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# Zoning Reforms and Housing Affordability: Evidence from the Minneapolis 2040 Plan\*

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## Abstract

In 2018, Minneapolis became the first U.S. city to eliminate single-family zoning through the Minneapolis 2040 Plan, with a central focus on improving housing affordability. Using a synthetic control approach, this paper finds that the reform reduced the growth of housing costs over the subsequent five years: home prices were 16–34% lower and rents 17.5–34% lower than a counterfactual Minneapolis. Placebo tests show these declines were the steepest among 83 donor cities ( $p=0.012$ ). The effects are consistent across multiple robustness exercises and are not the result of new housing supply, but are likely due to weakened housing demand.

**Keywords:** zoning reform, land use regulation, house prices, rental prices, synthetic control.

**JEL Codes:** R52, R30, R58

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# 1 Introduction

Cities across the United States are grappling with a growing housing crisis: only 35 out of every 100 low-income households have access to affordable rental units nationwide (National Low Income Housing Coalition, 2024). Home ownership has also become increasingly out of reach for modest income households, with the median sale price for a single-family home now 5.6 times higher than the median household income - a record high since the 1970s - reflecting the sharp decline in housing affordability (Hermann and Whitney, 2024). In response to these pressures, researchers and policymakers have increasingly pointed to zoning reform as a promising tool to expand housing supply and improve affordability (Manville et al., 2020; Wegmann, 2020). Although a growing number of state and municipal governments have introduced reforms to relax residential density restrictions, widespread resistance from NIMBY (Not In My Backyard) groups has made it difficult to implement such changes at scale (Brown and Glanz, 2018). Moreover, despite the policy momentum, there remains a limited body of empirical evidence evaluating the effectiveness of zoning reforms in reducing home and rental prices.

Minneapolis, MN serves as a compelling case study for assessing the impact of zoning reform on housing affordability. In December 2018, the City Council adopted the Minneapolis 2040 Plan - a comprehensive policy framework centered on land use reform - which took effect January 1, 2020. The Minneapolis 2040 plan demonstrates the city's effort to, in the words of Mayor Jacob Frey, "dismantle generations of intentional segregation" (City of Minneapolis, 2024). This plan abolished single-family-only zoning citywide, allowing up to three housing units by right on lots that were previously restricted to one or two units. It also removed minimum parking requirements and promoted higher density development in downtown areas and along major transit corridors. As a bold and highly scrutinized policy experiment, the Minneapolis zoning reform has garnered national attention and sparked considerable debate. Liang et al. (2024) attribute the city's relatively slow rent growth and increasing housing affordability to the implementation of the Minneapolis 2040 Plan. Conversely, Kuhlmann (2021) find that home prices increased after the plan was first

passed in 2018, particularly in comparison to un-upzoned parcels in neighboring cities.

In this paper, we evaluate the effect of the Minneapolis 2040 Plan on home and rental prices in the city using a synthetic control approach. We construct a donor pool of 83 control cities, from which we create a synthetic Minneapolis that replicates the home and rental prices of actual Minneapolis prior to the policy intervention. The analysis reveals that the zoning reform significantly reduced home and rental price growth in Minneapolis. The treatment effect is most pronounced for condos, as well as for smaller and more modestly priced homes. Placebo tests highlight that Minneapolis’s home and rental price trajectories are the lowest among all donor cities, and that they are statistically different from the “treatment effects” of non-treated cities ( $p=0.012$ ). To further explore the policy mechanism, we investigate the relationship between the zoning reform and new residential construction, finding that the reform, if anything, cause a slow-down in new construction. Instead, the reduction in home and rental price growth stems from a softening of housing demand, which is likely the result of altered expectations about the housing market.

## 2 Literature Review

Zoning regulations play a central role in shaping the supply, composition, and affordability of housing in the United States. Local governments use zoning codes to regulate density, aesthetics, and structure types. Among these, single-family-only zoning has been a predominant feature of the postwar American urban landscape, restricting the construction of multi-unit housing in vast swaths of residential land. A substantial literature highlights the exclusionary effects of these regulations, which have historically been employed as tools to preserve neighborhood homogeneity and exclude low-income and minority households (Berry, 2001; Rothwell and Massey, 2009; Trounstein, 2020; Whittemore, 2021). By capping the number of permissible housing units in high-demand areas, exclusionary zoning imposes supply-side constraints that contribute to higher housing prices and reduced affordability. Pendall (2000) documents that restrictive zoning disproportionately deters Black and His-

panic residents, reinforcing racial segregation. Similarly, Manville et al. (2020) argue that it is time to end single-family zoning for “its explicitly classist and implicitly racist motivations.”

In response to mounting concerns over housing affordability, policymakers have increasingly turned to zoning reform as a potential supply-side intervention. Various local, municipal, and state governments, along with think tanks, advocate for zoning reform as effective policy tools to encourage new construction and address affordability issues (The White House, 2016; California Department of Housing and Community Development, 2021; Vermont General Assembly, 2023). Multiple states like Colorado, Oregon, and Vermont have passed state-level legislation to remove single-family-only zoning in urban areas and allow duplexes by right in originally single-family-only zones in recent years. Municipalities across the US have also passed a wide range of zoning reforms, from relaxing parking requirements to encouraging ADU development, to enhance housing affordability.

A large body of economic literature contends that restrictive land-use regulations suppress housing supply and inflate prices (Quigley and Rosenthal, 2005; Glaeser and Ward, 2009; Chakraborty et al., 2010). Single-family-only zoning and policies alike, in theory, limit the number of housing units allowed in a given area and constrain the natural response of the market to growing demand. Glaeser et al. (2006) find that areas with more single-family-only zones are associated with less new construction in the Greater Boston area. Other regulatory barriers such as parking requirements and septic rules also discourage real estate developers from building new construction and slow down the growth of the housing supply (Kok et al., 2014). Liberalizing the artificial cap on housing construction will encourage new development and helps alleviate supply-side pressures in the housing market. Glaeser and Gyourko (2018) argue that single-family-only zoning effectively functions as a tax on new housing construction, creating negative externalities in the housing market and restricting the growth of the housing stock.

Some studies reach a general consensus that places with more regulations that limit the density of housing units have less new construction and higher home prices,

compared to places with fewer regulations (Ihlanfeldt, 2007; Glaeser and Ward, 2009). However, most of this work relies on cross-sectional comparisons between metropolitan areas at a single point in time or over limited periods (Glaeser et al., 2006; Glaeser and Ward, 2009; Kok et al., 2014). Cross-sectional studies are limited in terms of identifying the casual relationship between zoning reform and local housing prices.

More recent work has turned to quasi-experimental methods to evaluate the impact of zoning reforms. However, empirical evidence remains mixed, particularly on the question of whether up-zoning drives home prices upward or downward. A growing number of studies have examined the potential for zoning liberalization to induce speculative investment, increase land values, and accelerate displacement (Mast, 2019; Davis, 2021). Wolf-Powers (2005) finds that up-zoning in New York City contributed to a wave of speculative redevelopment, replacing industrial uses with luxury residential developments. In some cases, such speculation accelerates gentrification and displaces incumbent renters (Freemark, 2023). Also, up-zoning has been associated with a decline in racial diversity, with Liao (2022) documenting that renters in up-zoned neighborhoods are significantly more likely to relocate compared to those in otherwise similar neighborhoods. While up-zoning may increase the affordable housing stock, the benefits are often unevenly distributed, frequently at the expense of displacing long-term, low-income renters (Freemark, 2023).

Empirical assessments of recent zoning reforms across international and domestic contexts further underscore the heterogeneity in outcomes. In Auckland, New Zealand, the city government up-zoned more than three-quarters of the city in 2016 to encourage higher density residential development. While the reform successfully stimulated new construction, up-zoned parcels experienced a significant appreciation in property values — ranging from 1.5 to 4.2 percent — with underdeveloped properties realizing the largest gains (Greenaway-McGrevy et al., 2021; Greenaway-McGrevy and Phillips, 2023). In contrast, Anagol et al. (2021) find that a large-scale zoning reform in Brazil led to an increase in the housing stock and a modest decline in home prices in the long run. In the US context, recent studies report similarly mixed effects. Freemark (2020) find that transit-oriented up-zoning in Chicago resulted in a 15 percent increase in the value of treated parcels relative to non-upzoned

parcels. Dong (2025) show that density up-zoning in Portland, Oregon, led to a substantial increase in new construction in up-zoned areas, although with limited impact on property values. Early assessments of the Minneapolis reform, such as Kuhlmann (2021), document a 3 to 5 percent increase in property values one year after the city council passed the up-zoning ordinance in 2018, with the effects more pronounced in less expensive and underdeveloped neighborhoods.

Most empirical studies evaluating the effects of up-zoning rely on difference-in-differences (DiD) designs, typically comparing up-zoned parcels to non-upzoned parcels within the same city or to comparable properties in nearby cities. However, these strategies face several identification challenges. Zoning reforms are rarely implemented randomly, and pre-treatment trends between treated and control groups may differ. Intra-city comparisons may also underestimate the treatment effect by failing to account for potential spillovers — where upzoning in one neighborhood alters market dynamics in adjacent areas. Furthermore, the literature has disproportionately focused on property values, while the impact of up-zoning on rent remains significantly understudied. A notable exception is Büchler and Lutz (2021), who examine the effects of up-zoning in Zurich and find an increase in housing supply but no statistically significant change in rent levels. They highlight the need for more attention to this dimension of housing affordability. While Glaeser and Gyourko (2002) suggests that increasing supply should put downward pressure on rents, especially in low-income neighborhoods, the affordability impact depends on the nature of the new housing stock. If new development targets higher-income households, up-zoning may facilitate gentrification and displacement rather than meaningfully expand affordable rental options (Davis, 2021; Liao, 2022).

Zooming into Minneapolis, early evaluations of the city’s upzoning efforts include Kuhlmann (2021), which documents a 3 to 5 percent increase in property values one year after the Minneapolis City Council passed the upzoning ordinance in 2018, with larger gains in less expensive and underdeveloped neighborhoods. However, this study uses the 2018 ordinance passage as the intervention point, two years prior to the actual implementation of the Minneapolis 2040 zoning reform in 2020. As a result, the estimated effects may reflect anticipatory responses rather than the full treatment

effect, as the policy had yet to be implemented or fully internalized by market participants. Another relevant study by the Federal Reserve Bank of Minneapolis (Federal Reserve Bank of Minneapolis, 2021) tracks changes in new construction and rental prices through a public data dashboard. Using a synthetic control approach, the study finds a slight increase in rental prices in Minneapolis relative to its synthetic counterpart, although the effect is not statistically significant. This analysis, however, is limited to data through 2022 and relies on annual median rent figures from the ACS, which may obscure more granular, intra-year market dynamics. To build on these efforts, we use monthly data to track changes in home and rental prices over five years following the reform, allowing for a more detailed assessment of both short- and medium-term policy impacts.

This study contributes to the existing literature in three ways. First, it leverages a synthetic control methodology to the evaluation of zoning reform, offering an alternative a DiD approach that is better suited to settings with a single treated unit and no obvious comparison city (Abadie et al., 2010, 2015). Second, the study extends the temporal horizon of prior evaluations by examining medium to long-term effects of Minneapolis’s elimination of single-family zoning (Kuhlmann, 2021). It adds unique quantitative evidence to the debate on the effects of zoning reforms on housing supply and home prices (Freemark, 2020; Anagol et al., 2021; Greenaway-McGrevy and Phillips, 2023; Dong, 2025). Third, the study includes an analysis on the effect of up-zoning on rental prices, helping to fill an important gap in the literature and inform discussions about how zoning reform affects affordability for renters (Glaeser and Gyourko, 2002; Büchler and Lutz, 2021).

### **3 Institutional Background: Minneapolis 2040 Plan**

Minneapolis is the largest city in the state of Minnesota, with a population over 425,000 in 2020. It is one of the seventeen members of the Metropolitan Council, the regional government and metropolitan planning committee of the Twin Cities seven-county metropolitan area. Under the Metropolitan Land Planning Act, each



municipality in this region must update its comprehensive plan every ten years in alignment with the Metropolitan Council’s regional development guide Thrive MSP 2040 (Metropolitan Council, 2014). In response to this mandate, the Minneapolis City Council developed the Minneapolis 2040 plan, a comprehensive land use and development framework with an explicit focus on equity. The plan is organized around seventeen goals, one of which is to ensure access to affordable and accessible housing. Its overarching objective is to “undo barriers created by a history of policies that have prevented equitable access to housing, jobs, and investments, resulting in significant racial disparities” (City of Minneapolis, 2019).

The planning process began in 2016 with extensive public outreach, including community events and surveys to engage residents and stakeholders (City of Minneapolis, 2016). A draft of the plan was released in March 2018, and the proposed elimination of single-family-only zoning became a focal point of public discourse. The draft received tens of thousands of public comments - both in support and opposition - throughout the spring and summer. On December 7, 2018, the City Council voted 12–1 to approve the draft plan and submit it to the Metropolitan Council. The plan received formal approval on September 25, 2019, and went into effect on January 1, 2020.

A cornerstone of the Minneapolis 2040 plan is a series of zoning reforms intended to increase housing supply and options, particularly in low-density residential areas. Most notably, the plan eliminated single-family-only zoning citywide. Parcels previously zoned exclusively for single-family or two-family homes are now permitted to accommodate up to three dwelling units “by right.” Prior to this reform, approximately 70% of the city’s residential land was restricted to single-family homes. In theory, this change tripled the number of allowable units per parcel, thereby expanding housing choice and enabling “gentle density.” Importantly, this zoning liberalization was accompanied by updated built-form regulations that maintain the visual and physical character of existing neighborhoods. Two- or three-family dwellings in these zones must comply with the same bulk requirements (height, setbacks, lot coverage) as one-family houses, preserving neighborhood scale. In particular, new buildings are limited to 1 to 2.5 stories in height, ensuring that triplexes resemble large single-family

homes rather than apartment buildings. This approach aims to promote incremental density without significantly altering neighborhood aesthetics.

In addition to changes in low-density neighborhoods, the plan up-zoned downtown areas to allow for higher-density residential development and designated transit corridors for mid-density housing. These spatial strategies reflect a broader goal of increasing housing supply near job centers and public transportation, improving both affordability and access. Another key component of the Minneapolis 2040 plan is the introduction of inclusionary zoning. Developers of new multifamily buildings with 20 or more units must either reserve a portion of the units as affordable to low-income households or contribute to the city’s affordable housing fund through an in-lieu fee. The plan also eliminated citywide minimum off-street parking requirements for new developments. This reform, which took effect in May 2021, reduces the amount of land needed for construction and lowers development costs, especially in high-demand areas where vacant land is more scarce and expensive.

Overall, the Minneapolis 2040 plan represents a comprehensive policy package aimed at enhancing Minneapolis’s housing affordability. Rather than targeting a single neighborhood or regulatory barrier, it introduces a suite of reforms - including zoning changes, parking deregulation, and affordability mandates - that are designed to work in tandem. Due to the interconnected nature of the Minneapolis 2040 plan’s components, isolating the effect of a single policy - such as the elimination of single-family zoning - without considering the influence of concurrent reforms would overestimate the effect of triplex allowance on housing affordability. Therefore, our empirical strategy treats the Minneapolis 2040 plan as a bundled intervention. Using its implementation in 2020 as a quasi-natural experiment, we define the city of Minneapolis as the treated unit and construct a synthetic control to evaluate the policy’s cumulative impact on housing outcomes.

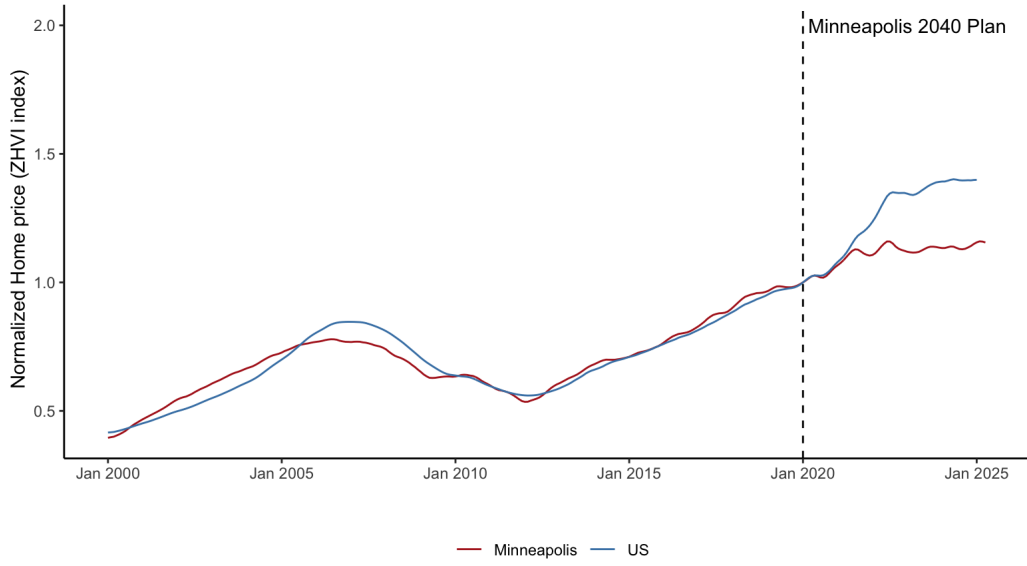
## 4 Data

### 4.1 Housing Market data

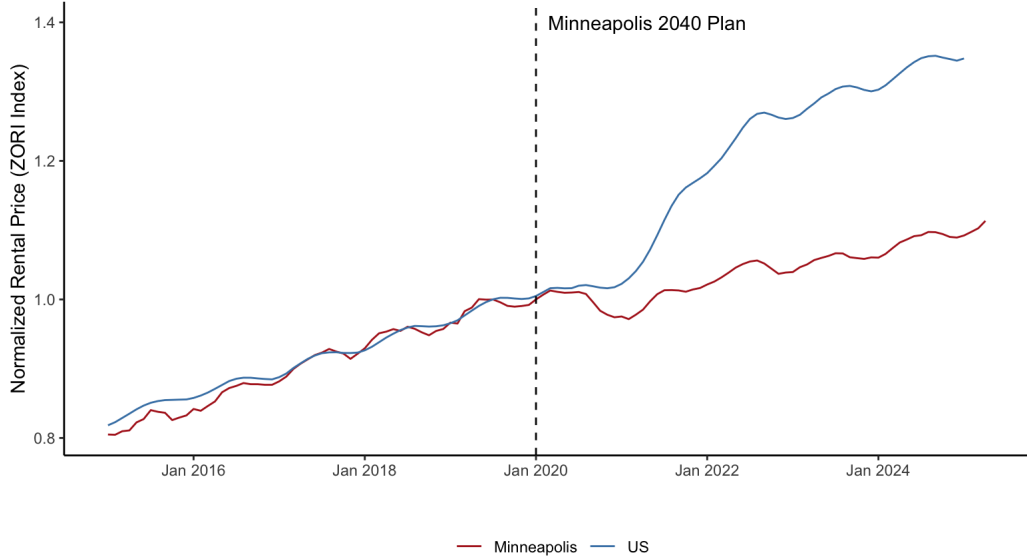
We use Zillow’s Home Value Index (ZHVI) to measure monthly changes in residential property values at the city/place level. The level of the price index is calculated by taking an average of the Zestimate in the area, where the Zestimate is a proprietary machine learning model that reflects a home’s real-time market value. The growth in the price index is calculated by taking a weighted appreciation of all properties within the area. The ZHVI index is designed to capture price changes in a typical home and reflects the typical value of homes in the 35th to 65th percentile range. Zillow also estimates changes in home values for top (65th to 95th percentile range) and bottom tier (5th to 35th percentile range), as well as by home types (single-family homes, condo/coops, homes with 1, 2, 3, 4, 5+ bedrooms). The ZHVI index is seasonally adjusted and smoothed using a 3-month moving average. The dataset begins in January, 2000 and is updated monthly; it is available at various geographic levels, including MSA, county, city, and ZIP code.

We use Zillow’s Observed Rent Index (ZORI) to measure monthly changes in rents at the city/place level. The ZORI is a repeat-rent index, tracking changes for individual units that have remained in the market across multiple time periods, and it is calculated as a weighted mean of listed rents within the 35th to 65th percentile ranges for all homes and apartments in a given region. Zillow re-weights the index based on structure type, year of construction, and year of rental to ensure it presents not only properties listed on Zillow but the broader market in general. ZORI is available from January, 2016 to present, and it is available for most medium to large cities.

To give readers a model-free sense of the data, we plot the ZHVI and ZORI for Minneapolis and the entire United States over their respective sample periods. To more clearly see the changes in these series in the post-2020 period, we normalize them to their respective values on January 2020, which are plotted in Figure 1.



(a) Homes Values (ZHVI)



(b) Rental Prices (ZORI)

Figure 1: Zillow data for home and rental prices, Minneapolis vs. U.S., normalized to January 2020 values.

The trends in home and rental prices in Minneapolis and the United States in Figure 1 generate qualitatively similar results to our more rigorous estimates presented

below. Although the post-2020 differences in Figure 1 should not be interpreted as causal, since Minneapolis could be systematically different from the nation, these results highlight that the general effects we estimate for Minneapolis are not an artifact of a specific statistical model.

In addition to Zillow housing data, we incorporate city-level housing market indicators from Redfin, a real estate brokerage that aggregates data from local Multiple Listing Services (MLS). Redfin provides a monthly panel of housing market characteristics at the city/place level. The median sale price reflects the median final transaction price of homes sold in a given month and serves as a complementary indicator to Zillow’s Home Value Index (ZHVI) for capturing trends in home prices. We also utilize a set of auxiliary housing market metrics, including the average list-to-sale price ratio (the average of each home’s sale price divided by its list price), housing inventory, new listings, median days on market, and total home sales - all of which are key indicators of housing demand and market tightness. Redfin housing market data are available monthly from 2010 onward, with comprehensive coverage of cities in the donor pool beginning in 2015.

## **4.2 Building Permits Survey**

The Building Permits Survey (BPS), conducted by the Census Bureau, provides monthly statistics on privately-owned residential construction permits at the national, state, and local levels. The BPS records the total number of construction permits issued and also disaggregates them by structure type, including one-unit, three-unit, four-unit, and five-plus unit buildings. For locations lacking reported data, the BPS imputes statistics based on historical construction trends. We utilize the city-level data on monthly construction permits from the BPS to analyze changes in new residential construction.

### 4.3 City-level Covariates

We use city-level covariates from the American Community 5-year estimate (2010-2019) on demographic and economic factors. We also incorporate Local Area Unemployment Statistics (LAUS) from the Bureau of Labor Statistics (BLS). The data is available on a monthly basis and it is available for all cities with a population greater than 25,000. These variables are used for fitting a synthetic control for Minneapolis. The specific covariates used in constructing synthetic Minneapolis are reported in Table A.1.

## 5 Methodology

We use a synthetic control method to estimate the effect of zoning reform on housing affordability in Minneapolis by comparing it to a weighted combination of cities that did not undergo similar reforms. Synthetic control is an extension of the difference-in-differences approach, developed by Abadie and Gardeazabal (2003), and it is widely used in the economics literature for evaluating the impact of policy interventions (Abadie et al., 2010; Pinotti, 2015; Cunningham and Shah, 2018). The motivation behind synthetic control is that when only one or a few units are treated, a weighted average of untreated units - rather than any single control unit - can provide a more accurate counterfactual (Abadie, 2021). Following the methodology outlined in Abadie et al. (2010) and Abadie et al. (2015), we construct a synthetic Minneapolis that approximates the pre-treatment characteristics of the city prior to the zoning reform to examine the effect of the reform on housing affordability. The remainder of this section outlines the theoretical framework and details the construction of the donor pool and synthetic Minneapolis.

## 5.1 Synthetic control framework

Suppose there are  $J$  units in the study, indexed by  $j = 1, 2, \dots, J$ . The first unit,  $j = 1$ , is the treated unit, and the remaining units,  $j = 2, \dots, J$ , are untreated. The data spans  $T$  periods and the treatment occurs at  $T = T_0$ . For each unit  $j$  and each time period  $t$ , we observe the outcome  $Y_{jt}$ . We also observe a set of predictors of the outcome for each unit  $j$ ,  $X_{1j}, \dots, X_{kj}$  which will be used to construct the synthetic counterfactual.  $Y_{1t}^I$  denotes the potential outcome of the treated unit with treatment, and  $Y_{1t}^N$  denotes the potential outcome in the absence of treatment. Hence,  $\tau_{1t}$  denotes of the treatment effect of the policy intervention at time  $t$  ( $t > T_0$ ).

$$\tau_{1t} = Y_{1t}^I - Y_{1t}^N. \quad (1)$$

Since  $Y_{1t}^I$  is observed in the data, the main question lies in the estimation of the counterfactual  $Y_{1t}^N$ . A synthetic control is formally defined as a “weighted average of units in the donor pool”, as specified in equation (2).

$$\hat{\tau}_{1t} = Y_{1t} - \sum_{j=2}^J w_j Y_{jt}, \quad (2)$$

where  $w_j$  is the weight attached to unit  $j$ . Therefore, the core question amounts to finding the optimal weight  $w^*$  so that the synthetic control approximates the trajectory of the treated unit in the absence of policy intervention when  $t > T_0$  and minimizes the difference between the treated and synthetic unit when  $t < T_0$ . Abadie and Gardeazabal (2003) find the optimal set of weights by minimizing the mean square prediction errors (MSPE) of this synthetic control with respect to  $Y_{1t}^I$  in pre-intervention periods. Abadie et al. (2015) also develop a cross-validation method to choose the optimal weight, which uses predictors in the training period to find the optimal weight that minimizes MSPE of the synthetic control over the validation period. The cross-validation method provides a way to assess how well the synthetic control constructed from a training period performs in a validation period prior to treatment, thereby addressing concerns about overfitting (Abadie, 2021). By compar-

ing the synthetic control unit with the treated unit during the validation period, we build confidence that the synthetic control unit well-approximates the pre-treatment trajectory of treated unit out-of-sample. We adopt the cross-validation approach throughout the main results of the paper since the out-of-sample validation explicitly speaks to the quality of the donor pool well as the reliability of the synthetic control unit.

## 5.2 Constructing the Synthetic Minneapolis

Following the theoretical framework outlined above, we construct a synthetic version of Minneapolis that closely replicates the city’s housing price trends in the pre-intervention period to examine the effect of the zoning reform. Because there is no natural donor pool for a city-level synthetic control, we compile a donor pool of other U.S. cities based on a combination of selection criteria, outlined by the Federal Reserve Bank of Minneapolis (2021), and additional constraints we introduce to ensure comparability<sup>1</sup>:

1. Cities must have a total population between 150,000 and 2,000,000.
2. Cities that have undergone state or municipal-level zoning reforms related to density requirements, minimum lot sizes, or similar measures are excluded.<sup>2</sup>
3. Cities with substantially different pre-intervention housing markets compared to Minneapolis are excluded (specifically, those with a Zillow Home Value Index

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<sup>1</sup>Federal Reserve Bank of Minneapolis published a data tool to track outcomes from the City of Minneapolis’ 2040 Plan in 2024. They used the synthetic control method to create a counterfactual Minneapolis. They selected their donor cities based on the following criteria: population between 150,000 and 2,000,000; exclusion of cities that adopted similar zoning reforms (e.g., Portland, OR) or may experience spillover effects (e.g., St. Paul, MN); inclusion of principal cities only (excluding suburbs); and consistency in census-defined place boundaries from 2010 onward. These criteria yielded a donor pool of 126 U.S. cities.

<sup>2</sup>We exclude all cities in California, Maine, Montana, Oregon, Vermont, and Washington, as these states have enacted statewide legislation eliminating single-family-only zoning and permitting higher-density development on parcels previously limited to single-family homes. We then use the Zoning Reform Tracker from the Othering & Belonging Institute (2023) to identify and exclude cities that have implemented major municipal-level zoning reforms.



(ZHVI) in December 2019 below \$100,000 or above \$400,000).

4. Cities that were not consistently classified as census-designated places throughout the study period (2010–2025) are excluded.
5. Cities without complete home price and rental price data—measured via the ZHVI and ZORI index—across the full study period are excluded.<sup>3</sup>

Using these selection criteria, we identify a donor pool of 83 cities across the United States. To construct the synthetic control, we divide the pre-intervention period into two sub-periods: a training period (2010–2015) and a validation period (2015–2019). Because the rental price index (ZORI) is not consistently available until April 2016 for most cities, we adjust the training and validation periods accordingly: training period from April 2016 to June 2018, and validation period from July 2018 to December 2019. Using predictors from the training period, we estimate the optimal set of weights that minimize the root mean squared prediction error (RMSPE) of housing prices during the validation period. The use of out-of-sample validation could address concerns about overfitting, especially in the context of larger donor pools. We then apply these weights to construct a synthetic Minneapolis for the post-intervention period—representing the counterfactual trend in the absence of reform.

The cities used to construct synthetic Minneapolis and their respective weights are reported in Table 1. Note that only four cities receive positive weights in each synthetic control unit, despite the relatively large donor pool. To address concerns about robustness and representativeness, below we conduct a subset analysis by systematically varying the donor pool with different geographic, socioeconomic, and demographic criteria and re-estimating the model. The subset analysis tests the sensitivity of the results to the specific composition of the synthetic control unit and ensures that the estimated treatment effect is not overly reliant on a few idiosyncratic control units. Table A.2 in the Appendix reports the covariates used to construct the baseline synthetic control unit and the corresponding weights assigned to each variable in

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<sup>3</sup>We exclude cities from the donor pool if they have more than five months of missing home or rental price data during the study period. For cities with five or fewer months of missing data, we interpolate the missing values using a three-month rolling mean.

both models.

<b>Panel A: Home Price (ZHVI) Model</b>	<b>Weight</b>
Dallas, TX	0.360
Fort Lauderdale, FL	0.264
Gilbert, AZ	0.037
Madison, WI	0.338
<b>Panel B: Rent (ZORI) Model</b>	<b>Weight</b>
Miami, FL	0.038
Fort Lauderdale, FL	0.067
Dallas, TX	0.231
Aurora, CO	0.664

Table 1: Synthetic Control Weights by City: Panel A (Home Price Model), Panel B (Rent Model)

Table A.3 in the Appendix compares the pre-treatment characteristics of Minneapolis and its synthetic counterpart. Panel A presents results for the home price model, while Panel B covers the rental price model. In each cases, the synthetic control unit provides a reasonable approximation of outcome of interests during the pre-treatment period. In Panel A, the synthetic Minneapolis closely matches the actual Minneapolis in terms of population, housing units, home ownership, and the proportion of single-family housing in the housing stock. In Panel B, the match is similarly strong across key demographic and housing stock variables, although some discrepancies remain for variables like poverty and home ownership.

## 6 Results

### 6.1 Impact on Home and Rental Prices

Figure 2 compares monthly rental prices in Minneapolis and its synthetic counterpart from 2016 to 2025. The synthetic control closely tracks Minneapolis’s rental prices before the zoning reform took effect in January 2020. The first dashed line

separates the pre-intervention period into training and validation phases; the close match during the validation period supports the robustness of the synthetic control. Both series exhibit similar patterns over time, though with varying magnitudes. For the first seven months after the reform, the difference between the two remains small. Starting in August 2020, however, rental prices in synthetic Minneapolis increase more rapidly. By early 2021, the gap becomes notable and continues to widen throughout the study period. Two years post-reform, synthetic Minneapolis's rent is 18.9% higher, and by January 2025, the gap reaches approximately 17%. Between 2020 to 2025, rental price at synthetic Minneapolis grew at an annual rate of 5.6% whereas actual rent at Minneapolis grew at only 1.8%, which suggests that the zoning reform has a substantial effect in moderating the rental price growth.

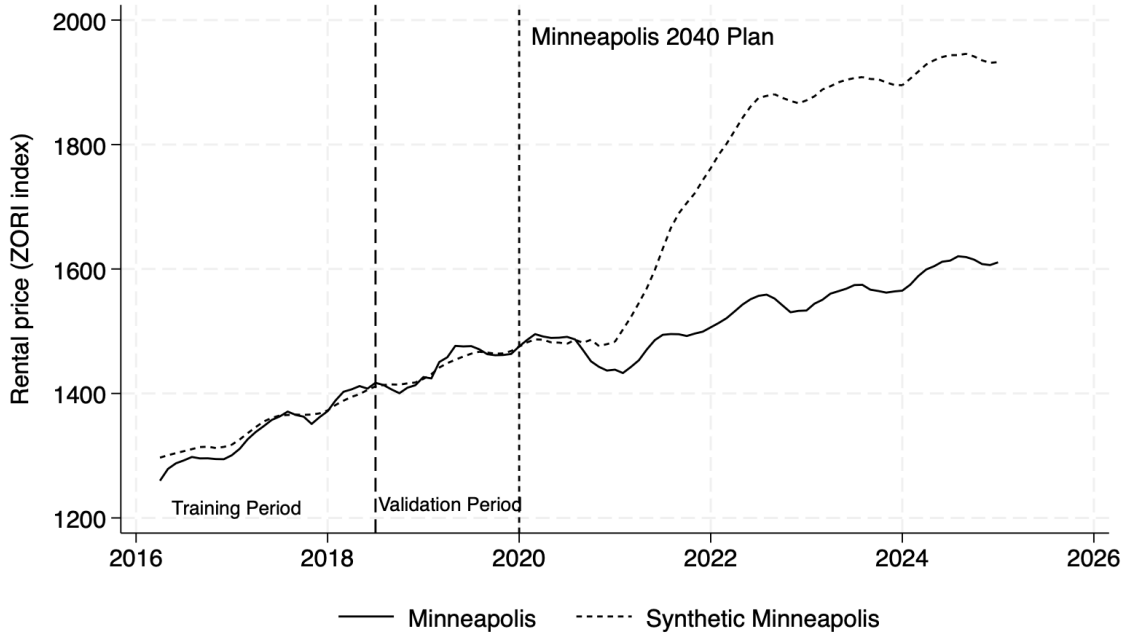


Figure 2: Monthly trend in rental price (ZORI index) Minneapolis vs. synthetic Minneapolis, 2016-2025

Figure 3 presents the monthly home price of Minneapolis and its synthetic counterpart from 2010 to 2025. Again, the synthetic Minneapolis closely reproduces the home price for Minneapolis in the pre-intervention period. The home price of syn-

thetic Minneapolis diverges from the actual Minneapolis starting in 2021. The estimated gap between actual and synthetic Minneapolis continued to grow and reached its maximum at around \$76,000 by the end of 2022. At that point, home prices in synthetic Minneapolis were about 24.9% higher than those in actual Minneapolis. This difference remained relatively stable through the end of the study period. Overall, the evidence suggests that the zoning reform had a significant effect in slowing home price growth: over the five-year period, home prices in synthetic Minneapolis increased by approximately 47%, compared to only 15.5% in actual Minneapolis.

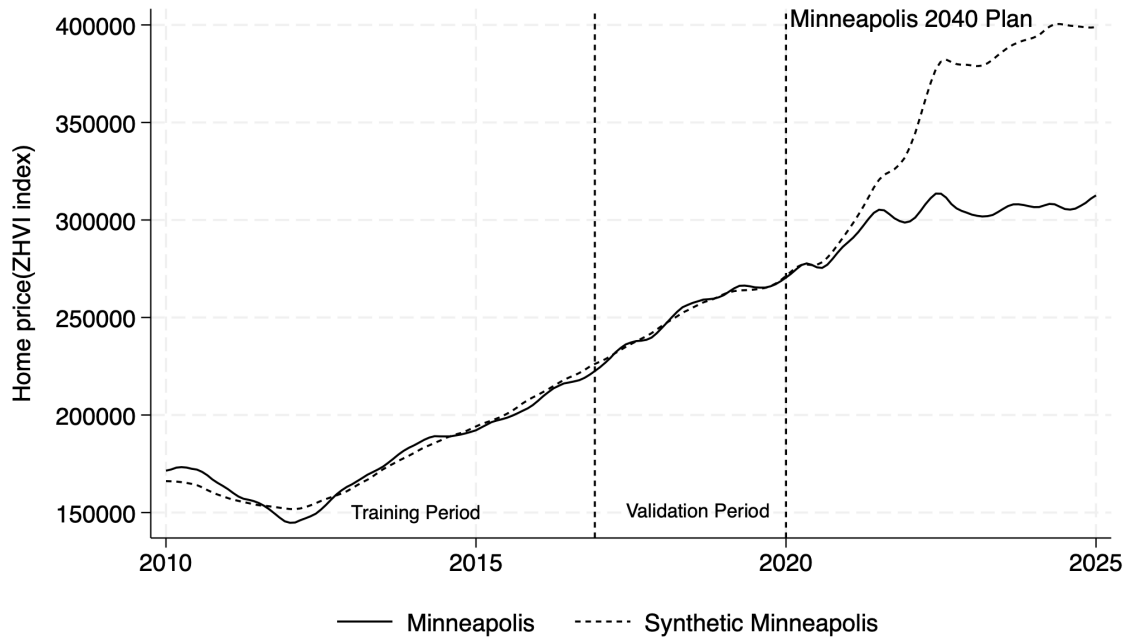


Figure 3: Monthly trend in home price (ZHVI index) Minneapolis vs. synthetic Minneapolis, 2010-2025

Kuhlmann (2021) examines the impact of the Minneapolis 2040 Plan on single-family home prices by comparing price changes in recently up-zoned areas of Minneapolis to those in neighboring cities during the year before and after the plan was passed by the City Council. He finds that the reform increased property values in Minneapolis by 3% to 5%. The findings in this study differ substantially, largely due to differences in research design and temporal scope. First, while Kuhlmann treats

the passage of the 2040 Plan in December 2018 as the intervention, this study defines treatment as beginning on January 1, 2020, when the zoning changes officially took effect. In practice, we observe that the home price index rises modestly following the plan’s passage and no significant difference in home prices appear until mid-2021, suggesting a delayed policy effect that aligns with the gradual implementation and market response to zoning reforms. Second, Kuhlmann uses the ZTRAX dataset and examines transaction-level sale prices, which are inherently more volatile and sensitive to short-term fluctuations. In contrast, this study uses the Zillow Home Value Index (ZHVI), a smoothed, seasonally adjusted measure that better captures long-run trends.

We also construct a synthetic Minneapolis using Redfin data on median sale prices, which serves as a robustness check to the ZHVI analysis. While the ZHVI reflects value changes across the entire housing stock, the Redfin median sale price data better aligns with the scope of Kuhlmann (2021) and focuses on transacted housing units. Figure A.1 in the Appendix presents the monthly median sale price of synthetic and actual Minneapolis in 2015-2025. The results align with the findings based on the ZHVI, showing that home prices in Minneapolis remained relatively similar to synthetic Minneapolis until mid-2021, followed by a notable divergence through the end of 2022. In June 2021, the median sale price in Minneapolis was 3.6% below that of its synthetic control. By the end of 2022, this gap had widened, with the median sale price falling by approximately 18% from its pre-intervention level. By the end of the study period, the difference in the median sale price reached 23.6%. Although the level of the median sale price is consistently higher than that of the ZHVI, and the observed divergence is somewhat smaller in magnitude, this exercise supports the conclusion that the Minneapolis 2040 zoning reform led to a substantial slowing in home price growth.

We also examine the heterogeneous treatment effects of the 2040 plan on home prices by property type (single-family homes vs. condos), size (number of bedrooms), and price tier (bottom, middle, top). The price decline is more pronounced for condos than single-family homes (Appendix Figure A.2); five years post-reform, condo prices were 42.68% lower than the synthetic counterparts, compared to 28.82% for

single-family homes. Smaller homes experienced larger price declines than larger ones: by the end of the study period, the price of one-bedroom homes were 34.9% lower, while prices for homes with five or more bedrooms were only 18.3% lower (Appendix Figure A.3). Homes in the bottom price tier experienced the most significant depreciation (Appendix Figure A.4). Overall, the effects appear to be concentrated in the segment the reform targeted: smaller, modestly priced homes.

It is important to note that the substantial effect of zoning reform in suppressing home and rental price growth, as shown in the results above, deviates notably from much of the existing literature, which generally report more modest impacts (Freemark, 2020; Greenaway-McGrevy and Phillips, 2023). One potential reason for this divergence is that the zoning reform examined in this paper - the Minneapolis 2040 Plan - is highly distinctive itself. Unlike many zoning reforms that target specific neighborhoods, this policy overhauled the zoning code city-wide, marking a rare and ambitious intervention in urban land use regulation. While the uniqueness and scale of the policy may help explain the stronger effect observed here, the contrast with the existing literature still warrants caution. In the next subsection, we perform a series of robustness checks to evaluate the statistical significance of the findings and to address potential concerns related to the study design and interpretation.

## 6.2 Robustness Checks

We conduct a series of robustness checks to examine the validity of the results. First, we implement placebo tests and calculate the post/pre-treatment root mean squared prediction error (RMSPE) ratio as outlined by Abadie et al. (2015) to evaluate the statistical significance of the synthetic control estimates. Next, we conduct a series of subset analyses that trims the donor pool using different criteria to test the sensitivity of the results. Lastly, we explore the possible impact of changes in crime in Minneapolis and its impact on the housing market.

### 6.2.1 Placebo tests

To assess the statistical significance of the estimated treatment effect, we conduct a series of placebo tests by reassigning the treatment status to each city in the donor pool. For each placebo city, we estimate the synthetic control model as if that city had implemented the zoning reform in 2020, following the procedures described in Section 4.2. We then compare the estimated effect for Minneapolis to the distribution of placebo effects across all donor cities. If Minneapolis exhibits a uniquely sharp post-treatment decline in rental prices - while no other city shows a comparably large or sustained drop - this strengthens confidence that the estimated effect is not driven by random chance but instead reflects a meaningful policy impact.

Figure 4 presents the results of the placebo tests for the rental price model. Each gray line shows the difference in rental prices between a donor city and its synthetic counterpart, while the orange line represents Minneapolis. Minneapolis stands out with the most pronounced price decline beginning in 2020. To complement this visual comparison, we compute the ratio of post/pre-treatment RMSPE for each city. As shown in Figure 5, Minneapolis has the highest RMSPE ratio among the 83 placebo cities: the post-treatment MSPE is approximately 30 times greater than its pre-treatment counterpart. If one were to reassign the treatment status to a random city, the probability of obtaining a ratio as big as Minneapolis' is 0.012 ( $=1/83$ ). We perform the same placebo test for the home price model and report the results in Figure A.5 and Figure A.6 in the Appendix. Minneapolis exhibits the most unique home price decline from all donor cities and has the highest post/pre-treatment RMSPE ratio, which further validates the effect of zoning reform in reducing home and rental price growth.

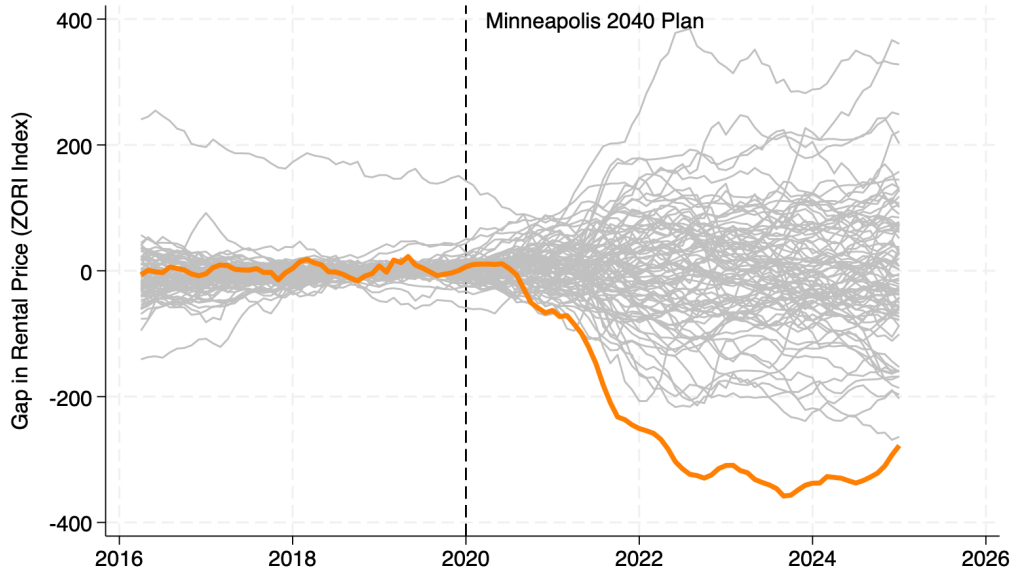


Figure 4: Rental price gaps in Minneapolis and all 83 donor cities, 2016-2025.

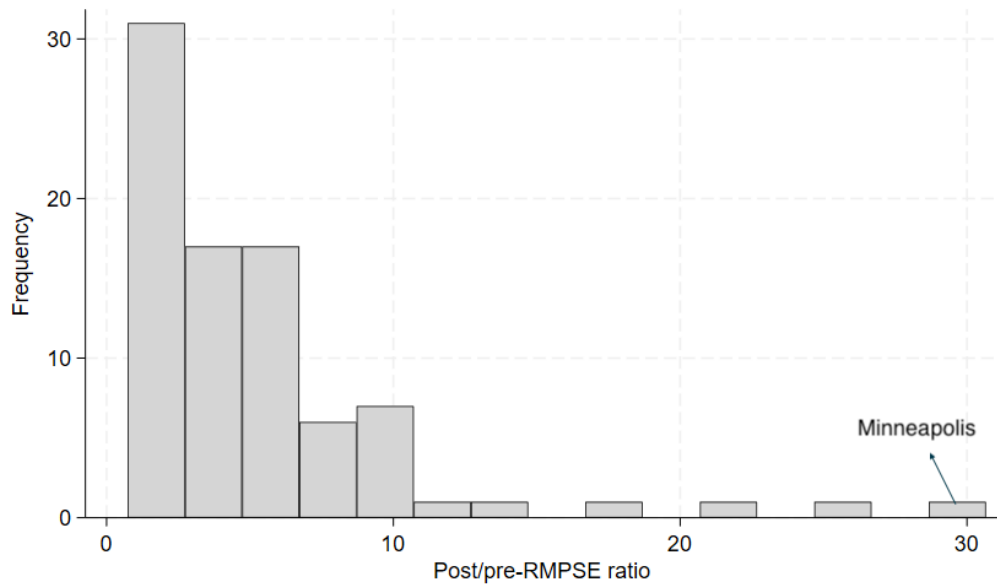


Figure 5: Post/Pre-treatment Root Mean Squared Prediction Error(RMSPE) of Minneapolis and all placebo estimates.



### 6.2.2 Subset Analysis

To further assess the robustness of the synthetic control estimates, we conduct a subset analysis by restricting the donor pool to cities that are geographically or socioeconomically more comparable to Minneapolis. Although we apply clearly defined selection criteria and implement out-of-sample validation to mitigate overfitting in the full-sample model, the presence of residual heterogeneity within a large donor pool may still bias the results or weaken their interpretability. For example, a major concern arises from the inclusion of larger cities with more prosperous housing markets. This may inflate the treatment effect of zoning reform in Minneapolis if housing/rental price growth was more pronounced in larger cities post-2020. To address this, and similar concerns, we construct four smaller donor pools using strict filtering rules that exclude cities with significantly larger economies and select cities that more closely mimic Minneapolis in geographic, socioeconomic, and demographic contexts. These subset donor pools are designed to be more homogeneous and conservative, potentially leading to an underestimation of the treatment effect. We then re-estimate the synthetic control model described in Section 4.2 using each subset and compare the resulting treatment effects to those from the full-sample model. This comparison helps to evaluate the sensitivity of the findings to donor pool composition and enhances the credibility of the results.

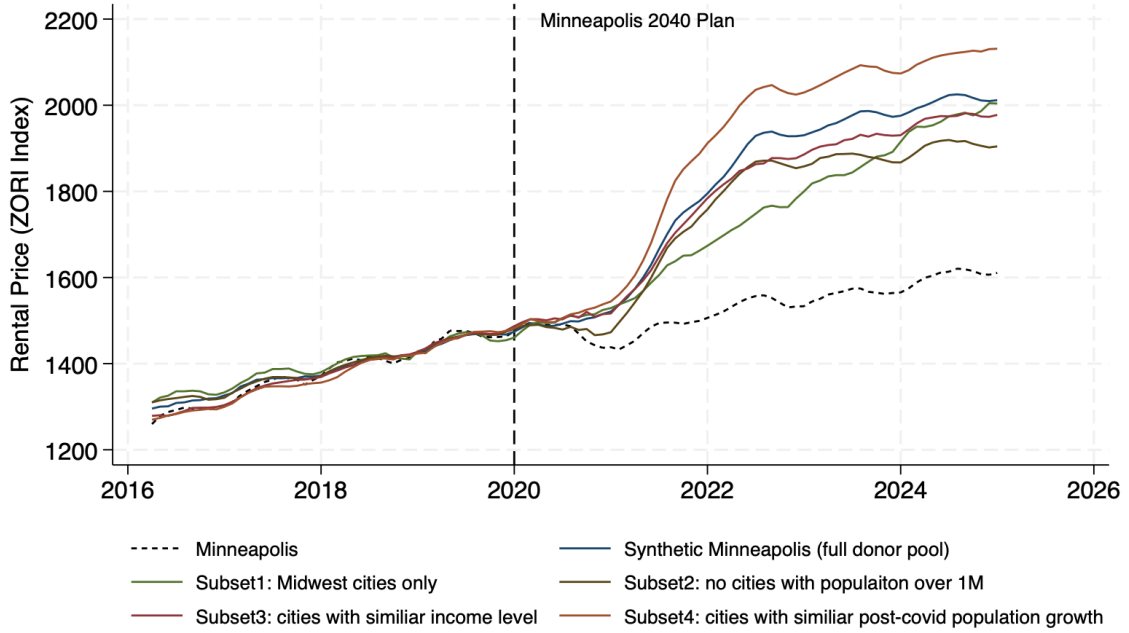


Figure 6: Monthly trend in rental price (ZORI index) Minneapolis vs. synthetic Minneapolis, 2016-2025. Synthetic Minneapolis is constructed repeatedly and plotted using four different subset donor pools as well as the full donor pool.

Figure 6 presents the results from four subset analyses alongside the baseline synthetic control estimate, each examining the effect of zoning reform on rental prices in Minneapolis. The first subset restricts the donor pool to cities located in the Midwest. This restriction is motivated by both theoretical and empirical considerations. Tobler’s First Law of Geography—“everything is related to everything else, but near things are more related than distant things” - suggests that Midwestern cities are likely to be more comparable to Minneapolis due to shared geographic, economic, and institutional characteristics. These cities often experience similar climate patterns, land use constraints, governance structures, and housing market conditions. By focusing on regionally proximate cities, this subset analysis tests whether a synthetic control composed of geographically similar cities can replicate the baseline treatment effect. As shown in Figure 6, the zoning reform is associated with a rental price decline of approximately \$400 in this estimate - similar to the full-sample estimate -

offering a geographically grounded robustness check for the main findings.

The second subset excludes cities with populations greater than one million as of 2019. In constructing the full donor pool, we follow guidelines from the Federal Reserve Bank of Minneapolis (2021), which recommend a population cap of 1.5 million. However, even within that threshold, larger cities tend to exhibit higher housing prices driven by stronger demand and more constrained supply, potentially biasing the synthetic control model by overstating the treatment effect for Minneapolis. To address this concern, we construct a synthetic Minneapolis using only smaller cities as potential donors. As shown in Figure 6, the estimated effect of the zoning reform in this model is a decline in rental prices of approximately \$293 by January 2025 - roughly 19% below the pre-treatment level. While the magnitude is smaller than that of the baseline model, the treatment effect remains substantial and statistically significant, suggesting that the core findings are not driven by the inclusion of larger, more expensive cities in the donor pool.

The third subset restricts the donor pool to cities with income levels closely aligned with that of Minneapolis. Specifically, we include only those cities whose median household income falls within  $\pm 10\%$  of Minneapolis's average median income over the 2010–2019 period. Given the lack of consistent city-level GDP data, median income serves as a practical proxy for local economic conditions and purchasing power. This restriction aims to ensure that the synthetic control is constructed from cities with comparable economic capacity and affordability dynamics, thereby reducing the influence of economic disparities on the estimated treatment effect. As shown in Figure 6, the post-treatment rental price trend in this subset closely tracks the baseline estimate through 2022, with the effect becoming slightly less pronounced thereafter. By the end of 2024, rental prices decline by 24.85% relative to the pre-intervention level - approximately three percentage points lower than the baseline estimate - but the magnitude remains substantial. These results suggest that differences in income levels among donor cities do not fully account for the observed treatment effect.

Fourth, we account for the potential influence of the COVID-19 pandemic on housing market dynamics - an external shock not explicitly modeled in the synthetic

control framework. The synthetic control approach cannot control for events that occurred after the zoning reform took effect that impacted markets differently. Although the pandemic was a global event that affected all cities in the donor pool, it generated highly uneven housing market responses across regions (Ramani and Bloom, 2021). The pandemic induced large-scale population movements, and cities that experienced significant inflows often saw housing demand - and prices - rise sharply (Liu and Su, 2021; Haslag and Weagley, 2024). Such pandemic-driven volatility could artificially inflate the estimated treatment effect if these cities are included in the donor pool. Minneapolis, by contrast, experienced relatively modest population growth during this period, with an average annual growth rate of approximately 0.18% between 2020 and 2023. To test the robustness of the baseline results against pandemic-induced housing volatility, we construct a subset donor pool limited to cities with similarly moderate post-COVID population growth, defined as ranging from  $-0.3\%$  to  $0.5\%$ . As shown in Figure 6, the estimated treatment effect in this subset is approximately \$100 larger than the baseline estimate by the end of 2024, indicating that the main findings are not driven by pandemic-related housing booms in other cities.

These four subset analyses consistently show a substantial decline in rental prices in Minneapolis following the implementation of zoning reforms in 2020. Across all specifications, the estimated reduction in rental prices ranges from \$293 to \$520, corresponding to an average decline of approximately 3.5% to 7% over the five-year post-treatment period. We also conduct the subset analysis for the home price model (see Figure A.7 in the Appendix). The results from the subset analyses also indicate a considerable effect of the zoning reform on lowering home price growth in Minneapolis. We estimate that the Minneapolis 2040 plan decreases home prices between around \$43,000 to \$92,000 by Jan 2025, which translates to an average decline of approximately 3.2% to 6.8% over the five-year post-treatment period.

### 6.2.3 Crime

As noted above, a fundamental concern with a synthetic control approach is whether something else, in addition to the zoning reform, uniquely affected Min-

neapolis. One important event in the post-treatment period was the murder of George Floyd in May 2020. This event led to widespread protests in the city, a breakdown of trust between community members and police, and calls for changes to policing in the city. This led to significant changes in a number of areas including the city launching Behavioral Crisis Response teams in 2021 (City of Minneapolis (2025)), and a significant, though temporary, decline in the number of police officers in the Minneapolis Police Department (Associated Press (2025)). Of course, this was a national event in many respects, and many other cities experienced similar changes, so it is not clear how unique Minneapolis was along these margins. It is also important to note that the synthetic control approach is constructing synthetic Minneapolis by matching characteristics of donor cities that themselves are correlated with crime (e.g., poverty rate and unemployment). However, it is still possible that Minneapolis experienced different changes in crime outcomes in the post-treatment period. Since crime may be an important determinant of property values, we explore this question directly.

To do this, we use monthly crime data from 2013 to 2023 (latest available) for Minneapolis and a set of donor cities from the FBI’s Crime Data Explorer (Federal Bureau of Investigation (2025)). For donors we include cities in either Subset 1 (Midwest) or Subset 3 (similar income), discussed above. Several cities in this set have missing crime data and are excluded from the donor pool. In total, the data include 30 cities (Minneapolis and 29 donor cities). We aggregate crime data into property crime and violent crime categories.<sup>4</sup> Since crime data is relatively noisy, we compute quarterly means of the monthly data and seasonally adjust it. To construct synthetic Minneapolis, we match covariates on population, racial characteristics, poverty, home ownership, number of housing units, and share of single family housing. The results for property crime and violent crime are plotted in Figure 7 and Figure 8, respectively.

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<sup>4</sup>Our property crime category includes burglary, larceny, and auto theft and the violent crime category includes aggravated assault, homicide, rape, and robbery.

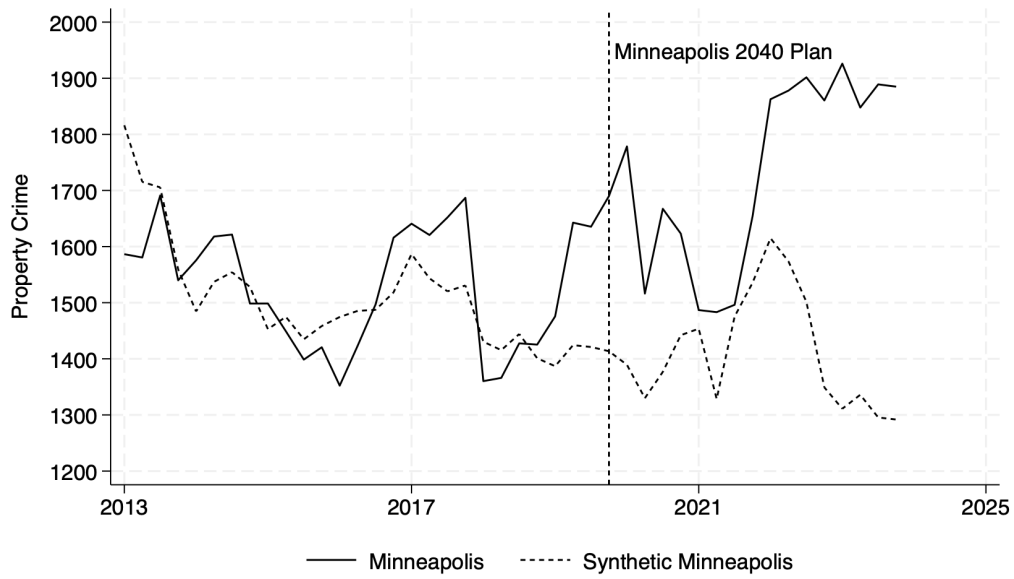


Figure 7: Average quarterly reported property crime in Minneapolis vs. synthetic Minneapolis, 2013-2023.

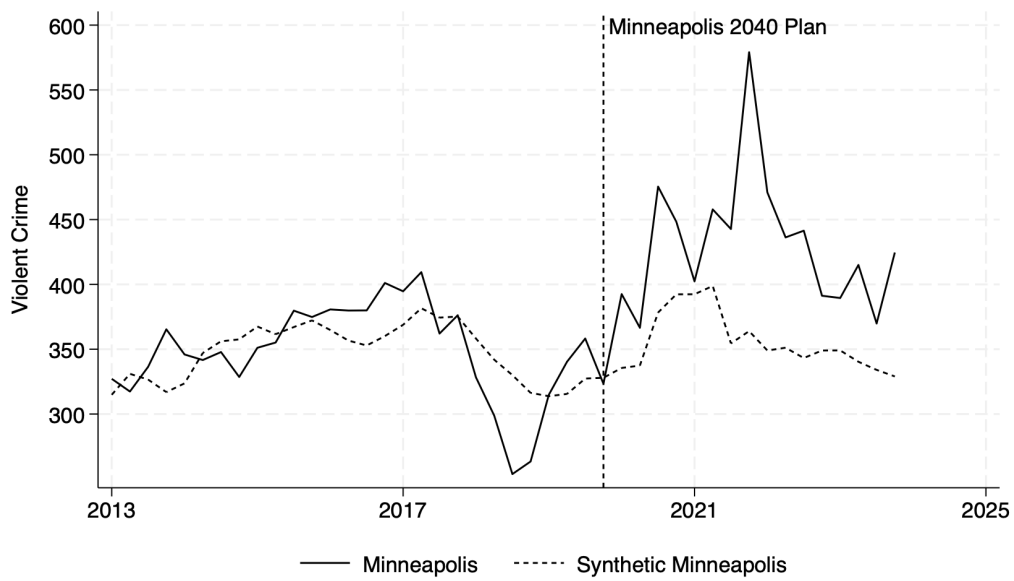


Figure 8: Average quarterly reported violent crime in Minneapolis vs. synthetic Minneapolis, 2013-2023.

While the crime data is relatively noisy, Figure 7 and Figure 8 show some evidence of elevated crime rates in Minneapolis post-2020. However, as with the housing data, we also conduct a placebo analysis on these results and fail to reject the hypothesis that the treatment effects for Minneapolis are equal to the mean those of the donor cities and conventional thresholds: The p-values from these test are 0.07 and 0.2 for property and violent crime, respectively.

Since the property crime outcomes for Minneapolis are approaching conventional significance levels (p-value=0.07), we conduct an additional robustness check where we estimate the treatment effects for home and rental prices using a Differences-in-Differences approach which allows us to explicitly control for changes in crime in the post-2020 period. We find that regressions with and without crime controls yield very similar results, suggesting that changes in crime are not driving our results on home and rental prices. These are reported in Appendix B.

## 6.3 Mechanism

While the results thus far suggest that the Minneapolis 2040 Plan has triggered a significant decrease in home and rental prices, the underlying mechanisms driving these effects remain unclear. In this subsection, we explore two main hypotheses that may explain the significant decrease in home and rental price growth.

### 6.3.1 Housing Supply

Advocates of up-zoning generally argue that legalizing triplexes in low-density areas reduces development costs and increases building potential. Such reductions in construction costs would, in theory, stimulate new housing construction and, consequently, lower housing prices (Glaeser et al., 2006; Kok et al., 2014). In this subsection, we perform a simple exercise to observe the effect of the zoning reform on new construction in Minneapolis. We use the number of new privately-owned residential units, reported in the Building Permits Survey (BPS) by the Census Bureau, as a proxy for changes in housing supply. We construct a synthetic Minneapolis to

examine the changes in new construction permits post-reform.<sup>5</sup>

Figure 9 compares the quarterly trends of building permits issued in Minneapolis and its synthetic counterpart from 2010 to 2024. The pre-treatment fit is relatively poor, which motivates the use of a difference-in-differences approach to validate the results (below). Following a small peak in new permits issued in the first quarter of 2022, Minneapolis experienced a sharp decline in permitting activity relative to the synthetic control, with the gap widening steadily through the end of the study period. Fewer than 500 new permits were issued per quarter in 2023, compared to a quarterly average of over 970 in 2022. By the end of 2024, less than 100 new construction permits were issued per quarter in Minneapolis, which points to a complete halt of new construction in the city. These findings suggest that the observed improvements in housing affordability are unlikely to be driven by increased housing supply.

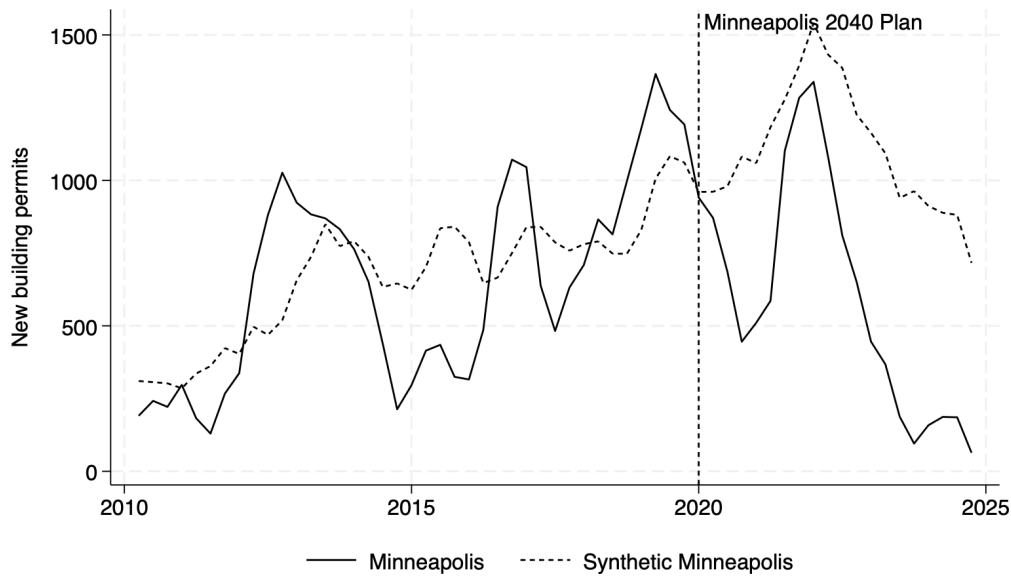


Figure 9: Quarterly trend in new residential building permits, Minneapolis vs. synthetic Minneapolis, 2010-2025.

<sup>5</sup>Only 79 out of 83 cities in the full donor pool is included in the following exercise due to missing data in the BPS. Other missing data (the number of building permits and valuation) are imputed by the annual averages.



We also compare the permitting activities across building types.<sup>6</sup> These results are plotted in Figure A.8 in Appendix A. Single-family homes, 4 unit homes, and 5+ unit structures show the most notably declines. The overall decline in permits documented in Figure 9 is dominated by the 5+ unit category since they typically represent more than 90% of all building permits. Building permits in the 3-unit category do not show a systematic difference relative to sythetic Minneapolis, possibly reflecting developers maximizing the “up to three units by right” change in the 2040 plan, but it is important to emphasize that 3-unit structures make up a small share of total building permits.

To complement the synthetic control approach, we also apply a difference-in-differences (DiD) model to estimate the impact of the Minneapolis 2040 Plan on new construction and housing supply. The model specification is outlined in Equation 3 below:

$$Y_{jt} = \beta \cdot I\{j = \text{Minneapolis}\} \cdot I\{t \geq 2020\} + \alpha_j + \gamma_t + \phi X_{jt} + \epsilon_{jt} \quad (3)$$

The variable  $Y_{jt}$  represents an outcome for city  $j$  at year  $t$  such as the number of building permits.  $\beta$  is the DiD estimator that measures the effect of zoning reform on new construction in Minneapolis. The model also includes city-fixed effects  $\alpha_j$ , time-fixed effects  $\gamma_t$ , and time varying city-level controls  $X_{jt}$ .

Table 2 presents estimates of the impact of the Minneapolis 2040 zoning reform on new residential construction. The zoning reform had a minimal effect on overall construction activity in the four years following implementation. In fact, there is a slight decrease in the number of multi-family apartment units being constructed, likely reflecting the effects of a permitting freeze in mid-2023. The reform is associated with a decrease of 1.78 building permits per 1,000 residents, which translates to an

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<sup>6</sup>For those unfamiliar with building permit data, it is important to emphasize how the data are recorded. An entry of “100” in the 5-units or more structure category refers to the number of total units, not the total number of buildings. The 100 units could come from one building with 100 units, or four buildings with 25 units each, for example.

average annual reduction of approximately 748 permitted housing units. By contrast, the number of new single-family homes and quadraplexes increased slightly, though these effects are not statistically significant. The DiD estimates are consistent with the findings from the synthetic control model and suggest that, if anything, Minneapolis experienced a construction slowdown post-reform.

	One-unit	Three-unit	Four-unit	5+ nits	Totals
Building permits per 1000	0.0285 (0.354)	-0.0592 (0.0600)	0.0482 (0.0439)	-1.629* (0.674)	-1.780* (0.833)
Fixed Effect	Yes	Yes	Yes	Yes	Yes
N	478	478	478	478	478

Table 2: Effect of Zoning Reform on New Residential Construction. This table reports difference-in-differences estimates of the effect of zoning reform on new construction activity. Standard errors are reported in parentheses. The dependent variable is the number of building permits per 1,000 residents by structure type. All specifications include city and time fixed effects. \*\*\*Significant at the 1% level, \*\*significant at the 5% level, \*significant at the 10% level.

The stagnation in construction can be attributed to two primary factors. First, the Minneapolis 2040 Plan was briefly halted between September 2023 and May 2024 due to a legal challenge over environmental concerns related to up-zoning and dense infill (Du, 2024). This resulted in a pause in issuing building permits under the new zoning rules, which effectively halted development and delayed several large-scale apartment projects. Although the legal dispute over the Minneapolis 2040 Plan was resolved in 2024, the momentum for new development may have already faded. After peaking at over 3,600 permits in 2022, building activity dropped to just 1,173 in 2023 and declined further to 452 in 2024, marking a sharp and prolonged slowdown despite the legal resolution. The legal challenge not only disrupted construction but also sent a negative signal to potential developers and investors, creating a climate of uncertainty that deterred further investment (Johnson, 2024).

Second, planning and executing new construction projects, particularly multi-story buildings, requires time. It is reasonable to expect that the effects of up-zoning

on new construction in Minneapolis may take time to materialize, a pattern observed in other urban contexts. Existing literature presents mixed evidence on the short-term impact of zoning reform. For example, Freemark (2020) finds no significant difference in construction activity between up-zoned and non-upzoned parcels in Chicago in the initial years following reform. In contrast, Auckland’s large-scale up-zoning initiative led to a near tripling of construction across much of the city within four years (Greenaway-McGrevy and Phillips, 2023). While there is broad agreement that up-zoning can significantly increase construction over the long run (Anagol et al., 2021; Büchler and Lutz, 2021), the scale and timing of these effects vary depending on the type of reform and local housing market conditions (Kok et al., 2014; Dong, 2025). Given these patterns, the muted construction response in Minneapolis to date may reflect the expected lag between policy adoption and development outcomes, rather than a failure of the reform itself.

We also examine the trends in average valuations (per unit) of newly permitted residential structures to observe shifts in the housing stock. The valuation series are quite noisy for many cities, and thus do not lend themselves well to serving as a donor pool in a synthetic control approach. As a result, for the valuation trends, we simply provide reduced form evidence by comparing normalized new construction values to those of the nation, plotted in Figure 10.

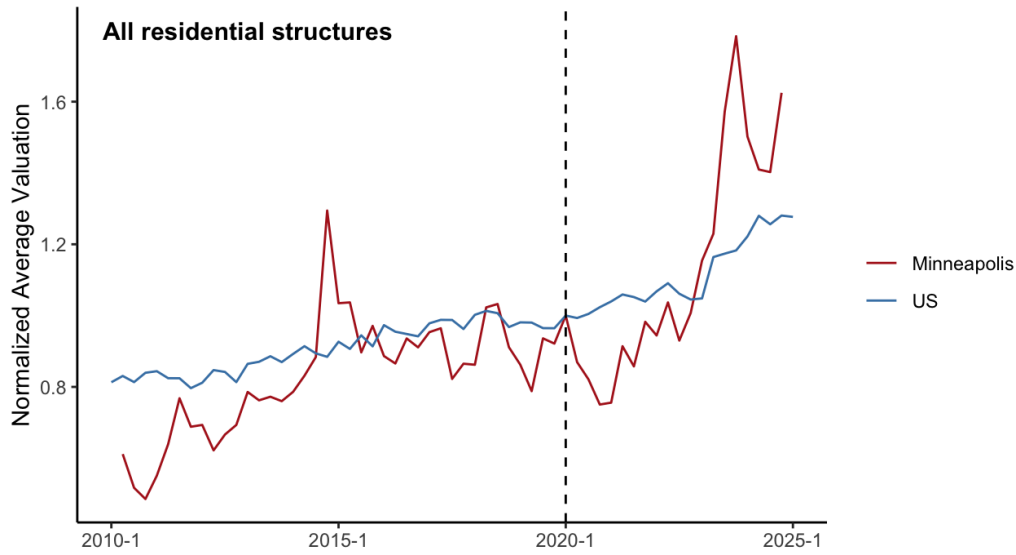


Figure 10: Normalized new construction values (to Jan. 2010), Minneapolis vs. USA, 2010-2025.

Relative to the nation, Minneapolis experienced an increase in the valuation of newly constructed properties. These trends for single-family and multi-family homes are plotted in Figure A.9 and Figure A.10 in Appendix A. The valuation increases for both categories, but for single-family homes returns to below the national trend by 2025, where as the increase in multi-family new construction values appears more persistent.

These increases in valuation occurred even as overall permitting activity declined, suggesting that the composition of new development may have shifted toward more capital-intensive projects. However, the underlying drivers of this increase remain unclear. One plausible explanation is that developers are pivoting toward higher-end or luxury housing in response to the loosening of zoning restrictions, generally consistent with the findings in Wolf-Powers (2005) for New York City. The valuation increases in multifamily homes could also be related to the piece of the 2040 plan which requires developers of new multifamily buildings with 20 or more units to reserve a portion of the units as affordable or to pay an in-lieu fee to the city's

affordable housing fund. If developers choose the second option, this could be viewed as an increased construction cost for developers of larger buildings. Insofar as higher priced units increase profit margins for developers, this shift toward more luxury development could be a result of this piece of the 2040 plan. Although this in-lieu fee may have the undesirable effect of causing developers to shift toward the development of luxury units, the overall impact on affordable housing also depends, of course, on how these in-lieu fees translate into the supply of affordable housing.

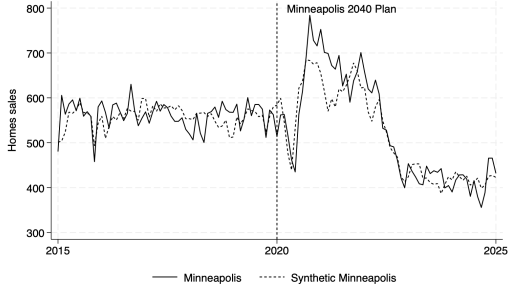
### **6.3.2 Housing demand**

An alternative explanation for the observed decrease in home and rental prices is a decline in housing demand. While the subset analysis controls for post-COVID population trends as a potential driver of cooling in the housing market, the zoning reform itself may also play a role in moderating demand. By signaling a future expansion in housing capacity, the reform likely shifted expectations among home buyers and investors. Anticipating a more elastic housing supply and a less heated housing market, potential buyers may have delayed their purchases or reduced their bidding aggressiveness. These expectation-driven behavioral shifts can suppress price growth even in the absence of immediate construction activity. In this sense, the reform's impact on market sentiment may have contributed to the observed price declines through a reduction in speculative or urgency-driven demand.

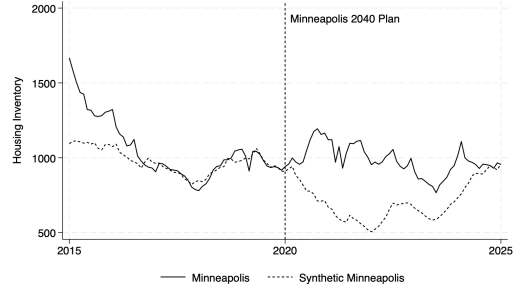
To test this hypothesis, we examine a series of demand-side housing market metrics to examine how the Minneapolis 2040 plan affected the housing demand. Figure 11 reports the monthly change in home sales, housing inventory, average list-to-sale ratio, and median days on the market for Minneapolis and synthetic Minneapolis. Beginning in early 2021, Minneapolis experienced a slight relative increase in median days on the market, a more notable increase in housing inventory, a slight decrease in average sale-to-list ratio, which are generally consistent with softened demand and a less heated housing market.

The most optimistic interpretation of this softening in housing demand is that prospective homebuyers and investors anticipate a long-run expansion in the hous-

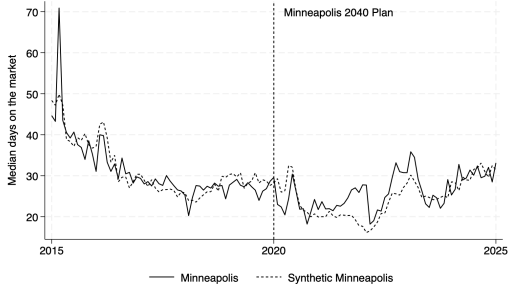
ing supply. For speculative buyers, this shift in expectations reduces the perceived scarcity of housing - a key driver of price appreciation - and thereby diminishes the expected returns from investment (Hartley et al., 2024). In this context, the zoning reform operates not through immediate changes in supply, but by altering forward-looking behavior. However, an alternative and less favorable interpretation is also plausible. The broad relaxation of density restrictions may have lowered the perceived amenity value of Minneapolis for some buyers. In particular, concerns over increased density, congestion, or neighborhood change may have reduced the city's attractiveness to marginal buyers, leading to lower demand (Davis et al., 2023). While there is currently little evidence of a large-scale residential flight, such sorting effects often unfold gradually. Future research should further explore whether zoning-induced changes in neighborhood character influence longer-term residential preferences and mobility patterns.



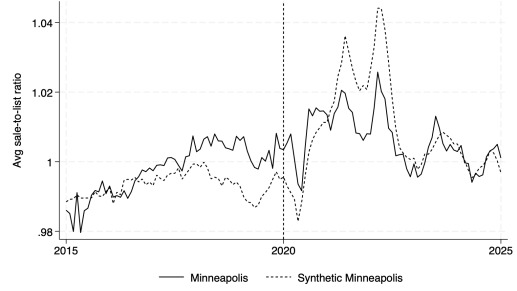
(a) Homes sales



(b) Housing inventory



(c) Median days on the market



(d) Average list-to-sale ratio

Figure 11: Monthly trend in demand-side indicators(home sales, housing inventory, median days on the market, and average list-to-sale ratio), Minneapolis vs. synthetic Minneapolis, 2015-2025.

## 7 Discussion and Conclusion

This paper investigates the impact of comprehensive zoning reform on housing affordability, using the Minneapolis 2040 Plan as a case study. We construct a synthetic Minneapolis in the absence of zoning reform to estimate the treatment effect of this reform and find a pronounced decrease in home and rental prices. Home prices grew by an average of 3.2% to 6.8% less per year, while rental prices grew by 3.5% to 7% less per year, relative to synthetic Minneapolis. Placebo tests document that post-reform Minneapolis experienced markedly different outcomes than other cities, having the lowest home and rent price trajectories among 83 cities in the donor pool. And these differences are statistically significant at the 5% level.

These impacts on home and rental prices are also economically significant, even after accounting for various geographic, socioeconomic, and post-COVID population trends. The decrease is more pronounced among condominiums, modest-sized, and smaller homes compared to single-family homes, affluent, and larger properties. However, the observed improvement in housing affordability does not appear to stem from an increase in housing supply, as Minneapolis has seen a construction slowdown post-reform, largely due to the legal dispute surrounding Minneapolis and the consequent permit freeze in 2023-2024. We examine a series of demand-side housing market indicators and find that bidding intensity and home sales have slowed down post-reform in Minneapolis, which suggests changing expectation among Minneapolis homebuyers. Minneapolis 2040 Plan may signal a future increase in housing supply, which discourages potential speculative buyers from entering the housing market and cause urgency-driven buyers to wait and see.

These findings diverge from some of the existing literature, which generally reports little to no effect of zoning reforms on home prices (Freemark, 2020; Greenaway-McGrevy and Phillips, 2023). The difference likely reflects the unusually comprehensive nature of the Minneapolis 2040 Plan. For example, Freemark (2020) evaluates the 2013 and 2015 transit-oriented development (TOD) reforms in Chicago, which eased parking minimums and increased allowable density for parcels located near transit corridors. The reform was limited in spatial scope and moderate in terms of up-scaling building capacity. In contrast, the Minneapolis 2040 Plan is a city-wide reform that eliminated single-family-only zoning and legalized up to four-unit dwellings across nearly all residential neighborhoods. The contrast in outcomes between these cases highlights the importance of institutional context and the specific design of zoning reform in determining policy effectiveness.

While this study presents consistent causal evidence that the Minneapolis 2040 zoning reform reduced both home and rental prices, several limitations remain. The results are robust across two independent data sources—Zillow’s Home Value Index (ZHVI) and Redfin’s median sale price estimates. However, ZHVI is derived from Zestimate, a proprietary machine learning algorithm with limited public documentation, raising concerns about the transparency and replicability of the price construction



process. In addition, both Zillow and Redfin provide data at the city/place level, which obscures within-city variation and limits the ability to examine heterogeneity in price effects across neighborhoods or housing types. Using transaction-level data would enable a more granular analysis of housing market dynamics across the city and allow for the estimation of heterogeneous treatment effects across different market segments.

In addition, while this study takes a first step at exploring the mechanisms through which zoning reform may influence housing affordability, much remains unclear. In particular, understanding what drives the apparent reduction in demand in the housing market is critical. Minneapolis continues to exhibit one of the largest racial homeownership gaps in the United States, and a substantial share of its single-family rental housing is owned by corporate landlords (Freemark et al., 2021). How these larger landlords respond to zoning changes - whether through disinvestment, holding strategies, or rent adjustments - remains an open and important question. Unpacking these dynamics will be essential not only for understanding the market consequences of zoning reform and the differential treatment effects across demographic groups, but also for informing policymakers on how to design more effective and equitable zoning reforms that account for local ownership structures, neighborhood disparities, and varying market responses.

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## A Additional Results on Housing Affordability

Table A.1: Summary statistics from Zillow(2010-2025), BPS(2010-2024), Redfin(2015-2025), American Community Survey(ACS) 5-year estimate(2010-2023).

Variable	Mean	SD	Count
<b>Outcome variables</b>			
Housing price (ZHVI)	208,311.80	100,689.00	15,204
Top-tier ZHVI	339,999.40	144,302.00	15,204
Bottom-tier ZHVI	134,571.30	81,924.04	15,204
Single-family home price	213,934.50	106,517.80	15,204
Condominium price	155,272.10	69,549.68	15,204
One-bedroom home price	111,229.90	59,868.95	13,757
Two-bedroom home price	145,347.60	78,970.82	14,480
Three-bedroom home price	197,717.30	95,141.25	14,480
Four-bedroom home price	281,127.40	117,795.50	14,480
Five-bedroom home price	403,022.90	228,353.00	14,480
Rental price (ZORI)	1,321.97	363.98	8,904
New building units permitted	150.53	178.44	13,937
Median sale price	261,624.30	106,670.50	9,801
Housing Inventory	1,298.29	1,120.96	9,196
Average list-to-sale ratio	0.99	0.02	9,075
Homes sold	466.13	357.45	9,801
Median days on market	43.94	22.57	9,801
<b>Covariates</b>			
Population	371,328.10	298,755.60	14,112
Housing units	160,325.30	125,668.60	14,112
% White	62.32	15.28	14,112
% Black	21.87	16.48	14,112
% in Poverty	17.45	5.79	14,112
Unemployment rate (%)	5.76	2.72	14,112
Median income (\$)	54,365.83	15,223.66	14,112
Homeownership rate (%)	54.44	9.72	14,112
% Single-family housing	46 55.23	15.05	14,112

Table A.2: Variable Weights in Synthetic Control Models for Home Price and Rental Price

Variable	Home Price Model	Rental Price Model
Population	0.3727	0.0000
%White	0.0227	0.0000
%Black	0.0027	0.9367
Poverty rate	0.0006	0.0000
Home ownership rate	0.0005	0.0000
Housing units	0.5865	0.0624
Unemployment rate	0.0015	0.0001
%Single family housing	0.0091	0.0000
Rental price(ZORI)	0.0026	0.0007
Home price (ZHVI)	0.0011	0.0000

*Notes:* This table reports the covariate weights (V matrix) used to construct synthetic control units in the home and rental price models. The weights in each column sum up to 1.



Table A.3: Predictor Balance for Home Price and Rental Price Models

<b>Panel A: Home Price Model</b>		
<b>Variable</b>	<b>Treated</b>	<b>Synthetic</b>
Population	0.4373	0.4402
%White	0.2994	0.3151
%Black	-0.2362	-0.1268
Poverty rate	0.8343	0.4793
Home ownership rate	-0.5368	-0.4914
Housing units	0.5574	0.5561
Unemployment rate	-0.3403	-0.0131
%Single family housing	-0.7336	-0.7670
Rent (ZORI index)	0.2941	0.1025
Home price (ZHVI index)	-0.1190	-0.2131
<b>Panel B: Rental Price Model</b>		
<b>Variable</b>	<b>Treated</b>	<b>Synthetic</b>
Population	0.5218	0.7277
%White	0.1216	-0.0229
%Black	-0.1760	-0.1763
Poverty rate	0.5707	-0.1857
Home ownership rate	-0.7281	-0.1890
Housing units	0.5706	0.5711
Unemployment rate	-0.9686	-0.8641
%Single family housing	-0.8228	-0.5210
Rent (ZORI index)	0.1797	0.1546
Home price (ZHVI index)	0.4720	0.7824

*Notes:* Predictor Balance Between Treated and Synthetic Units in Home and Rental Price Models. I calculate the natural log of pre-treatment home and rental prices and include them into the covariates. All covariates and lagged outcomes are standardized (mean = 0, SD = 1) prior to model fitting. This table compares the standardized values of each predictor in the treated unit (Minneapolis) to the weighted average of those same predictors in the synthetic control. Close correspondence between the two indicates a good pre-treatment fit.

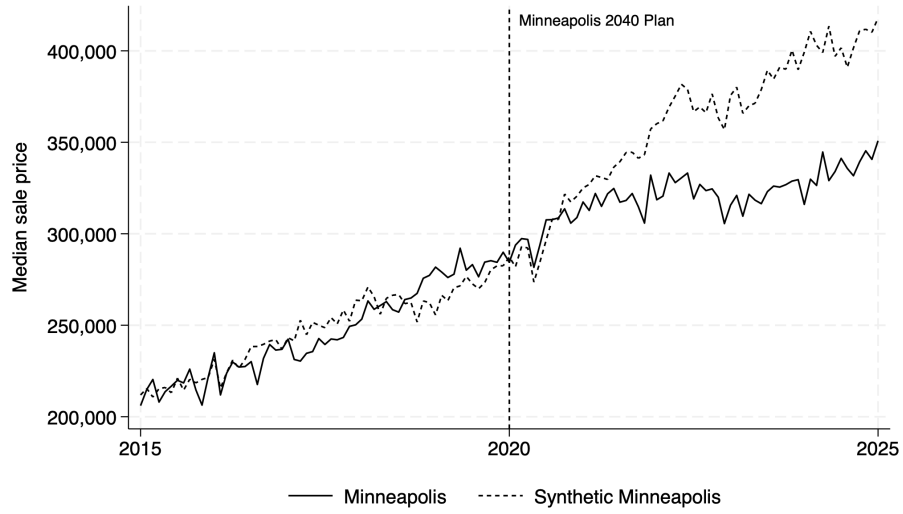


Figure A.1: Monthly trend in median sale price, Minneapolis vs. Synthetic Minneapolis, 2015-2025.

*Notes:* Redfin provides monthly estimates of median sale prices at the city level. I seasonally adjust the median sale prices for all cities in the dataset. 5 of the 83 donor cities are not covered by Redfin and are therefore excluded from the analysis. For 3 additional cities with missing data in the first four months of the study period, I impute values using their respective first available sale price.

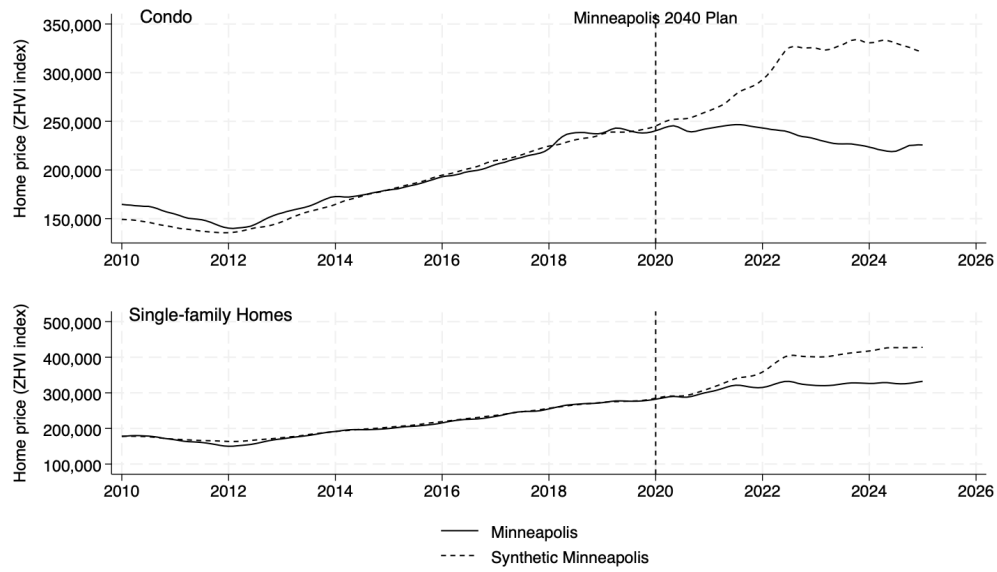


Figure A.2: Monthly trend in home price(ZHVI index) by home types(single-family homes an condos), Minneapolis vs. synthetic Minneapolis, 2010-2025.

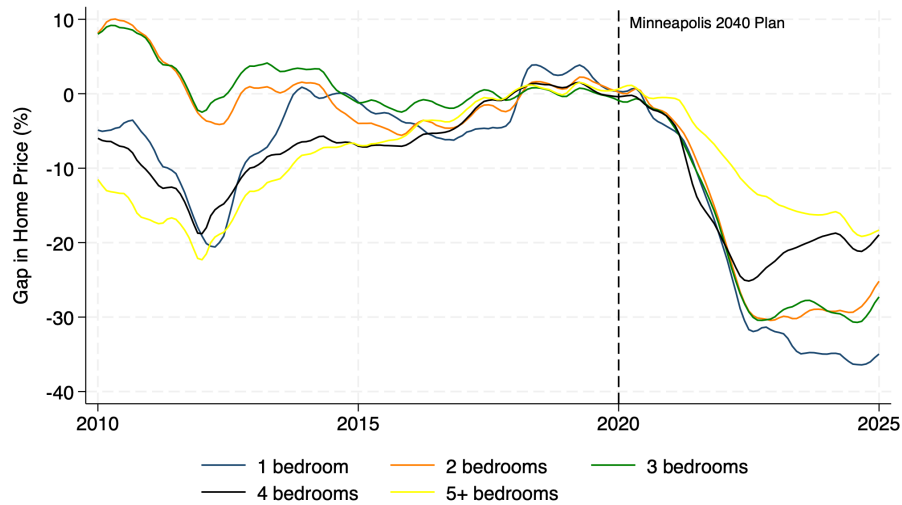


Figure A.3: Gap in home price(%) by number of bedrooms in the home, Minneapolis vs. synthetic Minneapolis, 2010-2025.

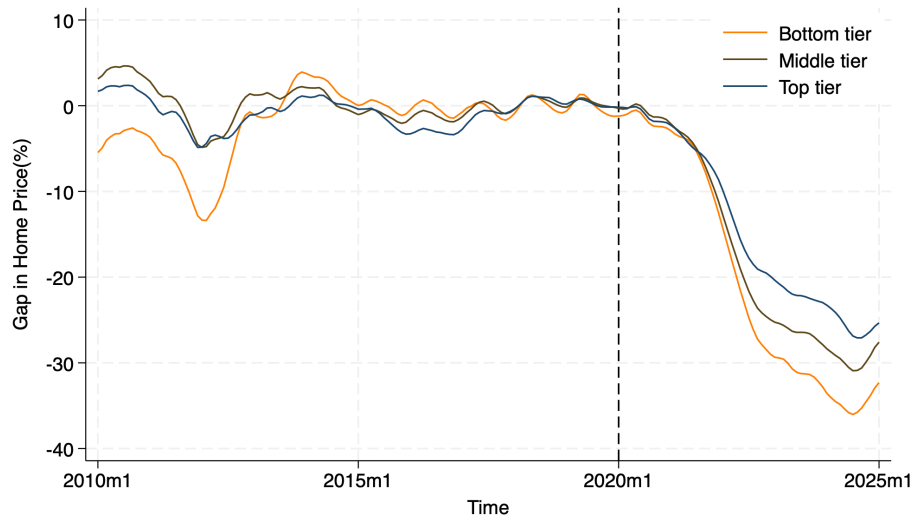


Figure A.4: Gap in home price(%) by percentile ranges Minneapolis vs. synthetic Minneapolis, 2010-2025. The bottom tier presents the home prices within the 5th to 35th percentile range in a given region, the middle tier within 35th to 65th percentile range, and top tier within the 65th to 95th percentile range.

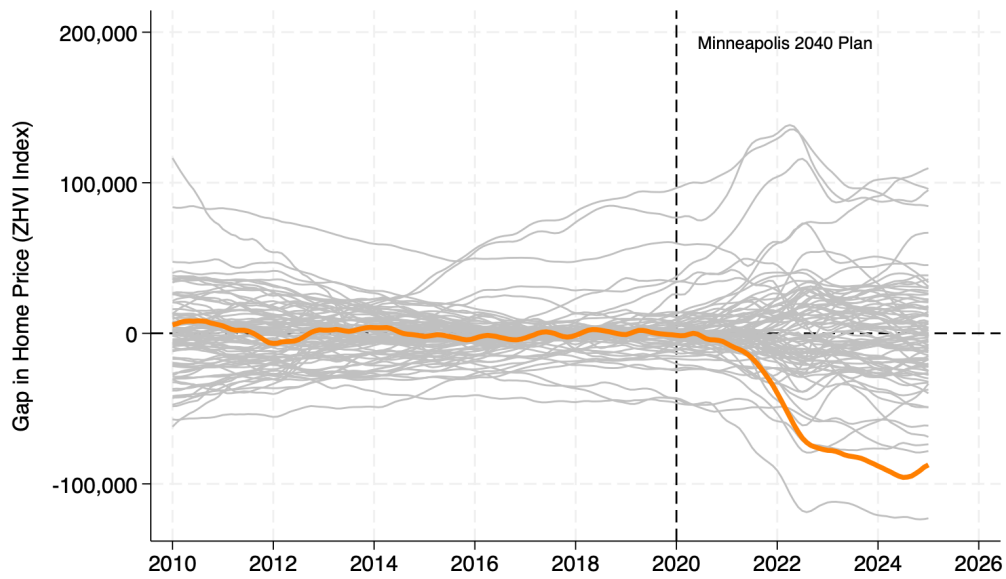


Figure A.5: Home price gaps in Minneapolis and all 83 donor cities, 2010-2025.

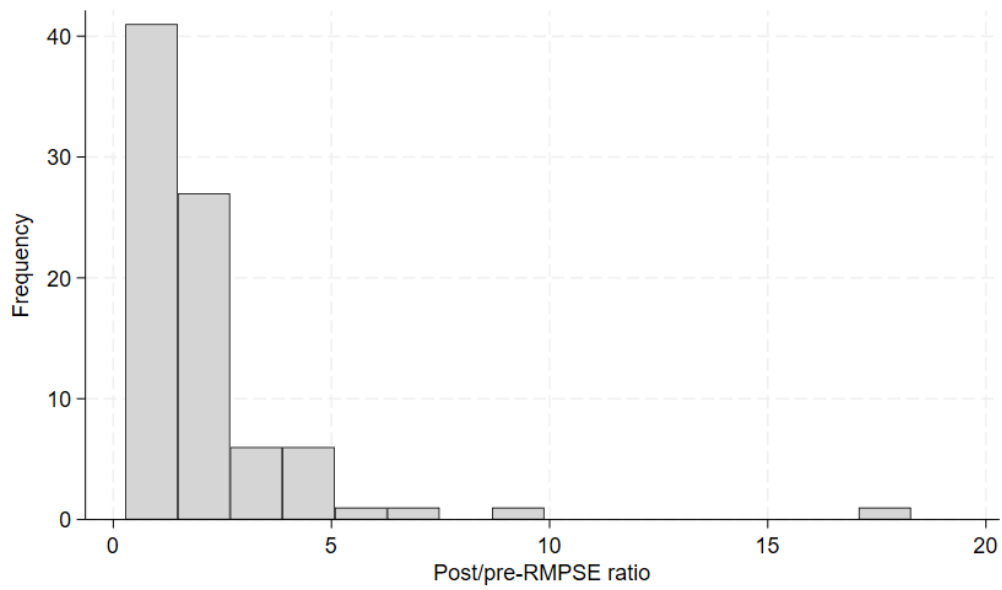


Figure A.6: Post/Pre-treatment Root Mean Squared Prediction Error(RMSPE) of Minneapolis and all placebo estimates.

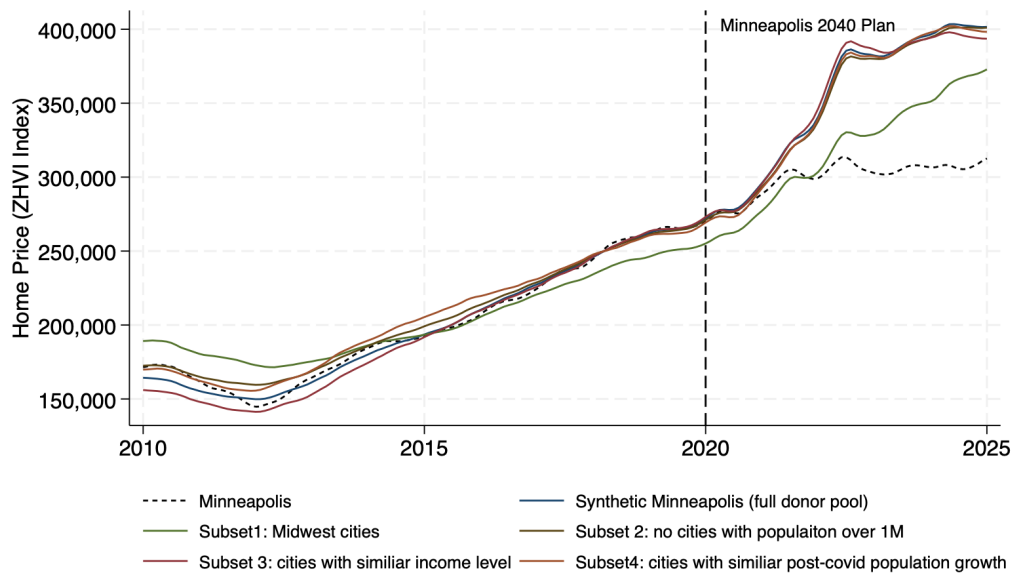
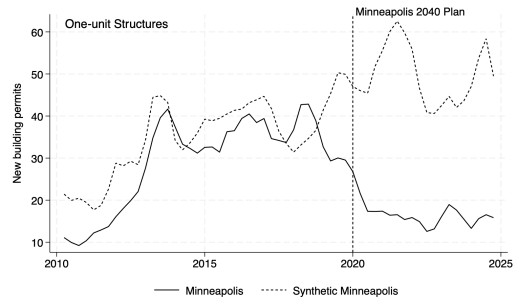
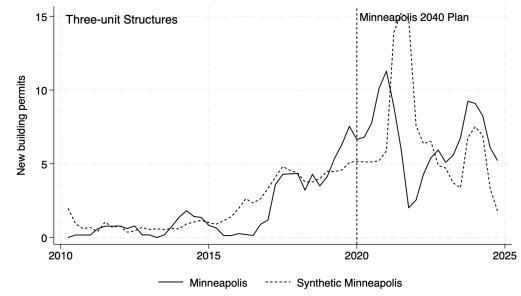


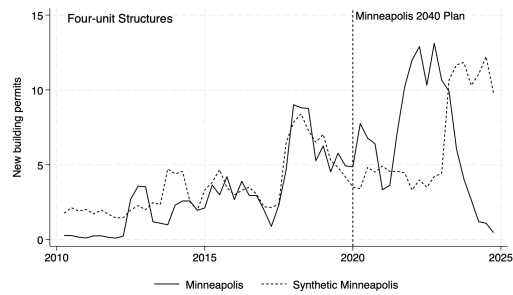
Figure A.7: Monthly trend in home price(ZHVI index) Minneapolis vs. synthetic Minneapolis, 2010-2025. Synthetic Minneapolis is constructed repeatedly and plotted using four different subset donor pools as well as the full donor pool.



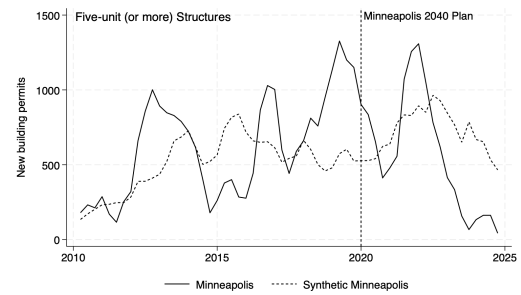
(a) Single-family homes



(b) Triplexes



(c) Quadraplexes



(d) Multi-family (five or more) structures

Figure A.8: Quarterly trend in newly issued residential permits by structure types, Minneapolis vs. synthetic Minneapolis, 2010-2024.

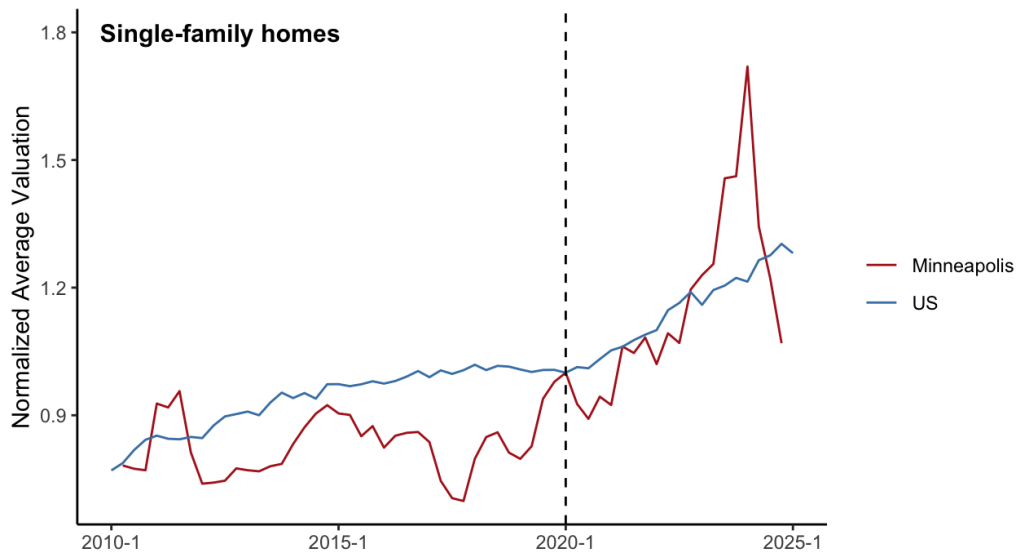


Figure A.9: Normalized new construction values single-family homes (to Jan. 2010), Minneapolis vs. USA, 2010-2025.

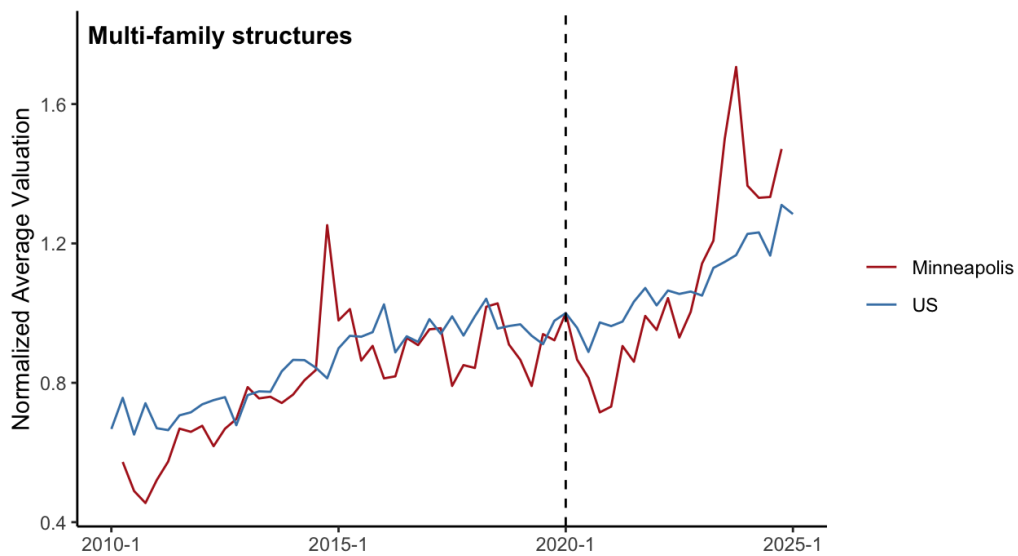


Figure A.10: Normalized new construction values multi-family homes (to Jan. 2010), Minneapolis vs. USA, 2010-2025.



## B Additional Crime Results

To explore the robustness of the housing results to crime outcomes, we implement a Differences-in-Differences approach where we regress either the home value or rental price index on a series of housing-related controls, violent and property crime data, a post-2020 indicator, and an interaction term for Minneapolis in the post-2020 period, which picks up the post-2020 changes in housing affordability in Minneapolis. We also include city and time fixed effects. We run this regression with and without the crime controls to explore the sensitivity of the post-2020 Minneapolis interaction term to controlling for crime outcomes. The specific regression equation is reported below, and the regression results are reported in Table B.1.

$$\begin{aligned}\log(Y_{it}) = & \beta_1 \log(\text{population}_{it}) + \beta_2 \text{white}_{it} + \beta_3 \text{black}_{it} + \beta_4 \text{poverty}_{it} + \beta_5 \log(\text{income}_{it}) \\ & + \beta_6 \text{homeownership}_{it} + \beta_7 \log(\text{housing units}_{it}) + \beta_8 \text{UR}_{it} + \beta_9 \text{single fam. housing}_{it} \\ & + \beta_{10} \log(\text{violent crime}_{it}) + \beta_{11} \log(\text{property crime}_{it}) + \beta_{12} \text{post2020}_t \\ & + \beta_{13} (\text{Minn.}_i \times \text{post2020}_t) + \alpha_i + \gamma_t + \epsilon_{it}\end{aligned}$$

Where:

- $\log(Y_{it})$  is either  $\log(\text{ZHVI}_{it})$  or  $\log(\text{ZORI}_{it})$
- $\text{white}_{it}, \text{black}_{it}$ : racial composition shares
- $\text{poverty}_{it}$ : poverty rate
- $\text{income}_{it}$ : median household income
- $\text{homeownership}_{it}$ : share of owner-occupied housing
- $\text{UR}_{it}$ : unemployment rate
- $\text{single fam. housing}_{it}$ : share of single-family housing units

- violent crime<sub>it</sub>, property crime<sub>it</sub>: logged crime rates
- post2020<sub>t</sub>: indicator for post-2020 period
- Minneapolis<sub>i</sub>: indicator for Minneapolis
- $\alpha_i$ : city fixed effects
- $\gamma_t$ : time fixed effects

In the baseline regression (column 1) without crime controls yields an estimated decline in post-2020 ZHVI for Minneapolis of 18%. Including crime controls has little impact on this estimate (column 2). For the ZORI regressions (columns 3 and 4), we see that crime has a negative, and significant impact on rental prices, but these effects are economically quite small. For example, a 10% increase in property crime would lower the rental index by 0.19%. Again, we see that the post-2020 Minneapolis interaction term is not meaningfully impacted by the inclusion of the crime variables.

Table B.1: Regression Results

VARIABLES	(1) ZHVI	(2) ZHVI	(3) ZORI	(4) ZORI
population	1.906*** (0.105)	1.922*** (0.106)	1.089*** (0.0878)	1.036*** (0.0878)
white	-0.00347*** (0.000473)	-0.00347*** (0.000474)	-0.00263*** (0.000387)	-0.00272*** (0.000384)
black	0.0204*** (0.00253)	0.0203*** (0.00253)	0.00535** (0.00218)	0.00480** (0.00217)
poverty	-0.0352*** (0.00222)	-0.0354*** (0.00223)	-0.00995*** (0.00194)	-0.00907*** (0.00194)
income	0.0412 (0.0741)	0.0352 (0.0746)	0.114* (0.0682)	0.171** (0.0689)
homeownership	0.000459 (0.00179)	0.000938 (0.00186)	-0.000903 (0.00157)	-0.000992 (0.00162)
housing units	-2.020*** (0.113)	-2.038*** (0.115)	-0.983*** (0.0914)	-0.886*** (0.0932)
UR	-0.00783*** (0.00156)	-0.00788*** (0.00156)	-0.00125 (0.00114)	-0.00142 (0.00113)
single fam. housing	-0.00185 (0.00241)	-0.00236 (0.00245)	0.0101*** (0.00215)	0.0109*** (0.00218)
violent crime		0.0113 (0.00800)		-0.0125** (0.00635)
property crime		-0.00797 (0.0107)		-0.0192** (0.00800)
post2020	0.621*** (0.0281)	0.620*** (0.0285)	0.326*** (0.0219)	0.300*** (0.0226)
MinnXPost	-0.180*** (0.0186)	-0.181*** (0.0187)	-0.139*** (0.0125)	-0.134*** (0.0125)
Constant	11.74*** (1.080)	11.82*** (1.086)	3.170*** (0.993)	2.252** (1.006)
Observations	1,320	1,320	930	930
Number of cities	30	30	30	30

*Standard errors in parentheses.*

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1