# 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 growing communities in the 1960s to hire urban planners to draft land-use plans, affected housing supply. Using newly digitized records merged with panel data across municipalities on housing and zoning outcomes, we exploit eligibility thresholds and capacity to approve funds across state agencies to identify effects. Planning assistance caused municipalities to build 20% fewer housing units per decade over the 50 years that followed. Regulatory innovation steered construction in assisted areas away from apartments and toward larger single-family homes. Textual evidence related to zoning and development politics further shows that, since the 1980s, assisted communities have disincentivized housing supply by passing on development costs to developers. These findings suggest that federal intervention in planning helped institutionalize practices that complicate community growth, with subsequent consequences for national housing affordability.

**JEL Codes:** H73, N92, P11, R31, R52
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# 1 Introduction

Throughout the 21st century, housing affordability in the United States has declined. This trend is paired with a reduced supply of smaller units available to first-time homebuyers, while renters pay increasingly large shares of their income for limited housing (Baum-Snow and Duranton, 2025). Although increasing housing supply has become a high policy priority, many adopted reforms have only gone so far as to repeal a surprising thicket of regulations applied by local governments on new development (Bronin, 2023). As late as 1960, such local policies intended to manage community growth remained rare. At that time, U.S. land use regulations did not reach "beyond the basic objectives of ... establishing minimum standards of development" (Delafons, 1960). Two decades later, numerous innovations in regulations and property law had been introduced to limit redevelopment or new construction (Schleicher (2017); Schleicher and Hills (2025)).

Multiple competing narratives exist on why U.S. residents became more skeptical of growth in their communities during that historical period (Fischel, 2004). However, we know much less about which incentives fostered local consensus around growth management; what prompted local governments to expand regulations; and how much these motives explain why parts of the country grew more housing inelastic than others (Glaeser and Gyourko, 2025). This paper evaluates the impact of one potential cause: how a 1960s policy promoting urban planning in America's rapidly growing suburbs led to growth-skeptical local planning practices.

We provide a causal evaluation of the Department of Housing and Urban Development's (HUD) "Section 701" Urban Planning Assistance Program. "701 assistance" subsidized the fixed costs of producing comprehensive plans that guide communities' desired growth trajectory, along with the regulations to enforce those plans. Our focus is on the Small Areas Program within 701 assistance, which funded urban planning assistance specifically for municipalities with fewer than 50,000 residents. Thousands of municipalities were matched with credentialed urban planners, even as federal and state agencies lacked the state capacity to check whether local plans were consistent with projected regional housing demand.

We study the program by digitizing project directories and archival records documenting which municipalities received 701 assistance, how much funding they received, and what planning products resulted from the process. We combine this with national panel data on housing supply, composition, prices, and regulatory restrictiveness to estimate how planning assistance affected subsequent development. Our preferred estimates imply that municipalities that underwent 701 assistance built 20% fewer housing units in the following decades than they would have had they not undergone the process. Planning assistance reduced apartment supply;

shifted the concentration of single-family supply toward less affordable market segments; and increased the propensity to adopt regulations in the following decades that impose delays and restrictions on new development.

We first characterize the typical municipality receiving a 701 assistance grant and the process it undertakes. At least 3,000 municipalities received 701 assistance funding, many of which had 5,000 or more residents in 1960.[1] An analysis of HUD's archival index also shows that, despite HUD's latitude in what could be provided with a 701 assistance grant, most projects ended with a "standard package" of three documents: a comprehensive plan that surveys local conditions and plans out land use for a set of population projections, and revised zoning ordinance and subdivision codes that implement desired physical planning.

When we link 701 assistance status to demographic tabulations from the 1940 Census, we find that adopting municipalities were more likely to have been higher-amenity areas than non-adopting municipalities. These differences indicate that without a research design, a policy evaluation of 701 assistance would suffer from selection bias: municipalities in high demand applied in greater proportions to the program, as their local governments had witnessed rapid population growth and were deliberating growth's costs and benefits.

We identify causal effects with an empirical strategy that combines two idiosyncratic features of how 701 assistance funds were disbursed. First, we build a control group of municipalities that were ineligible for the funds because they had more than 50,000 residents by 1960. Second, we apply a triple difference design in which the identifying variation is based on funding distribution policies of state agencies, which interacted with HUD on behalf of their municipalities. Their capacity and interest in receiving federal funds varied depending on institutional efficiencies and state-level political frictions. Agency effectiveness, defined as the rate of eligible municipalities in the state that received 701 assistance, is uncorrelated with confounders across a range of pre-period balance tests.

We apply our triple-difference design to a panel of housing market outcomes across municipalities from 1950 to 2010. Under our hypothesis that planning funded by 701 assistance increased the inelasticity of local housing supply, we expect 701 assistance to induce lower housing production. Housing prices following the policy should rise, but the magnitude depends on the local elasticity of housing demand. If growth management under 701 also complements local pressure to exclude new residents based on income, the policy's effects may be

[1] We find that about 20% of local governments in metropolitan areas, with the right to set land use regulations through zoning, took up 701 assistance by 1966. When we limit our analysis to a sample of mainly medium-sized suburbs at a reasonable distance from their metropolitan areas' downtowns, half of the remaining municipalities took up the program. Section 3.1 offers further details.

concentrated in lower-quality housing.

Our main result on housing supply uses decennial Census data on changes in the housing stock. We find that for the average municipality treated by 701 assistance, each decade following treatment in the 1960s had 18-20% fewer housing units started than the policy counterfactual. This result is robust to numerous specifications and confounding mechanisms, while an event study specification shows that the effect accelerates to 20-23% fewer units per decade after 1970.

Next, Census housing unit breakdowns allow us to examine whether the supply response is driven by specific housing types. Our first finding is that housing starts in 701-assisted municipalities shifted away from apartments, as early as the first multifamily construction boom in the late 1960s. Our second finding is that while the housing stock in 701-assisted communities shifted toward single-family homes, it also shifted toward single-family homes that were regulated to be larger than conventional postwar subdivisions. Based on a measure of minimum lot size restrictiveness introduced in Cui (2024), we find that local density regulations in 701-treated areas became more binding on the supply of smaller single-family homes from the late 1960s to the 2010s.

Turning to prices, we find increases in median house values and declines in the affordability of owner-occupied homes in 701-assisted communities. However, these results are smaller in magnitude than those for housing supply and are not estimated with a high level of statistical significance. One possible explanation for the small effect sizes is elastic demand for 701-assisted communities. In particular, supply restrictions in certain municipalities did not hinder residents from moving to nearby substitute communities. The aggregate implications of 701 assistance may not have therefore been apparent immediately after federal planning assistance's implementation. In the long run, as land suitable for development in urban areas is exhausted, substitute communities become harder to supply and affordability declines.

To understand why 701 assistance had the lasting effects on housing supply that it did, we processed unstructured text around the zoning and development processes to trace out *path dependence in regulations*. Our hypothesis is that after 701 assistance, the local permitting and planning processes of recipient cities were more dictated by advocates of additional regulations. The first set of patterns we find begins in the 1960s. In addition to added minimum lot size restrictiveness, we find municipalities eligible for 701 assistance also have more regulations today that exclude development, such as quotas on housing permits popularized only after 1970.

The second set of patterns involves the adoption of more "value capture" regulations — reg-

ulations that require developers to pay for public goods before getting building permits. We estimate 701 adopters to be 0.6 standard deviations higher on LLM-based indices of value capture prevalence. To see how the regulations play out in practice, we also analyzed newspaper reports over two and a half decades of housing development that required discretionary approval. Developers of discretionary projects, in 701 assisted cities, were 15 percentage points more likely to pay fees or offer community benefits when navigating the permitting process.

Together, the results suggest that cities that once received federally subsidized planning assistance did not stop at the planning practices recommended under the assistance program. Even if the information provided through 701 assistance became outdated, the planning regulations and processes introduced in the process evolved to include permit fees and value capture closer to the 21st century. After cities learned how to say "no" to growth after a one-time federal intervention in planning, they veered toward a broader skepticism of letting homes be built without further management.

Our results relate to three literatures that address current policy priorities. First, our results offer evidence for why policies that restricted housing supply and controlled growth diffused across local institutions, particularly those we document to have real supply consequences. In summarizing his pioneering work on the causes of growth management, Fischel (2017) emphasizes the interaction between self-interested homeowners who prioritize increasing their home values and a national conservationist movement opposed to dense development. Recent work detailing the "Abundance Agenda" has generalized this theory, explaining how interest groups since the 1970s shifted government priorities to prioritize stakeholder consensus over policy efficiency (Klein and Thompson (2025); Dunkelman (2025)).

By analyzing the role of federal urban planning assistance in localities less willing to develop housing, we highlight how interest groups motivated to block growth interact with historical policies that set up the processes that gave them the avenues to exercise influence. Federal planning assistance mattered, according to our results, by changing how regulations on development diffused to U.S. local government. Following work documenting general trends in policy diffusion (Nicholson-Crotty (2009); DellaVigna and Kim (2025)), our paper connects to a new literature that examines the causes of regulatory diffusion that affect specific industries.[2]

The persistent consequences of 701 assistance also complement recent findings based on surveys and field experiments: beliefs about who benefits from growth and supply-side reform can be internally inconsistent (Elmendorf, Nall and Oklobdzija, 2025) or embedded in social norms (Broockman, Elmendorf and Kalla, 2024). By collecting evidence on how 701 assis-

[2]Two recent examples include 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.

tance generated path dependence toward increased regulation of development, we highlight the economic consequences of establishing planning processes that must accommodate disparate beliefs about growth trajectories. Although 701 assistance had provided information and introduced a process to deliberate community planning, there was no federal intervention of the same scale in the following decades to align divergent planning practices with regional priorities.

Because we find a divergence in housing supply and land use regulations in 701-assisted municipalities that lasts until 2010, our results highlight the mechanisms through which urban planning policies generate path dependence in urban areas (Lin and Rauch, 2022). In the planning assistance case, the intervention resembles a modern practice: consultants prepare comprehensive projections of population growth and future land use approved by local constituents, and lay the groundwork for bureaucratic permitting processes. We quantify the growth in demand for these services when historical evidence suggested the demand would occur: when American suburbanites in the 1960s publicly debated the costs of suburban growth (Rome, 2001). The assistance helped formalize procedures that increased the hurdles to building new supply and increased the role of groups other than private parties in altering urban form.[3]

The rest of the paper proceeds as follows. Section 2 provides background on the Urban Planning Assistance Program and the hypotheses we test regarding how planning assistance may affect housing supply. Section 3 presents the data sources we use to measure local housing outcomes, along with stylized facts that corroborate the policy characteristics described in case studies. Section 4 explains our research design that exploits plausibly exogenous variation in 701 assistance eligibility, and Section 5 presents our main results. Section 6 uses a variety of measures on the restrictiveness of housing regulations to explore how 701 assistance produced path dependence in local planning practices. Section 7 concludes.

[3]The type of planning intervention we study complements two other types of planning policies better studied by economists. First, numerous studies demonstrate 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)). Second, we have a good understanding of how the earliest zoning restrictions had persistent effects on the neighborhoods they regulated (Shertzer, Twinam and Walsh (2018), Twinam (2018), Gallagher, Shertzer and Twinam (2024)). Such early restrictions stratified development by residential versus nonresidential uses, rather than the more sophisticated regulations introduced after the 1960s, which were intended to impose conditions on development and conduct value capture.

# 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)[4]

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).[5]

Though the initiative to apply for 701 assistance starts from local officials, the application process involves minimal communication between them an 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 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

[4]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)

[5]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.1 plots time series for the two variables side by side, also exhibiting the rise in applications following 1959.

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 management 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.[6] 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. [7] 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. [8] 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 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.

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 $A_1, A_2$.[9]

[6] 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.

[7] 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.

[8] 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.

[9] 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

The logic of Figure 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.

# 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,

attractiveness of local amenities to non-adopting cities.

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.[10]

## 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).[11] 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 adoption 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. [12] Instead, we digitized and coded the

[10] 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.

[11] 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.

[12] 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."

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 2 plots the rate at which 701 assisted municipalities received at least one document in a planning intervention category.[13] 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 2, 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 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.

[13]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.

## 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 $t$. [14]

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 Census 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

[14] 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)

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.

**Median home values.** Beginning in 1970, the Census Bureau began to tabulate home values at the municipality level. Based on a question that asks homeowners to report the amount their homes would sell for if listed, the Census tables report both median home values as well as counts of homes falling in 15-20 price bins, depending on the Census in question.

We retrieve median home value data for municipalities and express them at constant 2010 US dollar levels. For 1970, where the median home value is not reported, we use the binned home value distribution to interpolate the amount. If 50% of units are below a value bin, the interpolated value is the midpoint of the bin's range.

One limitation is that, unlike with the housing stock, the 1970 Census cannot be used to impute median home values for prior decades. Our alternative is to 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 over all owned homes matched to a particular municipality. 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.

**Housing affordability.** Our primary measure of affordability estimates how accessible a municipality is for households that can only afford lower-end homes. Using the binned distribution for house values in Census data, we calculate the 25th percentile home value in each metropolitan area in each decade. Then, we look at the share of owned units in each municipality that falls below this threshold. An increase in this outcome indicates that the municipality is becoming more affordable relative to the metropolitan area.

**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.[15] Restrictiveness is calculated decade by decade, so the underlying denominator in each period is the single-family component of the housing supply outcome.

We validate results using time-varying restrictiveness with additional information on MLS requirements coded in modern zoning ordinances. While the costs of acquiring zoning ordinances as they were written in the past are prohibitive, 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 employ the Bartik, Gupta and Milo (2024) (BGM) AI-Zoning database and extract the mean MLS requirement for each municipality, calculated over all residential zones having that requirement.

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.[16] 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.

**Historical trends for outcomes.** Figure 3 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

[15]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).

[16]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.

redeveloping lot-size-restricted parcels.

### 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 Selection Issues with 701 Takeup

To estimate how receipt of urban planning assistance affects urban growth outcomes, we could start without a research design and estimate a regression at the municipality level of 701 assistance receipt $D_i$ on long-differenced outcomes:

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

In this specification, the coefficient $\beta$ represents effects relative to a base level of 1960, and $X_{i,1960}$ represents jurisdiction characteristics that are predetermined as of 1960. Equation 1 identifies the unconfounded effect $\beta$ only if controlling on observables, and two-way fixed effects eliminates all sources of bias.

However, this specification cannot account for multiple sources of self-selection into the program. Cities experiencing an accelerating pace of suburbanization may have been more

willing to apply for 701 grants. If approval rates for 701 assistance are similar for all kinds of cities, self-selection created a positive correlation between continued growth and the probability of receiving a 701 grant.

If municipalities that entered the planning assistance program were in high demand, we should observe elevated prices in those municipalities prior to program entry. It is possible that the municipalities were already where high-income households lived, in which case high prices do not reflect high price-to-income ratios. Alternatively, municipalities may satisfy a property of Rosen-Roback spatial equilibrium, high demand for a location due to high amenity values could be capitalized through home prices instead of through wages, which would imply above-average price-to-income ratios in program participants.

We can test these hypotheses prior to the spread of 701 assistance using the 1940 full-count Census data we described in Section 3.3. Figure 4 visualizes the results, where we group 701 municipalities' 1940 demographic averages by the year the grant associated with that municipality was funded. Specifically, Panel (a) shows two facts related to our self-selection hypotheses. First, home values in most 701 municipalities before program adoption were higher than those of non-adopting municipalities. Second, comparing self-reported home values to our imputed household incomes, adopting municipalities had a price-to-income ratio of 1.8, compared with 1.68 for non-adopting municipalities.

The second fact suggests that 701 participants were more likely to be *high-amenity municipalities*. In Panel (b), we tabulate two more demographic variables: the share of 1940 residents who worked in manufacturing, which proxies if the municipality had been accessible to the working class, and the share of residents who worked in agriculture, a proxy for how undeveloped and rural the municipality was before postwar suburbanization. We observe that, prior to the expansion of 701 eligibility in 1960, adopters had a greater share of manufacturing workers than non-adopters in our sample. With accelerating suburbanization and increased funding, program participants in the 1960s became disproportionately drawn from localities that were more rural before the war. With 1961 as a dividing line, we find it plausible that selection into 701 can be split into two phases: a first phase involving existing suburbs that urban dwellers perceived to be higher amenity, followed by a second involving suburbs that were higher amenity and newly developed following World War II.

An additional set of balance tests over covariates prior to 1960, shown in Table A.2, confirms the trends in Figure 4. Adopting municipalities had higher owned home values, were larger in 1940, grew 0.06 log points faster in the 1950s, and were likelier to have a highway pass through their borders. The overall picture is that municipalities self-selected into 701 assistance

when their growth was driven by desirable amenities and when they became more accessible within the overall metro. To control for this form of selection bias in our analysis, we employ a research design that exploits policy eligibility criteria that do not depend on local government characteristics.

## 4.2 Adapting a triple difference framework

The research design we employ leverages two sources of eligibility for receipt of 701 assistance. First, we exploit state-level differences in program adoption rates that we argue reflect institutional differences in how state planning agencies administered disbursement of 701 funds, separate from any between-state factors that affect the growth and composition of suburban jurisdictions. Second, we exploit a population cutoff for 701 assistance receipt: eligibility was limited to localities with populations smaller than 50,000, as of the previous decennial Census.

To fix ideas, suppose half of the states did not approve any of their municipalities for 701 assistance, while the other half approved all interested municipalities. Whether a state approved 701 applications or not is a potential binary instrument for actual adoption. However, it would be unlikely to satisfy the exclusion restriction: for example, high-adoption states might have seen greater growth and migration on average than low-adoption states.

Instead, we construct two groups within each state: municipalities smaller than 50,000 residents in 1960 (eligible for 701 assistance) and those with 50,000 or more residents (ineligible), excluding the largest municipalities in the country.

Our outcome of interest, instead of average levels across cities or in each state, can instead be the difference in outcome trends between smaller cities that are eligible and larger cities that are not. If we estimated this trend for the states that approved no municipalities for 701 assistance, our estimate captures any systematic trends that affect small versus large municipalities within a state. The triple-difference estimator then compares the small city-large city trend for states that approved 701 assistance with the same trend for states that did not.

The key identifying assumption is 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. It might be that the identifying assumption only holds for cities not too more populous than 50,000 residents in 1960, and likewise for cities that are not too small. Under this assumption, the difference between these gaps identifies the causal effect of 701 assistance over the eligible cities that are still large enough.

Figure 5 verifies the relevance of the eligibility threshold. We split the sample into bins of equal width with respect to the running variable, the municipality's population in the 1960

Census. We then plot changes in the probability of adoption using nonparametric binned means and a LOESS regression fitted on the binned means. The figure shows a sharp drop in adoption probability around the eligibility threshold, though adoption rates are not uniformly zero above it. Our interpretation is that these outliers represent a minority of municipalities that had fewer than 25,000 residents in the 1950 Census, making them eligible for the first wave of 701 assistance funding prior to 1960.[17] The bubble sizes in the figure, which reflect municipality counts in each bin, reveal another important fact: most of the treated municipalities are well below the population eligibility threshold, with a median population of around 10,000 to 20,000 residents in 1960.

There are other reasons for preferring our triple differences approach to a regression discontinuity design, where the outcomes are either in levels or differences between periods (Butts, 2023). In a simpler regression discontinuity design (RDD) framework, effects are identified for the marginal municipality at 50,000 residents. For policy evaluation of possibly heterogeneous treatment effects, we are less interested in the effect size at a specific large city size than for a representative suburb that took up the program. Later in this section, we present a formal argument for why we can use triple difference estimates to recover a version of the average treatment effect on the treated.[18]

In practice, we implement the design by using the full variation in state-level adoption rates. We fix a year and consider every municipality approved for a 701 assistance grant up to that year. For each state $s$, we calculate the share of approved municipalities among all eligible municipalities with populations between 1,000 and 50,000 in 1960:

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

In Figure 6, we first plot the rates $\tilde{Z}_i$ for every state, using adoption status up to 1962. As we explain further in the next subsection, this year captures a snapshot of when 701 assistance demand began to accelerate, while state agencies themselves had not changed their application load management practices in response. Panel (a) shows that adoption rates are elevated in particular subregions of the United States, such as New England and the South.

[17] We found just four 701 municipalities that would never have been eligible under population eligibility: Albany, GA; Gaithersburg, MD; Town of Union, NY; and Parma, OH. All but the last municipality hosted a federal agency headquarters or military installations, 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."

[18] The method of estimation in our design also differs from standard practice for RDDs. Because not all eligible municipalities took up 701 assistance, the equivalent RDD specification is more properly a fuzzy discontinuity design. We estimate our policy parameter using regression adjustment, while the RDD specification relies on a less efficient two-stage least squares estimator.

However, within each subregion, there remains meaningful variation in adoption rates. In any region of the country, we observe at least one state with adoption rates of between 0 and 10 percent. Panel (b) shows that our preferred measure and an alternative adoption rate measure calculated as of mid-1966 (the end of our project directory data) are highly correlated but not identical.

Our main specification treats state-level adoption rates as a continuous instrument. Specifically, for municipality $i$ in state $s$ and year $t$, we estimate:

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

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

We break down the argument connecting the parameter of interest $\beta$ to a policy-relevant average treatment effect on the treated (ATT) into two parts, as we are not estimating the basic triple difference estimation with two groups (Olden and Møen, 2022). The variable $\tilde{Z}_s$ is a measure of policy "exposure," common in other empirical strategies. Treating the variable as multivalued, we can establish the following identification result:

**Assumption CI (Conditional independence of eligibility cutoff):** In a neighbourhood 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:

$$\mathbb{E}\left[Y_t^0 - Y_{t-1}^0 \middle| \text{Pop}_{1960} \in (\underline{P}, 50K]\right] - \mathbb{E}\left[Y_t^0 - Y_{t-1}^0 \middle| \text{Pop}_{1960} \in (50K, \overline{P}]\right] \perp\!\!\!\perp \tilde{Z} \middle| X.$$

**Proposition:** *Under Assumption CI and the presence of one state with $\tilde{Z} = 0$, we can identify from the data*

$$\mathbb{E}\left[Y_t - Y_t^0 \middle| Pop_{1960} \in (\underline{P}, 50K]\right],$$

*an average treatment effect on the treated if all jurisdictions in a certain population range received 701 assistance.*

*Proof.* See Appendix D. □

The proposition implies a *nonparametric* approach to using our between-state variation to

identify an ATT. In practice, we trade off functional form flexibility to ensure our estimator is sufficiently powered to detect effects. Our specification invokes the following *functional form specification* for $Y_t$, which satisfies Assumption CI:

$$\begin{aligned} Y_t \Big| \tilde{Z}, Pop_{1960} \in (\underline{P}, 50K] &= \xi_{it} + \beta z \\ \Rightarrow \mathbb{E}\left[ Y_t - Y_t^0 \Big| \tilde{Z} = z, Pop_{1960} \in (\underline{P}, 50K] \right] &= \mathbb{E}[\xi_{it} - \xi_{it}] + \beta(z - 0) = \beta z. \end{aligned}$$

It follows that we back out an ATT for municipalities close enough to the eligibility cutoff by rescaling the coefficient by the expected level of state adoption "exposure" among adopting municipalities:

$$\mathbb{E}\left[ Y_t - Y_t^0 \Big| Pop_{1960} \in (\underline{P}, 50K] \right] = \beta \int z dP(z) = \beta \times \mathbb{E}[\tilde{Z} | Pop_{1960} \in (\underline{P}, 50K]]. \tag{3}$$

To conclude this discussion, we use our panel of outcomes and a variety of covariate controls to estimate divergence in outcomes around the 701 eligibility threshold in each state. We can identify the causal effect of increased exposure to 701 eligibility, using a research design motivated by triple difference designs. Though we make an assumption that treatment effect heterogeneity is linear in exposure, that functional form assumption may well reflect the underlying data-generating process if the exposure coefficient is robust to alternative specifications. Our preferred interpretation of effects is to rescale the exposure coefficient $\beta$ by the expected exposure among the treated, which is enough to identify the ATT under substantive parametric assumptions.[19]

## 4.3 Evidence supporting cutoff conditional independence

It is not a violation of the conditional independence assumption we employ for our design if eligible municipalities in high-adoption states are on different growth trajectories than those in low-adoption states, or if municipalities below the population threshold are on different trajectories than slightly larger municipalities.[20] What is key is that we must reject other ex-

[19]The parametric assumption is not trivial, but it is analogous to the strong parallel trends assumption discussed in Callaway, Goodman-Bacon and Sant'Anna (2024), where its invocation lets the researcher identify an ATT defined over any unit receiving some "dose" of a treatment. The concept of taking a coefficient on a continuous instrument and rescaling it by an expected value was also employed in empirical macroeconomics studies Mian and Sufi (2012) and Berger, Turner and Zwick (2020), albeit using a binned distribution to estimate the expectation instead of calculating it directly.

[20]See Ortiz-Villavicencio and Sant'Anna (2025) for another exposition of why these two kinds of parallel trends assumptions are unnecessary.

planations for why a state may be both high-adoption and diverge in how smaller and larger jurisdictions around the 50,000 population threshold had grown over time. We offer arguments from both institutional knowledge — our interpretation of how the responsible state agencies processed applications based on historical sources — and balance tests that check whether the states were already diverging in urbanization patterns prior to World War II.

We find it plausible that staffing shortages in state planning agencies by their respective governments, along with idiosyncratic agency philosophies on which municipalities should receive 701 assistance, account for significant variation in adoption rates. One citation from a third-party report concludes: "there is no consistency among the states in the administration of the 701 Program." (Hammer Greene Siler Associates, 1969) The core mechanism behind why staffing matters is that a 701 project could not begin until the state agency filed with HUD the paperwork that outlined the details of the three-party contract, as defined in Section 2.2. The project then had to also wait for HUD to approve the funding, which could be delayed if growth in other states' applicants was enough to exhaust the amount appropriated for 701 assistance for that year.

Our identification argument shares similarities with how Collins and Shester (2013) exploited differences in how quickly states passed enabling legislation for federal urban renewal funds, attributing variation in their instrument not as policy reactions by rapidly growing states, but to delays attributed to legislators and judges. In the case of 701 assistance, state-level politicians designated an agency to receive and administer 701 assistance funds. In Appendix C, we detail how agencies varied greatly in where they were situated in a larger bureaucracy; the number of employees and planners on staff; and the funding states allocated to support the processing of 701 applications.

While we cannot rule out municipalities in low-adoption states taking up some other form of federal planning assistance, we think that this cannot drive our results for two reasons. First, documents provided under 701 assistance were themselves a prerequisite for applications to other federal grants. Second, as we have also shown in Section 3.1, 701 assistance in the 1970s became less focused on the built form and growth management as it moved to address a broader spectrum of planning priorities.

If we compare states with below-median or above-median 701 adoption rates, they may not satisfy covariate balance before the program went into effect. We show this is not the case, using possible confounders tabulated across municipalities using 1940s and 1950s data. Table 1 summarizes the tests we conduct, which can be split into tests on the outcome variable, as well as on other covariates, prior to policy take-up. The first test 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 pre-period 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. The two negative values in Panel (a) reflect the estimated "difference-in-difference" bias

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

over the high- and low-adoption states. If unobserved factors drive some states to have higher adoption rates and greater pre-period bias, we can reject the null hypothesis that the bias estimates differ. It is reassuring that we cannot reject the null, while noting there is a difference of -0.078 log points across groups.

In Panel (b), we reuse our tabulation of 1940 demographic characteristics by municipality, last discussed in Section 3.3. We run, for each possible confounder, a cross-sectional regression and report an interaction term between Census 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. We note that the eligible-ineligible difference in median home values is $4,500 less in high-adoption states than in low-adoption states, and Census population records suggest municipalities in high-adoption states grew 38% less in the 1950s compared to municipalities in low-adoption states.

While neither of the last two estimates is statistically significant at the 90% level, one could argue that point estimates are large enough to warrant controlling for population growth. In all our main specifications, we include the percentage change in municipality population between 1950 and 1960. All of the remaining covariates, including sociodemographic characteristics based on 1940 Census data, have small point estimates. That indicates how with our empirical strategy, states with higher 701 adoption rates did not have small municipalities more willing

[21] A map of the two groups is in Appendix Figure B.2, in which the continental U.S. is divided into two groups of 24 states.

to exclude new residents than those in low 701 adoption states.[22]

In our final analysis sample, we limit our focus to only cities with 1960 populations between 5,000 and 200,000. A concern is that this selection criterion includes cities whose populations are far away from the eligibility threshold. We note, however, that the choice of the upper and lower population bounds was data driven. In Appendix Figure B.5, we conduct a visual analysis of the residualized outcome pre-trend

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

for different binned values of 1960 population $Pop_{1960}$.

We adapt a heuristic introduced in Angrist and Rokkanen (2015); if our Assumption CI holds, then the analysis sample we use should not have relationships between the residualized outcome and $Pop_{1960}$. As seen in Figure B.5, our lines of best fit on both sides of the eligibility discontinuity are both nearly flat. We do not expand the sample to municipalities with less than 5,000 population in 1960, as the binned residuals suggests doing so would introduce a positive relationship with $Pop_{1960}$. A similar reasoning holds for why we do not consider a maximum threshold smaller than $Pop_{1960} = 200,000$.

# 5 The Growth Consequences of 701 Assistance

## 5.1 Supply Effects

We present our estimates on how 701 assistance altered housing supply, following Equation 2. Due to time variation in how well Census data covered small municipalities, we require that all municipalities used in our regressions must possess a balanced panel in the supply outcome. 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 40 kilometers from CBSAs' central business districts. The last row of Appendix Table A.1 shows that about 1,800 municipalities are eligible for 701, with around 300 ineligible municipalities in our sample.

Table 2 presents our estimates across specifications that vary in both the right-hand-side controls and the sample used. From columns (1) to (5), we add observable controls for pop-

[22]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.

ulation growth; municipality 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 order to pins down the group of treated municipalities over which our identification assumption is satisfied. Results for our preferred population thresholds, based on the analysis in Section 4.3, are given in the table's second panel.

Across all these specifications, our triple-difference coefficient stays within the range of -0.6 to -0.8. The additional controls we employ correct for positive omitted-variable bias in the estimates, while removing small municipalities with fewer than 5,000 residents in 1960 corrects for negative bias. Our preferred coefficient estimate is in Column (4) of the second panel, where the estimated triple difference coefficient is -0.675. An interpretation of this coefficient is that eligible municipalities in the first quartile state by adoption rate (Michigan) had lower housing supply than those in the third quartile state (Washington) by approximately $-0.675 \times (0.35 - 0.09) = -18\%$, due to the effects of 701 assistance.

However, our preferred interpretation is to scale the coefficient by the expected adoption rate value defined over the treated units. In our regression sample, 701 assisted municipalities have a mean adoption rate of 0.30. As such, Table 2 reports the rescaled ATT estimates corresponding to each regression specification. For our preferred estimate, the implied ATT is a 20.0% decline in new units built in the municipality every decade.

In Figure 7, we estimate results more flexibly over time by running the event study design analog to the triple-differences design:

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

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 from 1970-80 onward. In the bottom panel of Table 2, we estimate a triple-difference specification where effects in the 1960s period and the post-1970 period are estimated separately. The post-1970 period coefficient becomes more meaningful

at -0.78, or an implied ATT of 23% fewer units built per decade.

To make this result more comparable to other findings in the literature on how local institutions alter housing supply, we scale the percentage change in log units built by the average housing units built per 1,000 residents in the treated municipalities. Suppose that, for each decade, counterfactual new housing construction rates equal the average rate in states with adoption rates of zero by 1962. In the 1960s and 1970s, the average rate in those no-treatment states equals 12 to 15 annual units per 1,000 residents. Applying the 20% drop in rates from Table 2 to these no-treatment baselines, we estimate the policy effect to be a loss of, for example, $15.5 \times 20\% = 2.8$ annual units per 1,000 people in the 1970s. The no-treatment new housing rates, matching national slowdowns, fall to 7 to 8 units from the 1980s to 2010.[23] Applying the same 20% effect yield policy-induced declines of 1.5 annual units per 1,000 people in the 1980s, 2.0 units per 1,000 in the 1990s and 1.3 units per 1,000 in the 2000s.[24]

In Mast (2024), a change in the election of city councillors from at-large to ward-based systems led to 1.75 fewer annual housing units built per 1,000 people. Because our results are comparable in magnitude to that paper's findings, they suggest the information provisions related to 701 assistance could be as economically impactful as electoral systems' effects on support for growth in local political equilibria.[25] Our results for the 1970s come close to a large estimate in Ouasbaa, Solé-Ollé and Viladecans-Marsal (2025), with an increase of 10 units per 1,000 people in California after the election of a developer to a city council. However, they also show their effects are limited to development in the first term (4 years) of a councillor. On a 4-year basis, our effect estimate for the 1970s amounts to a loss of 11.2 units per 1,000 people. Effects for later decades are not as large, but continue to persist long after receipt of 701 assistance.

## 5.2 Effects on Housing Composition and Prices

In Figure 7, 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 effect up to 2010, indicated by the rightmost data point in the figure, is 13.7%.

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

[24] 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.

[25] The interaction of local political institutions with support for growth is a prominent part of recent political science research on local governance (Brouwer and Trounstine 2024).

[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 in total.

The bottom two panels examine composition changes in greater detail: we estimate the effects of 701 assistance on the share of homes built up to a year that are single-family and multifamily apartments. Although effects in the 1960s are significant at the 90% rather than 95% level, the point estimate for the apartment share of housing supply is negative over the decade, even if overall supply had not decreased as much. 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 cumulated apartment share up to 2010. Our preferred coefficient estimate is -0.077, implying an ATT of 2.3 percentage points fewer units being apartments with 5 units or more in the building. The substitute housing type seems to be entirely single-family units, as the last panels of Figures 7 and Table 3 indicate: We estimate an ATT of a 2.5 percentage point cumulative increase in the share of built units that are single-family, as of 2010.[27]

Because the changes in composition shares are small compared to the supply effect of 20%, we conclude the decline in housing supply was driven by less single-family and multifamily construction. The results indicate that, in planning their preferred communities, 701-assisted municipalities were more willing to permit single-family units than multifamily units. At an aggregate level, such permitting decisions stabilize the single-family home share in our sample after 1980: Table 3 reports the baseline rate of single-family homes in our sample to be 65%, while Figure 3 shows the stability holds in aggregate means by decade.

In Table 4, we show the effects of 701 assistance on home values and affordability. The results here are subject to a caveat first discussed in Section 3.2: we have a separate identifying assumption for price outcomes, as the only year for which we have comprehensive pre-period median home values is 1940. Comparing results across columns, we see the importance of including region fixed effects to control for a negative correlation between high-adoption Southern states and lower home values. Our preferred results imply ATTs of 6.7% higher median home values in 701-assisted municipalities, though we only estimate this at a 90% significance level from the null. While our estimates of the share of affordable owned units are negative, they do not reject the null with sufficient statistical significance.[28]

[27]The similarity in coefficient magnitudes is not mechanical. While single-family units and apartments make up most of the new units being supplied, we excluded in these categories the "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 that 701-assisted municipalities tried to plan for more or less of either type.

[28]As with Figure 7, we show graphs of the analogous event study estimates for these outcomes in Appendix Figure B.6. This figure also includes additional results for the degree of bunching on all MLS requirements, including smaller ones below 7,500 square feet.

We prefer to interpret these results not as highly informative point estimates, but rather as evidence against the hypothesis of inelastic demand for 701-assisted municipalities. For example, the upper bound of the 95% confidence interval for our home values coefficient is 0.575. Since this upper bound estimate remains lower in magnitude than the supply effects reported in Table 2, the result provides evidence against the hypothesis that demand for 701-assisted municipalities is on average inelastic or unit elastic: the upper bound of the confidence interval for $\beta^{price}$ remains smaller in magnitude than $\beta^{supply}$.

We also appear to be underpowered in detecting a decline in the affordable home share of 701-assisted municipalities — even if our point estimates are consistently negative. We tentatively conclude that when 701-assisted municipalities restricted supply most severely, they remained sufficiently similar to unassisted places so that "priced out" households could relocate elsewhere. However, long-run land supply is inelastic and pinned down by geographic fundamentals. The consequences of not building these communities more densely may be felt decades later, once constructing new communities far from the central business district becomes prohibitively costly. A small change in local unaffordability relative to the metropolitan area does not preclude 701-driven supply restrictions from causing a rise in metropolitan housing price levels later on.

## 5.3 Robustness Checks

**Robustness to sample and population threshold selection.** Appendix Table A.3 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.[29]

In contrast to the first two panels of Appendix Table A.3, which vary how much we include data away from the discontinuity, the last panel of the Table conducts a "donut" robustness

[29] 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.

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.4 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.

**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 would introduce a correlation between the supply-side effects of planning assistance with positive local demand shocks. A regression using variation in 701 adoption, as shown in Equation 1, would thus yield upward-biased estimates of supply and price effects.

Appendix Table A.5 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 housing supply is close to perfectly elastic in the counterfactual, 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 ($0.296 - (0.200) = 0.496$ log points) relative to the bias in median prices ($0.114 - 0.060 = 0.054$ log points).

**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 A.6, we add two predetermined state-level controls: 1940 manufacturer share and Black resident share in in-

corporated municipalities, tabulated from the full count data in Section 3.3. In Table A.7, 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 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 A.8 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.

**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 B.7, 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.[30]

# 6 Local Practices Driving Growth Management

In Section 5, we presented results on decreased housing supply and divergent housing market outcomes after 701 assistance. To understand whether local interest groups changed the regulatory mechanisms they use following 701 assistance, we employ textual data processed by large language models (LLMs) to extract regulatory characteristics. Having observed a decrease in supply, it is plausible that the role of profit-maximizing residential developers diminished in 701 assistance areas, a deviation from "Growth Machine" models of local political economy developed around this time (Logan and Molotch, 1987).

We consider a variety of regulatory mechanisms, not all of which are recorded in a single document such as a zoning ordinance. We are generally interested in any ways that local residents, or the municipality planners themselves, constrain the provision of housing by the private sector, or impose requirements during the permitting process outside of those listed under zoning — a change from the *by-right zoning* that were prevalent before 701 assistance.

## 6.1 Exclusionary Zoning Practices Since 1960

Section 5.2 provided evidence that 701-assisted jurisdictions lowered the supply of lower-quality housing segments, such as multifamily apartments. If regulations following 701 assistance shift supply away from smaller single-family homes to larger ones, we should observe an increase in our bunching measure of lot size restrictiveness described in Section 3.2. By construction, the measure sums up bunching around minimum lots of 7,500 square feet or more. Under those MLS requirements, conforming development would have 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 the lot size restrictiveness outcome, which is available as a panel. The coefficient for our preferred specification reflects an average of an additional 5.1 percentage points of development per decade that was restricted by minimum lot sizes. Rescaling this coefficient effect, we conclude that 701-assisted municipalities are estimated to have an average of 1.5 percentage points more units restricted

[30] 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.

in the post period, to the order of 30% of the baseline restrictiveness level.[31]

Additional minimum lot size requirements in the revised zoning ordinance after 701 assistance could have remained until the present day. For robustness, we also use a measure of MLS requirement prevalence that is not based on composition, but based on the letter of the regulatory text themselves. The BGM AI-Zoning database provides summary statistics across residential zoning districts defined in processed ordinances, such as the average minimum lot size requirement across districts. Because the AI-Zoning database captures only the cross section of 2010s zoning ordinances, we run the following regression over one time period, and 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 bottom panel of Table 5 shows the results from the cross-section regression. In our preferred specification, the coefficient on our 701 eligibility variable implies an ATT of a 0.52 acre increase in the area's mean MLS requirement. The coefficient is identified only under a stronger assumption on the conditional independence of outcome levels, rather than the parallel bias assumptions of our Assumption CI. We interpret the direction and magnitude of the effects, however, as consistent with our earlier results showing that 701 assistance led to more restrictive MLS requirements.

More detail on MLS requirement implementation is noted in the upper right panel of Appendix Figure B.6, plotting coefficients from an event study specification as in Equation 4. As with the results we showed for housing stock composition outcomes, the effects of 701 assistance on MLS restrictiveness are large and positive as early as the 1960s and remain similar in magnitude until the 2000s. The constancy of these dynamic estimates suggests that the MLS requirements in 701-assisted municipalities remained unchanged in the following decades, or any revisions up to 2000 were consistent with zoning districts or land use plans established soon after receipt of 701 assistance. [32]

We verify that, in designating land under more restrictive MLS requirements, municipalities reallocated that land away from dense residential uses. Appendix Table A.9 presents a collection of regressions that test for persistent associations between higher MLS restrictive-

[31] We also estimated effects for increasingly higher MLS requirements. 701 assistance had no statistically significant effect on the rate of units bound by MLS of at least 1 acre, but had a strong effect of 2 percentage points on the rate of units bound by MLS of at least 20,000 square feet ($\sim 1/2$ acre).

[32] In the earlier robustness checks shown in Appendix Tables A.4 to A.8, the effects of 701 assistance on MLS restrictiveness are shown in the far right column. The magnitude of the effects remain similar across specifications, and the coefficient magnitude is biased downwards in the endogenous treatment variable specification of Appendix Table A.5.

ness and housing market outcomes analyzed in Section 5. Across municipalities $i$, we estimate 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. We estimate regressions separately for data on MLS restrictiveness during the 1960s and for measured restrictiveness prior to that decade, as earlier restrictiveness should reflect zoning regulations that were not designed during 701 assistance.

We find that, in periods after the 1960s, past MLS restrictiveness in the same municipality is highly correlated with restrictiveness rates even two decades later. Contemporary or past restrictiveness has statistically significant and strong relationships with fewer apartment units in the housing stock, higher median home value growth, and a lower supply of affordable housing units relative to levels in the wider metropolitan area. We also find that, while the MLS restrictiveness for homes built prior to the 1960s is correlated with fewer apartment units, it is uncorrelated with house prices and affordability measures in the decade that follows.

Our results on MLS restrictiveness suggest that 701-assisted municipalities changed their planning of MLS requirements to reduce density in their built environment, with consequences for the provision of affordable housing units soon after adoption. However, the effect of 701-assistance on MLS requirements alone cannot explain the full magnitude of the effects in Section 5. For example, Table 3 shows that the impact of 701 assistance on the cumulative share of apartments in the housing stock was a decline of 2.3 percentage points. Scaling the association of Appendix Table A.9 with the impact of 701 assistance on MLS restrictiveness results in a mediating impact of $-0.3\% \times 1.52 = -0.5\%$ percentage points, less than a quarter of the overall effect.

We consider the possibility that land use planning after 701 assistance, in addition to zoning more land for restrictive lot sizes, also reduced the amount of land where different residential uses were permitted. In theory, we can measure the impact on both the extensive margin — whether the municipality has adopted regulations that make building certain housing types infeasible — and the intensive margin, the share of land zoned to enforce those regulations. However, a full analysis of intensive margin can only be sourced from a panel of zoning maps, which remain infeasible to collect for the entire country.

Empirical analysis is possible with outcomes on the extensive margin. We employ both the AI-Zoning database and the Mleczko and Desmond (2023) National Zoning and Land Use Database (NZLUD). We drill down to the level of individual regulations that, in these databases,

are coded as binary outcomes, except for two measures of height controls. 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.[33]

We run the cross-sectional design of Equation 5 on 22 separate regulations encoded in our zoning regulation databases. In Appendix Figure B.8, we output coefficients representing linear probability odds of adoption from a one-unit change in the interaction term. Panel (a) plots eight regulations that exclude or complicate certain types of residential development and were discussed by planners by the mid-1960s; therefore, they were likely to have been amended or introduced by any zoning ordinance prepared in the 701 process. For each regulatory outcome, we find null results for the effect of 701 assistance on adoption. These results imply that 701 assistance did not impose extensive margin restrictions on particular residential uses, or that any such restrictions were only temporary and cannot fully explain the persistent housing composition effects.

In Panel (b), we plot 14 regulations that were popularized following the 1970s. Their adoption was unlikely to have been directly implemented during the 701 assistance process; instead, they would have been adopted as a result of political decisions after the comprehensive plan. A surprising result is that there are no statistically significant effects of 701 assistance on discretionary procedures: the number of non-zoning local bodies who must approve a multifamily project before a permit is granted. However, we find statistically significant increases in growth control requirements, which set quotas on permits issued over a period of time (Frieden, 1979).

Our multiple sources of evidence paint a fuller picture of the progressive adoption of zoning practices after 701 assistance, as well as how those dynamics explain the acceleration of housing supply declines after 1970. In the years following the comprehensive planning process aided by 701 assistance, recipient municipalities abided by a land use plan that would have reallocated land to lower-density single-family housing development compared to the counterfactual. What would have been learned after the comprehensive planning process are additional controls that do not exclude high-density residential uses, but allow a municipality to set quotas on development beyond what is stated in the zoning ordinance.

Although we argue that 701 assistance shifted the incentives of multiple interest groups to establish more growth-skeptical behavior, the assistance program could have complemented other growth-skeptical motives discussed in prior work. Frieden (1979) and Fischel (2004) emphasize the collaboration between motivated local residents and environmental conservation groups, such as the Sierra Club, in lobbying for new land use regulations. However, the

[33]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.

fragmented nature of regulatory passage at the locality level, as well as an inability to pass regional development boundaries around existing density, meant that U.S. planning institutions, after 701, assistance could have further contributed to the urban sprawl conservationists wanted to reduce (Nechyba and Walsh (2004); Ehrlich, Hilber and Schöni (2018)).

## 6.2 Value Capture Practices Since 1980

A set of planning practices, distinct from use and density restrictions in zoning ordinances, began to spread from the 1970s onwards. In Appendix Figure B.9, we adapt a method used by Fischel (2017) to track the popularization of planning policies over time. Using the Google Ngram American English database, we produce time series of keywords related to specific urban planning concepts.

In Panel (a), we reproduce the time series from Fischel (2017) for terms related to growth control — regulations that extend beyond zoning and set quotas and requirements for new development. In Panel (b), we track the growth of a separate set of *value capture* policies — where new development is allowed in exchange for the developer providing local public goods (Manville (2021); Lebret, Liu and Valentin (2025)). We see steady rises from the late 1970s to the present in the mentions of value capture terminology. Examples include inclusionary zoning, a form of value capture intended to provide additional social housing, and traffic impact studies, a protocol whose intent is to decide the amount of transportation infrastructure to be financed through value capture.

We provide evidence on whether planners and local interest groups in 701-assisted places were more willing to design policies that induce value capture. A relevant framework for this scenario is the "Leviathan hypothesis" of local government (Brennan and Buchanan, 1980). Recent empirical tests of the hypothesis, as in Diamond (2017), find higher tax burdens by local governments when inelastic housing supply hinders alternative low-tax areas to which households can relocate. Applied to housing construction, recent reported costs associated with value capture are on the order of 8-12% of house values, impacting development viability (Emrath, 2021). In addition, the process of negotiating and finalizing the level of value capture creates a regulatory delay that negatively affects the scale and completion rates of new development (Manville et al. (2023); Gabriel and Kung (2025)). Despite the potential effects, 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 prevalence of value capture practices based on the text of zoning ordi-

nances. 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. In addition, the AI-Zoning database provides an "overall index" that sums up the standardized values of individual regulations' adoption and intensity. A higher value reflects more anomalous cases in which many restrictive regulations were passed and recorded in the zoning ordinance.

Table 6 estimates the effects of 701 assistance eligibility using the AI-Zoning indices, after standardizing each outcome variable. We estimate the cross-sectional variant of our empirical strategy, using Equation 5. We first find that 701 assistance explains a 0.37 standard deviation increase in the overall index.[34] Corroborating results in Section 6.1, we find that 701 assistance explains a 0.28 standard deviation increase in the database's exclusionary zoning component. According to Bartik, Gupta and Milo (2024), this principal component captures the presence of large minimum lot size requirements, as well as restrictions on non-single-family residential uses.

We find the largest effect size of 701 assistance, however, on the value capture index in the AI-Zoning dataset. 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[35]; senior-only restrictions for units; and wetland restrictions.[36] 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.

Evidence from zoning ordinances remains indirect, compared to evidence showing that planners and 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

[34] An analogous specification we ran, using the overall index in the NZLUD, outputs a similar effect size of 0.37 standard deviations, using the specification in Column (3).

[35] 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.

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

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 C, 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.

After LLM processing, we produce a dataset covering 1,840 developments reported over the years 1983–2009. Appendix Figure B.10 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 8, 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 8, 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 8 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 8

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.[37] 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.

## 6.3 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

[37] 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.

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.9, 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 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.

The other theory is based on a *strategic* framework. In this framework, the comprehensive planning process introduced by 701 repeats itself over time, which facilitates repeated bargaining between local urban planners and residents. One of the goals of the 701 program, expressed by HUD, was to begin an ongoing planning process. Some of our findings support this view. For example, one of our results from our newspaper analysis suggests that in municipalities eligible for 701 assistance, projects that were under review were more likely to have low-income units. Especially after the introduction of the 1986 federal low-income housing tax credit (LIHTC) program, developers could more easily finance the construction of housing units rented out to lower-income households in high-opportunity suburban neighborhoods (Soltas (2024); Cook, Li and Binder (2025)).

However, if low-income housing can be built somewhere by-right in municipalities that had no such units, it could introduce a wedge in local development politics. When parties empowered by 701 assistance negotiate, local planners' have a positive bliss point for low-income units (as they recognize regional needs for that type of housing), but local residents receive a disutility if they live next to any low-income housing. The two groups may reach an equilibrium in which low-income units are approved, but planners agree to compensate local residents for the utility loss. In this sense, path dependence in regulations is *extrinsic* path-dependent change under Acemoglu, Egorov and Sonin (2021): change was conditional on a national shock that increased credit access for low-income housing developers. The compensation could involve passing the cost of public good provision onto developers rather than residents through higher property taxes, or implicitly through growth controls that would prevent deviations from residents' ideal levels of community population.

# 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 local 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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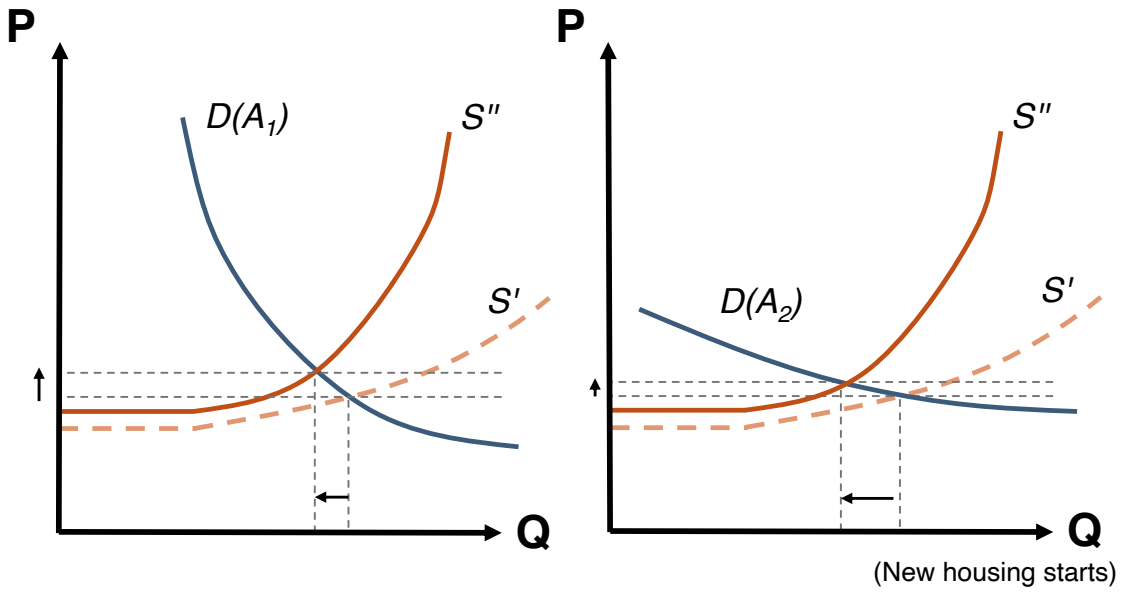
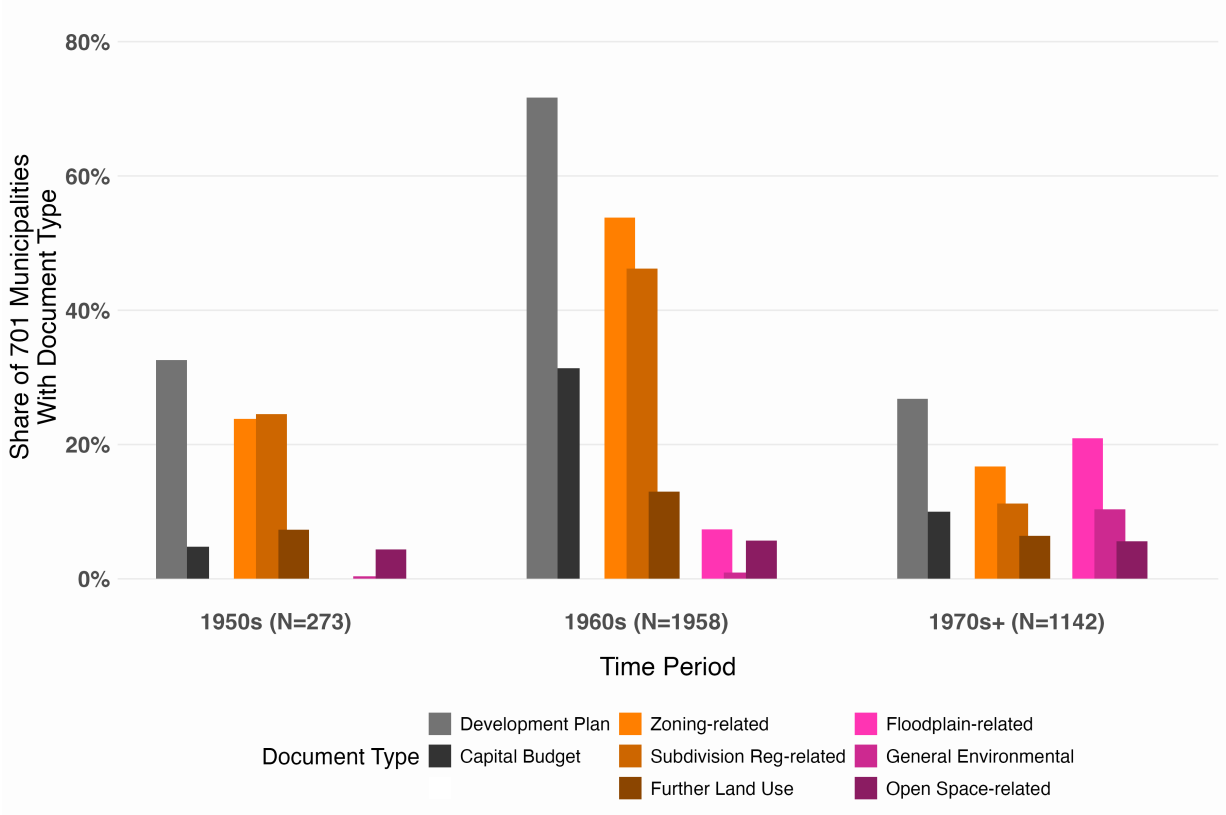
Figure 1: Housing market outcomes after local growth management



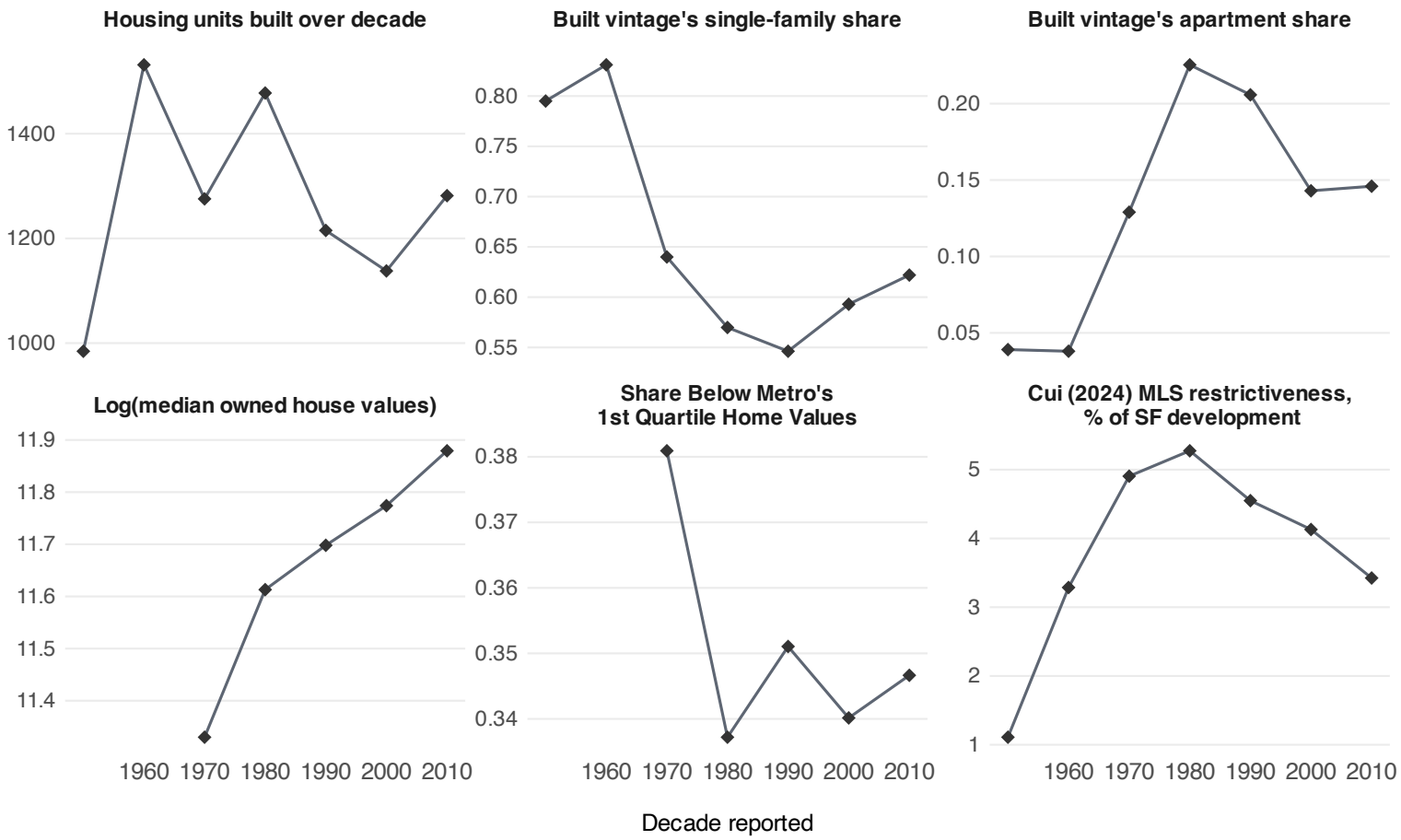

*Notes:* This figure plots two ways market outcomes can change following regulations that manage continued supply of a specific housing segment in a municipality, based on the analysis of 701 planning assistance consequences in Section 2.3.

Figure 2: 701 assistance project deliverables over time



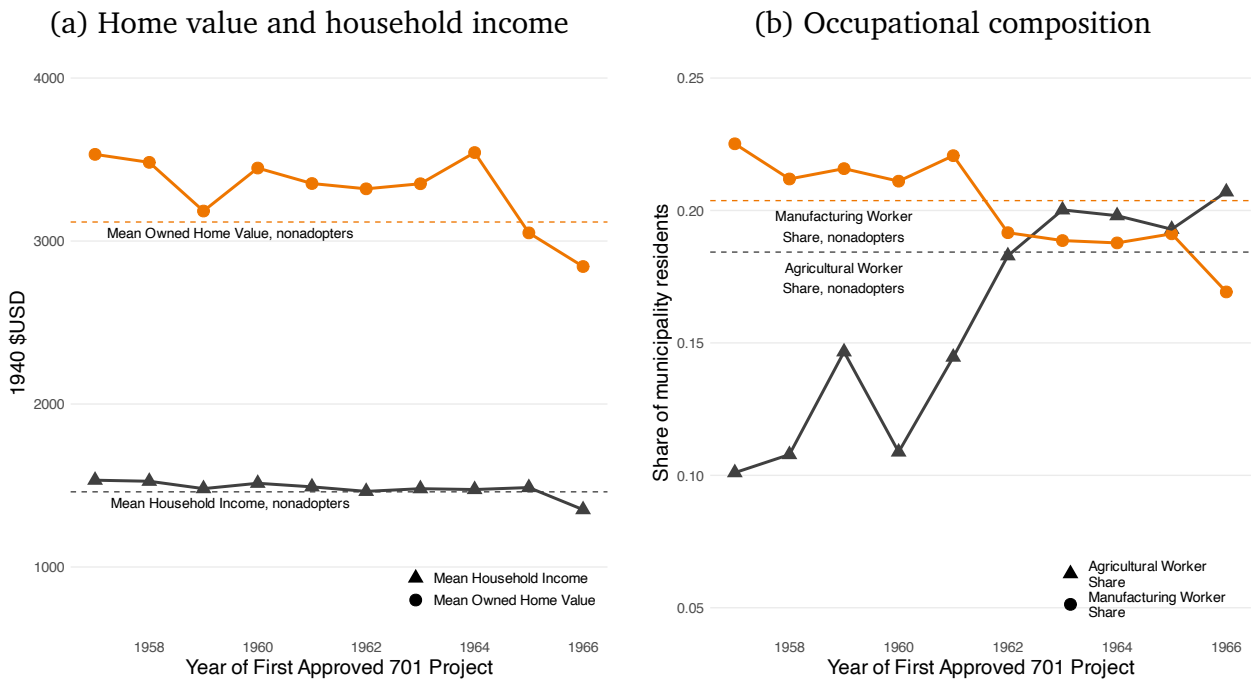

*Notes:* This figure tabulates the share of 701 adopter municipalities that can be linked to the HUD database of 701 assistance documents (Section 3.1) and had a document classifiable under one of eight types. We stratify documents by the decade in which they were produced and output results by three 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 C
*Sources:* Section 701 document index, HUD/National Archives; HUD/HHFA 701 project directories.

Figure 3: Decadal trends since the postwar era for primary outcomes



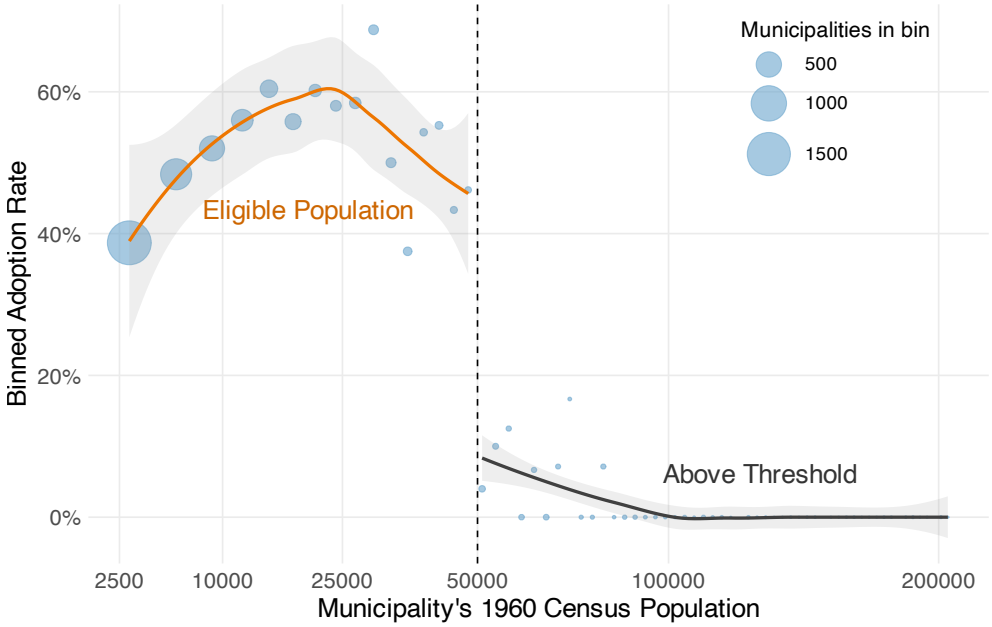

*Notes:* This figure plots decennial mean values for housing market outcomes we study in Section 5. Averages are tabulated over all incorporated municipalities with available data, without weighing for population and excluding municipalities under 1,000 population in 1960. For housing supply outcomes, "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. 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 4: 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.

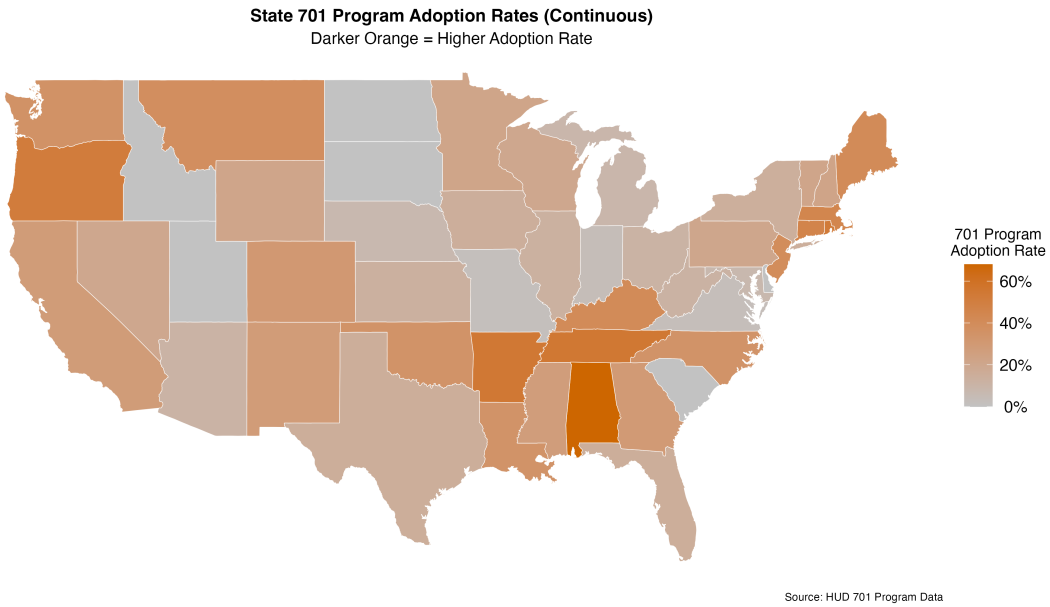
Figure 5: 701 program eligibility around the 1960 population cutoff



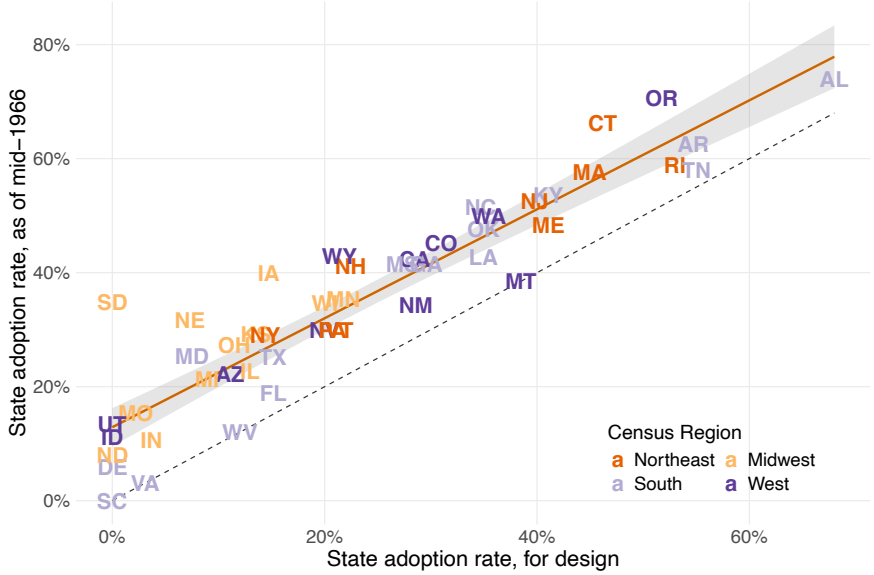

*Notes:* This figure plots the adoption rate of federal urban planning assistance by 1960 Census population for U.S. incorporated municipalities, defined by their presence in the "Section 701" project directories. We construct equal-width bins in 1960 population and plot each binned average as a point; 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 6: State-level adoption of 701 assistance

(a) Map of adoption variation, as of 1962

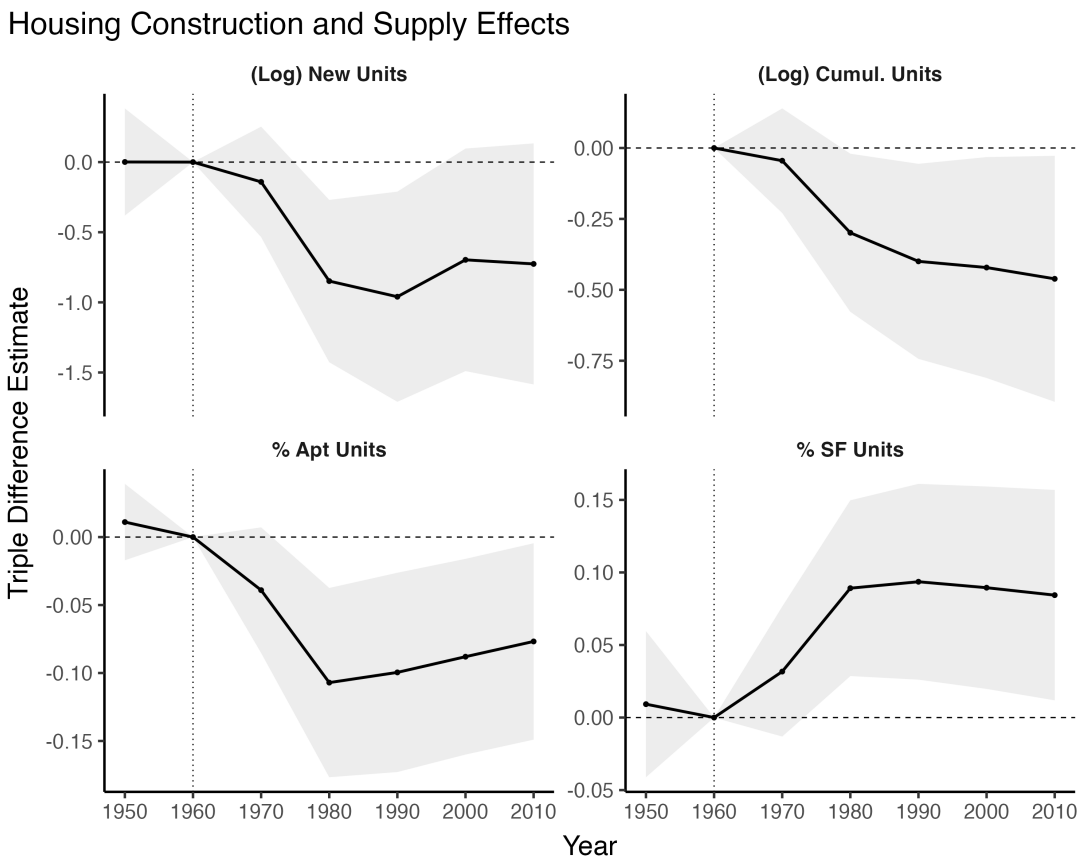



(b) Scatterplot of adoption variation over time



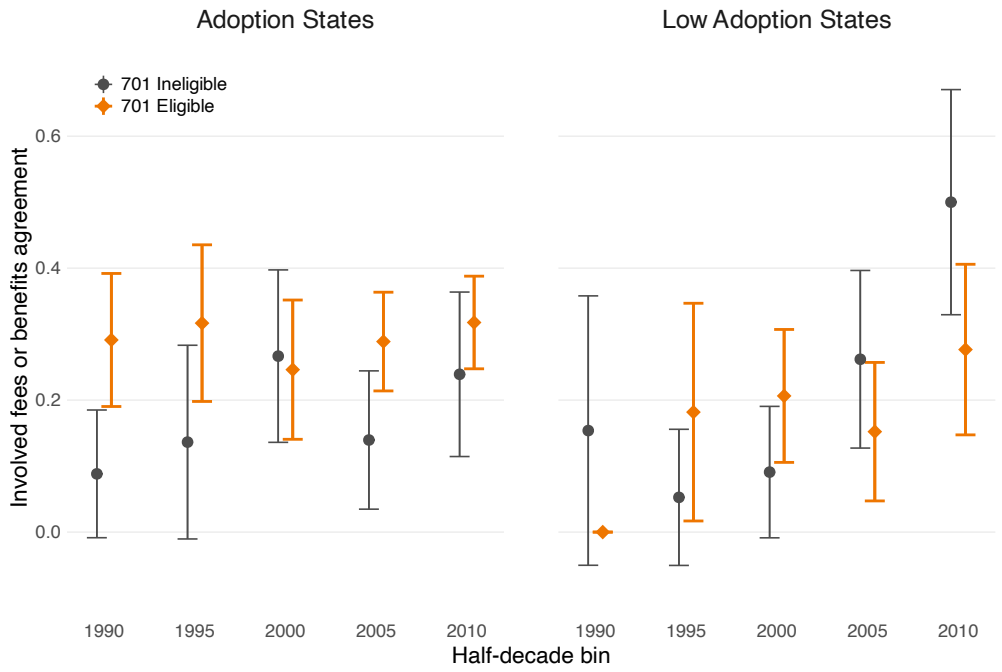

*Notes:* Both panels plot variation in the state adoption rate variable $\tilde{Z}_s$, which we use as a measure of 701 policy exposure. Adoption rates take incorporated jurisdictions with zoning power on development as the denominator, that also had at least 1,000 residents in 1960. The numerator sums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

Figure 7: 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 8: 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 C. 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

| **Panel (a): Testing pre-policy parallel bias** | | |
|---|---|---|
| Exposure Group | Coefficient (SE) | F-test |
| 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, 1940 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 $ | 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 ($\leq 200k$)** | | | | |
| Coefficient | -0.673** | -0.688** | -0.867*** | -0.769** | -0.658** |
| | (0.320) | (0.322) | (0.323) | (0.308) | (0.286) |
| Implied ATT | -0.199 | -0.204 | -0.257 | -0.228 | -0.195 |
| Observations | 14,630 | 13,727 | 13,720 | 13,720 | 13,720 |
| $R^2$ (within) | 0.029 | 0.117 | 0.049 | 0.049 | 0.049 |
| | **(Log) New Units - Restricted (5k–200k)** | | | | |
| Coefficient | -0.689** | -0.619* | -0.775** | -0.675** | -0.597** |
| | (0.328) | (0.329) | (0.329) | (0.317) | (0.297) |
| Implied ATT | -0.204 | -0.183 | -0.229 | -0.200 | -0.177 |
| Observations | 10,185 | 9,681 | 9,674 | 9,674 | 9,674 |
| $R^2$ (within) | 0.021 | 0.055 | 0.042 | 0.040 | 0.041 |
| | **(Log) New Units - Effects after 1970** | | | | |
| Coefficient | -0.813** | -0.746** | -0.902** | -0.784** | -0.692** |
| | (0.374) | (0.375) | (0.375) | (0.356) | (0.334) |
| Implied ATT | -0.241 | -0.221 | -0.267 | -0.232 | -0.205 |
| Observations | 10,185 | 9,681 | 9,674 | 9,674 | 9,674 |
| $R^2$ (within) | 0.024 | 0.057 | 0.044 | 0.044 | 0.045 |
| 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 | -0.074** | -0.074** | -0.077** | -0.086** |
| | (0.035) | (0.035) | (0.037) | (0.035) |
| Implied ATT | -0.022 | -0.022 | -0.023 | -0.025 |
| Baseline mean | | 0.193 | | |
| Observations | 9,746 | 9,746 | 9,746 | 9,746 |
| $R^2$ (within) | 0.036 | 0.036 | 0.027 | 0.028 |
| | **% SF Units (Cumul. to 2010) - Restricted** | | | |
| Coefficient | 0.081** | 0.081** | 0.084** | 0.093** |
| | (0.036) | (0.036) | (0.037) | (0.037) |
| Implied ATT | 0.024 | 0.024 | 0.025 | 0.028 |
| Baseline mean | | 0.649 | | |
| Observations | 9,746 | 9,746 | 9,746 | 9,746 |
| $R^2$ (within) | 0.041 | 0.041 | 0.017 | 0.019 |
| 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) |
|---|---|---|---|---|
| | **Med. Home Values - Restricted** | | | |
| Coefficient | -0.029 | -0.034 | 0.225* | 0.202* |
| | (0.145) | (0.143) | (0.134) | (0.122) |
| Implied ATT | -0.008 | -0.010 | 0.067 | 0.060 |
| Observations | 9,329 | 8,890 | 8,890 | 8,890 |
| $R^2$ (within) | 0.043 | 0.069 | 0.066 | 0.061 |
| | **Share Below Metro's 1st Quartile Home Values - Restricted** | | | |
| Coefficient | -0.067 | -0.112 | -0.103 | -0.083 |
| | (0.077) | (0.076) | (0.080) | (0.080) |
| Implied ATT | -0.020 | -0.033 | -0.030 | -0.025 |
| Baseline mean | | 0.275 | | |
| Observations | 7,448 | 7,076 | 7,076 | 7,076 |
| $R^2$ (within) | 0.037 | 0.082 | 0.087 | 0.091 |
| 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 Single-family Regulatory Restrictiveness

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **MLS Requirement Restrictiveness - Restricted** | | | |
| Coefficient | 5.869*** | 5.799*** | 5.150*** | 4.697** |
| | (1.903) | (1.877) | (1.887) | (1.973) |
| Implied ATT | 1.737 | 1.716 | 1.524 | 1.390 |
| Baseline mean | | | 5.155 | |
| Observations | 10,185 | 9,674 | 9,674 | 9,674 |
| $R^2$ (within) | 0.011 | 0.015 | 0.012 | 0.005 |
| | **BGM Mean Lot Size Across Districts - Restricted** | | | |
| Coefficient | 0.755*** | 0.693*** | 0.516*** | 0.356* |
| | (0.186) | (0.196) | (0.187) | (0.195) |
| Implied ATT | 0.223 | 0.205 | 0.153 | 0.105 |
| Baseline mean | | | 0.499 | |
| Observations | 889 | 837 | 837 | 837 |
| $R^2$ (within) | 0.064 | 0.095 | 0.047 | 0.039 |
| 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 the cross-sectional data on average minimum lot size requirements, in acres, over residential districts, based on 2010s zoning data in Bartik, Gupta and Milo (2024). 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*** | 1.503*** | 1.242*** | 1.102*** |
| | (0.402) | (0.402) | (0.384) | (0.400) |
| Implied ATT | 0.473 | 0.445 | 0.368 | 0.326 |
| Observations | 1,882 | 1,771 | 1,771 | 1,771 |
| $R^2$ (within) | 0.099 | 0.111 | 0.026 | 0.020 |
| | **BGM Exclusionary Zoning Component - Restricted** | | | |
| Coefficient | 1.280*** | 1.216*** | 0.958*** | 0.807*** |
| | (0.315) | (0.320) | (0.271) | (0.279) |
| Implied ATT | 0.379 | 0.360 | 0.283 | 0.239 |
| Observations | 1,882 | 1,771 | 1,771 | 1,771 |
| $R^2$ (within) | 0.117 | 0.131 | 0.057 | 0.046 |
| | **BGM Value Capture Component - Restricted** | | | |
| Coefficient | 2.377*** | 2.263*** | 2.020*** | 1.776*** |
| | (0.436) | (0.437) | (0.443) | (0.472) |
| Implied ATT | 0.704 | 0.670 | 0.598 | 0.526 |
| Observations | 1,882 | 1,771 | 1,771 | 1,771 |
| $R^2$ (within) | 0.121 | 0.131 | 0.035 | 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 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*** | 0.122** | 0.152*** | 0.135** |
| | (0.055) | (0.057) | (0.058) | (0.059) |
| Baseline mean | | | 0.246 | |
| Observations | 1,062 | 1,017 | 1,017 | 1,017 |
| $R^2$ (within) | 0.012 | 0.016 | 0.023 | 0.023 |
| | **Opposed By Local Planners - Restricted** | | | |
| Coefficient | 0.010 | -0.006 | 0.009 | 0.008 |
| | (0.058) | (0.059) | (0.061) | (0.063) |
| Baseline mean | | | 0.181 | |
| Observations | 1,062 | 1,017 | 1,017 | 1,017 |
| $R^2$ (within) | -0.001 | 0.002 | 0.006 | 0.004 |
| | **Incorporates Low-income Units - Restricted** | | | |
| Coefficient | 0.160 | 0.170* | 0.154* | 0.177** |
| | (0.099) | (0.093) | (0.080) | (0.082) |
| Baseline mean | | | 0.364 | |
| Observations | 1,062 | 1,017 | 1,017 | 1,017 |
| $R^2$ (within) | 0.038 | 0.046 | 0.029 | 0.028 |
| 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).