 1960, 1970, and 1980. We use this cross-section to identify cities for which we miss significant land changes, and use archival records and newspapers to estimate the timing and size of these missing annexations.[13]

A distinct source of municipal land growth is city-county consolidation: a city's borders expand to the county line and the two governments merge. We include the eight consolidations that occurred between 1944 and 1980: Newport News, Nashville, Virginia Beach, Jacksonville, Indianapolis, Columbus (GA), Lexington, and Suffolk.

The final panel contains 137 cities that annexed land between 1944 and 1980. The median city added 27.8 square miles, roughly doubling its land area. In total, these cities added more than 11,500 square miles over the period, with annexation peaking in 1963, when annexing cities collectively added 847 square miles.

Annual Population City populations are recorded only intermittently, so we impute the intervening annual series with a geometric growth model that accommodates the discrete jumps from annexation. We assume:

$$\text{pop}_t = (1 + \alpha_{t-1}) \cdot \text{pop}_{t-1} + \text{popannex}_t \tag{1}$$

so that a city's population at the end of year t equals the previous year's population grown at rate α_{t-1}, plus any population annexed during year t.

We anchor the series on observed populations from the Decennial Censuses (1940–1990), the 1965 Municipal Year Book, the 1975 City Data Book, and hand-collected figures for individual

[12]These reports give each city's total population under its 1975 and 1977 boundaries. Comparing them to the 1970 Decennial Census, we infer the population annexed between 1970 and 1975 and 1975 to 1977.

[13]Missing annexations stem from two sources. First, we lack coverage for certain years: 1944-1948 and 1968-1969. Second, a few large cities were either not respondents in the annual survey or provided incomplete information in some years. We have added supplementary annexation data for eight cities using archival records and newspapers.

cities, including special censuses.[14] From the annexation panel, we also know the population added to each city in each year. For each city, we then estimate the growth-rate path α_t that best fits these anchors—minimizing squared error in logs—subject to a penalty on year-to-year changes in the growth rate. The penalty is relaxed in annexation years, so the estimated series follows a smooth growth path punctuated by discrete jumps when a city adds territory.

City Sample Selection We restrict our sample to all cities with a population above 75,000 in 1990, comprising 296 cities. We further restrict our sample to cities with a population of at least 5,000 in 1940 as this is the minimum population to be included in the earliest Municipal Yearbook Surveys, corresponding to 237 final cities.

B. Municipal Boundary Geospatial Data

We complement our annual panel of annexations with geospatial data on city boundaries over time. We combine information about municipal boundaries from multiple sources to construct a long-run (1950–1980), decadal panel of municipal boundary shapefiles. Appendix X describes each source and the harmonization procedure in more detail.

Reconstituting Place Boundaries from Tract Data We use the subset of cities that were tracted in 1950 and 1960 to identify city boundaries in those decades. In 1950, 64 cities were tracted; in 1960, 175 cities were tracted. For cities with available tracts, we use information in the tract name file to identify which tracts fall within and outside formal boundaries and match this information with geospatial tract data published by NHGIS to recover the municipal boundaries as they existed in each decade.

For cities tracted in 1960 but not 1950, we can still recover their 1950 boundaries using a Census report that lists tracts for 29 large cities (population greater than 250,000 in 1960) that added at least 0.5% of their area through annexation (A. R. Miller and Varon 1961, A. R. Miller and Varon 1962).

[14] These special censuses are published as part of the subsequent Decennial Census.

1980 Place Shapefile

Starting in 1980, geospatial files of municipal boundaries are available for all legally incorporated municipalities through the Census's Boundary and Annexation Survey (BAS), accessed via NHGIS.

Individual City Boundaries

We additionally collect disaggregated annexation geospatial records for cities that make historical boundary change records publicly available. The complete set of cities with disaggregated spatial boundary change data is reported in Appendix X.

Summary of Constructed Datasets

We use these data sources to construct two datasets that allow us to track how the composition and population cities defined under tsable boundaries changed over time.

The first dataset contains information on municipal boundaries in 1950 and 1980, which also allows us to identify the area annexed in that intervening decades. To construct this, we use 1950 and 1980 boundaries for cities that were tracted in 1950 and intersect those boundaries with 1950 and 1980 tracts. This information allows us to identify characteristics of the municipal core as it existed in 1950 in both decades, and for a smaller subset of cities, the characteristics of areas not yet annexed as they existed in 1950.

The second dataset is a decadal panel covering large annexers that underwent concentrated annexations. For this dataset, we combine information from all sources to divide each city's urban area into three parts: (i) the core as it existed in the decade prior to annexation, (ii) the area annexed into the city, and (iii) land not annexed into the city. We define the urban area as all land tracted in the decade prior to annexation.

C. Outcome Data

We pair the annexation and population panel with a range of outcomes covering the fiscal operations, service provision, and economic activity of American cities, most spanning 1947 to 1980. Unless noted, all nominal dollar figures are deflated to 2019 dollars using the CPI-U.

City Government Finances We use annual data on municipal spending and revenue from the Census Bureau's Annual Survey of Governments for 1951 onward, which reports revenues and expenditures by category, functional spending by service (including police, fire, sanitation, parks, and highways), and city debt. We extend the series back to 1947 with newly digitized records from an earlier Census survey of all cities with populations above 25,000 in 1940.[15]

City Government Employment We measure municipal employment and payroll using the Census Bureau's City Government Employment survey, available annually from 1952. The survey reports employment, payroll, and implied wages by government function, which we use to separate labor inputs across services.

Quality of Service Provision. To look beyond fiscal outcomes, we assemble measures of the quality and intensity of municipal service provison.

Fire Departments. From the Municipal Year Books, we digitize annual surveys of city fire departments covering 1950 through 1970. The surveys record staffing, salaries, and apparatus (engines and ladder and aerial trucks), along with building fires and two ratings issued by the National Board of Fire Underwriters: total deficiency points, scored across 124 categories from 0 to 5,000, and the resulting fire insurance class.

Police Departments. From the same Municipal Year Book surveys, we digitize measures of police staffing, salaries, hours, and patrol equipment over the same period (1950–1970)

Crime. We measure additional information on the performance of police departments using crime data from the FBI's Uniform Crime Reports. Reports from 1960 onward are drawn from Kaplan 2021; we hand-digitize the 1953–1959 reports, which cover fewer offenses and omit arrests. We focus on offense rates per 1,000 residents for murder (including manslaughter), burglary, and auto theft, and on clearance rates where available.

Property Tax Rates and Total Assessed Valuations We record municipal tax rates, assessed

[15]The earlier survey reports a narrower set of variables for fewer cities. We validate the two sources against each other in their overlap years, 1951–1952.

valuations, and assessment ratios from Moody's Municipal Manuals, which begin in 1954. Compiled as guides for municipal-bond investors, the manuals report local public finance for every municipality, and we digitize these pages for each city in our sample. The recorded rate is the nominal rate, in mills per dollar of assessed value, which differs from the effective rate on a property's market value. Because cities occasionally adjust assessment ratios and nominal rates in equal and offsetting steps, leaving the effective rate unchanged, we also compute the effective rate as the ratio of property-tax revenue to total assessed valuations.

City Economic and Housing Characteristics From the County and City Data Book (ICPSR 7735), we use city-level outcomes at census and intercensal benchmark years: median family income, median gross rent and house value, new housing units authorized by permit, retail and service establishments and sales, manufacturing value added, bank deposits, and population density.

Demographic Composition We characterize each city's population using the decennial Censuses. For 1940 and 1950 we use the full-count Census; for 1960 through 1990 we use publicly available tract-level, place-level, and county-level aggregates. These data describe the age, race, nativity, and income composition of city residents. For the subset of cities with complete tract coverage and measured historical boundaries, we can also track these characteristics across areas within a city over time.

Local Economic Activity We measure economic activity using annual County Business Patterns data, 1946–1990 (Eckert, Fort, et al. 2021; Eckert, Lam, et al. 2022), which report employment and establishment counts by industry for every county. We focus on total, manufacturing, and service employment and on total establishments. We match counties to cities using each city's home county, and drop the small number of cities spanning multiple counties (e.g., New York City) from county-level analysis. We pair these with annual county populations, which we interpolate between decennial censuses using the timing of within-decade state population changes.

IV. Descriptive Facts on Post-War Annexation

The postwar annexation wave was a popular but regionally concentrated policy that drove a divergence in the scale of municipal governance across American metropolitan areas.

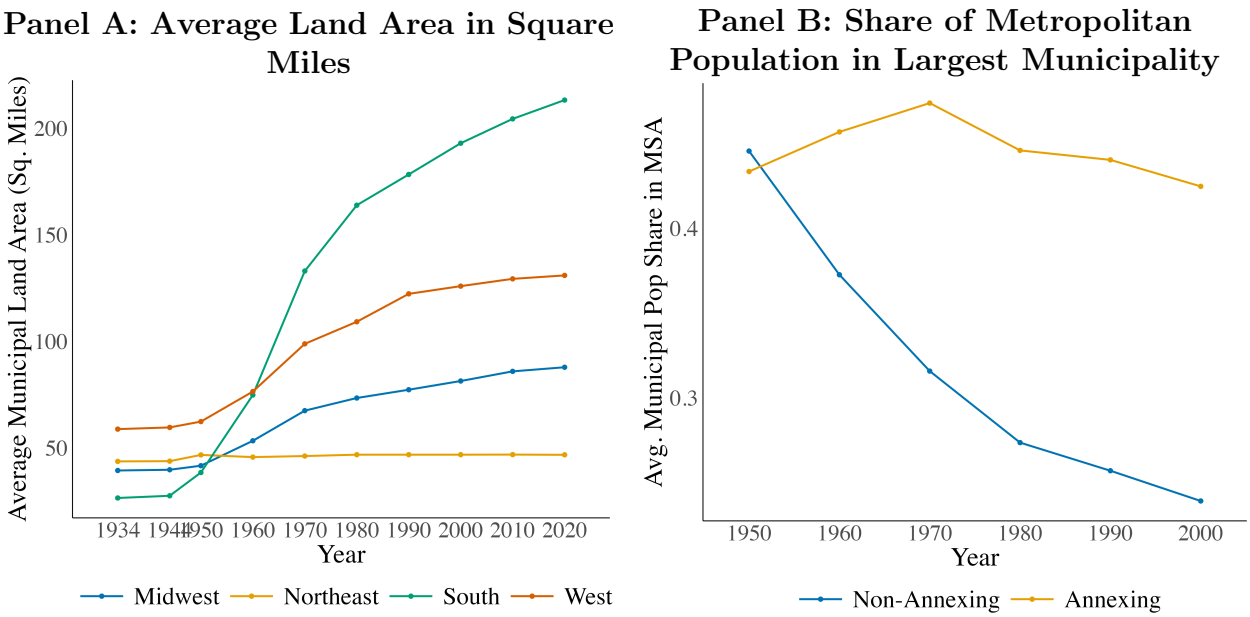
Using our long-run panel of municipal land areas from 1934 to 2020, we document a marked increase in annexation activity between 1950 and 1990, following little activity in the preceding decade and a half. This rise was, however, regionally uneven. Panel A of Figure 1 shows a sharp regional divergence in municipal land area, with rapid growth among Southern cities and virtually none among Northeastern cities. This divergence reflects differences not in suburban pressure on city finances, which was nationwide, but in the legal capacity to respond: aided by permissive state laws, nearly every large Southern city expanded, while Northeastern cities largely could not. Midwestern and Western cities grew, but more modestly; as we show later, their lower averages reflect a more idiosyncratic pattern in which only some cities expanded.

Panel B reports the central city's share of metropolitan population. In MSAs whose central city annexed, this share held steady between 1950 and 2000; where the city did not annex, it fell by half. The two groups were similar in 1950—roughly 45% of metropolitan population in the central city—but diverged as annexation took off. This divergence is largely driven by jurisdictional changes: annexing cities held their share by extending their boundaries to encompass dispersing population, not by retaining residents within a fixed boundary. We take up the downstream fiscal and operational consequences of these larger jurisdictions in the main analysis.

Table 1 describes the areas annexed between 1950 and 1980 as they looked in 1950, before annexation, for 13 cities.[16] The annexed areas were far less densely populated than the central cities, though this gap varied widely: the land Seattle annexed was about 15 times denser than the land Houston annexed. They were also almost always whiter than the cores they joined—by 8 percentage points at the mean, and in every city but Houston—so annexation shifted the racial composition of cities. This absorption of white residents likely reflects two forces: postwar suburbs were disproportionately white as a result of white flight, and in some cases cities deliberately

[16]The 13 cities are those whose core and urban fringe were both covered by census tracts in 1950 and that annexed at least 5,000 residents.

Figure 1: Municipal Annexation by Regions



NOTES: Panel A shows regional differences in average land area in square miles of cities over time. Panel B shows the share of a metropolitan area's population contained within its largest municipality for annexing and non-annexing cities. The sample includes the a balanced panel of the most populous cities within each MSA in 1990, where the city has a 1990 population of at least 75,000 and an MSA population of at least 200,000. This corresponds to 115 annexing cities/MSAs and 36 non-annexing cities/MSAs. Sources: Municipal Year Book (1934-1960), Boundary and Annexation Survey (1970-2020), and Decennial Censuses.

Table 1: 1950 City Core vs. Annexed Territory Characteristics

City	City Core			Annexed Territory			
	Pop.	Density (per sq mi)	Share White	Pop.	Density (per sq mi)	Share White	Med. Inc. Ratio
Atlanta, GA	331,314	9,348	0.63	96,461	1,192	0.84	1.50
Chattanooga, TN	131,041	4,631	0.70	18,303	761	0.95	1.42
Columbus, OH	375,901	9,618	0.87	33,343	646	0.90	0.99
Dayton, OH	243,872	9,519	0.86	20,377	2,138	0.93	1.00
Greensboro, NC	74,389	4,099	0.74	15,024	656	0.90	1.17
Houston, TX	596,163	3,873	0.79	35,583	224	0.67	0.84
Indianapolis, IN	427,173	7,851	0.85	82,364	673	0.99	0.99
Kalamazoo, MI	57,704	6,477	0.96	12,199	1,351	1.00	1.41
Milwaukee, WI	637,392	12,542	0.96	16,061	577	1.00	0.91
Norfolk, VA	213,513	6,889	0.70	46,995	2,013	0.92	1.25
Sacramento, CA	137,572	7,654	0.92	45,354	1,162	0.94	0.90
San Diego, CA	334,387	3,092	0.95	11,738	379	0.99	0.88
Seattle, WA	467,591	6,903	0.94	54,631	3,371	1.00	1.14
Mean	309,847	7,115	0.84	37,572	1,165	0.92	1.11

Notes: This table compares the 1950 characteristics of the city core with territories annexed by 1980. Density measured in people per square mile. Med. Inc. Ratio shows annexed median income divided by core median income. Only cities with annexed population $\geq$ 5,000 are included.

annexed white suburbs to preserve a majority-white electorate.[17] Annexed areas were also higher-earning, though less consistently so: median family income averaged 11% above the central city's, but in many cases sat at parity or slightly below.

In Table 2, we follow each city's 1950 geographic core through to 1980, fixing boundaries at their 1950 extent for the 87 cities covered by census tracts that year. Holding the footprint constant lets us compare changes within the same boundaries, rather than changes driven by the land and residents annexation pulled in. Within these fixed cores, both groups lost population—17% among non-annexers and 7% among annexers—showing that urban flight was widespread rather than confined to the Northeast, a decline that citywide population counts obscure.[18] The white share of the core fell by about a quarter in both groups. Real family income in the core grew at similar rates in both groups, and in both fell short of the national average: annexing cities were no better off

[17] An explicit example is Atlanta's 1952 Plan of Improvement," which tripled the city's land area and added roughly 100,000 overwhelmingly white residents; Mayor William B. Hartsfield had earlier framed such expansion as a way to keep the city's growing Black population from gaining political control of Atlanta." See Larry Felton Johnson, "The 1952 Atlanta Annexations," *Cobb County Courier*, May 20, 2016, `https://cobbcountycourier.com/2016/05/the-1952-atlanta-annexations/`.

[18] For non-annexing cities, the 1980 stable-core and total figures differ slightly because of small annexations below our 5-square-mile threshold for classification as an annexer.

Table 2: Characteristics of Annexing vs Non-Annexing Cities, 1950 to 1980

	1950 Boundaries			1980 Boundaries	
	1950	Stable Core 1980	% Change Stable Core	Annexed	Total
Panel A: Population					
Non-Annexing	496,324	411,516	-17.09%		413,290
	(1,149,436)	(1,009,827)			(1,009,245)
Annexing	409,086	382,387	-6.53%	138,448	520,835
	(626,899)	(642,207)		(172,572)	(660,628)
Panel B: Share White					
Non-Annexing	0.93	0.69	-25.64%		0.69
	(0.08)	(0.22)			(0.22)
Annexing	0.85	0.62	-26.62%	0.79	0.66
	(0.12)	(0.17)		(0.15)	(0.16)
Panel C: Median Family Income					
Non-Annexing	10,963	15,271	39.30%		15,418
	(1,570)	(3,360)			(3,433)
Annexing	10,052	13,643	35.72%	19,952	15,533
	(1,400)	(2,240)		(3,646)	(2,391)
Panel D: Housing Stock					
Non-Annexing	149,938	170,774	13.90%		171,391
	(351,594)	(420,435)			(420,233)
Annexing	129,718	159,774	23.17%	54,262	214,035
	(198,674)	(254,133)		(68,806)	(263,032)
Panel E: Median Home Value					
Non-Annexing	28,421	50,565	77.91%		50,798
	(8,541)	(24,189)			(24,411)
Annexing	25,637	45,305	76.71%	60,309	48,610
	(3,651)	(22,575)		(25,698)	(21,973)
N	Non-Annexing: 50, Annexing: 37				

Notes: Standard deviations in parentheses. Percent changes calculated as the change in group means from 1950 to 1980 stable core. Annexed areas not applicable for non-annexing cities.

economically than non-annexers, and neither kept pace with the country.[19] By 1980, annexed areas made up more than a quarter of annexing cities' population, and that population was substantially whiter (17 percentage points) and richer (46%) than the core.

Together, these facts describe a divergence in the scale and composition of American central cities: as metropolitan populations dispersed, some cities used annexation to extend their boundaries and capture population that others lost to the suburbs. While the scale of this expansion and the divergence across regions suggest the period mattered for how city governments operated, simple long-run comparisons cannot isolate annexation's effect: they cannot tell us how a city like Houston would have fared within its 1950 boundaries had it not annexed. To separate the effects of annexation from the broader regional trends affecting all cities, we turn to the impact of sharp annexation events.

V. Empirical Strategy

We estimate the causal impact of jurisdictional expansions on municipal finance, public good provision, and local economic activity. Using a staggered difference-in-differences design, we track how a city's outcomes evolve in the years before and after it annexes, relative to non-annexing cities in the same Census region and year. The estimating equation is:

$$Y_{it} = \alpha_i + \gamma_{r(i),t} + \sum_{\substack{k=-a \\ k\neq -1}}^{b} \beta_k \, \mathbf{1}[K_{it} = k] + \epsilon_{it}. \tag{2}$$

where Y_{it} is the outcome for city i in year t, α_i and $\gamma_{r(i),t}$ are city and region-by-year fixed effects, and the $\mathbf{1}[K_{it} = k]$ indicate years relative to a city's annexation ($K_{it} = t - E_i$), with non-annexing cities serving as controls. We omit $k = -1$ and estimate the β_k using the imputation estimator of CITE HERE.

We opt for this design for several reasons. First, we argue that the exact time of the annexation was exogenous, determined by when legal challenges cleared the courts, as opposed to immediate changes in incentives the cities. For example, Norfolk initiated its annexation in 1952 but the

[19] TODO: source the below-national-average claim — need national real median family income growth, 1950–1980, or add a national benchmark row to the table.

decision was held up in the courts until 1958; Atlanta failed 3 votes between 1947 and 1951, until the State Legislature intervention in 1952; North Carolina did not expand annexation powers until 1957 [tk citations]. Second, we restrict to sharp events for which changes can easily be attributed to the annexation in a specific. Third, we prefer the within region design to avoid colinearity with post-war regional convergence: that is our variation relies on comparing places within the South or Mid-West that for idiosyncratic legal reasons were able to annex in a given year to those that did not.

Therefore, in our sample, a treatment cities is one undergoing a sharp, large annexation spurt lasting up to five years. Given data availability, we focus on annexation spurts that start between 1952 and 1972 and add at least ten square miles and annex a population constituting at least 5% population growth.[20] To ensure the spurt represents a sharp event with negligible additional annexation outside of it, we require that over 50% of land annexed between 15 years prior and 10 years after occurred within the spurt, and over 85% of land annexed between 10 years prior and 5 years after occurred within the spurt.[21] Appendix **??** provides additional details on sample selection criteria. These criteria yield a sample of 38 treatment cities. Figure **??** Panel A displays the sample, plotting total population annexed against total land annexed for each city.

Figure **??** Panel B shows a map of all annexing and non-annexing cities. Treatment and control cities are not geographically random, which is unsurprising given regional differences in the ability to annex: large annexations were most common in the South, and X [tk] of our 47 treatment cities are southern. Since annexation in the Northeast was extremely rare due to policy constraints, we have no treatment cities in the Northeast.

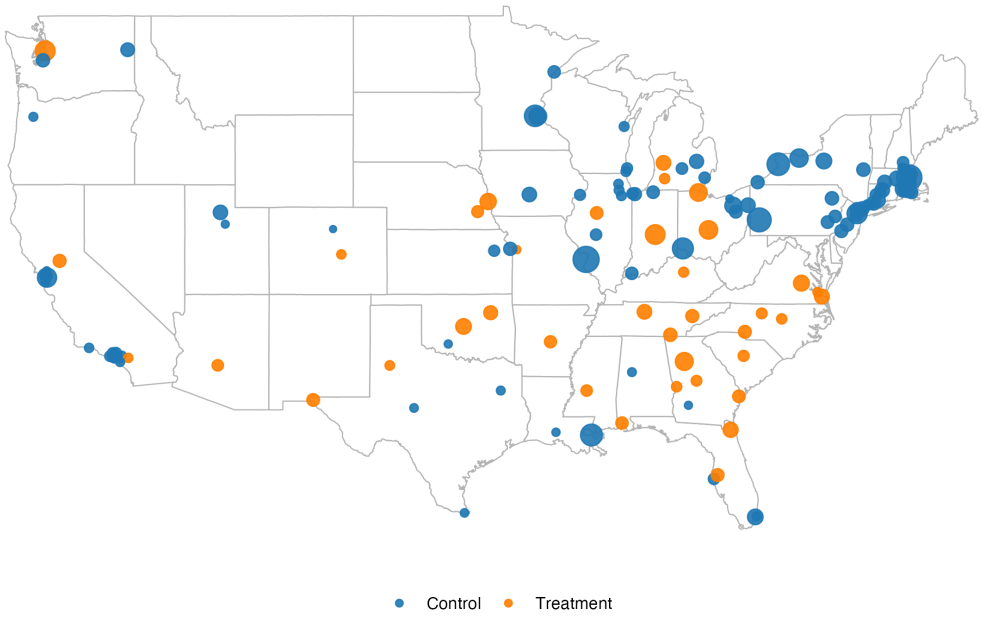
Table **??** compares treatment cities, control cities, and annexing cities ineligible for the treatment sample. Prior to the annexation wave, all three types of cities have similar land areas and populations. Reflecting the regional patterns shown in the map in Figure **??**, treatment cities in

[20] This restriction addresses cases where low population estimates might appear due to measurement of annexed population.

[21] Additionally, between 1944 and 1980, treatment cities must have annexed either at least 15 square miles while growing their borders by at least 20%, or at least 10 square miles while doubling their land area. For cities with multiple eligible spurts, we select the largest by land area. We exclude cities where our annual annexation panel misses more than 50% of total land added between 1944 and 1980, as well as Mesa (for which city finance data unavailable until 1967) and Gainesville (insufficient control cities due to small size at time of annexation).

1950 have higher shares of Black residents, are less manufacturing-intensive, and have smaller immigrant populations than control cities. Control cities also tend to be located in more populous MSAs than annexing cities. Annexing cities tend to be similar regardless of treatment status suggesting that the more restrictive set of cities who annexed rapidly are representative of the broader set. Despite these differences, all three types of cities exhibit similar municipal finance characteristics prior to the annexation wave, with comparable total debt, expenditures, and capital outlays.

Panel A: Map of Treatment and Control Cities



NOTES: This figure shows characteristics of the treatment and control cities used in our empirical analysis. Panel A shows, for the 38 treatment cities in our analysis, the years of the annexation spurt, the total population, and the total land annexed into the city over the spurt. Panel B shows a map of treatment (orange) and control (blue) cities in the stacked sample.

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We also estimate a secondary pooled specification:

$$Y_{it} = \beta^{pool} \left(Post_t \cdot Annex_i \right) + Post_t + \alpha_i + \epsilon_{it} \tag{3}$$

Here, we restrict the sample to five years prior to annexation and five years after the spurt window

[22]The smallest window we allow is $\{-3, 5\}$. When using the smallest window, we restrict the sample to clusters with non-missing data in every year.

($n = 5$ to $n = 9$), requiring only that each cluster have at least one observation before and after annexation. We use this more parsimonious specification to report and compare coefficients across many outcomes and to analyze outcomes that are not recorded annually or are only available for limited time windows.

A. Discussion of Assumptions for Empirical Design

As annexations were policy choices made by city governments and residents, often for strategic reasons, we discuss how standard difference-in-differences assumptions apply in our institutional setting.

First, absent the sharp increase in land area, treatment and control cities in the same cluster must exhibit parallel trends. We report estimates in log levels and per capita terms, so we assume parallel trends in both the growth rates and per capita levels of these outcomes. Parallel trends is partially testable through pre-trend analysis, but our primary concern is whether annexations occurred at moments when policymakers anticipated the city entering a different growth trajectory. Two aspects of the historical context mitigate this concern. First, many control cities were unable to annex due to policy constraints, meaning they often faced similar incentives to annex but operated under different policy environments. Second, while annexation was broadly more popular during high-growth periods, the specific timing of individual annexations was often driven by when city governments could capture sufficient political capital to push the policy through, introducing quasi-random variation in timing. In robustness tests, we restrict attention to a smaller set of treatment clusters where the timing of the annexation was particularly random.

Second, we might be concerned about anticipatory effects in our outcome variables. In particular, if the annexation process unfolds over multiple years or city leaders know they plan to annex, cities may begin adjusting their finances (by changing tax rates) and operations (by shifting spending priorities) in anticipation of the new land area and population entering the city. While some annexations unfolded over multiple years, many—particularly in states with liberal annexation policies—happened very quickly. Additionally, as annexations sometimes faced legal challenges, city leaders may have been reluctant to shift operations prior to court approval. Finally, we conduct two empirical exercises to address anticipation concerns. First, we distinguish between outcomes

cities could easily change prior to annexation (statutory rates, spending allocations) and those they could not (aggregate revenue conditional on a statutory rate). Second, we restrict the sample to treatment clusters where the annexation process unfolded rapidly.

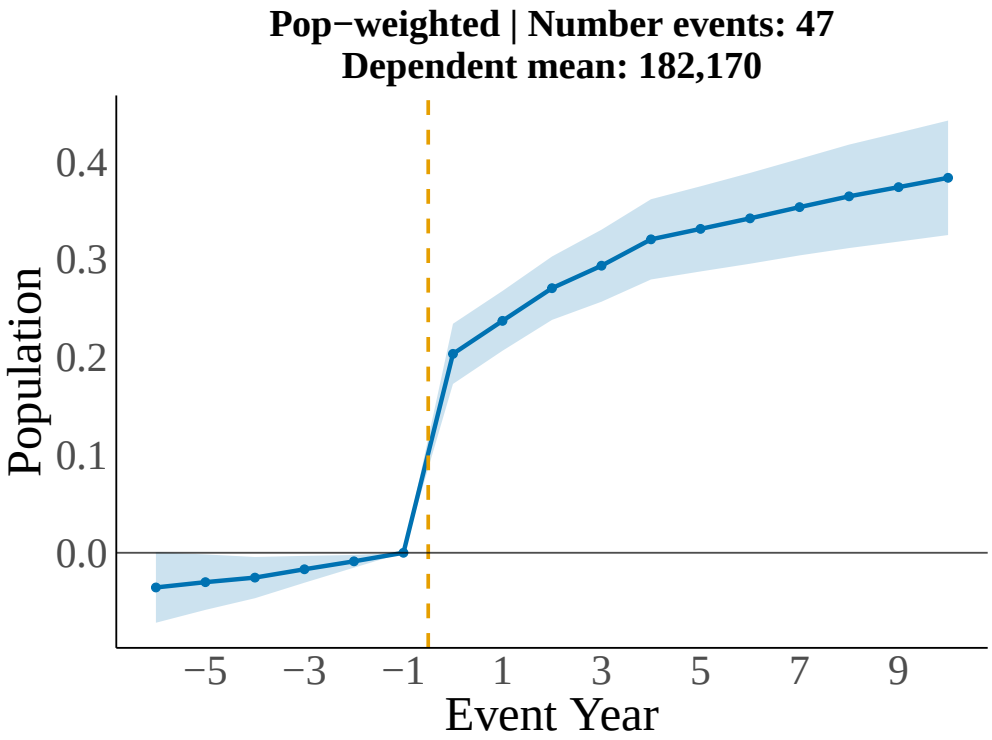
Finally, the last main assumption relevant to our setting is that there are no other correlated shocks: specifically, cities are not enacting annexation as part of a suite of policies or alongside a new government. In this case, effects we attribute to annexation might instead be driven by other policies or reflect the governing priorities or the new administration. Some policies (such as lowering property taxes) may be passed as a bundle to make annexation more palatable to residents (as in Atlanta), and we consider this part of the intended treatment effect. However, we would be concerned about cases where annexation is not the main driver of policy change but rather accompanies broader municipal reforms. To address these concerns, we conduct robustness exercises restricting the sample to cities where we have identified no other major policy changes adopted by city government around the time of annexation. We also conduct robustness exercises where we restrict to cities where annexation is not preceded by governmental turnover.

VI. Results

Our goal is to estimate the effects of municipal scale on local public finance and service provision. To estimate the causal effects of annexations, we focus on 45 sharp annexations events for which the exact the timing and scale of the annexation shock is unlikely to be co-incident with other shocks to the municipality.

We find that for many large urban municipalities in this period annexations were not only increases in land area, but also in terms of the population served. Across our 45 treatment events, the average annexation reflects an increase of roughly $120,000$ people—a 37% increase in population over the baseline year. Figure 2 demonstrates how treated cities record sharp increases in population from newly incorporated area. Population growth persists beyond the treatment year because we allow for annexation spurts lasting up to five years.

Figure 2: Log Population Event Study Estimates



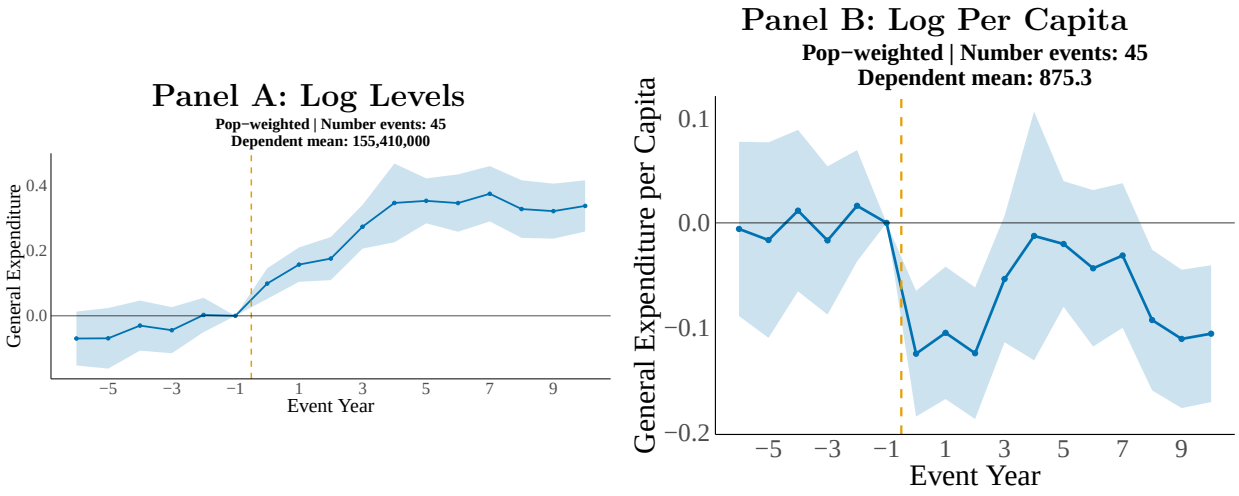
NOTES: This plot shows the estimated event study coefficients for population. Panel A shows the log of the raw means of the treatment and control samples, while Panel B shows the event study coefficients as specified in Equation 2. The baseline mean is reported in the top left of the figure. The vertical orange line reflects event year -1.

A. Municipal Finances

To what extent did these sharp increases in municipal scale have downstream consequences for local public finance? In our estimates, we distinguish between effects in levels and effects in per capita terms, which capture changes in outcomes in proportion to the population increase. The left-hand panel in the following figures reflects estimates in log levels and the right-hand panel reflects estimates in log per-capita terms.

Increases in municipal scale resulted in decreases in the average cost of service provision. The decreases in average costs was not driven purely by a denominator effect: as cities added population and land, total municipal expenditures rose by 33%, as can be seen in Panel A of Figure 3. However, as demonstrated in Panel B, the average expenditure per citizen decreased by −7%: that is, for the average annexation, the increase in expenditure was proportionally less than the increase in population.

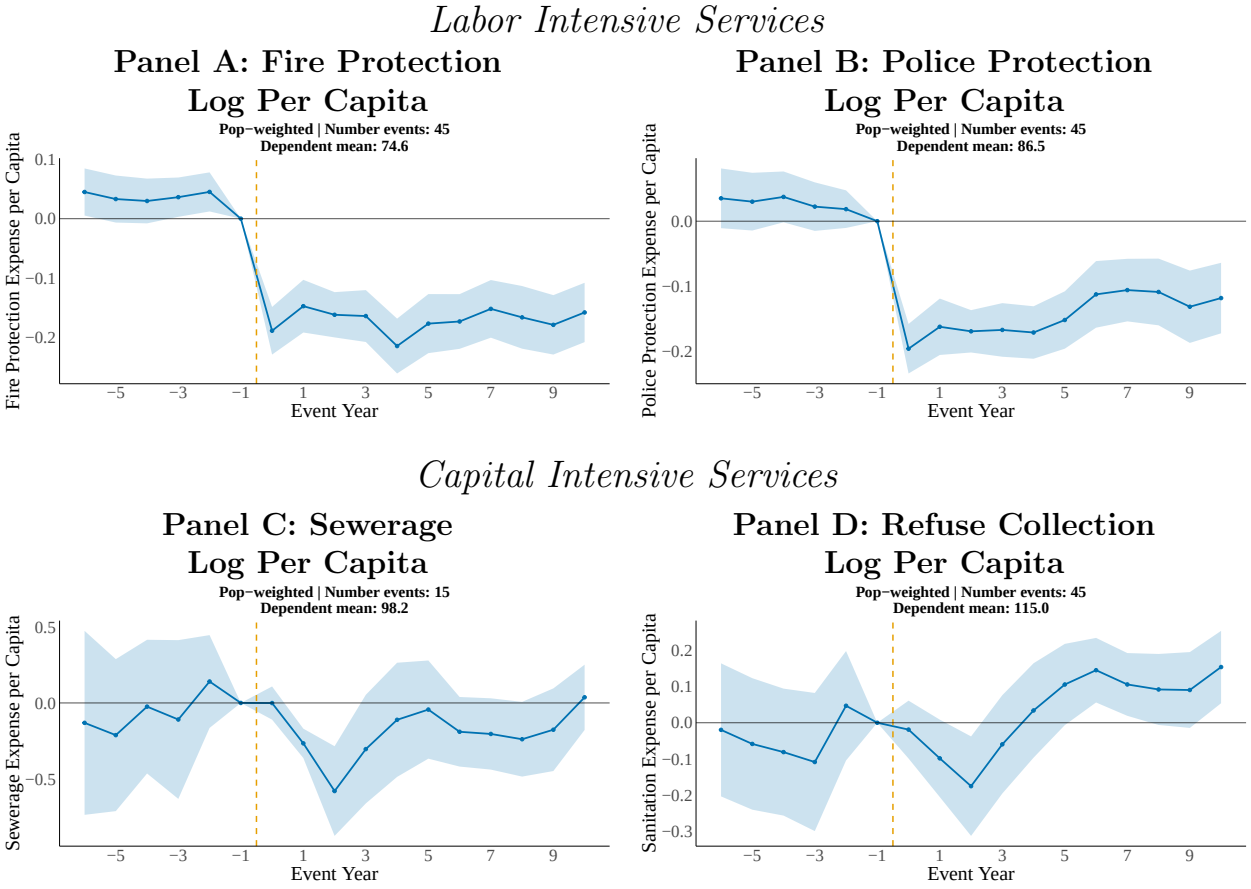
Figure 3: Impact of Annexation on Municipal Expenditures



Changes in the average cost of service provision varied across service types: per-service estimates suggest substantial differences in changes in averages across labor-intensive services, such as fire protection and policing, and capital-intensive services such as sewerage and refuse collection. As demonstrated in Figure 4, both fire protection and policing exhibit sharp and persistent drops in the average cost of service provision around −16%; while the point estimates for sewerage and refuse collection suggest flat or increasing per capita costs. In Appendix Figure [tk], we report

breakdowns of spending across all categories—as suggested by the average expenditure decrease, the number of municipal services demonstrating decreases in average costs is a larger share of total municipal expenditures. Heterogeneity by service type provides further evidence that our results are not driven purely denominator effects—municipalities annexing areas without providing adequate services is inconsistent with increases in per capita refuse collection.

Figure 4: Impact of Annexation on Expenditures by Service



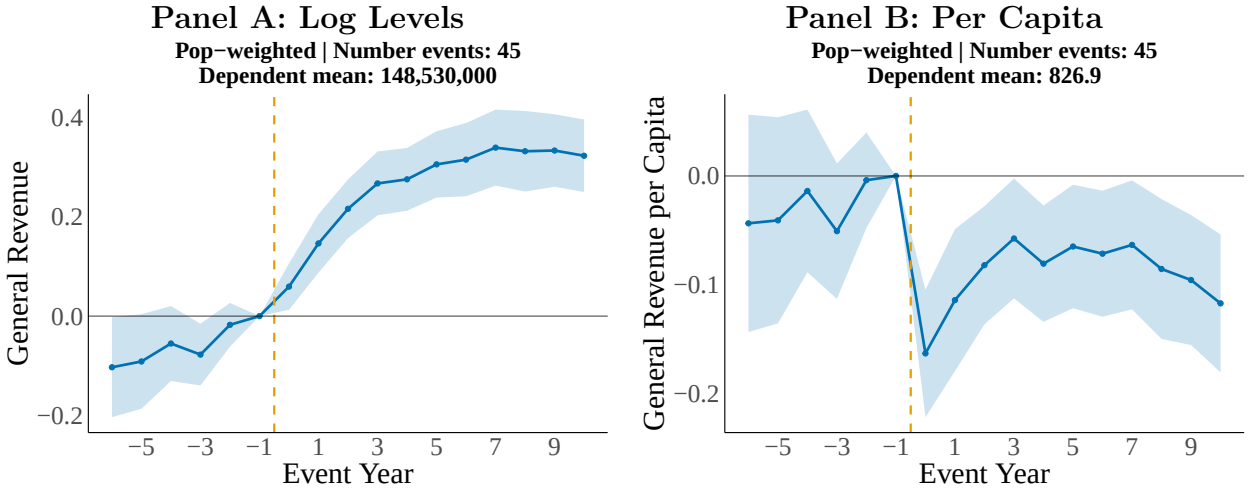
NOTES: This figure shows the impact of annexation on revenue sources and expenditure types. Panels A and B show revenue from overall taxes, property taxes, and total municipal debt in log levels and per capita terms, respectively. Panels C and D show expenditure broken down into current expenditures (ongoing operational costs) and capital outlay (fixed cost investments) in log levels and per capita terms, respectively. The shaded areas reflect the 95% confidence intervals for the estimated coefficients. The baseline mean is reported in the top right of each plot. The vertical red lines reflect event year -1.

Consistent with balanced budget operations of municipalities in the United States, average revenues decrease in similar proportion to average expenditures. Figure 5 shows estimates event-study estimates from equation 2 for general revenue (Panels A and B). Identically to expenditures,

we observe increases in overall municipal revenue (30%) but a corresponding decrease in per capita terms (-9%) relative to control cities. These results confirm that decreases in average expenditure are municipalities operating at a proportionally lower budget than control cities. Moreover, the proportionality of changes in average expenditures and revenues suggests that revenue decreases are not easily explained by changes in revenue raising efficiency.

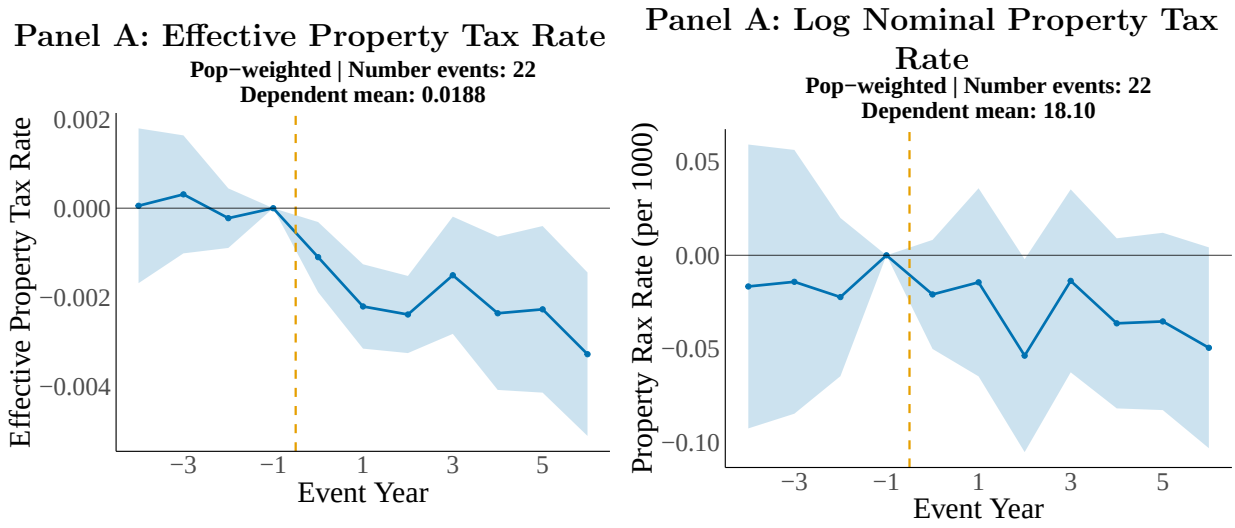
The lower proportional budget level for annexing cities results in lower property tax rates for all residents. Municipalities finance their expenditures by raising revenues, of which property taxes are the largest category: using new data on nominal property tax rates and total assessed valuations from Moody's Municipal Manuals in Figure 6, we find that both the effective property tax rate—the ratio of property tax revenue to total assessed valuations—and the nominal listed property decreases relative to control cities. We report both the effective property tax rate the nominal property tax rate for two reasons: first, cities occasionally perform changes of bases where they adjust nominal tax rates and assessment ratios in tandem, and the effective property tax rate is robust to these changes of basis[23]; however, the effective property tax rate is potentially contaminated by differential assessment or tax rates in annexed areas. That both the effective property tax rate and the nominal rate decrease suggests real changes in level of the property tax for all residents, including incumbents.

Figure 5: Impact of Annexation on Municipal Revenues



[23]For example, a city may list a property tax rate of 10 mills and assess properties at 50% of market value. Cities occasionally adjust in the vein of assessing at 100% of market value but charging a rate of 5 mills. The nominal tax rate is not robust to these change of basis, but the effective tax rate is.

Figure 6: Municipal Property Tax Rates



NOTES: This figure shows the impact of annexation on city-level statutory tax rates, effective tax rates, and assessed valuations in Moody's Municipal Manuals. Statutory tax rates are a city's legal reported tax rate per mil; effective tax rates are the ratio of property tax revenues to total assessed valuations.

B. Mechanisms for Changes in Municipal Expenditures

Reductions in the average cost of service provision can be explained by several competing effects. First, reductions in per capita costs can reflect decreases in the average quality of service provision. Second, the marginal cost of providing services to the annexed territories may be lower than the average cost for the existing core—for example, it may cost less to adequately protect low-density suburban areas than denser urban cores that may also include industrial development. Third, as cities increase their scale they may be able to spread certain fixed costs over a larger population.

To partially disentangle these competing effects, we turn to novel data in the form detailed surveys of fire and police departments recorded between 1950 and 1970. Because these surveys record different information in different years, we report single pre-post Difference-in-Difference coefficients. For example,the surveys record total property damage to fires in 1950, and between 1957 and 1961. While these data allow us to evaluate mechanisms for two important public good categories for which we observe average cost declines, we are limited in how much we can speak to the operations of other public good categories.

We do not find any evidence that reductions in average costs are explained by declines in service quality. In Figure 7, we report treatment effects across a broad range of outcome relevant to fire

department operations. The best measure of fire service quality we have is the total number of deficiency points and the fire insurance class recorded by the National Board of Fire Underwriters—the agency that provide the rubric and assessment that determined fire insurance rates. Total deficiency points are determined by the National Board of Fire Underwriters assessment of fire service across 124 categories: they range from 0 to 5,000 and each 500 point bin represents the Fire Insurance Class which would determine local fire insurance rates. As can be seen in the pooled estimates, we can rule out increases in total deficiency points past 0.1 standard deviations, suggesting that decline in fire service quality does not explain the decreases in average costs. We separately estimate whether cities were re-classified into a worse fire class—we find that treated treated cities were 6 percentage points likelier to be reclassified. This effect, while statistically significant, is relatively modest: this effect is explained by 2 of 33 cities being re-classified.

We do, however, observe an interesting substitution in fire service provision. Our estimates suggest an increase in fire department deficiency points: the specific fire department rating only accounts for 30% of the total rating, where the remaining categories are water supply, fire alarms, fire prevention, building departments, and structural conditions. The divergence between effects for total department deficiency points and fire department deficiency points suggests that some operations of fire departments were stretched thinner, but this effect was compensated by improvements in other operations (i.e. water supply, alarm systems, or fire prevention services.) The increase in fire department deficiency points is, also, modest: the estimated average treatment effect is roughly 0.17 standard deviations or 49 deficiency points, less than a tenth of the deficiency points required to move to another fire insurance class.

We find stronger evidence that, at least for fire protection, annexations reduce the average cost of service provision by adding territory where the marginal cost of service provision is lower than the existing average cost. In Figure 7, we present evidence that the annexation results in a roughly 20% decrease in building fires and total fire loss per capita. Because total deficiency points are flat, we likely cannot attribute this decline to improvements in service quality, but rather to the consequence of adding low-density areas with lower fire risk.

The decreases in fire incidence and loss suggests that, at least in part, annexations function through cross-subsidization between the urban core and the annexed fringe. The municipality can

only set a single property tax rate determined by the average cost of service provision: properties on the margin of expansion in space have lower marginal costs of service provision, and they are, at least, for fire protection, paying higher than their marginal cost. The aggregate effect, however, is that the annexation lowers average costs.

Finally, we find some suggestive evidence that the expansions alter the spending composition of fire protection though larger capital expenditures. For evidence on capital investments for fire departments, we focus on apparatus. Fire departments require both engines (i.e. pumpers) and regular trucks: among trucks, according to the National Board of Fire Underwriters, "all trucks preferably should be aerial" as opposed to the regular ladder trucks. Our estimates see increases in the number of engines, and corresponding decreases in the ratio of firefighters to engines and the ratio of engines to trucks. We also find that the ratio of aerial trucks to ladder trucks increases, suggesting that fire departments upgraded capital to the preferable aerial trucks. These results suggest that changes in the capital structure of fire protection services may also play a role in changes in average costs.

Importantly, we can only speak directly to the mechanisms of average cost reductions for fire and police departments—two important categories for which we observe large declines in average costs. While these patterns are informative of one source of cost reductions, they do no necessarily generalize across services.

C. Heterogeneity by Density

Annexations varied greatly in their scope. For example, Oklahoma City added 558 square miles at a density of 165 people per square mile while Seattle added 15.6 square miles at a density of 4,805. We focus on density because it captures as a single measure a proxy for potential over-reach: density captures not only the sheer land area added, but rather the land area relative to the underlying distribution of population. Density may matter because it may alter the underlying cost effects of annexations: for example, administering garbage collection across a newly added sparsely populated area 550 square miles may change costs differently than expanding service to a densely populated area of 15 square miles.

To examine how density impacts the effects of annexations, we split our sample at the median

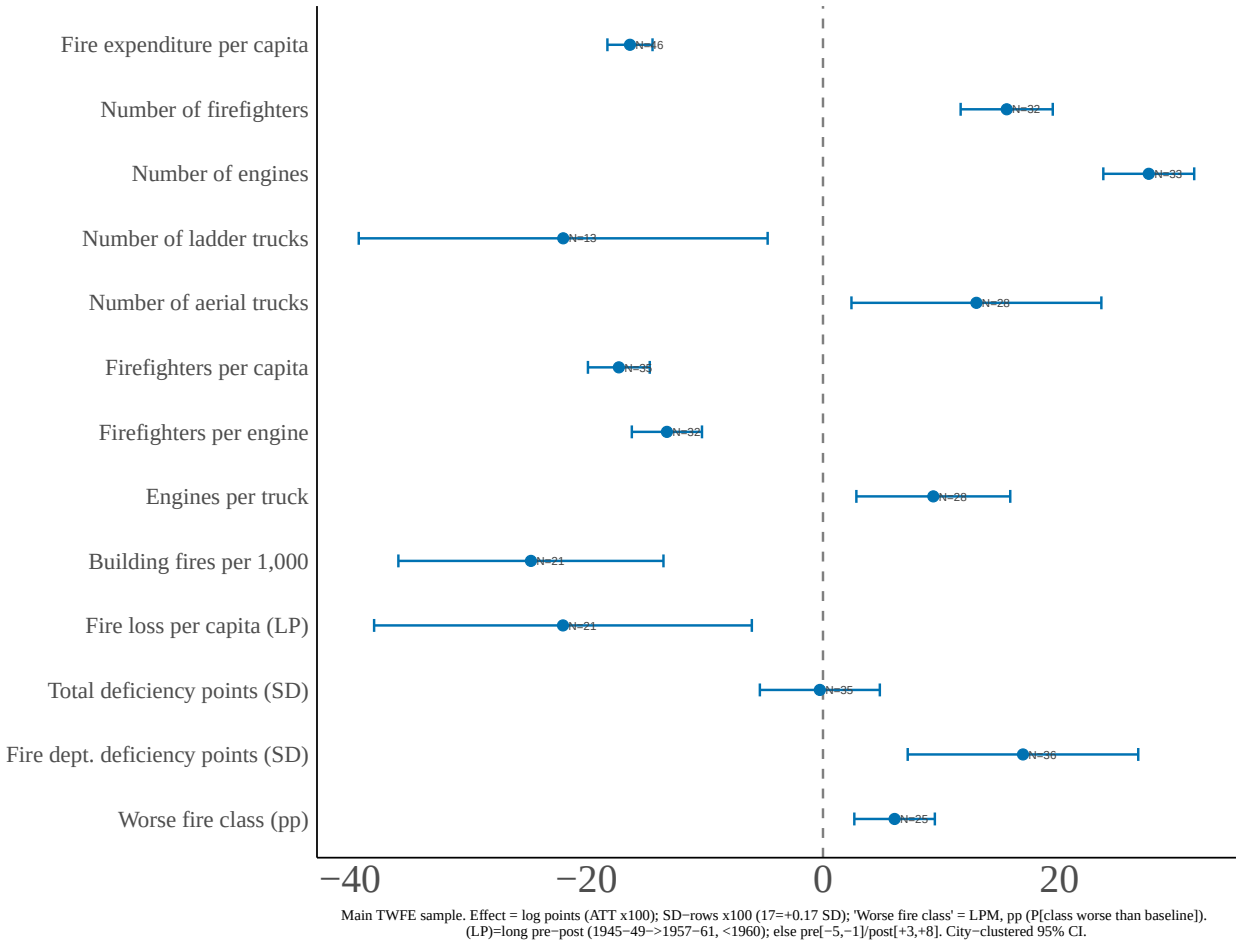
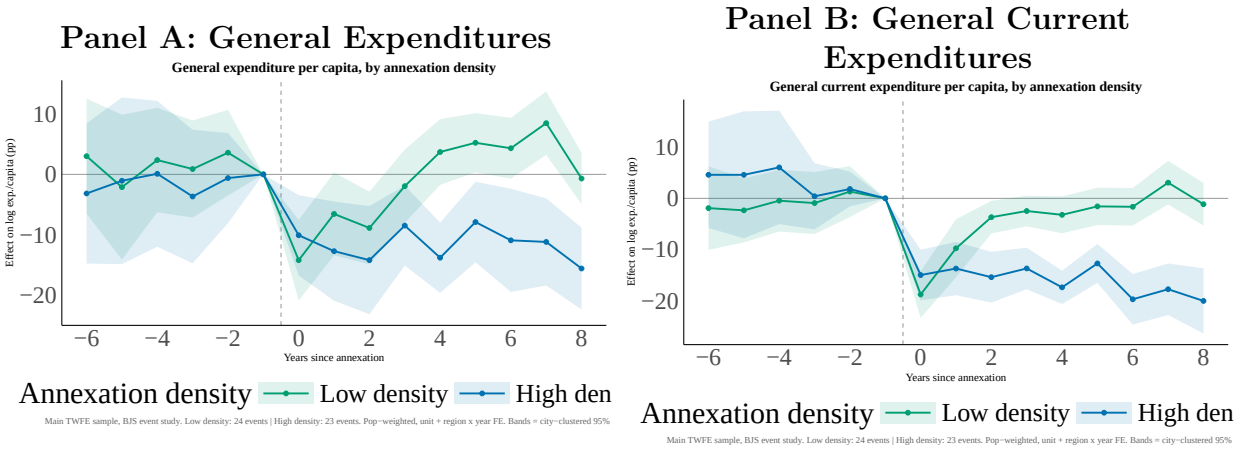


Figure 7: Changes to Fire Department Operations

density—991 people per square mile—and re-estimate the event studies on each sub-sample.

We find that our decreases in average costs are concentrated in the set of high-density annexations: for the average low density annexation, average costs exhibit short-run decreases, but return to levels comparable to control cities 3 years after the annexation event. We present the event studies splitting on the density of the annexed area in Figure 8—because density may be driven by differential costs of capital expenditures, we also display the treatment effects for current (i.e. non-Capital outlay) expenditures in Panel B. In terms of both general and general current expenditures, high density annexations exhibit persistent per capita cost reductions, while low-density annexations do not. This result suggests that moderate expansions in space produce stronger average cost reductions than sprawling land-grab expansions.

Figure 8: Heterogeneity by Density



NOTES:

D. Effects on Local Economic Activity

> *"Industries in this area can choose between a handful of small, autonomous cities, with duplicating services and inefficient operation, or one large municipality of the type now enjoyed by Phoenix. We'll choose the latter, and we think most people will agree."*
> —Arizona Republic, 1957

City managers often argued that expanding municipal borders would increase local economic outcomes by attracting businesses, new residents, and allowing for co-ordination under a single

governmental unit. We estimate the effects of annexations on local economic activity via the following equation:

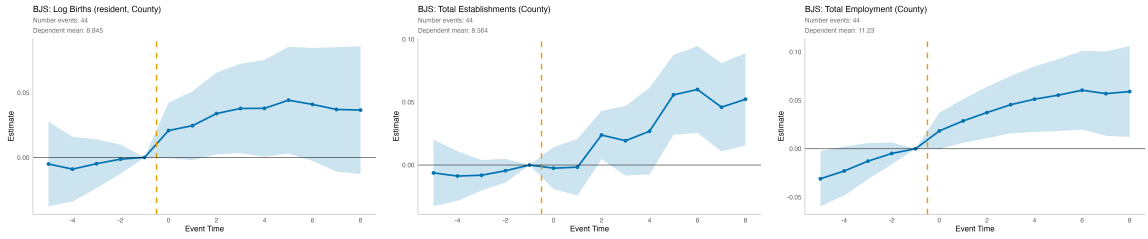
$$Y_{i(r),t} = \sum_{n=-T}^{T} \beta_n(EventTime_{n(t)} + X_{i,1947} \times Year_t + \gamma_{rt} + \alpha_i + \epsilon_{it}$$

We only keep counties in our sample that contain a city matching our criteria in Section V. and having a population within 30% of the treatment county in the year prior to annexation. Second, we exclude any control county if a sample city within that county experienced a major annexation prior to the treatment event.

We find suggestive evidence that annexations increased local economic activity. Figure 9 shows pooled coefficient estimates of annexation's impact on births, establishments, and total employment at the county-level. We include births as a proxy for local population: this measure is only suggestive of Similarly, despite being more noisily estimated, annexation has no impact on total employment (-0.03) or employment in manufacturing (0.05) or services (-0.03).

Taken together, these results suggest that while annexation had meaningful and large effects on city government operations, it generated little economic spillover to the larger metro areas, at least in the medium run. The promise that annexation would jumpstart new metropolises through outside investment and in-migration from other cities appears to have been overstated by annexation advocates.

Figure 9: County-level Economic Activity



NOTES: This figure shows the impact of large annexations on county population and employment. The figure displays pooled coefficients (equation 3) for log county population, log total employment, log total establishments, log manufacturing employment, and log service employment. Treatment counties are defined as counties containing cities with major annexation spurts, while control counties are similar popuplation counties where no annexation occurred within the county over the treatment period.

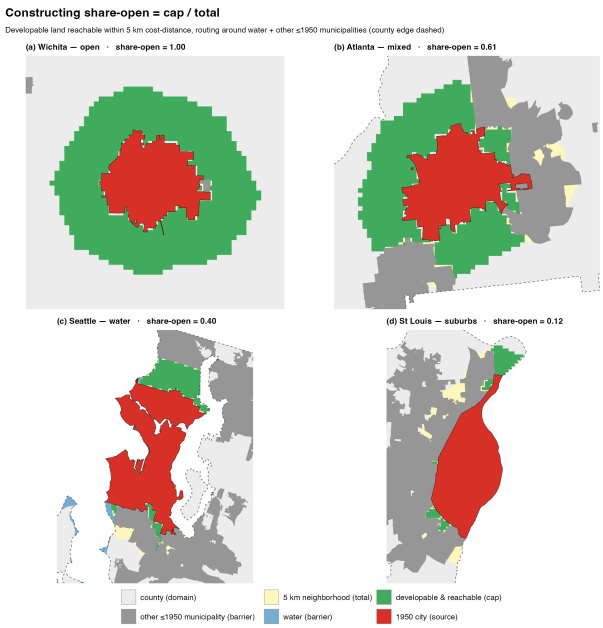


Figure 10: Examples of Instrument Construction

VII. Geographic Instrument for Annexations

Counties with annexing central cities by definition have larger central cities and fewer other incorporated suburbs. This means that annexation policy was also an important determinant of centralization and decentralization in public good provision in the U.S. in the mid-century. We construct an instrument for aggregate annexation as opposed to incorporation in a county area: the presence of obstacles in 1950 that block expansion of the central city. This intuition behind this instrument is similar to the instrument in Harari (2020): we The intuition for this instrument is that, within Census regions, places that shared the legal ability to annex may have differed in their ability to annex due to the relative position of geographic features, county borders, and other incorporated municipalities.

A. Instrument Definition

To define how "fenced-in" a city was in 1950, we begin by growing a fixed 5km distance from its boundary within the county. We restrict to growth within the county because municipalities generally cannot cross county lines. We then compute how much of that land is "annexable" by the central city. Areas are not annexable if they belong to another municipality, or if the city is

blocked from accessing it by the position of another municipality. The instrument is then the share defined as:

$$ShareOpen_i = \frac{Annexable5km_i}{Area5km_i} \tag{4}$$

In Figure 10, we give four examples of this measure. Wichita, Kansas is a prime example of an "open" city: located in the Plains, Wichita has no significant geographic or municipal barriers to block its growth. In contrast, Seattle has some room for growth, but its location on an isthmus means that any expansion northward or southward is blocked by other incorporation.

We report conditional and unconditional balance for the instrument in Table 4. Unconditionally, the instrument is at baseline correlated with pre-period growth. The lack of balance is intuitive: cities in counties with less density and development will naturally have fewer other incorporated municipalities and higher share open. To correct for the correlation of *ShareOpen* with baseline characteristics, we control for baseline size and population of the central city. In the second column, we report the the correlation of share open conditional on controlling for baseline characteristics and find that conditional on baseline size, share open is not correlated with pre-period growth trajectories.

Table 4: Instrument: Baseline Balance Table

	Coef. on share-open (per s.d.)		
Covariate (1950 level, or pre-1950 trend)	Unconditional	With controls	N
Panel A. 1950 city characteristics			
log population density	-0.204***	-0.015	108
	(0.047)	(0.018)	
log median house value	-0.092***	-0.032	108
	(0.019)	(0.021)	
log median gross rent	-0.010	0.024	108
	(0.012)	(0.020)	
log median income	-0.016	0.012	44
	(0.012)	(0.020)	
share white	0.018***	0.011	45
	(0.006)	(0.021)	
share black	-0.017**	-0.011	45
	(0.006)	(0.021)	
share foreign-born	-0.025***	-0.014**	45
	(0.005)	(0.005)	
share owner-occupied	0.055***	0.001	45
	(0.018)	(0.017)	
share manufacturing	-0.006	-0.008	45
	(0.007)	(0.015)	
share full plumbing	-0.009	-0.003	45
	(0.009)	(0.015)	
log fire-defic. score	0.201***	0.016	101
	(0.076)	(0.106)	
Panel B. Pre-1950 trends			
city pop. growth 1940–50	0.024***	0.010	108
	(0.008)	(0.016)	
city house-value growth 1940–50	-0.062***	-0.016	108
	(0.020)	(0.015)	
county pop. growth 1930–50	0.021*	0.002	108
	(0.012)	(0.020)	
county pop. growth 1940–50	0.016**	-0.000	108
	(0.008)	(0.012)	

B. Instrument Specification

We estimate the system of equations below via Two-Stage Least Squares:

$$\Delta LogCentralCityLandArea_{1980-1950,i} = ShareOpen_{1950,i} + X_{1950,i} + Region_i + \epsilon_i \tag{5}$$

$$\Delta Y_{1980-1950,i} = \Delta LogCentralCityLandArea_{1980-1950,i} + X_{1950,i} + \epsilon_i \tag{6}$$

Equation 4 is the first stage: we regress the endogenous variable—change in central city land area on share open. We control for some baseline characteristics in 1950 to isolate variation in the position of obstacles for jurisdictions similar in baseline characteristics. In our preferred specification, we control for baseline city population and area since less developed counties may mechanically have higher share opens. We also control for baseline geographic characteristics: the county area and proximity to water. We also confirm in columdn 4 that the instrument retains predictive power beyond the total extent of other municipal incorporation to ensure that our variation is driven by variation in the relative position of obstacles, not their presence.

In Table 5, we report estimates of regressing share-open on the change in log city land area between 1950 and 1980. The estimates are stable to addition of controls—the coefficient on share-open varies between 1.2 and 1.3 across all specifications. This suggests that going from 0% share open to 100% open land lead to a little more than doubling a cities original land area. This relationship persists after control for baseline size, suggesting that the effect is not simply convergence via growth. While the predictive power of the instrument is attenuated by controls, the instrument retains predictive power beyond baseline characteristics that may affect the construction of share-open i.e. baseline size, county area, and the presence of other incorporation.

C. City-level IV Estimates

We report the city-level two-stage least squares estimates in Table 6. The 2SLS estimates suggest that, within Census region, central cities that were less constrained had significantly higher population growth between 1957 and 1977. This growth is most likely through geographic growth. Measures of per capita spending are negative, but marginally significant. Reassuringly, the point estimates point in the same direction as the estimates from our Difference-in-Difference design. Similar to the diff-in-diff estimates, the sharpest declines in per capita spending are in fire protection.

Table 5: First-Stage Estimates

	Dependent variable: Δ log city land area, 1950–1980			
	(1)	(2)	(3)	(4)
Share-open (share, 0–1)	1.389***	1.316***	1.199***	1.352***
	(0.184)	(0.288)	(0.289)	(0.281)
log 1950 land area		-0.695***	-0.693***	-0.725***
		(0.123)	(0.123)	(0.125)
log county area		0.157***	0.157***	0.153***
		(0.042)	(0.040)	(0.043)
log 1950 population		0.501***	0.502***	0.464***
		(0.125)	(0.128)	(0.136)
local water share			-0.511	-0.492
			(0.351)	(0.365)
municipal area (km^2)				0.000
				(0.000)
Region FE	Yes	Yes	Yes	Yes
First-stage F	57.2	20.9	17.2	23.2
Observations	108	108	108	108

Table 6: 2SLS Estimates: City Level

Outcome (Δlog, 1957–1977)	2SLS	(s.e.)
Population		
City population	0.509***	(0.069)
Expenditure, per capita		
Total expenditure	-0.192	(0.134)
General expenditure	-0.228	(0.138)
Police protection	-0.164	(0.111)
Fire protection	-0.307***	(0.116)
Highways	-0.127	(0.262)
Parks & recreation	-0.097	(0.254)
Revenue, per capita		
Total revenue	-0.155	(0.119)
Property tax	0.015	(0.166)

First-stage $F = 17.2$; $N = 108$ central cities.

D. County Area IV Estimates

Finally, we estimate whether geographic obstacles to annexation by the central city in 1950 resulted in aggregate differences in municipal centralization vs decentralization with counties. As can be seen in Table 7, our instrument predicts area added by annexation, and negatively predicts newly incorporated land. On net, these fixed county areas don't differ in their total added incorporated land area, but rather in how that incorporation took place. This result is consistent with "open" central cities expanding, procluding the formation of other jurisdictions, where as "closed" central cities remaining hemmed in, leading to new incorporation instead.

In light of this pattern, we estimate the two-stage least squares of our instrument on total county area finances. We find that total expenditures per capita are lower in county areas where

geographic positioning allowed the central city to expand. The results do not isolate the source of the effect across services. There are two possible explanations for this: first, it may be the case that many services are slightly negative and we do not have power to detect the effect until we pool across all services; second, scale economies may arise from sharing general city functions as opposed to specific expenditure categories.

Notably, the sharp negative effect on fire protection at the city level attenuates at the county level, confirming our interpretation that city-level decreases in average cost for fire protection come from adding lower cost-to-serve suburban areas.

Table 7: County Area Municipal Centralization

Outcome (km^2)	Coef. on share-open	N
Municipal land added, 1950–1980		
Annexed (central city)	$+516^{***}$	108
	(161)	
Newly incorporated (other munis)	-474	108
	(436)	
Total added (= sum)	+42	108
	(481)	
Total incorporated area in county, 1980 (stock)		
Total municipal area	-328	108
	(571)	

Table 8: 2SLS Estimates: County Area Total Spending

Outcome	2SLS	(s.e.)
Expenditure, per capita (Δlog 1957–77)		
Total expenditure	-0.160*	(0.088)
General expenditure	-0.129**	(0.061)
Police	0.030	(0.109)
Fire (per incorporated pop.)	-0.112	(0.116)
Highways	-0.354	(0.218)
Parks & recreation	-0.078	(0.204)
Education	-0.048	(0.101)
Revenue, per capita (Δlog 1957–77)		
Total revenue	-0.125*	(0.068)
Property tax	-0.008	(0.084)
Segregation across jurisdictions (D)		
Black/white, 1980 level	-0.370***	(0.094)
Household income (bottom quartile), 1980	-0.066	(0.043)
Δ economic, 1950–80 (quasi)†	-0.091*	(0.051)

First-stage $F = 16.9$; $N = 109$ counties.

E. North-east Placebo

Finally, to test whether our instrument, conditional on baseline characteristics, is correlated with other structural changes that are not annexations, we run a placebo test in the Northeast. In the Northeast, state laws prohibited any central city growth. If the instrument were correlated with an effect in the Northeast, this might suggest that we are misattributing structural changes that were affecting hemmed in vs. non-hemmed in places over this period.

The instrument does not predict land area growth in the North-east, nor growth in city popula-

tions. If anything, more "open" cities were growing slower in the North-east, but the point estimate is not statistically significant. The instrument is also uncorrelated with changes in the central city finance, and in total and general expenditures per capita for fixed county areas.

"Open" and "closed" cities in the Northeast and outside the Northeast at this period may have differed significantly. While we cannot rule out structural forces that may have affected "open" and "closed" differentially outside of the Northeast, this placebo exercise re-assures us that there was at least no common *national* trend between "open" and "closed" cities conditional on baseline characteristics.

Table 9: 2SLS Estimates: Northeast Placebo

	Northeast (placebo)		Outside Northeast	
Outcome	Coef.	*N*	Coef.	*N*
Panel A. Annexation channel				
Δlog city land area	0.017	30	1.199***	108
	(0.068)		(0.289)	
Δlog city population	-0.196	30	0.768***	108
	(0.157)		(0.182)	
Panel B. Central-city budget (Δlog p.c., 1957–77)				
Total expenditure	-0.076	30	-0.231	107
	(0.255)		(0.164)	
General expenditure	-0.099	30	-0.274	107
	(0.264)		(0.172)	
Police protection	-0.129	30	-0.197	107
	(0.213)		(0.140)	
Fire protection	-0.041	30	-0.369**	107
	(0.126)		(0.175)	
Highways	-0.253	30	-0.153	107
	(0.305)		(0.319)	
Panel C. Fixed county-area budget (Δlog p.c., 1957–77)				
Total expenditure	0.096	30	-0.198**	108
	(0.178)		(0.095)	
General expenditure	0.145	30	-0.155**	108
	(0.184)		(0.071)	
Police	-0.003	30	0.038	108
	(0.177)		(0.132)	
Fire	-0.040	30	-0.046	108
	(0.110)		(0.152)	
Highways	0.059	30	-0.411	108
	(0.140)		(0.253)	

VIII. Conclusion

We have shown that after World War II there was large-scale regional divergence in the size and structure of municipal governments among medium- and large American cities. Due to differences in state policy, cities in the Sun Belt were able to liberally expand their borders, increasing city land area by nearly a factor of six. This expansion came at a critical time in the development of the American city, as suburbanization was occurring everywhere. This led to differences in urban-municipal structure: economic areas in the North became highly fractionalized across many smaller municipalities, while Sun Belt cities were able to retain population within their borders, particularly higher-income residents who moved to newly annexed suburban areas.

We show that these annexations had substantial impacts on the operation of municipal governments. Relative to non-annexing cities, total revenue and expenditure per capita in annexing cities declined by roughly 25%, and this decline persisted ten years out. We find evidence that this per capita decline came from realized modest economies of scale, dispersing costs over larger populations without leading to any measurable decreases in the quality of municipal services. However, despite the lofty promises at the time of larger positive spillovers from annexation to economic activity, we find no evidence that annexation had any meaningful impact on county employment.

We are currently working to improve our ability to measure the impacts of annexation through three ongoing digitization efforts. First, we are digitizing the Moody's Municipal Manuals to obtain information on changes in formal taxation and bonded debt. Second, we are digitizing additional measures of public good provision beyond fire and police departments. Finally, we are digitizing data on the residential characteristics of city populations to compare annexed and pre-existing populations and examine whether annexation induced any sorting responses or changes in residential composition.

IX. References

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Appendix – For Online Publication

Table 3: Balance Table: City Characteristics by Treatment and Annexation Status

	Mean (SD)			p-	
	Treated Annexing	Non-Treated Annexing	Non-Annexing Control	Treated vs. Non-Treated	Treat Con
Land Area (1944)	21.9 (15.0)	23.6 (50.5)	25.7 (26.2)	0.764	0.3
Land Area (1980)	153 (170)	81.6 (91.7)	28.5 (26.7)	0.019	0.0
Population (1950)	154,993 (108,739)	215,503 (621,158)	169,850 (179,024)	0.353	0.5
Population (1980)	262,433 (149,513)	294,540 (597,066)	148,999 (116,919)	0.621	0.0
1950 City Characteristics					
Share White	0.80 (0.14)	0.88 (0.13)	0.95 (0.06)	0.003	0.0
Share Black	0.20 (0.14)	0.12 (0.13)	0.05 (0.06)	0.003	0.0
Share Born in State	0.64 (0.17)	0.58 (0.19)	0.59 (0.18)	0.069	0.1
Share Native Born	0.96 (0.04)	0.95 (0.04)	0.87 (0.05)	0.106	0.0
Share Manufacturing	0.08 (0.04)	0.08 (0.05)	0.15 (0.06)	0.924	0.0
Share Services	0.11 (0.03)	0.11 (0.03)	0.09 (0.03)	0.937	0.0
Average Age	31.9 (2.1)	31.9 (2.7)	33.8 (1.9)	0.904	0.0
Average Occupational Score	25.0 (1.2)	25.6 (1.2)	26.3 (0.98)	0.012	0.0
1950 County and MSA					
MSA Population	400,378 (259,899)	562,899 (1,115,548)	1,653,908 (2,884,873)	0.199	0.0
Share White (County)	0.82 (0.13)	0.89 (0.13)	0.95 (0.05)	0.021	0.0
Share Manufacturing (County)	0.08 (0.04)	0.08 (0.05)	0.14 (0.05)	0.984	0.0
Average Occupational Score (County)	24.7 (1.2)	24.5 (1.6)	26.2 (0.77)	0.568	0.0
Municipal Finance (1951, 1000s)					
Total Debt	1,101 (811)	858 (647)	885 (965)	0.113	0.2
Total Expenditure	779 (479)	656 (388)	913 (800)	0.173	0.2
Capital Outlay	297 (262)	219 (231)	158 (127)	0.718	0.0

Note: