Taming the Growth Machine: The Long-Run Consequences of Federal Urban Planning Assistance *

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Abstract

We study how the federal Urban Planning Assistance Program, which subsidized communities in the 1960s to hire urban planners to draft land-use plans, affected housing supply. We digitize program records and link to a municipality level panel on housing and zoning outcomes. Eligibility thresholds, paired with state agencies' capacity to approve funds, identify effects. Planning assistance caused municipalities to build 20% fewer housing units per decade over the 50 years that followed. Regulatory innovation steered development in assisted areas away from apartments. Instead, new units concentrated in larger single-family homes. Text data related to regulation and development politics offer details behind the innovation process. With them, we show 701 recipients since the 1980s placed greater burden on developers to provide local amenities. These findings suggest how providing information under decentralized governance could increase market frictions, with long-run consequences for national affordability.

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1 Introduction

Over the past several decades, housing production in the United States has declined nationally, with the sharpest declines in certain highly demanded regions[1]. The result is a nationwide decline in housing affordability in the 21st century, particularly for renters and first-time homebuyers in America's major cities (Baum-Snow and Duranton, 2025). Although housing supply reform has become a policy priority, many adopted reforms amount to partial changes to a thicket of regulations authorizing local governments to restrict development (Schleicher, 2017; Bronin, 2023).

For most of American history until the 1960s, there was little to suggest a future housing supply slowdown. U.S. land-use regulations were not known for their complexity; judicial precedent limited them to not extend "beyond the basic objectives of . . . establishing minimum standards of development" (Delafons, 1962). We know surprisingly little about the transformation since then: why some, but not all, of the United States shifted from a permissive approach to building housing to a regulated environment producing limited supply. This paper evaluates one underexplored cause: a 1960s federal program that promoted urban planning in America's rapidly growing suburbs.

We estimate the causal effects of the Department of Housing and Urban Development's (HUD) Urban Planning Assistance program. Under Section 701 of the federal Housing Act, federal grants subsidized the fixed costs of producing comprehensive plans— documents that survey local conditions, project population growth, and guide desired land use—along with the zoning and subdivision regulations to enforce them. Our focus is on the Small Areas Program within "701 assistance", which funded planning specifically for municipalities with fewer than 50,000 residents. In the short run, substantial takeup of 701 assistance showed the program successfully matched local governments eager to learn planning techniques with trained urban planners willing to supply their services. However, because federal authorities did not designate which objectives planning assistance ought to achieve, it is an empirical question whether planning was used to expand or limit the capacity for future growth.

To study the program, we collect and digitize project directories and archival records documenting which municipalities received 701 assistance and the planning products produced through the process. We link these data with a national panel from 1940 to 2010 of munici-

[1] One illustrative example is the decline in California's permitting rate per 100,000 people. The rate was approximately 1,150 in the mid-1980s, more than 50% higher than national levels. Since the 21st century, permitting rates have been consistently 100-200 units per 100,000 people below the national level. Herkenhoff, Ohanian and Prescott (2018) offer a more comprehensive accounting exercise as part of their quantitative analysis on U.S. housing supply.

pal housing supply, composition, prices, and regulatory restrictiveness. We combine decennial Census tables with independent sources of planning regulations, including survey responses, administrative text, and text-as-data retrieved from local newspapers.

Municipalities already experiencing rapid growth and high housing demand may have sought planning assistance to address these challenges; we find evidence that highly-demanded municipalities were more likely to participate. We address the potential for selection bias with a research design that exploits two features of 701 assistance. First, municipalities with more than 50,000 residents by 1960 were ineligible for the Small Areas Program. Second, we exploit variation in program takeup across states. State agencies were responsible for interacting with HUD on behalf of their municipalities, and states differed widely in their capacity and interest in receiving federal funds[2].

Combining the features, we estimate a continuous triple differences design. We compare differences with eligible and ineligible municipalities across states with high and low program adoption rates. Our design can also be thought of as extending a regression discontinuity design (RDD) around a population eligibility threshold: we impose specific functional form assumptions to estimate treatment effects away from the marginally eligible municipality with 50,000 residents. We show our preferred specification passes various pre-trend and balance tests, and we discuss how it trades off the internal validity of an RDD estimate with the ability to infer effects for smaller municipalities most impacted by 701 assistance.

Our main finding is that 701 assistance caused significant, long-lasting reductions in housing supply. Compared to counterfactual trends estimated from municipalities in states receiving minimal 701 assistance, assisted municipalities built 18–20% fewer housing units per decade in the decades following treatment. Effects accelerate to 20–23% fewer units per decade after 1970. As thousands of suburbs received 701 assistance throughout the 1960s, the combination of effect persistence and scope yields a conservative back-of-the-envelope estimate that 1.5 million fewer housing units were provided as the implementation of 701 assistance.

Supply reductions were not uniform across housing types: 701-assisted municipalities shifted away from apartment construction in the 1960s, coupled with more development of single-family homes with larger lot sizes than prevailing market densities. By foreclosing the more affordable segments of the single-family market, owner-occupied units in the municipalities became less affordable and rose in median house values. However, these estimates are smaller in magnitude and less precisely estimated, which is consistent with a channel for demand readjustment. Households priced out of assisted communities can instead buy housing that is

[2]Section 4.2 explains in detail the differences in illustrative state agencies and the econometric details of our research design.

a close substitute supplied in unassisted communities, up to a point where new communities themselves are too costly to be supplied.

What explains the persistence of housing supply reductions due to 701 assistance up to the 21st Century? One explanation could be that zoning and building regulations passed through the 701 assistance process are difficult to change, introducing frictions in meeting local demand for elevated density and redevelopment. However, the best data we have that measure local variation in postwar regulations, which are primarily restrictions on housing density and on use, indicate the changes 701 assistance made to postwar regulatory restrictiveness cannot quantitatively account for the supply effects. Instead, we study how 701 assisted municipalities exhibited *regulatory innovation*: they were likelier to adopt more sophisticated regulations on growth implemented only after the 1970s, such as permit quotas and regulations that require developers of new developments to provide public amenities before approval.

A motivating fact comes from applying our 701 research design to zoning restrictiveness indices based on the underlying text, especially restrictiveness as measured by the co-occurrence of post-1970s value capture practices. We estimate that 701 adopters are 0.5 standard deviations higher in an index of value capture practices, reflecting procedures that pass through the costs of providing infrastructure or public amenities to development. We corroborate these patterns with specific regulations as an outcome, as well as practices recorded from ocal newspaper coverage of discretionary development projects. Using newly constructed data spanning two and a half decades, we find developers of discretionary projects in 701-assisted municipalities were 15 percentage points more likely to encounter fees or requirements to provide community benefits when navigating the permitting process.

Together, our results point to a historical case study where public efforts to reduce information costs in a sector sowed the seeds of more market frictions in the long run. "Section 701" planning assistance, apart from introducing specific legal practices, could have entrenched a political process where growth skeptical interest groups are more informed and more willing to hold up new development. The interaction of information provision with decentralized decision-making on land use creates *path dependence in regulations* between markets, complicating any efforts to reform planning processes from a top-down perspective.

Our results relate to three literatures. Multiple social science disciplines, including economics, have studied the political and institutional determinants of urban development. One line of thinking models restrictions on development as downstream of local incumbents maximizing their self-interest (Hilber and Robert-Nicoud, 2013; Fischel, 2017; Favilukis and Song, 2025). A related literature considers how path dependent urban growth is to historical insti-

tutional decisions (Lin and Rauch, 2022).[3] Our evidence offers a novel explanation for why economic incentives to restrict growth produce uneven regulatory burdens within an economy: adopting those burdens requires not just motivated interest groups, but also institutional knowledge on how to enact legally admissible planning regulations. U.S. local governments diverged on the latter aspect depending on their engagement in the Section 701 process, during a period when Americans began to assess the costs of suburban growth (Rome, 2001).

By highlighting the importance of planning processes and regulatory innovation behind the supply effects of 701 assistance, we study policy diffusion at the local government level for a class of regulations with real economic effects. We build on work documenting general trends in policy diffusion (Nicholson-Crotty, 2009; DellaVigna and Kim, 2025), as well as recent work showing persistent behavioral changes by public officials after exposure to persuasive ideas (Robertson, 2025; Ash, Chen and Naidu, 2026). The cycle of diffusion and innovation surrounding local planning practices appears to explain housing supply shortfalls in U.S. cities, which matters for affordability concerns but also for the productivity advantages of urban workers (Hsieh and Moretti, 2019; Duranton and Puga, 2019).[4]

Building on theories of housing regulation that emphasize motivated behavior of interest groups (Fischel, 2017), recent work detailing the "Abundance Agenda" generalized those theories to explain a broad range of post-1970s declines in productivity and policy efficiency (Klein and Thompson, 2025; Dunkelman, 2025). Our evidence on how 701 assistance generated path dependence toward increased development regulation highlights that today's burdensome regulations were contingent on engagement with past federal reforms. As 701 assistance scaled up in the 1960s, there was an active debate around whether it improved the planning capacity of treated places. Little attention was paid, to our knowledge, on the various ways planning processes could be used depending on local governments' beliefs regarding the costs of growth.[5] Our study suggests how recent regulatory reforms should not only change the letter of the law

[3]Examples of such decisions include the persistence of street plans in cities and their consequences for amenities and commercial activity (Brooks and Lutz, 2019; Baruah, Henderson and Peng, 2021; Michaels et al., 2021; Salazar Miranda, 2022), or how the earliest zoning restrictions affected neighborhood change and the distribution of commercial activity persistent effects on the neighborhoods they regulated (Shertzer, Twinam and Walsh, 2018; Twinam, 2018; Gallagher, Shertzer and Twinam, 2024).

[4]Our work also complements other longitudinal studies of regulations in specific industries, including Carollo et al. (2025) on the evolution of occupational licensing and Hanson, Rodrik and Sandhu (2025) on the implementation of place-based policies across U.S. government agencies. Those papers also study policies mostly passed at the state or federal executive levels, while our results offer new insights into what determines policymaking at the local level and the persistence of local policies.

[5]Both planning historians and federal agency evaluations attributed planning assistance to the growth of postwar urban planning (Scott, 1969; Kaiser and Godschalk, 1995). We were unable to find other work among urban planners, or planning historians, on how planning assistance shaped local opinions around planning and to which ends it should be used.

but also avoid unintended consequences by eliciting local actors' attitudes toward growth and designing incentives that align local interests with regional priorities.

The rest of the paper proceeds as follows. Section 2 provides background on the Urban Planning Assistance Program and our hypotheses on how planning assistance affects housing supply. Section 3 quantifies the scope and content of the Section 701 program, alongside data sources for our outcomes. Section 4 explains our research design, which exploits plausibly exogenous variation in 701 assistance eligibility, and Section 5 presents our main results. Section 6 uses a variety of housing regulation restrictivenss measures to explore regulatory innovation after 701 assistance. Section 7 concludes.

2 Historical Background and Framework

2.1 The Objectives of Suburban Planning

Following World War II, real incomes for American households rose sharply and suburban development expanded rapidly. The share of Americans living in suburban areas grew from 13% of the population in 1940 to 37% by 1970 — a rate of 1.9 million new suburbanites annually (Nicolaides and Wiese, 2017). Harmonizing Census Bureau permit data collected after 1958 with federal analysts' 1945–1958 estimates (Siskind, 1979), we estimate that annual housing starts in this period were 1.37 million.

Population mobility of this magnitude would begin to form its own problems, separate from concerns related to slum clearance in central cities that were prioritized by the federal urban renewal program. By the 1950s, three common concerns would emerge among suburban government actors. First, a lack of information about future population growth complicated the provision of local public goods: most notably, the preponderance of young families moving into the suburbs made the supply and siting of public schools difficult. Second, there was uncertainty about the capital expenditures needed to service suburban development; namely, which roads, sewage systems, and other infrastructure must be built and at what rates. While underinvestment in capital services could harm future neighborhood desirability, overinvestment would also burden homeowners with higher taxes to maintain physical capital. Scott (1969) details at length discussions around these two concerns.

Finally, a consensus emerged among planners and developers on the negative externalities of neighborhood "blight" in aging suburbs. Neighborhoods with poor-quality housing and infrastructure posed public health risks and were susceptible to rapid neighborhood change. Both types of risk could also spill over into nearby areas, as was perceived to have occurred in

neighborhoods in the urban core (von Hoffman, 2000).

While certain land-use controls, such as residential zoning, were ruled constitutional and widely adopted before the 1940s (Hirt, 2014), more sophisticated regulations would benefit suburban governments balancing these objectives. Planning the spatial allocation of housing density, which implicitly targets a population growth rate, could be achieved through a comprehensive planning process focused on the physical planning of the built environment. Additional infrastructure standards that complement development, such as subdivision codes that regulate street design, could have long-run benefits while slowing new housing construction.

2.2 701 Assistance Projects: Procedure and Timeline

The first version of 701 assistance was included in the Housing Act of 1954, which established a pilot program to match funds for local agencies interested in urban planning. Following engagement by the Eisenhower administration with urban planning academics, Congress took up the planning profession's recommendations to "make grants on a matching basis to state planning agencies for the provision of technical assistance." (Feiss, 1985)[6]

In 1959, Congress amended the legislation to fund the version of 701 assistance used by most municipalities that adopted it. A "small area," defined as a municipality with under 50,000 population as of the latest Census, could apply for a federal matching grant for the cost of producing a comprehensive plan. The matching share started out at 50%, but from 1961 onwards varied between 2/3 and 3/4 of planning expenses, depending on the project scope. Therefore, municipalities taking up 701 assistance paid limited expenses that averaged around \$6,500 to \$10,000 in 1970 dollars (\$62,000 to \$96,000 in 2024 dollars).[7]

Though the initiative to apply for 701 assistance starts from local officials, the application process involves minimal communication between them and federal agencies. Instead, the usual prerequisite for 701 assistance is a three-party contract between local officials, a contracted planning consultant, and a state agency designated to handle grant distribution. In reviewing documents describing 11 states' management of 701 assistance, we found that the vast majority of them lacked adequate staffing to handle planning services in-house. Faced with rapid demand for assistance in the 1960s, agencies prioritized matching localities with

[6]Lacking legislative language that specified acceptable forms of " technical assistance," the pilot version of 701 found early adopting municipalities and also cross-subsidized the expansion of urban planning programs in American universities (Scott, 1969)

[7]We conducted the calculation using our data on small area planning grants, which we elaborate on in Section 3.1. We divide the total amount of grants disbursed to small areas in a year by the new approved municipalities that year. Appendix Figure B.2 plots time series for the two variables side by side, also exhibiting the rise in applications following 1959.

available licensed local consultants or with national firms.

Contemporary case studies describe a state agency spending up to a year to finalize a 701 assistance proposal to HUD. Once approved, planning consultants usually took 1-2 years to compile planning outputs. One set of products involves the plan itself. The planner is paid to conduct a set of surveys and compile demographic data. Afterwards, he engaged in consultation and public comment with local residents to produce population projections and a land-use map that coordinates the location and characteristics of new development. To implement the physical planning recommended in the plan, the 701 consultant often rewrote the municipality's zoning ordinance, as well as produced new documents like subdivision ordinances and building codes (Hammer Greene Siler Associates, 1969).

As demand for 701 assistance outpaced the supply of consultants, the final product deviated from an idealized planning process. Hammer Greene Siler Associates (1969) discusses the lack of an incentive dissuading "boilerplate" plans that give little thought to a municipality's unique demographics or built form. Once the plan is in place, minimal funding is provided to check back in with local officials if their planners are unable to monitor plan implementation. In addition, citizen participation in the public comment process often descended into engaging with "a loose, albeit parochial, establishment of business leaders more concerned with commercial than human problems."

2.3 Potential Effects

Although 701 assistance eligibility and funding availability were revised multiple times throughout the 1960s, we interpret the planning product to have two fundamental roles. First, the deliberation behind the comprehensive plan and the creation of the land-use map produced information on the costs and benefits of future growth trajectories. Plans produced under 701 assistance innovated in offering population projections over the following decades, along with surveys and qualitative data on the costs of unplanned growth. The economic impact of the acquired planning information would then depend on how that information changed local residents' incentives toward supporting or opposing growth.

Following the existing literature, we can model land-use regulation as an endogenous bargain between local interest groups with competing objectives. In urban economics, prior theoretical work rationalizes restrictive development regulations as resulting from local governments prioritizing revenue extraction by local residents (Hilber and Robert-Nicoud, 2013) or from residents and developers strategizing to increase municipality desirability (Helsley and Strange, 1995). To our knowledge, existing models on the political economy of growth man-

agement implicitly assume every interest group has perfect information about the payoffs to growth. Little has been shown about how restrictions on development may or may not be planned out when some players strategize off of incomplete information.

How would a subsidy on planning information change how local governments regulate housing supply? The broader literature on bargaining games with incomplete information and costly information acquisition suggests theoretical predictions vary greatly depending on the information structure. More "Coasean bargaining" could occur: if planning information informed what forms of development generates more revenues than costs, local residents could be more willing to approve a greater variety of development than before.[8] Alternatively, planning assistance that revealed only the costs of growth could cause local residents to permit fewer developments, or limit the types of development that are higher density and more affordable for future residents. [9] It is then an empirical question whether housing supply increased or decreased after 701 planning assistance.

Second, the 701 assistance process introduced local governments to new regulatory techniques. A local interest group, which may already have been opposed to growth, could have learned about how it can obstruct growth through legal means. Qualitative research in urban history offers case studies of residents of growing suburban communities who disapproved of apartment building proposals but were able to codify their preferences in comprehensive plans produced in the 1970s. [10] In a more recent context, Manville and Osman (2017) notes that in California municipalities facing opposition to development, activists frame the municipality's comprehensive plan as an output of public comment, in contrast to "corrupt" bargaining between developers and local councilors.

Both consequences of planning assistance could result in a more inelastic housing supply curve, due to raising the marginal cost of providing new housing units. As Figure B.1 illustrates, growth management practices implemented after 701 assistance could move the supply curve from S' to S''. The standard supply-and-demand framework would then predict both decreases in the number of new housing units and increases in the price level of housing.

[8] Conversely, an equilibrium where information is too costly to obtain could feature residents with prior beliefs on the costs of growth and inclined to block additional development, just as car buyers are unwilling to buy potential lemons in a no-trade market equilibrium. The argument in this section relates to a formal model by Chatterjee, Dong and Hoshino (2025) on how cheap access to information revealing mutual benefits generates more Coasean bargaining in an ultimatum game. We can think of the seller in the ultimatum game as a developer proposing new housing on a parcel, and the buyer as a locality deciding whether to permit it.

[9] One theoretical mechanism, based on Crémer and Khalil (1992), is that if local residents use planning information to understand the community benefit of a future development, that process is less mutually beneficial than one where the developer negotiates an agreement with more terms attached.

[10] A recent example of such a study is Dain (2023), in which the author quotes extensively from comprehensive plans adopted in the late 1960s by Boston suburbs.

The degree of change in the two market outcomes depends on the underlying demand elasticity for housing in planned cities. We can compare changes in the left panel, where the demand curve $D(A_1)$ is close to unit elastic, with a price-elastic demand curve $D(A_2)$ in the right panel. In the latter case, large quantity declines after 701 assistance are linked to marginal price increases. The treatment effects captured by our empirical work are indicative of demand elasticities for the average 701 treated municipality, which would be based on elasticities for residential structure and for local amenity bundles A_1, A_2.[11]

The logic of Figure B.1 holds both for overall housing supply in a municipality, as well as for particular housing segments underlying the housing stock. We test the supply consequences of 701 assistance aggregating all segments and individually, estimating dynamic effects that are beyond a static framework. We also leave open the possibility, consistent with the theoretical literature on fiscal zoning, that 701 assistance enabled certain jurisdictions to adapt their zoning to regulate resident entry, allowing them to provide higher-quality local public goods without free-rider problems (Hamilton, 1975). Evidence of this fiscal zoning effect should appear as a capitalization effect in our home value outcomes.

Finally, we estimate whether 701-assisted municipalities increased the quantity and scope of their development regulations, as well as changes in regulatory characteristics. Because the redevelopment of a durable housing stock is prone to numerous frictions (Siodla (2015); Brooks and Lutz (2016)), persistent supply declines in the present can be generated just from decisions to restrict supply from the past. We therefore employ additional data from documents related to local development politics, such as zoning ordinances that may have evolved from the version revised during the 701 assistance process.

Empirical findings that employ text-as-data outcomes highlight the extent of *path dependence in regulations* following 701 assistance, which in our telling served as a change in initial conditions between adopter versus non-adopter municipalities. In a political economy framework such as Acemoglu, Egorov and Sonin (2021), path dependence over time in political practices results from the combination of political influence reallocated between interest groups, as well as economic shocks that alter strategic incentives without displacing the interest groups with power. As we think 701 assistance, in addition to informing planners and local residents, also offered a template of a growth management process where those two groups have influence, our results on regulations further suggest changes in those groups' preferences for growth over time.

[11] While it is possible that we estimate negative effects in supply and prices due to 701 assistance, that result would imply the impact of the program was less in changing the supply curve and more in affecting the relative attractiveness of local amenities to non-adopting cities.

3 Data

To study the long-run effects of the federal urban planning assistance program, we first collect information about the municipalities that received Section 701 planning assistance. The policy variation is merged to a panel dataset containing municipality-level housing, population, and land-use regulation outcomes.

By "municipality," we refer to incorporated zoning jurisdictions — local governments that, under their state's land use laws, possess the authority to adopt their own zoning and land use controls. This definition includes all U.S. municipalities tabulated in the Census Bureau's place data, along with townships in most Northern states that exercise comparable regulatory powers. Because unincorporated areas of county governments in Western and Southern states were subject to different eligibility rules for the 701 program, we exclude them from our main research design but return to them in supplementary analyses.[12]

3.1 Section 701 planning assistance data

We digitized the complete 1966 Project Directory of the Urban Planning Assistance Program, which documents all 701 grants approved through July 1966. This source provides detailed information for each grant, including the amount funded by federal authorities, approval and completion dates, and, importantly, the municipalities involved. In total, the program allocated approximately $41 million in federal planning assistance during this period (equivalent to roughly $490 million in 2025 dollars).

We focus on the approximately 3,100 recipient municipalities that are part of a 2013 Core-Based Statistical Area (CBSA).[13] We merge the 701 recipient directory to standardized Census municipality identifiers and to a historical dataset of municipality-level decennial Census populations by Schmidt (2018). The population data combines data from Steiner and Heppler (2018), which tracks postwar populations of all incorporated cities that had ever reached 2,500 residents, with historical local government populations crowd-sourced by Wikipedia editors.

While 3,100 recipients account for only 15% of the 21,000 zoning jurisdictions within CBSA boundaries, the majority of jurisdictions in the denominator are small towns disconnected from the metropolitan urban area. Appendix Table A.1 displays how the denominator and the adop-

[12]Most notably, 701 assistance funding was made available to unincorporated areas at a later date than to incorporated municipalities, and county-level assistance was not restricted based on population size.

[13]While 309 of the 381 Metropolitan Statistical Areas (MSAs) have at least one 701-receiving municipality, those municipalities are also part of 340 more Micropolitan Statistical Areas. These urban areas are centered around cities that, as of 2010, had between 10,000 and 50,000 residents.

tion rate change as we remove the smallest towns. In our main analysis, we exclude municipalities with a population of less than 1,000 in 1960. Our preferred definition of municipalities eligible for 701 assistance and with a nontrivial population includes around 2,600 recipients and a 701 adoption rate of 35%.

Parsing the content of all the products of 701 assistance would be cost-prohibitive, to the extent that the reports themselves can be uncovered. [14] Instead, we digitized and coded the National Archives' "Index of HUD Section 701 Final Reports" to better understand the specific planning interventions funded through the grants. The version we use appears to include the document titles of all documents produced with 701 funding with copies at HUD's Washington headquarters, along with the assisted local government unit and publication year. We classify documents into eight separate categories of planning interventions using keyword searches on the titles, which we further document in Appendix C.

Figure 1 plots the rate at which 701 assisted municipalities received at least one document in a planning intervention category.[15] We process results over municipalities found in both the project directory and in the index, and separately by the decade when the document was filed. In the early period (1955-1959), the program's focus was varied but saw notable representation of the two kinds of products described in Section 2.2. Throughout the 1960s, most municipalities receiving 701 assistance had received three types of planning interventions: development plans that are precursors to modern comprehensive plans, zoning ordinances, and subdivision regulations. Based on contemporary case studies, we believe that development plans generally include land-use guidelines and a population forecast.

Spending on 701 grew to even higher rates in the 1970s, up to $100 million annually. However, we also know that much of that spending was allocated to large cities and to regional councils of government that handled transportation and land-use planning (Hammer Greene Siler Associates (1969), Wellborn (1975)). For suburban municipalities with lower populations, the funded activities became more complex and went beyond planning for the physical environment. In Figure 1, we find that the three document types related to land and built form regulations were less likely to be produced in the 1970s. On the other hand, documents related to the environment and to floodplain management became increasingly prevalent.

The index data corroborate anecdotal evidence from historical documents, which indicate that most municipalities receiving 701 assistance through the Small Areas program received a

[14] Feiss (1985) suggests as much that: "it is now impossible to find a complete collection anywhere [of 701 reports] ... HUD libraries should have the most, but this is not clear."

[15] The keyword search approach inevitably results in false negatives for each category, missing titles that suggest a planning intervention but are unaccounted for. Thus, the presented rates represent lower bounds of the categories' true frequencies.

comprehensive plan along with revisions of regulations affecting housing supply. Other documents that were intended products of the program, such as capital budgets for infrastructure, were produced less often. We therefore focus on the effects of 701 assistance applied in the 1960s, when the program was primarily focused on shaping the future growth trajectories of suburban communities.

3.2 Outcome Variables

We construct a national municipality-level panel of housing supply, prices, and composition, and zoning restrictiveness. Our primary data sources are tables produced from the decennial long-form Censuses up to 2000, and then from the 2013 5-Year American Community Survey (ACS); we list additional data sources as we use them. Our final panel includes decadal data for our primary and secondary outcomes extending back to 1940 and forward to 2010, wherever the data are available.

Due to privacy concerns, the Census applied different population thresholds across years in determining which tables were made public. When a municipality fell below the threshold and was therefore missing data in earlier Censuses, we describe whether and how we impute the missing data using more recent records.

Decennial housing supply. We collect and harmonize Census Bureau data tracking the year built of occupied housing units, available from IPUMS NHGIS (Schroeder et al., 2025). For sampled municipalities, the Census reports the stock of housing built in ten-year intervals, which we refer to as different *housing vintages*. Our measure of new housing supply as of year t is thus the number of housing units reported by their residents to have been completed between years $t-10$ to t. [16]

If we relied solely on the latest Census ACS tables to measure past housing supply, we would capture some of the housing stock that was newly built in earlier decades. However, such estimates would be subject to measurement error: not only are older homes depreciated and demolished over time, but owners of housing units not recently built may misremember the true construction year (Molfino, 2021). To address this, for each ten-year housing vintage, we record the value based on the Census conducted at the decade's end, with two exceptions. First, we rely on the 1970 Census alone for homes built before 1970. Second, we use 2000 Cen-

[16]The same year-built data are tabulated to measure housing supply trends in Baum-Snow and Duranton (2025). Compared to supply measures based on the Census Building Permits Survey (BPS), Census new units built data cannot capture the characteristics of vacant homes, but they span a longer time horizon and avoid permit misreporting issues (Marantz, 2024)

sus tables to impute supply for municipalities that fell below population thresholds in earlier decades, applying a depreciation formula to these counts.

Composition of housing types. The same long-form Census tables that record housing completions over time further break down each housing vintage by type. From 1950 to the present, the U.S. Census has applied consistent definitions for single-family homes and for "apartments," defined as buildings containing five or more housing units. For each type of housing built in a decade, we calculate the ratio between type-specific counts and the total number of units. Our outcome is thus the share of new housing supply each decade accounted for by single-family homes and by multifamily apartments.

The tables also provide housing composition for smaller areal units, most granularly to the Census block group. With that level of variation, we can calculate a common metric — the dissimilarity index — to measure how segregated apartment units are from non-apartment units. Details are provided in Appendix C.

Home values and affordability measures. Beginning in 1970, the Census Bureau began to tabulate self-reported owned home values at the municipality level. The Census tables report both median home values and distributional data, counting owned units falling in 15-20 price bins depending on the Census in question. The same median values and distributional tables are also provided for monthly rents, over rented units.

One limitation is that, unlike with the housing stock, there are no digitized Census tables for pre-1970 data. Instead, we link the full-count 1940 Census data from IPUMS with the Census Place Project crosswalk (Berkes, Karger and Nencka, 2022), which lets us estimate 1940 median home values and distributional data for owned homes from the microdata. While the 1940 values are our primary source of pre-701 assistance home value data, we also incorporate median values reported in the County and City Data Books (CCDB) collection, which report figures for cities with populations over 25,000 at specific points in time.

Median home value data are reported in constant 2010 US dollars. In addition, for each MSA in a decade, we calculate percentiles of the aggregated owned value bins. Fixing a percentile threshold, the share of owned units in each municipality below the threshold is a measure for the relative supply of affordable housing types compared to the housing market the municipality is in. Other secondary affordability measures, using denominators defined at a housing market level to control for selection into municipalities, are defined in Appendix C.

Regulatory restrictiveness for single-family homes. We employ data from Cui (2024) that measures how regulations limit the supply of the most affordable single-family homes. The

measures are calculated from lot-level administrative data provided by CoreLogic, with no equivalent in Census data.

The algorithm in Cui (2024) seeks to measure the rate of recurring *bunching* of lot sizes at specific lot sizes. When an MLS requirement applied to certain neighborhoods requires developers to build less densely than the profit-maximizing development density, particular values of lot sizes exhibit "excess mass" in the citywide distribution of sizes, more so than with marginally larger or smaller sizes. Cui (2024) finds the subset of lot sizes with frequent bunching and measures the magnitude, while also focusing on a primary measure of lot size restrictiveness: the excess mass percentage, between 0 and 100, of lots developed at estimated MLS requirements that are above 7,500 square feet.[17] Restrictiveness is calculated decade by decade, so the underlying denominator in each period is the single-family component of the housing supply outcome.

Our panel data are complementary to additional information on MLS requirements coded in modern zoning ordinances, which lack the time variation we exploit in our main identification strategy. Case studies of zoning ordinances up to the 2010s show that municipalities maintain regulations adopted decades ago while compounding more recent requirements (Ellickson, 2021). As a result, a literature has used natural language processing techniques on modern ordinances to study binding regulatory constraints on development (Bronin et al. (2023), Bartik, Gupta and Milo (2024)). We use those data to study outcomes in Section 6 beyond lot size regulations.

Historical trends for outcomes. Figure 2 plots the time series of our housing supply and price outcomes. Each observation is the equal-weighted mean across municipalities with at least 1,000 residents in 1960.

The figure confirms that, while the panel is unbalanced, it captures, on average, trends and cyclical patterns in the outcomes observed in aggregate time series. Examples include the multifamily building boom of the 1970s, marked on the plot as the level as of 1980; an uptick in housing construction in the 2000s driven by a greater share of single-family starts; and constant real appreciation of 10% per decade in U.S. median house values. Also notable is the peaking of the Cui (2024) lot size restrictiveness measure by 1980, followed by a slow reversion to 1950s levels. Cui (2024) attributes this ratcheting effect in restrictiveness to the persistence of postwar zoning ordinances that established MLS requirements and frictions in redeveloping lot-size-restricted parcels.

[17] The use of 7,500 square feet as a threshold reflects the size of the median lot restricted by requirements, as estimated in Cui (2024).

3.3 Historical Characteristics of Municipalities

We link individuals from the 1940 Census full count data to the zoning jurisdictions in effect at the time, using the Census Place Project crosswalk described in the previous subsection. We tabulate demographic shares up to the municipality level, including race and foreign-born status composition; educational attainment; sectoral employment; and homeownership rates. Though the 1940 Census does not report a household's total income, we impute that value using occupation information and a machine learning model trained in Saavedra and Twinam (2020).

We augment these data with spatial information on the accessibility of municipalities to other locations in the metropolitan area. We collect shapefiles on historical transportation routes from Atack (2017), and on interstate highways from Weiwu (2024). For each municipality, we calculate the distance between the municipality centroid and the nearest route for each transportation mode recorded by the shapefiles. For robustness, we also calculate alternative metrics of overlap, such as whether an interstate or a railroad crosses through a municipality's borders.

4 An Eligibility-based Research Design

4.1 Self-Selection Into 701 Takeup

To estimate how urban planning assistance affects housing and growth outcomes, we could start with a long difference regression at the municipality level with the adoption of 701 assistance D_i as the treatment:

$$Y_{ist} - Y_{is,1960} = \delta + \beta D_i + X'_{ist}\gamma + \varepsilon_{ist} \tag{1}$$

The coefficient β in Equation 1 identifies a causal effect, relative to outcome levels in the pre-treatment year of 1960, only if municipality fixed effects and controlling on observables eliminate all sources of bias. However, that assumption is unlikely to hold given the potential for cities to self-select into the program. Cities that experienced or expected an accelerating pace of suburbanization may have been more willing to apply for 701 grants, based on information unobserved by the econometrician.

If municipalities self-selected into 701 in response to pre-701 demand pressures, we expect two patterns to hold. First, housing prices in adopting municipalities should already be elevated relative to non-adopters. Second, under Rosen-Roback spatial equilibrium, high local

amenity values could be capitalized through housing prices rather than through wages, implying that adopting municipalities exhibit higher price-to-income ratios. We test for both patterns using the 1940 full-count Census data described in Section 3.3.

Figure 3 visualizes the results, where we split 701 municipalities by the year they first received a 701 grant and calculate means across 1940 demographic characteristics. Panel (a) confirms two trends consistent with selection into 701 on demand pressures. First, average home values for 701 recipients over much of the program period were higher than those in non-adopting municipalities. Second, comparing self-reported home values in the 1940 Census to our imputed household incomes, adopting municipalities had a price-to-income ratio of 1.8, compared with 1.68 for non-adopting municipalities.

Table A.2 shows significant differences in means not only for price and household income, but over a variety of historical covariates determined before 701 assistance. In Panel (b) of Figure 3, we describe how 701 adopting municipalities differ in two other 1940 demographic variables. We tabulate the share of 1940 households with a worker in manufacturing, a proxy for the suburb having already been accessible to working-class households. We also tabulate the share of households with a worker in agriculture, a proxy for how rural the municipality was before postwar suburbanization. The results indicate that the first adopters of 701 were more occupied by manufacturing households, prior to an expansion of 701 eligibility and generosity in 1961. After that, 701 adopters appear to have been drawn from municipalities that were relatively more rural before World War II. Table A.2 further confirms that 701 adopters are more likely to have an Interstate highway pass through jurisdiction borders, as well as having 6 log points faster population growth during the 1950s.

Taken together, these patterns suggest that 701 adopters were fast-growing municipalities with amenities that were more attractive to postwar suburbanites. Local governments that expected persistent demand shocks likely self-selected into urban planning assistance. To address this selection bias, we employ a research design where identification comes from policy eligibility criteria, which do not depend on the particular conditions of local governments.

4.2 Empirical Strategy

Our research design leverages two sources of variation in receiving 701 assistance. First, we exploit a federal eligibility rule for 701 funding: the program was restricted to localities with populations below 50,000 at the most recent decennial Census. Second, we exploit cross-state variation in program adoption rates driven by institutional differences in how state planning agencies administered disbursement of 701 funds, a claim we verify using both historical

records and quantitative evidence.

Details of Eligibility Conditions. Figure 4, we verify the importance of the population cutoff for policy eligibility. Since most 701 assistance grants were funded after 1959, the eligibility of most 701 adopters was determined by population in the 1960 Census. We split the sample into bins of equal width with respect to the 1960 population running variable. We then fit a LOESS regression on the binned means to derive nonparametric predictions of 701 adoption rates. Conditional on being below the population cutoff, the 701 adoption rate for cities is 42%. By visualizing bin-specific municipality counts through bubble sizes, the Figure also shows that most treated municipalities are well below the population eligibility threshold: the interquartile range of adopters' 1960 populations is 2,800 to 12,700, and adoption rates are higher in cities with populations of 10,000 to 25,000.

Cities with a 1960 population above 50,000 are nearly absent from the 701 project directories, except for 10 exceptions. Seven of the 10 received all of their 701 funding before 1960, in which case we hypothesized they were approved for 701 assistance based on their 1950 population levels[18]. Confirming that the eligibility threshold was binding nationally, we can define in virtually every state a pool of cities ineligible for 701 assistance.

Apart from population-based eligibility, federal bureaucrats did not actively intervene in determining which cities received 701 assistance. Independent agencies in each state had to match planners to cities and prepare invoices for planning costs to the federal government before any federal grant was approved. The institutional context thus shares similarities with the allocation of federal urban renewal funds to states, which were ineligible for funding if they could not pass relevant enabling legislation (Collins and Shester, 2013).

A citation from a third-party report had concluded: "there is no consistency among the states in the administration of the 701 Program." (Hammer Greene Siler Associates, 1969) We focus on two particular ways where institutional details of each state's agency matter for the breadth of funds allocated to municipalities. First, agencies differed in how they promoted the program to cities, with a few listing requirements on who was even allowed to apply. Second, insufficient staffing of the agency caused delays in approving grants before the year's Congressional allocation for 701 funds was exhausted, which meaningfully altered the state's share of allocated 701 funds.

An illustrative contrast is comparing the states of Rhode Island and Texas. The Rhode Island

[18]The remaining three 701 municipalities are: Gaithersburg, MD; Town of Union, NY; and Bristol Township, PA. All three hosted a federal agency headquarters or a military installation after World War II, which could qualify them for 701 under an exceptional clause of facing "rapid urbanization ... expected to result from the establishment or ... expansion of a Federal installation."

government established a state planning council shortly after the 701 Program was introduced and soon hired five teams of in-house urban planners to work on planning projects. By contrast, the responsible state agency in Texas was a division of the state's Health Department, not led by any urban planner. We offer further case studies of agency conduct in Appendix D.1, which also explain why some states had near-zero 701 adoption until the late 1960s.

Adapting a triple difference framework. In our research design, we leverage variation in population-based eligibility as well as generate a measure of "exposure" to state agency capacity for processing 701 assistance funds. We construct a binary variable for whether a municipality is above or below 50,000 residents in the 1960 Census. Then, we count every municipality approved for a 701 assistance grant up to 1962. We calculate the *adoption rate* for each state s as the ratio of the share of approved municipalities among all municipalities with 1960 populations between 1,000 and 50,000:

$$\tilde{Z}_s = \frac{\sum D_i}{\sum \mathbf{1}[Pop_i \in [1K, 50K]]}.$$

The rate of eligible municipalities approved for federal planning grants, a year after program reforms in 1961, is our preferred measure of how effective state agencies were in meeting program demand at the local level. Alternative definitions, using adoption rates by the program's end, could introduce a reverse causality problem where faster-growing states expanded their agencies' capabilities to process program demand.[19] Figure 5 plots the rates $\tilde{Z}_s$ as of 1962 for every state; even within U.S. regions like the South, the West, and New England, there is meaningful variation in $\tilde{Z}_s$. Average adoption shares are slightly higher in the Northeast or in the South. Within each Census region, we still observe at least one state with adoption rates between 0 and 10 percent.

For municipality i in state s and year t, we estimate:

$$\begin{aligned} Y_{ist} = \theta_i + \delta_t + \beta \underbrace{E_i \times \tilde{Z}_s \times \mathbf{1}[t > 1960]}_{\text{DDD term}} \\ + \eta_1 E_i \times \mathbf{1}[t > 1960] + \eta_2 \tilde{Z}_s \times \mathbf{1}[t > 1960] \\ + X'_{ist} \times \mathbf{1}[t > 1960]\gamma + \varepsilon_{ist}, \end{aligned} \tag{2}$$

where E_i is an indicator for eligibility (population less than 50,000) and X'_{ist} represents a

[19] Nonetheless, the first panel of Appendix Figure B.3 shows high correlations between 1962 rates and the observed adoption rates as of 1966.

vector of controls. The regression parameter β reflects how a municipality's outcomes change from being exposed to a state agency that processed no 701 assistance funds to an agency that processed 701 funds for every eligible municipality.

To understand how our regression resembles the standard triple difference framework (Olden and Møen, 2022), we note that we can keep states with only high and low $\tilde{Z}_s$ and stratify them into two groups. An event study using only this set of groups can be invalidated due to non-institutional confounders that drive both higher $\tilde{Z}_s$ and urban growth. The advantage of using ineligible cities is that it requires a weaker identifying assumption than using only cross-state comparisons: the adoption rates across states must just be unrelated with how a state's more populous cities evolved compared to their less populous ones.

The same identifying assumption applies when we use the continuous adoption rate, which exploits the full support of cross-state variation. In Appendix D, we estimate the effect derived from a conventional triple difference design by constructing four different treatment groups. However, because a group in low-adoption states still has a positive $\tilde{Z}_s$, it cannot reflect the counterfactual in which 701 assistance was never applied. As that counterfactual is needed to calculate average treatment effects on the treated (ATT), we instead detail the extra assumptions we make so that ATT is backed out through a rescaling step: $\hat{\tau} = \beta \times E[\tilde{Z}|D = 1]$.

Our design also addresses a limitation of a conventional regression discontinuity design (RDD). An RDD estimator at the 50,000 threshold identifies effects only for municipalities near the cutoff. But the median adopter in the 701 context is closer to having 10,000 residents. Estimating an average effect inclusive of smaller municipalities requires extrapolating the conditional expectation function (CEF) using information from ineligible cities, which will fail if the no-701 counterfactual regression function is nonlinear (Angrist and Rokkanen (2015), de la Cuesta and Imai 2017).

In low-adoption states, eligible municipalities went largely untreated. Under the same "parallel bias" assumptions used for triple differences, the CEF for eligible cities in those states can serve as a direct counterfactual CEF for cities well below the threshold in other states. Then, even if there are nonlinear counterfactual CEFs, extrapolation is possible by setting up an RD over the high-adoption states and comparing CEFs with the same design over states least exposed to 701 grant funding. In Appendix D, we illustrate how the ATT of interest is approximately estimated as the difference of two RD designs.

Identification argument for policy effects. We show formally the assumptions needed to both identify β in the exposure regression and for β to have the correct structural interpretation such that a rescaling step converts it to an estimate of the ATT. The first assumption states that absent

the program, the gap between smaller and larger municipalities would have evolved similarly across high- and low-adoption states. Formally:

Assumption CI (Conditional independence of eligibility cutoff): In a window of 1960 populations around 50,000, $(\underline{P}, \overline{P})$, for all $t \geq T$, the year of first treatment, the potential outcomes in the absence of treatment Y^0 satisfy:

$$\mathbb{E}\Big[Y^0_{t,s} - Y^0_{t-1,s}\Big|\text{Pop}_{1960} \in (\underline{P}, 50K]\Big] - \mathbb{E}\Big[Y^0_{t,s} - Y^0_{t-1,s}\Big|\text{Pop}_{1960} \in (50K, \overline{P}]\Big] \perp\!\!\!\perp \tilde{Z}_s \Big| X.$$

The second assumption is on the functional form of how true treatment effects vary between states. True treatment effects scale linearly with state adoption rates:
Functional form assumption: $\mathbb{E}\big[Y_t - Y^0_t \big| \tilde{Z} = z, Pop_{1960} \in (\underline{P}, 50K]\big] = \beta z$.

The ATT can therefore be identified from the scaling effect β, further rescaled by a treated population's mean adoption rates:

$$\mathbb{E}\big[Y_t - Y^0_t \big| D, Pop_{1960} \geq \underline{P}\big] = \beta \int z dP_{D,Pop\geq\underline{P}}(z) = \beta \times \mathbb{E}[\tilde{Z}|D, Pop_{1960} \geq \underline{P}]. \tag{3}$$

Estimation of treatment effects requires us first to run the exposure regression in Equation 2, followed by plugging in the estimated β into treatment effect formulas. We exploit the symmetry of our econometric model with a framework studied in de Chaisemartin et al. (2025), concerning difference-in-differences with an exposure instrument. Using their methods, we prove a more general statement:

Proposition: *Under Assumption CI and a specification for conditional treatment effects linear in adoption rates and in 701 adoption status,* $\mathbb{E}[Y_t - Y^0_t] = \tilde{Z}\beta' \begin{bmatrix} D \\ 1-D \end{bmatrix}$*, the data identify:*

$$\tau_{treat} \equiv \mathbb{E}\big[Y_t - Y^0_t \big| D, Pop_{1960} \in (\underline{P}, 50K]\big], \qquad \tau_{elig} \equiv \mathbb{E}\big[Y_t - Y^0_t \big| Pop_{1960} \in (\underline{P}, 50K]\big],$$

where τ_{treat} *is the average treatment effect on treated municipalities and* τ_{elig} *is the average treatment effect on all eligible municipalities.*

Proof. See Appendix D.5. □

With a panel of outcomes and two known drivers of 701 eligibility, we can identify both a standard ATT, as well as a second treatment effect τ_{elig} over all eligible municipalities. The latter effect applies to a greater share of the U.S. urban population, but its magnitude relative

to τ_{treat} depends on whether other smaller municipalities mimicked 701 recipients' regulatory innovations —- a case of policy spillovers. When treatment linearity cannot be rejected in the data, the parametric assumption, while substantial, remains useful to improve the statistical power of the underlying research design (de Chaisemartin et al., 2025).[20]

4.3 Evidence supporting cutoff conditional independence

A key identifying assumption, Assumption CI states that absent the 701 program, the difference in outcomes between municipalities above and below the population cutoff would have evolved similarly across high- and low-adoption states. We first conduct a variety of balance tests that cannot reject outcome divergence before the 1960s. We then offer empirical facts further confirming the significant role state agency capacity played in determining state-level adoption rates.

Table 1 summarizes the balance tests we conduct on 1940s and 1950s data, which can be split into tests on the outcome variable, as well as on other covariates, prior to policy take-up. The first set of tests reproduces an exercise reported in Lin and Peri (2025), which compares pre-trends across groups stratified by a continuous treatment in a triple differences design. In our context, the equivalent test is that *with pre-period housing units built, our primary outcome, any difference between eligible versus ineligible municipalities is the same, whether estimated for high- or low-adoption-rate states.*

We stratify our sample into two groups of states based on whether each state's adoption rate is above the analysis sample mean of 22%.[21] We follow Lin and Peri (2025) and back out the outcome bias across the eligibility threshold using an event study specification, which involves estimating two-way fixed effects but not controlling for time-varying effects of observed confounders. Setting 1940s housing supply as the outcome, The two negative values in Panel (a) reflect the "difference-in-difference" bias

$$\mathbb{E}\left[Y_{1940s} - Y_{1950s} \middle| \text{Pop}_{1960} \in (\underline{P}, 50K]\right] - \mathbb{E}\left[Y_{1940s} - Y_{1950s} \middle| \text{Pop}_{1960} \in (50K, \overline{P}]\right]$$

estimated separately for high-adoption and for low-adoption states. Reassuringly, we cannot reject the null hypothesis that the bias terms are the same.[22]

[20] The concept of taking a coefficient in a continuous instrument and rescaling it by an expected value was also employed in empirical macroeconomics (Mian and Sufi (2012); Berger, Turner and Zwick (2020)), albeit using a binned distribution to approximate the expectation instead of direct calculation.

[21] A map of the two groups is in Appendix Figure B.3, in which the continental U.S. is divided into two groups of 24 states.

[22] Appendix Figure D.5 visualizes the differential trends test. Appendix Figure D.6 runs an alternative test that

In Panel (b), we reuse our tabulation of 1940 demographic characteristics by municipality, as defined in Section 3.3. We run, for each possible confounder, a cross-sectional regression and report an interaction term between population cutoff-based eligibility E_i and a dummy variable for being in a high-adoption or low-adoption state. We are further reassured by the small point estimates on most demographic composition characteristics: if estimates were significant, they reflect channels through which Assumption CI is violated. A more notable eligible-ineligible difference is median home values being $4,500 less in high-adoption states than in low-adoption states, but this is in 2010 dollar terms and only 4.5% of the baseline mean.

A more notable fact is a difference of 38 log points in 1950s population for municipalities in different states. First, this differential is not robust to definitions of the adoption rate dummy: if we define high- and low-adoption rates based on the top and bottom terciles of adoption rates, the difference reduces to one log point. Second, to control for any influential states with high 1950 growth rates, in all our main specifications we include the percentage change in municipality population between 1950 and 1960. With our empirical strategy, it does not appear that small municipalities in high-adoption rate states witnessed sociodemographic composition changes unique to them.[23]

In Section 4.2, we cited case studies and policy briefs suggesting the lack of consistency in how 701 agencies administered the allocation of funds. To quantify the importance of agency administration relative to other confounders in determining adoption rates of $\tilde{Z}_s$, we show adoption rates can be explained by two dimensions of agency conduct.[24] While we detail the analysis in Section D, our hypothesis is that state agencies with high capacity receive more 701 assistance funding per capita, as well as better exploit a pre-1961 program loophole that allows for large grants allocated over many small municipalities. A saturated linear regression of two measures for these strategic differences on $\tilde{Z}_s$ has an R^2 of 0.57.

While we cannot rule out municipalities in certain states being more likely to adopt other forms of federal planning assistance after the 1950s, we think that possibility cannot drive our results. 701 assistance adopters were unlikely to have received federal funding before the process, as a comprehensive plan provided under 701 assistance were themselves a prerequisite for urban renewal and for other federal funding. Considering the possibility that urban renewal

separately calculates the bias between states for eligible and for ineligible municipalities, which produces a smaller point estimate in bias differences as well. Pre-period tests are similar using alternative definitions for high- and low-adoption states, such as one based on terciles instead of on the median.

[23]This contrasts with the balance tables split on observed treatment status in Table A.2. There, treated municipalities are not growing much faster than untreated ones. However, treatment status is correlated with home values and sociodemographic differences in 1940 that signal future demand for those treated municipalities.

[24]The ideal data for this analysis – agency-level staffing counts – appear infeasible to retrieve.

efforts in high-adoption states' large cities could have spillover effects, we show in Appendix D that adoption rates appear uncorrelated with when state legislatures receipt of federal urban renewal funds. Finally, as we have also shown in Section 3.1, 701 planning grants in the 1970s shifted their focus away from assistance of physical planning and regulating housing construction. Differential receipt of federal planning funds after the 1960s has a more tangential impact on relative housing supply between local governments.

We finally note that our treatment effect is not estimated for every recipient of 701 assistance, but is conditional on a population lower bound $\underline{P}$. Our empirical strategy is unreliable for the smallest municipalities, for which regression functions in low-adoption states do not reflect the counterfactual in high-adoption states. In our final analysis sample, we limit our focus to only cities with 1960 populations between 5,000 and 200,000, a choice of both upper and lower population bounds that was data driven. We adapt a heuristic introduced in Angrist and Rokkanen (2015); we can reject Assumption CI applies in a window around 50,000 based on whether residualized outcomes in the pre-period have flat CEFs over Pop_{1960}.

In Appendix Figure D.7, we conduct a visual analysis of the pre-trend in residualized outcomes,

$$\mathbb{E}\left[Y_{1950s} - Y_{1940s} \middle| \tilde{Z}_s \text{ above median}, X\right] - \mathbb{E}\left[Y_{1950s} - Y_{1940s} \middle| \tilde{Z}_s \text{ below median}, X\right],$$

for different binned values of 1960 population Pop_{1960}. As seen in the Figure, our lines of best fit on both sides of the eligibility discontinuity are both nearly flat. The binned residuals over municipalities with less than 5,000 residents suggest that had we expanded the analysis sample to those cities, we would violate Assumption CI and introduce a negative bias in final estimates.

5 The Growth Consequences of 701 Assistance

5.1 Supply Effects

We present triple difference estimates of 701 assistance on decadal housing supply, following Equation 2. We require that all municipalities used in our regressions possess a balanced panel in the supply outcome, which excludes small municipalities not tabulated in the earliest Census data. We also exclude far-flung communities in metropolitan areas that are still included in county-based CBSA definitions by keeping only municipalities with centroid distances of 50 kilometers from CBSAs' central business districts. The last row of Appendix Table A.1 shows that about 2,150 municipalities are eligible for 701, coupled with around 300 ineligible mu-

nicipalities used in our design.

Table 2 presents our estimates across specifications that vary in both the right-hand-side controls and the analysis sample. From columns (1) to (5), we add observable controls for 1950s population growth; transportation accessibility variables (defined in Section 3.3); and fixed effects for Census regions and Census divisions interacted with decade fixed effects. We also present estimates using different population thresholds: our preferred sample drops small municipalities with fewer than 5,000 residents, in line with the analysis in Section 4.3 that suggests including them would violate our identification assumptions. Results following our preferred population thresholds are in the table's second panel.

Across all these specifications, our triple-difference coefficients stay within the range of -0.55 to -0.8.[25] Our preferred coefficient estimate is in Column (4) of the second panel, where the estimated triple difference coefficient is -0.678. One way to interpret this coefficient is that it reflects the decline in housing supply due to the application of 701 in the first quartile state by adoption rate (Michigan) relative to its application in the third quartile state (Washington): approximately $-0.678 \times (0.35 - 0.09) = -18\%$, due to the effects of 701 assistance.

Our preferred interpretation is to rescale the coefficient to the ATT τ_{treat}, where the treated population is the 1,000 municipalities we can confirm were 701 adopters. The mean level of state adoption rates among known program adopters in 1966 is 0.29. That average is the rescaling term we use to convert all the β to ATTs. Table 2 reports the ATT corresponding to each specification: the implied ATT for our preferred specification is a 20.0% decrease in new units built in the municipality every decade. We address concerns that this rescaling term depends on restrictive assumptions on treatment effects in the robustness section, Section 5.4.

In Figure 6, we estimate results more flexibly over time by running the event study design analog to the triple difference design:

$$\begin{aligned} Y_{ist} = \theta_i + \delta_t &+ \sum_{t \neq t_0} \beta_t \times \mathbf{1}[T = t] \times E_i \times \tilde{Z}_s \\ &+ \sum_{t \neq t_0} \eta_{1t} \times \mathbf{1}[T = t] \times E_i + \sum_{t \neq t_0} \eta_{2t} \times \mathbf{1}[T = t] \times \tilde{Z}_s \\ &+ \sum_{t \neq t_0} \mathbf{1}[T = t] \times X'_{ist} \gamma_t + \varepsilon_{ist}. \end{aligned} \tag{4}$$

[25] If small municipalities with fewer than 5,000 residents grew more quickly in low-adoption states than high-adoption states, that introduces a negative bias on β corrected for in the second panel. The additional controls we employ both correct for positive omitted-variable bias (due to residual positive correlation with demand shocks), as well as reducing standard error to improve our design's statistical power.

Focusing on the top-left panel, which shows results on the logarithm of new units each decade, we see that 701 assistance did not substantially change the supply of the 1960s housing vintage relative to the reference period of 1950s housing construction. Larger effect magnitudes became more apparent starting from the 1970s. In the bottom panel of Table 2, we estimate a more flexible specification than Equation 2, where effect estimates are only pooled over the post-1970 period. The post-1970 period coefficient is even greater in magnitude at -0.79, implying an ATT of 23% fewer units built per decade.

In Figure 6, the remaining three panels present additional results related to the composition of housing supply. The top-right panel expresses the housing supply effect in cumulative units built since the 1950s.[26] We estimate the total τ_{treat} up to 2010, indicated by the rightmost data point in the figure, is 15.0%.

The bottom two panels examine composition changes in greater detail: for each year, we estimate the effects of 701 assistance on the share of homes built up to a year that are single-family or multifamily apartments. Composition effects due to 701 may have occurred as of the 1960s, when it was unclear overall supply had decreased: the point estimate for the apartment (single-family) share already turned negative (positive), though effects in that decare are significant at the 90% rather than 95% level. As municipalities in our analysis sample underwent a multifamily construction boom from the late 60s to the 1980s, the cumulative multifamily share further declined before stabilizing in the 21st century.

Table 3 reports the total effect on the cumulative apartment share up to 2010. Our preferred coefficient estimate is on the order of -9.6 percentage points, implying $\tau_{treat} = 2.8$ percentage points fewer units in the long run that were inside apartment buildings. Unit production seem to have entirely substituted to single-family units, as the last panels of Figures 6 and Table 3 indicate: We estimate a long-run τ_{treat} of 3.1 percentage points in the single-family composition share, as of 2010.[27]

5.2 Effects on Prices and Affordability

Consistent with the modeling of 701 assistance as a change in local governments' housing supply curves in Section 2.3, the program should cause both supply declines and price increases

[26] Because the supply effects in Table 2 are heterogeneous by housing vintage and municipalities built more housing earlier in time, our results do not imply that 701-assisted municipalities built 20% fewer units since the 1950s.

[27] The similarity in coefficient magnitudes is not mechanical. Units not defined as either single-family units or apartments include "missing middle units" with 2-4 units per building, as well as mobile homes. Both housing typologies were produced more in the 1970s to meet demand, but our results do not suggest they meaningfully grew or declined in share among 701-assisted municipalities.

even when location demand is unchanged. In the first panel of Table 4, we show the effects of 701 assistance on median home values. The results here are subject to a caveat first discussed in Section 3.2: the identifying assumption for price outcomes uses trends relative to 1940, the only year prior to 701 assistance for which we have comprehensive median home values. The inclusion of population growth and transportation accessibility columns across columns corrects for negative omitted variable bias on the home values outcome, and our preferred results with region fixed effects imply $\tau_{treat} = 8.5\%$ higher median home values in 701-assisted municipalities.[28]

We prefer to interpret these results less as accurate point estimates and more as rejecting implausible inelastic demand for 701 locations. For example, the upper bound of the 95% confidence interval for our home values coefficient is 0.54. As this upper bound estimate of β^{price} remains lower in magnitude than β^{supply} reported in Table 2, we produce evidence against the possibility that marginal homeowners in equilibrium are price inelastic for 701-assisted municipalities, which should not occur when municipalities use regulation to exercise pricing power for locations.

The second panel shows that on our broadest measure of housing affordability, the share of owned homes with values below the metropolitan area's median value, we find an implied ATT of 4.9 percentage points lower share in 701-assisted municipalities. This suggests that 701 assistance can explain part of the cross-sectional dispersion in housing affordable to potential homebuyers within housing markets. However, other results indicate 701 assistance cannot explain cross-sectional dispersion in *affordability for renter households*. Figure B.5 shows 701 effects are null when the outcome measures the share of "bottom segment" homes below the 25th percentile market-wide value, or the share of rental units whose annual rents are less than 30% below the median renting household's income. In Figure B.6, we also use our triple difference design to find 701 assistance causes sorting of higher income owner-occupiers to treated municipalities, but there is little evidence that higher income renters are sorting there.

Should 701 assisted municipalities have both more unaffordable owned housing as well as similarly affordable rental homes, those facts are consistent with those municipalities having greater market segmentation on housing tenure, in the sense of Greenwald and Guren (2025). We note that, since many 701 assisted municipalities experienced strong population growth before adopting the program, the average municipality would have sizable rental unit stocks and homes that are nonconforming to updated land use regulations. If planning practices after

[28] As with Figure 6, we show graphs of the analogous event study estimates for these outcomes in Appendix Figure B.4. This figure also includes additional results for the degree of bunching on all MLS requirements, further discussed in Section 6.1.

701 assistance complicate any upgrading or redevelopment of those nonconforming units, the owned housing segment becomes segmented from the rental segment; adjustments to higher demand for owned housing in the housing market become predominantly through higher price-to-rent ratios than through growth in owned housing supply.[29]

There remain other channels through which 701 assistance increases the price levels of owned and rental housing in housing markets, in ways that are hard to detect in the cross-section. First, the long-run land supply of housing markets is pinned down by geographic fundamentals. The consequences of reduced density and supply in built-up communities may be felt decades later, once constructing new communities far from the central business district becomes prohibitively costly. Second, the price elastic responses we estimated in Table 4 were for owner-occupied units only. Should incumbent renters become more price inelastic for 701-assisted municipalities or a lack of new rental supply in the market drive down vacancy rates, rent dynamics in certain 701 adopters could trend towards unaffordability. We intend to model these general equilibrium effects of housing affordability in future work.

5.3 The Scope of Implemented Zoning Restrictions

Using panel variation in minimum lot size requirement restrictiveness detailed in Section 3.2, we estimate whether 701 assistance revised MLS requirements to restrict the production of single-family homes. By construction, the Cui (2024) lot size restrictiveness measure tabulates bunching around minimum lots of at least 7,500 square feet, focusing on binding densities lower than those of prewar housing or the earliest postwar subdivisions.

In Table 5, we estimate the triple-difference design of Equation 2 on lot size restrictiveness. The coefficient for our preferred specification is 4.4 percent of average decadal single-family development, rescaled to an ATT of $\tau_{treat} = 1.3$ percentage points over each decade's single-family development. Referencing the event study in Appendix Figure B.4, the treatment effect is persistent from the 1960s to at least the end of the 20th Century.[30]

Although a 1.3-percentage-point effect is meaningful against the baseline (28% of the post-1960 mean), it seems unlikely to account for all of the substitution toward single-family supply. Using estimates from Figure 6, the decline by 1980 in cumulative apartment supply due to 701

[29]The segmentation framework can also explain how higher income owner-occupiers crowd out potential home-buyers for the limited stock of listed owned units in 701-assisted municipalities.

[30]To address potential concern that the lot size restrictiveness is misspecified or cannot capture large lot zoning, Appendix Figure B.9 presents additional specifications where the outcome tabulates bunching around lots larger than 7,500 square feet, or cross-sectional data on municipalities' maximum MLS requirements recorded in Bartik, Gupta and Milo (2024). We broadly find that 701 assistance also causes greater prevalance of MLS requirements between 7,500 square feet and 1 acre.

assistance is approximately $(-30.7 + 10.15) \times 0.29 = -5.9$ log points. If that shortfall is only due to land parcels that were primarily developed for single-family units experiencing density restrictions, the counterfactual market-driven density on those parcels should be $5.9/1.3 = 4.5$ times higher than the densities dictated by MLS requirements, much denser than the observed density of postwar subdivisions.[31] This calculation suggests 701 assistance also implemented zoning that restricted *residential use* on parcels that were viable for apartment construction in the counterfactual.

As even the latest data sources on historical zoning maps are far from tracking them at a nationwide scale (Gallagher, Shertzer and Twinam, 2024), we use a proxy for allowed residential use based on the built form dissimilarity indices described in Section 3.2. We assume that if 701 assistance expanded single-family zoning on more parcels, apartment construction in 701 assisted municipalities will be more segregated from single-family construction. Having constructed the dissimilarity index using 1990s data, we estimate the pooled regression with Census region or division fixed effects $\theta_{r(i)}$:

$$Y_{is} = \theta_{r(i)} + \beta E_i \times \tilde{Z}_s + \eta_1 E_i + \eta_2 \tilde{Z}_s + X'_{is}\gamma + \varepsilon_{is}. \tag{5}$$

The results are provided in the second panel of Table 5. We estimate statistically significant effects of 701 assistance on apartment dissimilarity within municipality borders, with an ATT of 0.034 — an effect size of 0.26 standard deviations in the outcome over the analysis sample. Put together, the two sets of results indicate how the planning products introduced through 701 innovated on land use regulation on multiple dimensions, as one would expect if the suggested land use plan from the assistance process was binding on market supply.

5.4 Robustness Checks and Aggregate Implications

In Appendix F, we show that our research design passes donut and placebo tests used to verify RD designs; our results are robust to changes in the analysis sample, simultaneous changes in planning and environmental regulation, and alternative measures of housing supply and affordability; and that the best panel data we could find on municipality area show our results are not driven by distinct land annexation practices by 701 treated places. In the remainder of this subsection, we confirm the direction of selection bias corrected for by our research design,

[31] Compare the required density for a MLS requirement of 8,000 square feet — 1,750 square feet — with lot sizes of 5,000 square feet for postwar detached homes.

show whether our results differ based on alternative models of treatment effects and produce back-of-the-envelope estimates of supply effects better connecting our results to the broader housing literature.

Direction of selection bias. In Section 4.1, we offer evidence of self-selection into the Section 701 program by high-amenity and faster-growing municipalities. Without a research design, this form of self-selection confounds supply-side effects of planning assistance with positive local demand shocks. Appendix Table A.3 presents results of the endogenous regression over two samples stratified by 1960 population, as was done for Table 2. As we would expect from self-selection bias based on positive demand shocks, all estimates on quantity and price outcomes are more positive than our design-based results in Tables 2 to 4.

The magnitude of bias is also informative about the baseline supply elasticity without 701 assistance: if the counterfactual housing supply is highly elastic, positive demand shocks induce large changes in supply even if changes in prices are small. This claim is consistent with the larger bias magnitude in housing supply estimates ($0.263 - (0.200) = 6.3$ log points) relative to the bias in median prices ($0.112 - 0.085 = 2.7$ log points).

Regression design allowing for distinct spillover effects. Appendix Table A.4 presents, for outcomes with panel variation, results from a more flexible regression specification. In them, we allow 701 assistance effects to vary between municipalities confirmed to have received a 701 grant and municipalities that were eligible for 701 while unconfirmed to have received assistance. Our sample of confirmed 701 adopters includes both 701 municipalities recorded in the project directories, as well as cities we impute to be adopters from 1966-1970 based on the document types provided for them according to the HUD Final Reports database; details are provided in Appendix C.

The average effects for the eligible but untreated group should be both different from those of confirmed adopters and nonzero due to policy spillovers: eligible local governments could, instead of waiting to receive 701 assistance to build planning capacity, emulate the planning products of comparable 701 adopters. However, if the resulting treatment effects do not differ strongly from the ATTs produced from our more parsimonious main specification, the main specification is a reasonable approximation with the benefit of added statistical power (de Chaisemartin et al. (2025))

Our results generally point to estimate robustness even when allowing distinct spillover effects. The coefficients for exposure regressions in Panel A point to different margins of adjustment in confirmed adopters versus 701 eligible municipalities: it is only with confirmed

adopters that we see reallocation away from apartment units and growth in lot size restrictiveness after 701 assistance. However, even eligible non-adopters see large supply declines after planning assistance scaled up. In Panel B, we compare the average treatment effect over confirmed adopters with the average treatment effect over all eligibible municipalities, the latter being a larger treatment population. The supply ATT over confirmed adopters appear to be lower at 18% instead of 20%, while non-supply effects over the entire eligible population are smaller in magnitude than what was estimated in our main specification. Effect differences between specifications are far from being statistically significant, with results for the moderate segment housing share a potential exception.

Aggregate implications. Comparing our results to related literature on the local political economy of housing supply requires converting the effect to a units per capita basis. We proceed by calculating average units built and population for all cities in states with the lowest levels of 701 adoption rates, which we treat as a counterfactual rate of housing production per 1,000 residents in each decade. We limit to states with rates $\tilde{Z}_s \leq 5\%$ and apply the 20% supply decrease to estimate policy effects.[32] In the 1970s, this calculation leads to a policy effect of

$$7.86 = (100\% - 20\%) \times (7.86 + T) \Rightarrow T = 1.74 \text{ annual units per 1,000 people.}$$

Mirroring lower production on a national level, the counterfactual rates decrease from 7.9 in the 1970s to 6.0 in the 2000s. Applying the same pooled effect yield policy-induced declines of 1.2 to 1.5 annual units per 1,000 people in the ensuing decades.[33]

Our results are comparable to magnitude, if not slightly less, than average treatment effects found by changes in city council electoral systems in Mast (2024) (1.75 annual units per 1,000). It is smaller than supply effects of electing a real estate developer to city council in Ouasbaa, Solé-Ollé and Viladecans-Marsal (2025), on the order of of 10 units per 1,000 people in California during an electoral term. On a 4-year basis, our effect estimate for the 1970s amounts to a loss of 7 units per 1,000 people. Unlike that paper's results, our results persist for decades beyond the developer's first electoral term.

We can also convert our effect into total units by scaling the proportional decline with a counterfactual level of housing production in the treated population. To accomplish this, we sum up national housing starts from the Census Building Permits Survey between 1960–2013 and scale it twice: first by the share of residents living in metropolitan areas in 1970 and

[32] Average trends are similar if we use the national median of 22% to define the no-treatment sample.

[33] If we used, instead, the Table 2 effect on the treated estimated only after 1970, policy-induced declines in the 1970s are 3.2 annual units per 1,000 people in the 1970s to 1.4 annual units in the 2000s.

then by the share of metropolitan population growth from 1960 to 2010 that took place in 701-eligible municipalities above the 1960 resident cutoff: we find that share to be 21%. The persistence of supply effects over many decades, coupled by the scope of program adoption, results in a large policy effect of 2.7 million units not built due to 701 assistance.[34]

Heterogeneity in housing market characteristics. Appendix Figure B.7 presents ATT results derived from the main specification in Equation 2, but estimated over two subsamples stratified by a dimension of heterogeneity. We check if racial composition matters for 701 treatment effects by stratifying on whether the metropolitan area the municipality is in had a 1960 Black American share of over 5 percent. We check if pre-period population growth matters by stratifying on whether growth in the 1950s exceeded 39%, the national median across analyzed municipalities. Finally, we estimate effects separately for states in the Midwest, New England and Pacific Census Divisions separately from Southern and Mountain Interior states.

Though effects across our supply and affordability outcomes differ little depending on 1950s growth, the other stratified analyses reveal two sets of patterns. Cities in MSAs with a smaller Black presence altered the composition of new supply more than average levels, with a large ATT on the order of a 4 percent rise in lot size restrictiveness. These results appear consistent with a stylized model proposed in Cui (2024): postwar local governments' willingness to adopt MLS requirements balanced property value gains from excluding denser homes affordable to Black homeowners with transaction costs in regulatory implementation. As Section 701 grants reduce implementation costs, they incentivize municipalities in housing markets with even small levels of Black subrbanization to alter their housing regulations.

We also find that our Equation, estimated only over Southern and Interior states, yield point estimates close to zero for all of our supply outcomes. Coefficients remain negative estimated over the Coastal and Midwest states, suggesting that the aggregate unit loss due to 701 assistance was concentrated in the markets the literature identifies as most impacted by land use regulations (Glaeser and Gyourko, 2018).

6 Local Regulatory Innovation After Federal Assistance

In Section 5, we show that 701 assistance meaningfully decreased housing supply due to having persistent effects: while supply declines caused by adoption of 701 planning productions

[34]The effect in units built is similar if we shorten the summation frame to start at 1970, but use the pooled supply effect estimated only on post-1970 data. A lower bound on the unit effect, assuming away spillovers on eligible but non-adopted municipalities as of 1966, reduces the second scaling factor to 12% and the unit effect to 1.5 million units.

began in the 1970s, those declines continued to the end of and beyond the 20th Century. We found evidence that some regulations that were promoted during the 1960s, namely MLS requirements, persisted in their restrictiveness over multiple decades. However, MLS restrictiveness alone does not explain a meaningful share of 701 assistance's persistent effects. We checked the association between MLS restrictiveness and housing market outcomes with the pooled regression:

$$Y_{it} = \theta_{r(i)} + \beta \text{ MLS}^{Excess}_{i,t-10k} + X_i'\gamma + \varepsilon_{it},$$

where $\text{MLS}^{Excess}_{i,t-10k}$ marks MLS restrictiveness lagged by k decades from observation year t; we consider up to 2 lags. Appendix Table A.5 presents the results, stratified by outcomes up to 1970 and after 1970. Contemporaneous and lagged MLS restrictiveness are associated with substitution of apartment construction to single-family construction in both time period; they are associated with declines in affordability indexes used in Section 5.2 only after 1970. To take the cumulative apartment units share as an example, scaling the association of Appendix Table A.5 with the impact of 701 assistance on MLS restrictiveness results in a mediating impact of $-0.33\% \times 1.29 = -0.4\%$ percentage points, less than a sixth of the 701 ATT of 2.8%.

We instead investigate the role of 701 assistance in changing how eager local goverments were to implement novel regulatory practices, as well as if there was a focus on regulatory practices beyond ones accepted by developers and the courts in the 1960s. We consider a variety of regulatory mechanisms, not all of which are recorded in a single document such as a zoning ordinance. We therefore build up a body of evidence not just using structured data parsed from zoning ordinances, but other textual data processed using large language model (LLM) agents.

6.1 701 Assistance and Increased Zoning Code Complexity

We employ both the Bartik, Gupta and Milo (2024) AI-Zoning database discussed in Section 3.2, as well as the National Zoning and Land Use Database (NZLUD) (Mleczko and Desmond, 2023). Although underlying data availability affects how much we cover the municipalities of different MSAs, 72% of the municipalities in our analysis sample are covered in at least one of the two datasets.[35] Both databases include a set of binary outcomes that flag the presence of a particular regulation, and a set of continuous variables. Examples of the latter category include

[35]The AI-Zoning database includes 3,500 municipalities. The NZLUD spans around 2,100 municipalities, all of which were also surveyed in the Gyourko, Saiz and Summers (2008) dataset.

the total number of zoning districts in the ordinance, or moments of the MLS requirement or height limit distributions across zoning districts with the regulations.

The BGM AI-Zoning database provides an "overall index" for each municipality, which standardizes all regulatory outcomes and takes the sum. We estimate how 701 assistance impacted this measure, which we interpret as reflecting zoning code complexity (inclusive of regulations that may not impact supply much on the margin). Table 6 presents results following Equation 5, as the AI-Zoning data are only available as a cross-section. Should we interpret the regression effect as causal, a treatment of 701 assistance explains a 0.33 standard deviation increase in zoning code complexity.[36]

One concern with a cross-sectional analysis is that identification requires stronger assumptions than the parallel biases assumption discussed for Equation 2. We address the concern by running Equation 5 on 22 regulations separately encoded in our zoning regulation databases. We also separate out eight regulations that the planning literature indicates were in practice by planners in the mid-1960s. The remaining 14 regulations are known to be popularized in the 1970s and 1980s, years after 701 assistance for physical planning of small communities were phased out.

In Appendix Figure B.8, we visualize results by plotting the exposure regression coefficients β that represent effects from a one-unit rise in $\tilde{Z}$. Panel (a) plots the regulations popularized before or at the same time as 701 assistance; we find null results on regulatory adoption for all outcomes. These results imply that cities were eager to enforce the relevant regulations regardless of going through the 701 assistance process, or that amendments to these regulations due to 701 were only temporary and cannot fully explain persistent housing supply effects.

In Panel (b), we plot 14 regulations that were popularized following the 1970s, meaning they were unlikely to have been implemented during the planning assistance process for 701 adopters. We find 6 regulations with a statistically significant effect due to 701 assistance, and others with an effect significant at the 90% level. A surprising result is that there are no statistically significant effects of 701 assistance on discretionary procedures, measured by the number of non-zoning local bodies who must approve a multifamily project before a permit is granted.

In contrast, one regulation for which we find statistically significant increases is the growth control requirement, which set quotas on future permits regardless of compatibility with zoning. These regulations are emblematic of regulatory innovations promoted by motivated residents in the 1970s, in alliance with environmental conservation groups, (Frieden (1979), Fis-

[36]The specification in Column (3), applied on the analogous index of zoning complexity in the NZLUD, outputs a similar effect size of 0.37 standard deviations.

chel (2004). We also reproduce an illustrative trend first shown in Fischel (2017) in Panel (a) of Appendix Figure B.10. In the Google Ngram American English corpus, text containing keywords related to growth control regulations surged only after 1970 and persisted afterwards.

We conclude that the regulations that drove the positive effect in Table 6 are concentrated in regulations popularized after the 701 planning process. While different regions of the United States may have witnessed distinct campaigns for certain regulations, implementation of regulatory innovation seemed conditional on whether the 701 assistance process already institutuionalized a planning framework in a local government. In the case of growth controls, partial implementation at the local level, in locations Section 5.2 idenified as having elastic demand, may have relocated development to new communities that further contribute to the urban sprawl conservationists wanted to reduce (Nechyba and Walsh (2004); Ehrlich, Hilber and Schöni (2018)).

6.2 The Adoption of Value Capture Practices

A distinct set of planning practices, where new development is authorized in exchange for the developer providing local public goods, began to spread from the 1970s onwards (Manville (2021); Lebret, Liu and Valentin (2025)). In Panel (b) of Appendix Figure B.10, we confirm using the Google Ngram corpus that mentions of value capture terminology steadily grew from the late 1970s to the present. Examples include inclusionary zoning, a form of value capture intended to provide additional social housing, and traffic impact studies, which recommends how much additional transportation infrastructure should be built after residential development that could be financed through value capture.

Though value capture practices have redistributive ends, their impact depends on whether local governments can extract monopoly rents from residents[37]. If applied, value capture regulation imposes a premium on new development both in costly design changes as well as through the time value of delays (Manville et al., 2023; Gabriel and Kung, 2025). Empirical estimates of cost premia along those two channels range from 8-12% of house values (Emrath, 2021) to up to 36% of construction costs (**?**).

We test whether value capture can remain out of institutional stasis: local planning institutions after 701 assistance adopt more value capture practices over time, even if their presence decreases housing starts and imposes an excess burden on the municipality. We first detect the

[37]Recent empirical tests of the scope of this "Leviathan hypothesis" (Brennan and Buchanan, 1980) find higher tax burdens by local governments are motivated to have higher taxes when inelastic housing supply hinders how easily households can relocate to low-tax areas (Diamond, 2017).

prevalence of value capture practices based on the text of zoning ordinances. While the BGM AI-Zoning database and the NZLUD record the presence of individual regulations, they also use principal component analysis (PCA) to aggregate jointly occurring regulations into zoning indices. A higher value reflects more anomalous cases in which many restrictive regulations were passed and recorded in the zoning ordinance.

Looking at results over BGM's regulatory components in Table 6, we find a larger effect on the value capture index than what we found for regulatory complexity. Our coefficient estimate implies an ATT of 0.6 standard deviations in additional value capture practices due to 701 assistance. Coefficient estimates are robust to the addition of controls and finer fixed effects, and we also confirm our cross-sectional design finds effects of 701 assistance on the regulations loaded on to the principal component. The value capture index increases with the presence of inclusionary zoning requirements; approval of accessory dwelling units (ADUs) on the same lot of another property[38]; senior-only restrictions for units; and wetland restrictions.[39] We revisit Panel (b) of Figure B.8 and verify that adoption of every component regulation we cited is explained by 701 assistance, many of which have effects that are statistically significant at the 95% level and all of which are statistically significant at the 90% level.

6.3 Value Capture Practices: Evidence From Discretionary Permitting

A form of evidence supplemental to results from zoning ordinances would be testing whether interest groups in 701-assisted municipalities were more willing to request value capture when considering whether to permit new development. Our last piece of evidence uses documents that contain that kind of information and are available for much of the 20th Century — newspaper articles related to the permitting process for new housing development.

We build a sample of newspaper articles tracking developments that could not be built by-right, using the Newsbank Access World News database. In a process we detail further in Appendix E, we use a keyword search to find articles that describe the permitting process for a new development in which the development's unit count is specified and where either local neighbors or local planners opposed the initial proposal. We then process the articles using a large language model, prompting the generative AI interface to check whether certain features of the permitting process were described in the article body.

[38]Unlike other post-701 policies affected by 701 adoption, legalizing ADUs is a reform aimed at increasing production of a denser housing typology. However, results from Stacy et al. (2023) caution that such a reform may not increase housing supply on its own, especially if the municipality lacks undeveloped land for greenfield development.

[39]Bartik, Gupta and Milo (2024) describes the value capture component as most closely capturing the behavior examined in Diamond (2017).

After LLM processing, we produce a dataset covering 1,840 developments reported over the years 1983–2009. Appendix Figure B.11 shows that our sample includes few developments under five units, but a wide variety of project scales based on proposed unit counts: many of the profiled developments are apartment complexes on the order of up to 250 units, but a significant share are large-scale subdivisions that exceed 1,000 units. After filtering the sample to include only municipalities with between 5,000 to 200,000 residents in 1960, we use 1,062 developments for our analysis.

Among the questions considered by the LLM when processing the text data is whether the article describes the developer agreeing to public goods provision or a benefits agreement with the municipality. In Figure 7, we plot the rate at which these agreements were applied to the debated developments, but stratified on two dimensions: whether the city with the development was eligible for 701 assistance in 1960, and whether the city is in a state that is above the sample's median rate of state 701 adoption ("Adoption States") or not ("Low Adoption States"). We note that 701 eligible municipalities in the left panel, both at the start and at the end of the time frame, have marked increases in benefit agreement rates compared to 701 ineligible cities. This difference is not obviously due to differences in city size either: the right panel, which aggregates over states where 701 assistance funds were more limited, shows no persistent differences in means between eligible and ineligible municipality groups.

In the spirit of Figure 7, Table 7 shows results following the regression specification 5, reflecting our empirical strategy that uses variation in policy eligibility to estimate a pooled effect of 701 assistance. Because we work with a smaller sample at the housing development level, we replace the continuous-state adoption rate instrument $\tilde{Z}_s$ with a binary treatment indicator D_s^Z. In the same way that the panels in Figure 7 were divided, $D_s^Z = 1$ if a municipality is in a state whose 1962 adoption rate for 701 assistance was above the national median of 22%.

In the top panel, we see that the regression corresponding to the visual exercise of Figure 7 confirms the observed trends. The effect of 701 assistance, represented here by the interaction term $E_i \times D_s^Z$, on the rate of extracting community benefits from a development that needs regulatory approval, is estimated to be 15 percentage points. As this effect is large relative to the baseline rate of 25%, the result suggests that the diffusion of value capture practices was very different among planners in 701-assisted municipalities than in non-adopting municipalities.

In the second panel, we show a lack of evidence that planners in 701-assisted municipalities are more likely to express opposition to development, when opposition was already expressed

by local residents.[40] We do find, however, one way in which the characteristics of housing projects under review differed. In the last panel, we show that projects under review after 701 assistance are more likely to incorporate low-income units in the development.

6.4 Discussion

Applying our 701 eligibility design to multiple data sources on local development regulations, we find that 701-assisted municipalities were more likely to adopt regulations along two dimensions. MLS requirement bunching from as early as the 1960s shows a steady rise in the restrictiveness and complexity of exclusionary zoning, up to the adoption of quotas on new building permits by the late 1970s. We find the same rise in restrictiveness and complexity for value-capture practices, the tactic of permitting development conditional on providing public goods. We estimate a greater application of value capture in 701-assisted municipalities as early as the 1990s; by 2010, their zoning ordinances had both more restrictive regulations and a concentration of regulations that facilitated value capture.

For decades after 701 assistance, we estimate that municipalities participating in the program adopted growth management innovations that were not introduced in the 1960s. We interpret the divergence in planning practices along prior program participation as a form of what Acemoglu, Egorov and Sonin (2021) call *path-dependent change*. Our event study results suggest that the information provided by the comprehensive planning process caused supply declines 10 or 20 years later. Beyond 20 years, we find continued negative effects on supply, as well as contemporaneous evidence that zoning and the permitting process differ based on 701 eligibility. We believe that interest groups empowered by 701 assistance continued to lobby for regulations that would advance their desired path for community change.

Our analysis has one caveat: we have not collected any evidence on the growth trajectories local interest groups stated they want, in 701-assisted municipalities or elsewhere. We note two theories of how path-dependent change can occur in local government institutions empowered to conduct comprehensive planning, which could be subject to further empirical tests.

One theory is based on a *sociological* framework, in which the 701 process precipitates an ongoing process of learning about "cutting-edge" land use regulation. As noted in Appendix Figure B.10, Google Ngrams data shows a steady increase in text that discussed value capture concepts from the 1970s to 2010; the pattern is not identical to growth control concepts in Panel (a), which began to decline in relative mentions as soon as 2000. It could be that, keeping

[40] As we select developments in our sample based on whether neighbors opposed the project, we do not report regressions where neighbor opposition is the outcome — though no significant effects are found there, either.

the influence of growth-skeptical interest groups empowered by 701 assistance constant, the groups learned from each other and expanded their knowledge of value-capture techniques. In this sense, path dependence in regulations derives from an evolution of planning theory interacted with where planners are influential; it is closer to what Acemoglu, Egorov and Sonin (2021) call *intrinsic* path-dependent change.

7 Conclusion

What drove American local governments in the last 50 years to build fewer housing and consider new limits on the right to develop land? We show that both trends can be explained, in part, as the long-run consequences of a large-scale federal intervention in urban planning. For local actors in thousands of municipalities, participation in the 1960s "Section 701" Urban Planning Assistance Program subsidized their knowledge about their communities' future growth trajectory, along with the state of the art in land use regulations.

Our triple difference design exploits plausibly exogenous variation in which municipalities were eligible to receive 701 assistance. Two decades after 701 assistance, 701-assisted municipalities produce 20% fewer new housing units than the counterfactual and have implemented regulations restricting the development of multifamily or higher-density housing. The supply shortfall persists from then and into the 21st century, a period when we find suggestive evidence that 701-assisted municipalities further limit supply through passing the costs of public good provision on housing developers.

Understanding the extent to which subsidized information created path dependence in local support for regulations, as well as the coordination failures between agencies when administering the 701 assistance program, provides a broader case study on how to improve local state capacity without misaligning local incentives. In newly industrialized economies, the lack of state capacity complicates the public sector's ability to produce or maintain neighborhood amenities (Glaeser, 2020). Although urban areas in those economies could benefit from planning assistance that scaled at a similar rate as America's experiment, the historical evidence we offer points to the program's unintended consequences, in an environment where federal and state agencies offered minimal guidance on how to plan after the program scaled.

Our results also have implications for the implementation of the "pro-housing" and "abundance" agendas in the United States. To understand the mechanisms behind the persistent supply effects of 701 assistance, we demonstrated that a historic planning intervention that would have changed interest groups' perceptions of growth has had long-run effects on lo-

cal innovation in regulatory practices. Piecemeal changes in local regulatory authority do not directly target underlying growth-skeptical attitudes that motivate the adoption of new regulations. Policy alternatives could explicitly seek to change the incentives of growth skeptics, such as issuing federal development grants or having federal authorities amortize the infrastructure costs of density (Ozimek and Lettieri, 2024)

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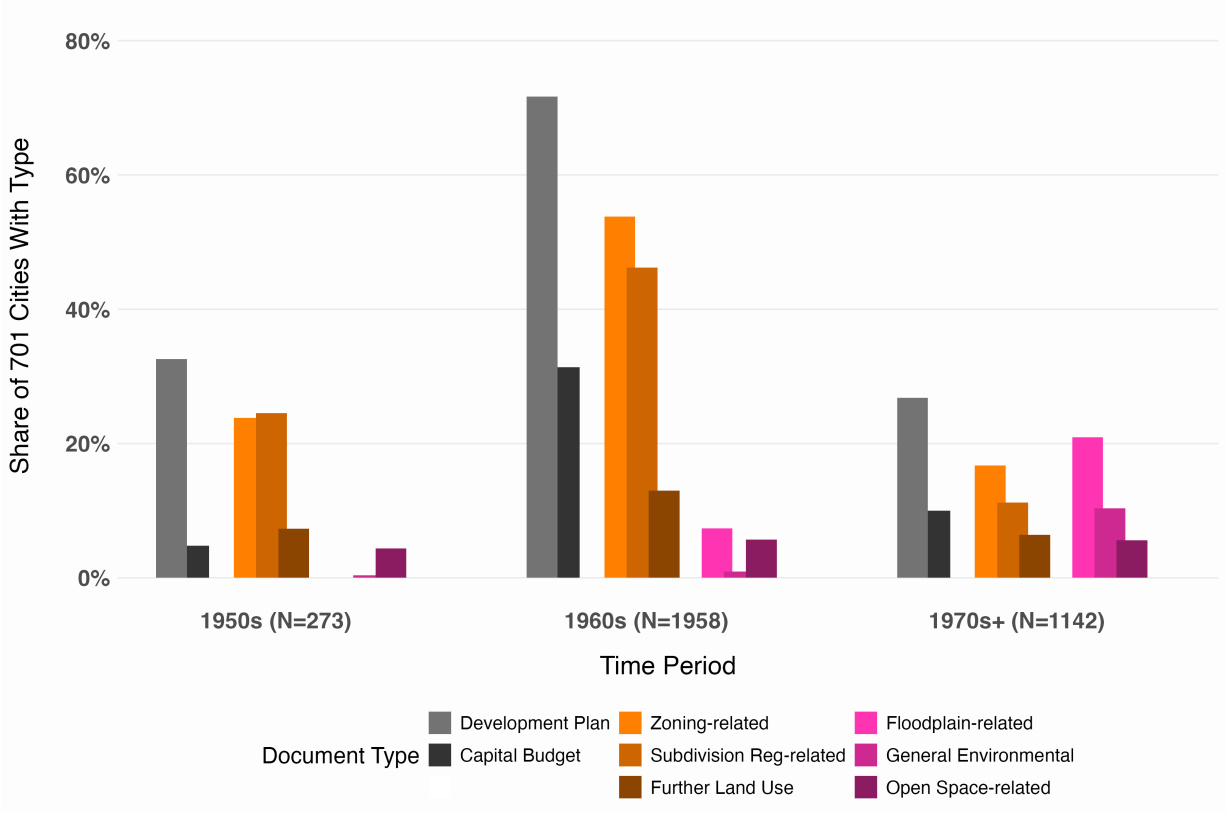
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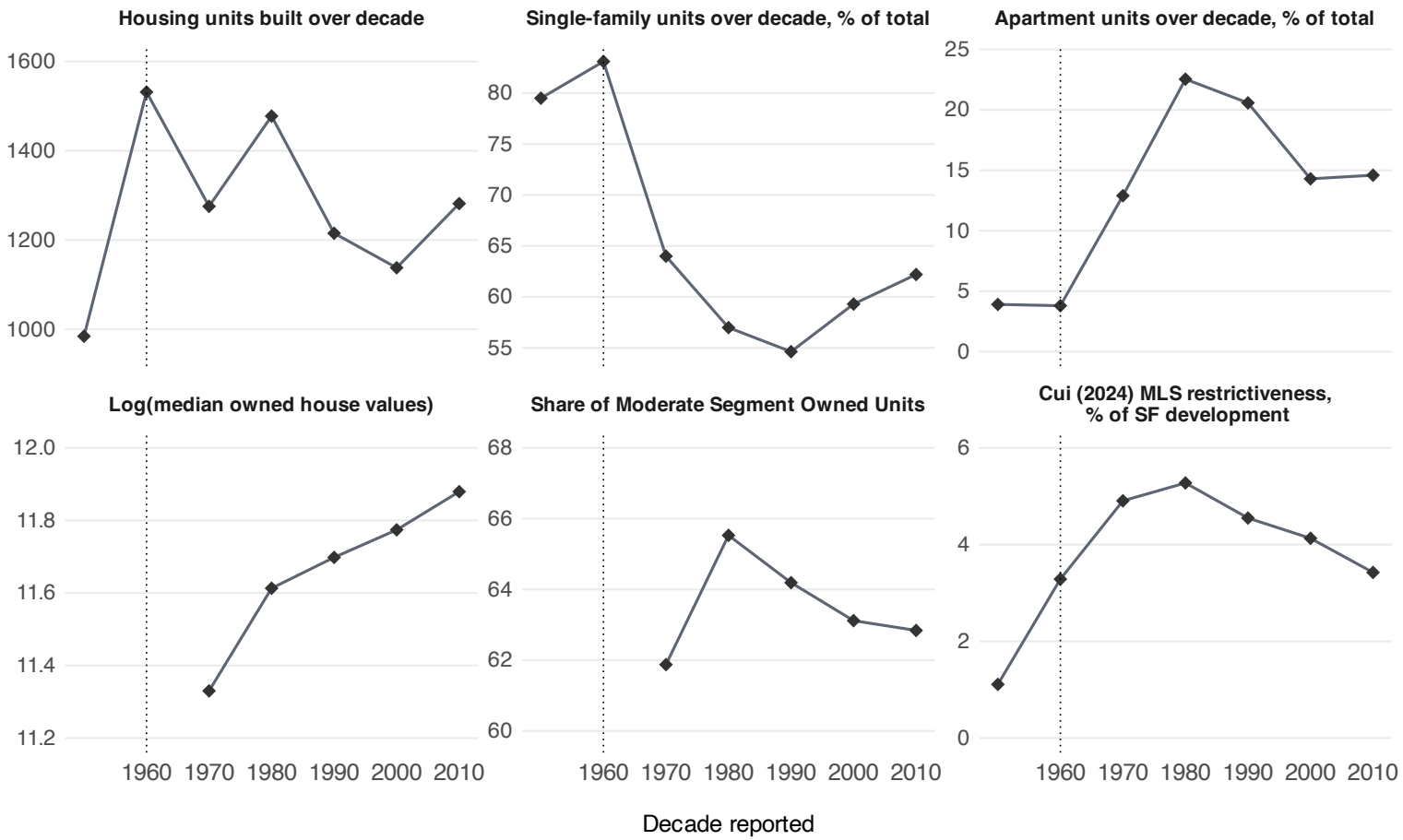
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Figure 1: 701 assistance project deliverables over time



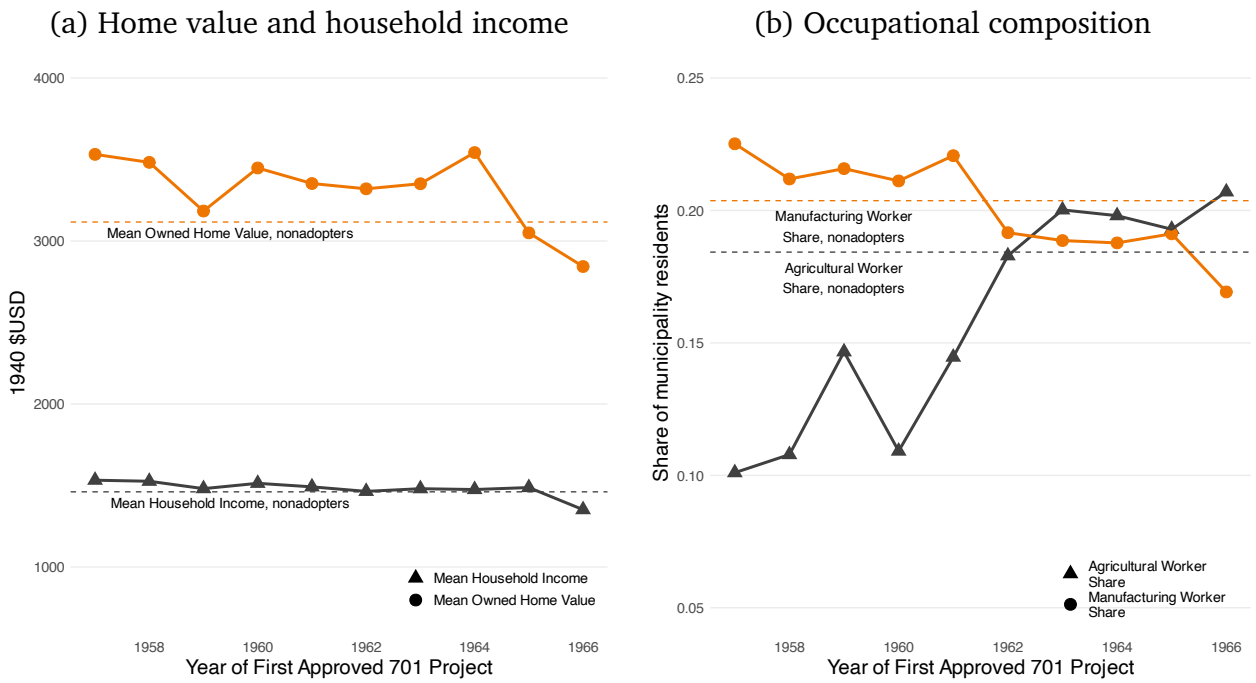
Notes: This figure tabulates the share of 701 adopter municipalities that can be linked to the HUD database of 701 assistance documents (Section 3.1) and had a document classifiable under one of eight types. We stratify documents by the decade in which they were produced and output results by three panels. For each panel, we report the number of municipalities that had at least one document over the time period. Further details on the type classification is in Appendix E
Sources: Section 701 document index, HUD/National Archives; HUD/HHFA 701 project directories.

Figure 2: Decadal trends since the postwar era for primary outcomes



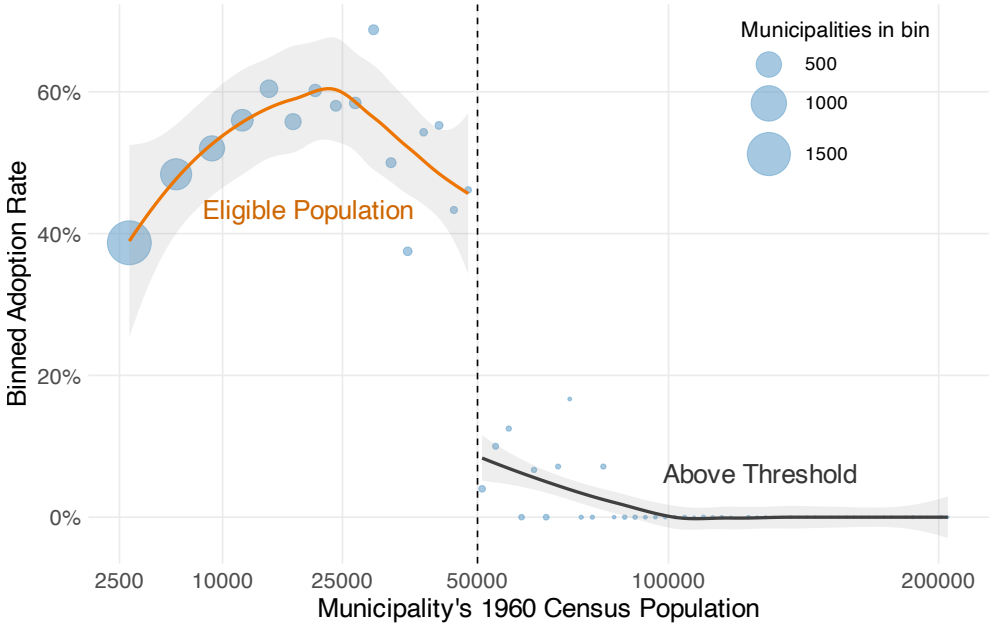
Notes: This figure plots decennial mean values for housing market outcomes we study in Section 5. Averages are equally weighed over all incorporated municipalities with available data, excluding municipalities under 1,000 population in 1960. For housing supply outcomes, "decade reported" reflects units built in the decade up to that year (e.g. the 1970 value reflects all 1960s development). Results for housing supply built prior to 1970 use unit counts for housing still standing as of 1970. As a reference, the base period year in the research design is marked with dashed lines. Further details on outcome definitions are in Section 3.2.
Sources: Decennial long-form census tables, 1970–2000; 2009–2013 ACS Tables (Schroeder et al., 2025); Cui (2024) bunching measures.

Figure 3: Prewar characteristics of 701 adopting municipalities



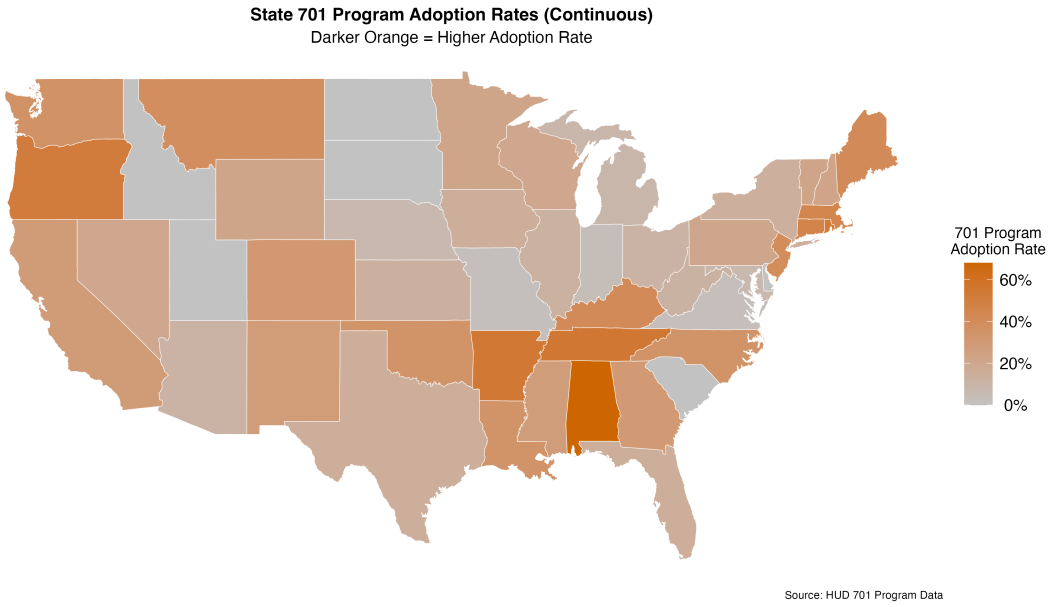
Notes: This figure reports four demographic characteristics of 701 adopting municipalities (as time series) and of non-adopters (as dotted reference lines). Averages are taken over municipalities based on if they are matched to the 701 assistance project directory (Section 3.1) and if they are have over 1,000 population in 1960. 701 adopter municipalities are binned separately based on the first year they were approved for a 701 grant. Household income data, in Panel (a), is imputed from a machine learning algorithm exploiting occupational status, developed by Saavedra and Twinam (2020).
Sources: IPUMS 1940 Full-Count Census tabulations (Ruggles et al., 2021); HUD/HHFA 701 project directories.

Figure 4: 701 program eligibility around the 1960 population cutoff



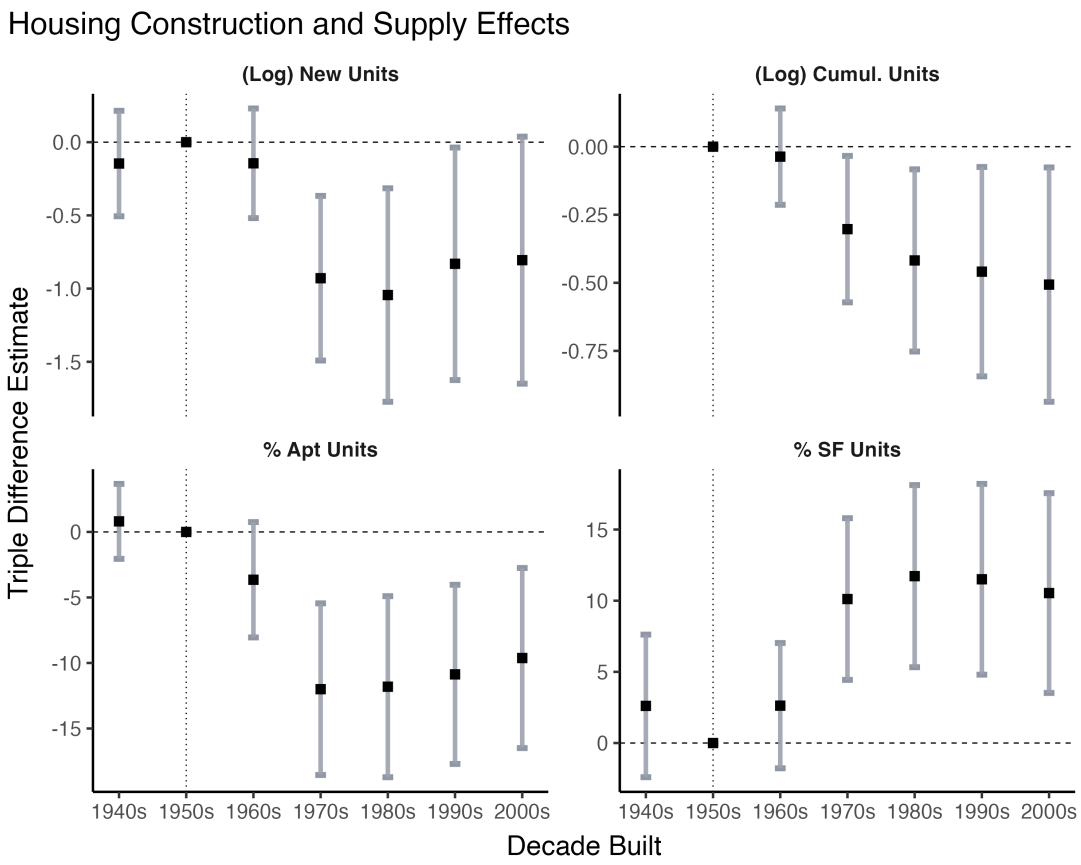
Notes: This figure plots the adoption rate of federal urban planning assistance by 1960 Census population for U.S. incorporated municipalities, defined by their presence in the "Section 701" project directories. We construct equal-width bins in 1960 population and plot each binned average as a point; the size of the point marks the number of municipalities in the bin. LOESS fits on the binned data and their 95% confidence intervals are shown separately on both sides of the 50,000 population cutoff. Further details on eligibility based on the 50,000 population cutoff are in Section 4.2.
Sources: HUD/HHFA 701 project directories; Historical US city populations panel (Schmidt, 2018).

Figure 5: State-level adoption of 701 assistance, as of 1962



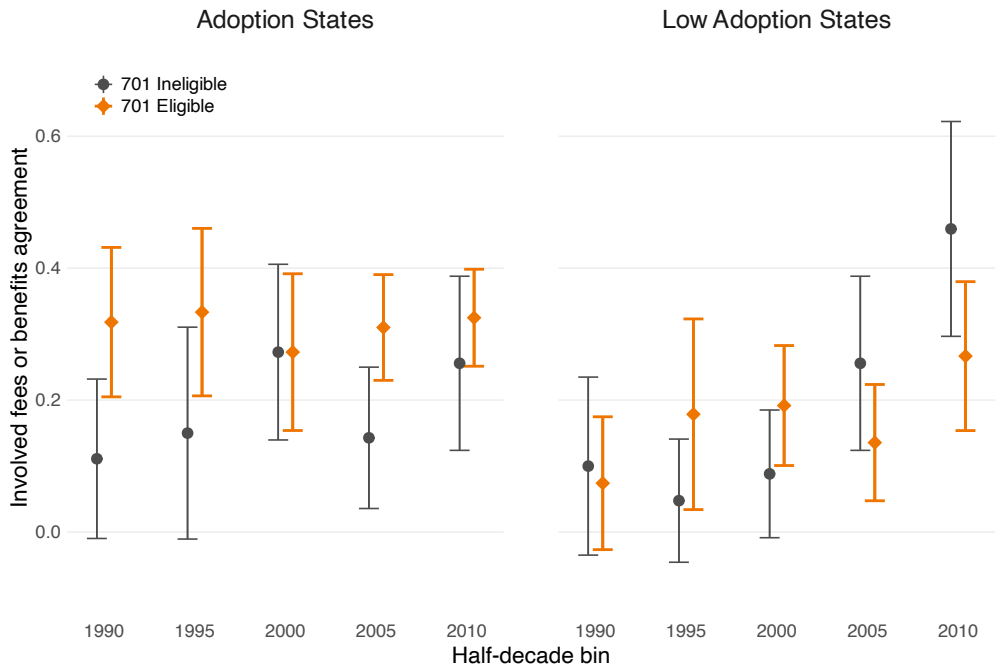
Notes: Both panels plot variation in the state adoption rate variable $\tilde{Z}_s$, which we use as a measure of 701 policy exposure. Adoption rates take incorporated jurisdictions with zoning power on development as the denominator, that also had at least 1,000 residents in 1960. The numerator sums up 701 adopters as of the end of 1962.
Sources: HUD/HHFA 701 project directories.

Figure 6: Triple difference event studies for supply outcomes



Notes: This figure plots individual coefficients from an event design version of the main triple-differences design, shown as Equation 4 in Section 5.1. The value as of each year reflects units built in the decade up to that year (e.g. the 1970 value reflects all 1960s development). The event studies apply two-way fixed effects and sets 1960 as the base period. Estimates are produced over an analysis dataset restricted to municipalities between 5,000 and 200,000 residents in the 1960 Census. 95% confidence intervals are given, with standard errors clustered at the municipality level. Further details are in Section 5.
Sources: IPUMS NHGIS tables (Schroeder et al., 2025); HUD/HHFA 701 project directories; Schmidt (2018) population panel data; and transportation access control variables from Atack (2017) and Weiwu (2024).

Figure 7: Average rates of benefits agreements in projects awaiting approval



Notes: This figure plots binned means of the rate at which developments requiring discretionary approval, covered in local newspapers, involved a proposed benefits agreement or degree of value capture by the developer. Coding of the binary outcome was done by a LLM in a process described in Section 6.2 and Appendix E. Bins were based on the first year the development was mentioned in a newspaper article, and two separate dimensions: whether the development was in a city eligible for 701 assistance in the 1960s (based on the population threshold described in Section 4.2), and if they were in a state with adoption rates above the median level for the municipality in our national sample. Each year reflects units built in the 5 years up to that year, with the exception of "1990" covering 1983–1989. 95% confidence intervals are plotted for all mean values. Further details are in Section 6.2.
Sources: Newsbank/Access World News database; HUD/HHFA 701 project directories; Schmidt (2018) population panel data.

Table 1: Covariate Balance Around Population Eligibility Threshold

Exposure Group	Coefficient (SE)	F-test
Panel (a): Testing pre-policy parallel bias		
High adoption states	-0.257 (0.063)	$F = 1.01$
Low adoption states	-0.336 (0.048)	$p = 0.314$
Panel (b): Pre-period Covariate Balance		
Median Home Value, 2010 000$	-4.483 (6.129)	0.465
Manufacturing Workers Share	0.009 (0.014)	0.499
Agriculture Workers Share	-0.013 (0.013)	0.317
College Education Share	-0.003 (0.011)	0.799
College-Educated Owners Share	-0.002 (0.007)	0.806
Total Family Income, 1950 000$	0.026 (0.059)	0.664
Foreign-Born Head Share	0.009 (0.018)	0.615
Black Head Share	-0.006 (0.016)	0.723
Distance to Central City, km	1.621 (2.717)	0.551
Muni. population growth in 1950s	-0.378 (0.255)	0.139

Notes: This table reports results from multiple tests checking covariate and outcome balance prior to the 1960s, for cities eligible and ineligible for 701 assistance. Panel (a) conducts the parallel pre-trends test employed by Lin and Peri (2025) regarding their triple differences identification assumptions. For different 1940 full-count Census outcomes, Panel (b) reports the coefficient on an interaction term between whether a municipality has fewer than 50,000 residents in 1960, as well as if they are in a state with a 701 assistance adoption rate above or below the national rate. Panel (b) thus reflects a cross-sectional version of the empirical strategy described in Section 4. All standard errors are clustered at the municipality level.
Sources: IPUMS 1940 Full-Count Census tabulations (Ruggles et al., 2021); IPUMS NHGIS tables (Schroeder et al., 2025); HUD/HHFA 701 project directories; and Schmidt (2018) population panel data.

Table 2: The Effect of 701 Assistance Eligibility on Housing Supply

	(1)	(2)	(3)	(4)	(5)
	(Log) New Units - One side restricted (≤ 200k)				
Coefficient	-0.697**	-0.711**	-0.776***	-0.802***	-0.696**
	(0.312)	(0.314)	(0.301)	(0.301)	(0.278)
Implied ATT	-0.205	-0.209	-0.229	-0.236	-0.205
Observations	16,625	15,666	15,659	15,659	15,659
R^2 (within)	0.029	0.112	0.063	0.059	0.055
	(Log) New Units - Restricted (5k–200k)				
Coefficient	-0.674**	-0.621*	-0.634**	-0.678**	-0.574**
	(0.320)	(0.321)	(0.308)	(0.308)	(0.286)
Implied ATT	-0.199	-0.183	-0.187	-0.200	-0.169
Observations	11,193	10,661	10,654	10,654	10,654
R^2 (within)	0.022	0.054	0.059	0.054	0.050
	(Log) New Units - Effects after 1970				
Coefficient	-0.800**	-0.751**	-0.765**	-0.791**	-0.674**
	(0.365)	(0.366)	(0.350)	(0.346)	(0.322)
Implied ATT	-0.236	-0.221	-0.225	-0.233	-0.199
Observations	11,193	10,661	10,654	10,654	10,654
R^2 (within)	0.025	0.057	0.062	0.058	0.055
Pop. Controls		✓	✓	✓	✓
Location Controls			✓	✓	✓
Region × Year FE				✓	
Division × Year FE					✓

Significance levels: * = 10%; ** = 5%; *** = 1%.

Notes: This table outputs the effects of 701 assistance on new units completed each decade, recorded in Census tables. Reported coefficients are from a triple-difference design with two-way fixed effects, detailed in Section 4.2, along with their rescaling into average treatment effects on the treated, following Equation 3:

$$Y_{ist} = \theta_i + \delta_t + \beta \underbrace{E_i \times \tilde{Z}_s \times \mathbf{1}[t > 1960]}_{\text{DDD term}} + \eta_1 E_i \times \mathbf{1}[t > 1960] + \eta_2 \tilde{Z}_s \times \mathbf{1}[t > 1960] + X'_{ist}\gamma^{t>1960} + \varepsilon_{ist},$$

Across columns, specifications differ in controls X_{ist}. Across panels, specifications differ in 1960 population thresholds applied to the sample or in the outcome. Standard errors are clustered at the municipality level. "Location Controls" include measures of municipal distance to various modes of transportation, detailed in Section 3.3.
Sources: IPUMS NHGIS tables (Schroeder et al., 2025); HUD/HHFA 701 project directories; Schmidt (2018) population panel data; and transportation access control variables from Atack (2017) and Weiwu (2024).

Table 3: Effects of 701 Assistance Eligibility on Housing Composition

	(1)	(2)	(3)	(4)
	% Apt Units (Cumul. to 2010) - Restricted			
Coefficient	-8.650**	-8.650**	-9.623***	-9.375***
	(3.458)	(3.458)	(3.504)	(3.428)
Implied ATT	-2.548	-2.548	-2.835	-2.762
Baseline mean			19.176	
Observations	10,730	10,730	10,730	10,730
R^2 (within)	0.059	0.059	0.044	0.038
	% SF Units (Cumul. to 2010) - Restricted			
Coefficient	9.441***	9.441***	10.535***	10.823***
	(3.560)	(3.560)	(3.578)	(3.659)
Implied ATT	2.781	2.781	3.104	3.188
Baseline mean			65.027	
Observations	10,730	10,730	10,730	10,730
R^2 (within)	0.076	0.076	0.031	0.032
Pop. Controls		✓	✓	✓
Location Controls		✓	✓	✓
Region × Year FE			✓	
Division × Year FE				✓

Significance levels: * = 10%; ** = 5%; *** = 1%.

Notes: This table outputs the effects of 701 assistance on housing composition shares, recorded in Census tables. We report the ratio of all units of a type built from 1950 to 2010, over all units built during that same time. Reported coefficients are from a triple-difference design with two-way fixed effects, detailed in Section 4.2, along with their rescaling into average treatment effects on the treated, following Equation 3:

$$Y_{ist} = \theta_i + \delta_t + \beta \underbrace{E_i \times \tilde{Z}_s \times \mathbf{1}[t > 1960]}_{\text{DDD term}} + \eta_1 E_i \times \mathbf{1}[t > 1960] + \eta_2 \tilde{Z}_s \times \mathbf{1}[t > 1960] + X'_{ist}\gamma^{t>1960} + \varepsilon_{ist},$$

Across columns, specifications differ in controls X_{ist}. Both panels use the same 1960 population threshold of 5,000 to 200,000 residents. Standard errors are clustered at the municipality level. "Location Controls" include measures of municipal distance to various modes of transportation, as detailed in Section 3.3.
Sources: IPUMS NHGIS tables (Schroeder et al., 2025); HUD/HHFA 701 project directories; Schmidt (2018) population panel data; and transportation access control variables from Atack (2017) and Weiwu (2024).

Table 4: Effects of 701 Assistance Eligibility on Prices

	(1)	(2)	(3)	(4)
	(Log) Med. Home Values - Restricted			
Coefficient	-0.010	0.216	0.290**	0.344***
	(0.143)	(0.131)	(0.126)	(0.115)
Implied ATT	-0.003	0.064	0.085	0.101
Observations	10,210	9,746	9,746	9,746
R^2 (within)	0.043	0.150	0.080	0.080
	Share of Moderate Segment Owned Units - Restricted			
Coefficient	-10.075	-14.362**	-16.554**	-16.516**
	(7.501)	(7.252)	(7.433)	(7.438)
Implied ATT	-2.968	-4.231	-4.877	-4.866
Baseline mean		56.322		
Observations	9,316	8,906	8,906	8,906
R^2 (within)	0.034	0.082	0.073	0.078
Pop. Controls		✓	✓	✓
Location Controls		✓	✓	✓
Region × Year FE			✓	
Division × Year FE				✓

Significance levels: * = 10%; ** = 5%; *** = 1%.

Notes: This table outputs the effects of 701 assistance on home values for owner-occupied homes, recorded in Census tables and detailed in Section 3.2. Reported coefficients are from a triple-difference design with two-way fixed effects, detailed in Section 4.2, along with their rescaling into average treatment effects on the treated, following Equation 3. Due to data limitations, this Table's results uses only 1940 levels, tabulated from the full-count Census, as pre-period data when estimating triple-difference effects.
Both panels use the same 1960 population threshold of 5,000 to 200,000 residents. Standard errors are clustered at the municipality level. "Location Controls" include measures of municipal distance to various modes of transportation, as detailed in Section 3.3.
Sources: IPUMS NHGIS tables (Schroeder et al., 2025); HUD/HHFA 701 project directories; Schmidt (2018) population panel data; and transportation access control variables from Atack (2017) and Weiwu (2024).

Table 5: Effects of 701 Assistance Eligibility on Density Regulation Restrictiveness

	(1)	(2)	(3)	(4)
	MLS Requirement Restrictiveness - Restricted			
Coefficient	6.919***	4.348**	4.366**	5.631***
	(1.988)	(1.977)	(1.956)	(2.047)
Implied ATT	2.039	1.281	1.286	1.659
Baseline mean			4.584	
Observations	12,757	12,145	12,145	12,145
R^2 (within)	0.019	0.040	0.024	0.008
	Dissimiliarity Index for Apartments - Restricted			
Coefficient	0.088*	0.115**	0.116**	0.115**
	(0.045)	(0.045)	(0.045)	(0.045)
Implied ATT	0.026	0.034	0.034	0.034
Baseline mean			0.560	
Observations	1,576	1,501	1,501	1,501
R^2 (within)	0.019	0.080	0.064	0.052
Pop. Controls		✓	✓	✓
Location Controls		✓	✓	✓
Region × Year FE			✓	
Division × Year FE				✓

Significance levels: * = 10%; ** = 5%; *** = 1%.

Notes: This table outputs the effects of 701 assistance on two outcomes: the panel data on lot size restrictiveness detailed in Section 3.2 and cross-sectional data on how segregated apartments with 5 or more units in the building are from less dense housing typologies, based on small area tabulations in the 1990 Census. For the former outcome, reported coefficients are from a triple-difference design with two-way fixed effects, detailed in Section 4.2, along with their rescaling into average treatment effects on the treated, following Equation 3. For the latter, we estimate the cross-sectional equation:

$$Y_{is} = \theta_{r(i)} + \underbrace{\beta E_i \times \tilde{Z}_s}_{\text{Coefficient}} + \eta_1 E_i + \eta_2 \tilde{Z}_s + X_{is}'\gamma + \varepsilon_{is}.$$

Across columns, specifications differ in controls X_{ist}. Both panels use the same 1960 population threshold of 5,000 to 200,000 residents. Standard errors are clustered at the municipality level. "Location Controls" include measures of municipal distance to various modes of transportation, as detailed in Section 3.3.
Sources: IPUMS NHGIS tables (Schroeder et al., 2025); HUD/HHFA 701 project directories; Schmidt (2018) population panel data; and transportation access control variables from Atack (2017) and Weiwu (2024).

Table 6: Path Dependence in Regulations in 701 Assistance Eligible Municipalities

	(1)	(2)	(3)	(4)
	BGM Overall Index - Restricted			
Coefficient	1.600***	1.233***	1.136***	1.195***
	(0.402)	(0.381)	(0.397)	(0.408)
Implied ATT	0.471	0.363	0.335	0.352
Observations	1,882	1,771	1,771	1,771
R^2 (within)	0.099	0.219	0.067	0.058
	BGM Value Capture Component - Restricted			
Coefficient	2.377***	1.843***	1.720***	1.750***
	(0.436)	(0.464)	(0.472)	(0.482)
Implied ATT	0.700	0.543	0.507	0.516
Observations	1,882	1,771	1,771	1,771
R^2 (within)	0.121	0.206	0.087	0.046
Pop. Controls		✓	✓	✓
Location Controls		✓	✓	✓
Region × Year FE			✓	
Division × Year FE				✓

Significance levels: * = 10%; ** = 5%; *** = 1%.

Notes: This table outputs the effects of 701 assistance on cross-sectional indices of zoning ordinance restrictiveness, constructed in Bartik, Gupta and Milo (2024) and detailed in Section 6. For municipalities with available data, we estimate the cross-sectional equation:

$$Y_{is} = \theta_{r(i)} + \underbrace{\beta E_i \times \tilde{Z}_s}_{\text{Coefficient}} + \eta_1 E_i + \eta_2 \tilde{Z}_s + X'_{is}\gamma + \varepsilon_{is}.$$

Outcomes are standardized, so effects should be interpreted in terms of standard deviations. Coefficients are rescaled into average treatment effects on the treated, following Equation 3. All panels use the same 1960 population threshold of 5,000 to 200,000 residents. Standard errors are clustered at the municipality level. "Location Controls" include measures of municipal distance to various modes of transportation, as detailed in Section 3.3. *Sources:* AI-Zoning database (Bartik, Gupta and Milo, 2024); HUD/HHFA 701 project directories; Schmidt (2018) population panel data; and transportation access control variables from Atack (2017) and Weiwu (2024).

Table 7: 701 Assistance Eligibility and Processes For Opposed Development

	(1)	(2)	(3)	(4)
	Involved Fees or Benefits Agreement - Restricted			
Coefficient	0.149***	0.150***	0.158***	0.129**
	(0.055)	(0.057)	(0.058)	(0.060)
Baseline mean			0.246	
Observations	1,062	1,017	1,017	1,017
R^2 (within)	0.012	0.025	0.022	0.024
	Opposed By Local Planners - Restricted			
Coefficient	0.010	-0.026	-0.002	0.006
	(0.058)	(0.060)	(0.062)	(0.062)
Baseline mean			0.181	
Observations	1,062	1,017	1,017	1,017
R^2 (within)	-0.001	0.012	0.008	0.007
Pop. Controls		✓	✓	✓
Location Controls		✓	✓	✓
Region × Year FE			✓	
Division × Year FE				✓

Significance levels: * = 10%; ** = 5%; *** = 1%.

Notes: This table outputs effects from a regression across different development projects that faced obstruction in receiving permits, based on newspaper reports from 1983–2010 further detailed in Section 6. We estimate the linear probability model:

$$Y_{ist} = \theta_{r(i)t} + \underbrace{\beta E_i \times D_s^Z}_{\text{Coefficient}} + \eta_1 E_i + \eta_2 D_s^Z + X_{is}'\gamma + \varepsilon_{is},$$

where D_s^Z is a binary treatment equaling 1 if state-level 701 adoption rates $\tilde{Z}_s$ is above the national rate. All panels are restricted to municipalities with 1960 populations between 5,000 to 200,000. Standard errors are clustered at the municipality level. "Location Controls" include measures of municipal distance to various modes of transportation, as detailed in Section 3.3.

Sources: Newsbank/Access World News database; HUD/HHFA 701 project directories; Schmidt (2018) population panel data; and transportation access control variables from Atack (2017) and Weiwu (2024).

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A Appendix Tables

Table A.1: Construction of 701 Adopter and Main Analysis Samples

Sample Restriction	Total Munis	701 Adopters	Adoption Rate
All zoning jurisdictions in CBSAs	20,956	3,093	14.8%
Municipalities with historical population	15,222	2,968	19.5%
+ between [1000, 50000] 1960 residents	7,441	2,579	34.7%
Balanced panel of municipalities since 1970	2,561	1,264	49.4%
+ distance to CBD filter	2,153	1,081	50.2%

Notes: This table overviews the data processing steps we take to process the universe of U.S. zoning jurisdictions into the samples we use for 701 assistance statistics and regression analysis. We report the number of adopters at each successive sample restriction, based on if they are matched in our 701 project directories, as well as total municipalities. The balanced panel of municipalities includes municipalities above 50,000 residents that are ineligible for 701 assistance.

Table A.2: Overall covariate means between adopters and non-adopters

	Mean Values			
Variable	Treated	Control	Difference	P-value
Distance to metro CBD (km)	29305.633	28987.201	318.432	0.624
Population Growth Rate 1950-1960	0.537	0.479	0.058	0.115
Highway Intersects Municipality	0.318	0.201	0.116***	0.000
Distance to Highway (km)	17.396	19.206	-1.810**	0.023
Number of Households (1940)	1815.153	1273.490	541.663***	0.000
Total Income per Household (1940 $)	1516.372	1461.267	55.105***	0.000
Average Home Value (1940 $)	3445.462	3116.748	328.713***	0.000
College Graduate Share	0.093	0.084	0.009***	0.000
Blue Collar Worker Share	0.442	0.427	0.016***	0.000
Agricultural Worker Share	0.140	0.184	-0.044***	0.000
Manufacturing Worker Share	0.209	0.204	0.005*	0.068

Significance levels: $^{*} = 10\%$; $^{**} = 5\%$; $^{***} = 1\%$.

Notes: Following the tabulation of 1940 covariates as described in Section 4.1, this table compares means tabulated based on 701 adoption status, as recorded in the HUD/HHFA project directories. Municipalities analyzed have 1960 Census populations of at least 1,000 and at most 50,000; ineligible municipalities are excluded in this Table. Units of the variable are reported in the leftmost column. All covariates tested in this table are also used in the covariate balance tests of Table 1.

Table A.3: Primary outcome results using 701 adoption treatment

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcomes - One side restricted ($\leq 200K$)					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Coefficient	0.153***	0.850**	-1.137**	0.087***	-2.926***	1.524***
	(0.038)	(0.367)	(0.451)	(0.019)	(1.044)	(0.266)
Observations	15,659	15,659	15,659	13,993	13,056	17,806
R^2 (within)	0.038	0.050	0.025	0.028	0.050	0.016
	Outcomes - Restricted ($5K - 200K$)					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Coefficient	0.263***	0.312	-0.697	0.112***	-5.432***	1.577***
	(0.043)	(0.435)	(0.485)	(0.021)	(1.173)	(0.314)
Observations	10,654	10,654	10,654	9,746	8,906	12,145
R^2 (within)	0.046	0.037	0.023	0.036	0.055	0.019

Significance levels: * = 10%; ** = 5%; *** = 1%.

Table A.4: Primary outcome results accounting for heterogeneous spillover effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcomes - Restricted ($5K - 200K$)					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Panel A: Coefficients for adopted and for eligible non-adoptees						
β_{adopt}	-0.649**	-12.898***	10.617***	0.342**	-12.996	4.929**
	(0.326)	(3.669)	(3.781)	(0.143)	(7.982)	(2.218)
β_{elig}	-1.113***	-6.617	11.702***	0.215	-22.389**	1.919
	(0.374)	(4.140)	(4.372)	(0.153)	(9.333)	(2.608)
Panel B: Comparison of identified treatment effects on the treated						
Original τ_{treat}	-0.200	-2.818	2.928	0.085	-4.877	1.286
Het. τ_{treat}	-0.182	-3.614	2.975	0.096	-3.642	1.381
Combined τ_{elig}	-0.204	-2.683	2.765	0.075	-4.102	0.971
Observations	10,654	10,654	10,654	9,746	8,906	12,145
R^2 (het. model)	0.057	0.050	0.031	0.080	0.074	0.025

Significance levels: * = 10%; ** = 5%; *** = 1%.

Table A.5: Persistent associations of MLS restrictiveness with market outcomes

Outcome		Pre-1960s Variation		Post-1960s Variation		
		1 Lag	2 Lags	0 Lags	1 Lag	2 Lags
Excess Land	Coefficient	0.555**	0.390**	—	0.665***	0.497***
		(0.191)	(0.153)		(0.028)	(0.045)
	R^2 (within)	0.229	0.104		0.531	0.370
% Apartments	Coefficient	-0.220**	-0.234**	-0.338***	-0.273***	-0.187***
		(0.076)	(0.089)	(0.030)	(0.034)	(0.038)
	R^2 (within)	0.010	0.006	0.029	0.020	0.012
% Single Family	Coefficient	0.466**	0.430**	0.593***	0.517***	0.439***
		(0.175)	(0.131)	(0.049)	(0.059)	(0.072)
	R^2 (within)	0.032	0.022	0.071	0.059	0.046
Log(Price)	Coefficient	0.000	0.002	0.008***	0.009***	0.010***
		(0.004)	(0.003)	(0.001)	(0.001)	(0.002)
	R^2 (within)	0.012	0.020	0.041	0.045	0.050
Moderate Segment Share	Coefficient	-0.121	-0.056	-0.297***	-0.338***	-0.479***
		(0.250)	(0.192)	(0.052)	(0.051)	(0.046)
	R^2 (within)	0.010	0.009	0.017	0.020	0.037
Bottom Segment Share	Coefficient	-0.131	-0.081	-0.263***	-0.285***	-0.395***
		(0.198)	(0.123)	(0.047)	(0.058)	(0.042)
	R^2 (within)	0.028	0.022	0.031	0.030	0.047
Not Cost Burdened Renters	Coefficient	0.050	0.012	-0.215***	-0.234***	-0.244***
		(0.237)	(0.193)	(0.045)	(0.059)	(0.054)
	R^2 (within)	0.051	0.048	0.046	0.038	0.035
Ownership rates	Coefficient	0.280**	0.271***	0.341***	0.351***	0.367***
		(0.099)	(0.065)	(0.039)	(0.040)	(0.048)
	R^2 (within)	0.048	0.042	0.069	0.074	0.084

Significance levels: * = 10%; ** = 5%; *** = 1%.

Table A.6: Table 2 results under different bandwidths

	(1)	(2)	(3)	(4)	(5)
	(Log) New Units - Full sample				
Coefficient	-0.772***	-0.658**	-0.822***	-0.838***	-0.810***
	(0.280)	(0.286)	(0.271)	(0.270)	(0.252)
Implied ATT	-0.228	-0.194	-0.242	-0.247	-0.239
Observations	17,024	16,065	16,058	16,058	16,058
R^2 (within)	0.035	0.112	0.069	0.065	0.062
	(Log) New Units - More Restricted (5k–150k)				
Coefficient	-0.658*	-0.598*	-0.593*	-0.635*	-0.565*
	(0.344)	(0.350)	(0.332)	(0.332)	(0.307)
Implied ATT	-0.194	-0.176	-0.175	-0.187	-0.166
Observations	11,067	10,535	10,528	10,528	10,528
R^2 (within)	0.020	0.052	0.056	0.051	0.047
	(Log) New Units - Donut hole around 50k				
Coefficient	-0.656**	-0.668**	-0.649**	-0.724**	-0.646**
	(0.332)	(0.324)	(0.317)	(0.319)	(0.298)
Implied ATT	-0.193	-0.197	-0.191	-0.213	-0.190
Observations	10,248	9,751	9,744	9,744	9,744
R^2 (within)	0.018	0.053	0.052	0.047	0.044
Pop. Controls		✓	✓	✓	✓
Location Controls			✓	✓	✓
Region × Year FE				✓	
Division × Year FE					✓

Significance levels: * = 10%; ** = 5%; *** = 1%.

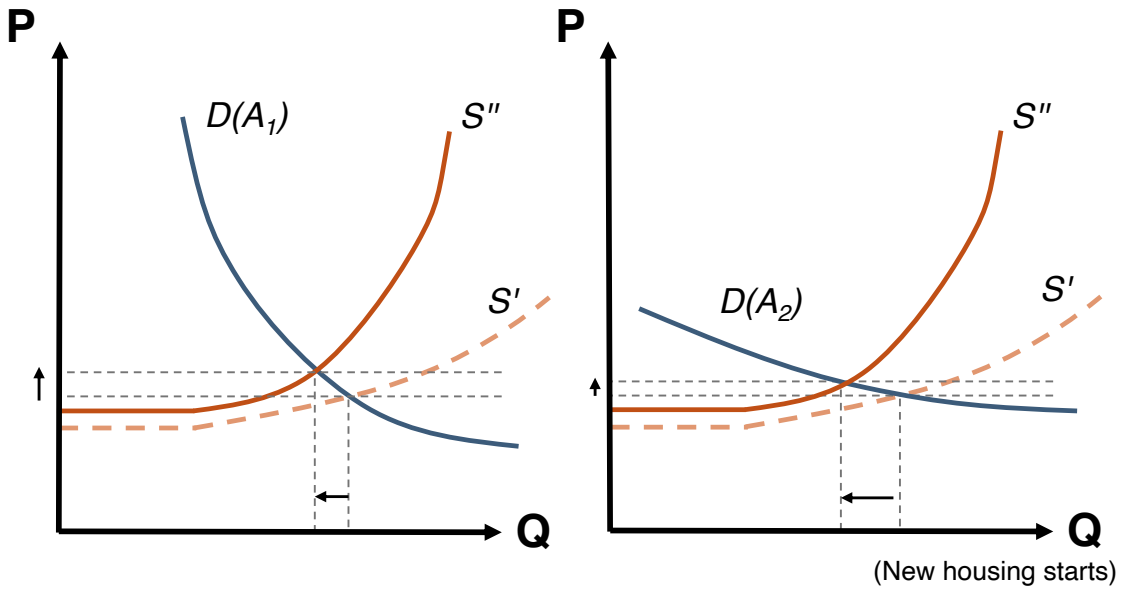
Table A.7: Primary outcome results over placebo eligibility threshold

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcomes - Restricted w/ 20K threshold					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Coefficient	-0.107	-0.633	2.934	0.133	-4.400	0.181
	(0.297)	(3.064)	(3.488)	(0.144)	(7.292)	(2.364)
Observations	9,121	9,121	9,121	8,080	7,603	10,393
R^2 (within)	0.059	0.066	0.034	0.081	0.083	0.026

Significance levels: * = 10%; ** = 5%; *** = 1%.

B Appendix Exhibits

Figure B.1: Housing market outcomes after local growth management



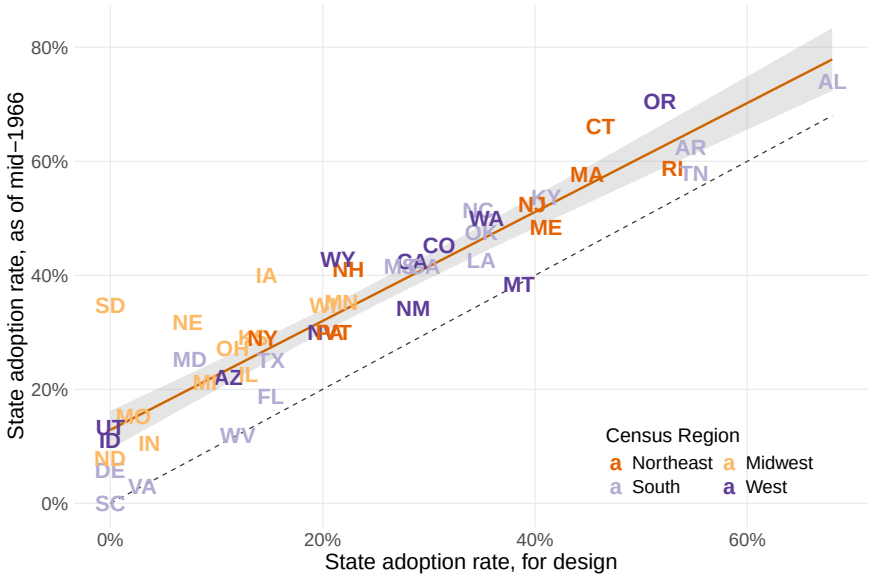
Notes: This figure plots two ways market outcomes can change following regulations that manage continued supply of a specific housing segment in a municipality, based on the analysis of 701 planning assistance consequences in Section 2.3.

Figure B.2: The scaling up of 701 grants and expenditures

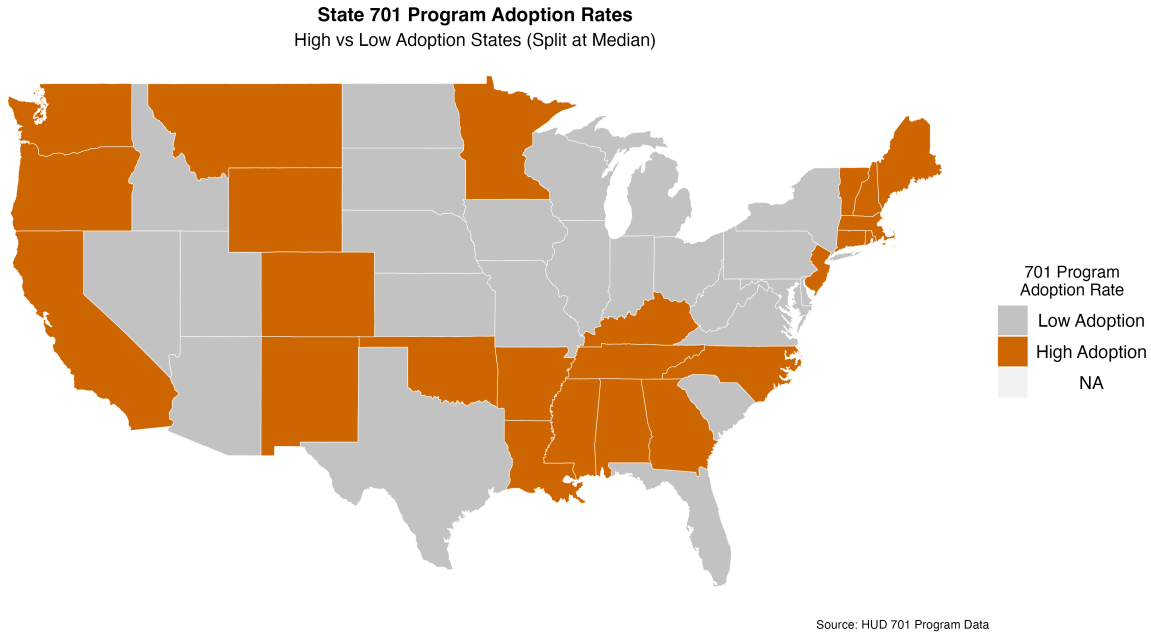


Figure B.3: Further state-level trends in adoption rates

(a) Scatterplot of adoption variation over time



(b) Two tiers of state-level 701 adoption rates, as of 1962



Notes: Both panels plot variation in the state adoption rate variable $\tilde{Z}_s$, which we use as a measure of 701 policy exposure. Panel (a) separates the states at the threshold of $\tilde{Z}_s = 23\%$, which is the median value of state adoption rates computed over the municipalities in our national sample. In Panel (b), we plot the correlation between adoption rate constructions defined as of 1962 and as of the latest project directory available in 1966.

Figure B.4: Triple difference event studies for non-supply outcomes

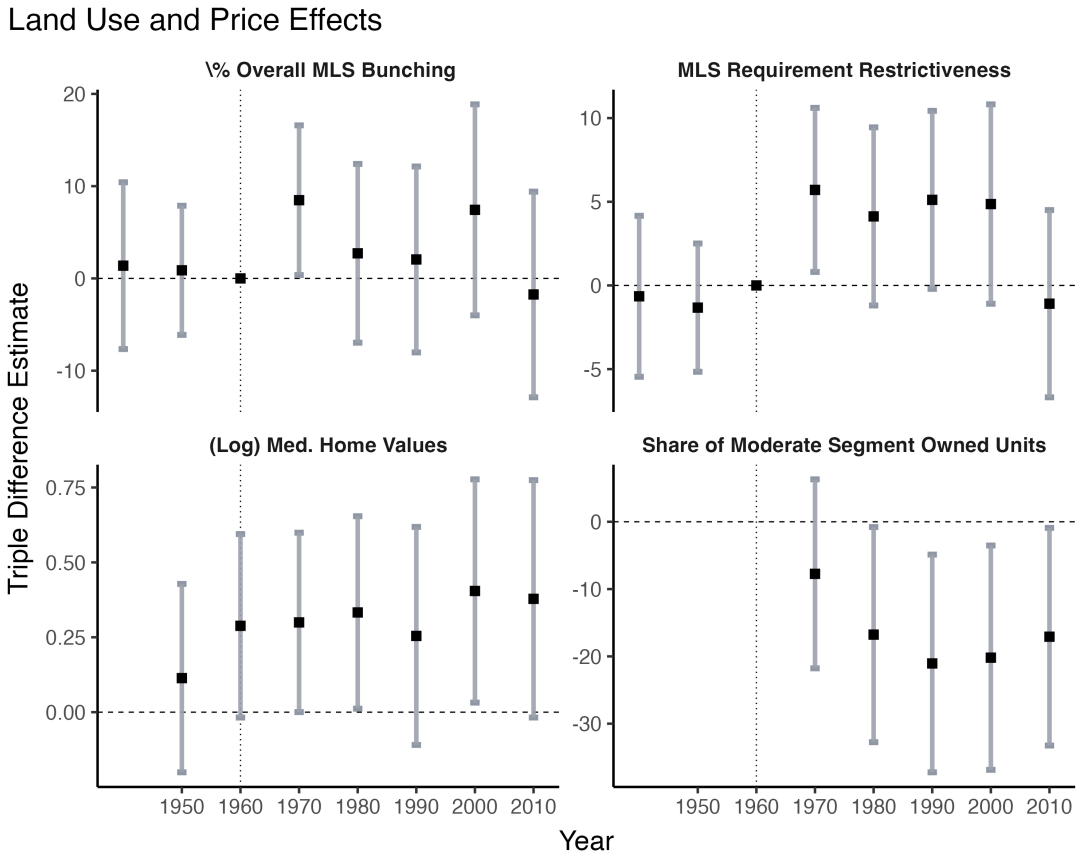


Figure B.5: Triple difference event studies for affordability across market segments

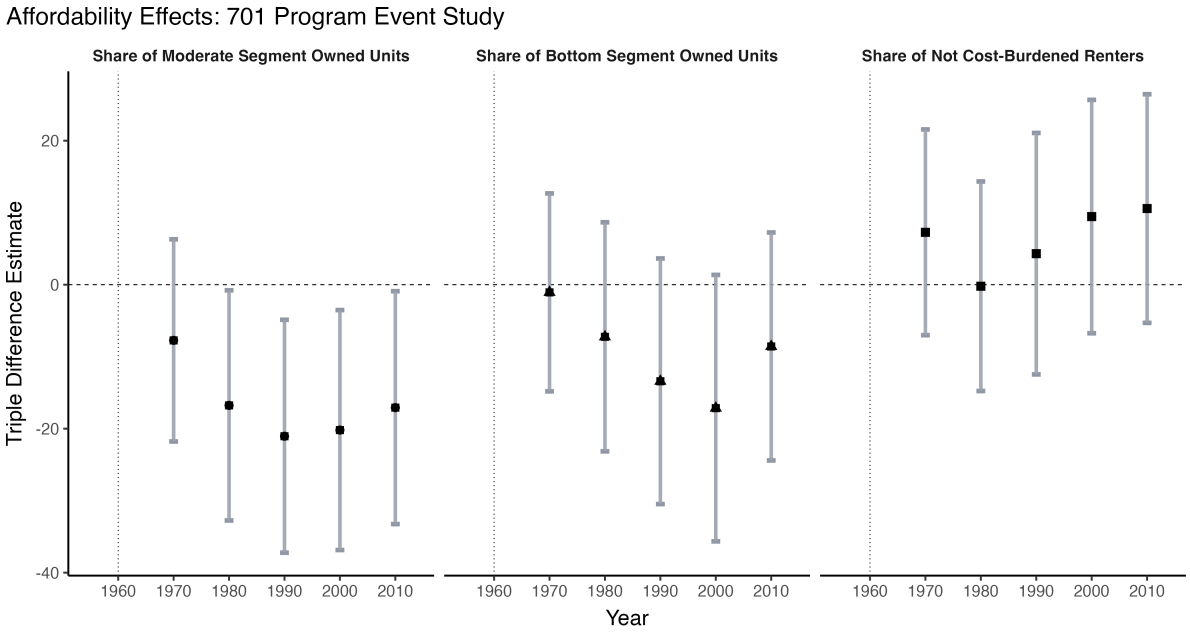


Figure B.6: Triple difference event studies for median income outcomes

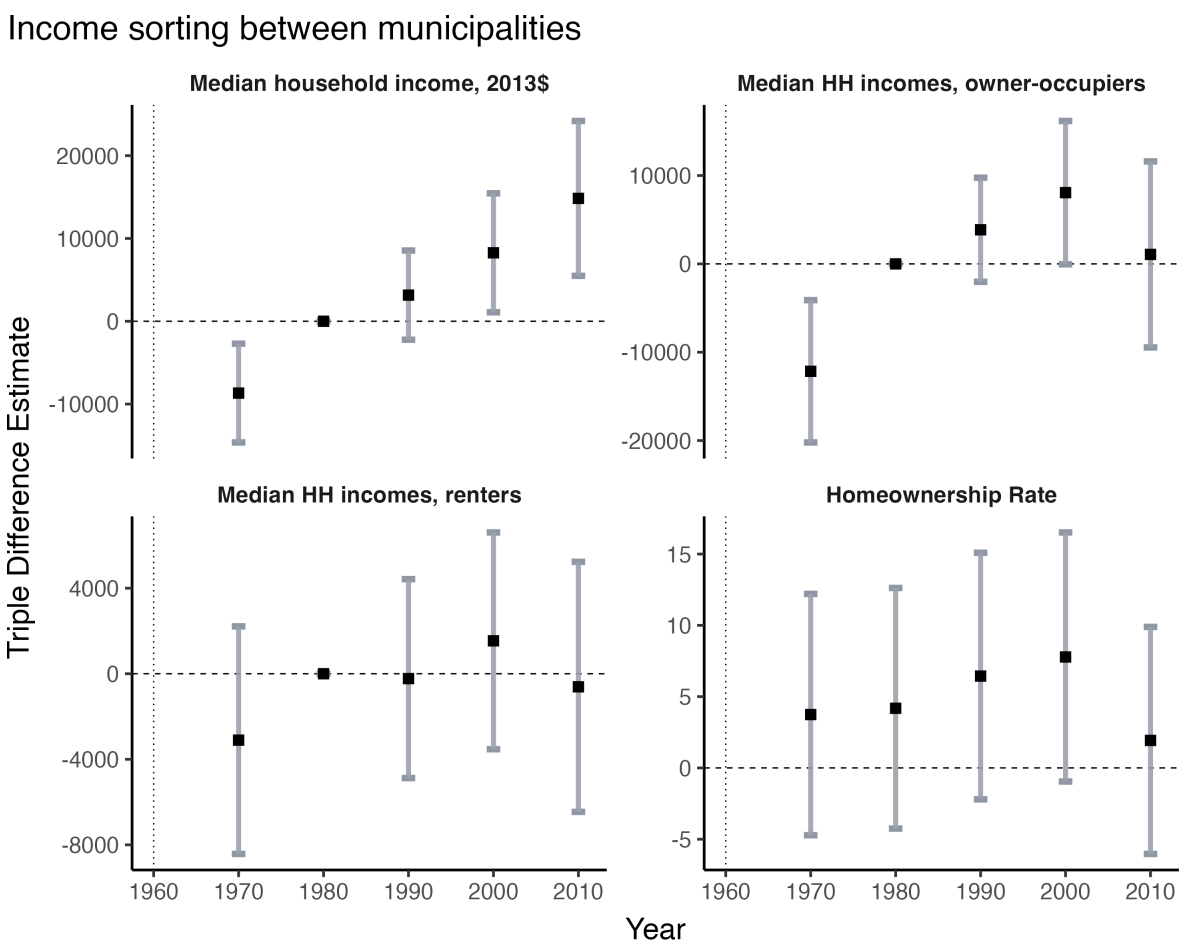


Figure B.7: Heterogeneity in supply & affordability ATTs following 701 assistance

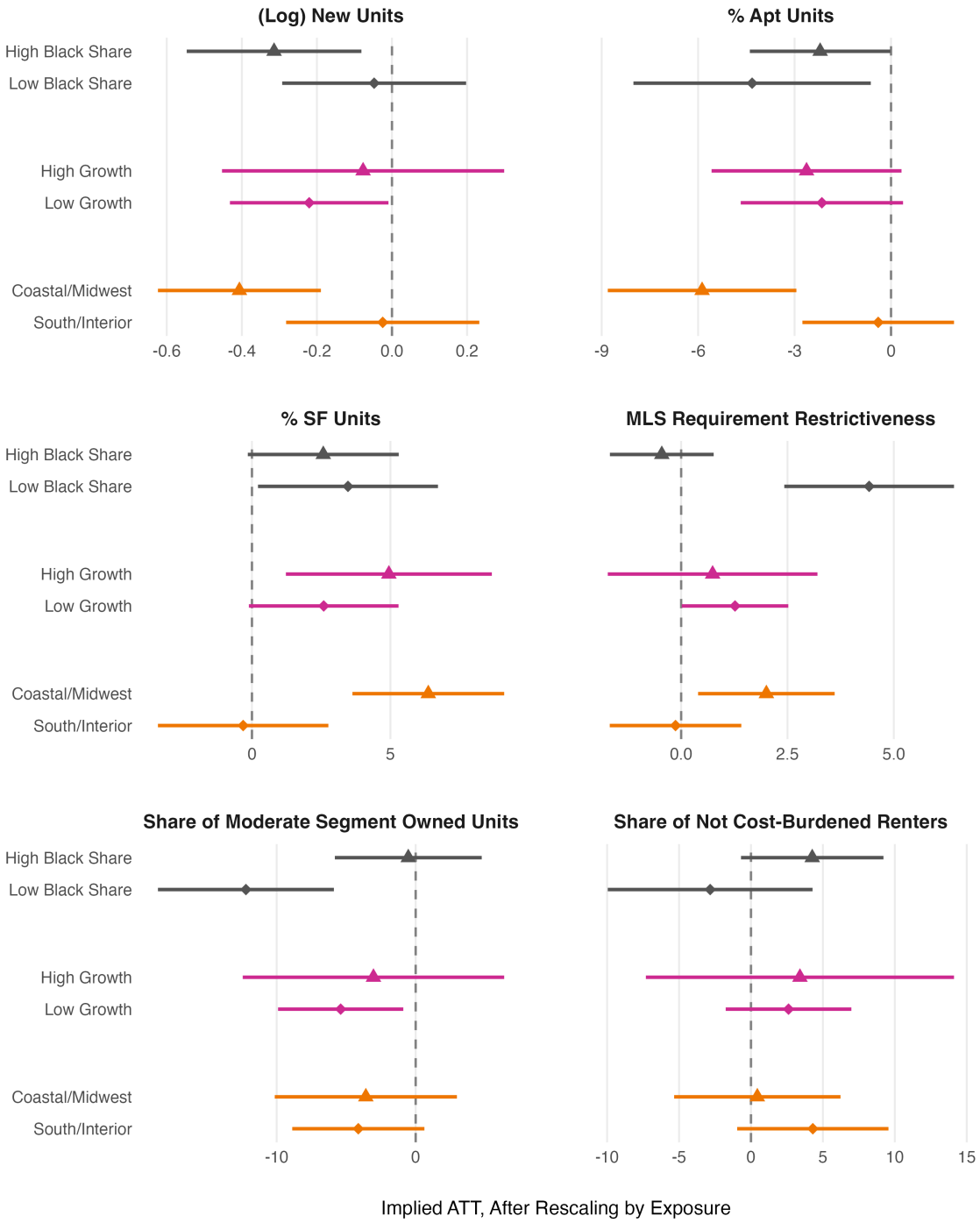
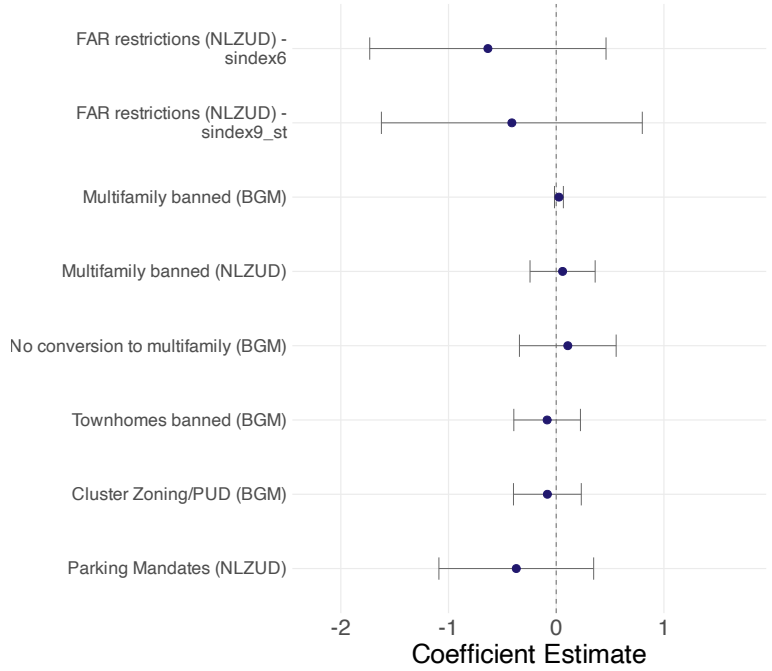
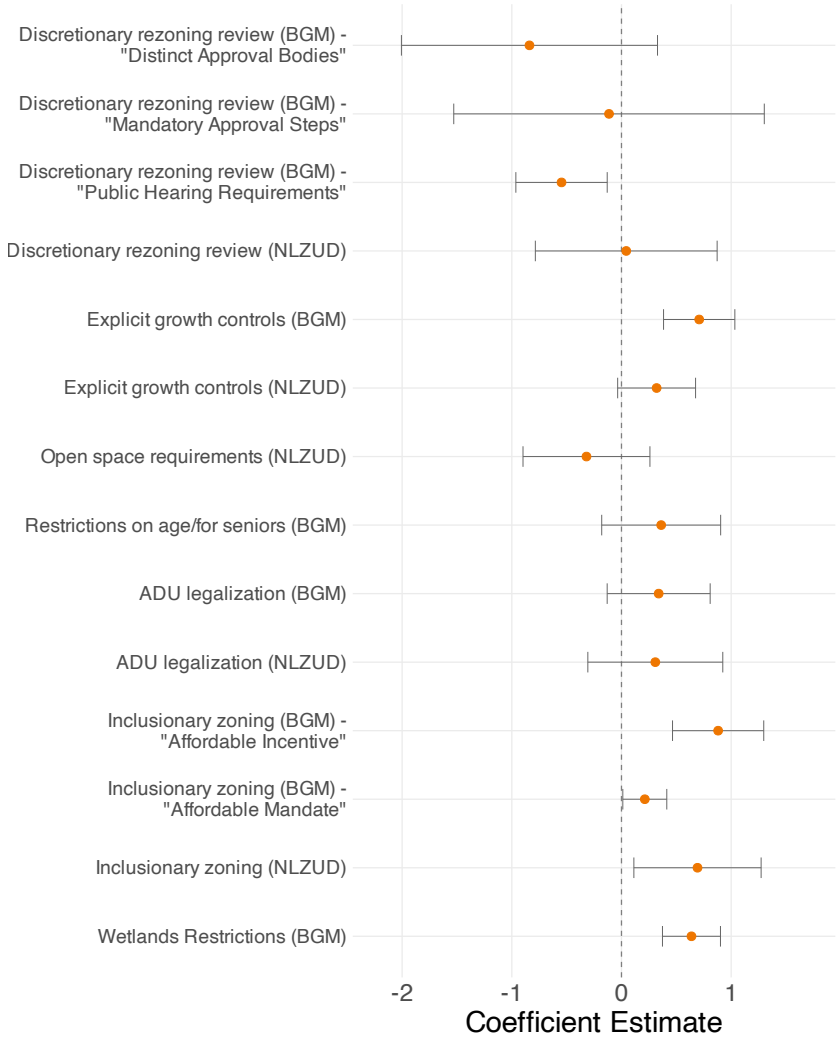


Figure B.8: Path Dependence In Specific Growth Controls and Land Use Regulations

(a) Changes to regulations generally adopted pre-701



(b) Regulations generally adopted post-701



Notes: This figure plots binned means of the rate at which developments requiring discretionary approval, covered in local newspapers, involved a proposed benefits agreement or degree of value capture by the developer. Coding of the binary outcome was done by a LLM in a process described in Section 6.2 and Appendix E. Bins were based on the first year the development was mentioned in a newspaper article, and two separate dimensions: whether the development was in a city eligible for 701 assistance in the 1960s (based on the population threshold described in Section 4.2), and if they were in a state with adoption rates above the median level for the municipality in our national sample. Each year reflects units built in the 5 years up to that year, with the exception of "1990" covering 1983–1989. 95% confidence intervals are plotted for all mean values. Further details are in Section 6.2.
Sources: Newsbank/Access World News database; HUD/HHFA 701 project directories; Schmidt (2018) population panel data.

Figure B.9: Consistency of MLS restrictiveness results across measures

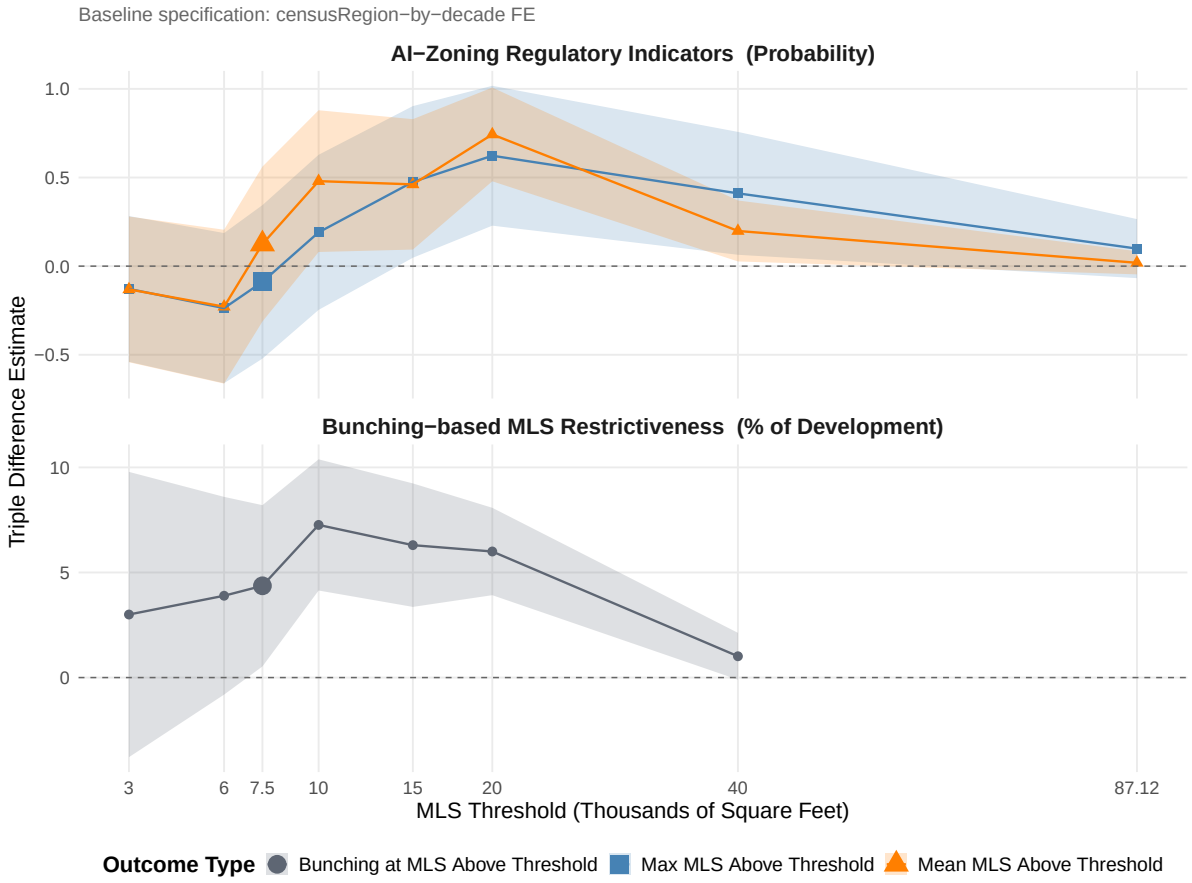


Figure B.10: Google Ngram analysis of planning practices

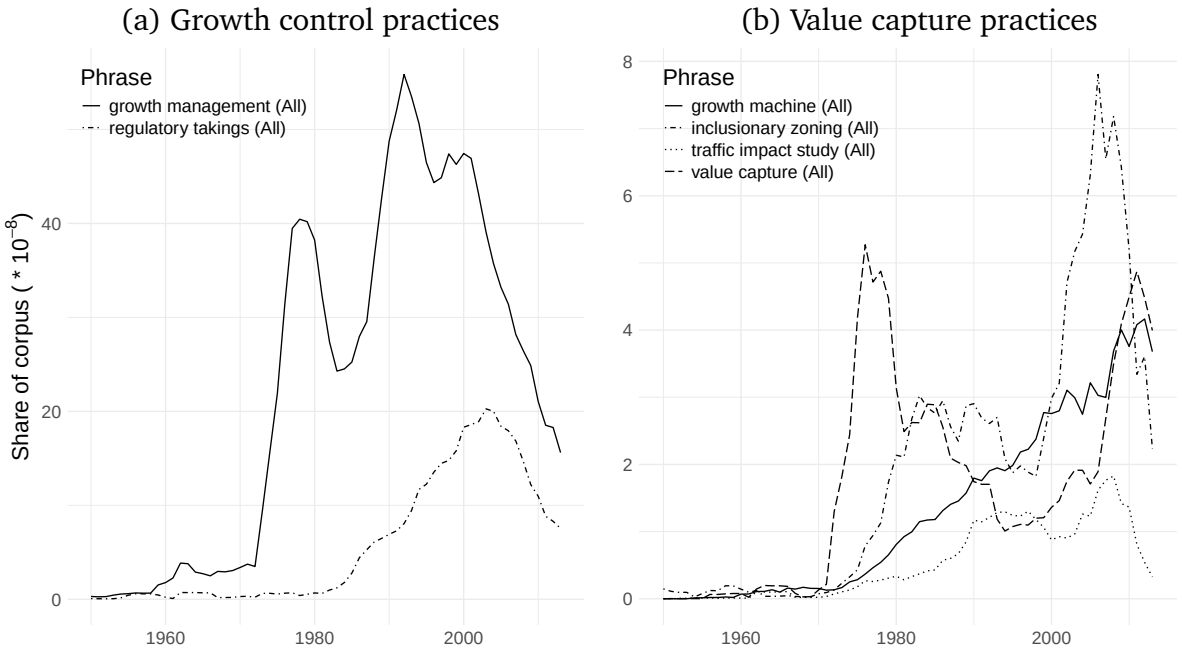
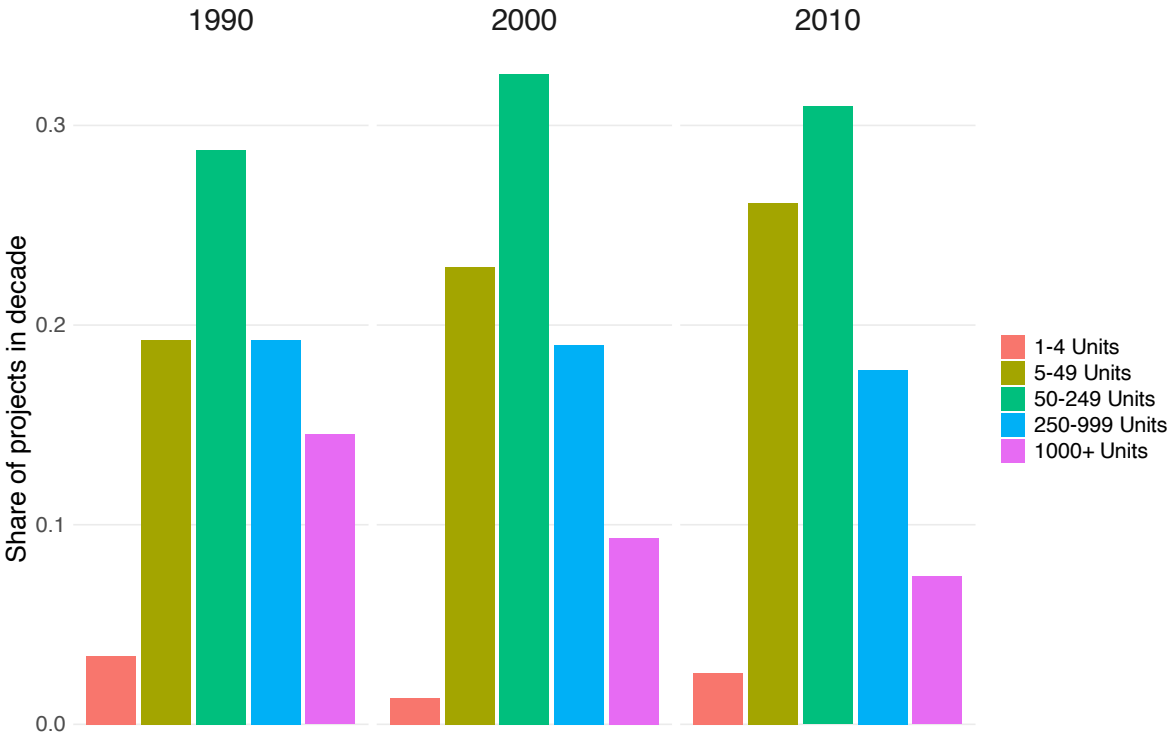


Figure B.11: Development-level distribution of unit counts



C Details on additional data cleaning

C.1 Classification of HUD index

After loading in the 62,314 separate titles recorded in the HUD document index, we first clean and standardize the "geographic place names" (GPNs) assigned to every title. Planning documents produced under the Section 701 program may both refer to municipalities (the focus of the small areas urban planning assistance program), or other geographies like planning areas, counties and metropolitan areas. From the roughly 15,000 unique GPNs in the index, we conduct a fuzzy string match of GPNs to a directory of zoning jurisdictions.

After the match and filtering only to the incorporated municipalities and townships in our analysis sample, we retain approximately 6,000 municipalities. Around 2,800 of the remaining municipalities were also found in the HUD/HHFA project directories for 701 planning assistance (i.e. the link to the project directory municipalities is not perfect, but has high coverage).

Over the municipalities linked to both the index file and the project directories, we classify documents associated to each municipality by keywords occurring in the document titles. A flexible keyword to capture if a document is related to zoning, for example, not only checks for the string 'ZONING ORDINANCE' but also related terms like 'ZONING RESOLUTION', 'ZONING MAP' and so forth. Below, we list the exact regular expressions used as the keyword to classify titles.

- Development plan: "`(COMPREHENSIVE|MASTER|GENERAL|LAND USE) .*PLAN`"
- Capital budget: "`(CAPITAL|PUBLIC) IMP`"
- Zoning-related: "`ZONING (ORDI|REGUL|RESOLUT|STANDARD|MAP)`"
- Subdivision regulation-related: "`SUBDIVISION `"
- Further land use: "`^LAND USE`"
- Floodplain-related: "`FLOOD |WATERSHED | STORM `"
- General environmental: "`ENVIRONME`"
- Open space-related: "`OPEN SPACE |RECREATION'`"

We allow for the same document title to be classified into multiple groups, though the deliberate usage of spaces is aimed at limiting misclassification based on the keyword matching to parts of longer words. Then, as long as a document linked to a municipality is classified to be in one of the eight types, the municipality is classified to have that type present, which is the outcome we illustrate in Figure 1.

C.2 Imputation of post-1966 adopters

Figure 1 suggests that municipalities receiving planning assistance under the 701 program often received a standard package of planning products: a document that comprises the final

plan, plus regulatory amendments related to zoning and to land use. To impute additional municipalities that may have received 701 grants after July 1966, we search for cities with both types of documents in the HUD document index.

For GPNs matched to a standardized zoning jurisdiction but not also matched to the project directory, we filter to municipalities whose earliest document in the index is 1966 or later and whose 1960 population is below 50,000 residents. We search for documents produced up to a final year, which we set to be 1973.

A municipality is imputed to be a post-1966 adopter if, among products produced for that municipality up to a final year, one of two conditions are satisfied:

1. It has one document classified as either a development plan or a capital budget, *plus* a document classified as related to zoning, subdivision or further land use;

2. There are four or more documents produced for the municipality, at least two of which are classified to be in the development plan/capital budget category or the zoning/land use category.

The filters we use represent a tradeoff that is biased towards a conservative definition of post-1966 adopter. We aim to avoid false positives, where a document may have been issued for a zoning jurisdiction which was separate from a comprehensive planning grant issued to that municipality. That is a possibility when 701 grants became more commonly issued to regional planning or state planning agencies in the late 1960s.

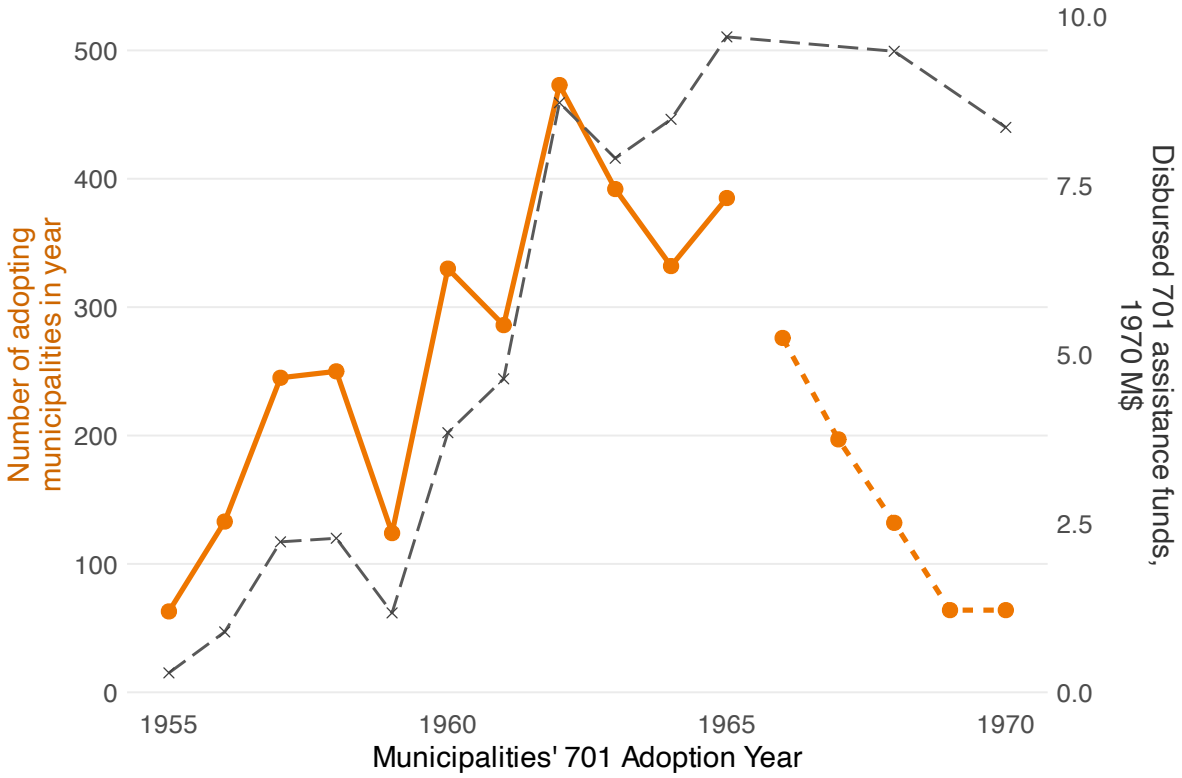
In addition, we use a rule of thumb that the planning products produced in the 701 process were dated two years before grants were approved and the municipalities adopted 701, using information from federal reports that the process usually took 18 months. By comparing approval dates for pre-1966 municipalities to HUD document years, we found that development plans were issued 3 years after grant approval on average. The minimum of the document years, shifted forward following the rules above, is the adoption year of post-1966 imputed municipalities.

We detect 893 additional jurisdictions through this process. In our analysis, we pool these jurisdictions along with adopters as of 1966 specifically in the spillovers specifications estimated in Table A.4. Figure C.1 superimposes the imputed time series of municipalities onto Appendix Figure B.2. While the level of 701 funds disbursed stays steady past 1966, the amount becomes more diverted away from municipalities that had been taking up federal planning assistance; the trend of imputed adopters is on a steady decline, until it reaches fewer than 100 municipalities in a year by 1970.

C.3 Details and formulas for non-supply outcomes

Construction of affordability measures. For median home values at the metropolitan level, as well for municipalities in 1970 (when medians of the distribution were not precisely reported), we calculate the median value from the (possibly aggregated) home value bin data. We use the binned home value distribution to interpolate the median value. For the first bin where 50% of units have values below the bin, the interpolated value is the midpoint of the bin's range. We also interpolate median values the same way with 1940 pre-701 data, as home values in the full count Census are reported, but rounded to the nearest hundred.

Figure C.1: Visualizing imputed trends in 701 grants



Our primary measure of affordability is to take the MSA-level median home value, then count the share of units in each municipality below the threshold. We repeat the interpolation process for the 25th quantile for each MSA, constructing a separate measure of "bottom segment" owned units that are present in each municipality.

We also construct a measure of rental affordability using MSA-level data as the denominator. For each MSA, we calculate the median renter household income at that level. Using standard definitions of rental cost burden, we define a MSA-wide affordable rent as 30% of that median. Then, using binned tabulations for rent at the municipality level, we can calculate the share of units available in the municipality whose rents would not impose a cost burden on the median renter in the wider housing market.

Segregation index for multifamily apartments. In analysis of racial segregation, it is common to use a dissimilarity index of segregation when only two racial groups are considered. We can adapt the same index by splitting the housing stock built up to a year as either units in multifamily apartments or non-apartment units.

Define the probability that a unit is in block group k, given the unit is in a municipality and belongs to group g, as $P_{k|g}$. The dissimilarity index of segregation for a municipality is then:

$$I = \frac{1}{2} \sum_{k} \left| P_{k|A} - P_{k|B} \right|$$

In other words, it is the difference of ratios in how much the share of the overall group is in each block group. Using 1990 Census data, we match the municipalities in our analysis sample to block groups that are mainly inscribed in one municipality's borders. We iterate, over those block groups, the dissimilarity index calculation for each municipality as of 1990.

Bunching measure of minimum lot size restrictiveness. Though details are provided in Cui (2024), the general idea behind estimating a national panel of MLS requirements is to simultaneously back out which requirements were ever adopted, then estimate the degree of bunching at the requirement levels at any one time. The estimation is done with reference to the lot size distributions of single-family homes in a jurisdiction every decade, so it does not require any spatial information on zoning district boundaries.

We classify a lot size as being one mandated by a requirement somewhere in the municipality if we detect a concentration of development at that specific lot size, such that the density estimate at the size is much larger than the density estimate over marginally smaller lots. The excess mass measure of MLS restrictiveness requires estimating a counterfactual density without bunching induced by the lots, which we can do using kernel estimation methods leveraging density info for lots marginally larger or smaller than the lot size mandated by MLS requirements.

Unlike the Census data before it, the restrictiveness outcomes can be missing for municipalities that either did not build meaningful amounts of housing units over a decade or lack an online version of their zoning ordinances.[1] We do not impute these measures where they are missing, but we note that the median population of eligible municipalities missing lot size restrictiveness data is 624.

[1] This is a separate case from, e.g., lot size restrictiveness having a value of 0, indicating that there were enough units built but no discernible bunching on lot sizes.

D More on identification assumptions

D.1 Overview of state agency practices around 701 assistance

We offer evidence from primary sources on two claims: the lack of consistency in how agencies encouraged municipalities to apply for funds, and the importance of understaffed agencies in affecting their share of 701 funds. We evaluated policy memos issued by federal agencies (HUD and the Housing and Home Finance Agency, the Department's predecessor) and documents from 17 state agencies as they approved 701 assistance funds in the 1960s.

Agencies had discretion to publicize the 701 assistance program to any municipalities in the state, as well as consult with local government on how to prepare an application for the federal grants. While we found evidence that federal officials would occasionally cap how much funding can be allocated to one state, there were no minimum allocations to states or attempts to prioritize grants for large states with low adoption.

There were examples of states at both extremes of publicity and consultation. One model used by states like Alabama, Rhode Island or Oregon was to match municipalities with planners directly employed by the agency, replacing the role of planner consultants in the process entirely. This model was also complemented by state financial assistance to make up for the gap in federal funding, leading to minimal financial and transaction costs for any municipalities interested in planning assistance.

On the other hand, New Hampshire and Virginia were particularly selective in screening applications for 701 assistance from municipalities. To pursue assistance only for localities with "concrete commitments to planning,", they screened out applications from municipalities with inexperienced planning commissions or where no full-time planner was already hired (Hammer Greene Siler Associates, 1969). In another case, certain states did not pass legislation allowing for regional planning or to enable receiving federal planning funds, as also noted in Collins and Shester (2013). Idaho and Utah did not pass enabling legislation until 1965, while South Carolina did not pass all required legislation until 1968.

Our second point is that insufficient staffing of the agency caused delays in approving grants before the year's Congressional allocation for 701 funds was exhausted, which meaningfully altered the state's share of allocated 701 funds. For five states, we can retrieve the number of planners responsible not just for filing 701 applications at different points in time, but also to supervise the caseloads for grants in progress until they were completed. That allows us to calculate the average caseload per supervising planner, derived from the jurisdictions with a 701 grant approved but still not yet completed at the point in time.

In Table D.1, we report the point-in-time estimates of planner caseloads for available states. There is large variation in caseloads: active municipalities supervised by planners range from 30 to 3 municipalities, depending on the state. This measure of planner caseloads is also an underestimate of applications that supervisory planners are preparing for submission to new grants. Even when caseload ratios are similar, a state like Texas is dealing with much larger municipalities on average, so each planner is managing planning services that cover a greater number of residents.

A larger caseload per planner not only plausibly delays the time between when new municipality began the 701 application process to when a planner can first be paid for work, but also affects whether they receive 701 assistance at all. First, Hammer Greene Siler Associates

Table D.1: Caseload burden variation between 701 state agencies

State	Date of data	Planners	Active 701 munis	Caseload/Planner	1960 Residents/Planner
MI	05/1966	3	90	**30**	189K
TX	03/1966	4	78	**19.5**	145K
MN	03/1966	4	74	**18.5**	60K
WA	03/1966	3	33	11	80K
RI	10/1957	5	13	2.6	29K

Notes: For a given year and month, we reference the project directory to see which cities had an active grant as of that point in time. The rightmost columns are ratios of the activee municipalities and of those municipalities' 1960 Census populations. Bolded cells reflect caseload ratios above the HHFA caseload limit of 12 to 16 municipalities per agency planner.
Sources for the planner counts are: Gillings (1967) for Michigan, Hammer Greene Siler Associates (1969) for Texas, Minnesota Department of Business Development (1966), Washington Department of Commerce (1966) and Simha (1957) for Rhode Island.

(1969) notes that indeterminated delays to the start of planning work can harm the initial enthusiasm local officials had to learn about comprehensive planning. Second, the guidelines published by the HHFA indicate a requirement for state agencies is to have an active caseload of no more than 12 to 16 municipalities per planner. Documents describing Michigan and Washington's 701 experiences mention that federal authorities had warned a suspension in funds to those states until they increase their staffing numbers.

Finally, due to demand for 701 increasing nationwide, by the mid-60s there were reports in state documents of municipality backlogs: cities that filled out the paperwork, but was not processed before a year's appropriations for 701 assistance ran out. We find it plausible that states with lower capacity and higher caseloads per planner were less capable of clearing their backlogs, such that many interested municipalities could not get their applications approved before the late 1960s. Being aware of excess demand for the program, the HHFA first set additional caps on grant amounts, and then imposed a moratorium on any applications from October 1966 to the start of 1968 (Minnesota Department of Business Development, 1966).

By the late 1960s, 701 funding had expanded to many potential state and local planning authorities or government agencies, and for purposes beyond physical planning of the built environment. Additional requirements and proof of local planning capacity were put in place for the allocation of 701 funds after 1968. The extra administrative costs behind any 701 applications, consistent with a reorienting of the program towards funding regional and state planning authorities, could explain the decline in 701 assistance adopters we backed out from the imputation process in Appendix C.

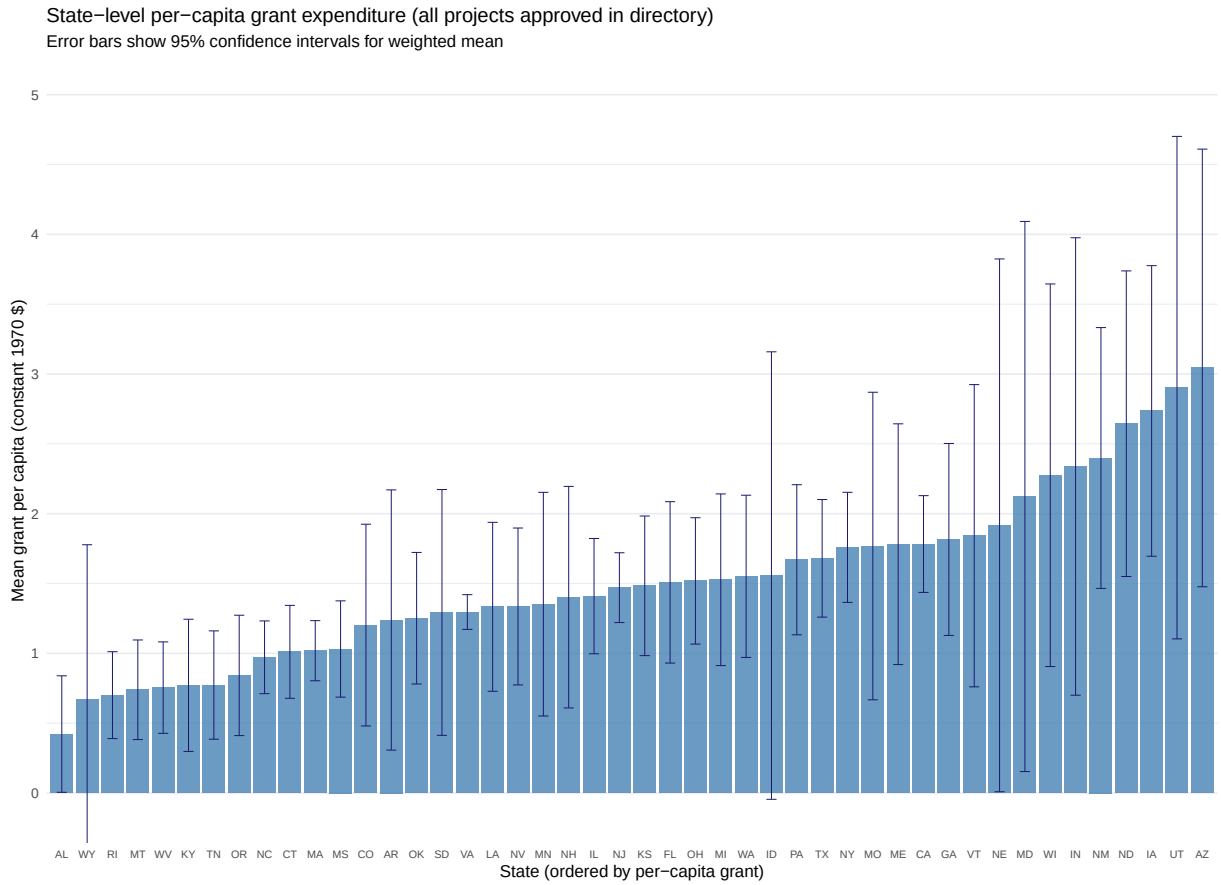
D.2 Adoption rates are well explained by state agency strategies

We conducted more quantitative analyses that show how the capacity of state agencies mattered for 701 receipt. The main idea is that we check how predictive dimensions of state agency conduct are on the adoption rate measure we use to rank states by agency capacity,

$\tilde{Z}_s$. If adoption rates are driven mainly by differences in municipality characteristics between states, regressions excluding those characteristics to predict $\tilde{Z}_s$ should have poor fit. As it is unlikely we can get systematic data for the most obvious measures of capacity (e.g. the number of planners on staff at each agency), we focus on two measures that can be constructed from the 1966 project directory data.

First, we can calculate per-capita grant expenditures for each state. For each 701 grant approved in the directory, we sum the 1960 population counts for the municipalities to which the grant was applied. Figure D.1 shows mean levels of per-capita expenditures taken over the grants, further weighed by the population to which each grant was applied. Across the grants we have in the project directory, most states had per-capita expenditures of between 1.2 to 1.8 constant dollars per capita for their 701 grants. However, there are both Southern states with lower per-capita expenditures (like Alabama and North Carolina), as well as high-generosity states like Arizona.

Figure D.1: Between and within-state variation in grant generosity



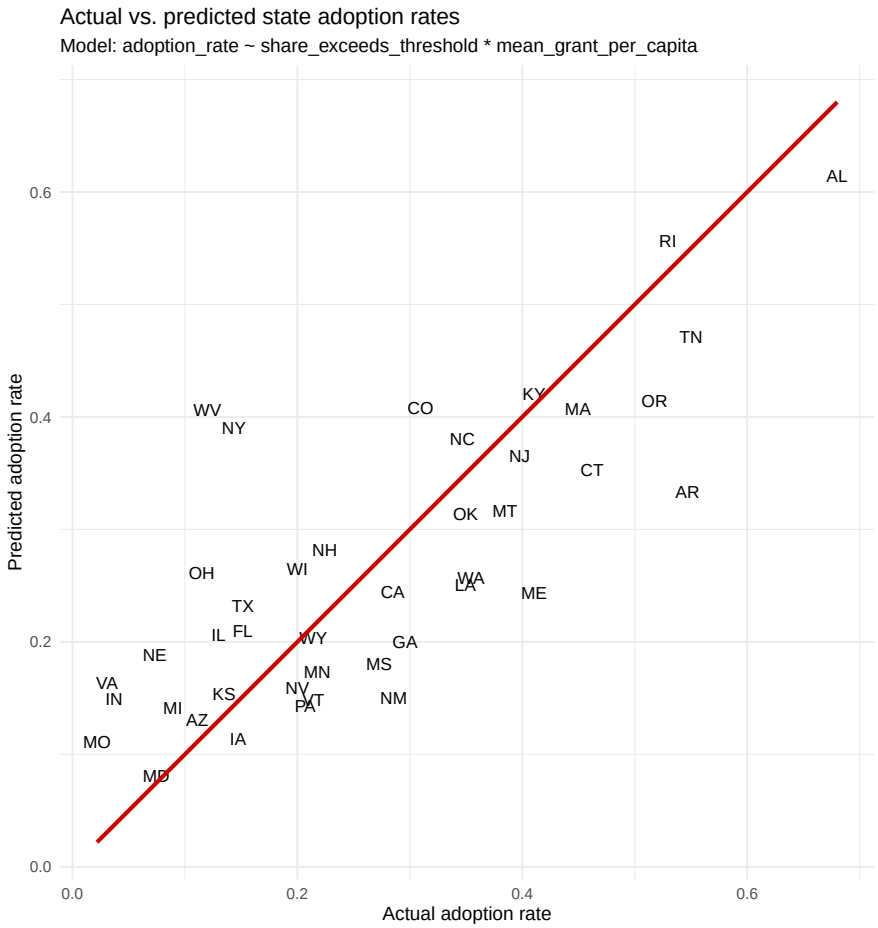
Second, we leverage a loophole that state agencies might have exploited in the early phase of the Section 701 program. After 1961, state agencies can save paperwork and repeated filings by applying for a common grant applied to multiple neighboring municipalities, as long as their combined population is below 50,000. However, prior to 1961, the statute placed no limit on how many municipalities can be approved on a joint grant, as long as each municipality

was below 25,000 residents. The degree to which state agencies packaged grants serving over 25,000 residents in sum is thus a proxy for the underlying state agency's knowledge of the policy, and its ability to use staff to coordinate joint applications across multiple municipalities.[2]

For each grant, before 1961, we classify if it served a group of municipalities that had over 25,000 population in 1950. For each state, we calculate the share of grants satisfying the criterion. With the two variables, we run a state-level regression where $\tilde{Z}_s$ is the outcome and the right hand side variables are the two measures, as well as their interaction.

Figure D.2 shows predicted values of $\tilde{Z}_s$ from the state agency conduct regression, plotted against actual $\tilde{Z}_s$. The overall R^2 is 0.566, reflecting a good fit without invoking additional municipality level covariates. Outliers where the predicted rate is poor are also rare, with most predictions close to the plotted 45-degree lines. Outliers include the States of New York and Ohio (where actual rates are lower than predicted rates) and the States of Arkansas and Tennessee (where actual rates are higher than predicted, possibly reflecting other unobserved characteristics of high capacity with those agencies).

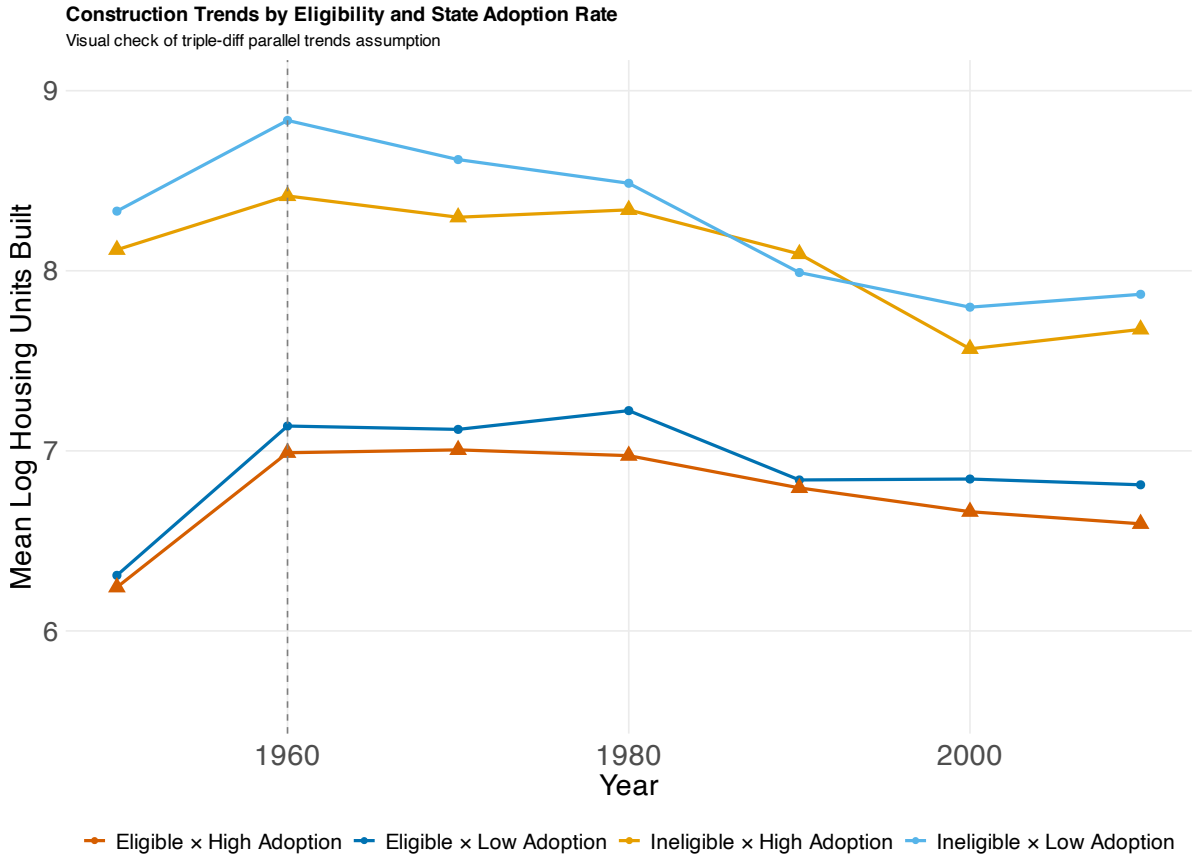
Figure D.2: Predicting adoption rates based on only state agency strategies



[2]The two state agencies that were staffed with in-house professional planners — the ones in Alabama and Rhode Island — always pooled municipal grants steadily over the year.

D.3 Comparison of empirical strategy to other research designs

Figure D.3: Representing ID strategy as two difference-in-differences



D.4 Additional statistical tests

Figure D.4: Representing ID strategy as two differences-in-discontinuities

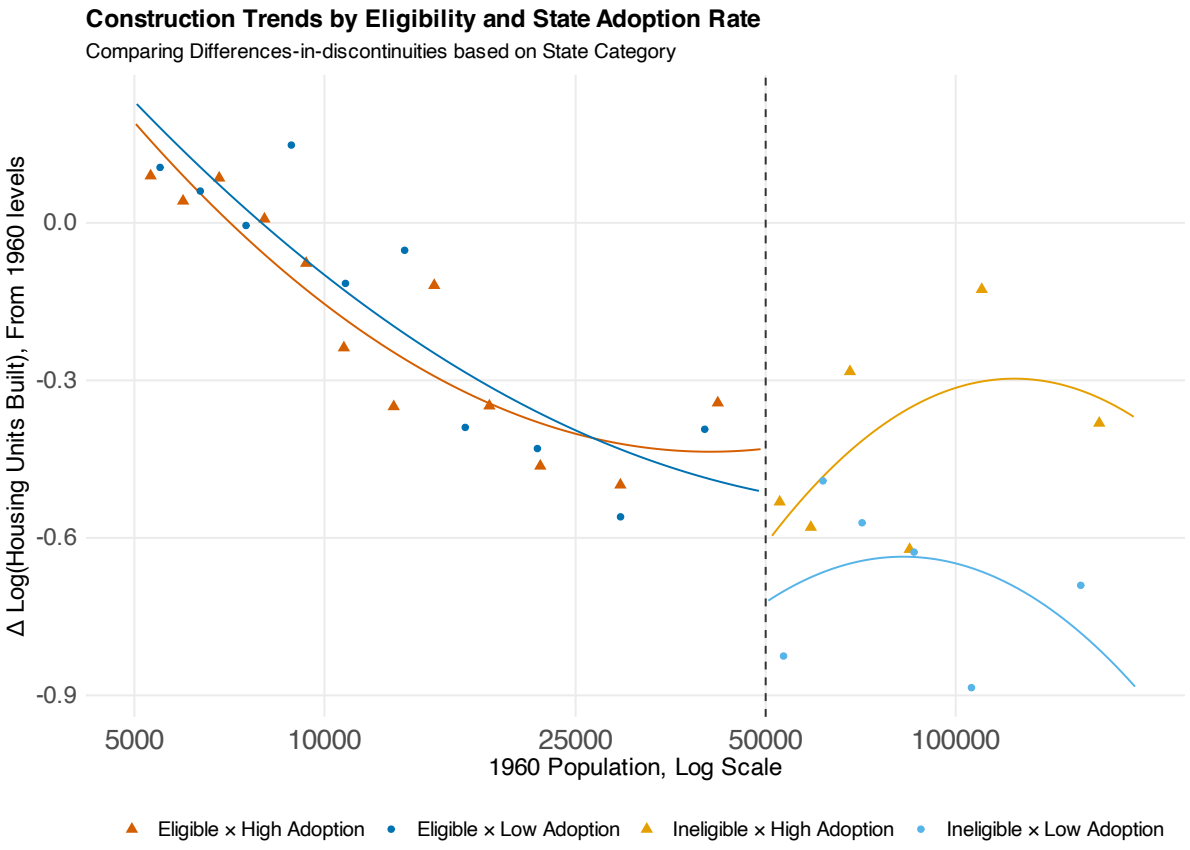


Figure D.5: Parallel trends comparisons for Lin and Peri (2025), Assumption I

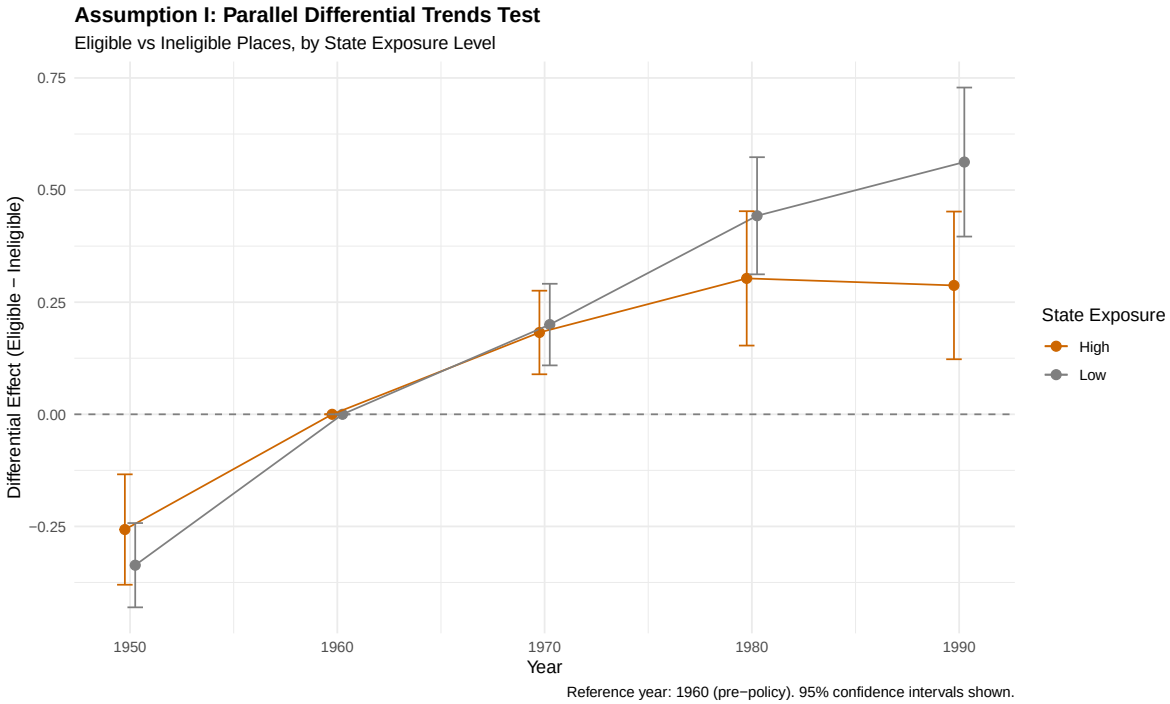


Figure D.6: Parallel trends comparisons for Lin and Peri (2025), Assumption II

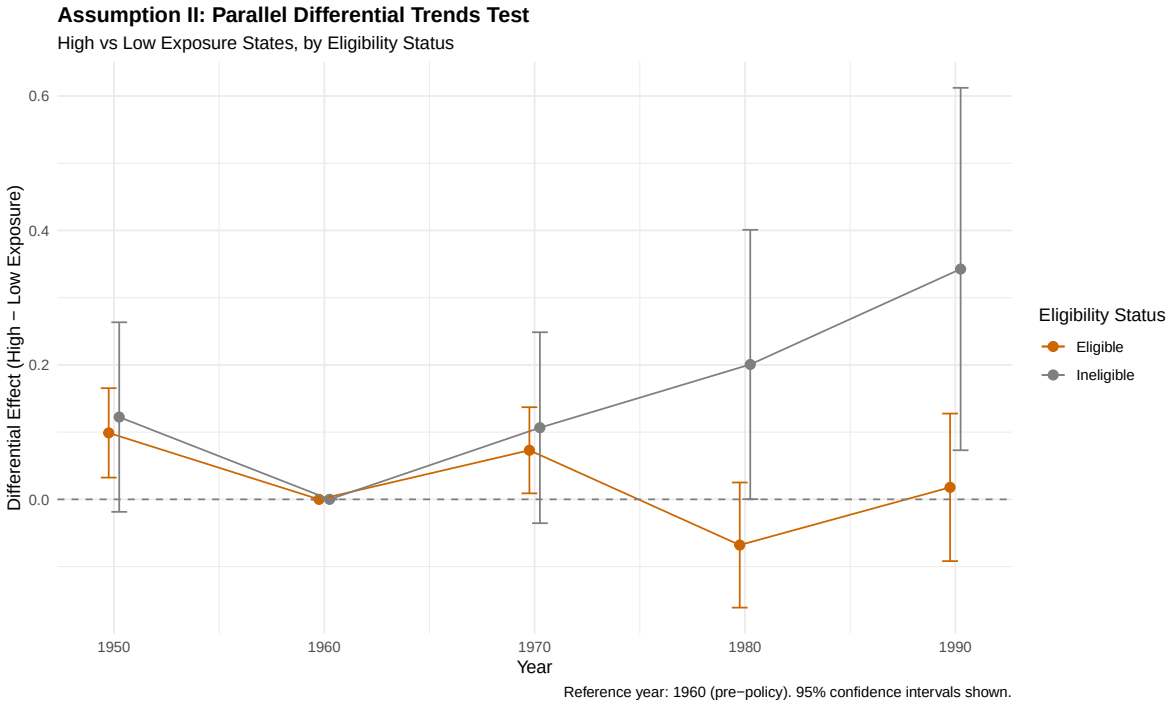
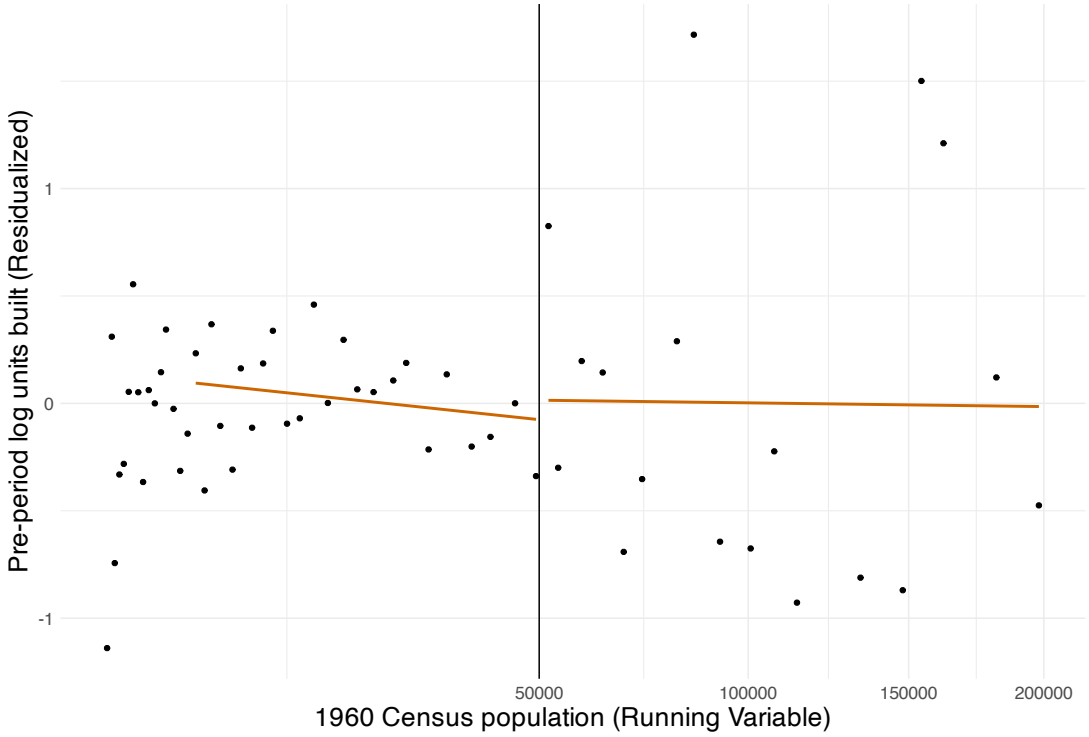


Figure D.7: Pre-period conditional independence over analysis sample thresholds



D.5 Proof of Proposition in Section 4

Proof. Assumption CI directly implies, for $z, z' \in \text{supp}(\tilde{Z})$,

$$\mathbb{E}\left[Y_t^0 - Y_{t-1}^0 \middle| \tilde{Z} = z, \text{Pop}_{1960} \in (\underline{P}, 50K], X\right] - \mathbb{E}\left[Y_t^0 - Y_{t-1}^0 \middle| \tilde{Z} = z, \text{Pop}_{1960} \in (50K, \overline{P}], X\right]$$
$$=$$
$$\mathbb{E}\left[Y_t^0 - Y_{t-1}^0 \middle| \tilde{Z} = z', \text{Pop}_{1960} \in (\underline{P}, 50K], X\right] - \mathbb{E}\left[Y_t^0 - Y_{t-1}^0 \middle| \tilde{Z} = z', \text{Pop}_{1960} \in (50K, \overline{P}], X\right],$$

which also implies that $\mathbb{E}\left[Y_t^0 - Y_{t-1}^0 \middle| \tilde{Z} = z, E_t, X\right] = \gamma_0 + \gamma_1 E_t$, where the analysis sample is filtered to the window $\text{Pop}_{1960} \in (\underline{P}, \overline{P}]$ and $E_t = 1$ if $Pop_{1960} \leq 50K$ and 0 otherwise.

Define a vector of covariates $\tilde{X} = [1 \quad D \times E_t \quad (1-D) \times E_t]$. The assumptions we imposed imply the following two linearity conditions:

$$\mathbb{E}[Y_t^0 - Y_{t-1}^0 | \tilde{Z}, \tilde{X}, X] = \tilde{X}'\gamma, \tag{6}$$
$$\mathbb{E}\left[\frac{Y_t - Y_t^0}{\tilde{Z}} \middle| \tilde{Z}, \tilde{X}, X\right] = \tilde{X}'\eta, \tag{7}$$

but a restricted version where the coefficients on $D \times E_t$, $(1-D) \times E_t$ equal zero in Equation 6, while coefficients on 1 is equal to 0 in Equation 7.

Using the arguments for identifying TWFE with covariates in de Chaisemartin et al. (2025), we can express:

$$\mathbb{E}[Y_t - Y_{t-1} | \tilde{Z}, \tilde{X}, X] = \tilde{X}'\gamma + \tilde{Z}\tilde{X}'\eta,$$

and a regression featuring the covariates in $\tilde{X}$ and their interactions with $\tilde{Z}$, but specifically ones whose coefficient terms in the two linearity conditions are assumed to be nonzero.

Our estimands of interest, τ_{treat} and τ_{elig} are treatment effects conditional on $D \times E_t$ and E_t, respectively. The treatment effects can be rewritten as:

$$\begin{aligned}
\tau_{treat} &= \mathbb{E}\left[\tilde{Z} \times \left(\frac{Y_t - Y_t^0}{\tilde{Z}}\right) \middle| D \times E_t\right] \\
&= \mathbb{E}\left[\tilde{Z} \times \tilde{X}'\eta \middle| D \times E_t\right] \\
&= \eta^{D \times E} \mathbb{E}\left[\tilde{Z} | D \times E_t\right],
\end{aligned}$$

where $\eta^{D \times E}$ reflect the coefficients for the coefficient for specifically the $D \times E_t$ coefficient.

Likewise,

$$\begin{aligned}\tau_{elig} &= \mathbb{E}\left[\tilde{Z} \times \left(\frac{Y_t - Y_t^0}{\tilde{Z}}\right) \middle| E\right] \\ &= \mathbb{E}\left[\mathbb{E}[\tilde{Z} \times \tilde{X}'\eta \,|D] \middle| E\right] \\ &= \left[\eta^{D\times E}\mathbb{P}[D=1\,|E] \quad \eta^{(1-D)\times E}\,\mathbb{P}[D=0\,|E]\right]\begin{bmatrix}\mathbb{E}[\tilde{Z}\,|D=1,E] \\ \mathbb{E}[\tilde{Z}\,|D=0,E]\end{bmatrix}.\end{aligned}$$

□

We briefly discuss the plausibility of the assumption that there is no "intercept" coefficient in Equation 7. Our preferred interpretation of that assumption is that for the smallest values of $\tilde{Z}$, the coefficient being zero implies 701 eligible municipalities after the policy rollout did not alter their growth trajectories, nor cause spillover effects on larger ineligible cities.

In our context, because we have near-zero levels of $\tilde{Z}$ for certain states, those states are analogous to the "quasi-untreated group" invoked by de Chaisemartin et al. (2025) as necessary for effect identification. It is then unlikely that states where 701 assistance was rarely applied could generate intercept effects, and the assumption on no spillovers to ineligible cities is no stronger than what is implied by invoking Assumption CI.

E Details on LLM parsing of developments awaiting permits

We detail the procedure we used to code a novel sample of newspaper articles that describe specific developments that had to be approved for a building permit or land entitlement. The source of our local newspaper data is the Newsbank Access World News database, which is licensed for non-commercial use to New York University. We processed newspapers between the years of 1983 to 2009 throughout the United States.

We first retrieve a sample of articles specifically about developments that satisfy two qualities: the article mentions it is not yet approved or permitted, and the article mentions some form of opposition to a developer's plans. To do so, we used the Newsbank platform to search for articles with the following three terms in the article body: "housing units", "developer*" and "neighbor* oppos*". The asterisks are wildcard terms, so i.e. both "developer" and "developers" would be searched for in this search. We also require the article to either have the term "approv*" or "permit."

About 4,500 articles are returned from the search, a sample that is biased towards recent years due to data availability. We undersample articles in recent years while processing nearly all available articles in early years. The sample thus includes around 180 articles prior to 1990, up to 360 articles in the 2003-5 3-year period.

For the final sample of around 2,000 articles, we process each article through three separate LLM prompts to verify their relevance and extract details of the permitting process. We leverage the `gpt-5-mini` model from OpenAI and iteratively execute the prompts through the `DocETL` data processing platform. The tasks we prompt the LLM model to do are either retrieval of specific terms from the text, or annotation tasks that asks if the article describes a certain trait or practice; the LLM then returns a binary yes/no answer and potentially a "N/A". Our process thus closely resembles the tasks instructed to the LLM in Bartik, Gupta and Milo (2024) and Djourelova et. al. (2024). To ensure accuracy, we also execute our prompts in a "few-shot" manner by including examples of the tasks at hand.

We produce the three prompts verbatim below. The first prompt works to filter the article out of later processing, if the LLM decides the article does not concern a residential development at all. The second prompt extracts characteristics of the development, while the third prompt asks for any description of specific processes. Variables embedded in curly brackets reflect variables referenced through the `DocETL` interface.

Relevance prompt:

> Analyze the following news article:
> Title: "{{ input.title }}"
> Content: "{{ input.body }}"
>
> Determine if this article is relevant based on the following criteria:
> Does the article concern the opposition to approving, or process of approving, a residential building in a city?
>
> Some examples of articles that do not concern the approval process:
> (1) An article that just describes the character of a city or neighborhood.

(2) An article about a developer's biography, without reference to a specific building under the approval/entitlement process.

Respond with 'true' if the article satisfies the criteria, otherwise respond with 'false'.

Project characteristics prompt:

Analyze the following news articles that involve some sort of local government body approving or opposing residential buildings:
Title: "{{ input.title }}"
Content: "{{ input.body }}"

Most of each article should refer to the city in which a building is to be built. Another possible case is if the local government unit is a County, in which case there should be reference to bodies named, e.g. the "county planning commission."

DO NOT confuse a reference to the city or county with a reference to an area or neighborhood. If you cannot find an unambiguous reference to the city in question, check the field in JSON: input.city and that value where it is available.

DO NOT include references to projects unrelated to residential buildings, such as malls, streets, infrastructure and so forth.

Respond with blank or NA values if information is unavailable.

With that knowledge, answer the following questions for every building reported on in the article:
(1) In which municipality or general local government unit did the reported event takes place? If the local body is a county, always add "County" to the end of the field. Otherwise, ignore indicators like "City of," "Town of," etc.
(2) What was the originally proposed number of housing units in the building which might be approved? This column should consist of a single number.
(3) Only if the article discusses a final number of housing units in question: what is the agreed upon number of housing units that are permitted in the approved project?

Process characteristics prompt:

Analyze the following news articles that involve some sort of local government body approving or opposing residential buildings:
Title: "{{ input.title }}"
Content: "{{ input.body }}"

Most of each article should refer to the city in which a building is to be built. Another possible case is if the local government unit is a County, in which case there should be reference to bodies named, e.g. the "county planning commission."

We will ask you about citizen interest groups, examples of which include "neighbors" and "neighborhood associations." We will also ask you about "local planning staff", examples of which include "city planners," "city planning commission" or any governmental body other than the city council voting on approving a project.

By "low-income housing", we mean housing units deed restricted to be sold at below market rents. Articles will generally refer to a specific number or share of units as allocated to "low-income residents/households," and these projects may also be mentioned as being financed by housing tax credits at the federal or state/local agency level.

Respond with blank or NA values if information is unavailable.

With that knowledge, answer the set of binary questions for every building reported on in the article, where 1=Yes, 0=No;
(1) Are citizen interest groups quoted to have spoken out in opposition of the project?
(2) Have local planning staff, up to now, delayed or rejected the project?
(3) Has the residential building reduced in size as part of deliberations to get a permit to build?
(4) Has the developer of the building offered to provide additional community benefits, or impact fees?
(5) Are a share of the building's housing unit intended to provide low-income housing?
(6) Is the entirety of the building intended to provide low-income housing?

Mapping of prompt questions to outcomes. As with other text-based datasets in this Appendix, we use only the developments whose municipality (from Question (1) of the project prompt) can be fuzzy matched to a directory of zoning jurisdictions. In our analysis, the distribution of development unit counts shown in Appendix Figure B.11 reflect the output of Question (2) of the project prompt.

The main value capture outcome we analyze — the presence of additional community benefits or impact fees — comes from Question (4) of the process prompt. We have manually checked 11 developments classified as "1" by the LLM, noting that all but one had an article that describes a common value capture practice. Examples of correctly identified practices include developer commitments to finance any roads or infrastructure around the development; provision of public amenities, like sound barriers next to the highway and a community center; in-lieu fees to support the maintenance of affordable housing in the municipality; and leaving up to half of the acquired parcel as public open space.

Table 7 also reports two other outcomes. The low-income housing outcome maps to Question (5) in the process prompt, while the local staff opposition outcome maps to Question (2) in the process prompt. We have ran our cross-sectional design on the opposition outcomes related to Questions (1), (3) and (6), all of which outputs null results similar to the local staff opposition outcome.

F Details of robustness checks

Robustness to sample and population threshold selection. Appendix Table A.6 produces robustness checks using different ways of cutting off the maximum and minimum 1960 populations in the analysis sample. In our triple differences design, such sample adjustments are analogous to changing the local bandwidth over which outcome functions are estimated in a regression discontinuity design (Cattaneo and Titiunik, 2022). We note that a specification where no population thresholds are used produces more negative effect estimates on housing supply. A specification with a smaller population range (1960), from 7,500 to 160,000 residents, yields similar negative effect estimates to those in Table 2, but increases the standard errors due to a smaller sample size.[3]

In contrast to the first two panels of Appendix Table A.6, which vary how much we include data away from the discontinuity, the last panel of the Table conducts a "donut" robustness check close to the discontinuity. When we drop municipalities whose 1960 populations were between 40,000 to 60,000, supply effects are more negative across all specifications. Finally, Appendix Table A.7 conducts a placebo regression discontinuity at a nearby population level not used as a threshold in 701 assistance. In a setup similar to Aaronson, Hartley and Mazumder (2021), we run the design for municipalities with populations below 20,000 and those above 20,000 but below 50,000. For our supply and price outcomes, we find point estimates close to zero, with no significant effects at the 90% confidence level.

Post-1960s state divergence. A competing mechanism for our results is that states whose agencies were more capable of distributing 701 assistance funds may also have had a greater interest in passage of additional planning legislation. That increased interest may have been endogenous to the states' economic structure prior to the 1960s: for example, if the state had large manufacturing sectors that induced pollution and environmental disamenities. While empirical work exists on the housing supply consequences of green belts and regional land use restrictions (Yu (2019); Koster (2024)), those effects operate separately from our proposed channel of 701 assistance, changing perceptions about growth among local interest groups.

We verify that our results are robust to a variety of additional state-level controls marking differences in states' industrial composition and planning laws. In Table F.1, we add two predetermined state-level controls: 1940 manufacturer share and Black resident share in incorporated municipalities, tabulated from the full count data in Section 3.3. In Table F.2, we control explicitly for the adoption of state environmental policy acts (SEPA), implementing environmental review that have been leveraged to delay and cancel residential development (Elmendorf and Nall, 2024). Furthermore, we also conduct an analysis excluding the three states whose SEPAs allow for delays and remedies for non-compliant private-sector projects: California, New York, and Minnesota.

In the tables we listed, we find minimal changes in the estimated coefficients for housing

[3] As discussed in Section 4.3, the identifying variation in our research design is not local variation around the 50,000 population eligibility cutoff, but variation in state agency capacity that is plausibly exogenous to differential trends between small and large municipalities. In this context, one interpretation of the role of standard checks for an RDD is to ensure that our results are not driven by outlier values clustered around specific values of the 1960 population running variable.

supply, the cumulated apartment share, and median values. Standard errors are generally larger, but the least significant robustness result for the supply outcome maintains 90% statistical significance.

In addition, Table F.4 reports the results of a saturated regression that includes predetermined state-level controls, as well as multiple indices of state planning law restrictiveness from the American Planning Association's 2022 Survey of State Planning Laws. The survey controls include binary variables for state requirements on local planning practices, as well as a three-level index for how comprehensive state laws are for incentivizing "community resilience." When all the controls are included, we find more negative results for housing supply and more precise estimates for all our supply and price outcomes.

Urban renewal slum clearance cannot explain results.

Land annexation as a competing mechanism. Changes in how incorporated 701 adopting municipalities annex development around it could impact our housing supply effects in a mechanical way. The concern would arise if, after 701 assistance, municipalities annex less unincorporated territory than ones that did not receive assistance. In that case, even if the built environment and density of the two types of municipalities were the same, we would see faster log unit growth in 701 ineligible municipalities than in 701 eligible municipalities.

Ruling out this mechanism is complicated by a lack of panel data on U.S. municipality land areas that cover our analysis period and have nationwide coverage. We proceed in two parts and leverage the best land area data that are currently digitized. First, we calculate land areas for incorporated municipalities in the 1980 and 2010 TIGER files, which are comprehensive nationally. Next, we retrieve land areas prior to 1980 recorded in the CCDB collection, which is an unbalanced panel for larger municipalities with populations of over 25,000.

The two sets of results are presented jointly in Appendix Figure F.1, where we apply the event-study design of Equation 4 to the data. The triple-difference estimates on annexation changes from 1980–2010 are close to zero. Results on pre-1980 data, as measured on the smaller CCDB sample, are not indicative of a negative treatment effect on the annexation outcome. We find, if anything, a positive treatment effect that suggests larger 701-eligible municipalities annexed more land.[4]

[4]We have also tried a specification that includes only municipalities with more than 25,000 residents as of 1960, which further balances the post-1980 data with the CCDB data. In it, we detect insignificant negative treatment effects on the land area outcome, due to wide confidence intervals.

Figure F.1: Triple difference event studies for available annexation outcome

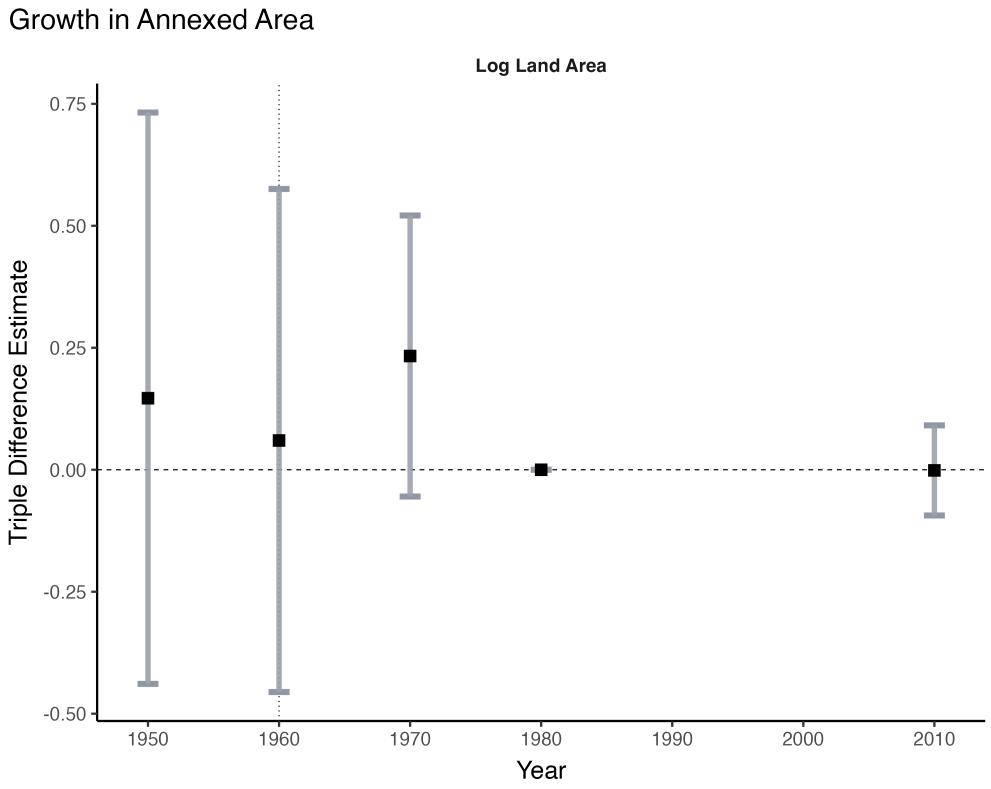


Table F.1: Primary outcome results controlling for workforce confounders

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcomes - Restricted w/ state-level workforce composition					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Coefficient	-0.631**	-7.942**	8.843**	0.313**	-14.189*	5.085**
	(0.309)	(3.523)	(3.590)	(0.132)	(7.486)	(2.000)
Observations	10,654	10,654	10,654	9,746	8,906	12,145
R^2 (within)	0.043	0.031	0.021	0.070	0.069	0.022

Significance levels: * = 10%; ** = 5%; *** = 1%.

Figure F.2: Triple difference event studies for Census population outcome

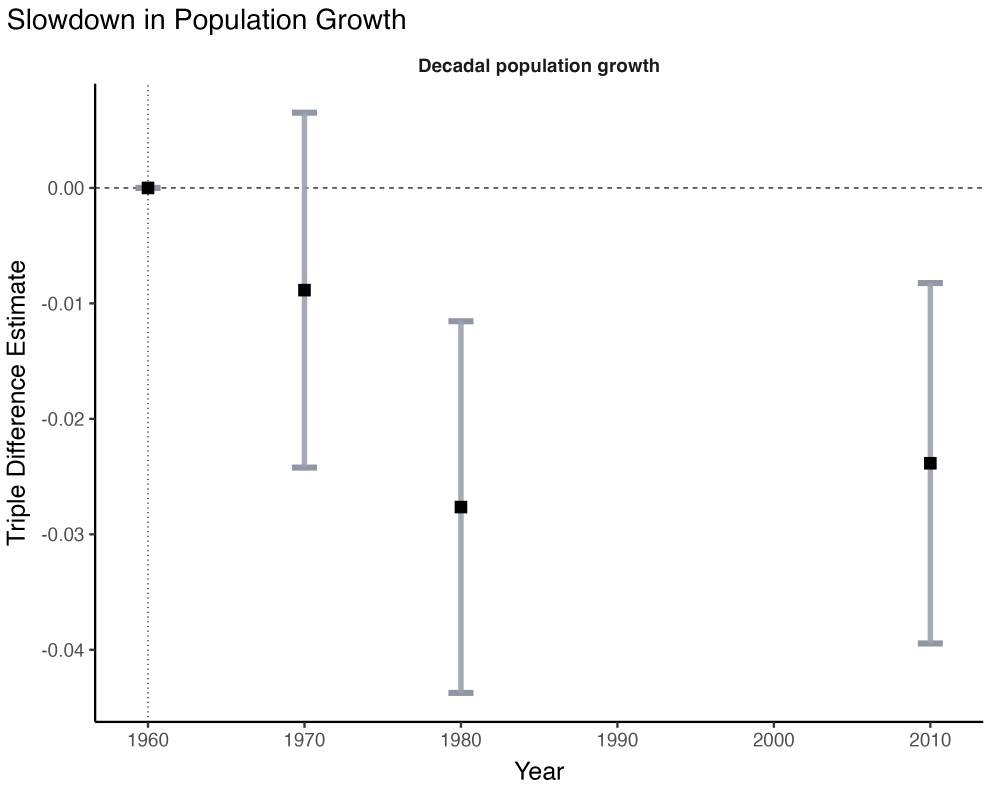


Figure F.3: Triple difference event studies for price-to-income affordability measure

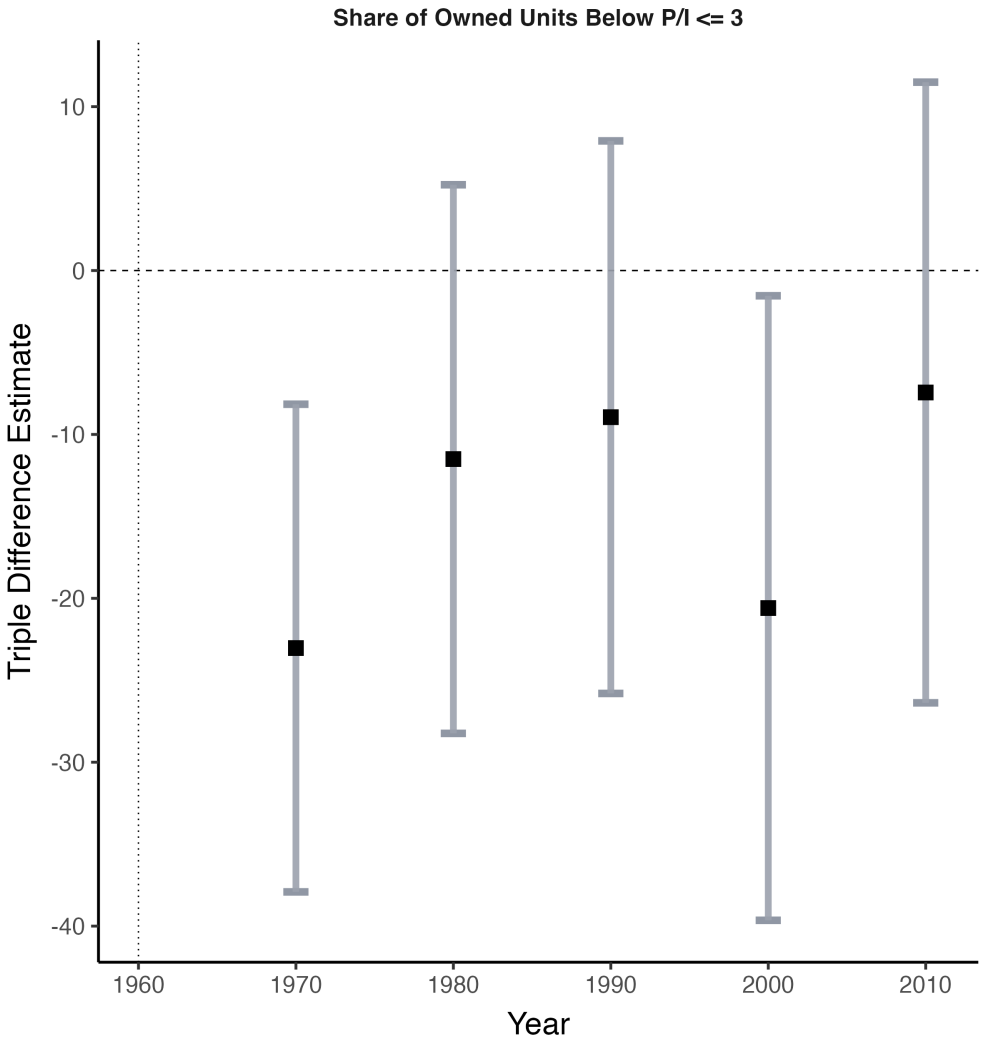


Table F.2: Primary outcome results controlling for state environmental protection

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcomes - Restricted w/ SEPA adoption dummy					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Coefficient	-0.641**	-7.888**	9.161**	0.255**	-12.270	6.163***
	(0.310)	(3.590)	(3.635)	(0.127)	(7.568)	(1.985)
Observations	10,654	10,654	10,654	9,746	8,906	12,145
R^2 (within)	0.041	0.029	0.018	0.075	0.068	0.018
	Outcomes - Restricted w/out SEPA states					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Coefficient	-0.604*	-9.130***	6.961*	0.237*	-17.525**	3.956**
	(0.312)	(3.508)	(3.743)	(0.121)	(7.546)	(2.010)
Observations	8,400	8,400	8,400	7,716	7,054	9,569
R^2 (within)	0.048	0.045	0.043	0.086	0.085	0.025

Significance levels: * = 10%; ** = 5%; *** = 1%.

Table F.3: Primary outcome results controlling for statewide planning legislation

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcomes - Restricted w/ planning index controls					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Coefficient	-0.780**	-9.232***	10.038***	0.345***	-17.206**	4.567**
	(0.306)	(3.496)	(3.555)	(0.130)	(7.484)	(1.927)
Observations	10,654	10,654	10,654	9,746	8,906	12,145
R^2 (within)	0.059	0.047	0.033	0.077	0.078	0.033

Significance levels: * = 10%; ** = 5%; *** = 1%.

Table F.4: Primary outcome results controlling for urban renewal intensity

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcomes - Restricted w/ planning index controls					
	(Log) New Units	% Apt Units (Cumul. to 2010)	% SF Units (Cumul. to 2010)	(Log) Med. Home Values	Moderate Owned Unit Shares	MLS Requirement Restrictiveness
Coefficient	-0.646**	-8.739**	10.180***	0.291**	-17.373**	4.656**
	(0.309)	(3.465)	(3.592)	(0.125)	(7.469)	(2.080)
Observations	10,654	10,654	10,654	9,746	8,906	12,145
R^2 (within)	0.063	0.059	0.045	0.082	0.075	0.033

Significance levels: * = 10%; ** = 5%; *** = 1%.