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

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November 15, 2026

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Abstract

Postwar suburbanization shifted residents beyond the borders of central cities, and many cities responded to the resulting fiscal pressure by annexing the suburban fringe: between 1950 and 1980, the median annexing city more than tripled its land area and added nearly 50% to its population through annexation. We construct new data from archival records for 177 American cities to study how this annexation wave affected the cost and quality of municipal services. Using the sharp, episodic timing of annexations in a staggered difference-in-differences design, we find that annexation reduced per-resident spending by 9%, with no evidence of a decline in service quality. These savings reflect two offsetting forces: at fixed density, spending rose less than proportionally with population, but the sparser areas that cities annexed were costlier to serve. Countywide, annexation brought fringe residents up to city service levels without detectably raising public spending per county resident, and it increased the number of business establishments by about 5%.

Keywords: annexation, municipal boundaries, local public finance, economies of scale
JEL Codes: H72, H77, N92, R51

*We are grateful to our advisors, Leah Boustan and Ilyana Kuziemko, for their invaluable guidance on this project. We also thank Nick Buchholz, Allan Hsiao, Henrik Kleven, Eduardo Morales, Stephen Redding, Owen Zidar, and Evan Soltas for helpful feedback. We also thank participants in Princeton's Student Labor and Graduate Labor Workshops.

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I. Introduction

Postwar suburbanization shifted people and economic activity from central cities to their suburbs, creating "the metropolitan problem": how to organize local governance, taxation, and public services across increasingly sprawling metropolitan areas (Nolting and Arnold 1952). A popular solution for central cities was to annex the newly developed unincorporated suburban fringe (Bollens 1956; Sengstock 1960; Rusk 2013). City leaders argued that annexations would generate economies of scale in service provision by de-duplicating administrative functions and spreading out fixed costs. Suburban residents often resisted these annexations, arguing that a larger government would add bureaucratic inefficiency and could not tailor service provision to local preferences (Tiebout 1956). In this paper, we estimate how increases in central-city municipal scale realized during the postwar annexation wave affected the cost and quality of municipal services and whether this increased centralization affected aggregate efficiency in local public finance and economic activity.

Central cities provide an extensive range of municipal services that account for roughly 15% of total state and local government expenditure in this period. Because they are economic hubs of their metropolitan areas, many of these services benefit residents and firms beyond immediate city limits. Postwar annexation was consequential for the size and structure of these cities; between 1950 and 1980, the median annexing city more than tripled its land area and in doing so retained a large portion of the metropolitan population within its city boundaries. Much of the empirical evidence on the effects of municipal scale comes from natural experiments using imposed mergers or splits of local governments (Narasimhan and Weaver 2024; Dahis and Szerman 2026; Tricaud 2025). We instead study city-initiated annexations, where the underlying variation stems from how constrained cities were in their ability to expand. Our setting therefore asks a distinct, policy-relevant question: when cities can choose their own scale, does centralization improve efficiency?

To analyze the effects of annexations on municipal operations, we construct a new dataset that tracks the operations of 177 US central cities from 1940 to 1990. From surveys of

city managers, Moody's municipal manuals, and a range of Census publications, we assemble an annual panel of annexations, city finances, service provision, and economic activity throughout the annexation wave. To complement the annual panel, we construct 1950 city boundaries from Census maps and pair them with existing 1980 boundary data. These historical city boundaries are then linked to spatially disaggregated data, allowing us to measure changes within fixed geographic limits. To our knowledge, the result is the most comprehensive dataset covering the structure and operations of US municipalities in the postwar period.

These data allow us to document three new facts about the postwar annexation wave. First, the decline of urban cores was common to annexing and non-annexing cities: between 1950 and 1980, both lost population within their 1950 borders, saw slower real income growth than the national average, and experienced substantial white flight. Second, annexed areas were typically whiter and faster-growing than the absorbing central city, and by 1980 richer, which significantly transformed the demographic composition of the annexing cities. Third, although per-resident municipal spending rose everywhere, it grew more slowly in cities that underwent substantial annexations.

To study the effects of annexation, we use a staggered difference-in-differences design leveraging the sharp, episodic nature of many of the border expansions. We compare 49 cities that annexed in short, concentrated bursts to similar-sized cities in the same census region that never annexed or had not yet annexed. Matching on size and region means the two groups faced comparable economic conditions, but a concern of this design is that cities strategically opt into annexation precisely at moments when they expect trend breaks in their own development. 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.[1] Second, cities had weak control over the exact timing of annexation: assembling a plan

[1]Nearly all border expansions 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..

and the political support to execute it took years, and even in permissive states, ordinances were often delayed, challenged in court, or rejected.[2] 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.

We find that large annexations reduce overall per-resident spending, consistent with these annexations being cost-saving for central cities. Annexation spurts were a sizable shock to the structure of city governments: the median spurt increased the city's land area by 143%, and the average treated city increased its population by 44%. While the overall size of the government grew to extend service to new areas, the average annexation reduced per-resident spending by 9%, corresponding to an elasticity of spending with respect to population of 0.75. Using variation in the size and population density of annexations, we find that while these cost reductions scale with annexed population, they are partially offset by a higher cost of serving lower-density areas of the county. Given the density gradient of American urban areas, the tradeoff between population and density suggests that the marginal cost of extending the city boundary is decreasing at first, but increasing later as areas become sparser and more difficult to serve. In fact, because city planners initiated annexations under imperfect expectations of future growth (Reid 1959), we observe empirically the interior optimum of this tradeoff: large land-grab annexations do not reduce per-resident spending, while annexations of denser suburban areas do.

What generated these cost savings and which residents of the city may have benefited from this increased scale? The declines are concentrated in services where fixed costs are proportional to the area served, such as fire protection and policing, which decrease persistently (by 16% and 12%, respectively). For these services, single stations cover roughly fixed areas, allowing costs to be spread effectively over denser population. Services such as sanitation (mostly garbage collection) and sewerage that scale with the number of households served are

[2]For example, the annexation court refused Norfolk, VA's 1952 annexation suit, and the city added territory only in 1955 and 1959, after renewed proceedings. Atlanta's earlier efforts to expand failed until the state legislature passed the 1951 Plan of Improvement.

roughly flat. More detailed administrative data from police, fire, and sanitation operations reports show three key factors in generating lower costs per resident: cross-subsidization from lower marginal-cost suburban areas, consolidating operations, and capital intensification. Through these same data, we also find no evidence of service decline—measured through external ratings of fire department and police department efficiency–accompanying this lower spending. Finally, using assessed values and statutory tax rates from Moody's municipal manuals, we find that these cost savings were passed on to incumbent residents, but newly annexed residents paid higher taxes in return for the cities' more expansive service bundle. These forces highlight the political economy of annexation at the time, when central cities and their residents strongly supported the policy but residents of the annexed fringe did so case by case, depending on whether the benefits of higher service provision outweighed the costs of higher taxes.

Given these substantial effects on central cities, annexation plausibly also affected the broader metropolitan area, particularly since central cities were economically important to firms and residents outside their boundaries. To study these long-run and spillover effects, we construct an instrument for a city's capacity to annex from both geographic and policy constraints on expansion. For the geographic constraint, we measure how fenced in a city was by incorporated municipalities in 1950; for the policy constraint, we use the stringency of the state's annexation law before the annexation wave. Conditional on baseline controls for city and county size, the instruments are balanced on baseline characteristics and predict future land-area growth.

We first estimate the effect of municipal centralization on aggregate spending across all local governments. Measuring this effect is challenging because annexation had two competing effects. First, it moved residents from unincorporated land into central cities, where they received a more expansive bundle of municipal services. Second, it changed the cost of providing municipal services, both in the central city and potentially in other layers of government. We find that these forces roughly offset: total spending per county resident

is roughly flat, but this reflects residents receiving more municipal services at a lower average cost.

Finally, advocates of annexation argued that by improving local services, cities would attract economic activity. Using annual county data on business establishments, employment, and births, we find modest positive effects after large annexations: establishments rise by about 5%, with similar but less precise effects on employment and births. Because the effects are strongest for establishments, commercial land use rises, and sorting responses appear weak, we interpret these effects as driven primarily by firm location rather than by residents moving into the city.

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 2026). Others have considered the efficiency effects of decentralized public goods provision (Bordeu 2026; Hoxby 2000; Lipscomb and Mobarak 2017). Empirical estimates have found mixed evidence on returns to scale when several adjacent municipalities coordinate service provision (Tricaud 2025; 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, which may provide services at a different scale than jurisdictions such as villages or towns (Narasimhan and Weaver 2024; Tricaud 2025) or counties (Dahis and Szerman 2026). 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 2026; 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 postwar 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, Troesken and Geddes 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: Section II. describes the institutions and rules that governed annexation in this period; Section III. explains our data sources; Section IV. presents descriptive facts on postwar annexation and municipal finance; Section V. describes the event-study design and estimates the effects of annexation on city finances and service provision; Section VI. turns to the metropolitan effects of annexation on local finances and economic activity; and Section VII. concludes.

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.*"[3]

[3]Excerpted from a letter to annexed residents by Norfolk City Council President W. Fred Duckworth, reprinted in *The Commonwealth* (Virginia State Chamber of Commerce), 1954.

A central challenge of postwar 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, and 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 1873, 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 governed two factors: (1) the legal ability of existing municipalities to expand their boundaries into surrounding territory; and (2) the ease with which surrounding areas could incorporate into municipalities of their own and thereby protect themselves 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 1873 slowly shifted states, particularly in the Northeast and Midwest, toward 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. Figure 8 maps an index of how loose each state's annexation law was in 1958, which we code from their state-by-state descriptions. 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 1873 annexation of Brookline: 6,291 of 7,775 voters in Boston approved Brookline's annexation, but 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 a policy context that had been shaped by annexation debates earlier in the 20th century. Therefore, only city leadership in certain states found itself capable of annexing. Nevertheless, there was a resurgence in the popularity of annexation. City leaders made the case in fiscal terms. Stanley Draper, the longtime head of the Oklahoma City Chamber of Commerce, argued that the city "had to expand or be hemmed in by small satellite communities" and that "with annexation, the city tax base would continue to expand, and the city could control wealthy suburbs and outlying industries."[4] Decades later, Tucson's former mayor framed the same argument as one of fairness: "The city is the loser when it can't tax the unincorporated area, and the tax base is restricted... The City of Tucson resident is without a doubt subsidizing the lifestyle of the unincorporated residents."[5] 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 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 postwar period in the history of annexation in the United States for two reasons. The first is practical: data on municipal finance and boundary changes

[4]Smallwood (1977). Draper was managing director of the Oklahoma City Chamber of Commerce from 1930 until his retirement in 1968.

[5]Lew Murphy, mayor of Tucson from 1971 to 1987, quoted in Devine and McKasson (1997).

become readily available only in the postwar period. The second is that, as argued above, this period was an important one 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 postwar period, with cities adding dozens and sometimes hundreds of square miles in the span of a couple of 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 177 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 US municipalities in the postwar period.

A. Annual Municipal Annexation and Population City Panel

Annexations

Our annexation data come primarily from the Municipal Year Books.[6] From 1948 to 1967, 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.[7]

From 1970 to 1980, we use the annual Boundary and Annexation Survey (BAS), which records the effective date and square mileage of each annexation and detachment but not population. We recover an estimate of annexed populations from the 1977 County and City Data Book and the Census Bureau's Current Population Reports for 1977.[8]

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

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

[8]These sources give each city's 1970 population on its 1975 and 1977 boundaries. Comparing them to

Finally, using data from the Municipal Year Books and the Decennial Censuses, we measure the total land area of cities in five anchor years: 1944, 1950, 1960, 1970, and 1980. We use these anchor years to identify cities for which we miss significant land changes, and fill the missing annexations from cities' own dated annexation records, archival records, and newspapers.[9] Appendix A. describes how the sources are combined and checked against the anchor years.

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 seven consolidations in our sample that occurred between 1944 and 1980: Baton Rouge, Newport News, Nashville, Jacksonville, Indianapolis, Columbus (GA), and Lexington.

Of the 177 sample cities, 136 annexed land between 1944 and 1980. The median annexing city added 30 square miles, more than tripling its 1944 land area. In total, these cities added 9,526 square miles over the period, with annexation peaking in 1968, when annexing cities collectively added 915 square miles. Jacksonville's consolidation with Duval County accounts for 717 of these; excluding consolidations, annexation peaked in 1960 at 572 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), drawn from the County and City Data Books and the NHGIS place time series, and on 32 hand-collected

the 1970 Decennial Census count, we infer the population annexed between 1970 and 1975 and between 1975 and 1977.

[9]Missing annexations stem from two sources. First, the annual survey does not cover annexations in 1944–1947 or 1968–1969. Second, a few large cities were either not respondents in the annual survey or provided incomplete information in some years. We add dated annexation records from municipal ordinance lists and GIS layers for 51 cities (Table B.1) and archival and newspaper records for 28 cities.

figures for individual sample cities in non-census years, mostly special censuses.[10] We also measure the population added to each city in each year, primarily from two Census Bureau reports that tabulate the population of areas annexed to central cities in 1950–1960 and 1960–1970, and for the 1970s from the sources described above (Appendix B.). For each city, we then estimate the growth-rate path $\{\alpha_t\}$ 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.

To compare cities with their metropolitan areas, we sum decennial county populations from NHGIS, 1940–1990, over the counties of each city's metropolitan area, holding the set of counties fixed at the 2013 Office of Management and Budget definitions.

City Sample Selection

We restrict our sample to cities that were the largest incorporated place in their county at the 1950 Census, had a population of at least 75,000 in 1990, and had a population of at least 25,000 in 1950, corresponding to 177 final cities. Appendix A. describes the construction of the sample.

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 C. 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. Tract-based boundaries are available for 68 of our sample cities in 1950 and 104 in 1960. For cities with available tracts, we use information in the tract name file to identify which tracts

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

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).

1950 Boundaries from Other Sources

Tracts do not cover every city, so we draw 1950 boundaries from three further sources: dated annexation polygons in cities' own GIS layers, whose union up to 1950 is the city's 1950 boundary; the enumeration-district maps the Census Bureau drew for the 1950 census, on which we trace the city limit by hand; and the 1940 enumeration districts, for cities whose land area was unchanged between 1944 and 1950. For each city, we adopt the boundary from the most direct source whose land area matches the published 1950 figure. This yields measured 1950 boundaries for 158 of the 177 sample cities.

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 Table B.1.

Neighboring Municipalities in 1950

To measure how hemmed in each city was by other municipalities in 1950 (Section A.), we map every municipality around it. We start from the 1980 place boundaries and, in the nine states where townships and boroughs rather than places are the units that block annexation, substitute

county subdivisions from the Census TIGER files. We then cut each boundary back to the ground the municipality held in 1950, using the geocoded households of the 1940 full-count Census, and take water from the TIGER water layer. Appendix A. describes the construction.

Summary of Constructed Datasets

We use these data sources to construct two datasets that allow us to track how the composition and population of cities defined under stable 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 the intervening decades. To construct this, we intersect each city's 1950 and 1980 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 county into three parts: (i) the core, the city's 1950 boundary; (ii) the annexed ring, its 1980 boundary less the core; and (iii) the remainder of the county outside the 1980 boundary. The core and ring are fixed polygons, so what changes across decades is only the tract vintage they are measured with.

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 1946 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. For county governments and for all local governments within each county, we use the Census of Governments, conducted every five years from 1957. We extend the city series back to 1946 with newly

digitized records from an earlier Census survey of all cities with populations above 25,000 in 1940.[11]

County Government Finances by Type of Government

To divide county spending among layers of government, we use the 1957 and 1982 Censuses of Governments. The 1982 Census reports the spending of every local government; for 1957, we digitize the Census table of local government finances in the central county of each standard metropolitan area, which reports spending by type of government (United States Bureau of the Census 1959).[12] Our county sample consists of the 90 counties of the instrument sample that were the central county of a 1957 metropolitan area outside New England, where the table reports towns rather than counties. We drop the city–county consolidations of the period and counties where the central city held 90 percent of residents in 1957, because in those counties the city and county layers cannot be told apart.

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 provision.

Fire Departments. From the Municipal Year Books, we digitize annual surveys of city fire departments covering 1950 through 1967, plus 1970. The surveys record staffing, salaries, and apparatus (pumpers and ladder trucks, both service and aerial), 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. Two editions, 1954 and 1966, also

[11] 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.

[12] We read each cell of the 1957 table three times, with optical character recognition and two vision models, and take the value at least two readings agree on; the layers then add up to the printed total in 99 percent of rows.

count each city's fire stations, and the 1950–1953 and 1955–1961 editions report fire losses.

Police Departments. From the same Municipal Year Book surveys, we digitize measures of police staffing, salaries, hours, and patrol equipment for 1950–1968, plus 1970, including the number of patrol cars and the shifts on which a city runs one-man patrol cars. For Milwaukee, we also digitize the police department's annual reports for 1940–1970, archived by the Milwaukee Police Historical Society, which report staffing, patrol cars, area, population, and complaints investigated for each police district.

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 resident for murder (including negligent manslaughter from 1958 onward), aggravated assault, burglary, auto theft, and robbery, which we combine into a single crime index.

Sewerage. We measure the scale and quality of city sewerage systems from two federal inventories of municipal waste facilities, the Public Health Service's for 1957 and the Federal Water Quality Administration's for 1968, which we digitize. Each lists every community sewer system with the population it serves, its level of treatment, its sewer type, and its design capacity (Appendix H.).

Property Tax Rates and Total Assessed Valuations

We record municipal tax rates, assessed valuations, and assessment ratios from Moody's Municipal Manuals, which begin in 1954. Some entries in the early editions report prior years, which extend the series back to 1952 for a subset of cities. 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

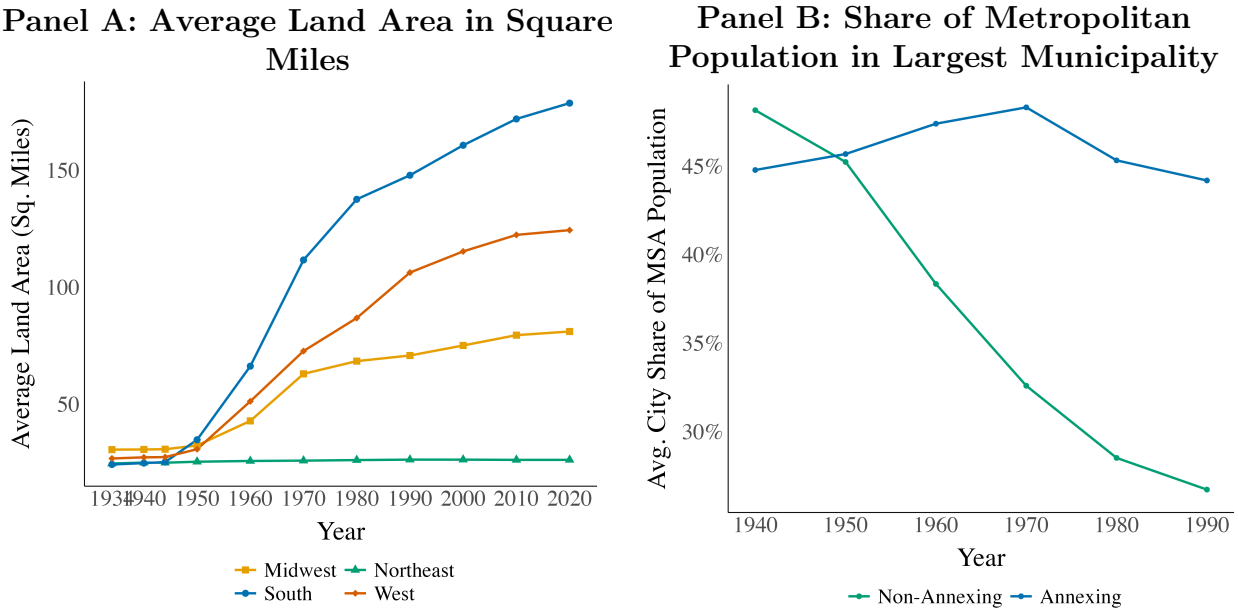
We measure economic activity using County Business Patterns data, 1947–1990 (Eckert, Fort, et al. 2021; Eckert, Lam, et al. 2022), which report employment and establishment counts by industry for every county. We focus on total, manufacturing, and service employment and on total establishments. As a proxy for local population growth, we also use annual county births to residents from the county natality files of Bailey et al. (2018). We match counties to cities using each city's home county, and drop the small number of cities spanning multiple counties from county-level analysis. We pair these with annual county populations, which we interpolate between the 1940, 1950, 1960, and 1970 censuses using the timing of within-decade state population changes and take from Census Bureau intercensal estimates thereafter.

IV. Descriptive Facts on Postwar Annexation and Municipal Finance

Postwar Annexation

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

Figure 1: Municipal Annexation by Region



NOTES: Panel A reports average city land area by census region for a balanced panel of 156 cities. Panel B reports the share of metropolitan population living in the largest municipality, separately for metropolitan areas whose central city annexed and those whose central city did not. An annexing city is one that annexed more than 10 square miles between 1950 and 1980. All other sample cities are non-annexing. The sample is the largest city in its county in 1950 with at least 75,000 residents in 1990 and 25,000 in 1950.

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 sharp increase was, however, regionally uneven. Panel A of Figure 1 shows a regional divergence in municipal land area, despite similar baseline areas, with rapid growth among Southern cities and virtually none among Northeastern cities. The divergence likely reflects how easily cities could annex, not whether they wanted to: cities everywhere faced suburbanization shocks, but only cities in states with permissive laws and nearby unincorporated land could respond by expanding. Midwestern and Western cities grew more modestly, and 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. Metropolitan areas whose central city annexed and those whose central city did not were similar in 1940, with just under half of metropolitan population in the central city. Over the following five decades, they diverged: annexing cities kept a steady share of metropolitan population, while for non-annexing cities the share fell by nearly half. As shown in Table 1, the divergence is due to changing boundaries: annexing cities held their share by extending their city limits to encompass dispersing population, not by retaining residents within a fixed boundary.

Demographic and Population Changes across Stable Geographies

Annexation changed the population and the demographic composition of a city directly, by absorbing suburbanizing territory. In Table 1, we follow population and demographic change across cities with boundaries fixed at their 1950 extent. 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, annexing and non-annexing cities appear similar: both lost roughly a fifth of their residents, and in both the white share of the core fell by about a quarter. Real family income in the cores rose, but at about half the national rate.[13]

Postwar decline in urban cores was national, but in published city statistics it is masked by

[13]The national figures beside each panel header of Table 1 are built from the County and City Data Books, which cover every county, over the 3,091 counties reported in both 1950 and 1980. The 1950 county sum reproduces the published 48-state population of 150,697,361 and the 1980 sum the published 226,545,805.

Table 1: Characteristics of Annexing vs Non-Annexing Cities, 1950 to 1980

	1950 Boundaries			1980 Boundaries	
	1950	1980	% Change	Annexed	Total
Panel A: Population (national +49%)					
Non-Annexing	317,463	250,581	-21%		254,163
	(397,479)	(289,926)			(288,499)
Annexing	253,024	210,064	-17%	163,153	373,218
	(172,514)	(171,750)		(173,507)	(318,683)
Panel B: Share White (national -7%)					
Non-Annexing	0.93	0.71	-24%		0.71
	(0.07)	(0.21)			(0.21)
Annexing	0.85	0.65	-24%	0.81	0.71
	(0.12)	(0.16)		(0.14)	(0.13)
Panel C: Median Family Income (national +119%)					
Non-Annexing	36,588	59,860	64%		60,231
	(5,128)	(11,374)			(11,350)
Annexing	35,387	55,178	56%	78,606	64,897
	(5,039)	(8,962)		(9,635)	(8,340)
Panel D: Housing Stock (national +91%)					
Non-Annexing	96,034	103,725	8.01%		105,204
	(117,080)	(119,271)			(118,684)
Annexing	80,134	89,849	12%	64,001	153,850
	(55,031)	(74,381)		(70,569)	(133,451)
Panel E: Median Home Value (national +93%)					
Non-Annexing	100,564	146,244	45%		147,255
	(25,349)	(87,069)			(87,020)
Annexing	85,641	131,614	54%	186,890	153,408
	(14,429)	(54,923)		(53,109)	(51,422)
N	Non-Annexing: 59, Annexing: 114				

NOTES: This table shows characteristics of city demographics between 1950 and 1980, reported separately by a city's future annexation status. An annexing city is one that annexed more than 10 square miles between 1950 and 1980. Columns 1 and 2 report cross-city average demographics within the city's 1950 borders, held fixed at that extent, and column 3 is the change in the group mean between them. Columns 4 and 5 report 1980 demographics within the territory annexed between 1950 and 1980 and within the city's whole 1980 extent. Characteristics within the 1950 boundary and within annexed territory are aggregated from census tracts weighted by area overlap. Income is median family income. Annexing cities are reweighted so that their mean log 1950 population matches that of non-annexing cities. National figures in the panel headers are from the County and City Data Books. Standard deviations are in parentheses. Values in 2019 dollars.

the compositional changes due to annexation. By 1980 close to half the population of the average annexing city lived in annexed territory, and that territory was whiter and over a third richer than the historic core.

Table A.1 in the appendix further splits the county into three geographic parts and follows each across the decades: the 1950 core, land annexed by 1980, and the rest of the county. This exercise covers the annexing cities whose core and annexed ring were both well covered by 1950 census tracts.[14] Before it was annexed, the ring was whiter than the core, far less dense, and newly built, with about half of its 1950 housing stock built in the previous ten years.[15] It was not, however, richer—in 1950 all three parts of the county had similar incomes. Over the following three decades the core and the ring diverged. While the core lost population and white residents, the ring grew more than fivefold, remained whiter than the core, and by 1980 its median family income was about two-fifths above the core's. However, by the end of the period (1970–1980), the county outside the annexed ring grew faster in population and incomes than the ring itself, while the ring's median family income fell.

Trends in Postwar Municipal Finance

The postwar population boom, widespread suburbanization, and rising incomes were accompanied by a transformation in the scale of municipal government spending. Figure 2 shows differences in municipal finance trends by future annexation status, where Panel A compares annexing and non-annexing cities reweighted to match on 1950 characteristics.[16] Across all cities, spending per resident nearly tripled between 1952 and 1982, growing more than twice as fast as the family

[14]The exercise covers cities whose annexed territory held at least 5,000 residents in 1950 and at least 50% of whose annexed land or annexed population fell inside a 1950 census tract. Because larger cities were tracted earlier, these are among the largest annexing cities.

[15]The 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 annexed white suburbs to preserve a majority-white electorate. Atlanta's "Plan of Improvement," which took effect in 1952, 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/`.

[16]Annexing cities are those that annexed more than 10 square miles between 1950 and 1980. Weights are overlap weights from a logit of annexing status on eight covariates: a Sun Belt indicator, the logs of 1950 population and land area and of 1951 general expenditure per resident, the city's share of county population in 1950, and its 1950 Black share, manufacturing share, and mean occupational score. Bars are plus or minus one bootstrap standard error.

incomes of city residents. The rise in costs is not driven by an expansion in government scope: spending per resident rose across nearly all functions, including core services such as fire protection and sanitation, whose growth was similar to that of the overall government budget. Because cities are required to maintain balanced budgets, net of debt financing, growth in revenue per resident mirrors spending (Appendix Figure A.2). Revenue streams compositionally shifted away from taxes directly on residents, as intergovernmental transfers explain over two-fifths of the increase in revenues, split roughly evenly between state aid and other intergovernmental aid.

Cities that annexed over the period had slower growth in overall spending per resident and no higher cumulative capital outlay per resident, while the incomes of their residents grew at a similar rate. These descriptives are consistent with lower costs of municipal services. These patterns are also mirrored when spending is broken out by function. Fire protection, organized around stations and companies with large fixed costs, grew more slowly in annexing cities, whereas sanitation, which scales with the households and network served, grew at the same rate in both groups. Panel B shows that the pattern also holds along the intensive margin: among annexing cities, those that had larger proportional increases in land area due to annexation had lower growth in expenditures.[17]

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 complementary research designs: a staggered event study using sharp annexation events and an instrumental-variables design using the relative availability of nearby unincorporated land and the permissiveness of state annexation law.

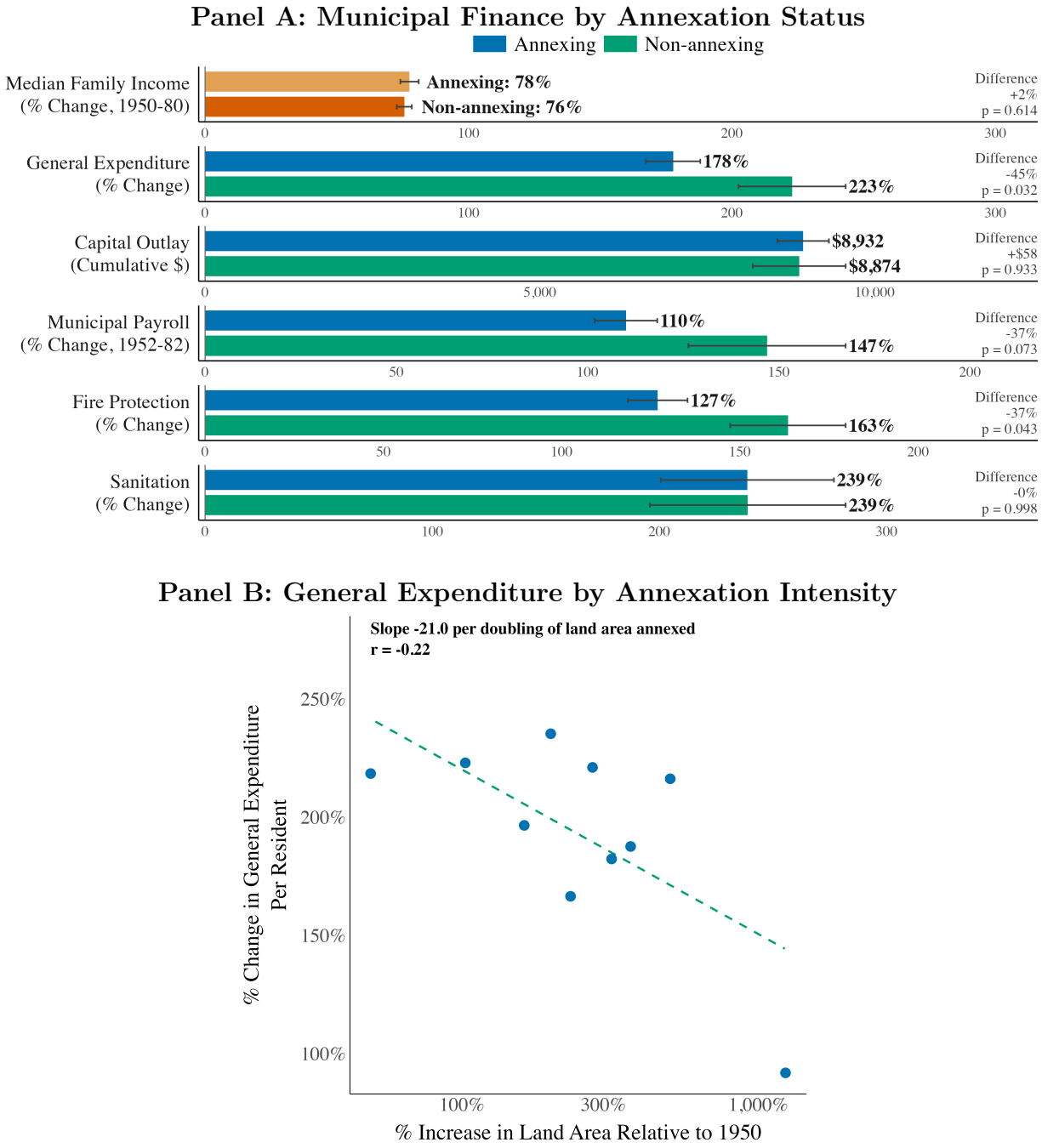
V. The Effect of Annexation Events on City Finances and Service Provision

A. Empirical Strategy

Using a staggered difference-in-differences design, we estimate the impact of jurisdictional expansions on municipal finance, public good provision, and local economic activity. The design uses

[17]Figures A.3 through A.6 in the appendix repeat Panel B for capital outlay, debt issued, fire protection, and sanitation.

Figure 2: Municipal Finance and Annexation, 1952 to 1982



NOTES: Panel A compares the growth of municipal finances per resident in annexing and non-annexing cities, reweighted to match on eight baseline characteristics. The right column of Panel A is the difference and its p-value. Capital outlay is cumulated over 1952–1982; the other rows are percent changes. Panel B plots growth in general expenditure per resident against land-area growth for annexing cities, in 10 equal-count bins. Bars are plus or minus one bootstrap standard error. Spending excludes utilities. Values in 2019 dollars.

sharp, concentrated changes in municipal land area to compare annual outcomes around annexation, relative to non-annexing cities in the same census region and year:

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

Here, 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 the start of a city's annexation spurt ($K_{it} = t - E_i$), with non-annexing and not-yet-annexing cities serving as controls. We implement Equation 2 through local projections (Dube et al. 2023), and summarize each outcome by its pooled ATT, the average of the β_k over event years 5 to 10. To credibly identify the effects of annexation, we restrict treatment to cities undergoing sharp, large annexation spurts lasting up to five years.[18] Due to data availability, we restrict our analysis to spurts beginning between 1952 and 1976. Appendix B. details the full criteria, including the eligibility of not-yet-annexing cities to enter estimation. These criteria yield 49 treated cities; Appendix Figure C.1 shows two example spurts, from Atlanta and Oklahoma City.

For a subset of outcomes, including many of our measures of the level and quality of service provision, we observe only infrequent survey years. For these outcomes, we estimate a stacked long difference:

$$Y_{i,q_s} - Y_{i,p_s} = \delta D_{is} + \lambda_{s,r(i)} + \varepsilon_{is}, \tag{3}$$

where p_s is the last survey year before the spurts of the treated cities in sub-experiment s and q_s the survey year after them, D_{is} indicates a treated city, and $\lambda_{s,r(i)}$ are sub-experiment-by-region

[18] A land spurt is a five-year window that adds at least ten square miles, including at least 1.5 square miles in its first year, and at least 5,000 residents. It must account for over half of the land the city annexed between ten years before and eight years after its start, at least 85% of the land annexed in the window and the nine years preceding it, and at least 70% of the population annexed over that same span. A population spurt annexes at least 10,000 residents, including at least 1,000 in its first year, and meets the same conditions with land and population reversed; it must also account for at least half of the land annexed between ten years before and eight years after its start. Additionally, between 1940 and 1980, treated cities must have annexed either at least 15 square miles while growing their borders by more than 20%, or more than 10 square miles while more than doubling their land area. For cities with multiple eligible spurts, we select the earliest. We exclude spurts whose concentration does not survive charging the city's unrecorded land growth, measured from its land area in census years, to the nine years before the spurt, and spurts whose general expenditure is not observed in nearly every year from six years before to ten years after the spurt. Appendix Figure C.4 moves each of these thresholds in turn, and the estimates change little.

fixed effects. Control cities in each sub-experiment are those observed in both years that remain untreated and eligible through q_s. With one year on either side of the spurt, this specification cannot test for parallel trends leading into annexation or trace post-annexation dynamics.

We argue that these sharp annexation events provide a credible design, for three reasons. First, while annexation is a strategic policy choice pursued by city governments, most central municipalities wanted to expand, and whether they ultimately could depended more on state law and on past patterns of municipal incorporation than on the city's own wishes.[19] Second, even conditional on ever annexing, city leaders often had weak control over the timing of annexation, which was set by political constraints and by when legal challenges cleared the courts. As an example, Norfolk and Atlanta both waited years for their eventual annexations: Norfolk's annexation court refused the city's 1952 suit, and Norfolk added territory only in 1955 and 1959, after renewed proceedings; Atlanta's earlier efforts to expand failed, and the city expanded only after its leaders lobbied the state legislature to pass the 1951 Plan of Improvement.[20] Third, by focusing on sharp, large events paired with annual outcomes, we can more credibly separate the effects of annexations from other slow-moving differences across cities. These annexations were concentrated shocks: the median treated spurt added 28.4 square miles (a 143% increase in land area) and roughly 47,300 residents (a 34% increase in population).

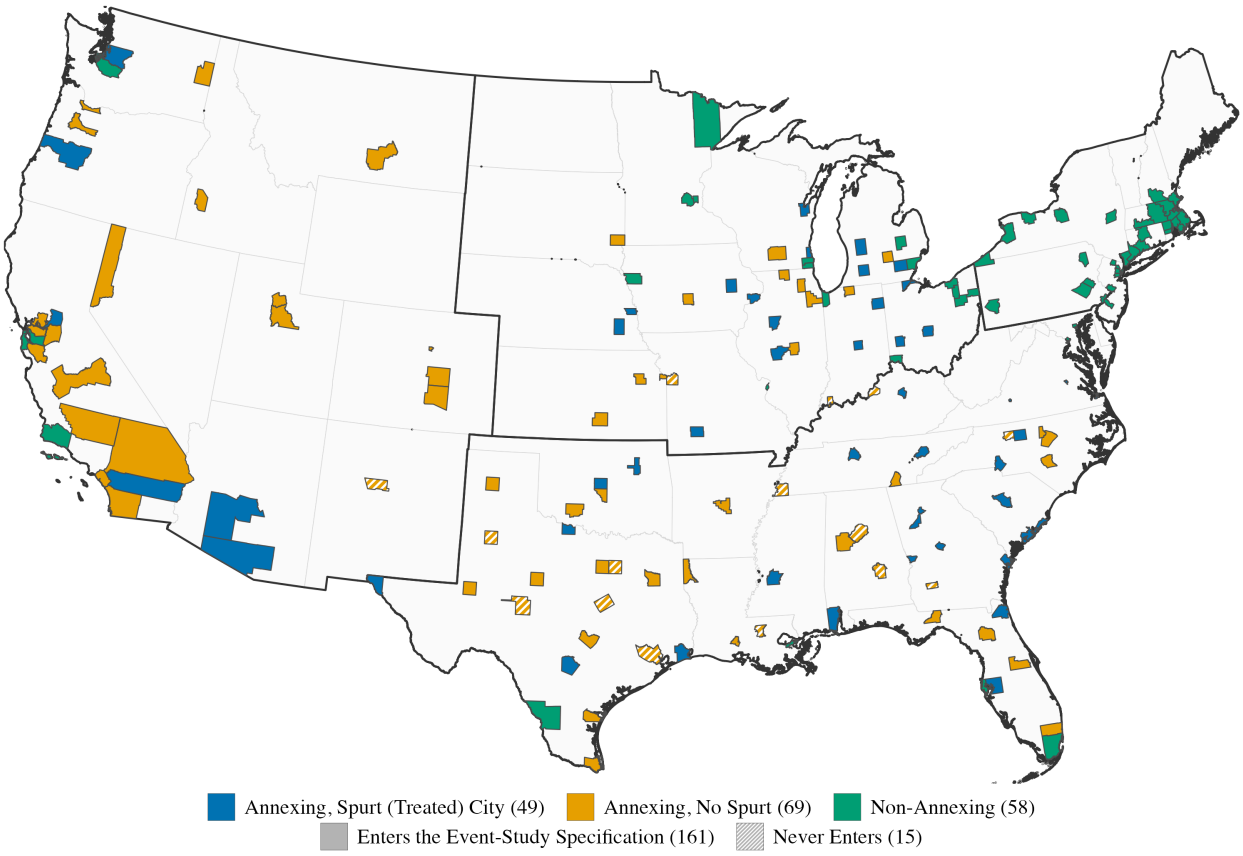
Figure 3 maps annexing and non-annexing cities and identifies those entering estimation as treated and control cities, while Appendix Figures A.7–A.9 show, for the South, Midwest, and West (no Northeast city is treated), the years each sample city contributes to the pool of control cities. Some annexing cities do not annex in a clear spurt, but are eligible as control cities until their first large annexation. The map shows that treated and control cities are not geographically random, as annexations were most common in the South (26 of our 49 treated cities are southern). This geographic non-randomness in annexation motivates our design to make all comparisons within

[19] In a 1957 survey of city managers, most rated the fringe's importance to "the growth and prosperity of the city," and the case that fringe residents "should bear part of the cost" of the city services they used, as major reasons to annex (Dixon and Kerstetter 1959).

[20] On Norfolk, see Bain (1966); *City of Norfolk v. County of Norfolk*, 194 Va. 716 (1953); and *City of Norfolk v. County of Princess Anne*, 200 Va. 105 (1958). The court's final order refusing annexation from Norfolk County is dated May 6, 1952; the renewed proceeding took effect on January 1, 1955, and the suit against Princess Anne County took effect on January 1, 1959. Atlanta's Plan of Improvement took effect in 1952.

region, so as not to attribute regional differences and regional shocks to the effects of annexation.

Figure 3: Sample Cities by Annexation and Event-Study Status



NOTES: Each shaded county contains one sample city. Blue counties contain a treated city, orange counties an annexing city without a dated annexation spurt, and green counties a city that did not annex. Hatched counties do not enter the population event study. Honolulu is not shown.

Columns 1–3 of Appendix Table A.2 compare treated (spurt) cities, non-treated annexing cities, and never-annexing cities, while column 4 compares treated cities to only their eligible control cities: same-region cities that are never-annexing or not yet treated.

A.a. Discussion of Assumptions for Empirical Design

We now discuss how the standard difference-in-differences assumptions apply in our setting.

Parallel trends. Absent annexation, a treated city's outcomes must have followed the same growth path as those of untreated cities in its census region (parallel trends). While our estimates

show parallel trends in the years before annexation, a first-order concern is that cities annexed in years when policymakers anticipated a different growth path. As previously emphasized, many control cities faced similar incentives to annex but were barred from doing so by state law, and even cities that did annex often had weak control over timing. As the Norfolk and Atlanta examples show, the timing of individual annexations was often set by courts and state legislatures rather than by the city's own growth path.

Anticipation. Cities may respond before the annexation itself. If the process unfolds over several years, or city leaders know they intend to annex, cities may adjust tax rates and shift spending priorities in anticipation of the new land area and population entering the city. Some annexations did unfold over several years, but many—particularly in states with liberal annexation policies—happened very quickly, and where annexations faced legal challenges, city leaders may have been reluctant to move before court approval. We address the concern by distinguishing outcomes cities could easily change in advance, such as statutory rates and spending allocations, from those they could not, such as aggregate revenue conditional on a statutory rate.

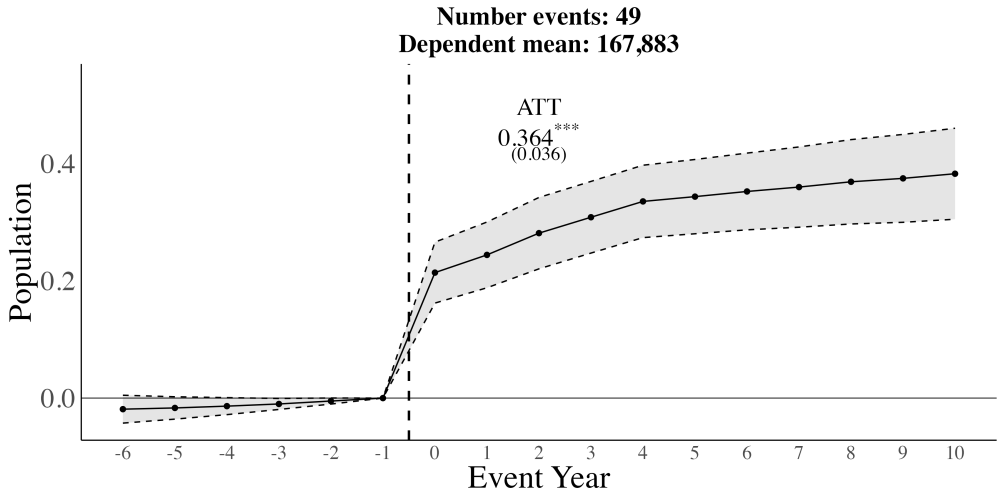
Finally, annexation should not coincide with other shocks: cities must not be annexing as part of a suite of policies or alongside a new government, whose priorities would otherwise be attributed to the annexation. Some measures, such as the property tax cuts that made Atlanta's expansion palatable to residents, were passed as a bundle with annexation, and we consider these part of the intended treatment effect. The concern is the reverse case, where annexation accompanies a broader municipal reform it did not drive.

B. Results

We estimate the effects of municipal scale on local public finance and service provision for 49 sharp annexation events. These annexation spurts were sizable shocks to the land area and population of cities. Figure 4 visualizes the event study estimates, which act as a quasi-first stage for the scale effect of annexation: relative to control cities, the population of the average treated city rises by 44%. In the years prior to annexation, we observe no pre-trend, suggesting that control cities in the same census region grew similarly to cities that annexed. After annexation, there is an immediate

mechanical jump in population that extends for up to five years (the length of the spurt), after which the estimates stabilize.

Figure 4: Log Population Event Study Estimates



NOTES: This figure reports event-study estimates of annexation's effect on log city population from Equation 2. Coefficients are relative to event year −1, and shaded bands are 95 percent confidence intervals. The pooled ATT averages event years 5 to 10. Standard errors are clustered at the city level.

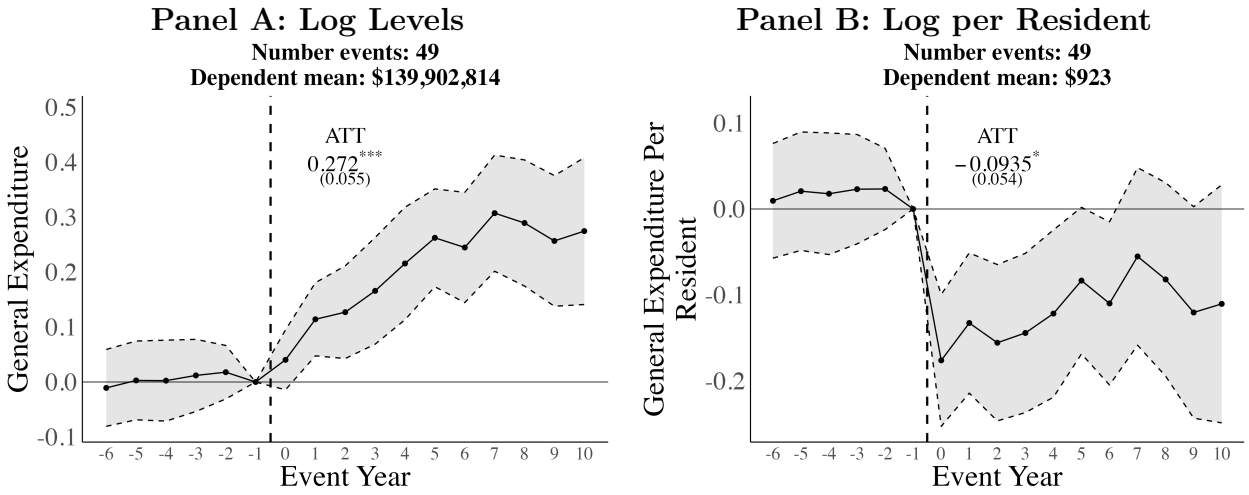
C. Municipal Finances and Service Provision

How did these sharp increases in municipal scale affect local public finances? Figure 5 shows the impact of annexations on overall municipal expenditure, with Panel A in log levels, which captures the aggregate impact on total spending. As expenditure increases by 31% overall, this result demonstrates that as cities added population and land, they also expanded government functions to accommodate these newly added areas. Panel B then shows the effects per resident, which can be interpreted as the shift in the average cost of providing services accounting for both incumbent and newly added residents. Here, we see 9% lower average spending for cities that annex relative to those that do not, and this effect persists a decade after annexation. Per-resident costs are rising in both annexing and non-annexing cities (Appendix Figure A.10), so this effect reflects a slowdown in rising costs but not a reversal in the trend.

Neither effect depends on the particular thresholds that define a spurt. Appendix Figure C.4 moves each threshold of the treatment rule in both directions, one at a time, from the window

length and the concentration bars to the size floors and Toledo's hand dating. Across these 19 variants, the pooled population effect ranges from 0.32 to 0.42 log points, and general expenditure per resident falls in every variant, though it is less precisely estimated in some. Both effects also keep their signs under a stricter rule for control cities, a more conservative set of treated cities, and a comparison with not-yet-treated cities alone (Appendix E.).

Figure 5: Impact of Annexation on Municipal Expenditures



NOTES: This figure reports event-study estimates for general expenditure from Equation 2, in log levels (Panel A) and log per resident terms (Panel B). Coefficients are relative to event year −1, and shaded bands are 95 percent confidence intervals. The pooled ATT averages event years 5 to 10. Standard errors are clustered at the city level. Values in 2019 dollars.

As this effect characterizes the impact of the average annexation on expenditure, we can use the population effect from Figure 4 to construct elasticities of municipal expenditure with respect to population:

$$\varepsilon_h = \frac{\beta_h^{\log \text{Exp}}}{\beta_h^{\log \text{Pop}}}, \tag{4}$$

where the elasticity is the equivalent of taking the coefficient of Panel A of Figure 5 over the coefficient of Figure 4. Alternatively, it can be estimated directly by instrumenting with annexation:

$$\Delta_h \log \text{Exp}_{it} = \varepsilon_h \, \Delta_h \log \text{Pop}_{it} + \gamma_{r(i),t} + u_{it}, \tag{5}$$

with $\Delta_h \log \text{Pop}_{it}$ instrumented by annexation. In Column 1 of Table 2, we present these effects as elasticities with respect to annexation-induced population, overall and by function. These effects imply that a 10% increase in population through annexation corresponds to only a 7.4% increase in overall spending.

Table 2: Elasticities of Municipal Spending with Respect to Annexation-Induced Population

	Single regressor	Two regressors		
	Population	Scale, density fixed	Density, population fixed	Treated cities
	(1)	(2)	(3)	(4)
Panel A: Aggregates				
General expenditure	$0.745^{\dagger}$	$0.601^{\dagger\dagger\dagger}$	-0.114	49
	(0.143)	(0.099)	(0.070)	
General expenditure net of education	$0.781^{\dagger}$	$0.614^{\dagger\dagger\dagger}$	-0.102^{*}	49
	(0.125)	(0.084)	(0.058)	
General current expenditure	$0.679^{\dagger\dagger}$	$0.567^{\dagger\dagger\dagger}$	-0.077	49
	(0.143)	(0.091)	(0.073)	
General revenue	$0.781^{\dagger}$			49
	(0.133)			
Panel B: By function				
Fire protection	$0.527^{\dagger\dagger\dagger}$	$0.372^{\dagger\dagger\dagger}$	-0.026	49
	(0.082)	(0.093)	(0.054)	
Police protection	$0.636^{\dagger\dagger\dagger}$	$0.660^{\dagger\dagger\dagger}$	0.071^{*}	49
	(0.081)	(0.087)	(0.043)	
Sanitation	1.198	1.036	-0.072	49
	(0.204)	(0.236)	(0.116)	
Highways	1.030	0.880	-0.041	41
	(0.208)	(0.177)	(0.110)	
Parks and recreation	$0.506^{\dagger\dagger\dagger}$	$0.541^{\dagger\dagger}$	0.032	49
	(0.180)	(0.228)	(0.117)	
Financial administration and general control	$0.443^{\dagger\dagger\dagger}$	$0.467^{\dagger\dagger\dagger}$	0.018	37
	(0.161)	(0.105)	(0.093)	
General public buildings	1.088	$0.426^{\dagger\dagger\dagger}$	-0.203	39
	(0.421)	(0.213)	(0.185)	
Libraries	$0.292^{\dagger\dagger\dagger}$	0.669	0.149	39
	(0.236)	(0.510)	(0.261)	

NOTES: This table reports elasticities of city spending with respect to municipal population, scale, and density. Panel A reports estimates for aggregate government spending, and Panel B reports elasticities by government function. Column 1 reports the elasticity of each spending category with respect to population (Equation 5). Columns 2 and 3 report the jointly estimated elasticities with respect to scale and density (Equation 6). All estimates are pooled over event years 5–10. Appendix Figure A.11 reports the event study for each function in Panel B. Financial administration and general control excludes the four cities that consolidated with their county during their treatment window. Standard errors, clustered at the city level, are in parentheses. Columns 1 and 2 are tested against one, the value at which spending rises in proportion to population: $^{\dagger}p < 0.1$, $^{\dagger\dagger}p < 0.05$, $^{\dagger\dagger\dagger}p < 0.01$. Column 3 is tested against zero: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

However, as annexations varied in both the scale and population density of areas added to

the city, we consider whether these characteristics of an annexation shaped the overall impact on average costs in the government budget. We decompose population growth into scale and density:

$$\Delta_h \log \text{Exp}_{it} = \theta_1 \, \Delta_h \log \text{Pop}_{it} + \theta_2 \, \Delta_h \log(\text{Pop}_{it}/\text{Area}_{it}) + \gamma_{r(i),t} + u_{it}, \tag{6}$$

where the two changes are instrumented by the log change in population and in density implied by the annexation. The scale elasticity θ_1 measures the response of spending to proportional growth at constant density, and the density elasticity θ_2 measures its response to residents per square mile at constant population. Columns 2 and 3 of Table 2 show offsetting forces in the impact of newly annexed areas on the overall city budget. Holding the density fixed at the pre-annexation level, there are strong economies of scale: every 10% increase in population is accompanied by only a 5.9% increase in spending. However, lower-density annexations are more costly to serve (a 10% lower density implies 1% higher per-resident spending), and areas added to the city tended to be much lower density—even the densest annexation was less dense than its annexing core. Our estimates are therefore local to the densities that cities actually annexed, and compare denser to sparser suburban annexations: they suggest that higher-density suburban annexations are less costly than rural ones, but not that per-resident costs decline monotonically in density.

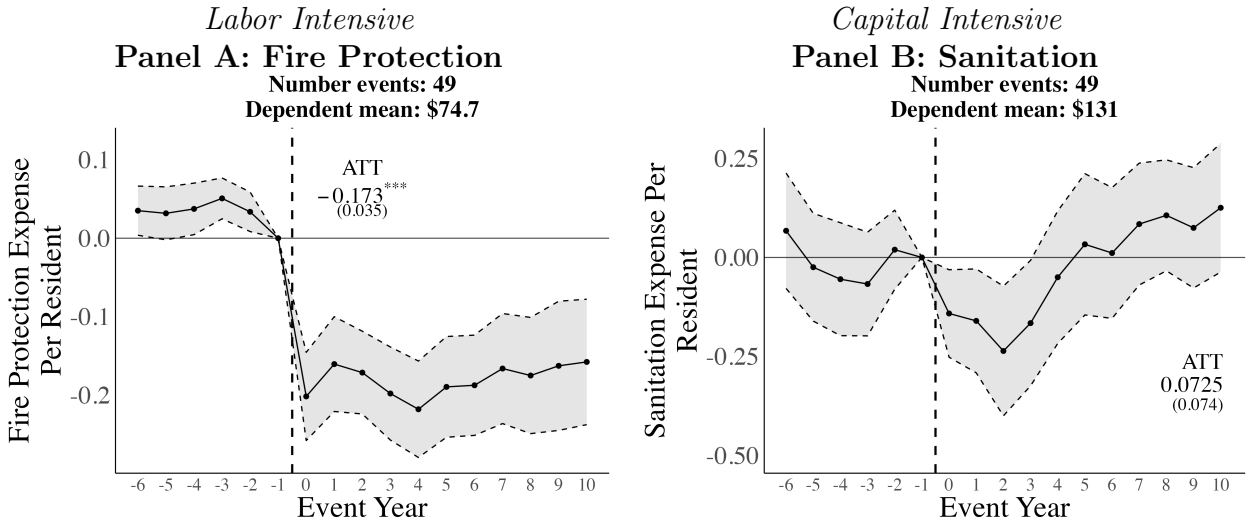
Appendix Figure A.12 illustrates these offsetting forces by comparing two cities (Charlotte and Mobile) with similar pre-annexation populations that annexed similar populations over different land areas. We predict cost savings for Charlotte (-8.5%) but not for Mobile ($+7.5\%$), and in practice costs decline in Charlotte and rise in Mobile. Generalizing across our 49 treated cities, we find that for 14 of them, the higher costs we predict from lower density more than offset the savings from scale (Appendix Figure A.13).

To understand what drives these effects, we turn to how added scale changed the cost of providing different city functions (Appendix Figure A.11 reports the event study for each function). Contemporaneous annexation planning reports show that these functions had different production technologies and therefore different cost responses to municipal scale. Fire and police departments operate out of fixed stations that each serve an area, so adding residents within a station's reach adds little to its cost. Garbage collection and street maintenance instead have to reach every

household and every street.[21]

In line with these planning reports, Figure 6 shows immediate and persistent per-resident declines in fire spending, whereas the point estimates for sanitation suggest flat or increasing per-resident costs. Panel B of Table 2 reports population, scale, and density elasticities by major municipal function. These elasticities show the same split: spending on fire and police protection, parks, and administration rises far less than proportionally with population, while spending on sanitation and highways rises roughly in proportion to it.[22]

Figure 6: Impact of Annexation on Expenditures by Function (Log per Resident)



NOTES: This figure reports event-study estimates for fire protection (Panel A) and sanitation (Panel B) expenditure per resident, in logs, from Equation 2. Appendix Figure A.11 reports every function. Coefficients are relative to event year −1, and shaded bands are 95 percent confidence intervals. The pooled ATT averages event years 5 to 10. Standard errors are clustered at the city level. Values in 2019 dollars.

We propose three main mechanisms for how municipal scale reduces average costs. First, functions run out of dispatch stations, such as fire stations and police precincts, can centralize operations or add residents to existing stations. Second, these departments undergo capital intensification, which shifts them toward inputs that scale, such as vehicles and equipment. Third, for some functions the annexed fringe was cheaper to serve per resident than the core, because it generated fewer

[21]The 1956 Greensboro planning report budgets one garbage collection route for every 750 houses, but one fire company for every 10,000 residents and one patrol car beat for every 15,000 residents (Esser, Mace, and McCallum 1956).

[22]Appendix Figure A.11 reports the event study for every function, including police and highways.

crimes and fires per resident. Cross-subsidization does not conflict with the higher cost of serving sparser areas reported earlier because it can be that for some services it was lower cost to serve the residential suburbs than the urban core, but also lower cost to serve the residential suburbs than mostly undeveloped outer areas (Ladd 1992). The fringe that cities annexed may have needed less service than the core without being so sparse that the cost of covering it dominated.

Table 3 provides evidence for each of these mechanisms using newly digitized detailed surveys of fire and police departments. We complement these findings with a case study of annual police reports from Milwaukee around its major annexations in 1953 and 1956. Annexing cities on average increased their number of fire stations by 34% over the period, an increase that amounts to roughly 13% fewer stations per resident; Milwaukee's police department consolidated in a similar way.[23] Firefighters, police officers, and fire apparatus all decline on a per-resident basis, while patrol cars per resident rise, and the ratio of labor to capital declines after an annexation. The same shift appears in Milwaukee, where the 1954 report notes that five additional squad cars were required to patrol the new areas, and where the fleet doubled between 1945 and 1956 while patrolmen rose by only half. Finally, crime and building fire incidence per resident fall sharply, and while some of this decline could reflect increased service provision, it may also reflect lower criminality and fire risk in annexed areas, which were therefore in part cheaper to serve. Milwaukee's annexing Sixth and Seventh Districts, for example, generated 62 complaints per 1,000 residents in 1956, against 159 in its four inner districts, and were staffed by 10.6 rather than 15.7 patrolmen per 10,000 residents.

We find that lower average municipal costs are not associated with a clear deterioration in the quality or level of municipal services, for the functions for which we can measure proxies of service provision, in Table 3 and, in event-study form for a smaller number of outcomes, in Appendix Figure A.14. We have the best measurement of the quality of fire departments, from the external audits of the National Board of Fire Underwriters. After an annexation, fire departments do not accumulate substantially more deficiency points or get re-evaluated into a worse fire insurance class. Fires are also rarer and less destructive: building fires per resident fall by 0.34 standard deviations, and the loss per building fire declines by 40%. As discussed previously, some of this effect is likely

[23] As the city grew from 51 to 74 square miles during its 1952–56 annexation spurt, it merged two districts into a single new station, serving the larger city with six stations instead of seven.

explained by the lower fire risk of the suburban fringe, so it is a mix of quality and compositional shift. In policing, the share of shifts that send out one-man patrol cars is flat and, if anything, declines, suggesting that police departments are not overtaxed at lower staffing levels per resident. We also see an overall decline in crime, though this decline, like that in fires, may be explained in large part by a compositional shift in the residential base of the city. Finally, in the municipal sewerage reports, service provision is flat across a number of indicators: sewage treated beyond primary treatment, pollution load removed, and residents on combined storm and sanitary sewers.

Consistent with balanced budget operations of municipalities in the United States, average revenues decrease in similar proportion to average expenditures. Appendix Figure A.15 shows event-study estimates from Equation 2 for general revenue, and Appendix Table A.3 breaks revenue down by source. The lower proportional budget level for annexing cities results in lower property tax rates, as the property tax is the primary source of revenue for municipal governments. Using new data on nominal property tax rates and total assessed valuations from Moody's Municipal Manuals in Appendix Figure A.16, we find that both the effective property tax rate—the ratio of property tax revenue to total assessed valuations—and the nominal listed property tax rate decrease relative to control cities. The rate cut benefits incumbents, whose city rate falls from 19.9 to roughly 14.7 mills; newly annexed residents, who previously paid no city property tax, now pay that rate, so the value of annexation to them is the benefit of additional city services net of these higher taxes.

VI. Metropolitan Effects: Local Finances and Economic Spillovers

In Section V., we showed that annexation was a large shock to central cities that led to an overall lower budget per resident with no measured deterioration in service provision. Given these effects, it is also plausible that the scale of annexation had spillovers onto the greater economic area, particularly since these central cities may have been of special economic importance to the firms and residents outside the city boundaries. Slow-moving and indirect effects, such as economic spillovers to the rest of the county or the reallocation of government functions across administrative units, are harder to separate from other slow-moving differences between annexing and non-annexing cities.

Table 3: Fire, Police, Sewerage, and Street Operations

	Baseline mean (1)	δ (2)	Treated cities (3)	Control cities (4)
Panel A: Fire protection				
Inputs				
Fire stations per 10,000 residents (log)	0.82	-0.143** (0.064)	23	45
Firefighters per apparatus (log)	15.5	-0.083** (0.037)	31	83
Pumpers per ladder truck (log)	3.44	0.084* (0.046)	30	82
Firefighters per 1,000 residents (log)	1.40	-0.168*** (0.026)	37	83
Apparatus per 1,000 residents (log)	0.10	-0.071* (0.039)	31	83
Pumpers per 1,000 residents (log)	0.08	-0.077** (0.038)	32	83
Ladder trucks per 1,000 residents (log)	0.03	-0.172*** (0.049)	31	82
Incidence				
Building fires per 1,000 (SD)	4.07	-0.335** (0.163)	26	83
Loss per building fire (log)	10,800	-0.503** (0.243)	14	58
Quality				
Total deficiency points (SD)	1,510	-0.011 (0.048)	37	83
Fire department deficiency points (SD)	460	0.047 (0.059)	37	83
Worse fire insurance class (share)	0.02	-0.003 (0.024)	37	83
Panel B: Police protection				
Inputs				
Officers per 1,000 residents (log)	1.46	-0.102*** (0.029)	39	83
Patrol cars per 1,000 residents (log)	0.15	0.235*** (0.065)	26	81
Officers per patrol car (log)	12.2	-0.317*** (0.065)	26	81
Quality				
Shifts with one-man patrol cars (share)	0.67	-0.096 (0.155)	29	47
Crime per resident index (SD)		-0.258*** (0.051)	39	87
Panel C: Sewerage				
Inputs				
Plants per 100,000 served (log)	2.19	0.221 (0.228)	20	39
Quality				
Sewage treated beyond primary (share)	0.67	-0.013 (0.059)	20	39
Pollution load removed (share)	0.61	-0.000 (0.111)	14	28
Load relative to plant design capacity (log)	0.61	-0.425 (0.465)	15	29
On combined storm and sanitary sewers (share)	0.40	-0.014 (0.043)	20	39
Panel D: Streets (States)				
Inputs				
Maintenance per municipal street mile (log)	1,201	-1.059** (0.434)	42	
Quality				
Local road miles with any surface (share)	0.66	0.052 (0.271)	42	

NOTES: This table reports long-difference estimates for fire protection, police protection, sewerage, and streets. Column (1) is the treated cities' mean in the year before the spurt. Column (2) reports δ from Equation 3, differencing the year before the spurt and three years after in Panels A and B and 1957 and 1968 in Panel C. Appendix F.a. defines each row and lists the rows observed in other years. Panel D is a state-level instrumental-variables long difference, in which the change in the sample cities' share of the state's urban population between 1950 and 1960 is instrumented with the log growth of their land area. Panel D controls for the outcome's base-year level and the state's 1950 urban population. In Panel D, columns (1) and (3) report the state mean in the base year and the number of states. Rows marked SD are in standard deviations. Estimates in Panels A to C are weighted by pre-year population, with standard errors clustered by city. Panel D is unweighted, with robust standard errors. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In this section, we study the impact of annexation on two broader outcomes: the overall cost of service provision across government units and overall economic activity; Appendix E studies residential sorting across different parts of the economic area. Annexation may have moved residents between layers of local government and changed which households lived inside and outside the city's limits, and it was often promoted as a way to bring businesses and residents to the region. As these effects likely develop slowly, and we do not measure most of these outcomes annually, we construct instruments for a city's capacity to annex from its geographic constraints and from state policy.

A. Instruments for Long-Run Annexation Capacity

We use two determinants of how far a city could expand its borders—the unincorporated land around it in 1950, and how permissive its state's law was toward annexation at the end of the 1950s.

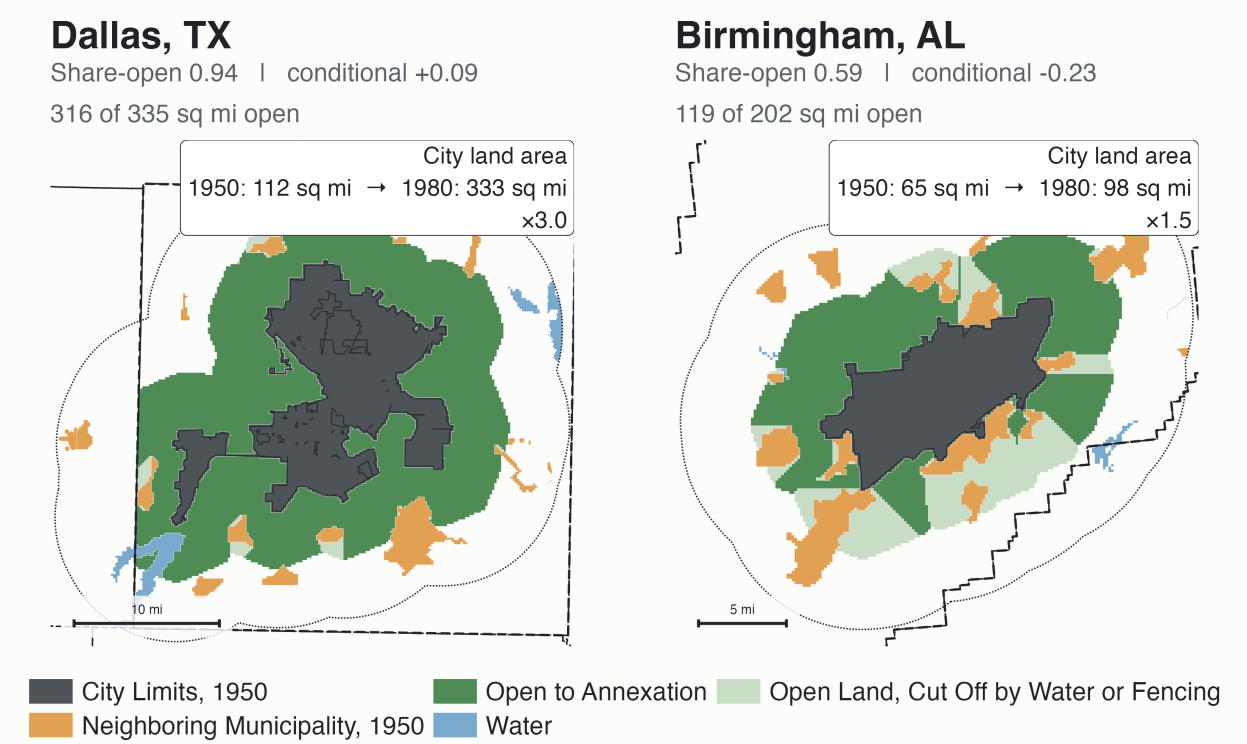
A.a. Geographic Instrument for Annexation

> *"But others have been prevented from doing so because towns in Suburbia incorporated and thereby halted their inclosure in the much larger adjacent city. Birmingham is a prime example of this. It is rimmed by a half dozen or so towns that hem the Magic City. So far it has been unsuccessful in doing anything about it. As a result, the census reported a very small increase in population for Birmingham, though the small cities and remainder of Jefferson County had large ones."* —Reese T. Amis, *Huntsville Times*, June 12, 1960

We construct an instrument for total municipal land growth from the share of nearby land that was unincorporated in 1950. As shown through an illustrated example in Figure 7, almost all annexation in this period was through absorbing unincorporated territory; land already incorporated was effectively blocked from annexation. Birmingham was fenced in by the presence of already incorporated nearby areas, while Dallas was relatively open. Despite having similar amounts of incorporated land within 8 miles, Dallas was almost entirely surrounded by open land in 1950 (share-open 0.94) and tripled its land area by 1980, from 112 to 333 square miles, while Birming-

ham, fenced in by nearby incorporated suburbs, had a share-open of 0.59 and grew its land area by only 51 percent.

Figure 7: Examples of Instrument Construction



NOTES: Each panel shows the land around a city's 1950 boundary. The dotted line bounds the band over which share-open (Equation 7) is computed, and the dashed line is the county boundary. Open land cut off by water or another municipality cannot be reached by the city. Share-open, unconditional and net of the first-stage controls, is reported above each panel, and the inset reports land area in 1950 and 1980. Panels are drawn at different scales.

Building on the intuition of Harari (2020), we define how "fenced in" a city was in 1950 by other incorporated municipalities. Within 4 miles of a city's 1950 boundary, we take the land in the city's own county that is not water (Area_i). We then compute how much of that land the central city could annex (Annexable_i). Land is not annexable if it belongs to another municipality, or if another municipality's position blocks the city from reaching it. Because water and land in other counties are left out of both the numerator and the denominator, share-open measures how much of the land a city could reach was already incorporated, not how much land there was. The

instrument is then the annexable share:

$$\text{ShareOpen}_i = \frac{\text{Annexable}_i}{\text{Area}_i}. \tag{7}$$

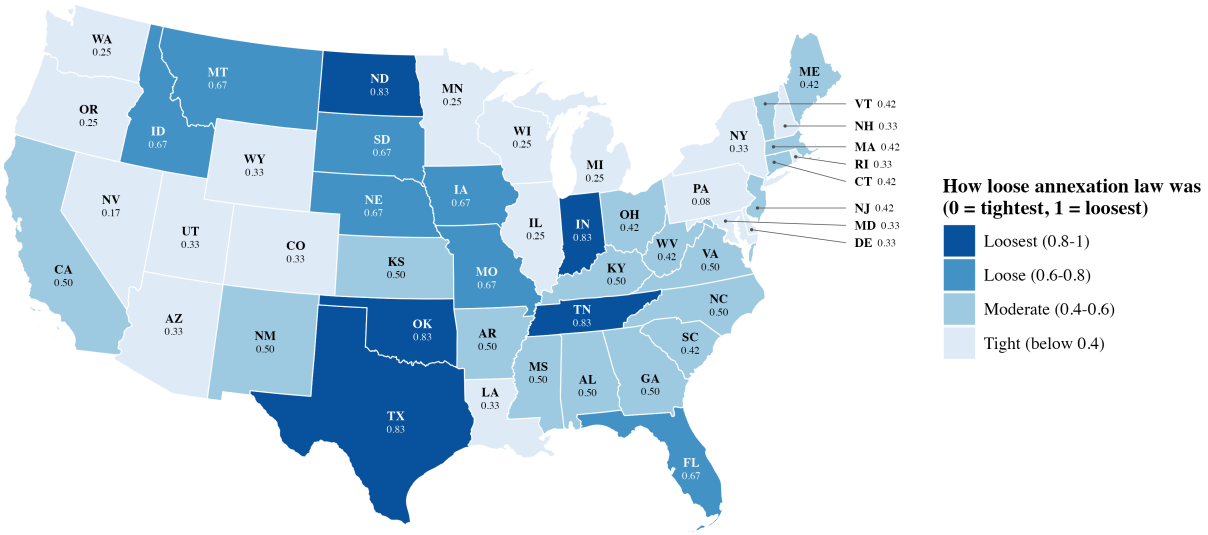
A concern of using this instrument is that patterns of incorporation in 1950 are not random and in particular reflect which areas outside of a city are already developed. This means that counties with a more developed suburban fringe in 1950 will appear more closed, and any effect we estimate may be biased by developmental convergence rather than effects of annexation. We address this concern by comparing cities in the same census region and conditioning on characteristics that capture the level of fringe development in 1950: the city's land area and population, the land area of its county and the county's population growth between 1930 and 1940, and the shares of the land within 8 miles of the city that were water, other municipalities, and other counties (Appendix A.c.). Appendix Table A.4 shows that with these controls, the instrument is largely balanced on the 1950 economic structure of the city and its county and on pre-1950 trends (Section A.c.).

A.b. Policy Instrument for Annexation

We also construct an instrument that utilizes the looseness of state annexation law as digitized from Dixon and Kerstetter (1959), who describe and interpret the statutes of all 48 states as of 1958. For each state, we take the looser of its two routes to annexation, general law or a special act of the legislature, and score three features of the law between 0 and 1: whether a city can annex without the consent of the annexed area, whether the fringe can avoid annexation by incorporating (defensive incorporation), and whether the annexed area must take on the city's existing debt. Appendix A.d. describes the scoring and the other provisions of state law we code.

Figure 8 maps the resulting index, the average of the three scores, Appendix Figure A.18 maps each score, and Appendix Figure A.19 maps the other provisions we code. As described in Section II., states in the Northeast are the most restrictive, while states in the South are the most permissive. Texas is among the loosest states because its cities could annex by council ordinance alone, and its law also restricted the incorporation of new places near existing cities, so the fringe could not escape annexation by incorporating defensively. We may be concerned that the political

Figure 8: State Annexation Law, 1958



NOTES: This map reports an index of how loose each state's annexation law was in 1958, coded from the state-by-state descriptions in Dixon and Kerstetter (1959). Each state is scored on its looser route, general law or special legislative act. The index is the mean of three scores between 0 and 1: whether a city can annex without the consent of the annexed area, whether the fringe can avoid annexation by incorporating, and whether the annexed area is shielded from the city's existing debt. Higher values are looser.

environments that lead to favorable annexation policies are also selected on the economic conditions of the state, or are accompanied by other policies that differentially shape the economic outcomes of cities. For this reason, we again compare cities within census regions and use the same baseline controls as in the openness instrument.

Combined Instrument Openness and looseness capture different constraints, and they are only weakly related: across the 134 cities, their correlation is 0.20. For instance, while the Midwest tended to have quite loose annexation laws, there are also many cities like Gary, Indiana, that are fenced in by incorporated municipalities. Conversely, while most of the South outside of major cities was unincorporated land, it was far easier to annex in Texas than in South Carolina. We combine both sources of variation into a single instrument by averaging the two after standardizing each to have mean zero and unit standard deviation:

$$Z_i^{\text{comb}} = \frac{1}{2}\left(\frac{\text{ShareOpen}_i - \overline{\text{ShareOpen}}}{\sigma_{\text{ShareOpen}}} + \frac{\text{Looseness}_i - \overline{\text{Looseness}}}{\sigma_{\text{Looseness}}}\right). \tag{8}$$

We give the instruments equal weight, and because we combine them into a single instrument, our analyses are just identified.

A.c. Instrument First Stage and Balance

For each of the three instruments, the first stage relates a city's land growth between 1950 and 1980 to the instrument:

$$\Delta \log \text{LandArea}_i = \pi\, Z_i + X'_{1950,i}\gamma + \text{Region}_i + \eta_i, \tag{9}$$

where Z_i is share-open, looseness, or the combined instrument, $X_{1950,i}$ is the vector of baseline controls, and Region_i is a census region fixed effect. Observations are weighted by 1950 city population, and standard errors are clustered by county for share-open and the combined instrument and by state for looseness.

The endogenous regressor—the change in logged city land area—is proportional growth of the municipal footprint, so a coefficient on it is an elasticity with respect to city land area growth. Over

1950–1980, the median city in the sample ends the period at 3.2 times its 1950 land area.

Table 4 reports the first stage for all three instruments, with and without the baseline controls, on a fixed sample of 134 cities. All three instruments predict land growth, and their coefficients change little when the controls are added. With the controls, moving from a fully fenced-in city to a fully open one raises land growth over 1950–1980 by 1.669 log points (first-stage F of 19.6), and moving from the strictest annexation law to the loosest raises it by 1.49 log points (F of 31.4). A one-unit increase in the combined instrument, a city one standard deviation more open in a state whose law is one standard deviation looser, raises land growth by 0.52 log points, with a first-stage F of 33.7, above that of share-open alone. Appendix Figure A.20 plots each first stage.

Table 4: First-Stage Estimates

	Dependent variable: Δ log city land area, 1950–1980					
	Share-open		Looseness		Combined	
	(1)	(2)	(3)	(4)	(5)	(6)
Share-open (share, 0–1)	+1.410***	+1.669***				
	(0.183)	(0.377)				
State annexation-law looseness (0–1)			+1.245***	+1.495***		
			(0.357)	(0.267)		
Combined instrument (mean of standardized)					+0.418***	+0.520***
					(0.052)	(0.090)
1950 Land Area (logged)		-0.767***		-0.823***		-0.992***
		(0.146)		(0.139)		(0.148)
County Area (logged)		+0.100		+0.051		+0.131
		(0.097)		(0.105)		(0.084)
1950 Population (logged)		+0.616***		+0.459***		+0.779***
		(0.127)		(0.108)		(0.137)
Water Share within 8 Miles		-0.867**		-1.418***		-0.741**
		(0.337)		(0.416)		(0.334)
Municipal Land Share within 8 Miles		+0.469				+0.191
		(0.632)				(0.330)
Other-County Land Share within 8 Miles		-0.341		-0.354		-0.170
		(0.286)		(0.255)		(0.252)
County Population Growth, 1930–40 (Δ log)		+0.589		+0.174		+0.372
		(0.579)		(0.570)		(0.571)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Standard errors clustered by	County	County	State	State	County	County
Instrument SD (net of controls)	0.260	0.112	0.174	0.154	0.905	0.514
First-stage F	59.2	19.6	12.2	31.4	64.2	33.7
Observations	134	134	134	134	134	134

NOTES: This table reports first-stage estimates from Equation 9: the change in log city land area between 1950 and 1980 on share-open (columns 1–2), on the looseness of the state's annexation law (columns 3–4), and on the two combined (columns 5–6, Equation 8), each without and with the baseline controls, on a fixed sample of 134 cities. All specifications include census region fixed effects and are weighted by 1950 city population. The looseness specifications omit the municipal land share within 8 miles from the controls. Standard errors are clustered by county for share-open and the combined instrument and by state for looseness. Standard errors are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

We report balance for all three instruments in Appendix Table A.4, on the 1950 economic structure of the city and its county (Panel A) and on pre-1950 trends (Panel B). Within region but without additional controls, all three instruments are negatively correlated with pre-existing economic development: cities with more open land and cities in states with looser laws were less dense and had lower house values in 1950, and they sat in less dense counties; the counties around cities with more open land were also whiter. Conditional on the baseline controls, these differences largely disappear. The one gap that remains is in house values: cities in looser states, and so cities with a higher value of the combined instrument, had slightly lower house values in 1950. Conditional on the controls, pre-1950 county population and employment growth are unrelated to all three instruments, although cities with more open land were already growing slightly faster in the 1940s.

A.d. Benchmarking Against Event-Study Estimates

The event study in Section B. is well suited to margins, such as municipal finance, that react sharply and directly to a large annexation. To check the instrumental-variables design against it, we estimate across designs the long-run elasticity of a city's spending with respect to its population. The instrumental-variables counterpart of Equation 4 is

$$\Delta \log \text{Spending}_i = \varepsilon \, \Delta \log \text{Population}_i + X'_{1950,i}\delta + \text{Region}_i + u_i, \tag{10}$$

where spending changes between 1952 and 1982, population changes between 1950 and 1980, and the change in population is instrumented by Z_i.

The event study design and long-run instrumental-variables design draw on distinct sources of variation, so Panel A of Table 5 first puts them on a common scale by evaluating each at its average annexation. For the event study, we take the average spurt's increase in land area and the pooled effect on population five to ten years after the spurt. For the instruments, we take the average increase in land area between 1950 and 1980 among the annexing cities in the instrument sample and multiply it by the IV estimates of the effect of land growth on population and on density. The spurts and three decades of cumulative annexation add similar numbers of residents: the average

spurt raises city population by 44%, against 64%, 44%, and 51% implied by share-open, looseness, and the combined instrument. Spurts annexed more land, 201% against 163%, and emptier land, so density in the spurt cities fell by 52%, against 38% to 45% in the instrument designs.

Table 5: Benchmarking the Instruments against the Event Study

	Staggered DiD (1)	IV: share-open (2)	IV: looseness (3)	IV: combined (4)
Panel A. At the average annexation (percent change)				
Evaluated at	Average spurt		Average annexing city	
City population	+44%	+64%	+44%	+51%
City land area	+201%	+163%	+163%	+163%
Population density	-52%	-38%	-45%	-42%
Panel B. Elasticity of spending with respect to city population				
General Expenditure	$+0.745^{***\dagger}$	$+0.653^{***}$	+0.394	$+0.544^{**\dagger}$
	(0.143)	(0.241)	(0.531)	(0.239)
Net of Education	$+0.781^{***\dagger}$	$+0.549^{**\dagger\dagger}$	$+0.016^{\dagger\dagger}$	$+0.324^{\dagger\dagger\dagger}$
	(0.125)	(0.214)	(0.443)	(0.216)
Fire Protection	$+0.527^{***\dagger\dagger\dagger}$	$+0.521^{***\dagger\dagger\dagger}$	$+0.160^{\dagger}$	$+0.365^{**\dagger\dagger\dagger}$
	(0.082)	(0.174)	(0.458)	(0.183)
Police Protection	$+0.636^{***\dagger\dagger\dagger}$	$+0.686^{***}$	+0.334	$+0.536^{**\dagger}$
	(0.081)	(0.195)	(0.646)	(0.241)
Highways	$+1.030^{***}$	$-0.102^{\dagger\dagger}$	$-0.251^{\dagger}$	$-0.158^{\dagger\dagger}$
	(0.208)	(0.518)	(0.717)	(0.457)
Sanitation	$+1.198^{***}$	$-0.379^{\dagger\dagger}$	-0.092	$-0.229^{\dagger\dagger}$
	(0.204)	(0.577)	(0.865)	(0.587)
First-stage F (population)		20.0	5.3	22.6
Cities		133	133	133

NOTES: This table compares the staggered difference-in-differences (column 1) with the three instruments (columns 2–4). Panel A evaluates each design at its average annexation. For the staggered difference-in-differences, Panel A reports the average spurt's land increase and the pooled population effect. For the instruments, Panel A reports the average 1950–1980 land increase among annexing cities in the IV sample, weighted by 1950 population, and the population and density changes implied by the IV coefficients at that average. Panel B reports the elasticity of city spending with respect to city population from the staggered difference-in-differences of Equation 4 (column 1) and from two-stage least squares estimates of Equation 10, instrumented by share-open (column 2), by the looseness of the state's annexation law (column 3), or by the two combined (column 4, Equation 8). The looseness specification omits the municipal land share within 8 miles from the controls. Standard errors are clustered by county for share-open and the combined instrument and by state for looseness. Standard errors are in parentheses. Panel B is tested against zero, $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$, and against one, the value at which spending rises in proportion to population, $^{\dagger}p < 0.1$, $^{\dagger\dagger}p < 0.05$, $^{\dagger\dagger\dagger}p < 0.01$.

Panel B compares the elasticity of spending with respect to population, which is the most direct comparison across the three sources of variation. For all three instrument designs the overall elasticity of spending with respect to population closely matches the difference-in-differences, but

the instrument estimates are less precise. Although the two designs draw on different variation, they agree on the direction and size of the economies of scale in municipal spending.

B. Aggregate Local Finances

Municipal scale allowed annexing cities to provide services at a lower average cost, but that does not by itself imply that the aggregate spending of local governments across the county changed. For instance, if the lower costs were largely driven by cross-subsidization and the annexation acted as a transfer of function from county to municipal government, then the aggregate budget should be unchanged.

Annexation can change local spending across the county in three ways. First, it can change the total supply of services, by extending the central city's broader set of services to residents of formerly unincorporated land. Second, it can reallocate spending between layers of government, from the county government and special districts that served the fringe to the central city. Third, the city's lower costs may be partly compositional: a city that absorbs residents who are cheaper to serve lowers its own average cost without any change in the cost of serving the county as a whole.

For instance, as Table 6 shows, municipalities accounted for over 90 percent of local spending on fire protection in 1957, and three quarters of county governments did not spend on fire protection at all. Residents of unincorporated parts of the county often relied on volunteer fire departments or had limited fire protection. We can therefore think of the effect of annexation on fire spending across the county as a mixture of moving unincorporated residents to a higher level of fire provision and of shifting the cost of provision. Looking across functions, fire, sanitation, and sewers were rarely provided by the county government, while for police and parks, the county government operated but at a much lower level than municipal governments. Finally, functions like highways, administration, health, and welfare were split between layers of government.

To disentangle these competing effects of higher service and potentially lower costs, we construct an index of county costs that holds the coverage of each layer of government fixed. We divide the governments of a county into three layers: the central city; countywide governments, which are the county government and special districts; and other incorporated municipalities. For county c in

Table 6: County and Municipal Shares of Local Spending, 1957

Function	Share of all local spending				Counties with no county spending
	County government	Special districts and townships	Central city	Other municipalities	
Fire protection	3%	3%	82%	13%	123 of 163
Sanitation other than sewers	1%	1%	85%	13%	140 of 159
Sewers (current)	5%	29%	55%	11%	127 of 158
Police	9%	1%	79%	11%	10 of 162
Parks and recreation	11%	12%	66%	11%	80 of 162
Administration	43%	3%	46%	9%	10 of 163
Highways	26%	7%	56%	11%	19 of 164
Health	31%	3%	63%	3%	23 of 154
Public welfare	61%	1%	38%	0%	11 of 160

NOTES: This table reports, for the central counties of the 1957 standard metropolitan areas outside New England, the shares of local government spending on each function made by the county government, by special districts and townships, by the central city, and by other municipalities, from the 1957 Census of Governments. School districts account for the remainder. The last column counts the counties whose government spent nothing on the function.

year t, spending per county resident on a given function is

$$a_{ct} = g_{ct} + s_{ct}\, x^C_{ct} + m_{ct}\, x^M_{ct}, \tag{11}$$

where g_{ct} is countywide spending per county resident, s_{ct} is the central city's share of county population and x^C_{ct} the city's spending per city resident, and m_{ct} is the share of county residents living in other municipalities and x^M_{ct} those municipalities' spending per resident. Residents of unincorporated land receive only countywide services.

We then decompose the change in a_{ct} between 1957 and 1982 into two parts, in the spirit of an Oaxaca–Blinder decomposition:

$$\Delta a_c = \underbrace{\Delta g_c + s_{c,1957}\, \Delta x^C_c + m_{c,1957}\, \Delta x^M_c}_{\text{cost index}} + \underbrace{\Delta s_c\, x^C_{c,1982} + \Delta m_c\, x^M_{c,1982}}_{\text{coverage}}. \tag{12}$$

The first part is a cost index: the change in spending per resident within each layer, holding each layer's share of the county's residents at its 1957 value. The second is coverage: residents moving into different layers, either into the central city through annexation or from unincorporated land into other municipalities through incorporation. We express each term as a percentage of the

county's average spending per resident in 1957 and 1982, so that the terms add up to the total change and each term approximates 100 times a log change.

Appendix Table A.6 reports the inputs to the decomposition in the 90 central counties of our sample, described in Section III.. In 1957, most county residents lived in the central city and about a fifth on unincorporated land, while by 1982, the shares living in other municipalities and on unincorporated land had both risen, and the share in the central city fell.

We first show that the combined instrument unsurprisingly shifts residents out of living in unincorporated parts of a county into the central city. We then relate the change in each layer's share of county residents between 1957 and 1982, and later each term of Equation 12, to the central city's land growth between 1950 and 1980:

$$T_c = \beta \, \Delta \log \text{LandArea}_i + X'_{1950,i}\delta + \text{Region}_i + \varepsilon_c, \tag{13}$$

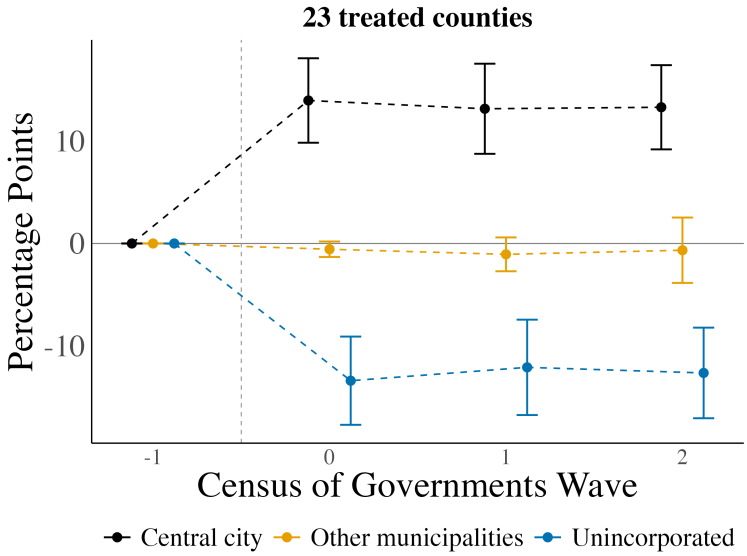
where T_c is the outcome for county c, the home county of central city i. We estimate Equation 13 by two-stage least squares, instrumenting the city's land growth with the combined instrument (Equation 8), and by ordinary least squares, with observations weighted by the county's 1950 population. Appendix Table A.7 reports the estimates for share-open and looseness separately. Table 7 shows that in counties where the central city grew more, 10.2 percent more county residents lived in the central city, with all of the effect being drawn out of unincorporated areas.

Using Equation 12, we estimate how each term of the decomposition shifted under centralizing governments. The coverage term measures how much spending rose, holding spending per layer fixed, due to residents shifting from unincorporated to central city areas, while the cost index measures, holding the share of residents in each type of government layer fixed, how much spending changed in each layer of government. To interpret the cost index in efficiency terms, we would need to assume that the service provision for each type of county resident is stable over this period. If, for instance, residents living in unincorporated areas receive higher service through the emergence of special districts, this additional service would push the cost index upwards. We are also assuming that countywide government services provide equal services to every resident of the county and therefore any cost savings to the central city are not being generated by a transfer of suburban

Table 7: Impact of Annexation on Municipal Centralization

	Share of county residents living in		
	Central city	Non-central municipalities	Unincorporated
	(1)	(2)	(3)
Panel A: Change 1957–1982, per doubling of central-city land area			
IV, combined instrument	10.2**	−2.1	−8.1**
	(4.0)	(3.3)	(4.1)
OLS	8.0***	0.5	−8.5***
	(1.4)	(1.0)	(1.6)
First-stage F		22.7	
Counties		90	

Panel B: Event study, change from the wave before the spurt (wave −1)



NOTES: This table reports changes in the percentage of county residents living in the central city, in non-central municipalities, and on unincorporated land; the three sum to zero. Panel A reports estimates of β from Equation 13 for the change between 1957 and 1982, by two-stage least squares with the combined instrument and by ordinary least squares, on 90 central counties weighted by 1950 county population, with the baseline controls. Panel B plots the stacked long difference of Equation 3 on Census of Governments waves, from the last wave before the central city's annexation spurt (wave −1) to each later wave, for spurts beginning after 1957, with equal weights, with 95 percent confidence intervals. Non-central municipal population is interpolated between the 1940, 1950, 1960, 1970 and 1980 censuses, with places entering at their incorporation date. Standard errors, clustered by county, are in parentheses: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

services. This assumption fits administration, courts, and welfare; for police, where the sheriff mainly patrols unincorporated land, it understates how much countywide police spending falls on the residents who remain outside municipalities.

Table 8 reports the decomposition and shows that municipal centralization lowered aggregate costs while increasing average service provision, and these two forces roughly offset each other for most functions. The effect of municipal centralization on aggregate spending (Column 1) is noisy and not distinguishable from zero across all functions. When we disaggregate this effect into its component parts, we find that the cost from increasing coverage is positive and significant for most functions, while the cost index is negative and significant for most functions.

Municipal centralization moved the average resident into a jurisdiction with much more extensive government services. In 1982, the governments serving a central-city resident spent 72 percent more per resident on services other than education than those serving a resident outside the city, and coverage into the central city rises with land growth in every function, while coverage by other municipalities falls (column 2; Appendix Table A.8 separates the two). Holding coverage fixed, governments in counties with more centralized cities were more cost efficient: the cost index falls with land growth in every function, significantly so for fire protection, police protection, administration, and general expenditure net of education (column 3). The savings come both from a lower average cost of the central city's own services, most clearly for fire protection and administration, consistent with the economies of scale in Table 2, and from lower countywide spending on police and highways, plausibly because counties whose central city absorbed suburban growth did not need to duplicate administration or build their own service districts (Appendix Table A.8, columns 6 and 8). Because wider coverage and lower costs offset each other, total county spending per resident does not move with land growth in a way we can distinguish from zero (column 1).

C. Economic Spillovers

> *"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."*

Table 8: County Spending per Resident by Type of Government, 1957–1982: Coverage and Cost

	Total (1)	Coverage (2)	Cost index (3)
Panel A: IV, combined instrument (first-stage F = 22.7)			
Fire protection	−5.8	28.2**	−34.0***
	(7.1)	(11.0)	(11.2)
Police protection	−5.7	18.3**	−23.9***
	(7.3)	(7.4)	(9.0)
Highways	−10.9	16.7***	−27.6
	(15.7)	(5.3)	(17.4)
Administration	−7.6	11.3***	−18.9**
	(8.0)	(3.9)	(9.1)
Gen. exp. net of educ.	0.9	15.8***	−14.9**
	(5.5)	(4.8)	(6.9)
Panel B: OLS			
Fire protection	2.3	18.9***	−16.6***
	(2.7)	(3.3)	(3.3)
Police protection	5.4**	14.5***	−9.1***
	(2.3)	(2.8)	(2.9)
Highways	5.0	10.3***	−5.3
	(7.6)	(1.7)	(7.8)
Administration	0.1	9.0***	−8.9**
	(3.0)	(1.6)	(3.6)
Gen. exp. net of educ.	3.8	11.8***	−8.0***
	(3.2)	(1.9)	(2.9)

NOTES: This table reports the effect of a doubling of the central city's land area between 1950 and 1980 ($\ln 2$ times β in Equation 13) on the change in county spending per resident between 1957 and 1982 (column 1) and on its two parts from Equation 12, coverage (column 2) and the cost index (column 3). Panel A is two-stage least squares instrumented by the combined instrument, and Panel B is ordinary least squares. Each effect is in percent of the mean of the county's 1957 and 1982 spending per resident. Columns (2) and (3) sum to column (1). Appendix Table A.8 breaks coverage and the cost index into their parts. Countywide governments are the county government, special districts, and townships. The sample is 90 central counties, weighted by 1950 county population, with the baseline controls. Standard errors, clustered by county, are in parentheses: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

—*Arizona Republic*, 1957

City managers often argued that expanding municipal borders would improve local economic outcomes by attracting businesses and new residents and by allowing for coordination under a single governmental unit. We evaluate the short-run effects of annexation on county economic activity with the event-study design of Section A.. For county c in year t, we estimate

$$Y_{ct} = \alpha_c + \gamma_{r(c),t} + \sum_{\substack{k=-a \\ k\neq -1}}^{b} \beta_k \, \mathbf{1}[K_{ct} = k] + \epsilon_{ct}, \tag{14}$$

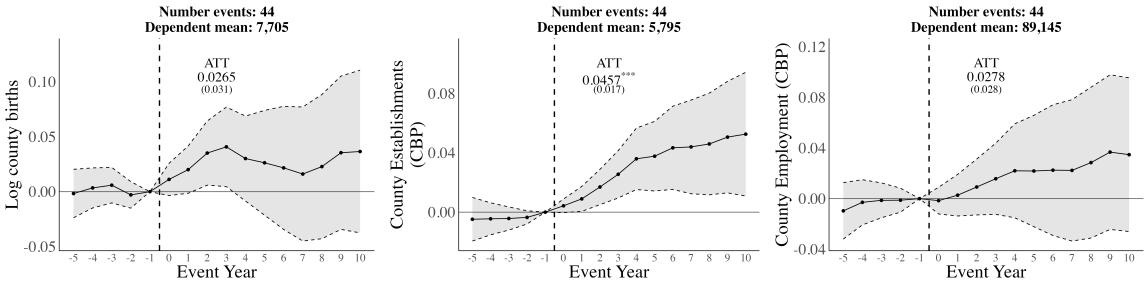
where Y_{ct} is log births, establishments, or employment, α_c and $\gamma_{r(c),t}$ are county and region-by-year fixed effects, and the $\mathbf{1}[K_{ct} = k]$ indicate years relative to the annexation of the county's largest sample city ($K_{ct} = t - E_c$), as in Equation 2. A county is treated when that city annexes, and counties whose sample city made a major annexation earlier are excluded from the comparison group. We estimate Equation 14 by synthetic difference-in-differences (Arkhangelsky et al. 2021): within each annexation cohort and census region, control counties are reweighted so that their path before annexation tracks that of the annexing counties, so parallel trends needs to hold only for this reweighted comparison group. We summarize each outcome by its pooled effect five to ten years after annexation.

Figure 9 shows modest but noisily estimated economic spillovers after a large annexation. The number of establishments rises modestly (5%), and births and employment have similar, if slightly smaller and noisily estimated, results. City leaders indicated that annexations would make land more appealing to firms, in part because it would subsidize the initial costs of connecting a firm to electricity, water, and the road networks needed for shipping and transportation. Converted to elasticities (Panel B), the results imply that a 50% increase in central municipal population results in roughly 7% more establishments.

Appendix Table A.9 then uses the combined instrument to estimate the effects on countywide economic activity and demographic characteristics over the long run (1950–1980). Here, nearly none of the IV estimates are statistically different from zero, but the magnitudes of the economic spillover estimates mirror those of the shorter-run event study. If anything, we do see compositional shifts

Figure 9: County-Level Economic Activity

Panel A: Event Studies



Panel B: Elasticities

	Central-city population (1)	Predicted change in city spending per resident (2)
Births	0.087	-0.277
	(0.111)	(0.245)
Establishments	0.158***	-0.393***
	(0.058)	(0.131)
Employment	0.095	-0.262
	(0.106)	(0.271)

NOTES: Panel A reports synthetic difference-in-differences event-study estimates (Arkhangelsky et al. 2021) for log county births, establishments, and employment. The event window runs from five years before to ten years after annexation. Virginia is excluded. The sample holds 44 treated counties in 29 cohort-by-region blocks. Coefficients are relative to event year -1, and shaded bands are 95 percent confidence intervals. The pooled ATT averages event years 5 to 10. Panel B reports the elasticity of each outcome with respect to the central city's population (column 1) and the reduced-form effect of the central city's predicted change in log general expenditure per resident (column 2), pooled over event years 5–10. The predicted change is built from the elasticities in Table 2. Births are counted by the mother's county of residence. Standard errors are from a county-level bootstrap: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

in the county away from residential to commercial use, and that the households that stay are lower income and living in lower-value homes. Appendix E further shows that if anything annexation led to increased densification of the urban fringe and a greater non-white share. These effects are consistent with the central economic area urbanizing and residential populations sprawling into neighboring counties. The effect is also consistent with the establishment-level results being larger and more precisely estimated than population ones.

Taken together, these results suggest that the increase in municipal scale through annexation may have had modest but positive economic spillovers onto the larger economic area. However, both the staggered event study, which uses sharp annexation spurts, and the annexation-capacity instrument are underpowered to make precise statements about the overall economic effect.

VII. Conclusion

Policymakers and city leaders viewed municipal centralization through annexation as a solution to the fiscal and service-provision challenges introduced by suburban sprawl. Those cities that were able to expand did so dramatically, and the median annexing city more than tripled its land area, often in very concentrated bursts. This divergence 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. In advocating for the benefits of such a large transformation in the scale of their central local governments, these policymakers promised lower costs and taxes, superior services, and broad positive effects on the local economy.

In this paper, we find, contrary to some of the recent literature on municipal scale, that some of these promises were borne out. In particular, in a period of dramatically rising municipal costs, annexation led to a lower average cost, especially in municipal functions based around operational dispatch hubs, which was passed on to incumbent residents in the form of a lower property tax bill. We also find no clear deterioration in the quality or level of municipal service provision. However, there appear to be clear offsetting forces: absorbing already developed suburban areas lowers costs, while some of the most expansive annexations, taking in rural undeveloped land, actually raised costs relative to not annexing.

In evaluating some of the loftier goals of the annexation agenda, we find mixed evidence of an impact on the broader economic area. Overall spending by county and local governments within central counties that experienced annexation stays flat, and if anything slightly rises in some functions, but we interpret this effect as being driven by an expansion of municipal services rather than rising costs. We also find noisy but positive evidence that municipal centralization attracted economic activity, but this effect may be associated with a slight transition of the central county from residential to commercial use and with the sorting of higher-income and whiter households out of annexing areas.

Should central cities have the power to decide the extent of their borders, or is it efficient to allow the suburban fringe to have veto power? We find evidence that empowering central cities may lead to more efficient provision, particularly during high-growth periods such as the postwar Sun Belt.

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Appendix – For Online Publication

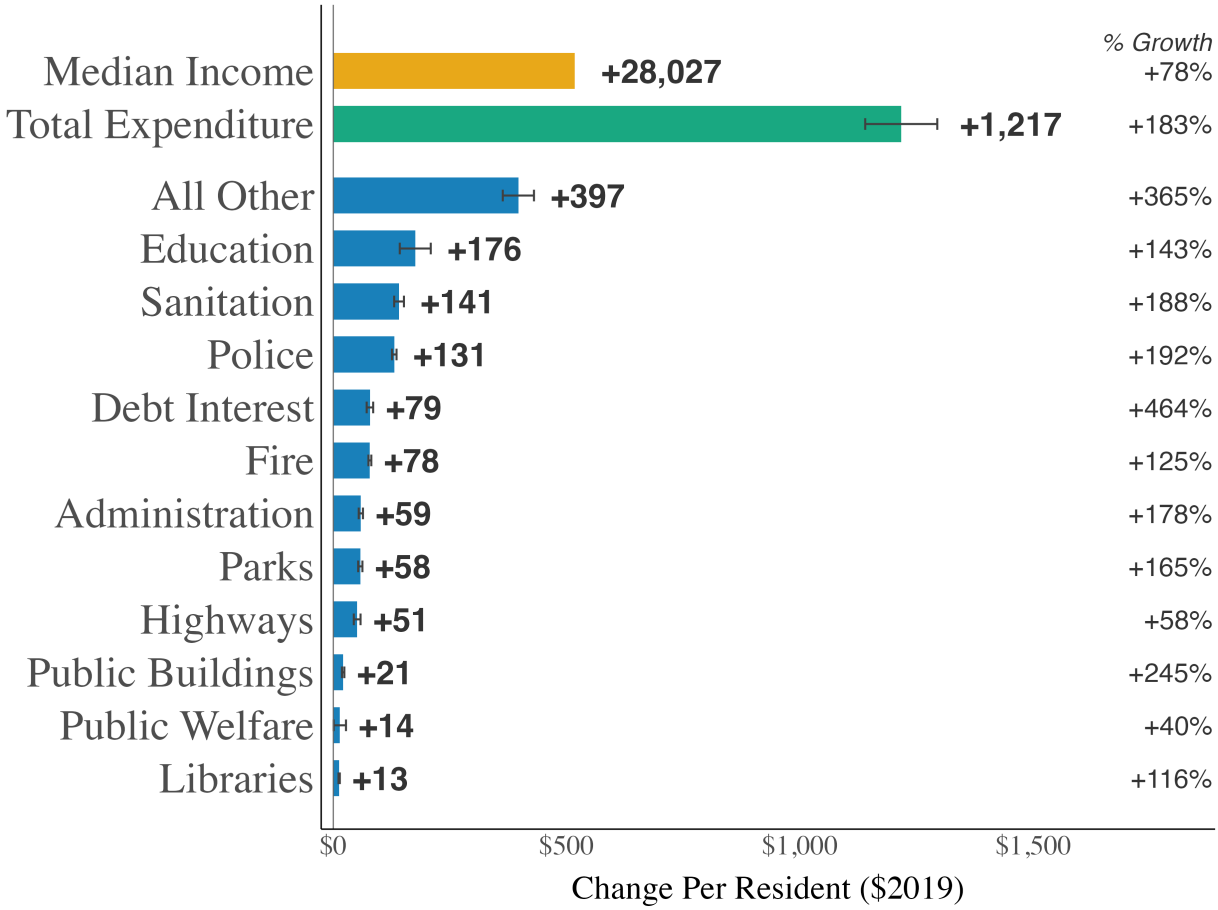
A Additional Figures and Tables

Table A.1: City Core and Annexed Territory over Time, 1950–1980

	1950	1960	1970	1980
Panel A: Population				
1950 Core	298,946	309,973	291,889	263,131
	(192,690)	(209,843)	(224,622)	(221,031)
Annexed Ring	36,647	102,515	152,860	193,575
	(23,725)	(68,051)	(130,895)	(207,260)
Remainder of County	125,411	229,637	308,754	410,335
	(65,682)	(128,422)	(210,242)	(328,174)
Panel B: Share White				
1950 Core	86%	79%	72%	63%
	(10%)	(10%)	(12%)	(13%)
Annexed Ring	91%	90%	87%	73%
	(9%)	(8%)	(11%)	(17%)
Remainder of County	87%	91%	92%	90%
	(17%)	(14%)	(8%)	(8%)
Panel C: Median Family Income (2019$)				
1950 Core	$36,689	$50,470	$60,047	$55,699
	($5,277)	($7,412)	($8,331)	($9,616)
Annexed Ring	$40,715†	$59,633	$79,694	$74,182
	($5,925)	($7,420)	($8,626)	($9,858)
Remainder of County	$36,107†	$55,274†	$73,796†	$84,415†
	($10,546)	($16,264)	($14,873)	($11,444)
Panel D: Population Density (Per Sq. Mi.)				
1950 Core	6,959	6,831	6,020	5,194
	(2,819)	(2,474)	(2,034)	(1,597)
Annexed Ring	997	2,011	2,484	2,627
	(886)	(1,433)	(1,403)	(1,173)
Remainder of County	274	450	593	682
	(422)	(591)	(663)	(602)
Panel E: Housing Units Built in the Last 10 Years				
1950 Core	21%	19%	13%	9%
	(12%)	(10%)	(6%)	(5%)
Annexed Ring	53%	57%	38%	26%
	(10%)	(12%)	(11%)	(12%)
Remainder of County	43%†	55%†	42%†	40%†
	(12%)	(12%)	(10%)	(14%)

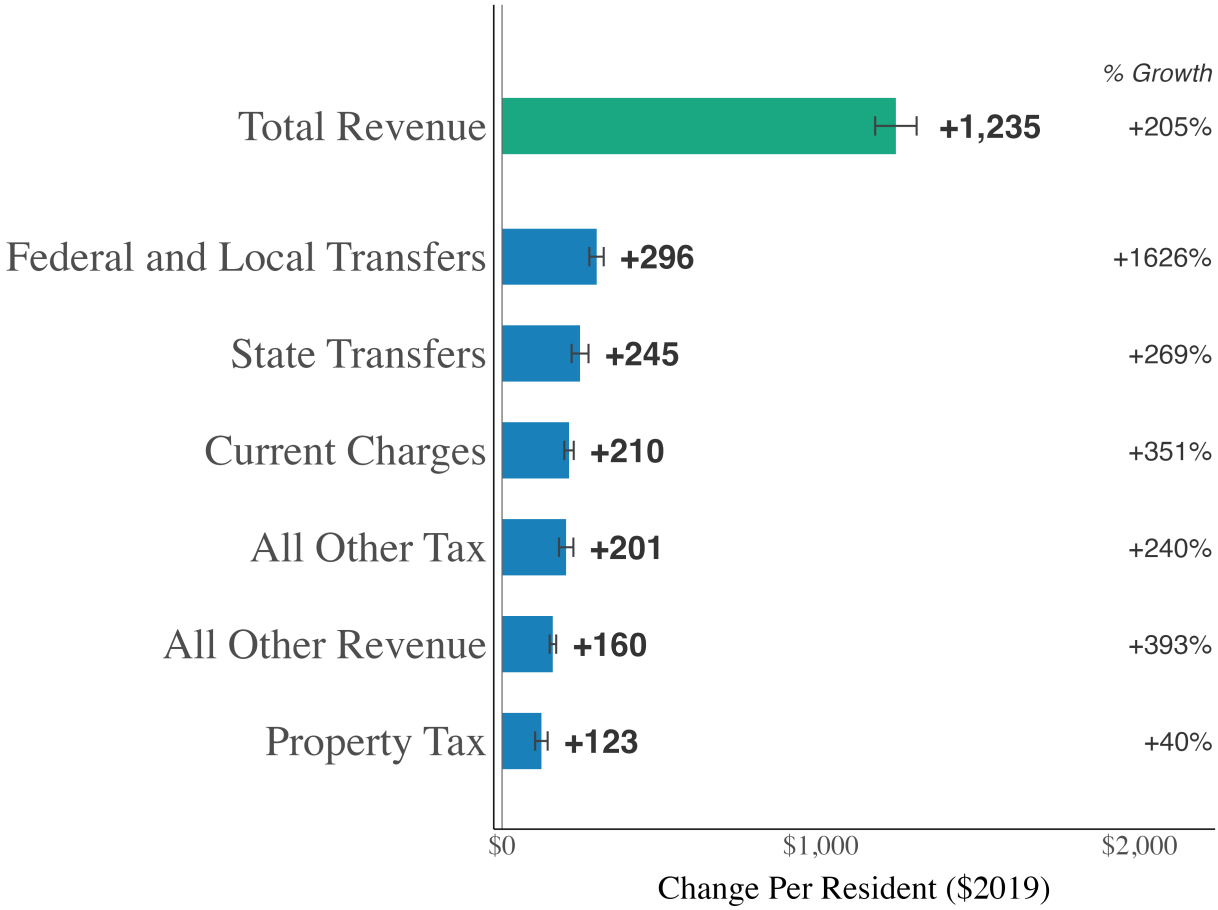
NOTES: This table reports unweighted means across 12 cities for the 1950 core (the 1950 boundary held fixed), the annexed ring (the 1980 boundary less the core), and the rest of the county. Core and ring are aggregated from census tracts weighted by area overlap. Cells marked † are estimated rather than measured. Standard deviations are in parentheses. Values in 2019 dollars.

Figure A.1: Growth in Municipal Expenditure per Resident, 1952 to 1982



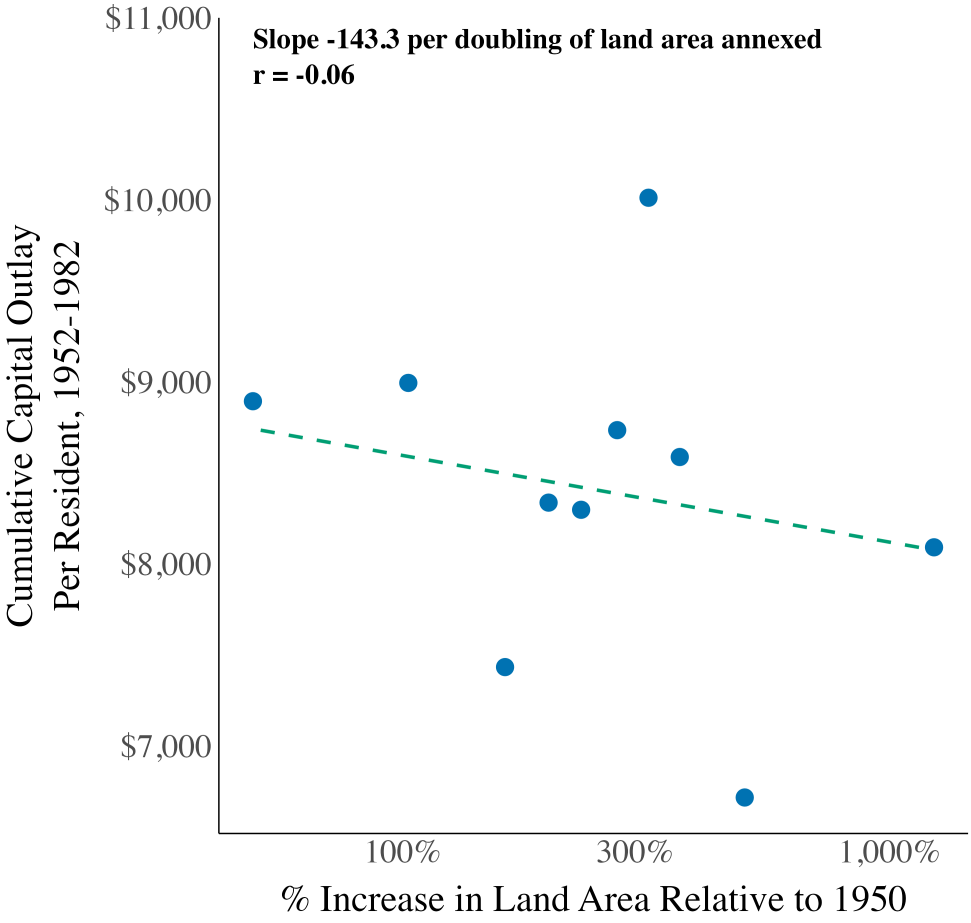
NOTES: This figure reports the change in general expenditure per resident between 1952 and 1982 by function, with endpoints measured as three-year means. Labels give the dollar and percent change. The orange bar scales the total by the growth in median family income between 1949 and 1979. All Other includes the functions the source does not report before 1955. Whiskers are plus or minus one standard error. Spending excludes utilities. Values in 2019 dollars.

Figure A.2: Growth in Municipal Revenue per Resident, 1952 to 1982



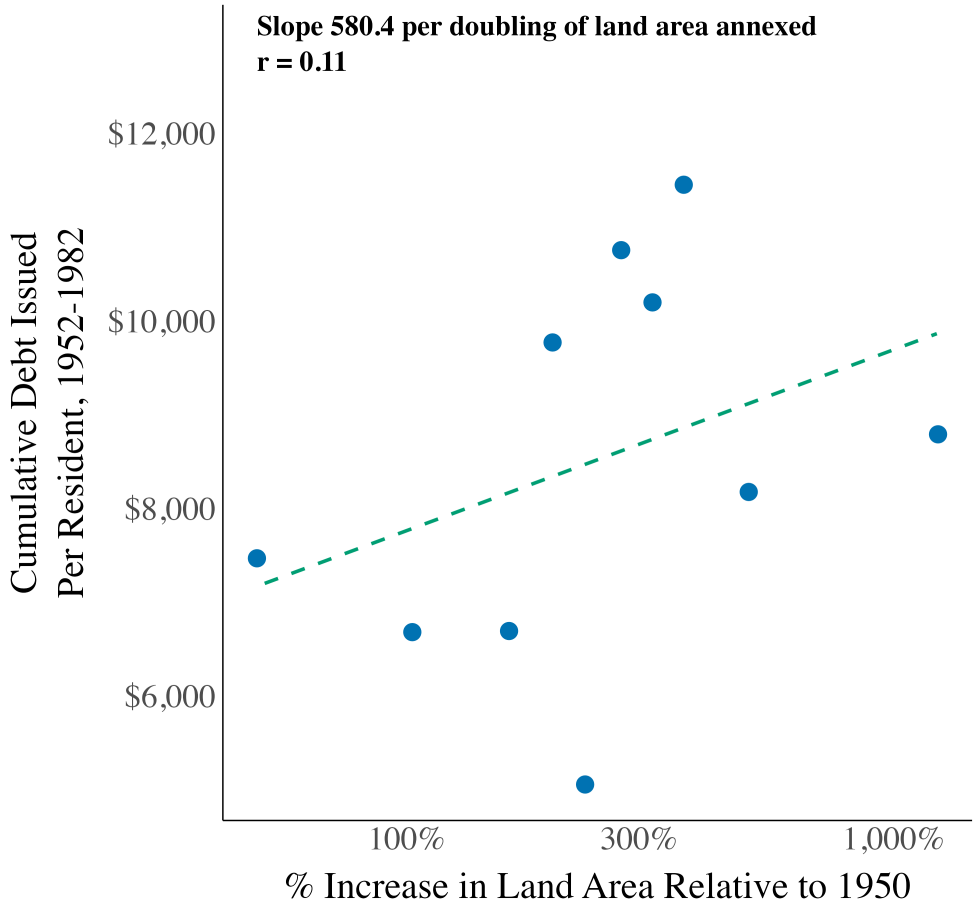
NOTES: This figure reports the change in general revenue per resident between 1952 and 1982 by source, measured as in Figure A.1. Federal and local transfers share a bar. Whiskers are plus or minus one standard error. Revenue excludes utilities. Values in 2019 dollars.

Figure A.3: Cumulative Capital Outlay by Annexation Intensity



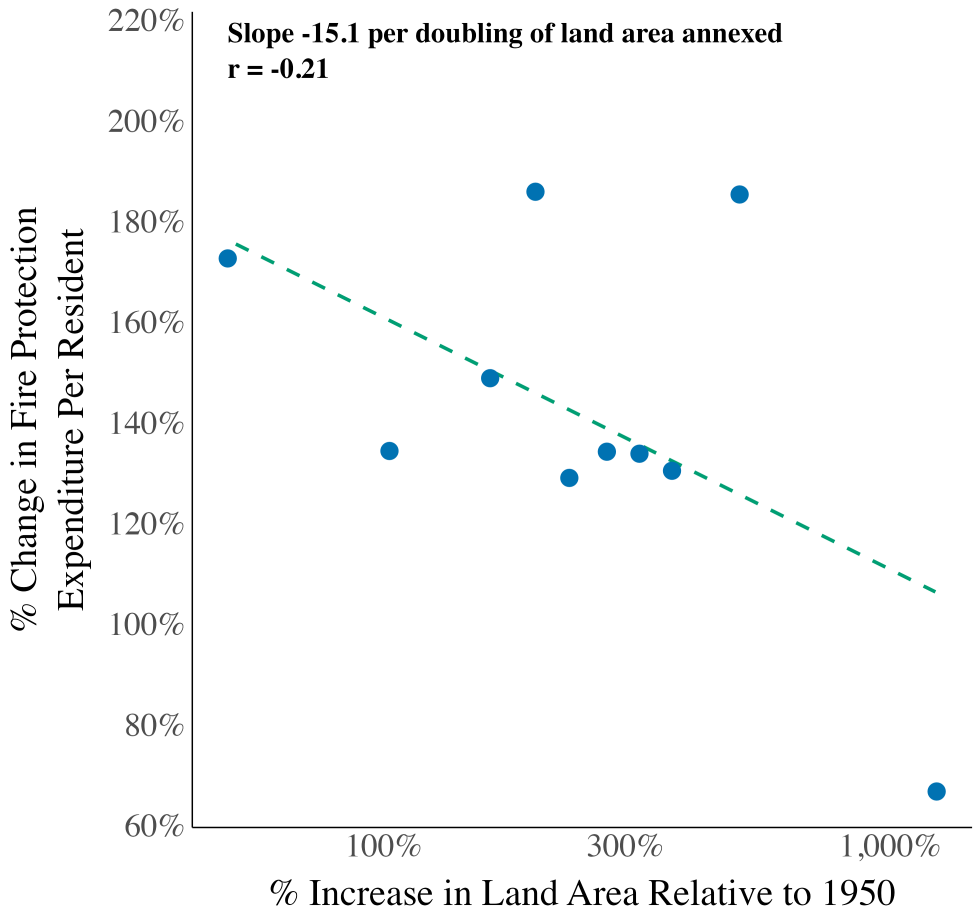
NOTES: This figure repeats Panel B of Figure 2 for cumulative capital outlay per resident. Values in 2019 dollars.

Figure A.4: Cumulative Debt Issued by Annexation Intensity



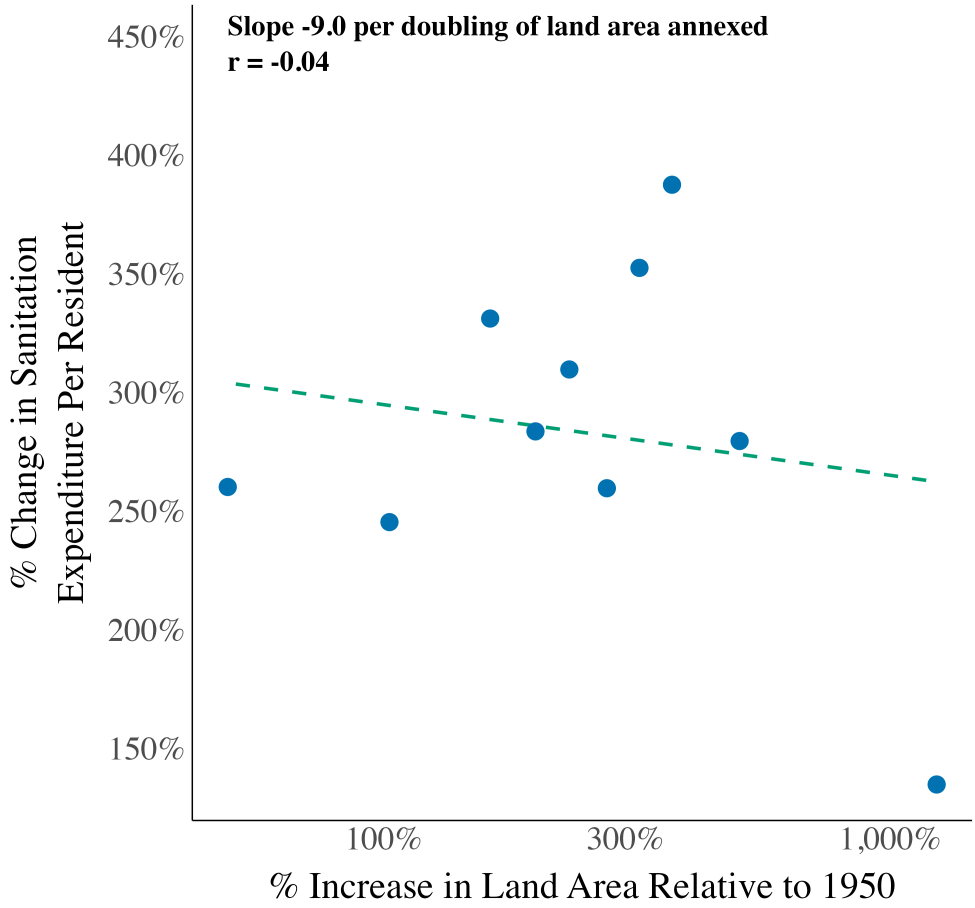
NOTES: This figure repeats Panel B of Figure 2 for cumulative debt issued per resident. Values in 2019 dollars.

Figure A.5: Fire Protection Expenditure by Annexation Intensity



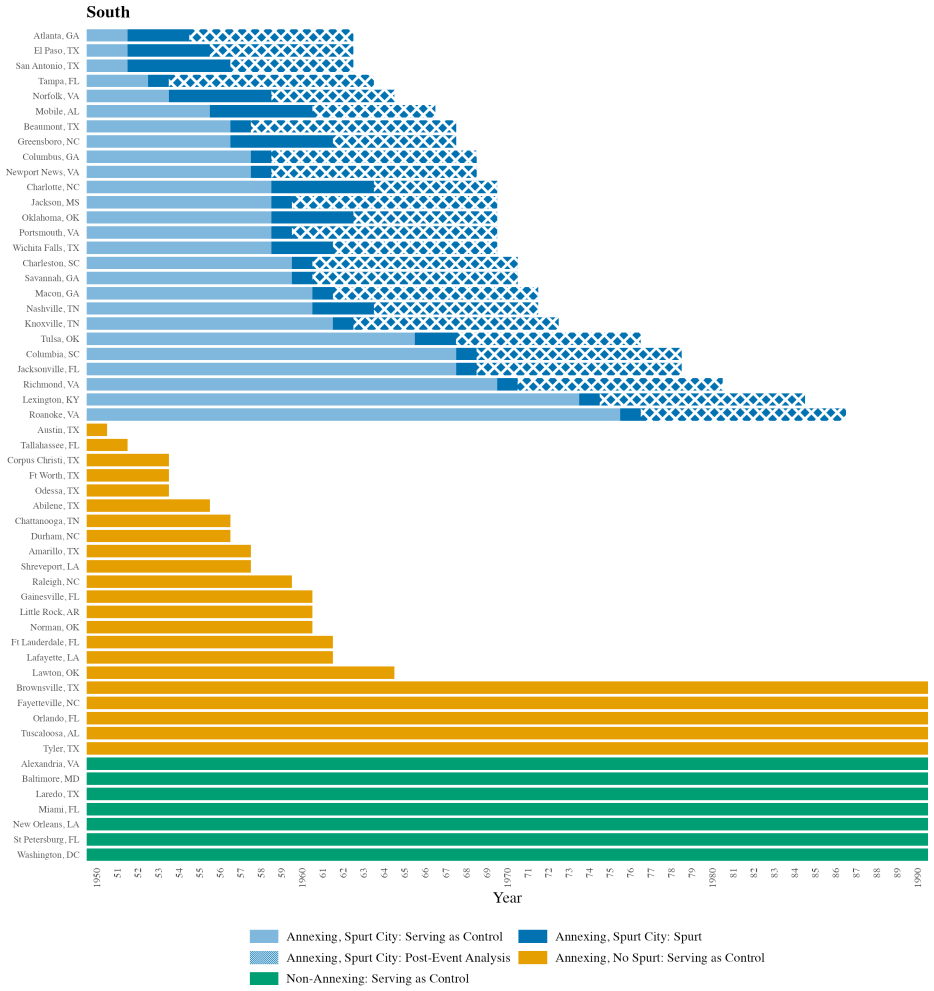
NOTES: This figure repeats Panel B of Figure 2 for fire protection expenditure per resident. Values in 2019 dollars.

Figure A.6: Sanitation Expenditure by Annexation Intensity



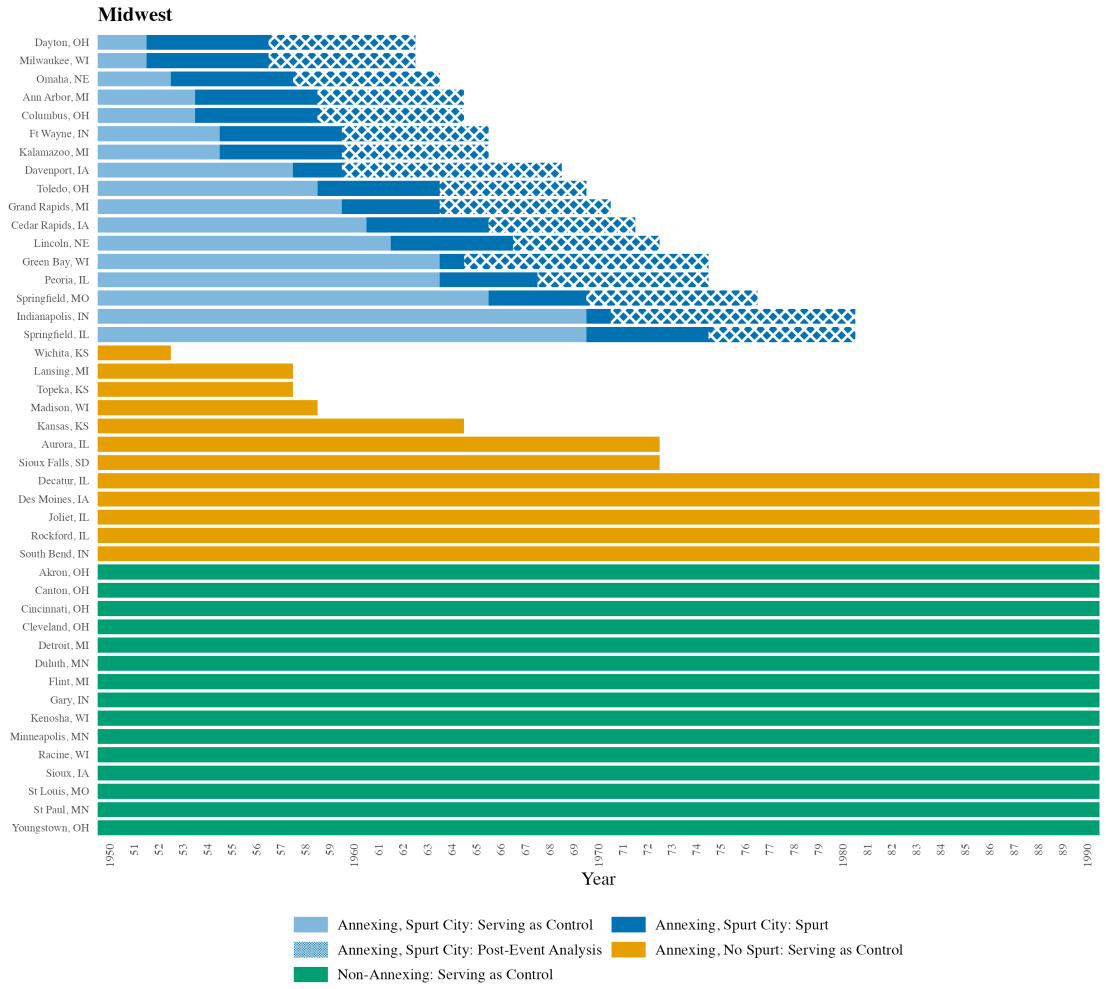
NOTES: This figure repeats Panel B of Figure 2 for sanitation expenditure per resident. Values in 2019 dollars.

Figure A.7: Event Study Timeline: South



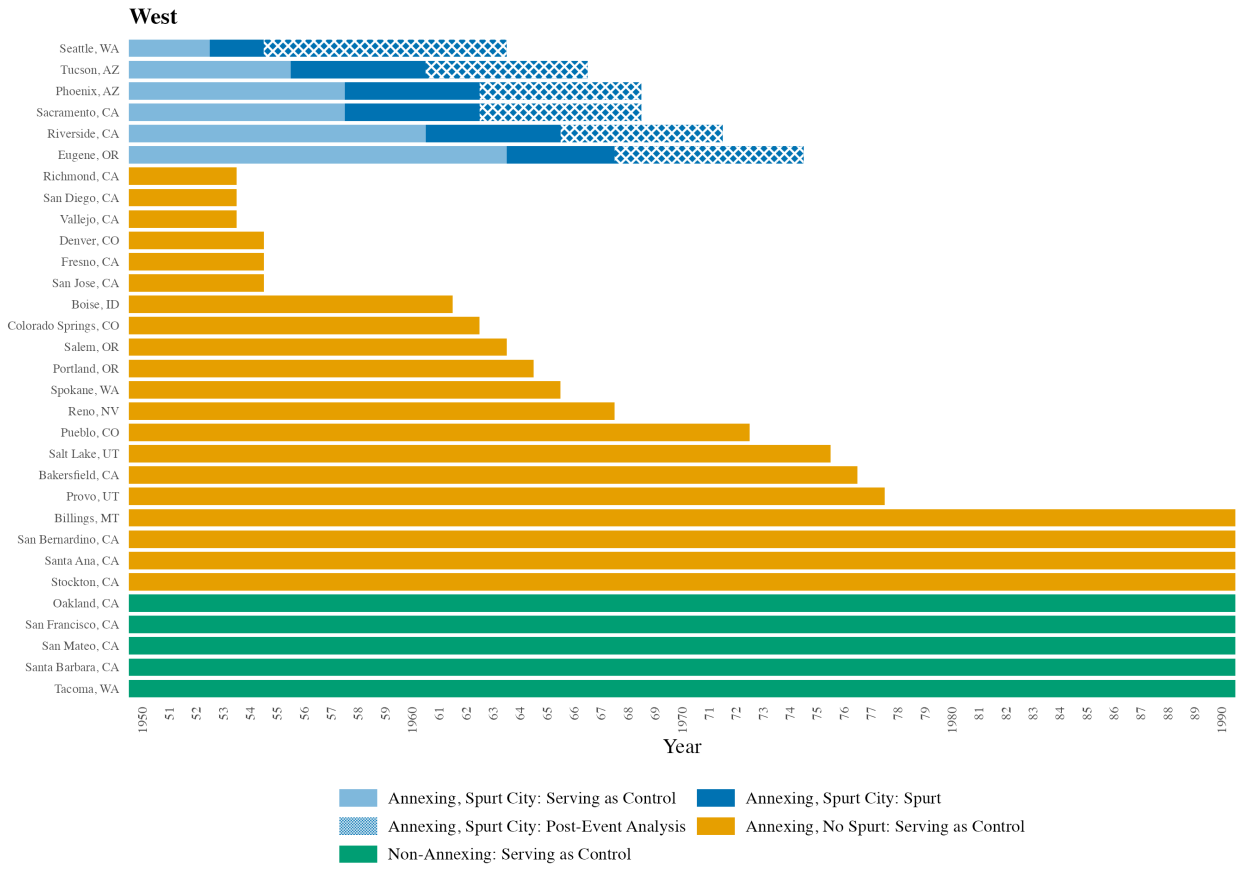
NOTES: This figure shows the years in which each Southern sample city contributes to the event study. Blue cities are treated: light blue marks years as not-yet-treated controls, solid blue the spurt, and the cross-hatch the post-event window. Orange cities annexed without a dated spurt and are shaded while eligible as controls. Green cities did not annex.

Figure A.8: Event Study Timeline: Midwest



NOTES: This figure is drawn identically to Figure A.7, whose notes describe the shading.

Figure A.9: Event Study Timeline: West



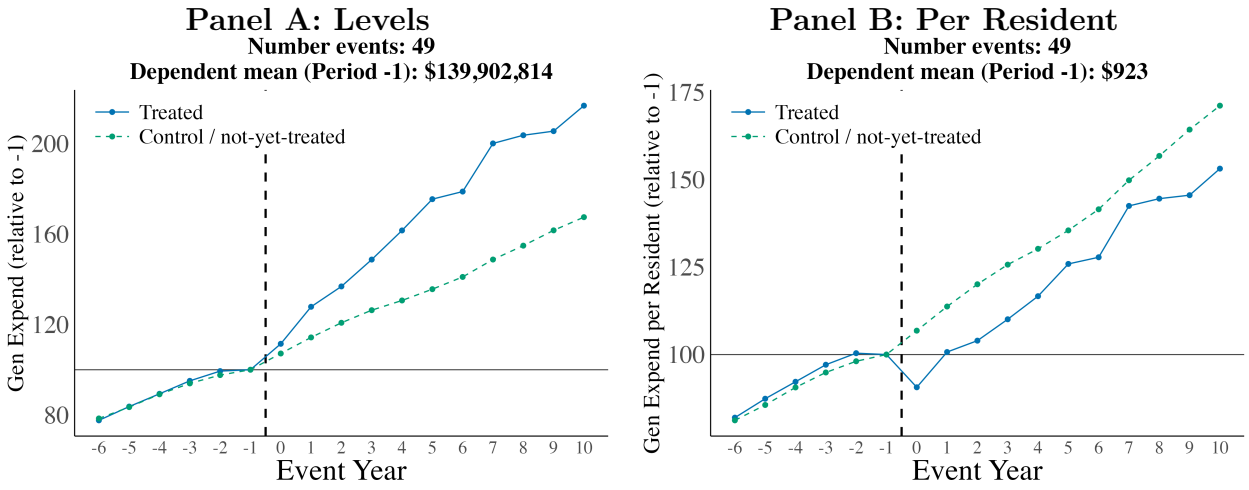
NOTES: This figure is drawn identically to Figure A.7, whose notes describe the shading.

Table A.2: City Characteristics by Annexation and Event-Study Status

	Mean (SD)				*p*-value		
	Annexing, spurt (treated) city (1)	Annexing, no spurt (2)	Non-annexing (3)	Eligible controls (4)	(1) vs (2)	(1) vs (3)	(1) vs (4)
Cities	49	69	58	104			
City size							
Land area, 1940 (sq mi)	20.5 (14.4)	18.8 (19.6)	37.0 (34.6)	31.0 (38.1)	0.593	0.001	0.080
Population, 1950 (000s)	157 (129)	121 (125)	323 (399)	211 (303)	0.131	0.004	0.204
City characteristics, 1950							
Share white	0.83 (0.15)	0.87 (0.13)	0.92 (0.08)	0.84 (0.13)	0.137	0.000	0.389
Share Black	0.17 (0.15)	0.13 (0.13)	0.08 (0.08)	0.16 (0.13)	0.143	0.000	0.430
Share born in state	0.65 (0.17)	0.60 (0.19)	0.62 (0.16)	0.60 (0.19)	0.191	0.400	0.112
Share native born	0.96 (0.04)	0.96 (0.03)	0.87 (0.05)	0.94 (0.05)	0.961	0.000	0.010
Share in manufacturing	0.08 (0.04)	0.07 (0.05)	0.14 (0.06)	0.09 (0.06)	0.221	0.000	0.873
Share in services	0.11 (0.03)	0.11 (0.03)	0.09 (0.02)	0.11 (0.03)	0.597	0.000	0.777
Average age	32.0 (2.0)	31.4 (2.6)	33.6 (1.9)	32.4 (3.0)	0.215	0.000	0.316
Average occupational score	25.3 (1.2)	25.5 (1.1)	26.0 (0.78)	25.3 (1.2)	0.321	0.001	0.945
County and metro, 1950							
Metro population (000s)	398 (266)	552 (1,091)	2,489 (3,687)	796 (1,283)	0.262	0.000	0.006
Share white	0.85 (0.14)	0.87 (0.13)	0.94 (0.07)	0.85 (0.13)	0.475	0.000	0.728
Share in manufacturing	0.08 (0.04)	0.07 (0.04)	0.14 (0.06)	0.08 (0.05)	0.077	0.000	0.381
Average occupational score	24.8 (1.1)	24.4 (1.5)	26.0 (0.97)	24.5 (1.6)	0.070	0.000	0.109
Municipal finance, 1950 (per resident, 2019$)							
Total debt	1,028 (720)	966 (742)	973 (691)	975 (770)	0.652	0.689	0.597
General expenditure	529 (249)	484 (223)	772 (351)	539 (283)	0.323	0.000	0.808
Capital outlay	134 (128)	131 (104)	124 (73.9)	114 (89.3)	0.876	0.636	0.147

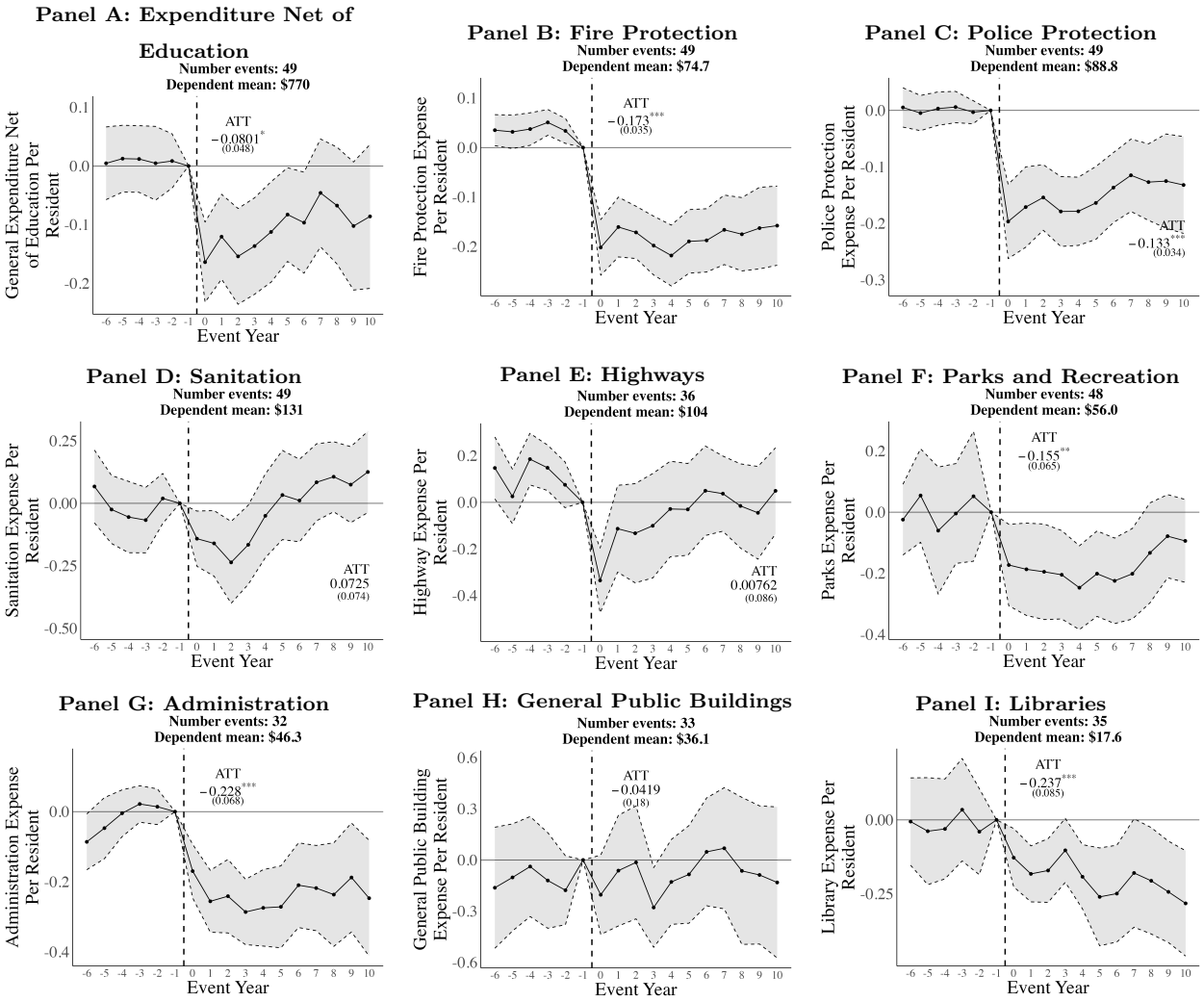
NOTES: This table compares 1950 characteristics of treated cities (column 1), annexing cities without a dated annexation spurt (column 2), and cities that did not annex (column 3), the groups of Figure 3, and of the event study's control cities (column 4). Column 4 pools the 32 region-by-base-year comparisons behind Equation 2, with each comparison's controls weighted to count as much as its treated cities; the (1) vs (4) p-value is from a regression with comparison fixed effects, clustered by city. Standard deviations are in parentheses, and the other p-values are from two-sample t-tests. Values in 2019 dollars. Honolulu, whose annexation history cannot be classified, is in no column.

Figure A.10: General Expenditure Around Annexation: Means by Event Year



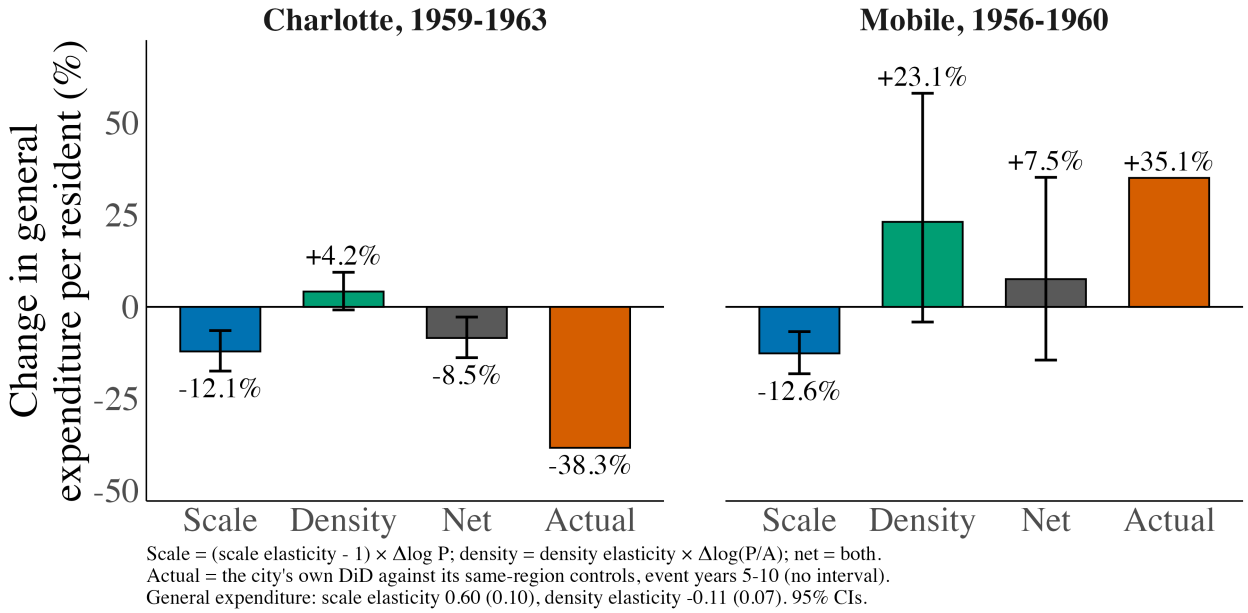
NOTES: This figure reports mean general expenditure (Panel A) and general expenditure per resident (Panel B) by event year, indexed to 100 in event year -1, for treated cities (solid) and comparison cities (dashed), on the sample and weights of Figure 5. Values in 2019 dollars.

Figure A.11: Impact of Annexation on Expenditures, All Functions (Log per Resident)



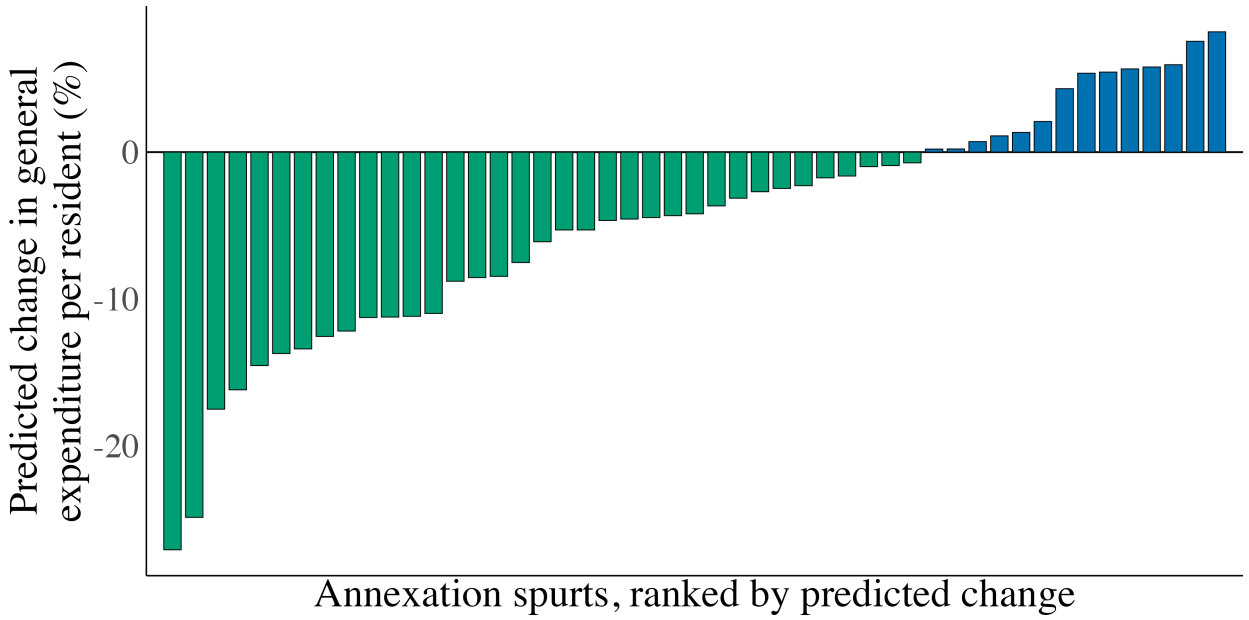
NOTES: This figure reports event-study estimates from Equation 2 for general expenditure net of education and the eight functions of Table 2, per resident, in logs. Administration is financial administration and general control and excludes the four city–county consolidations. Coefficients are relative to event year −1, and shaded bands are 95 percent confidence intervals. The pooled ATT averages event years 5 to 10. Standard errors are clustered at the city level. Values in 2019 dollars.

Figure A.12: Scale and Density: Charlotte and Mobile



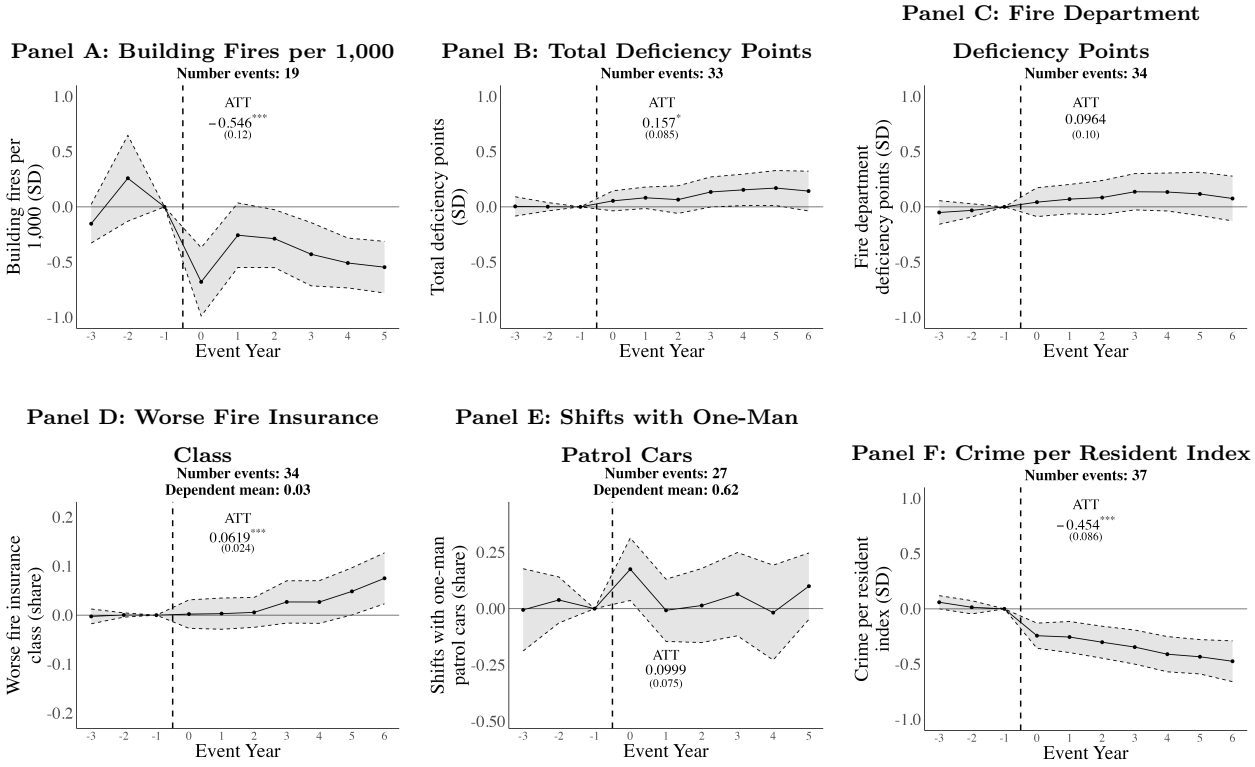
NOTES: This figure reports the change in general expenditure per resident that the scale and density elasticities of Table 2 predict for one city's annexation spurt, with 95 percent confidence intervals, beside the city's own difference-in-differences estimate. The scale bar is $(\theta_1 - 1)$ times the spurt's log change in population, the density bar is θ_2 times its log change in density, and the net bar is their sum. Charlotte's 1959–1963 spurt added 55,700 residents (38%) and 32.3 square miles (98%). Mobile's 1956–1960 spurt added 57,400 residents (40%) and 90.6 square miles (774%).

Figure A.13: Predicted Change in General Expenditure per Resident, by Annexation Spurt



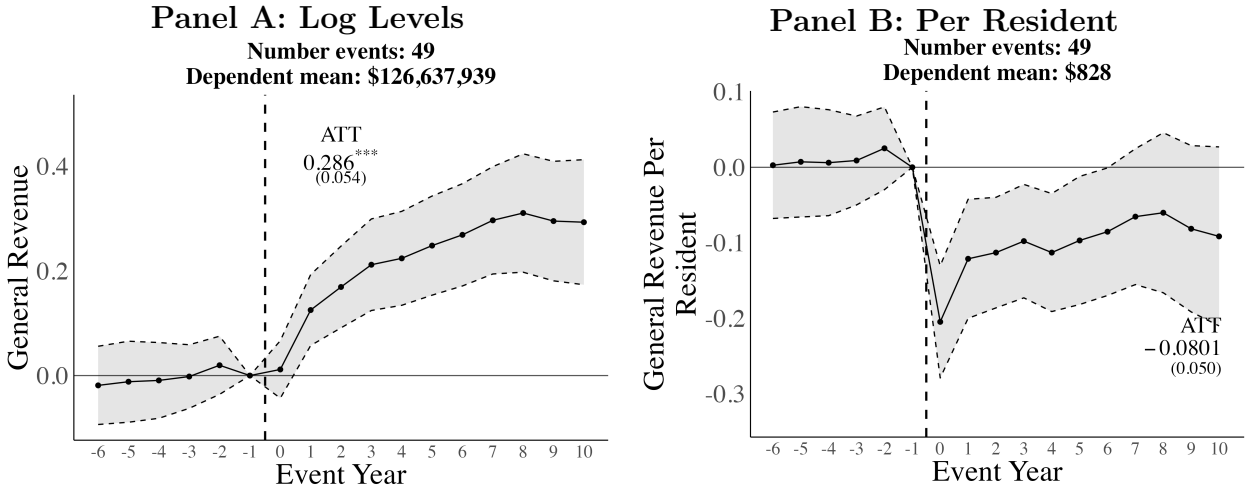
NOTES: This figure reports, for each of the 49 treated cities' annexation spurts, the change in general expenditure per resident predicted by the scale and density elasticities of Table 2: $(\theta_1 - 1)$ times the spurt's log population growth plus θ_2 times its log density change. Spurts are ranked by the predicted change. Green bars are predicted declines and blue bars predicted increases.

Figure A.14: Fire and Police Incidence and Service Quality: Event Studies



NOTES: This figure reports event-study estimates from Equation 2 for the incidence and quality rows of Table 3 observed every year, as in Figure A.17. Each vertical axis spans at least one standard deviation of the outcome on either side of zero.

Figure A.15: Impact of Annexation on Municipal Revenues



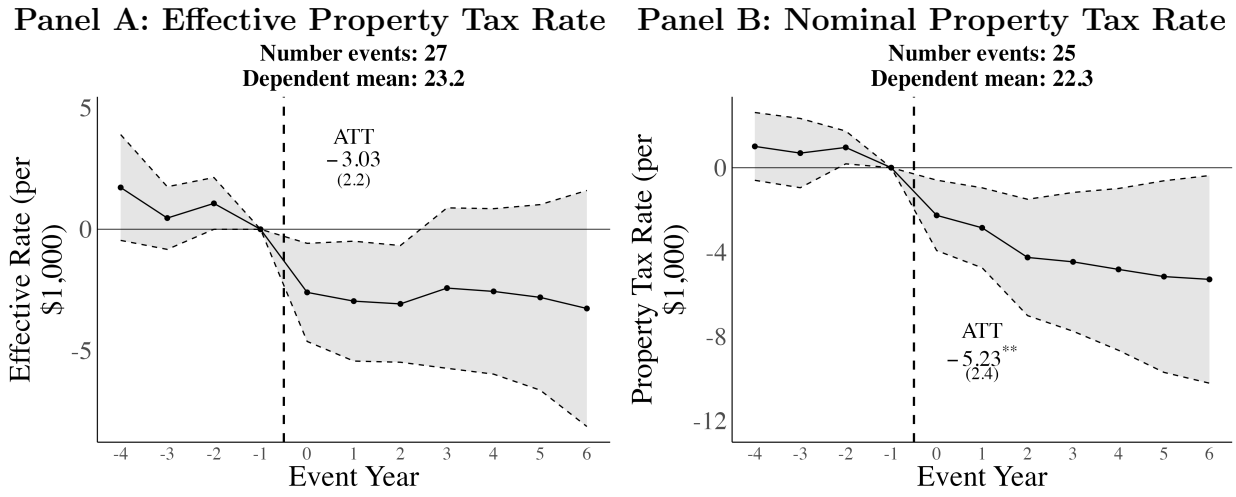
NOTES: This figure reports event-study estimates for general revenue from Equation 2, in log levels (Panel A) and log per resident terms (Panel B). Coefficients are relative to event year −1, and shaded bands are 95 percent confidence intervals. The pooled ATT averages event years 5 to the end of the event window. Standard errors are clustered at the city level. Values in 2019 dollars.

Table A.3: Impact of Annexation on Municipal Revenue and the Property Tax

	Total (1)	Per resident (2)	Treated cities (3)
Panel A: Expenditure and revenue			
General expenditure (log)	0.272***	-0.094*	49
	(0.055)	(0.054)	
General revenue (log)	0.286***	-0.080	49
	(0.054)	(0.050)	
Panel B: Own sources and transfers			
Own sources (log)	0.297***	-0.069	49
	(0.050)	(0.043)	
Intergovernmental (log)	0.458***	0.092	49
	(0.161)	(0.144)	
Own-source share of general revenue (pp)		0.074	49
		(1.273)	
Panel C: Property tax			
Property tax revenue (log)	0.303***	-0.063	49
	(0.052)	(0.042)	
Assessed valuation (log)	0.387***	0.030	27
	(0.063)	(0.042)	
Statutory tax rate (mills)		-5.228**	25
		(2.407)	
Effective tax rate (mills)		-3.032	27
		(2.198)	

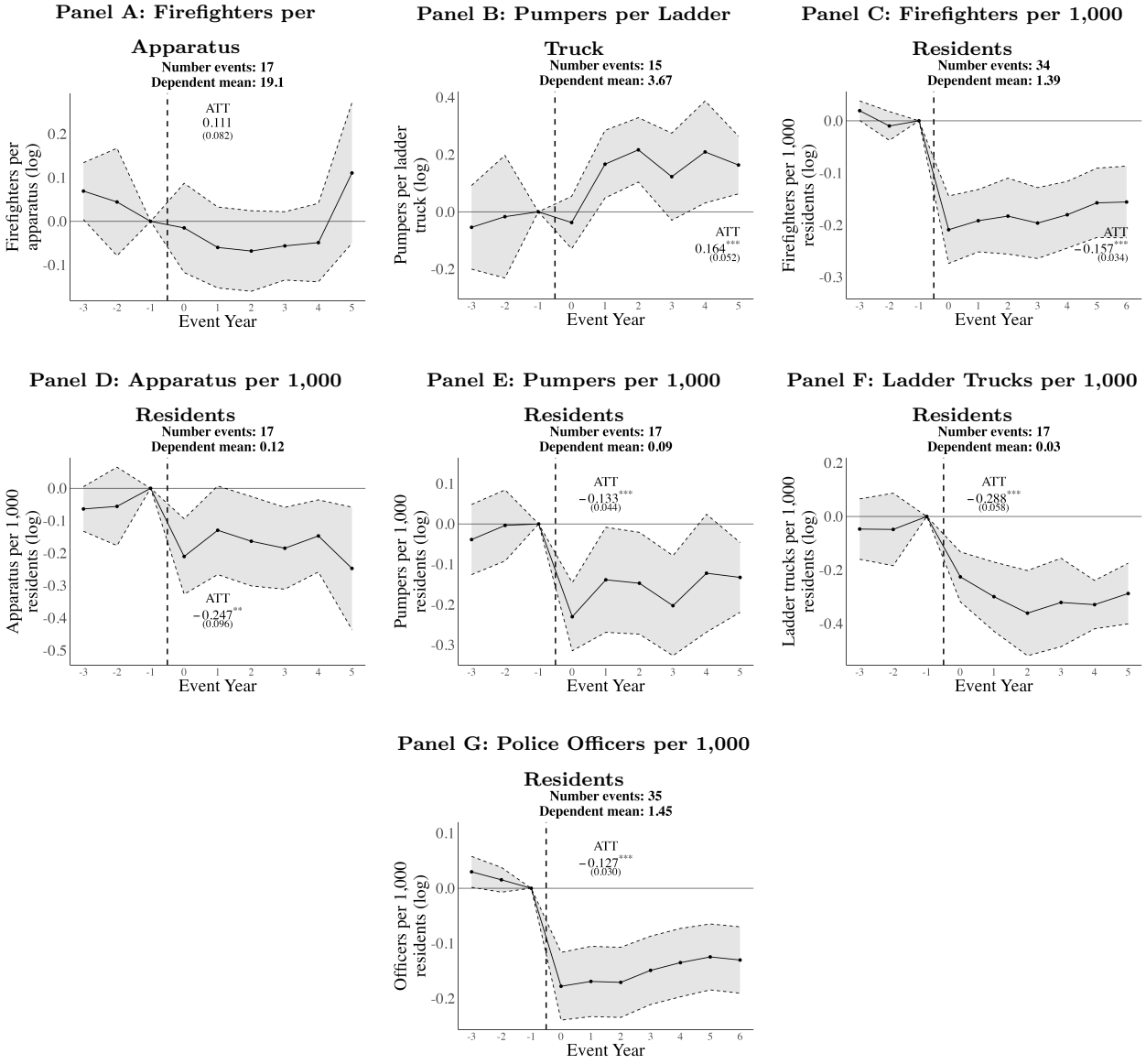
NOTES: This table reports pooled ATTs from Equation 2 for municipal expenditure, revenue, and the property tax, in total (column 1) and per resident (column 2). Log rows are pooled over event years 5 to 10. Assessed valuation and the statutory tax rate are from Moody's Municipal Manuals, estimated over event years -4 to 6 and pooled from event year 5. The effective tax rate is property tax revenue per \$1,000 of assessed valuation, estimated on the same window. Standard errors, clustered at the city level, are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.16: Municipal Property Tax Rates



NOTES: This figure reports event-study estimates from Equation 2 for the effective property tax rate, property tax revenue over assessed valuation (Panel A), and the nominal rate printed in Moody's Municipal Manuals (Panel B), both in mills. Coefficients are relative to event year −1, and shaded bands are 95 percent confidence intervals. The pooled ATT averages event years 5 to the end of the event window. Standard errors are clustered at the city level.

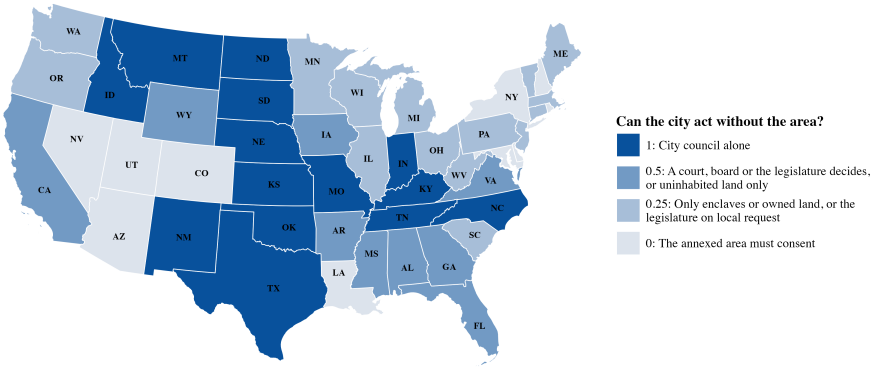
Figure A.17: Fire and Police Inputs: Event Studies



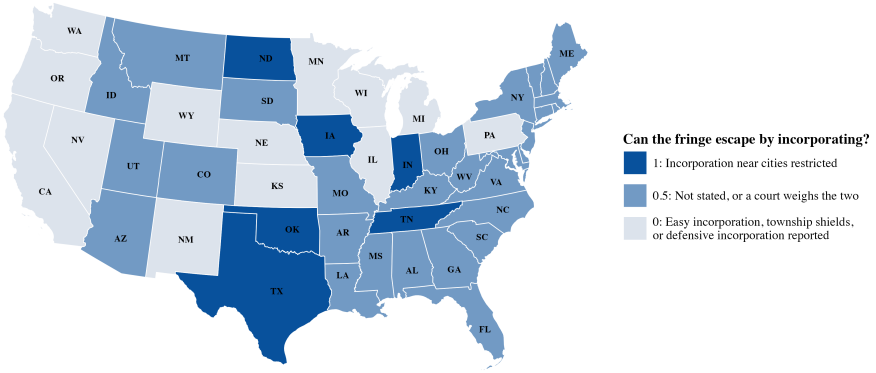
NOTES: This figure reports event-study estimates from Equation 2 for the rows of Table 3 observed every year, in logs. Each panel's window is the one, among $[-4, 6]$, $[-3, 6]$, $[-4, 5]$, and $[-3, 5]$, that keeps the most treated cities. Coefficients are relative to event year -1, and shaded bands are 95 percent confidence intervals. Standard errors are clustered at the city level.

Figure A.18: Components of the Annexation-Law Looseness Index, 1958

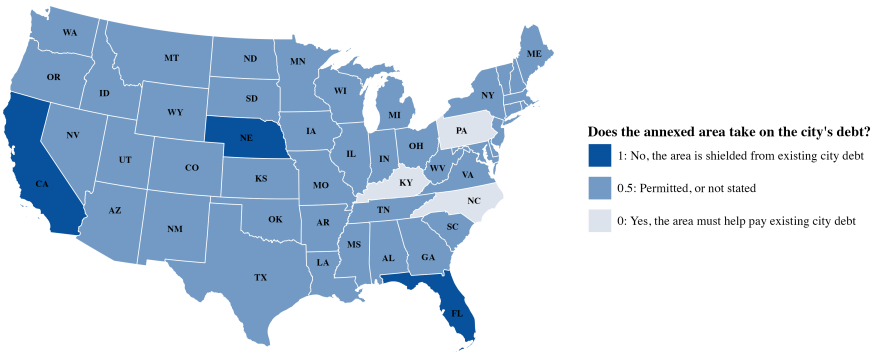
A. Can the city act without the annexed area?



B. Can the fringe escape by incorporating?



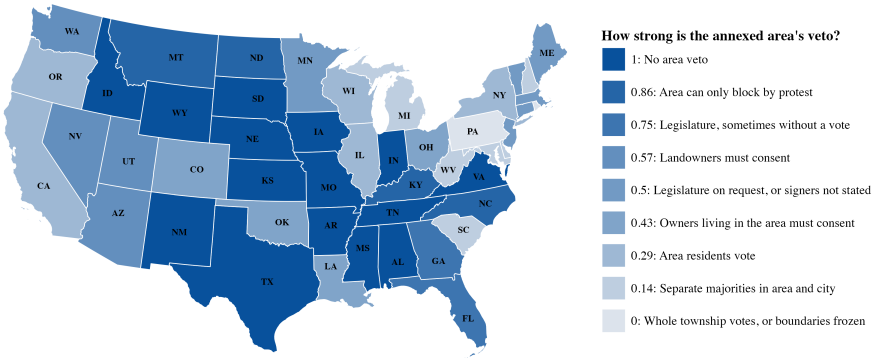
C. Does the annexed area take on the city's debt?



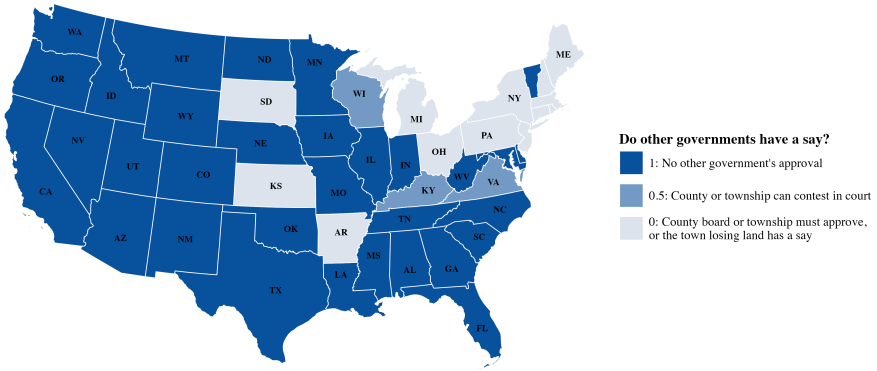
NOTES: Each map reports one of the three scores behind the looseness index of Figure 8, coded from Dixon and Kerstetter (1959) on each state's looser route to annexation, general law or special legislative act. Higher values are looser.

Figure A.19: Other Provisions of State Annexation Law, 1958

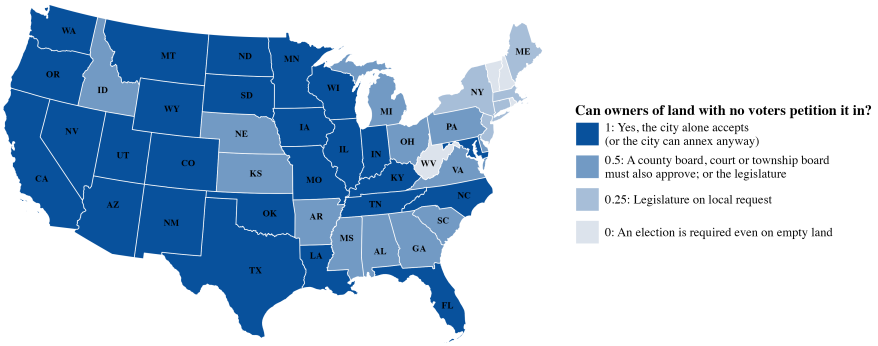
A. How strong is the annexed area's veto?



B. Do other governments have a say?

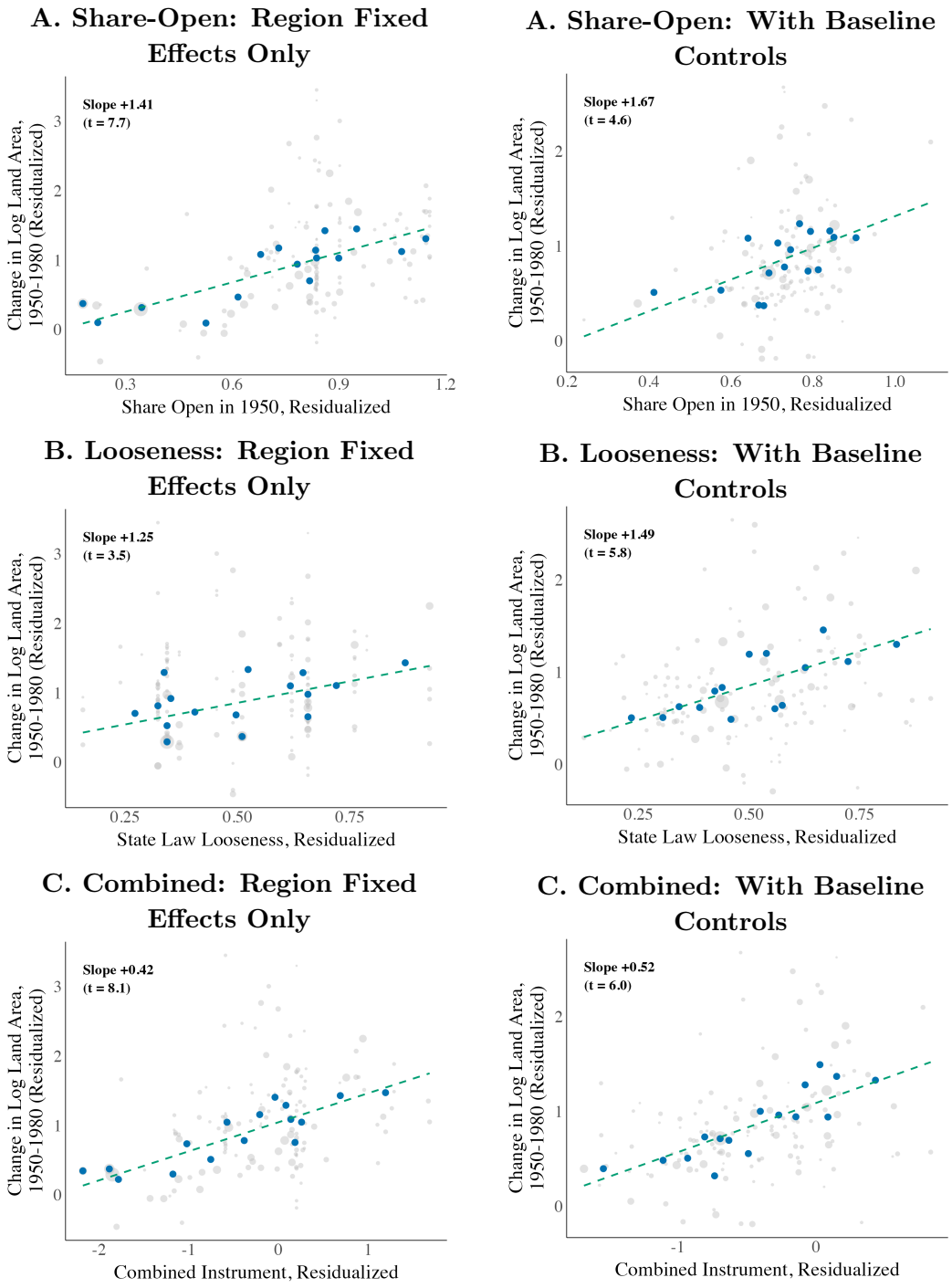


C. Can owners of land with no voters petition it into the city?



NOTES: Each map reports a provision of state annexation law that we code from Dixon and Kerstetter (1959) as in Appendix Figure A.18 but do not include in the looseness index of Figure 8. Higher values are looser.

Figure A.20: First Stage: City Land-Area Growth against Each Instrument



NOTES: This figure plots the change in log city land area between 1950 and 1980 against share-open (Panel A), the looseness of the state's annexation law (Panel B), and the combined instrument of Equation 8 (Panel C), residualized on census region fixed effects (left) and also on the baseline controls of Equation 9 (right). The looseness controls omit the municipal land share within 8 miles. Gray points are cities, blue points are 16 population-weighted bins, and the dashed line is the population-weighted fit, whose slope is printed on each panel. Standard errors are clustered by county for share-open and the combined instrument and by state for looseness.

Table A.4: Instrument: Baseline Balance

	Share-open (per s.d.)		Looseness (per s.d.)		Combined (per s.d.)		
Covariate	Within region	With controls	Within region	With controls	Within region	With controls	N
Panel A. 1950 Economic Structure, City and County							
City Population Density (logged)	-0.150***	-0.008	-0.211***	-0.002	-0.202***	-0.006	134
	(0.028)	(0.012)	(0.058)	(0.006)	(0.031)	(0.009)	
City Median House Value (logged)	-0.050***	-0.051*	-0.051***	-0.043**	-0.061***	-0.060**	133
	(0.010)	(0.029)	(0.017)	(0.021)	(0.012)	(0.024)	
City Median Gross Rent (logged)	-0.009	+0.007	-0.001	-0.010	-0.008	-0.005	133
	(0.012)	(0.025)	(0.024)	(0.018)	(0.014)	(0.021)	
City Median Family Income (logged)	-0.006	+0.013	-0.003	-0.008	-0.007	+0.001	124
	(0.008)	(0.017)	(0.017)	(0.013)	(0.010)	(0.016)	
City Share White	+0.016**	+0.001	+0.024	+0.015	+0.022***	+0.012	132
	(0.006)	(0.019)	(0.020)	(0.021)	(0.008)	(0.017)	
County Population Density (logged)	-0.438***	-0.052	-0.267**	-0.005	-0.479***	-0.028	134
	(0.074)	(0.036)	(0.115)	(0.026)	(0.084)	(0.025)	
County Share White	+0.012*	+0.000	+0.028	+0.024	+0.020***	+0.018	134
	(0.006)	(0.017)	(0.017)	(0.019)	(0.007)	(0.016)	
Panel B. Pre-1950 Trends							
County Population, 1930–1950 (Δ log)	-0.007	-0.001	+0.042*	+0.021	+0.007	+0.016	134
	(0.016)	(0.010)	(0.022)	(0.012)	(0.016)	(0.010)	
County Population, 1940–1950 (Δ log)	-0.003	-0.001	+0.027*	+0.021	+0.006	+0.016	134
	(0.008)	(0.010)	(0.014)	(0.012)	(0.008)	(0.010)	
City Population, 1940–1950 (Δ log)	+0.018**	+0.027*	+0.025	-0.006	+0.024**	+0.008	117
	(0.008)	(0.014)	(0.024)	(0.027)	(0.010)	(0.019)	
County Employment (CBP), 1947–1951 (Δ log)	+0.005	+0.012	+0.033	+0.023	+0.016*	+0.025	131
	(0.006)	(0.016)	(0.025)	(0.028)	(0.009)	(0.018)	
County Establishments (CBP), 1947–1951 (Δ log)	+0.000	+0.009	-0.001	-0.009	-0.000	-0.002	132
	(0.004)	(0.007)	(0.009)	(0.010)	(0.004)	(0.008)	
County Manufacturing Employment, 1947–1951 (Δ log)	+0.015	-0.006	+0.055**	+0.024	+0.031**	+0.016	131
	(0.010)	(0.026)	(0.026)	(0.028)	(0.014)	(0.024)	

NOTES: This table reports regressions of 1950 characteristics on each instrument, per standard deviation of the instrument: share-open (columns 1–2), the looseness of the state's annexation law (columns 3–4), and the combined instrument of Equation 8 (columns 5–6), each within region and with the first-stage baseline controls. Panel A is the city's and county's economic structure in 1950, and Panel B pre-1950 trends. All specifications include census region fixed effects and are weighted by 1950 city population. The looseness controls omit the municipal land share within 8 miles. Standard errors are clustered by county for share-open and the combined instrument and by state for looseness. Standard errors are in parentheses. Values in 2019 dollars. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Expenditure and Revenue Breakdown, IV and OLS

Outcome (Δ log per resident, 1952–1982)	IV	OLS	First-stage F	N
Expenditure, per resident				
Total expenditure	-0.129	-0.063	19.6	134
	(0.144)	(0.056)		
General expenditure	-0.187	-0.065	19.6	134
	(0.140)	(0.057)		
Police Protection	-0.128	-0.083*	19.6	134
	(0.103)	(0.043)		
Fire Protection	-0.178*	-0.206***	19.6	134
	(0.099)	(0.056)		
Highways	-0.617**	-0.079	19.6	134
	(0.288)	(0.078)		
Sanitation	-0.714**	-0.002	19.6	134
	(0.324)	(0.105)		
Parks & recreation	-0.613***	-0.282***	19.7	129
	(0.215)	(0.071)		
Libraries	+0.028	-0.300	16.2	92
	(0.360)	(0.210)		
General public buildings	+1.465**	+0.695***	15.4	122
	(0.603)	(0.201)		
Financial admin. & general control	-0.271	-0.074	19.6	134
	(0.175)	(0.080)		
Interest on debt	+0.756	+0.138	19.6	134
	(0.569)	(0.164)		
Salaries and wages	-0.022	-0.092**	19.6	134
	(0.104)	(0.044)		
Revenue, per resident				
Total revenue	-0.093	-0.002	19.6	134
	(0.135)	(0.052)		
General revenue	-0.005	+0.053	19.6	134
	(0.115)	(0.062)		
Property tax	+0.031	+0.016	19.6	134
	(0.234)	(0.084)		
Intergovernmental revenue	-0.914*	-0.169	19.6	134
	(0.497)	(0.194)		

NOTES: This table reports the effect of city land-area growth between 1950 and 1980 on spending and revenue per resident by category, by two-stage least squares instrumented by share-open (column 1) and by ordinary least squares (column 2), in log changes between 1952 and 1982. City land growth is instrumented through the first stage of Equation 9, with its baseline controls. F is the first-stage F. Standard errors, clustered by county, are in parentheses. Values in 2019 dollars. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: County Spending by Type of Government: Population Shares and Spending per Resident, 1957 and 1982

	Central city		Other municipalities		Countywide governments		County total	
	1957	1982	1957	1982	1957	1982	1957	1982
Panel A. Share of county residents (percent)								
Residents	59	50	18	28		(all residents)	100	100
Unincorporated					23	22		
Panel B. Spending per resident of the layer (2019 dollars)								
Fire protection	71	140	36	74	3	13	52	103
Police protection	93	228	56	148	11	40	77	194
Highways	119	135	101	121	55	52	144	156
Administration	34	89	39	81	36	86	62	151
Gen. exp. net of educ.	700	1,565	441	861	355	971	857	1,990

NOTES: This table reports the inputs to the decomposition of Equation 12 in the 90 central counties of Table 8: the share of county residents in each layer (Panel A) and each layer's spending per resident (Panel B), for the central city per city resident, for other municipalities per resident of those municipalities, and for countywide governments and the county total per county resident. Means are weighted by 1957 county population. Other municipalities' spending is averaged over the counties that have them. Values in 2019 dollars.

Table A.7: County Spending per Resident by Type of Government: Each Instrument Separately

		Coverage			Cost index			
			Into the	Into other			Other	
	Total	All	central city	municipalities	All	Central city	municipalities	Countywide
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: IV, instrumented by share-open (first-stage F = 19.1)								
Fire protection	−3.6	31.5**	33.5***	−2.0	−35.1***	−24.9*	−3.6	−6.7
	(7.4)	(12.4)	(11.7)	(3.5)	(10.8)	(13.1)	(2.8)	(5.2)
Police protection	1.3	19.5**	24.3***	−4.9	−18.1**	−3.3	−8.3**	−6.5*
	(7.4)	(7.9)	(8.4)	(3.9)	(9.0)	(8.4)	(3.6)	(3.5)
Highways	−22.2	18.2***	21.7***	−3.5	−40.4**	−24.9**	−1.8	−13.7**
	(17.4)	(5.0)	(6.6)	(3.8)	(19.4)	(12.4)	(4.2)	(7.0)
Administration	0.5	12.2***	14.5***	−2.3	−11.7	−13.1*	−2.4	3.9
	(8.5)	(4.3)	(3.7)	(2.5)	(9.0)	(7.1)	(1.7)	(11.0)
Gen. exp. net of educ.	0.7	17.2***	19.3***	−2.1	−16.4**	−14.4**	−1.9	−0.1
	(6.5)	(5.0)	(5.1)	(2.3)	(7.5)	(6.5)	(1.3)	(5.4)
Panel B: IV, instrumented by state annexation-law looseness (first-stage F = 5.1)								
Fire protection	−8.8	26.7*	23.3*	3.4	−35.4*	−26.7	2.3	−11.0
	(11.7)	(13.8)	(13.0)	(4.0)	(19.5)	(16.8)	(4.6)	(8.2)
Police protection	−15.0	18.3*	17.1*	1.1	−33.3*	−14.7	0.0	−18.6**
	(16.8)	(10.0)	(9.7)	(4.6)	(17.6)	(13.4)	(6.2)	(7.6)
Highways	4.7	16.8*	13.0	3.8	−12.1	−11.7	7.2	−7.6
	(21.3)	(9.4)	(8.0)	(4.3)	(23.9)	(19.6)	(5.8)	(12.4)
Administration	−18.6	11.7*	13.1*	−1.4	−30.3*	−32.3*	0.6	1.4
	(15.2)	(6.1)	(6.6)	(3.0)	(17.3)	(18.3)	(2.9)	(9.9)
Gen. exp. net of educ.	3.1	15.7**	15.0*	0.7	−12.6	−15.0	−0.7	3.1
	(10.1)	(7.1)	(7.6)	(2.5)	(12.4)	(11.1)	(2.6)	(10.8)

NOTES: This table reports the decomposition of Appendix Table A.8 by two-stage least squares instrumented by share-open (Panel A) and by state annexation-law looseness alone (Panel B), following Equation 13, per doubling of the central city's land area. The looseness specification omits the municipal land share within 8 miles from the controls. Standard errors, clustered by county for share-open and by state for looseness, are in parentheses: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Table A.8 breaks the decomposition of Table 8 into its parts, and the first row, fire protection instrumented by the combined instrument, shows how to read it. Every entry is the effect of a doubling of the central city's land area on one part of the change in county fire spending per resident from 1957 to 1982, in percent of the county's average fire spending per resident, about $77. Moving residents into the central city, which spends far more per resident on fire protection than countywide governments, adds 28.3 percent (column 3). The matching decline in the residents of other municipalities removes spending at their level, -0.1 percent (column 4), so coverage as a whole changes by 28.2 percent (column 2). Holding each layer's share of residents at its 1957 level, the cost index changes by -34.0 percent (column 5): -24.6 from the central city's spending per city resident, weighted by the city's 1957 share of residents (column 6); -1.5 from other municipalities, weighted by their 1957 share (column 7); and -7.9 from countywide governments, which serve every resident and so carry a weight of one (column 8). Coverage and cost together give the change in total fire spending per county resident, -5.8 percent (column 1).

Table A.8: County Spending per Resident by Type of Government: Coverage and Cost by Type

		Coverage			Cost index			
			Into the	Into other			Other	
	Total	All	central city	municipalities	All	Central city	municipalities	Countywide
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: IV, instrumented by the combined instrument (first-stage F = 22.7)								
Fire protection	−5.8	28.2**	28.3***	−0.1	−34.0***	−24.6**	−1.5	−7.9*
	(7.1)	(11.0)	(10.4)	(2.9)	(11.2)	(12.0)	(2.4)	(4.4)
Police protection	−5.7	18.3**	20.7***	−2.4	−23.9***	−7.8	−4.8	−11.4***
	(7.3)	(7.4)	(7.7)	(3.2)	(9.0)	(8.0)	(3.4)	(4.1)
Highways	−10.9	16.7***	17.3***	−0.6	−27.6	−19.2	2.3	−10.6*
	(15.7)	(5.3)	(6.0)	(3.1)	(17.4)	(12.3)	(3.7)	(5.7)
Administration	−7.6	11.3***	13.3***	−2.0	−18.9**	−20.1**	−1.4	2.6
	(8.0)	(3.9)	(3.8)	(2.1)	(9.1)	(7.8)	(1.5)	(9.4)
Gen. exp. net of educ.	0.9	15.8***	16.8***	−1.0	−14.9**	−13.8**	−1.5	0.4
	(5.5)	(4.8)	(5.0)	(1.9)	(6.9)	(6.5)	(1.5)	(5.0)
Panel B: OLS								
Fire protection	2.3	18.9***	17.3***	1.7	−16.6***	−8.3**	1.9	−10.2***
	(2.7)	(3.3)	(2.9)	(1.2)	(3.3)	(3.7)	(1.5)	(2.6)
Police protection	5.4**	14.5***	14.2***	0.3	−9.1***	0.1	−0.6	−8.5***
	(2.3)	(2.8)	(2.5)	(1.2)	(2.9)	(3.1)	(1.5)	(1.6)
Highways	5.0	10.3***	9.7***	0.6	−5.3	−1.9	2.4*	−5.9**
	(7.6)	(1.7)	(1.7)	(1.0)	(7.8)	(6.0)	(1.4)	(2.3)
Administration	0.1	9.0***	8.6***	0.4	−8.9**	−7.5***	0.3	−1.7
	(3.0)	(1.6)	(1.6)	(0.8)	(3.6)	(2.6)	(0.7)	(3.3)
Gen. exp. net of educ.	3.8	11.8***	11.0***	0.8	−8.0***	−6.1**	0.4	−2.3
	(3.2)	(1.9)	(1.8)	(0.8)	(2.9)	(2.4)	(0.7)	(2.5)

NOTES: This table breaks the two parts of Table 8 into their components from Equation 12: coverage into the central city and into other municipalities, and the cost index of the central city, other municipalities, and countywide governments (the county government, special districts, and townships). Each entry is the effect of a doubling of the central city's land area between 1950 and 1980, in percent of the mean of the county's 1957 and 1982 spending per resident. Columns (2) and (5) sum to column (1), columns (3) and (4) to column (2), and columns (6) to (8) to column (5). Specification, sample, and standard errors are as in Table 8. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.9: County-Level Economic Activity

Outcome	IV	OLS	First-stage F	N
Panel A. Business Activity, 1953–1982				
Establishments	+0.060	+0.098**	35.9	133
	(0.121)	(0.047)		
Employment	+0.041	+0.148**	35.9	133
	(0.187)	(0.063)		
Resident Births (1952–1982)	+0.124	+0.122**	35.9	134
	(0.091)	(0.048)		
Panel B. Population and Housing, 1950–1980				
County Population	+0.028	+0.089*	35.9	134
	(0.083)	(0.046)		
County Housing Units	+0.025	+0.100**	35.9	134
	(0.078)	(0.043)		
Panel C. Incomes and Housing Prices, 1950–1980 (constant dollars)				
Median Family Income	-0.078**	-0.002	35.9	134
	(0.032)	(0.015)		
Median Home Value	-0.040	-0.000	35.9	134
	(0.082)	(0.027)		
Median Gross Rent	-0.018	+0.026	35.9	134
	(0.043)	(0.022)		
Panel D. Housing Price-to-Rent Ratio, 1980 Level				
Price-to-Rent Ratio (logged)	-0.083	-0.013	35.9	134
	(0.095)	(0.033)		
Panel E. County Land Use, 1950–1980				
Developed Land	+0.038	+0.096***	35.9	124
	(0.063)	(0.029)		
Share Residential (pp)	-2.566**	-1.481***	35.9	124
	(1.006)	(0.428)		
Share Commercial (pp)	+2.546***	+1.000***	35.9	124
	(0.807)	(0.319)		
Share Industrial (pp)	+0.604	+0.086	35.9	124
	(0.540)	(0.239)		

NOTES: This table reports the effect of city land-area growth between 1950 and 1980 on county economic activity, population, and housing, by two-stage least squares instrumented by the combined instrument (column 1, Equation 8) and by ordinary least squares (column 2); panel heads give each window. Panel D is the 1980 log ratio of median home value to annual rent, and Panel E counts developed 250-meter cells from the HISDAC-US land use surfaces. Both columns use the baseline controls. F is the first-stage F. Standard errors, clustered by county, are in parentheses. Values in 2019 dollars. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.10: Division of the County, 1980

Outcome	IV	OLS	First-stage F	N
Panel A. Across Municipal Jurisdictions				
Population Fractionalization	-0.310***	-0.145***	23.1	134
	(0.055)	(0.026)		
Black/White Dissimilarity	-0.171***	-0.109***	23.1	134
	(0.060)	(0.022)		
Panel B. Across Neighborhoods				
Black/White Dissimilarity across Tracts	+0.063	+0.001	23.1	134
	(0.041)	(0.011)		
Mean Distance from the CBD (km)	-1.33	+0.47	20.7	114
	(1.48)	(0.37)		

NOTES: This table reports the effect of city land-area growth between 1950 and 1980 on the 1980 division of the county, by two-stage least squares instrumented by share-open (column 1) and by ordinary least squares (column 2). Fractionalization is $1 - \sum_j s_j^2$ over the county's municipalities and its unincorporated remainder, with s_j each unit's population share, and dissimilarity is $D = \frac{1}{2}\sum_j |b_j/B - w_j/W|$, where b_j and w_j are the Black and white populations of unit j and B and W the county totals; the units are the county's municipalities and its unincorporated remainder, or its census tracts. City land growth is instrumented through the first stage of Equation 9, with its baseline controls. F is the first-stage F. Standard errors, clustered by county, are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Data Appendix

This appendix documents further details on the sourcing and construction of the underlying data described in Section III..

A. Municipal Land Areas

We use a combination of sources to construct changes in municipal land area from 1940 through 1990.

A.a. Anchor-Year Sources

We identify years for which we observe a cross-section of almost all incorporated municipalities. We use this intermittent series to generate long-run changes in municipal areas across cities and regions as well as to corroborate completeness of the annual panel. The cross-sectional anchor years are: 1934, 1940, 1944, 1950, 1960, 1970, 1980, 1990, and 2000. For the decennial Census years, we recover land areas from published census tables that we OCR. For 1934 and 1944, the data come from the Municipal Year Book's "Governmental Data" tables, hand-transcribed from scanned pages.

A.b. Annexation Event Sources

Between anchor years, dated annexation events are drawn from a mixture of sources. The primary sources are the Municipal Year Book's annual annexation survey (1948–1967) and the Census Bureau's BAS (1970 onward) as described in Section III.. We supplement these data with individually collected and publicly available city records from annexation ordinances or GIS shapefiles, listed with their sources in Table B.1, and we use hand-collected archival material, including newspaper archives (*Newspapers.com*) used to fill the Municipal Year Book's 1944–1947 and 1968–1969 gaps and to supplement cities with otherwise incomplete coverage. City–county consolidations, listed in Section III., are tracked as a distinct source from ordinary annexation, since a consolidation is a one-time merger of city and county government rather than an incremental boundary change.

Table B.1: Hand-Collected City Annexation Records

City	Record	Provider	Years	Annexation years	
				All	1942–80
Abilene, TX	GIS layer	City of Abilene	1895–2025	47	19
Albuquerque, NM	GIS layer	City of Albuquerque†	1891–2019	73	34
Atlanta, GA	GIS layer	City of Atlanta Planning†	1904–2025	53	20
Austin, TX	GIS layer	City of Austin	1946–2023	73	30
Bakersfield, CA	GIS layer	City of Bakersfield GIS	1898–2026	86	36
Baton Rouge, LA	GIS layer	East Baton Rouge Parish GIS	1923–2018	46	27
Billings, MT	GIS layer	Yellowstone County	1948–2025	74	30
Boise, ID	GIS layer	City of Boise†	1866–2023	87	32
Charlotte, NC	GIS layer	City of Charlotte	1907–2024	63	26
Columbus, OH	GIS layer	City of Columbus	1909–2025	94	30
Corpus Christi, TX	GIS layer	City of Corpus Christi GIS	1852–2026	58	26
Dallas, TX	GIS layer	City of Dallas GIS†	1900–2017	92	38
Dayton, OH	GIS layer	Montgomery County Auditor (2017 public copy)	1805–2014	68	25
Decatur, IL	GIS layer	City of Decatur	1829–2026	141	36
El Paso, TX	GIS layer	City of El Paso Planning	1873–2021	53	25
Evansville, IN	GIS layer	Evansville–Vanderburgh County GIS	1817–2011	42	12
Fort Wayne, IN	GIS layer	Allen County GIS	1931–2025	52	22
Fresno, CA	GIS layer	City of Fresno	1885–2021	94	38
Gainesville, FL	GIS layer	City of Gainesville†	1869–2021	41	7
Greensboro, NC	GIS layer	City of Greensboro	1808–2025	84	32
Houston, TX	GIS layer	City of Houston	1836–2023	80	26
Joliet, IL	GIS layer	City of Joliet	1852–2024	96	39
Lawton, OK	GIS layer	City of Lawton	1901–1985	49	31
Lincoln, NE	GIS layer	City of Lincoln†	1908–2025	82	31
Little Rock, AR	GIS layer	City of Little Rock†	1943–2023	74	35
Lubbock, TX	GIS layer	City of Lubbock	1909–2026	56	22
Milwaukee, WI	GIS layer	City of Milwaukee	1846–2002	67	23
Mobile, AL	GIS layer	City of Mobile	2023	1	0
Omaha, NE	GIS layer	City of Omaha	1854–2019	84	32
Orlando, FL	GIS layer	Not recorded	1950–2026	76	30
Phoenix, AZ	GIS layer	Maricopa County‡	1885–2023	101	34
Pueblo, CO	GIS layer	City of Pueblo	1947–2026	70	32
Raleigh, NC	GIS layer	City of Raleigh	1857–2010	65	30
Reno, NV	GIS layer	City of Reno	1946–2024	73	31
Richmond, VA	GIS layer	City of Richmond	1810–1970	8	2
Riverside, CA	GIS layer	Not recorded	1954–2016	46	22
Roanoke, VA	GIS layer	City of Roanoke	1874–2004	15	6
Rockford, IL	GIS layer	City of Rockford	1900–2025	103	38
Sacramento, CA	GIS layer	City of Sacramento	1849–2024	44	24
Salem, OR	GIS layer	City of Salem†	1903–2024	76	30
Salt Lake, UT	GIS layer	Salt Lake County	1911–2008	80	35
San Antonio, TX	Ordinance list	City of San Antonio	1940–2018	58	27
San Bernardino, CA	GIS layer	Third-party ArcGIS layer	1866–2025	67	35
San Jose, CA	GIS layer	City of San Jose	1911–2023	87	36

Continued on next page

Table B.1: Hand-Collected City Annexation Records (Continued)

City	Record	Provider	Years	Annexation years	
				All	1942–80
Savannah, GA	GIS layer	SAGIS (Savannah–Chatham County)†	1969–2024	31	1
Shreveport, LA	GIS layer	City of Shreveport	1908–2021	82	35
Sioux Falls, SD	GIS layer	City of Sioux Falls	1934–2024	78	32
South Bend, IN	GIS layer	St. Joseph County	1865–2025	80	34
Stockton, CA	GIS layer	San Joaquin County (SJMap)†	1850–2023	74	36
Tallahassee, FL	GIS layer	Tallahassee–Leon County GIS†	1824–2024	55	10
Tampa, FL	GIS layer	City of Tampa (Tampa Monitor copy)	1855–2013	23	3
Topeka, KS	GIS layer	City of Topeka	1856–2024	62	22
Tucson, AZ	GIS layer	City of Tucson	1877–2024	88	33
Tuscaloosa, AL	GIS layer	City of Tuscaloosa	1949–2026	76	30

NOTES: This table lists the individually collected city annexation records in the annexation source file, with links to each provider. † marks a link to the provider's data portal and ‡ a page believed, but not confirmed, to host the record. Years gives the first and last annexation year in the record, and annexation years counts the distinct years with a recorded annexation, overall and within the 1942–1980 land panel.

Together, we use each of these sources in tandem with the anchor years to create an annual panel of annexed land areas that is externally corroborated by anchor-year cross-sections.

A.c. Coverage

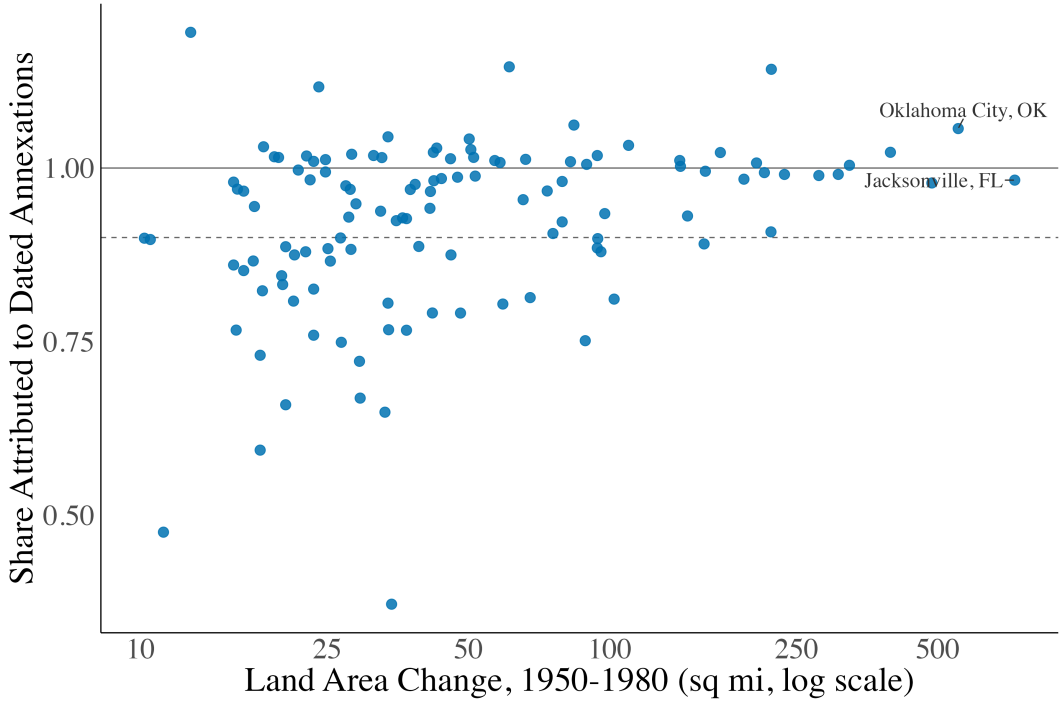
For 97% of the 177 sample cities, we have a measured land-area anchor in every anchor year (1944, 1950, 1960, 1970, and 1980).[24] Of the 117 cities whose land area grew by more than 10 square miles between 1950 and 1980, the median city has 97% of that growth attributed to specific annexation years, and 63% have nearly complete coverage (Figure B.1).

B. Annual City Population

To complement our annual land area series, we construct an annual municipal population series that incorporates discrete jumps from annexed populations. As there is no annual series of populations, we use known population anchors mixed with population jumps through annexation smoothed via a geometric growth series.

[24]The published 1950 Census land area for Reading, PA contains a transcription error, which we correct.

Figure B.1: Share of Land-Area Growth Attributed to Dated Annexations, 1950–1980



NOTES: This figure reports, for the 117 sample cities whose land area grew by more than 10 square miles between 1950 and 1980, the share of that growth attributed to dated annexations, against the growth itself on a log scale. The solid line marks a share of one and the dashed line a share of 0.9.

B.a. Population Anchors

As known population anchors, we use decennial census population counts (1940–1990). These counts are published via Census publications and corroborated with data from County and City Data Books (United States Bureau of the Census 2008a). We supplement these decennial anchors with 32 hand-collected figures for individual sample cities, mostly special censuses published within the subsequent decennial census, as noted in Section III..

B.b. Annexed Population

Annexed population is measured through a series of complementary data sources. The Municipal Year Books (1948–1967) report estimated population annexed into the city but coverage relative to annexed land areas is sometimes incomplete or understated. Between 1951 and 1970, decadal Census Bureau supplementary reports directly tabulate the population of areas annexed to central cities in the prior decade. Relative to the MYB estimates, these estimates report annexed population over a decade and reflect national population growth. To blend both data sources, we assess the MYB value as a minimal annexed population in that year, and then use backward-projected assessments to reallocate the rest of population proportionally.

Beyond these two primary data sources, we compare Current Population Report estimates of each city's 1970 population within its 1975 boundary with its 1970 population within its 1970 boundary to recover the population annexed in 1970–1975. We then backcast this estimate in the same way we do for the Census reports. We also use a small sample of hand-collected figures on large annexations and consolidations from newspaper reports. Finally, for cities with disaggregated shapefiles of individual annexations, we recover the 1980 population of each annexed area, which we backcast to cover missing data.

B.c. Annual Interpolation

We interpolate between anchors and known population jumps by assuming geometric natural population growth, following the recursion for pop_t in Section III.. The growth rates $\{\alpha_t\}_{t=\tau_0}^{\tau_K-1}$ minimize

$$\sum_{k=1}^{K}\left[\log\left(\text{pop}_{\tau_k}-\text{popannex}_{\tau_k}^{\text{post}}\right)-\log\text{anchor}_{\tau_k}\right]^2+\lambda\sum_{t=\tau_0+1}^{\tau_K-1}w_t\left(\alpha_t-\alpha_{t-1}\right)^2, \tag{B.1}$$

where $\lambda = 10$ and $\text{popannex}_{\tau_k}^{\text{post}}$ is the population annexed after the April 1 census date of anchor year τ_k, defined below. The weight on each change in the growth rate is

$$w_t=\max\left\{0.1,\ \frac{1}{1+5\,\text{popannex}_t/\text{pop}_{t-1}}\right\}\cdot\left(1+0.1\,d_t\right).$$

Here d_t is the number of years from t to the nearest anchor year.

Anchor-year annexations occasionally require adjustments to the decennial anchors where the exact dating of the annexation relative to the Census enumeration is unknown. In Equation B.1, the anchor is therefore compared with the fitted level net of $\text{popannex}_{\tau_k}^{\text{post}}$, the population annexed after April 1 of the anchor year. An annexation in an anchor year counts as post-census if the land adjudication in Appendix A. places more of that year's annexed land in the period opening at the anchor year than in the period closing there. Where the land evidence is silent or tied, an annexation counts as post-census if the census annexed-population reports assign it to the decade opening at the anchor year.

C. Municipal Boundary Geospatial Data

This subsection documents our reconstruction of city boundaries in 1950 and 1980. These data supply inputs to Section V. and are used to construct the share-open instrument in Section A..

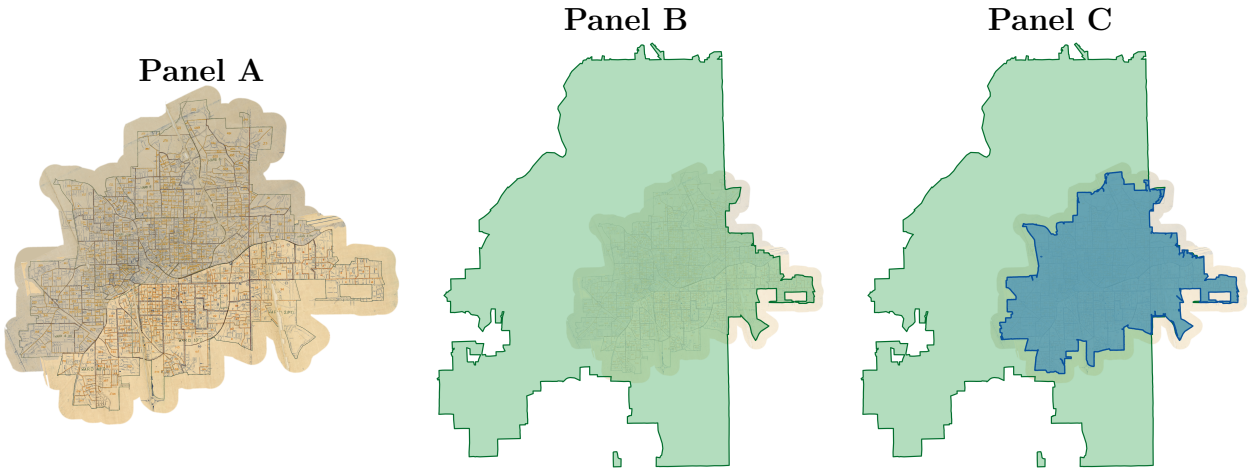
C.a. Place and County-Subdivision Shapefiles

The 1980 Census place shapefile (Manson et al. 2024) supplies the 1980 universe of incorporated and census-designated places, with each place's incorporation year added from Goodman's municipal incorporation-date dataset (Goodman 2021).

C.b. Sources of 1950 Boundaries

Our main source for recovering 1950 boundaries is scans of enumeration-district maps from the 1950 census, on which the city limit is marked. We georeferenced each map to modern street centerlines and traced the city limit by hand; Figure B.2 shows Atlanta as an example of the 1950 and 1980 boundaries overlaid. We complement the georeferenced city boundaries with the 31 sample cities for which we collect the public GIS shapefile recording dated annexations. These additional data are used to verify the tracing process, and we validate the resulting land area against published land area statistics.

Figure B.2: Tracing Atlanta's 1950 Boundary



NOTES: Panel A is the 1950 Census enumeration-district map of Atlanta (two sheets). Panel B overlays the 1980 city boundary (green), and Panel C adds the 1950 city limit traced from the map (blue).

C.c. Constructing 1950 City Boundaries

For each city, we compare the candidate 1950 boundaries from each source and adopt the one with the most direct evidence whose land area matches the published 1950 land area. Where a city has no traced map or annexation record, we fall back on the union of its 1950 census tracts, which were drawn within city limits, or, for cities whose land area did not change, on its 1940 or 1960 tracts or its 1980 boundary. For the few cities with none of these, we approximate the 1950 boundary with a hull around the city's geocoded 1950 census households, grown to the published land area

(Appendix A.a.); these approximations are used only to construct the instrument, never to measure outcomes inside the 1950 boundary.

C.d. City Core and Annexed Territory, by Decade

We use the 1950 and 1980 boundaries to divide each annexing city into its 1950 core and the territory it annexed between 1950 and 1980. Both areas are held fixed across census years, so changes in Table A.1 reflect changes in the residents of the same land rather than changes in boundaries.

D. Census Population and Housing Characteristics

We measure the 1950 characteristics of cities and their counties from the IPUMS 1950 full-count census (Ruggles et al. 2024), including the racial composition, nativity, and industry and occupation of residents. City median family income, house values, rents, and housing units in 1950 come from the County and City Data Book (United States Bureau of the Census 2008a).

For areas within cities, namely the 1950 core, the annexed territory, and the rings around each city in Appendix E, we use census tract tables and boundaries for 1950 through 1980 from NHGIS (Manson et al. 2024). We allocate tract counts to each area in proportion to the share of the tract's land that falls within it, and average tract medians weighted by the population, families, or housing units they describe. Where tract data are not available, such as family income by tract in 1950, we use citywide or countywide figures and mark the resulting estimates in the tables.

Metropolitan population sums county populations over each city's metropolitan area, held fixed at the 2013 definitions. Our measure of land unavailable for development is that of Saiz (2010), matched to each city's metropolitan area.

E. Municipal Finance and Employment Records

Our municipal finance panel combines two sources. For 1946 through 1950, we digitize the Census Bureau's annual *Compendium of City Government Finances* through OCR and hand validation.[25] From 1951 onward, we use the Census Bureau's Annual Survey of Governments. The panel begins in 1946 because the predecessor series, *Financial Statistics of Cities*, covers a smaller set of cities.

[25] United States Bureau of the Census (1947–1951).

The two sources report spending by function on different bases: the earlier publications report operating spending only, while the Annual Survey of Governments reports total spending, including capital outlay. For police, fire, and sanitation, we place the earlier figures on a total-spending basis using the ratio of total to operating spending in the years both sources cover. We treat reported zeroes as missing for items that an operating city government always reports, such as general expenditure and police spending, and interpolate gaps of three years or less.

E.a. Variable Definitions

All dollar values are in 2019 dollars, and per-resident values use the annual population series of Appendix B.. Finance variables follow the Census Bureau's definition of general government, which excludes city-owned utilities, liquor stores, and employee retirement funds. General expenditure covers all general-government functions, and general current expenditure excludes capital outlay; general expenditure net of education subtracts any spending on schools. Sanitation includes both sewerage and garbage collection. General revenue is the sum of taxes, current charges, intergovernmental transfers, and miscellaneous revenue, and own-source revenue excludes intergovernmental transfers. Cumulative capital outlay and debt issued sum a city's annual values over 1952–1982.

E.b. City Government Employment

City government employment and payroll come from the Census Bureau's City Government Employment survey, conducted each October from 1952. Municipal payroll covers all full- and part-time employees of the city government.

F. Fire and Policing Data

The Municipal Year Book's annual surveys of fire departments (1950–1967 and 1970) and police departments (1950–1968 and 1970) report staffing, salaries, equipment, and activity for each city. We hand-transcribed them from scanned pages and harmonized the columns, whose headings change across editions. Staffing counts the departments' full-time paid employees. The fire surveys also report the ratings of the National Board of Fire Underwriters, which grades each city's fire defenses by charging deficiency points against a standard schedule and assigns an insurance class from 1

(best) to 10. Because editions reprint gradings that are often several years old, we date each rating to the year of the grading.

Crime data come from the FBI's Uniform Crime Reports, compiled by Kaplan (2021) from 1960 and hand-digitized for 1953–1959.

F.a. Rows of the Fire and Police Panels of Table 3

Column (1) of Table 3 reports the mean of each outcome among annexing cities in the year before the spurt. Column (2) compares the change in the outcome for annexing cities with the change for comparison cities over the same years, using the comparison group of the event study. The comparison runs from the year before the spurt to the third year after, or to the last year an item is reported where the surveys stop asking it. The crime index is the average of the murder, aggravated assault, burglary, auto theft, and robbery rates, each standardized.

G. Municipal Streets and Local Roads by State

Panel D of Table 3 compares states rather than cities. Municipal street spending comes from the Bureau of Public Roads' *A Quarter Century of Financing Municipal Highways, 1937–61*, and road and street mileage by surface type from *Highway Statistics*; we digitized both from scanned pages. The instrument is the growth of the land area of a state's sample cities between 1950 and 1960, and the endogenous variable is the change in those cities' share of the state's urban population.

H. Municipal Waste Inventories

The sewerage rows of Table 3 come from two federal inventories of municipal sewer systems, the Public Health Service's for 1957 and the Federal Water Quality Administration's for 1968, which we digitized from scanned pages. Each lists every community's treatment plants, with the population they serve, the type of treatment and sewers, and the plants' design capacity. We measure the plants each city runs itself, and exclude cities served entirely by another community's or a district's system.

I. Moody's Municipal Manuals

Moody's *Municipal & Government Manual*, an annual guide for bond investors, reports each city's assessed valuation, property tax rates, tax levy and collections, and bond rating. We digitize the manuals from the 1954 edition onward, using prior years reported in the earliest editions to extend the series back to 1952. Because the same city-year often appears in several editions, we reconcile the transcriptions across editions and check outliers against the scanned pages.

Moody's often prints both a city-only tax rate and a combined rate that includes the county, schools, and other overlapping governments; we use one rate for each city throughout, the city rate wherever the city reports it. When a city changes the basis on which it assesses property, its assessed value and tax rate jump in opposite directions; we identify these changes and rescale later years to the earlier basis. The effective tax rate is property tax revenue, from the Census Bureau, divided by Moody's assessed valuation.

J. County-Level Data

J.a. County Business Patterns

County employment and establishments by industry come from County Business Patterns, which we assemble from several vintages: a panel covering 1947–1974, Census county files for 1975–1985 that we digitized, and the imputed series of Eckert, Fort, et al. (2021) and Eckert, Lam, et al. (2022). County Business Patterns was not conducted in 1952, and the 1949 and 1950 surveys report manufacturing only.

J.b. County Population

County population combines decennial census counts from NHGIS (Manson et al. 2024) with intercensal estimates from the Census Bureau and interpolation where neither is available.

J.c. County and County-Area Finance

County finance comes from the Census of Governments, conducted every five years from 1957, which reports the finances both of county governments and of all local governments within each county. To

divide county spending by type of government in Section B., we digitize the 1957 tabulation of local government finances by type of government in the central county of each metropolitan area (United States Bureau of the Census 1959), and for 1982 we use the Census Bureau's file of the finances of every individual local government (U.S. Census Bureau 2012). We group governments into the central city, other municipalities, and countywide governments (the county, special districts, and townships), and assign residents to each by where they live.

J.d. Other County Data

County housing, income, home values, and rents in 1950 and 1980 come from the County and City Data Book (United States Bureau of the Census 2012; United States Bureau of the Census 2008b), along with counts of overlapping local governments. Definitions can differ across editions: for example, median home value covers nonfarm single-family homes in 1950 and non-condominium homes in 1980. County net migration comes from Winkler et al. (2013), and births from Bailey et al. (2018).

J.e. Land Use

County land use comes from the HISDAC-US historical land use surfaces (Leyk and Uhl 2018), which date the construction of today's properties on a 250-meter grid. Because the data are built from present-day records, they miss structures demolished since, and we exclude counties with implausible jumps in recorded construction.

C Event Study Appendix

This appendix describes how we select the sample, how the event study sorts cities into treated and comparison cities, and how the main estimates change under alternative estimators and alternative definitions of treatment and control.

A. Sample Construction

Our sample is the set of cities that were the largest incorporated place in their county in 1950, had at least 25,000 residents in 1950, and had at least 75,000 residents in 1990. We exclude Anchorage, a unified city-borough, and New York, Los Angeles, and Chicago, whose size and governments are unlike those of other cities. This leaves 177 cities, which every exhibit and design in the paper uses.

B. Identifying Treated Cities

We define a treated city by an annexation spurt: a short window in which the city annexed a large amount of land or population relative to its annexations in the surrounding years. We search for spurts only among cities that, between 1940 and 1980, annexed at least 15 square miles while growing their land area by more than 20 percent, or more than 10 square miles while more than doubling it. A *land spurt* is a five-year window, beginning in 1952 or later, that adds at least 10 square miles, including at least 1.5 square miles in its first year, and at least 5,000 residents. The window must account for over half of the land the city annexed from ten years before its start to eight years after, for at least 85 percent of the land annexed in the window and the nine years before it, and for at least 70 percent of the population annexed over those same fourteen years. These conditions separate a city that expanded once from a city that expanded continuously. A *population spurt* applies the same conditions with land and population reversed, with a threshold of 10,000 residents annexed, at least 1,000 of them in its first year, and no minimum land area. It must also account for at least half of the land the city annexed from ten years before its start to eight years after. We date each city by its earliest spurt. The one exception is Toledo, whose 1959–60 annexation the rule dates to 1960; we date it to 1959 from the city's records.

Because our annual annexation records are incomplete for some cities, missing records in the

years before a window could make steady annexation look like a single burst. We guard against this with the land areas measured in the anchor years: we attribute any land growth that the annual records miss to the years before the window, and require the window to remain concentrated after doing so, allowing a small tolerance for measurement error in the anchor-year land areas. Finally, a spurt enters the event study only if general expenditure is observed in nearly every year from six years before to ten years after it. This yields 49 treated cities, with spurts beginning between 1952 and 1976. Table C.1 lists them, with the years of each spurt, the land and population it added, and, where the sources record them, the legal terms of the annexation, and Figure C.1 shows two examples.

Table C.1: Annexing Cities in the Event Study

		Land added		Population added			Land area		Terms of annexation		
City	Spurt	Sq mi	%	Persons	%	Density	1940	1980	Method	Consent	Home-stead
Ann Arbor, MI	1954–1958	6.9	95	14,806	29	2,149	6.0	24.6			
Atlanta, GA	1952–1954	88.6	241	132,232	40	1,492	34.7	131.0	Legis.	Yes	$2,000
Beaumont, TX	1957	40.1	127	8,000	7	200	10.4	72.9			
Cedar Rapids, IA	1961–1965	14.7	51	8,420	9	572	27.1	53.9			
Charleston, SC	1960	9.8	218	15,984	24	1,633	4.5	25.5			
Charlotte, NC	1959–1963	32.7	99	55,872	38	1,707	19.3	139.7			No
Columbia, SC	1968	80.0	592	28,372	32	355	8.8	107.1			
Columbus, GA	1958	13.8	101	40,000	50	2,899	6.3	217.5			$2,000
Columbus, OH	1954–1958	47.6	116	66,701	17	1,401	39.0	180.9	Area	Yes	No
Davenport, IA	1958–1959	28.0	155	8,274	10	295	18.1	59.5			
Dayton, OH	1952–1956	5.2	21	19,741	8	3,830	23.7	48.4			
El Paso, TX	1952–1955	73.8	247	113,200	84	1,534	13.6	239.2	City	No	No
Eugene, OR	1964–1967	10.7	92	23,368	45	2,178	4.9	32.5			
Fort Wayne, IN	1955–1959	15.9	93	25,906	19	1,625	17.1	52.6			
Grand Rapids, MI	1960–1963	16.1	68	54,918	34	3,400	23.0	43.4			
Green Bay, WI	1964	19.6	143	16,852	26	858	13.8	43.2			
Greensboro, NC	1957–1961	32.3	155	32,727	39	1,014	18.0	60.3	City	No	No
Indianapolis, IN	1970	300.0	379	280,771	60	936	53.6	379.4			
Jackson, MS	1959	19.0	115	31,791	29	1,677	16.1	106.2			
Jacksonville, FL	1968	716.8	2,374	360,861	196	503	30.2	759.7	Consol.	Yes	$5,000
Kalamazoo, MI	1955–1959	14.6	163	27,303	47	1,865	8.5	24.6	Area	Yes	No
Knoxville, TN	1962	51.0	201	67,000	60	1,314	25.4	77.1			No
Lexington, KY	1974	259.0	1,151	66,000	58	255	5.4	284.7			No
Lincoln, NE	1962–1966	22.6	91	15,655	12	693	24.3	60.0			
Macon, GA	1961	35.0	373	69,326	99	1,981	8.0	49.6			
Milwaukee, WI	1952–1956	46.3	91	113,326	18	2,447	43.4	95.8			
Mobile, AL	1956–1960	90.6	774	57,445	40	634	11.7	123.0			

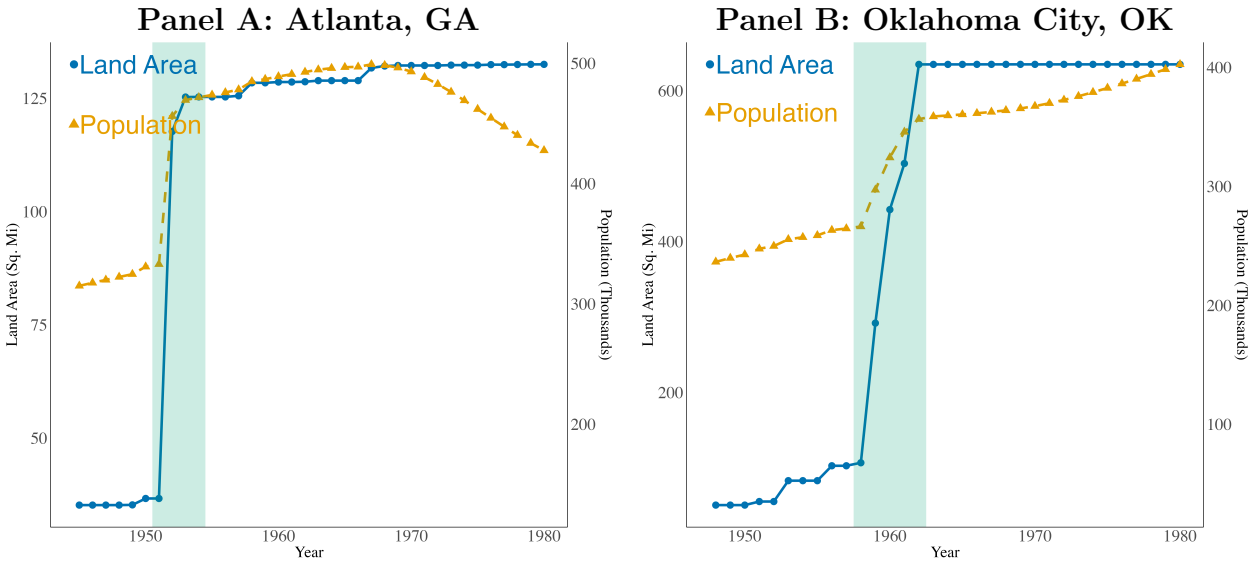
Continued on next page

Table C.1: Annexing Cities in the Event Study (Continued)

		Land added		Population added			Land area		Terms of annexation		
City	Spurt	Sq mi	%	Persons	%	Density	1940	1980	Method	Consent	Homestead
Nashville, TN	1961–1963	475.0	2,125	291,297	170	613	22.0	507.8	Consol.	Yes	No
Newport News, VA	1958	70.0	1,667	66,894	150	956	3.7	65.3			No
Norfolk, VA	1954–1958	24.7	88	98,038	45	3,976	28.2	53.0	City	No	No
Oklahoma City, OK	1959–1962	532.0	498	88,939	33	167	49.8	603.6	City	No	$1,000
Omaha, NE	1953–1957	9.5	24	27,449	11	2,882	38.9	90.9			
Peoria, IL	1964–1967	21.4	150	33,650	34	1,573	12.4	41.0			
Phoenix, AZ	1958–1962	185.7	518	286,910	163	1,545	9.7	324.0			
Portsmouth, VA	1959	10.0	161	36,091	44	3,609	6.2	29.9			
Richmond, VA	1970	22.6	135	47,262	23	2,090	21.4	60.1			No
Riverside, CA	1961–1965	28.4	65	33,792	41	1,189	39.1	71.8			
Roanoke, VA	1976	15.4	57	18,137	21	1,174	10.7	43.1			
Sacramento, CA	1958–1962	64.8	267	79,743	50	1,231	13.7	96.1			No
San Antonio, TX	1952–1956	88.2	120	112,651	27	1,277	35.7	262.7			
Savannah, GA	1960	26.1	235	40,076	33	1,534	11.1	56.8			$2,000
Seattle, WA	1953–1954	15.3	22	69,958	15	4,572	68.5	83.6	Area	Yes	No
Springfield, IL	1970–1974	10.6	75	8,133	10	766	9.5	39.8			
Springfield, MO	1966–1969	24.2	63	16,869	17	696	13.6	64.9			
Tampa, FL	1953	46.2	240	98,919	75	2,139	19.0	84.4	Legis.	No	$5,000
Toledo, OH	1959–1963	39.8	103	99,980	33	2,515	37.1	84.2			No
Tucson, AZ	1956–1960	57.6	404	143,751	191	2,498	8.0	98.8			
Tulsa, OK	1966–1967	118.2	281	87,654	35	742	21.4	185.6			$1,000
Wichita Falls, TX	1959–1961	22.0	134	38,844	57	1,763	10.8	49.2			

NOTES: This table lists the treated cities of the event study, one row per city. Spurt is the first and last year of the annexation window. Land and population added are the window's totals. Percentages are relative to the city's land area and population in the year before the window opens. Density is population added per square mile of land added. Land area is in square miles. † marks a 1990 area where 1980 is not measured. Method: Legis. = state legislature; City = initiated by the city; Area = initiated by the annexed area; Consol. = city-county consolidation. Consent records whether the annexed area consented, and Homestead the homestead exemption for annexed residents. A blank cell is not recorded.

Figure C.1: Two Annexation Spurts: Atlanta and Oklahoma City



NOTES: This figure plots each city's annual land area (solid line, left axis) and population (dashed line, right axis). Shaded years are the city's annexation spurt: 1952–1954 for Atlanta and 1959–1963 for Oklahoma City.

C. Identifying Eligible Comparison Cities

Comparison cities are cities that have not yet annexed substantially. A city serves as a comparison city until the year before its first single year of annexation that adds 5 or more square miles or 10,000 or more residents, and it does not return to the comparison group afterward. This includes the cities that never annexed more than 10 square miles between 1940 and 1980, annexing cities without a dated spurt, and treated cities before their own spurt. Cities whose land records are too incomplete to rule out unrecorded annexation are excluded. Appendix E. reports estimates under a stricter rule based on cumulative annexation.

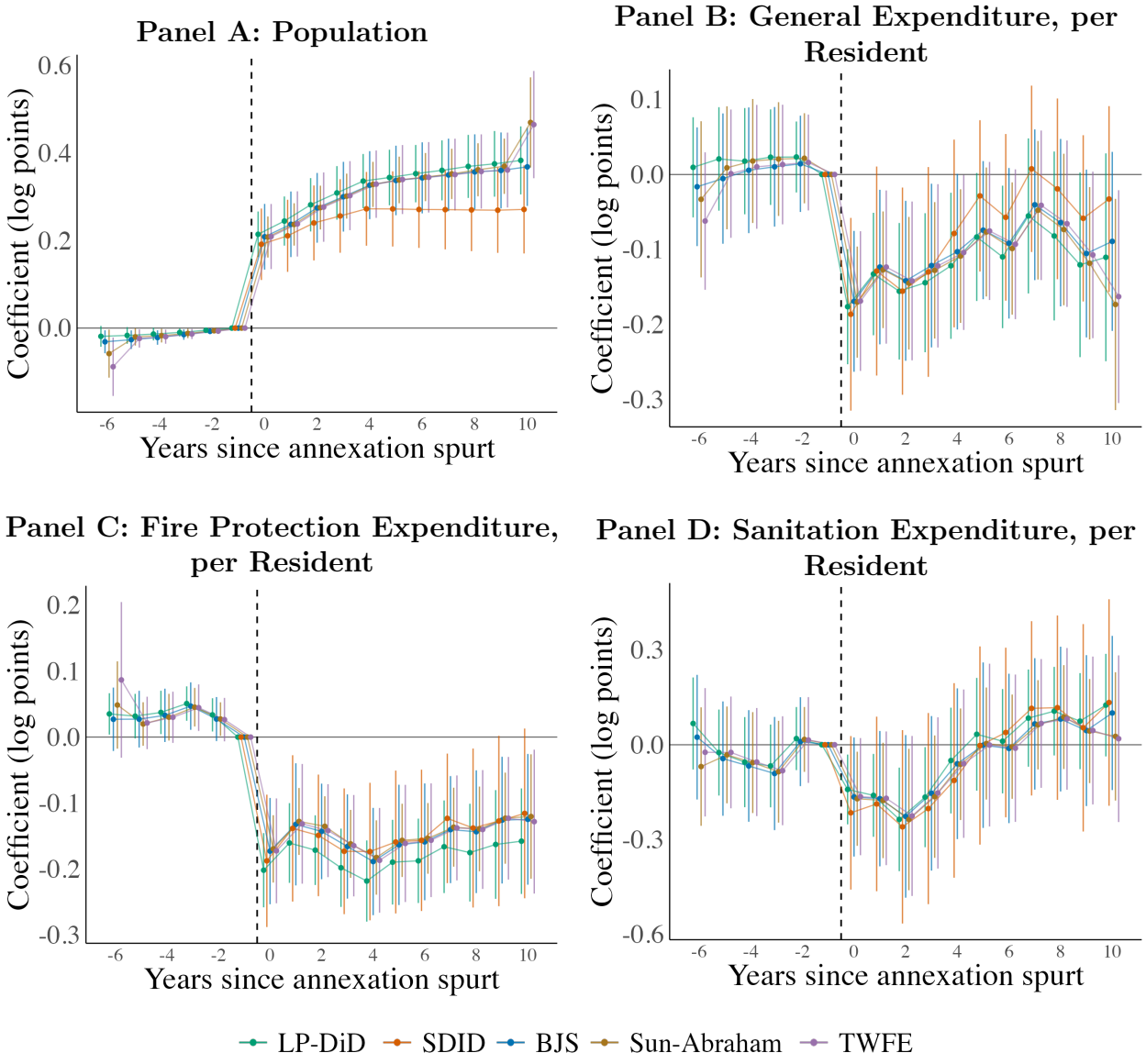
We estimate the event study with the local projections difference-in-differences (LP-DiD) estimator of Dube et al. (2023). For each event year, a treated city is compared with comparison cities in the same census region that remain eligible over the full window. Post-spurt outcomes are measured relative to the city's mean over the six years before the spurt, observations are weighted by population in the year before the spurt, and standard errors are clustered by city. The pooled effect averages the event-year coefficients from year 5 to 10.

D. Robustness to Alternative Estimators

Figure C.2 reports the headline outcomes under four alternatives to LP-DiD: the imputation estimator of Borusyak, Jaravel, and Spiess (2024), the interaction-weighted estimator of Sun and Abraham (2021), synthetic difference-in-differences (Arkhangelsky et al. 2021), and two-way fixed effects. The first three are built for staggered timing with effects that differ across cohorts; two-way fixed effects is not, and partly compares treated cities with cities already treated (Goodman-Bacon 2021).

The imputation, interaction-weighted, and two-way fixed effects estimators give pooled effects close to LP-DiD on all four outcomes. Synthetic difference-in-differences gives somewhat smaller effects on population and on general expenditure per resident. Under every estimator, annexation lowers fire protection spending per resident and leaves sanitation spending per resident roughly unchanged.

Figure C.2: Annexation and Municipal Finance across Estimators



NOTES: This figure compares event-study estimates for log outcomes across five estimators: LP-DiD (Equation 2), synthetic difference-in-differences (SDID), the imputation estimator (BJS), the interaction-weighted estimator of Sun and Abraham, and two-way fixed effects (TWFE). Coefficients are relative to event year -1, and bars are 95 percent confidence intervals. SDID standard errors are from a block bootstrap; the others are clustered by city.

E. Robustness to Alternative Treatment and Control Definitions

Table C.2 and Figure C.3 re-estimate log population and log general expenditure per resident under three alternative definitions, each changing one part of the design. *Strict controls* remove a city from the comparison group once its cumulative annexation, not just a single year's, becomes substantial. The *conservative treated set* requires spurts to be more sharply concentrated, and the cities that no longer qualify become comparison cities. *Not-yet-treated controls* compares treated cities only with cities whose own spurt comes later.

Strict controls and the conservative treated set give effects close to the main specification. Restricting the comparison group to not-yet-treated cities gives a smaller population effect and a larger decline in spending per resident, but treated cities in this variant already spent somewhat more per resident before the spurt, so we read it with caution.

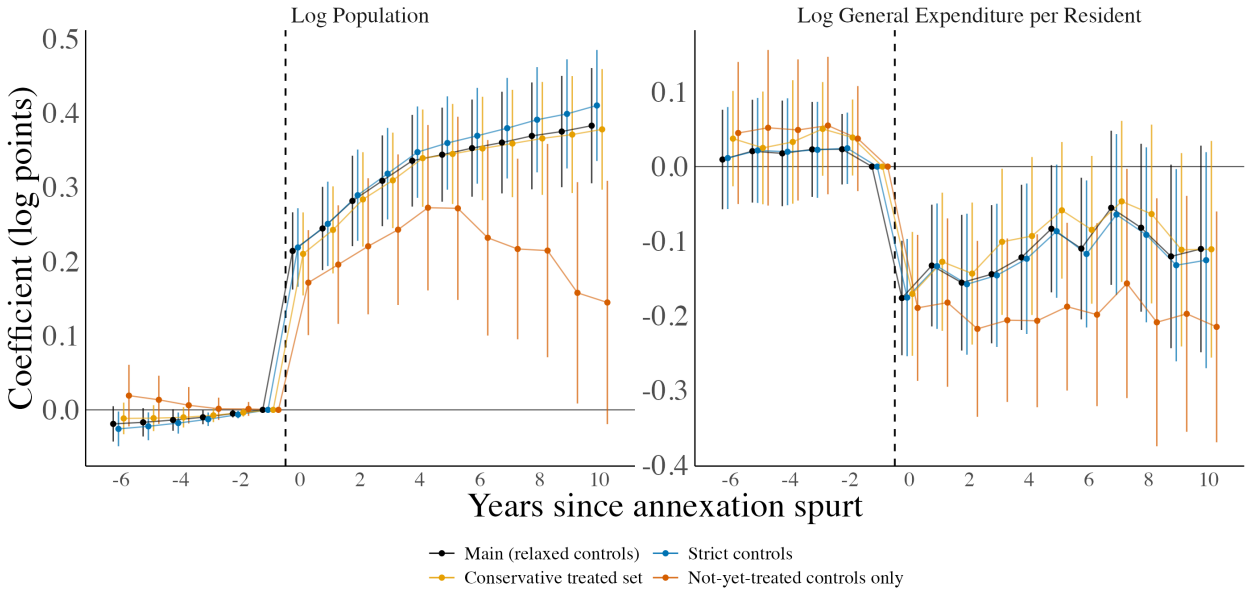
Table C.2: Annexation Effects under Alternative Treatment and Control Definitions

	Log Population			Log General Expenditure per Resident		
	Pooled ATT (1)	Treated events (2)	Comparison cities (3)	Pooled ATT (4)	Treated events (5)	Comparison cities (6)
Main (relaxed controls)	0.364***	49	111	-0.094*	49	104
	(0.036)			(0.054)		
Strict controls	0.385***	49	105	-0.103*	49	99
	(0.035)			(0.057)		
Conservative treated set	0.362***	39	111	-0.079	39	103
	(0.038)			(0.057)		
Not-yet-treated controls only	0.206***	44	36	-0.194***	44	36
	(0.067)			(0.069)		

NOTES: This table reports LP-DiD pooled effects over event years 5–10 under alternative definitions of treatment and comparison cities, described in Appendix E.. Standard errors, clustered by city, are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure C.4 moves each threshold of the spurt rule in Appendix B. one at a time, in both directions, holding the others at their main values. The variants change the length of the spurt window, including a seven-year window, the length of the lookback, the two concentration bars, the share of all of the city's annexation the spurt must account for, the size floors, the number of residents a land spurt must add, the tolerance in the coverage guard, and the first cohort; one further variant dates Toledo by the rule rather than by hand. Across the 19 variants, the

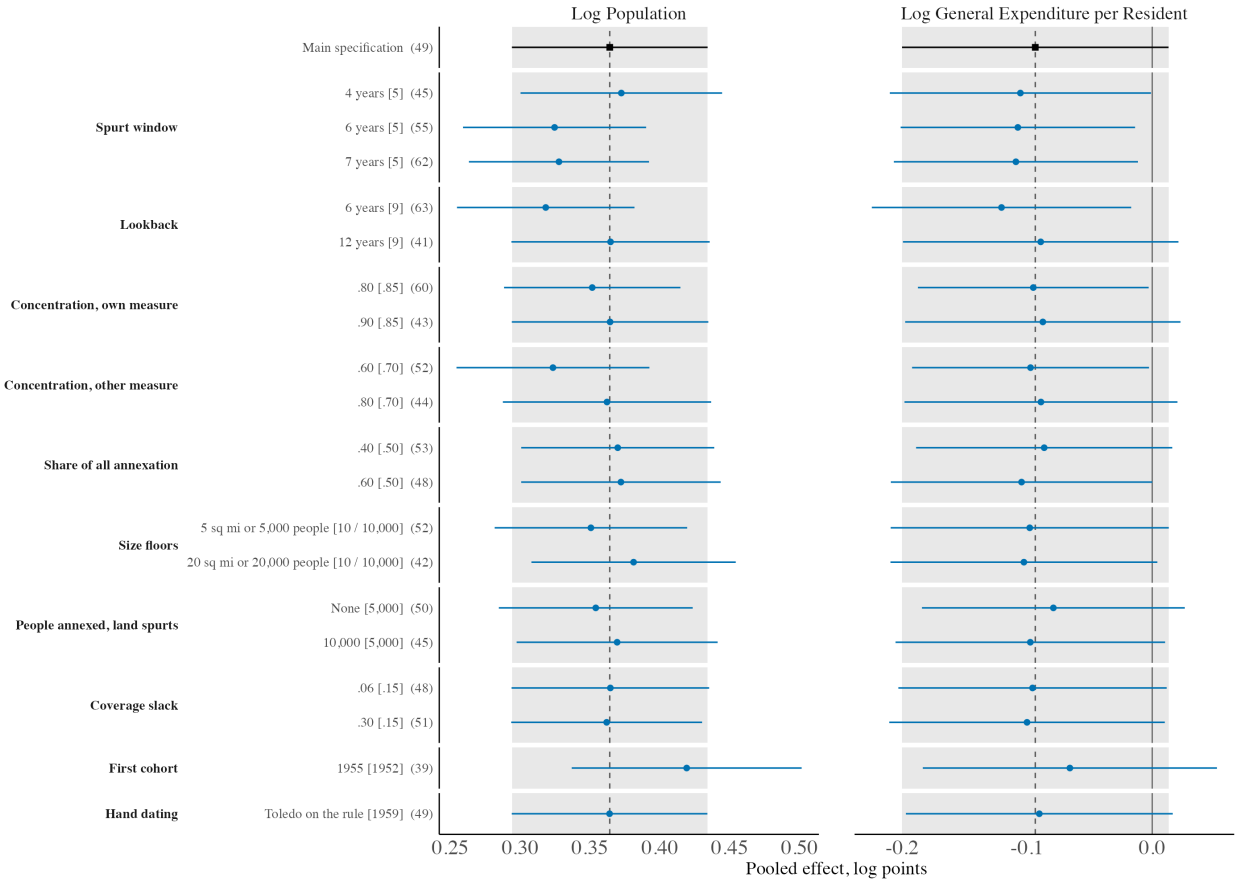
Figure C.3: Event Studies under Alternative Treatment and Control Definitions



NOTES: This figure reports LP-DiD event-study estimates for log population (left) and log general expenditure per resident (right) under the definitions of Table C.2. Post-spurt coefficients are relative to the mean of the six years before the spurt, and pre-spurt coefficients to event year -1. Bars are 95 percent confidence intervals, and standard errors are clustered by city.

number of treated cities ranges from 39 to 63. The population effect remains positive and large in every variant, and the effect on general expenditure per resident remains negative, though it is less precisely estimated in some.

Figure C.4: Annexation Effects under Alternative Spurt-Rule Thresholds



NOTES: This figure reports LP-DiD pooled effects over event years 5–10 for log population (left) and log general expenditure per resident (right). Each row moves one threshold of the spurt rule described in Appendix B.; the row label gives the moved value, with the main value in brackets and the number of treated cities in parentheses. The dashed line and shaded band are the main estimate and its 95 percent confidence interval. Bars are 95 percent confidence intervals, and standard errors are clustered by city.

D Instrument Appendix

Section A. defines share-open as the share of the land within 4 miles of a city's 1950 boundary that was open to annexation, and looseness as an index of how permissive the state's annexation law was. Share-open requires two inputs the historical record does not supply directly: each city's own 1950 boundary, and the boundaries of the other municipalities that already existed in 1950 and could refuse to be annexed. This appendix describes how we construct both instruments, the geographic variation they carry, and the first stage at radii other than 4 miles.

A. Instrument Construction

A.a. The 1950 City Boundary

We use the 1950 city boundaries described in Appendix C.c.. For cities without a traced map, an annexation record, or usable census tracts, we approximate the 1950 boundary from the 1950 full-count census: we take the outline of the households the census assigns to the city and grow it, starting from the city's 1940 households, until its area matches the published 1950 land area. Where both exist, this approximation closely matches the boundary drawn from historical sources. Because share-open measures the open land *around* the city, it needs only a roughly correct outline, so these approximations are used to construct the instrument but never to measure outcomes inside the 1950 boundary.

A.b. The 1950 Fence

The fence is the set of municipalities that existed in 1950 and whose land the city therefore could not annex. We build it from the 1980 census place file in three steps. First, we drop places that incorporated after 1950, using the incorporation dates of Goodman (2021), since a suburb that incorporated later was not a barrier in 1950 and may have incorporated in response to the city's annexations; we also drop census-designated places, which have no government and cannot refuse annexation. Second, in the nine states where towns and townships are incorporated governments that cover all land and can refuse annexation (Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, and Vermont), we use county subdivisions in

place of census places. Third, because a 1980 place outline includes the place's own annexations after 1950, we shrink each place back to its pre-1950 land area, taken from the 1944 *Municipal Year Book* or the 1940 census, keeping the land closest to the place's 1940 households where we observe them.

A.c. Share-Open and the Baseline Controls

We compute share-open on a 250-meter grid. Land counts as within 4 miles of the city if it can be reached within that distance from the 1950 boundary without crossing ocean, bays, or the county line. The denominator of Equation 7 is this land, and the numerator is the part of it the city can still reach when the 1950 fence also blocks movement. Land that a neighboring municipality cuts off from the city is therefore closed even if it is unincorporated.

The baseline controls are the logs of the city's 1950 land area and population, the log of its county's land area, the growth of county population between 1930 and 1940, and three shares of the land within 8 miles of the city: water, other municipalities, and other counties. The 8-mile controls measure how much land surrounds the city, while share-open measures how much of the land the city could reach was already incorporated. The combined instrument standardizes share-open and looseness across the cities of the first-stage sample.

A.d. The Looseness Index

We code each state's annexation law from the state-by-state descriptions in Dixon and Kerstetter (1959), scoring each provision between 0 (most restrictive) and 1 (most permissive). Where a state allowed annexation both under general law and by a special act of the legislature, we score the more permissive route. The looseness index is the mean of three scores. Consent scores whether the city can annex without the annexed area's approval: 1 where the city council can act alone, intermediate values where a court, board, or the legislature decides or the city can annex only uninhabited land, and 0 where the area must consent. Defensive incorporation scores whether the fringe can escape annexation by incorporating: 1 where incorporation near cities is restricted, 0.5 where the law is silent, and 0 where incorporation is easy. Debt scores whether the annexed area must help pay the city's existing debt: 1 where it is shielded, 0.5 where the law permits it or is

silent, and 0 where it must. We also code the strength of the annexed area's veto, whether other governments can block an annexation, whether owners of uninhabited land can petition to join the city, waiting periods after a failed attempt, and tax concessions to annexed areas; Appendix Figure A.19 maps the first three.

B. Variation and Geography

Figure D.1 maps share-open for the 164 cities that have one. Share-open is undefined for consolidated city-counties, since the city's own land fills its county, and we also exclude St. Louis, New Orleans, and Philadelphia, which were already coterminous with their counties in 1950. Share-open is high and skewed: the median city in the estimation sample is 0.94, and only 8 of the 134 cities were majority fenced in. The most fenced-in cities are concentrated in the Northeast, where towns and townships cover all land.

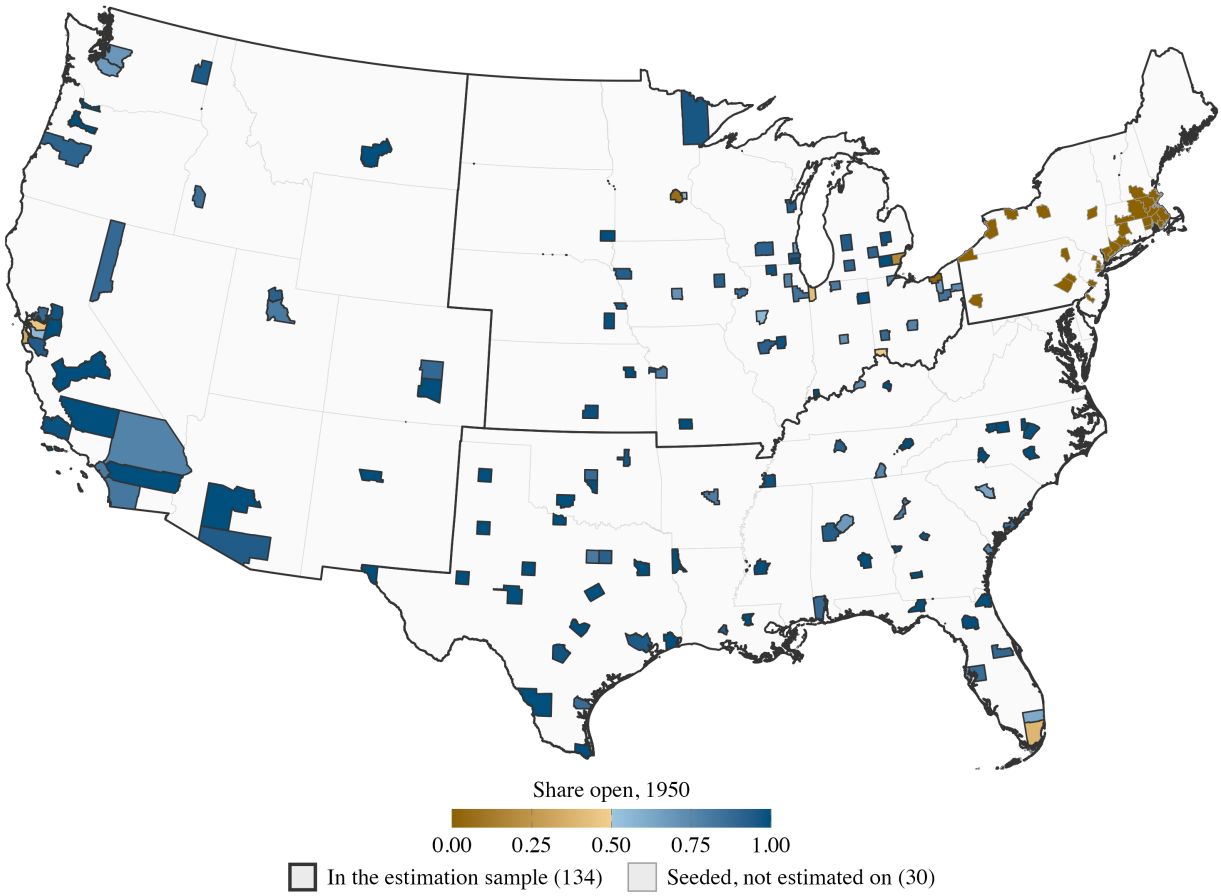
C. Alternative Catchment Radii

Table D.1 reports the first stage and the balance of share-open at radii of 1, 2, 3, 4, and 6 miles, on the same cities and with the same controls as Table 4. The first-stage coefficient is similar across radii, between +1.561 and +1.706, and the first stage strengthens as the radius widens, from an F of 10.9 at 1 mile to 26.3 at 6 miles. At every radius, share-open is jointly unrelated to the 1950 city characteristics of Table A.4, conditional on the baseline controls.

D. Additional Instrument Exhibits

The exhibits in Appendix A report the first stage as a binned scatter, the balance of the instruments against 1950 characteristics, the ordinary least squares counterpart of the main table, and the full breakdown of expenditure and revenue under both estimators.

Figure D.1: Share Open in 1950



NOTES: This map shows share-open (Equation 7) for the 1950 county of each sample city: blue where more than half the land within 4 miles of the city was open to annexation and orange where less was. Heavy outlines mark the 134 cities in the city-level two-stage least squares estimates.

Table D.1: First Stage and Balance at Alternative Catchment Radii

	Share-open		First stage		Joint balance
Catchment radius	Mean	S.D.	Coefficient	F	p-value
1 mile	0.891	0.176	$+1.561^{***}$	10.9	0.085
			(0.473)		
2 miles	0.884	0.181	$+1.686^{***}$	13.4	0.235
			(0.461)		
3 miles	0.878	0.185	$+1.706^{***}$	15.4	0.412
			(0.434)		
4 miles (published)	0.872	0.189	$+1.669^{***}$	19.6	0.484
			(0.377)		
6 miles	0.862	0.195	$+1.588^{***}$	26.3	0.594
			(0.310)		

NOTES: This table reports the first stage of Equation 9 with share-open measured at alternative distances from the city's 1950 boundary, on the 134 cities with share-open defined at every radius. F is the squared t-statistic. The balance column is the p-value of a joint test that the 1950 city characteristics of Table A.4, Panel A, are unrelated to share-open. Standard errors are in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

E Sorting Around the 1950 City Boundary

Annexation may have shifted the density and mixture of people across different parts of the economic area. For instance, if municipal services are being provided more efficiently, this may attract residents or firms to move into the larger city. However, if cities annex suburbanites involuntarily, they may choose to leave the city, or annexation may systematically shape which type of new residents choose to locate in the city.

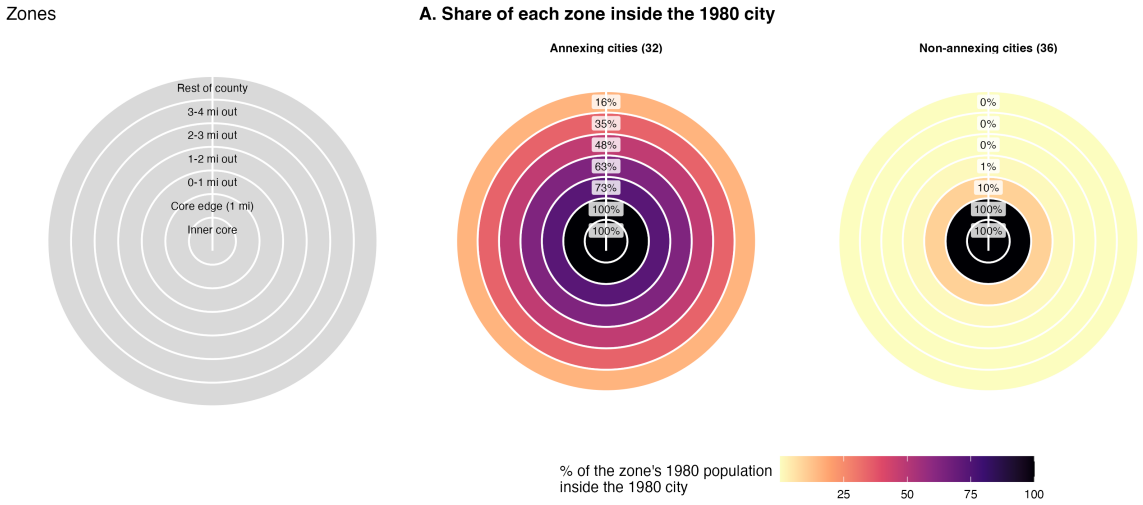
To identify how aggregate annexation shifted population and housing, we divide each city's 1950 county into concentric zones defined by distance from the 1950 boundary: the 1950 city more than a mile inside its boundary, the mile-wide band inside the boundary, four one-mile rings outside it, and the rest of the county. The zones are drawn for each city, regardless of its ultimate annexation status, and we allocate census tract data for 1960, 1970, and 1980 to them in proportion to area for the set of counties that were completely tracted by 1960. Figure E.1 shows how much of each zone the city had absorbed by 1980. By 1980, in annexing cities, 73% of the population living in the first mile outside the 1950 border lived in the central city, against 10% in non-annexing cities. Even four miles out from the 1950 border, 35% of residents in counties with annexing central cities lived in the central city.

To compare the zones of annexing and non-annexing cities, we estimate

$$y_{czt} = \alpha_{cz} + \lambda_{zt} + \mu_{r(c)t} + \sum_k \mathbf{1}[z = k]\,\mathbf{1}[t = 1980]\left(\beta_k\, A_c + \mathbf{X}_c'\boldsymbol{\delta}_k\right) + \varepsilon_{czt}, \tag{E.1}$$

where y_{czt} is the outcome in zone z around city c in census year $t \in \{1960, 1980\}$, A_c indicates that city c is one of the annexing cities of Section V., and α_{cz}, λ_{zt}, and $\mu_{r(c)t}$ are city-by-zone, zone-by-year, and census region-by-year fixed effects. $\mathbf{X}_c$ holds the city's log 1950 land area and population, its county's log land area and 1950 population, and the share of land in its metropolitan area unavailable for development (Saiz 2010); each enters with its own coefficient by zone, so that each zone's change may depend on the size of the city and county at the start of the annexation wave and on its geographic constraints, in the same way for annexing and non-annexing cities. Each β_k is the difference between annexing and non-annexing cities in the change in zone k from 1960 to

Figure E.1: Annexation Around the 1950 City Boundary



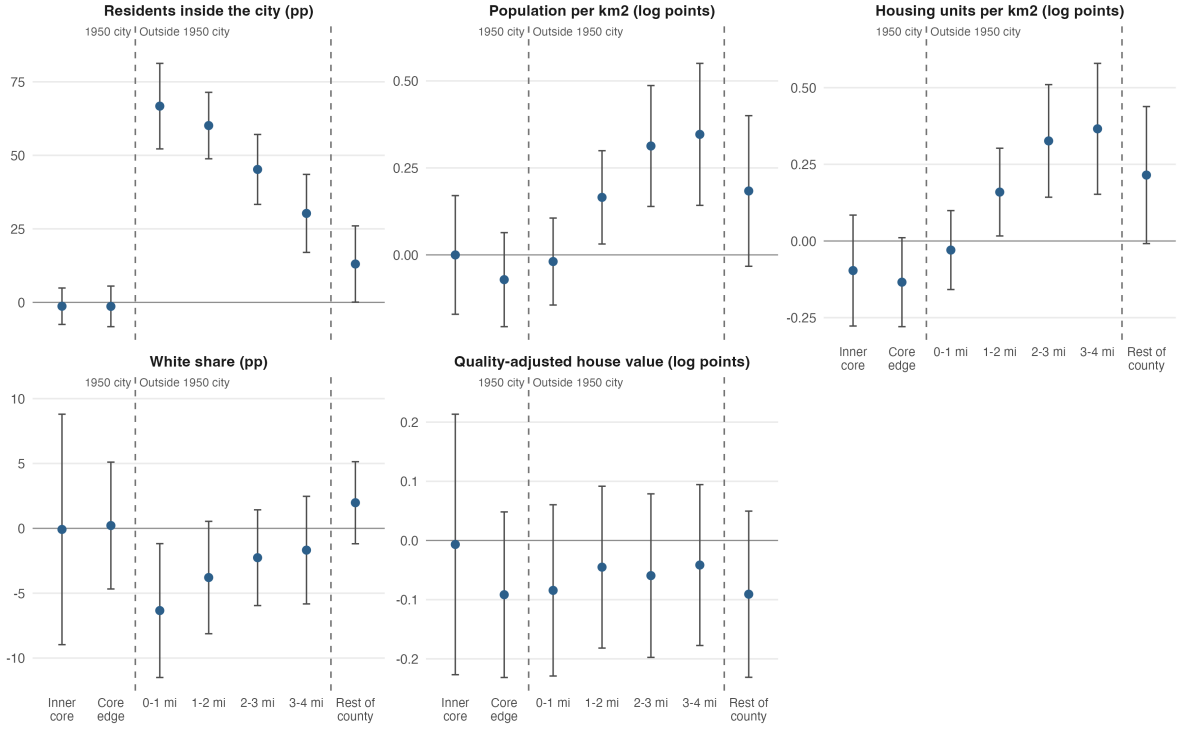
NOTES: This figure divides each city's 1950 county into zones by distance from its 1950 boundary, with census tracts allocated by area. It reports the share of each zone's 1980 population living inside the 1980 city, for annexing and non-annexing cities. Annexing cities are the treated cities of Section V..

1980, net of the change in the city's census region and of these baseline characteristics. The city-by-zone fixed effects absorb any level difference between the zones of annexing and non-annexing cities, so only changes identify β_k. Population and housing density and the quality-adjusted house value enter in logs, and the white share in percentage points; standard errors are clustered by city. The sample holds the 66 cities matched to the land-availability measure, 31 of which annexed, and Figure E.2 reports the β_k.

Three patterns stand out in how population shifted around annexation. First, the rings that annexing cities absorbed grew denser. Population and housing density in the rings two to four miles outside the 1950 boundary grew by up to 0.35 log points more than around non-annexing cities, while the density of the 1950 city did not change detectably, suggesting that annexation added population to the fringe it absorbed rather than drawing residents out of the old city. Second, this densification came with racial change: the first two miles of the annexed fringe became less white than the fringe of non-annexing cities, while the racial composition of the 1950 city barely changed. One caveat is that much of the fringe of non-annexing cities lay in other municipalities, which may have used their own zoning and services to shape who moved in, so the comparison may partly reflect the fragmentation of the comparison fringe rather than annexation alone. Third,

holding the observable characteristics of the housing stock fixed, house values in the annexed fringe grew somewhat more slowly than around non-annexing cities, but these differences are small and imprecisely estimated once we control for the baseline size and geography of the city.

Figure E.2: Annexation and Change Around the 1950 City Boundary, 1960–1980



NOTES: This figure reports the coefficients β_k of Equation E.1, the change from 1960 to 1980 in each zone of annexing cities relative to non-annexing cities, with 95 percent confidence intervals. The first panel instead reports the difference in the share of each zone's 1980 residents living inside the 1980 city, from one 1980 cross-section with zone and region fixed effects and the controls of Equation E.1 by zone. Quality-adjusted house value is the median value at fixed housing characteristics, adjusted with one hedonic regression for 1960–1980 on rooms per unit, the share of one-unit structures, persons per household, the age of the housing stock, and plumbing. Standard errors are clustered by city.