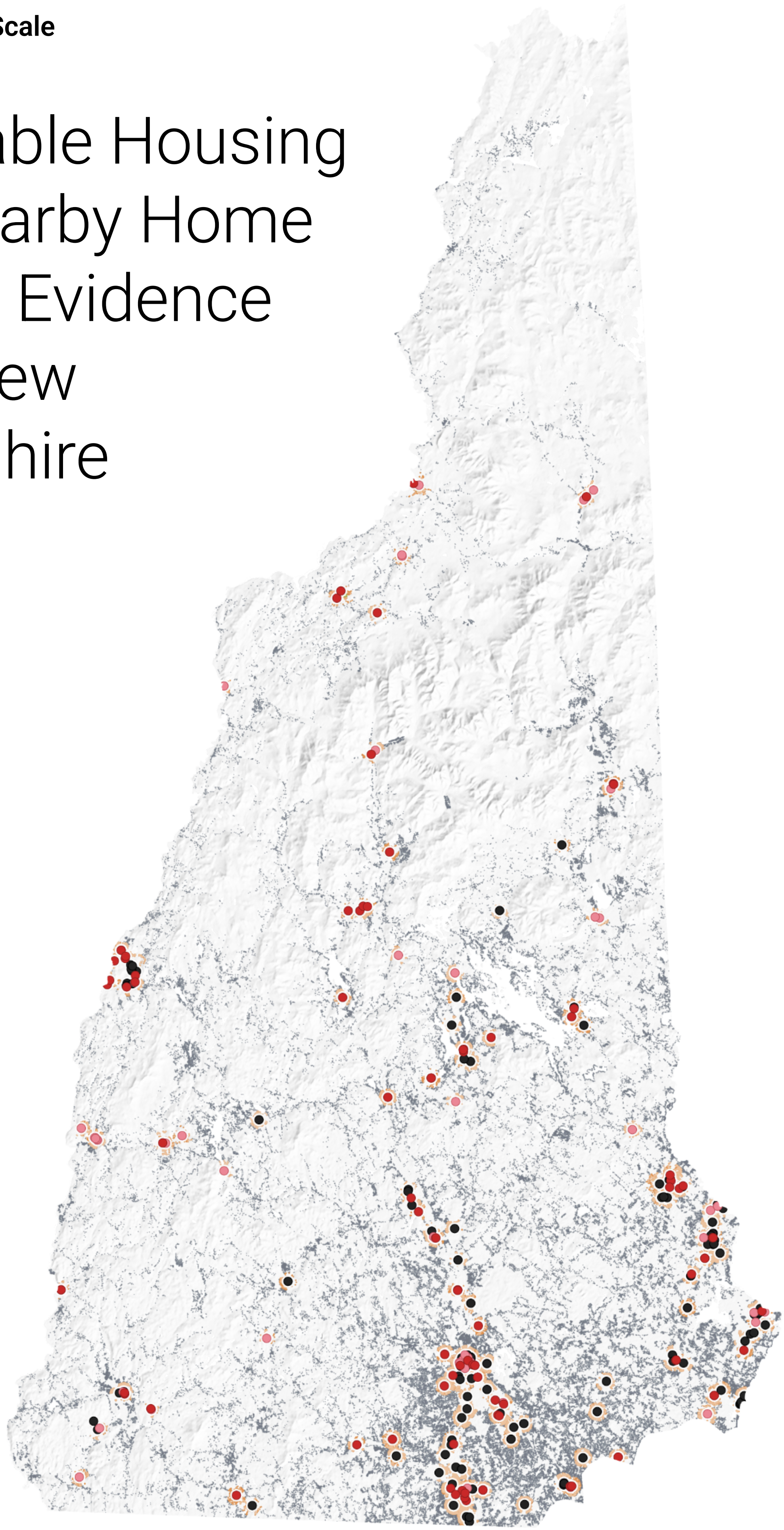
CommunityScale

Affordable Housing and Nearby Home Values: Evidence from New Hampshire



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Executive Summary

Affordable housing proposals in New Hampshire are often met with concerns that new developments will lower nearby property values. In this study, "affordable housing" refers to income restricted, publicly subsidized housing developments, rather than homes that are simply affordable to a particular household. Although property value concerns frequently shape local housing discussions, they had not previously been tested using statewide New Hampshire housing market data.

This study examined whether affordable housing developments completed between 2010 and 2023 affected nearby home sale prices. It also compared those effects with similarly sized market rate multifamily developments. The statewide results show no measurable average decline in nearby home values associated with affordable housing.

What We Studied: The analysis included 93 affordable housing developments with 3,124 units and 136 market rate multifamily developments with 11,334 units. These developments were linked to 68,488 open market home sales recorded between 2008 and 2025.

The study compared changes in sale prices for homes near each development before and after completion with changes for similar homes farther away in the same local housing market. The models accounted for differences in home characteristics, sale timing, and local market conditions. The findings were also tested using alternative distances, time periods, project sizes, and comparison groups.

Key Findings

1. Affordable housing had no measurable average effect on nearby home prices. Under the preferred statewide model, every 10 affordable units built within a quarter mile was associated with a statistically insignificant 0.17 percent increase in nearby sale prices. That is equal to about $680 on a $400,000 home. In practical terms, the study found no measurable average effect on nearby home values.

2. The preferred statewide model does not support a large average decline. The evidence rules out an average statewide property value loss of 5 to 10 percent. The study also found no construction period dip, opening shock, or delayed decline in value. This conclusion applies to the average statewide effect under the preferred model. It does not rule out a decline of that size for every individual project, subgroup, or neighborhood.

3. Affordable housing and market rate housing produced similar results. When the two types of development were compared, neither had a measurable average effect on nearby home values.

4. Most subgroup analyses showed no measurable effect. One negative estimate was found for small developments within a half mile, but further testing indicates that it reflects preexisting characteristics of the sites rather than the housing built there. Homes near these locations were already selling at a discount before construction, and the result was not confirmed by the study's repeat sales analysis, direct before and after comparison, random location test, or adjustment for multiple comparisons.

What This Means: For New Hampshire communities, these findings provide strong evidence that concerns about affordable housing lowering nearby property values are not supported by observed housing market data. This does not mean every development will have the same effect, since projects differ in design, location, scale, management, and surrounding market conditions. However, the statewide evidence indicates that affordable housing, on average, does not produce the widespread decline in nearby property values often assumed during local development discussions. Decisions about proposed affordable housing should focus on directly reviewable factors such as site design, traffic, infrastructure capacity, and overall project quality, rather than generalized concerns about declining property values.

Introduction

Affordable housing proposals in New Hampshire are frequently met with claims that new development will lower surrounding property values. Those claims shape planning board deliberations, neighborhood opposition, project timelines, and the broader political environment for housing production. But are they empirically supported? This paper examines that question using New Hampshire-specific evidence. The analysis combines a statewide inventory of affordable housing developments, a comparison set of market-rate multifamily developments, and a large set of typical open-market residential sales to estimate whether nearby affordable housing development is associated with changes in surrounding home prices. The goal is to move beyond anecdote and assumption and directly test the impact of affordable housing in the context of New Hampshire's own development patterns and housing market geography, which differ substantially from the dense metropolitan settings that dominate the existing research.

The paper is organized around four empirical research questions:

- Does new affordable housing lower, raise, or have no measurable effect on nearby residential property values in New Hampshire?
- Do any nearby property value effects differ from those associated with market-rate multifamily development?
- Do effects vary with distance from the development and with time before and after completion?
- Do effects vary across neighborhood and project characteristics, including urban-rural setting and development type or scale?

These questions address the multiple ways affordable housing could plausibly affect surrounding property values. New development may improve underused sites, add investment, and stabilize local conditions. It may also generate concern about congestion, fiscal pressure, or neighborhood change. The direction and magnitude of any effect therefore cannot be assumed in advance. It must be evaluated empirically, with attention to comparison groups, spatial scale, timing, and local context.

To answer these questions, this paper estimates how residual sale prices change in relation to nearby development over time. It tests whether affordable housing is associated with systematic price changes in surrounding residential sales. It also evaluates whether any such pattern differs from market-rate multifamily development, and whether estimated effects are concentrated in particular distance bands, time windows, settings, or project categories. This framing moves the paper beyond a simple yes-or-no debate and toward a policy-focused assessment of what the New Hampshire evidence does and does not show.

The findings do not show evidence that affordable housing developments in New Hampshire have a measurable negative impact on nearby property values. The results suggest that affordable housing does not create a greater property value impact than market-rate multifamily development. At the same time, the study does not claim that every project in every location will have no effect at all. The study contributes a careful, New Hampshire-specific analysis of the property value effects that can reasonably be ruled out based on the evidence.

Background

Like much of the country, New Hampshire faces significant housing affordability challenges: nearly 1 in 2 renter households spend at least 30% of income on gross rent, and more than 1 in 4 owner households with a mortgage spend at least 30% of income on monthly housing costs (U.S. Census Bureau 2024). These pressures help explain the demand for additional affordable housing supply and preservation.

In this paper, however, "affordable housing" is used in a programmatic rather than a cost-burden sense. The term specifically refers to income-restricted or publicly subsidized housing developments identified through administrative and program records. These include properties supported through public investment tools such as Low-Income Housing Tax Credits (LIHTC), tax-exempt bond financing, HOME Investment Partnerships Program, New Hampshire Housing financing, and project-based rental assistance. These tools support new construction, adaptive reuse, rehabilitation, and the preservation of existing subsidized housing. This distinction is important because different forms of affordable housing are developed, financed, and integrated into neighborhoods under different conditions. Accordingly, this paper focuses on the programmatic subset of affordable housing, which is most relevant to the research question because it is representative of the housing types which are still being proposed and developed.

Because this subset of affordable housing differs from market-rate multifamily housing primarily in its financing and regulatory structure, market-rate multifamily developments provide the most appropriate comparison group, allowing the analysis to distinguish between the effects of subsidized affordable housing and the broader effects of multifamily development itself. The practical question is not whether affordable housing differs from no development at all, but whether it produces different neighborhood effects than otherwise comparable market-rate multifamily development. The literature review that follows helps locate that question within the broader empirical record and clarifies why New Hampshire is an informative test case.

In Figure 1, three examples of affordable housing projects funded during the study period show the range of project types across the state. Local context, number of units, and project goals all differ across the three developments, though each reserves a set percentage of apartments for income-eligible households. These units are reserved for households that meet income eligibility requirements and are subject to long-term affordability restrictions. In all properties in our analysis sample, residents pay rent, but rents are limited to help ensure housing remains affordable for eligible households. Depending on the program, residents may include working families, older adults, veterans, people with disabilities, teachers, healthcare workers, retail employees, and others whose incomes fall within established eligibility limits.

Figure 1: Examples of New Hampshire Affordable Housing Projects

Ruth Lewin Griffin Place
Built 2022, Portsmouth

New construction · 64 units ·100% affordable

Ruth Lewin Griffin Place is a newly built affordable development in downtown Portsmouth, one of New Hampshire's most expensive markets. It shows how affordable housing can be delivered through new construction in a supply-constrained Seacoast city where land is scarce and prices are among the highest in the state.

Sugar River Mill
Rehabilitated 2011, Claremont

Rehabilitation · 162 units · 95% affordable

Sugar River Mill was converted to housing in the 1980s and acquired and rehabilitated in 2011 to keep its affordable units in service. It illustrates the preservation pathway, recapitalizing and extending the life of aging affordable housing rather than building new, which accounts for a large share of New Hampshire's affordable housing activity.

Conway Pines (Phase II)
Built 2016, Conway

New construction · 30 units ·100% affordable (age-restricted)

Conway Pines (Phase II) is a small, age-restricted addition to an existing affordable community in the Mount Washington Valley. It reflects the modest-scale, often senior-focused developments that help meet housing needs in New Hampshire's rural communities and resort towns.

What Makes New Hampshire Different

The 2024 American Community Survey (ACS) 5-year estimates report that New Hampshire had 1.38 million residents and 657,000 housing units; among occupied housing units, only 27% were renter-occupied (U.S. Census Bureau 2024). Owner-occupied housing, especially single-family housing, occupies an important place in both household wealth and public policy. Home prices have risen sharply in recent years. Based on New Hampshire deed records and Multiple Listing Service (MLS) listing data, the statewide median sale price increased from $210,000 in 2008 to $493,000 in 2025, reflecting growing pressure on the state’s housing market. As home values increase, concerns about property values often become a more prominent factor in local discussions and decisions about new housing development.

New Hampshire is also valuable as a study setting because of its substantial geographic variation. Much of the state’s housing activity is concentrated in the southern portion of the state along the Massachusetts border and the Seacoast, but this study also includes smaller cities, suburban towns, resort communities, and large rural areas. The state’s range of settings makes it possible to ask whether nearby price effects are consistent across places or instead depend on local context.

For example, a 30-unit development in downtown Manchester, where housing density is roughly 1,580 units per square mile, represents a relatively modest addition in one of the state's larger employment and housing markets. By contrast, a 10-unit affordable development in a rural setting such as Conway, where housing density can be closer to 70 units per square mile, can constitute a visible change in local supply. The two places also face different kinds of market pressure. Manchester reflects the dynamics of a larger regional center, while Conway displays the constraints of a lower-density, high-demand resort-area market.

These contrasts make New Hampshire an especially useful setting for testing whether the property value effects identified in larger metropolitan areas also hold across suburban, rural, and mixed-market communities.

Related Literature

The empirical literature does not support the claim that affordable housing lowers nearby property values. The prevailing finding is that affordable housing has either no detectable effect or a modest positive effect on nearby home prices, with the direction and magnitude of any spillover depending on local market conditions, project characteristics, and research design. The literature identifies several channels where property value effects may operate at once, though a quasi-experimental study can only realistically measure the net effect on observed prices. Affordable housing may remove blight, reduce vacancy, improve neighborhood appearance, affect crime or disorder, or change who is willing to buy nearby. At the same time, it may also increase housing supply or trigger concerns that vary across neighborhood types. Understanding these channels within the local context is how researchers try to tease out the potential counteracting effects.

Because affordable housing is not randomly sited, simply comparing neighborhoods with and without affordable housing can produce misleading results. Credible studies instead compare a neighborhood before and after a development is built or compare properties close to a development with properties farther away. These approaches are better able to distinguish the effects of housing development from the characteristics of a neighborhood itself.

Across the relevant literature, the strongest studies are those that employ the most rigorous, quasi-experimental research design that their data can support. Regression discontinuity is powerful when a sharp eligibility rule exists to exploit (Baum-Snow & Marion use the Qualified Census Tract threshold in their 2009 study). Alternatively, repeat-sales designs offer the best control for unobserved property quality because they track the same home across multiple transactions. Both methods, however, require more data than is often available, requiring either a usable rule or a robust local history of repeat sales. Where neither is available, which is often the case, researchers typically use hedonic difference-in-differences (DiD) models with distance rings that compare nearby and more distant property values while controlling for observable property characteristics. Although this approach cannot account for every unobserved difference between properties, it can be applied in a much wider range of settings.

Figure 2 summarizes the findings of related empirical literature, including the period of time studied, median housing density, distance analyzed, research design, and the key findings on the impact of affordable housing on nearby property values. Because the state of New Hampshire is significantly less dense than the areas studied by any previous reports, it is not clear how those findings extend to the New Hampshire context.

Figure 2: Summary of Related Literature

Study	Setting	Period	Density (units/mi²)	Distance	Design	Key finding
An et al. (2022)	Los Angeles County, CA	1987-2016	5,900	0.25 mi rings	Spatial DiD	+3.4% within 1/4 mi; positive across contexts

Study	Setting	Period	Density (units/mi²)	Distance	Design	Key finding
Stacy & Davis (2022)	Alexandria, VA	2000-2020	7,700	1/16 mi rings	Repeat-sales DiD	+0.09% per unit within 1/16 mi; small positive
Diamond & McQuade (2019)	15 states, 129 counties	1987-2012	1,100	Continuous	Spatial DiD	+6.5% low-income, -2.5% high-income; highly localized
Young (2016)	20 high-cost metros (Trulia)	1996-2006	8,400	0.38 mi (2,000 ft) rings	DiD	Null in 17 of 20 markets
Deng (2011)	Santa Clara County, CA	1987-2000	3,000	0.19 mi (1,000 ft)	Hedonic DiD	Significantly positive for nearly all projects
Baum-Snow & Marion (2009)	National metro QCTs	1994-1999	2,500	Census tract (RD)	Regression discontinuity	+14.9% per 100 units in declining/stable tracts
Ellen (2007)	New York City, NY	1986-2000	29,000	0.19 mi (1,000 ft) rings	Hedonic DiD	Rehab +8.9% to +11.4%; federal programs mixed
Schwartz et al. (2006)	New York City, NY	1980-1999	29,000	0.38 mi (2,000 ft) rings	Hedonic DiD	Positive spillovers, decaying with distance
Pollakowski et al. (2005)	Suburban Boston, MA	1982-2003	4,800	Impact area (custom)	Hedonic	No effect on nearby single-family values

Note. Study-area density is the project-weighted median housing density of the census tracts containing each study's subsidized developments, from HUD LIHTC project locations, ACS 5-year housing units (B25001), and Census land area, matched to the nearest ACS vintage (earliest 2006-2010 used as a labeled proxy for pre-ACS periods). DiD is difference-in-differences; QCT is Qualified Census Tract (a low-income tract targeted for tax credits).

Ellen (2007) offers the most useful framework for interpreting the evidence from existing literature: spillover effects depend on baseline market conditions, and on what a new development replaces. When affordable housing replaces blight, vacancy, or obsolete structures, it tends to raise nearby property values. In already-strong markets that positive impact lessens toward zero, and in higher-income neighborhoods it can reverse into a modest decline at the shortest distances. Effects are consistently localized, concentrated near the project and evaporating at greater distances. Although public opposition to affordable housing is often focused on assumptions about who will move in, the evidence does not bear them out: where negative spillover effects appear at all, researchers attribute them to weak management, physical deterioration, or clustering of distressed units, not to housing affordability or the residents (Nguyen 2005; Center for Housing Policy 2009; A Mark Foundation 2023). Taken together, the literature does not support a general nearby-value penalty; specific effects vary with market strength, neighborhood composition, and project form.

Several implications follow for this study. The prior literature finds no systematic depreciation in property values, so the useful question is not whether nearby prices fall, but how large a decline the data can plausibly rule out. Where spillover effects are detected, they tend to be localized, within 0.1 to 0.5 miles, which will serve as the starting point for this analysis. The literature also repeatedly points to neighborhood income, market conditions, project type, concentration, and management as factors that may shape results, making analysis by place and project type an important part of this study rather than an optional extension. Methodologically, the strongest previous studies rely on hedonic difference-in-differences and distance-sensitive spatial designs, which together form the backbone of the approach used here.

The existing literature also leaves an important and substantive gap. Most prior studies compare affordable housing either to places without development or to broader neighborhood trends, not to market-rate multifamily development. For local communities and decision-makers, however, that is often the more realistic comparison. This study addresses that gap by comparing nearby affordable and market-rate multifamily developments within the same empirical framework. If affordable housing does not have a meaningfully different effect on nearby property values than market-rate multifamily development, then objections based specifically on concerns about property values become more difficult to support.

A separate issue with existing studies is whether findings from denser urban markets apply to a lower-density state like New Hampshire. Even the less-dense settings in prior studies are substantially denser than the neighborhoods examined in this study. The least-dense set of study areas in Diamond and McQuade (2019) is roughly 1,100 housing units per square mile – about four times higher than the estimated median project-weighted density of 290 housing units per square mile in New Hampshire. This study addresses that gap with New Hampshire-specific evidence.

Methods

Overview

This study examines whether new affordable housing developments are associated with changes in nearby home sale prices. At a high level, it compares homes sold very close to a newly completed affordable housing development with other homes sold somewhat farther away in the same local market over the same time period. The goal is not only to observe whether prices rise or fall after a new affordable housing development opens, but also to determine whether price changes differ for homes based on proximity to a new affordable housing development, than for other nearby properties facing the same broader market conditions.

The formal framework used to make that comparison is a difference-in-differences model. This approach was chosen because affordable housing developments are not randomly sited. Sites selected for development may already differ from other locations because of land availability, zoning, redevelopment pressures, neighborhood characteristics, or local market trends, and those underlying differences may themselves be related to home prices. A simple comparison between homes near affordable housing and homes elsewhere would therefore be difficult to interpret and only identify correlation, not causality. The difference-in-differences design sidesteps that issue by separating changes associated with affordable housing development completion from preexisting differences between locations and from broader housing-market trends.

Primary Research Design

The main analysis is estimated at the level of the individual home sale, meaning that each observation in the dataset is a single real estate transaction. For each sale, the analysis measures its exposure to nearby affordable housing development. Exposure is defined by the property's proximity to a new development, the timing of the sale relative to the development's opening, and the number of affordable housing units added. This sale-level design is the paper’s primary research strategy because it measures exposure directly at the transaction level and keeps the observation structure simple and transparent. The lead model specification relates sale prices to differences in nearby affordable housing exposure, with the exact exposure definition described in the next section. The model also includes census-tract-by-year fixed effects, which account for broader differences in price levels across places and over time. This means the estimated relationship is identified after considering broader market conditions that vary geographically and over time.

The lead model regresses the log of sale price on nearby affordable housing exposure while controlling for property characteristics (hedonic model) and census-tract-by-year fixed effects:

$$\ln(P_{it}) = \beta(U_{it}/10) + X_{it}\gamma + \alpha_{ct} + \epsilon_{it}$$

where:

- P_{it} is the sale price of property i at time t.
- U_{it} is the number of completed affordable housing units nearby at the time of sale
- β is the estimated association between affordable housing exposure and sale price, measured per 10 nearby affordable units
- X_{it} represents property and transaction controls
- α_{ct} represents census-tract-by-year fixed effects, which compare sales within the same local area and year
- ϵ_{it} is the remaining unexplained variation in sale prices

The coefficient β is interpreted as the percent change in sale price associated with ten additional completed affordable units nearby. A companion binary specification replaces the nearby unit count with an indicator for whether any affordable development has completed within the treatment radius, allowing the analysis to distinguish between the marginal effect of additional units and the coarser effect of a project's arrival.

The key identifying assumption is that, absent new affordable housing development completion, sale prices for more-exposed and less-exposed homes within the same local market would have followed similar trends. The paper evaluates that assumption using event-study estimates and related diagnostic checks. Those event studies test whether prices near future development sites were already moving differently before completion, and whether any observed post-completion effect appears suddenly, gradually, or not at all. This matters because the design's credibility depends on nearby and slightly farther-away homes serving as reasonable local comparisons before treatment begins.

How Exposure to Development is Measured

Within that design, exposure is defined along three dimensions: distance, timing, and intensity. Distance determines whether a home is close enough to a development to be meaningfully exposed. Timing determines whether the development had already been completed when the home was sold. Intensity determines how much affordable housing was nearby at the time of sale. These three dimensions work together to translate the broad idea of "living near an affordable housing development" into a measurable treatment definition that can be used consistently across the statewide dataset.

In the primary specification, a sale is considered exposed if it occurs within 0.25 miles of a development, and the main comparison group consists of sales located between 0.25 and 1.0 miles away. Sales beyond 1.0 mile are excluded from the lead design so the comparison remains local rather than relying on homes that may differ more substantially in ways that are harder to observe. Treatment begins at completion rather than at permit issuance or construction start, so a development contributes to exposure only once its units are actually in service. Exposure intensity is measured in two ways: first, as the number of completed affordable units within 0.25 miles, scaled in tens of units; and second, in the companion model, as an indicator for whether any affordable development has been completed within that radius.

The paper also tests whether its conclusions depend on the definition of nearby development exposure. To examine distance, the model is re-estimated using alternative treatment radii, asking whether the results change when "nearby" is defined more narrowly or more broadly. It also uses concentric distance bands to test whether any relationship is strongest very close to a development and fades with distance, rather than assuming the same effect everywhere within a single buffer zone. A density-adjusted adaptive radius provides another check by allowing the treatment area per development to vary with the density of surrounding housing stock, which helps address the possibility that a quarter-mile buffer may capture different kinds of local markets in the suburban and rural settings that are characteristic of New Hampshire. To examine development scale, the analysis uses a proportional exposure measure that expresses nearby completed affordable units relative to the baseline local housing stock, asking whether the same number of units matters differently in smaller versus larger surrounding markets. Finally, timing is tested through event-study models centered on the first nearby affordable development completion, which show if price patterns were already diverging before completion, and whether effects appear immediately, emerge gradually, or fade over time. Taken together, these checks test whether the main findings depend on a particular choice about distance, scale, or timing.

Analysis by Place and Project Type

The study also examines whether results differ across neighborhood settings and development characteristics, rather than assuming that the statewide average applies equally in all contexts. To do so, the analysis uses interacted regressions within a common framework, which allows differences across groups to be evaluated directly rather than inferred from separate models. On the neighborhood side, it tests for differences across lower-, middle-, and higher-income neighborhoods, as well as across urban and rural settings. On the development side, it examines variation by property type (age-restricted vs. general population), new construction vs. rehabilitation project, the number of units in the development, and the concentration of income-restricted or subsidized units within the development. These results are interpreted more cautiously than the primary statewide estimates because analyses of these neighborhood and development categories rely on smaller effective samples and, in some cases, less precise identifying variation. Accordingly, patterns across these groups are treated as informative only where the groups appear to follow similar price patterns before affordable housing development is completed.

Affordable Versus Market-Rate Comparison

A separate question in the study is whether the nearby price effect is specific to affordable housing or instead reflects the arrival of multifamily housing more generally. This is a policy-relevant extension because many local debates are about whether affordable multifamily housing is viewed differently from market-rate multifamily housing on otherwise comparable sites. To address that question, the study estimates a joint sale-level model that includes nearby affordable exposure and nearby market-rate multifamily exposure at the same time.

The estimating equation for that comparison is:

$$\ln(P_{it}) = \beta_{AH}(U_{AH,it}/10) + \beta_{MR}(U_{MR,it}/10) + X_{it}\gamma + \alpha_{ct} + \epsilon_{it}$$

where:

- P_{it} is the sale price of property i at time t.
- $U_{AH,it}$ is the number of completed affordable housing units nearby at the time of sale
- $U_{MR,it}$ is the number of completed market-rate multifamily units nearby at the time of sale
- β_{AH} is the estimated association between affordable housing exposure and sale price, measured per 10 nearby affordable units
- β_{MR} is the estimated association between market-rate multifamily exposure and sale price, measured per 10 nearby market-rate units
- X_{it} represents property and transaction controls
- α_{ct} represents census-tract-by-year fixed effects, which compare sales within the same local area and year
- ϵ_{it} is the remaining unexplained variation in sale prices

The contrast β_{AH} - β_{MR} tests whether affordable housing has a distinct nearby price relationship relative to market-rate development. As a secondary check, the analysis repeats the comparison in specific places where affordable and market-rate developments occur under more similar neighborhood and market conditions. This helps assess whether any difference reflects affordability status or simply the different kinds of places where each type of development tends to be built.

Alternative Benchmark Models

In addition to the primary sale-level design, the study estimates several benchmark models that use different observation structures and identifying assumptions. These models make it easier to compare the New Hampshire findings with prior studies and test whether the broad conclusions depend on the lead model alone. Each benchmark also involves tradeoffs, however, so the sale-level design remains the primary framework.

The main alternative benchmark uses sale-development pairs, classifying each sale as near or farther from a particular development and as occurring before or after its completion. This structure is common in the literature, but a sale may appear more than once if it is linked to multiple developments, giving greater representation to sales near several projects. The primary design instead counts each transaction once and combines all nearby exposure into a single measure. This keeps the interpretation centered on the relationship between sale prices and the development activity surrounding each property.

Two additional benchmarks address other potential limitations. A repeat-sales model follows changes in the price of the same home over time, accounting for property characteristics that remain constant. A staggered-timing model examines groups of projects completed at different times and prevents already-exposed homes from serving as untreated comparisons for later projects. Whereas the repeat-sales model addresses unobserved differences between properties, the staggered-timing model addresses differences in when projects were completed. Both designs use less of the available data: the repeat-sales model includes only homes sold more than once, while the staggered-timing model restricts comparisons within each group of projects. These narrower samples can reduce statistical precision, so the models serve as supporting checks rather than replacements for the primary sale-level design.

Robustness and Inference

Several additional tests examine whether data quality or modeling choices drive the findings. The primary models are re-estimated using only sales and developments with high-precision geographic coordinates. Spatial placebo tests randomly relocate developments, while temporal placebo tests assign artificial completion dates during the period before projects were actually completed. These tests assess whether the model produces similar estimates where no development-related effect should be present. A specification-curve analysis then brings together results across many reasonable choices about distance, timing, controls, sample restrictions, and model structure. This provides a broader view of whether the overall conclusion is stable or depends on a narrow set of analytical decisions.

The analysis also tests whether the findings depend on how statistical uncertainty is measured. Because nearby home sales may be affected by the same local market shocks, the primary sale-level models use Conley spatial standard errors, which allow unexplained price movements to be correlated across nearby transactions. The distance cutoff follows the geography of each model: it is set to one mile in the primary specification and adjusted when a narrower outer comparison ring is used.

As additional checks, uncertainty is recalculated by grouping sales geographically by census tract and, separately, according to the dominant nearby development. Because the development-based approach produces a relatively limited number of clusters, a wild cluster bootstrap provides an additional finite-sample check on the reliability of the resulting statistical tests. In the event-study and sale-development pair models, uncertainty is clustered by both development and sale, accounting for dependence along both dimensions. Together, these approaches test whether the statistical conclusions depend on any one method of estimating uncertainty.

Data

Summary of Data Studied

This analysis studied over a decade of affordable housing across New Hampshire, including:

- **93 new affordable housing developments** with 3,124 affordable housing units completed 2010-2023
- **136 new market rate multifamily developments** with 11,334 units completed 2010-2023
- **68,488 nearby home sales** spanning 2008-2025

Sources

This study combines transaction, listing, administrative, commercial, and Census-derived data to construct the affordable housing development sample, the market-rate multifamily comparison sample, and the residential sales panel. All developments and sales are located in New Hampshire, while neighborhood characteristics are measured at the census-tract level.

Residential transaction data come from The Warren Group deed records for New Hampshire sales from 2008 through 2025 and are supplemented by PrimeMLS listings over the same period. The Warren Group provides the core transaction outcomes, including sale price and sale date, while PrimeMLS contributes property characteristics used to classify transactions and control for differences among the homes sold.

The affordable housing development sample covers projects completed from 2010 through 2023 and is assembled from three administrative sources: the National Housing Preservation Database (NHPD), New Hampshire Housing administrative records, and the federal Low-Income Housing Tax Credit (LIHTC) database. These sources are used jointly to identify affordable multifamily developments across the state, reconcile overlapping records, and validate project characteristics and completion dates. The market-rate comparison sample is drawn from CoStar multifamily development records for New Hampshire developments completed during the same period. These records are used only in the affordable-versus-market-rate comparison and do not define exposure in the primary affordable housing analysis. Two contextual sources are added at the census-tract level. Neighborhood income is measured using American Community Survey (ACS) five-year estimates matched to the year of sale. USDA Rural-Urban Commuting Area codes, which classify census tracts using settlement patterns and commuting connections, are used to group New Hampshire tracts into urban, suburban, and rural categories.

Sample Construction

After reconciling duplicate and overlapping affordable housing records, 150 affordable housing projects completed from 2010 through 2023 were confirmed as distinct physical projects with verifiable locations. Each project was matched to the tax parcel or parcels it occupies using the New Hampshire GRANIT statewide parcel database. The matching process combined geographic coordinates, parcel addresses, and manual review where the location remained uncertain. When a project occupied multiple adjoining or functionally-related parcels, those parcels were combined into a single site. Distances to nearby sales were measured from the actual boundary of the development site rather than from a single address or map point.

The resulting statewide affordable housing sample represents 5,560 units. Of these, 93 projects with 3,124 units are new construction or adaptive reuse developments and form the primary treatment sample. The remaining 57 developments with 2,436 units are rehabilitation or preservation projects retained for descriptive reporting and secondary analyses. Since those properties already exist in the neighborhood they do not carry the same potential concerns from neighboring property owners as new properties.

The market-rate records underwent the same site-matching and duplicate-reconciliation process. The final comparison sample contains 136 New Hampshire multifamily developments completed from 2010 through 2023, representing 11,334 units. These developments are used only in the analysis comparing new affordable housing to new market-rate multifamily developments. The full sample is summarized in Figure 3.

Figure 3: Affordable Housing and Market-Rate Projects Sample

Project sample	Projects	Total units	Median project size (units)	Represented towns	Exposed sales
Affordable: new construction or adaptive reuse	93	3,124	28	42	7,111
Affordable: rehabilitation or preservation	57	2,436	31	31	2,571
Market-rate multifamily	136	11,334	48	34	7,135

Notes. Exposed sales refers to the number of distinct lead analytic sales within 0.25 miles of a completed project of that type by the sale date; a sale near more than one project type is counted under each, so the columns overlap and do not sum.

After cleaning, geocoding, and adding property characteristics, the statewide sales file contains 484,230 residential transactions from 2008 through 2025. The analysis is restricted to arm's-length sales of single-family homes, condominiums, and mobile or manufactured homes. An arm's-length sale is a transaction between independent buyers and sellers under ordinary market conditions. Family transfers, foreclosures, related-company transfers, and other transactions that may not reflect market value are excluded. The primary affordable housing model uses 68,488 sales located within one mile of a studied affordable development. Of these, 7,111 occurred within 0.25 miles of an affordable development completed by the sale date. Appendix A describes the data sources in more detail and the cleaning, deduplication, geocoding, site matching, and cross-source reconciliation procedures.

Descriptive Statistics

Affordable housing developments were completed throughout the 2010–2023 study period, although production varied by year. Across the full sample of 150 projects (including new construction and rehabilitation), project completions averaged 10.7 per year but varied considerably from one year to the next, with the highest activity in 2017 and 2023. New construction accounted for most projects in most years, while rehabilitation and preservation activity was more episodic and contributed to several of the larger annual changes in unit production (shown in Figure 4).

The new affordable housing construction sample, which serves as the primary sample for the remaining analysis, includes developments of varied size and occupancy type. Most are modest in scale, but the sample also contains larger developments capable of producing more substantial changes in nearby housing supply. General-occupancy and age-restricted properties are both well represented, while supportive-housing developments make up a much smaller segment (shown in Figure 5).

Geographically, the developments are most concentrated in Hillsborough and Rockingham counties, New Hampshire's more populous counties, but they are not confined to one metropolitan corridor. The sample extends across urban, suburban, and rural census tracts and includes projects in both southern New Hampshire and less densely developed parts of the state. That variation is particularly valuable because much of the existing research focuses on dense metropolitan markets (shown in Figure 6).

Figure 4: Affordable Housing and Market-Rate Projects by Completion Year

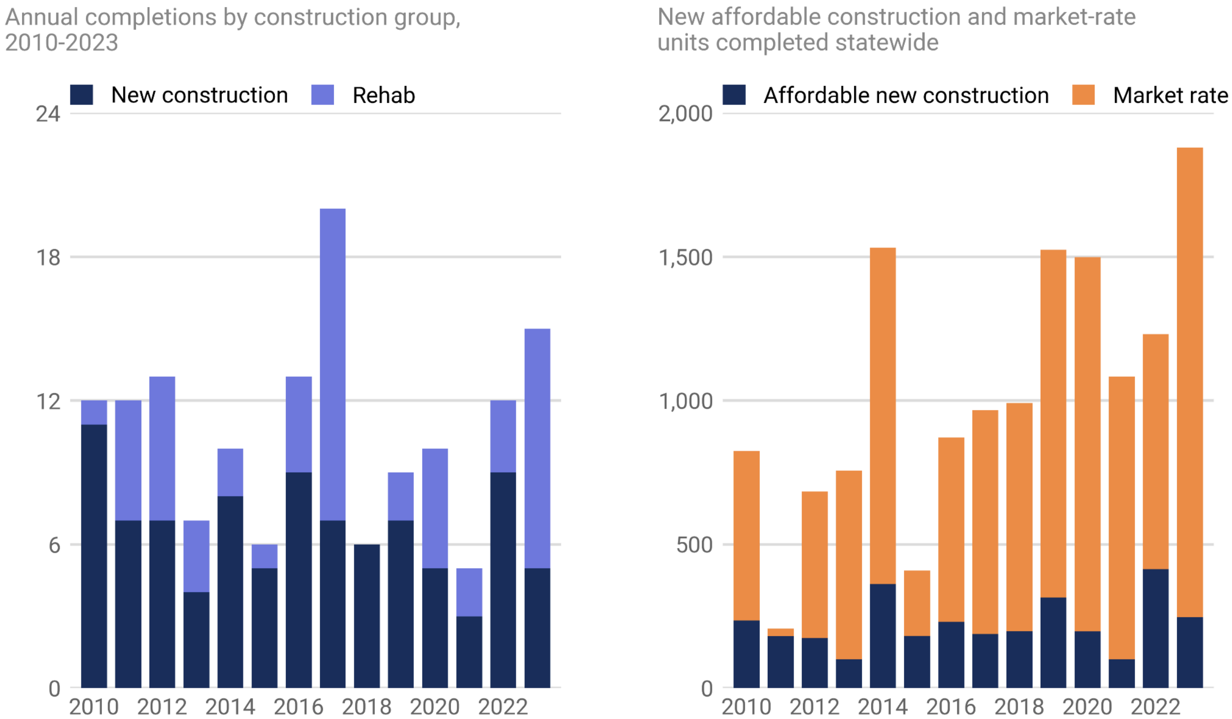
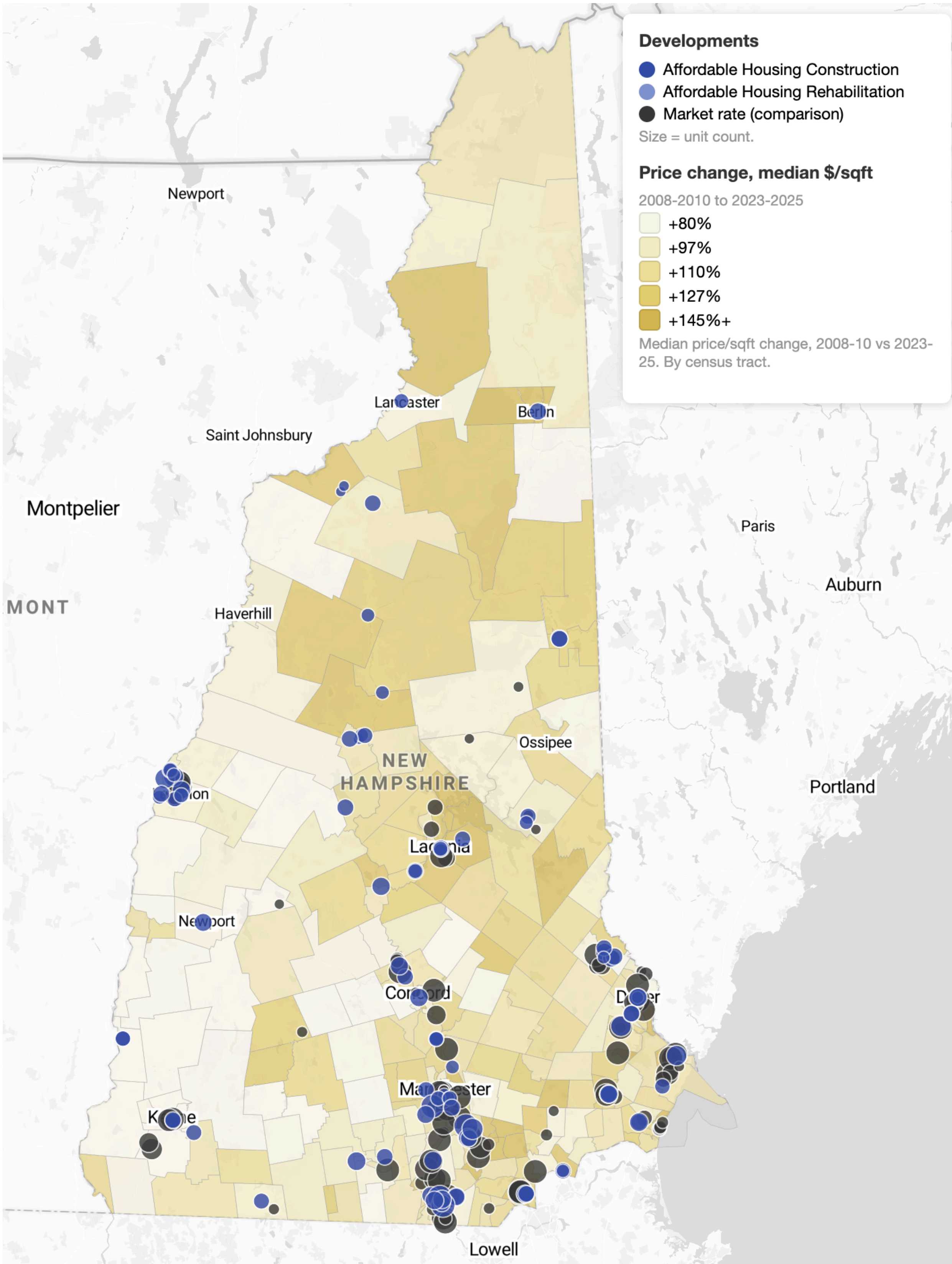


Figure 5: Affordable Housing Primary Sample Characteristics

Category	Projects	Share
General occupancy	58	62%
Senior housing (age-restricted)	32	34%
Supportive housing	3	3%
Fully affordable (100% income-restricted)	81	87%
Urban tract	47	51%
Suburban tract	24	26%
Rural tract	22	24%
Low-income tract	27	29%
Middle-income tract	36	39%
High-income tract	30	32%
Small: 1-20 units	23	25%
Medium: 21-43 units	51	55%
Large: 44+ units	19	20%

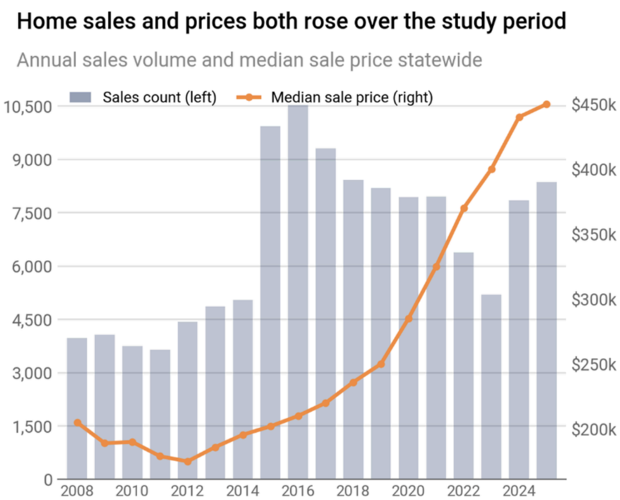
Notes. New-construction affordable projects (N = 93), the primary sample that carries treatment in the lead model. Occupancy, geography, project size, and tract income each partition the projects; fully affordable is a standalone share. Tract income is the ACS category of the project's tract at placement. Shares are of all new-construction projects.

Figure 6: Affordable and Market-Rate Multifamily Developments in New Hampshire, 2010–2023



The residential sales sample covers a broad range of prices, property types, and neighborhood settings. More important for the research design, treated sales (those exposed to completed affordable development) remain a minority of the sample, leaving a substantial group of nearby transactions that provide local comparisons. The two groups are similar in price and neighborhood income but differ in several expected ways. Treated sales are more likely to be condominiums, somewhat more rural, and modestly smaller. These differences reflect where multifamily housing is built and reinforce the need to account for property characteristics and highly localized market conditions. The sale-level model addresses them through housing controls and census-tract-by-year fixed effects, while the table reports the underlying comparisons.

Figure 7: New Hampshire Residential Sales Timeline and Characteristics



Characteristic	Treated	Comparison
Mean sale price	$315,828	$304,104
Condominium	44%	29%
Urban tract	60%	68%
Rural tract	26%	16%
Bedrooms	2.6	2.8
Living area (sq. ft.)	1,723	1,835
Market-rate units within 0.25 mi	16.6	11.5

Notes. Treated sales (N = 7,111) have at least one affordable development completed within 0.25 miles by the sale date; comparison sales (N = 61,377) do not. Entries are group means, except condominium, urban tract, and rural tract, which are group shares.

Results

Does new affordable housing have any effect on nearby property values?

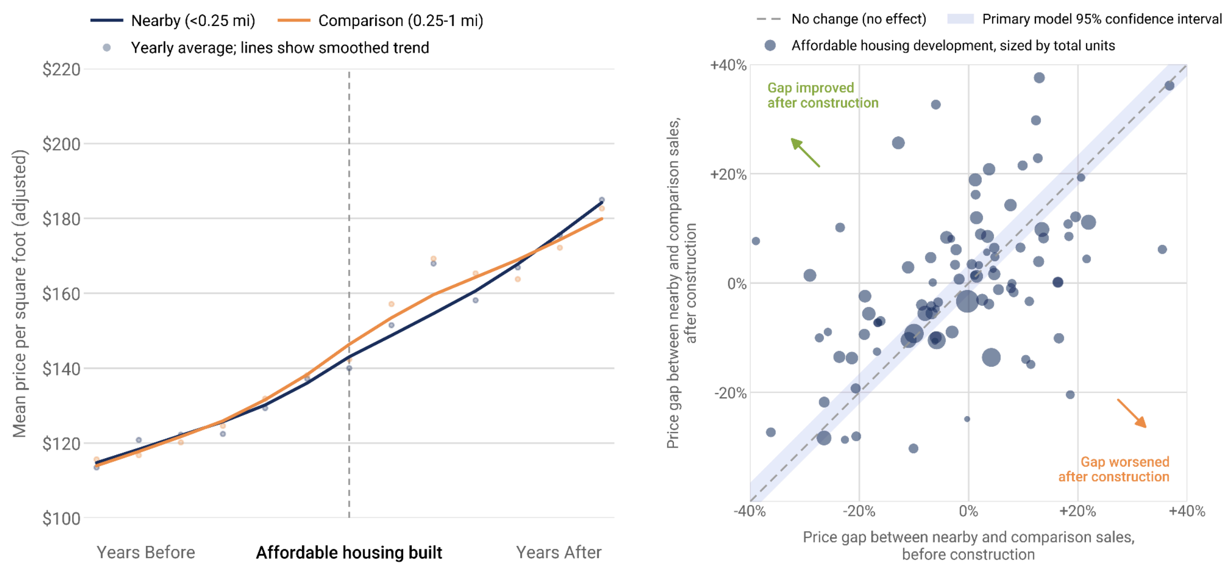
New affordable housing completions do not show a measurable effect on nearby home prices. Across a set of complementary model specifications, interpreted alongside robustness and identification checks, the estimated effects are consistently statistically indistinguishable from zero. The analysis gives the greatest weight to specifications that use more of the available sales data and show that nearby and comparison homes were following similar price trends before development occurred.

On that basis, the evidence points to a near-zero average effect, with no indication of a sizable decline in nearby home values.

The most informative model is the sale-level per-unit specification. It is preferred because it uses the full set of individual home sales and measures affordable housing exposure by the number of completed units nearby, rather than only whether any development occurred. This gives the model more information about the scale of nearby development and makes it the most precise specification in the analysis. It also passes the event-study diagnostic, which tests whether prices near future affordable housing sites were already diverging from comparison areas before the developments were completed. The event-study pattern in the left panel of Figure 8 shows no visible break in nearby price trends after completion, meaning prices near future affordable housing sites were not already moving differently before completion, and they do not show a construction-period dip, opening shock, or delayed decline afterward.

The right panel of Figure 8 plots the percent gap between the near and far sales prices for each affordable housing development. The plot does not show any consistent relationship between sales price changes across developments and the 95% confidence interval straddles the diagonal slope that represents a null effect. The first row of Figure 9 reports the exact results: the estimate is small, and slightly positive, and statistically indistinguishable from zero. More importantly, the result is measured with enough precision to rule out large negative effects. The one-sided 95% lower bound is -0.38% per 10 new units of affordable housing built within 0.25 miles (Figure 9). This means the preferred model does not confidently detect any decline in nearby home values, and that an average loss larger than 0.38% per 10 nearby affordable units would be unlikely to fit the observed sales patterns.

Figure 8: Main Model Parallel Trends Test and Results



Other model specifications reach the same broad conclusion, though they are less informative individually than the main sale-level per-unit model (results shown in Figure 9). The binary sale-level model, which measures only whether any affordable development had occurred nearby, also finds no statistically significant effect; this model is less precise because it does not use information about the number of units added. Traditional paired difference-in-differences, repeat-sales, and other benchmark models likewise do not produce reliable evidence that affordable housing lowers nearby prices. Because the benchmark models are less precise, they are treated as supporting checks rather than headline estimates. The full event-study coefficients, pre-trend tests, and related diagnostics are reported in Appendix B.

Figure 9: Main Model and Benchmark Results

Specification	Estimate	Scale	p-value	One-sided 95% lower bound	Parallel trends check
Sale-level, per-unit model (DiD)	+0.17%	per 10 units	0.617	-0.38%	Pass
Sale-level, binary model (DiD)	+0.37%	any nearby	0.814	-2.19%	Pass
Paired sale-development model (DiD)	+0.66%	any nearby	0.796	-3.45%	Pass
Repeat-sales model	+1.81%	any nearby	0.360	-1.42%	NA
Stacked, staggered per-unit model (DiD)	-0.54%	per 10 units	0.749	-3.26%	Pass

Notes. All models use a 0.25-mile treatment radius with a 0.25-to-1.0-mile comparison ring and include property and transaction controls. Fixed effects and inference by model: the sale-level models use tract-by-year fixed effects with Conley spatial-HAC standard errors; the paired sale-development model uses tract-by-year fixed effects with standard errors clustered by development; the repeat-sales model uses standard errors clustered by property; the staggered-timing model uses stack-by-tract-by-year fixed effects with standard errors clustered by sale. The repeat-sales and staggered-timing models are estimated on sales within five years of completion; the other models use the full panel. The parallel trends check is not applicable to the repeat-sales design.

Does the definition of exposure (distance, intensity, or timing) change the results?

The main model in the previous section defines exposure as the number of completed affordable units within 0.25 miles of a home at the time of sale. That definition could miss an effect if the relevant geography, scale, or timing were different. A fixed quarter-mile radius could overlook effects concentrated closest to a project or impose the wrong definition of "nearby" across urban, suburban, and rural settings. A raw count of completed units could also miss effects that depend on the size of the development relative to the surrounding housing stock, or effects that begin before completion when a project first becomes known. To test those possibilities, the analysis re-estimates the model using alternative distance bands, a density-adjusted radius, a proportional measure of affordable housing exposure, and different development timing windows.

Overall, the near-zero result does not appear to be driven by any single choice about how exposure to affordable housing is measured by distance, timing, or intensity.

Distance is the most direct place to test whether the main exposure definition is too narrow or too broad. This is especially important in New Hampshire, where affordable housing developments are spread across compact urban neighborhoods, village centers, suburban corridors, and lower-density rural settings. A quarter mile may capture several blocks of nearby sales in Manchester, Nashua, or Portsmouth, but include a much thinner set of nearby transactions in smaller towns or rural areas. The main model compares sales within 0.25 miles of completed affordable units to sales between 0.25 and 1.0 miles away. A narrower local-control version keeps the same inner treatment radius but compares those nearby homes only to homes 0.25 to 0.5 miles away. That estimate is -0.16% per 10 units, compared with +0.17% in the main model. A broader specification that treats homes within 0.5 miles as exposed estimates +0.01%. These fixed-radius alternatives do not produce statistically reliable evidence of a measurable price effect.

Fixed radii, however, are only one way to define nearby exposure. An effect could be concentrated among the homes closest to a project, then diluted when all homes within 0.25 miles are grouped together. The same distance could also represent a different kind of neighborhood relationship across New Hampshire's housing markets: walking-distance adjacency in an urban neighborhood, a nearby subdivision in a suburban town, or a more dispersed rural setting. Ring-based and continuous-distance checks, reported in Appendix B, test for a gradient by distance from the development. They do not provide statistically reliable evidence of a negative effect concentrated at the project boundary, though some distance-specific estimates are imprecise. The adaptive-density model addresses the urban/rural concern by allowing the treatment radius to vary with local housing density; it estimates -0.57% per 10 units and is not statistically significant. Taken together, the distance results do not prove that distance never matters, but they provide no evidence of a clear, detectable spatial penalty that the main 0.25-mile model is missing.

Scale raises a related measurement issue. The main model uses unit counts because it directly measures the number of completed affordable units near each sale. But given the variation in local market size described above, the same number of units could represent different levels of neighborhood change in different places. The analysis therefore re-estimates exposure as the affordable share of nearby housing stock, rather than simply the number of completed units nearby. This proportional measure is best read as a sensitivity check: the dataset observes developments completed during the study period more completely than it observes the full history and exact location of affordable housing that already existed before the sample period. Even with that limitation, the proportional-exposure estimate remains close to zero and statistically insignificant. The result does not show evidence that the main per-unit model is hiding an effect that only appears when affordable housing is measured relative to the size of the surrounding housing market.

Figure 10: Distance and Scale Results

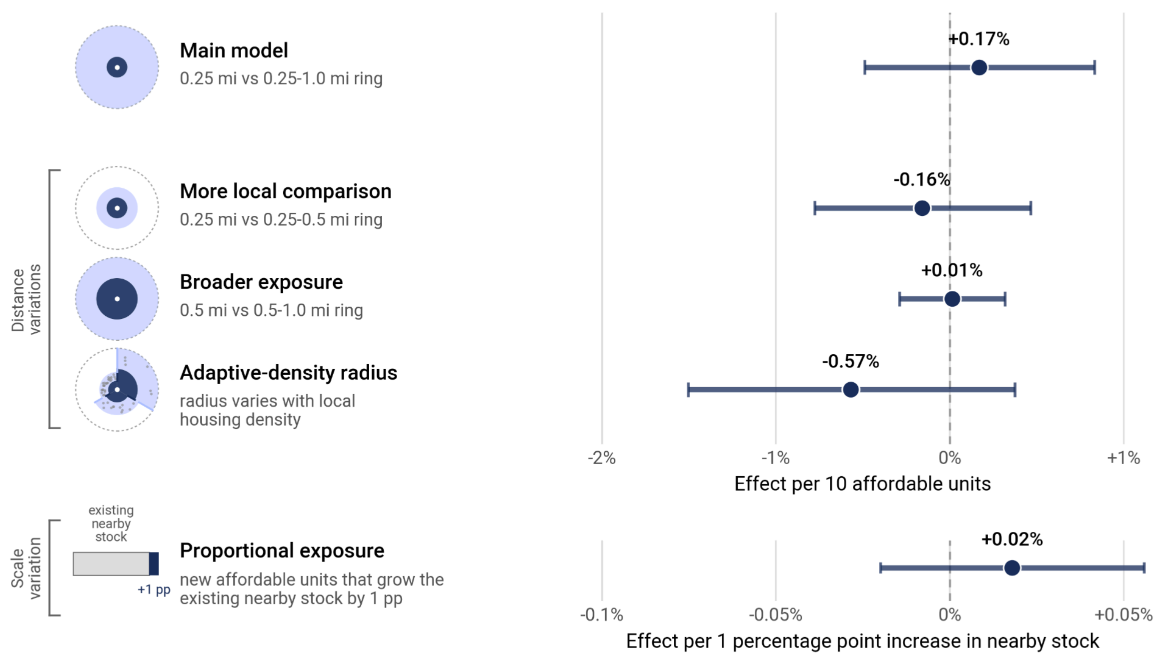
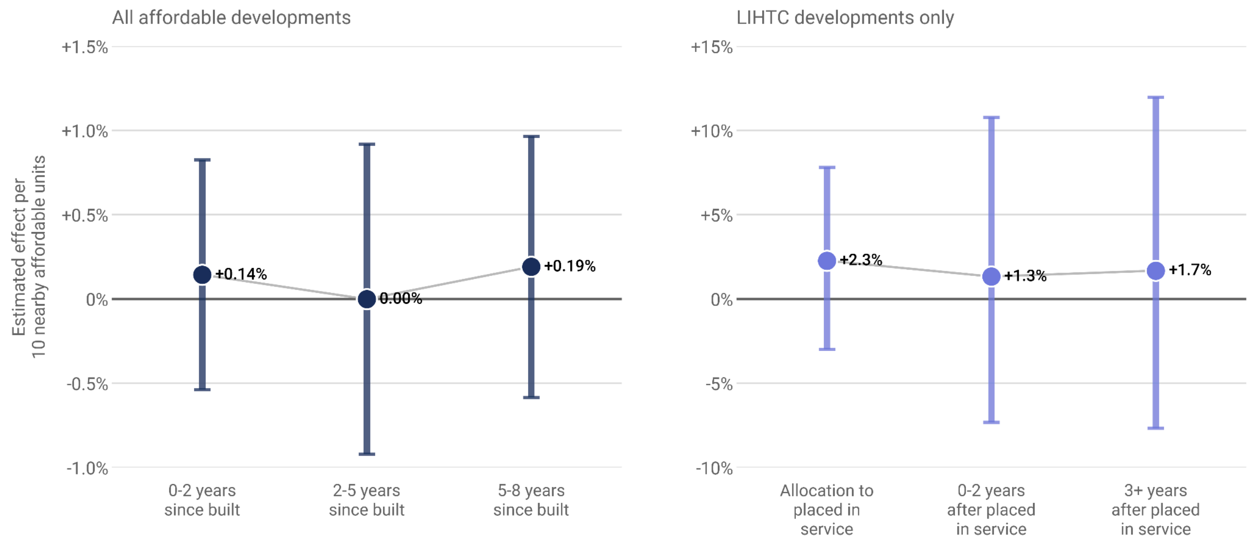


Figure 11: Timing Results



Timing matters for a different reason: nearby buyers and sellers may respond before units are completed, or effects may appear only after a development has been occupied for some time. The main model treats exposure as beginning when affordable units are completed. To test whether the post-completion pattern changes over time, the analysis splits the years after completion into separate windows. The estimates are +0.14% in the first two years after completion, approximately 0% three to five years after completion, and +0.19% more than five years after completion. A joint test does not reject equality across those periods. A separate analysis uses the LIHTC subset to distinguish between allocation, when a project becomes a known future development, and placed-in-service, when the units are completed and available for occupancy. Those estimates likewise do not show a separate anticipation effect before completion or a distinct occupancy effect after units are placed in service. In practical terms, the timing results do not show an opening-period effect, a delayed decline, or evidence that the completion date is masking an earlier response to the project becoming known.

Altogether, these tests do not show that the main finding is an artifact of how timing, distance, or the intensity of exposure to a new affordable housing development is defined.

The evidence is not equally precise for every alternative, especially for more granular distance patterns, so the results should not be read as proving that geography, scale, or timing never matter. Rather, the tests do not reveal a clear, statistically reliable price effect that is hidden by the main 0.25-mile, per-unit, completion-based model.

Do the results differ by place or property type?

The literature review suggests that nearby price effects, where they appear, are unlikely to be uniform across all neighborhoods and development types (Ellen 2007; Diamond and McQuade 2019; Nguyen 2005). Prior studies have found that effects depend on local market conditions, baseline neighborhood income, project scale, and the type of housing being built. This motivates a final question: even if the statewide average is near zero, are there specific places or project types where affordable housing is associated with different nearby price effects?

Because this analysis divides one statewide design into sixteen subgroups, its estimates depend more heavily on identification and robustness diagnostics than the statewide result does. Figure 12 reports each estimate together with the diagnostics needed to interpret it. The analyses are exploratory: the study's confirmatory conclusion rests on the statewide average estimate. The subgroup p-values reported in Figure 12 are not adjusted for the number of tests performed; when sixteen estimates are each tested at the conventional 5 percent significance level, at least one is likely to appear significant by chance alone even if no true differences exist. Applying standard corrections across the sixteen arms leaves no subgroup estimate significant at the 5 percent level under family-wise control, and the small-development estimate is the only one that remains notable under a 10 percent false discovery rate criterion. Reporting both treatment rings serves as a further stability check: patterns that reflect genuine nearby price effects should generally point in the same direction across distances, while estimates that change sign between rings with wide confidence intervals are more consistent with sampling noise around a true effect of zero.

The last two columns of Figure 12 address a subtler risk. The parallel trends check asks whether prices near a subgroup's future sites were moving differently from comparison prices before construction. It is the standard check for a difference-in-differences design, but it detects only a gap that changes; it is blind to a gap that already exists and is stable. The pre-construction price gap diagnostic tests for exactly that standing gap. For each arm, it compares pre-completion sales of homes within the treatment radius of a site where a development in that arm would later be built against sales of homes in the same model sample that are never within the treatment radius of any development in that arm, using the same controls in the main model: census-tract-by-year conditions, property and transaction characteristics, and exposure to the other arms. It therefore measures whether homes near future sites were already priced differently in ways the model cannot address. This matters because exposure in every specification is the number of completed affordable units nearby, zero before completion and positive afterward, and the model has no separate term for simply being near a not-yet-built site. A persistent, pre-existing price gap has nowhere to load except the exposure coefficient once the development opens, and the parallel trends check will not catch it. An arm whose pre-construction gap is comparable in size to its estimate may therefore be measuring where developments were sited rather than what happened to prices when they opened.

Figure 12: Heterogeneity Results

Category	**Subgroup**	**Treated sales** (0.25mi / 0.5mi)	**Estimate** (0.25 mi) [95% CI]	**Estimate** (0.5 mi) [95% CI]	**Joint Wald p** (0.25 mi / 0.5 mi)	**Pre-construction price gap** (p-value)	**Parallel trends check**
Income tercile	Low	4,181 / 10,871	+0.64 [-0.18, +1.45]	+0.08 [-0.33, +0.49]	0.112 / 0.640	-1.7% (0.431)	Fail
Income tercile	Middle	1,801 / 5,055	+0.33 [-0.70, +1.36]	+0.09 [-0.41, +0.59]		+2.5% (0.496)	Pass
Income tercile	High	1,129 / 2,996	-0.46 [-1.44, +0.51]	-0.21 [-0.79, +0.37]		+3.6% (0.302)	Fail
Property type	General occupancy	4,573 / 13,238	+0.00 [-0.61, +0.61]	-0.10 [-0.45, +0.24]	0.281 / 0.238	+0.1% (0.981)	Pass
Property type	Age-restricted	3,196 / 8,228	+0.79 [-0.72, +2.28]	+0.45 [-0.36, +1.26]		+0.4% (0.867)	Fail
Construction	New construction	6,519 / 16,584	+0.15 [-0.53, +0.84]	+0.02 [-0.29, +0.34]	0.259 / 0.143	+1.3% (0.529)	Pass
Construction	Rehabilitation	3,083 / 9,035	-0.35 [-0.91, +0.22]	-0.32* [-0.66, +0.03]		-2.7% (0.233)	Pass
Development size	Small	2,333 / 5,947	-2.29 [-5.32, +0.69]	-2.21*** [-3.87, -0.59]	0.275 / 0.023	-5.2% (0.200)	Pass
Development size	Medium	4,197 / 11,319	+0.46 [-0.94, +1.85]	+0.32 [-0.26, +0.90]		+0.7% (0.694)	Pass
Development size	Large	1,218 / 3,923	+0.19 [-0.39, +0.76]	-0.06 [-0.45, +0.33]		+2.6% (0.438)	Pass
Affordability	Fully affordable	6,641 / 17,675	+0.17 [-0.54, +0.88]	+0.07 [-0.30, +0.44]	0.967 / 0.451	+0.8% (0.755)	Pass
Affordability	Mixed-income	783 / 2,136	+0.15 [-0.90, +1.20]	-0.17 [-0.66, +0.31]		-0.2% (0.937)	Pass
Urban / rural	Urban	4,301 / 11,894	+0.04 [-0.70, +0.77]	+0.11 [-0.19, +0.42]	0.763 / 0.599	+1.9% (0.485)	Pass
Urban / rural	Suburban	971 / 2,902	+0.58 [-1.45, +2.62]	-0.33 [-1.53, +0.87]		-0.8% (0.875)	Pass
Urban / rural	Rural	1,839 / 4,126	+0.65 [-1.17, +2.47]	-0.52 [-2.09, +1.05]		-3.2% (0.283)	Pass

Notes. All results are from the single interacted sale-level per-unit model with Conley inference; estimates are percent price effects per 10 nearby affordable units. These analyses are exploratory and p-values are unadjusted for the sixteen comparisons made. The joint Wald p, shown once per dimension, tests equality across subgroups. The parallel trends check tests whether pre-completion trends differ, not whether nearby and comparison areas start from the same price level; it passes when the subgroup's pre-period test has p >= 0.10. The pre-construction price gap tests for that starting difference: the adjusted price difference between homes near an arm's future sites and homes in the same sample never near that arm's sites, over pre-completion sales. No estimate should be read in isolation from the diagnostic columns: an arm whose pre-construction gap is comparable in size to its estimate may reflect where developments were sited rather *than a price effect of completion, and definitive interpretation requires the full diagnostic record in Appendix B5. The small-development arm is the clearest case. Property type excludes supportive housing due to small sample size. * p < 0.10, ** p < 0.05, *** p < 0.01.*

Read against those diagnostics, most of the table is unremarkable. Aside from the small-development arm, every estimate at both rings is within one percentage point of zero, none of the joint tests rejects equality at the quarter-mile ring, and no category shows the same significant difference at both rings. The diagnostics are also mostly quiet: no pre-construction gap at the half-mile ring is statistically distinguishable from zero, and most are within roughly four percent of zero with no consistent direction.

The small-development arm is the exception, and it is the clearest illustration of why the diagnostics matter. Its estimate is the only notable one in the table: -2.21 percent per 10 nearby units at the half-mile ring (95 percent confidence interval -3.9 to -0.6, p = 0.008), similar in size but insignificant at the quarter-mile ring (-2.29, p = 0.13). Taken alone, that would read as evidence that the smallest developments (23 projects totaling 368 units) lower nearby prices, however the diagnostics indicate otherwise. Homes near future small-development sites were already selling about 5 percent below comparable homes before construction began, the largest pre-construction gap in the table. The gap itself is not statistically significant (p = 0.20), but the diagnostic screens on magnitude rather than significance: these level contrasts carry wide standard errors, and a gap more than twice the size of the coefficient is contaminating whether or not it can be distinguished from zero, because no term in the model can absorb it. The small-arm event study is as negative before completion as after, and the direct difference-in-differences test at completion, the change in the near-versus-comparison gap when the developments actually opened, is -0.5 percentage points with a 95 percent confidence interval of -2.7 to +1.8 (p = 0.69). The repeat-sales specification, which compares each property to itself, finds +0.4 percent (p = 0.66). Assigning the developments random completion years reproduces the negative estimate (every one of 500 draws is negative), while relocating them to random sites collapses it to -0.15 percent; an estimate that survives random timing but not random location is measuring a feature of the locations, not an effect of the developments built there. The estimate is also sensitive to where the size cutoff is drawn and does not survive correction for the sixteen comparisons. Preliminary evidence points to the character of the sites themselves: the negative pattern is concentrated among the small developments located near commercial and industrial land uses, where nearby homes were already selling about 7 percent below their tract average before construction, while small developments embedded in residential surroundings show no measurable pattern at any horizon. With roughly a dozen developments on each side of that split, this is suggestive rather than conclusive, but it is consistent with the siting reading: the discount attaches to locations near non-residential uses, not to the housing built on them. Appendix B5 reports the full record.

The income pattern is suggestive but not definitive. Estimated effects become less positive as neighborhood income rises: +0.64% per 10 units in lower-income tracts, +0.33% in middle-income tracts, and -0.46% in higher-income tracts. This ordering is consistent with prior research finding that affordable housing can have more positive spillovers in lower-income neighborhoods, where new investment may replace weaker site conditions or signal reinvestment. However, the evidence here is not strong enough to establish that pattern in New Hampshire. The contrast specifically between low-income and high-income neighborhoods is marginal under some inference methods, but the joint test across all three income groups does not reject equality at conventional levels. In addition, the lowest- and highest-income arms show weaker support from the pre-trend diagnostics. The right interpretation is not that income differences are established, but that the results are directionally consistent with somewhat more positive estimates in lower-income tracts and less positive estimates in higher-income tracts. None of the income groups shows reliable evidence of a negative property value effect.

Other characteristics do not show reliable differences. Rehabilitation carries the only other consistently negative point estimate (-0.35 percent at the quarter-mile ring and a marginal -0.32 at the half-mile ring, p = 0.07), and it earns the same diagnostic reading as the small arm even though the two groups share no projects (the size categories are defined within new construction). The joint test does not distinguish rehabilitation from new construction at either ring, the estimate does not survive correction for the sixteen comparisons, and homes near future rehabilitation sites were already priced modestly below comparisons before the projects began (-2.7 percent), which is unsurprising for projects that by definition renovate older existing buildings rather than build on chosen sites. Fully affordable and mixed-income developments are similar, general-occupancy and age-restricted developments are statistically similar, and the urban-suburban-rural comparison is flat.

The practical implication is that the statewide result remains the right summary for most places and project types. The subgroup analysis does not identify a broad class of neighborhoods where affordable housing consistently lowers nearby prices.

The results flag project scale, site conditions, and neighborhood income as areas where larger datasets and richer project-level information could be useful.

How do the property value effects from new affordable housing compare to those of new market-rate multifamily developments?

When communities consider a proposal for affordable housing, the realistic alternative is usually not that nothing gets built. More often, the alternative is that a different type of housing, such as market-rate multifamily housing, might be built instead. That distinction matters for interpreting potential effects on nearby property values. If new construction improves a vacant or underused site, any nearby price increase may reflect the effect of reinvestment or new housing generally rather than affordable housing specifically. Conversely, if market-rate multifamily development brings different amenities, design features, or market signals than income-restricted housing, then affordable housing could have a different nearby price relationship even when both add housing units. The comparison in this section asks a narrower but important question: do affordable developments affect nearby sale prices differently from market-rate multifamily developments of similar scale?

Because affordable and market-rate multifamily developments are not always built in the same kinds of places, the comparison is limited to a matched set of developments. Affordable developments are compared with market-rate developments that are similar in local context, completion period, neighborhood income, and project scale. This reduces the risk that the results simply reflect affordable and market-rate projects being built in systematically different neighborhoods. Within that matched set, Figure 13 reports three versions of the same comparison: the estimated effect of affordable multifamily development minus the estimated effect of market-rate multifamily development. A negative estimate would mean that affordable housing is associated with lower nearby prices than market-rate multifamily development of the same scale; a positive estimate would mean the opposite.

Figure 13: Affordable Housing vs. Market-rate Multifamily Comparison Results

Specification	Estimate	Scale	p-value	One-sided 95% lower bound	Parallel trends check
Sale-level per-unit, AH-MR difference	-0.54 pp	per 10 units	0.220	−1.27 pp	Pass
Pair-level per-unit DDD	+0.07 pp	per 10 units	0.715	−0.25 pp	Fail
Repeat-sales per-unit DDD	+0.22 pp	per 10 units	0.428	-0.23 pp	NA

Notes. Each row reports the affordable vs. market-rate comparison from a different design, estimated on matched development samples. All models use a 0.25-mile treatment radius with a 0.25-to-1.0-mile comparison ring and include property and transaction controls. The sale-level AH-MR difference is the difference of the affordable and market-rate exposure coefficients in a joint model with tract-by-year fixed effects and Conley spatial-HAC standard errors, in percentage points per 10 nearby units, on the nearest-neighbor matched sample. The pair-level per-unit DDD is the *difference in per-unit effects from the paired sale-development design on the nearest-neighbor matched sample, with standard errors clustered by sale. The repeat-sales per-unit DDD is the gap in within-home price growth per change in nearby completed units between a property's consecutive sales within 2-7 years, with sale-year fixed effects and standard errors clustered by property; its design does not support a parallel trends check. * p < 0.10, ** p < 0.05, *** p < 0.01.*

Each model summarized in Figure 13 approaches the comparison from different angles. The sale-level model observes each individual transaction and compares nearby affordable and market-rate exposure within the same local market; it passes the parallel trends check and estimates a small, statistically insignificant difference between affordable and market-rate developments. The pair-level triple-difference model restructures the data around individual developments to each sale is paired with a specific affordable or market-rate comparison, creating a more direct before-after, near-far, and affordable vs. market-rate comparison, but it also means a single sale can enter the dataset multiple times if it is near multiple developments. That model fails the parallel-trends check, so it receives less weight. The repeat-sales model follows the same homes between transactions, relying on a much narrower subset of homes with usable repeat sales and dual exposure to affordable and market-rate developments.

Across those three designs, the estimates are small, statistically insignificant, and do not provide reliable evidence of a price penalty specific to affordable housing.

The evidence is not strong enough to prove that affordable and market-rate developments have identical effects in every setting, but the matched comparison does not support the claim that income-restricted multifamily housing lowers nearby sale prices relative to comparable market-rate multifamily development.

Limitations

This study provides strong evidence against the claim that affordable housing systematically reduces nearby home values in New Hampshire, but its findings should be interpreted with appropriate caution. A null result does not prove that every individual development has exactly zero effect in every setting – it means that we do not detect a meaningful, systematic decline in nearby home values associated with affordable housing development in the statewide data analyzed here. The analysis is especially informative for evaluating the larger negative effects often asserted in public debate, and the results do not support those broader claims. The analysis is less suited to resolving whether very small effects, highly localized effects, or effects confined to a narrow subset of project types or neighborhood conditions might exist in isolated cases. Several of our secondary and subgroup analyses are less precise than the primary models and should be read more cautiously. The findings are best understood as evidence against a general property value penalty, not as proof that no affordable development could ever affect nearby prices under any circumstances.

Several more specific boundaries define what the study can establish. The lead causal estimates are built around new-construction and adaptive-reuse rental developments, so they should not be extended to affordable homeownership, which we do not study; rehabilitation and preservation projects enter only secondary analyses, where completion timing is less cleanly identified. Because treatment is dated at project completion, any price reactions that occur earlier, at announcement or permitting, may be understated.

The study also measures the presence and scale of nearby affordable units, but not the many project characteristics that prior research suggests can shape neighborhood outcomes. We did not observe management or operational quality, even though several syntheses tie negative spillovers, where they appear, to distress and management rather than to affordability itself (for example, Nguyen 2005; Center for Housing Policy 2009); a well-run and a poorly-run development enter our data identically. We could not examine the depth or level of affordability, such as whether units serve households at 30 versus 60 percent of area median income, or how income targeting is structured, which could plausibly change outcomes. Mixed-income developments are limited in the New Hampshire inventory (12 of 93 new developments) over our study window, even though mixed-income models are increasingly emphasized in affordable housing practice and are typically more financially feasible under current conditions. Our evidence primarily addresses affordable projects and says little about mixed-income spillovers. We also did not measure design or contextual fit, including building height, architectural design, or public benefits such as open space, and we did not characterize what occupied each site prior to the affordable housing development. Whether a project replaced a vacant lot, an underused or distressed structure, or previously open land is exactly the kind of pre-existing condition that the literature (for example, Ellen 2007) identifies as decisive for net effects, and it lies outside what our design can isolate.

These gaps are not only a matter of unavailable data. Even with rich measures of management, design, affordability depth, and site history, New Hampshire contains only a few hundred affordable developments, and far fewer of any single type, so the underlying population is too small to estimate the separate influence of every such factor, let alone how they interact. A study of this kind fundamentally estimates a marginal, all-else-equal effect, while the outcome for any single development is shaped by its entire context at once: how the building, its residents, its management, its site, and its surrounding neighborhood fit together. Statistically holding each feature constant would isolate a narrower quantity, and in some cases would remove the very mechanisms through which a development affects its surroundings. The net effect we report is therefore both the quantity most relevant to the public debate – what happened to nearby prices when these developments were built in their actual settings – and a reminder that no single coefficient can substitute for careful judgment about a specific project in a specific place.

New Hampshire's lower-density housing markets are a genuine strength of this study, since most prior research comes from dense metros where a single development is a negligible addition to the surrounding stock. But lower-density settings are also inherently harder to analyze: sales are sparser, comparison groups thinner, and individual transactions more idiosyncratic, so estimates in the most rural areas carry more noise. The statewide average we report may not hold uniformly across every suburban and rural location, and our design does not let us say precisely which places depart from it. The result is best read as evidence about the typical New Hampshire development, not a guarantee for any specific site.

Two final caveats concern the comparison itself. The market-rate comparison is strong enough to reject an obvious affordable-specific penalty, but it remains observational and is not a perfect site-by-site counterfactual. And one subgroup result remains unresolved: the smallest developments (23 projects) carry a negative estimate at the half-mile ring, but the diagnostics point to siting rather than effect. Homes near the future sites already sold below comparable homes before construction, the gap does not change when the developments open, and the estimate does not survive correction for the number of subgroup comparisons; still, 23 projects cannot settle the question, and the evidence rules out a lasting decline more confidently than a temporary one. Taken together, these limitations do not undermine the central finding, which remains robust. Instead, they highlight priorities for future work: the roles of management, design, affordability depth, mixed-income structure, and site conditions, including proximity to commercial and industrial uses, in shaping how any individual development fits its neighborhood.

Implications and Future Research

The most direct implication is that the claim at the center of many local debates, that a nearby affordable development will lower surrounding home values, is not supported by statewide New Hampshire evidence spanning more than a decade. That does not settle every question about every project, but it shifts where the burden of proof should sit: an objection framed as general property value harm is not consistent with what actually happened to prices near the affordable developments built across the state.

For planning boards and local officials, the study provides clear guidance: concerns that affordable housing will lower nearby property values are not supported by New Hampshire's experience and should not be used as a reason to deny, delay, or place additional conditions on a proposal. The findings speak to a concern raised at nearly every hearing on new housing. Zoning boards must weigh property value impact as one of the five statutory variance criteria, so the research bears directly on a question they must consider. Planning boards, whose site plan review is limited to the standards in a municipality's adopted regulations, will nevertheless benefit from having an evidence-based answer when the concern is raised. For developers and advocates, the most useful result is the direct comparison with market-rate housing, because it corrects the counterfactual. The realistic alternative to an affordable development on a given parcel is usually not an empty lot but a market-rate building of similar size, and on this evidence the two affect nearby prices about the same. That evidence-based framing can be more persuasive in a hearing than general reassurance. The result also locates where proponents actually have leverage: because design, scale, management, and site fit are what the wider research ties to neighborhood outcomes, attending to those deliberately, and being able to describe them concretely, does more to build local confidence in a new project than arguing about property values in the abstract.

For residents and neighbors, the finding speaks to what is often a household's largest financial asset: across New Hampshire, homes near new affordable developments did not lose value relative to comparable homes nearby, a reassurance grounded in local sales rather than assertion. That is not a claim that every concern about a given project is unfounded. It is a redirection: neighbors who want a say will accomplish more by engaging early on the specific and changeable features of a proposal, how it is designed, how it will be managed, and how the buildings relate to the neighborhood, than by opposing it on property value grounds the evidence does not bear out.

Research from larger cities is useful context, but it should not be treated as a substitute for New Hampshire evidence. The best-known studies from places such as New York City and Los Angeles generally do not find broad declines in property values from affordable housing; their results are more often null, positive, or highly localized, with negative effects tied to specific settings such as weaker market conditions, distressed properties, or specific neighborhood contexts. Those studies matter because they show that the "affordable housing lowers property values" claim is not strongly supported even in the literature most often cited in these debates. But dense urban markets also differ from New Hampshire, where a single development can represent a larger change in the surrounding housing stock. The appropriate answer is to read the broader literature alongside New Hampshire data; both point away from a general loss in property values from affordable housing developments.

The most valuable next step is to expand the evidence base beyond a single state. New Hampshire's main limitation is sample size: the number of new developments is meaningful but still small for detecting differences by project type, location, density, or design. A harmonized New England panel linking deed and assessment records, state housing-finance data, and consistent geocoding across New Hampshire, Vermont, Maine, Massachusetts, Rhode Island, and Connecticut would make it possible to test whether these findings hold across a broader range of markets, from Greater Boston to rural northern New England. That kind of regional dataset would be the highest-leverage way to move from a statewide average finding to more precise evidence about where, when, and why effects vary.

Future research would also benefit from collecting project-level information that cannot be reconstructed reliably after the fact. Design, massing, setbacks, open space, prior site use, management structure, operator type, income targeting, and unit mix are the characteristics most likely to explain why one development fits smoothly into a neighborhood and another faces concern. Standardizing a small set of these fields at the point of funding or permitting would make future analysis more actionable. The same research infrastructure could also support related questions that this study could not answer, including effects on rents and rental availability, assessed values and the municipal tax base, nearby permitting and construction, neighborhood outcomes such as crime or school enrollment, and specialized housing models such as affordable homeownership, mixed-income housing, and supportive housing.

Conclusion

The most rigorous evidence in the existing literature to support the claim that affordable housing does not impact local property values came from denser cities outside of New England. This study's goal was to test that hypothesis with evidence from New Hampshire using a rigorous and transparent analysis of how affordable development relates to the value of nearby homes across the range of places where the state actually builds.

The study examines the relationship across urban, suburban, and rural settings; at varying distances from a development; before, during, and after construction; by the type and scale of project; and against a market-rate benchmark. The primary statewide estimate is near zero and stable across the main distance, timing, and benchmark specifications. Most subgroup estimates are also near zero, but the small-development result remains uncertain. Affordable development shows no measurable effect on the value of nearby homes, whether in a city neighborhood or a rural town.

For local officials and policymakers, that means this longstanding question can now be informed by evidence from New Hampshire itself rather than by assumptions or findings from other housing markets.

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Appendix A. Detailed Data Processing

Figure A1: Data Sources

Dataset	Coverage	Role in analysis
The Warren Group deed records	Residential transactions, 2008–2025	Primary source for sale prices, transaction dates, and the core residential sales universe
PrimeMLS sales listings	Residential listings/sales, 2008–2025	Provides property characteristics and supports transaction classification and gap-filling for hedonic controls
NHPD affordable housing records	Federally assisted affordable developments completed 2010–2023	Primary source for federally assisted developments in the affordable development universe, with project characteristics and completion timing
NH Housing affordable housing records	State-financed affordable developments completed 2010–2023	Primary source for state-financed developments in the affordable development universe, with project characteristics and completion timing
Federal LIHTC database	LIHTC developments placed in service 2010–2023	Used to enrich, validate, and reconcile the affordable development file
CoStar multifamily records	Market-rate multifamily developments completed 2010–2023	Primary source for market-rate multifamily properties in the comparison group, with project characteristics and completion timing
ACS 5-year tract data	Census tracts, 2010–2023 vintages (year-matched)	Provides neighborhood income context and tract-level covariates
USDA RUCA codes	Census tracts, 2010 vintage	Classifies locations as urban, suburban, or rural for subgroup analysis

Notes. All sources cover New Hampshire.

This appendix documents the processing steps that most directly affect the analytic universe and the measurement of treatment exposure. After duplicate reconciliation across source systems, the statewide affordable-housing universe contains 668 canonical projects across all years. Restricting that universe to physical development, redevelopment, or rehabilitation events in the 2010-2023 study period yields 181 candidate project events. Review of those in-window cases removes records that are duplicate or alias entries, recapitalization-only transactions, non-comparable non-permanent housing, or scattered-site and placeholder records that cannot be confirmed as distinct physical developments. The resulting analytic development set contains 152 confirmed affordable developments in the study window. Of these, 150 can be resolved to final parcel footprints and therefore enter the parcel-based sale-matching panel, totaling 5,560 units. The lead treatment sample is the parcel-resolved subset of 93 new-construction or adaptive-reuse developments totaling 3,124 units; the remaining 57 developments and 2,436 units are rehabilitation or preservation projects used in descriptive and secondary analyses.

The market-rate comparison group is used only in the affordable-versus-market-rate analyses, not in the headline of affordable-housing specification. After restricting the CoStar extract to comparable New Hampshire multifamily developments completed between 2010 and 2023 and removing subsidized, noncomparable, or duplicate records, the point-geometry comparison inventory contains 137 market-rate developments. After parcel-based reconciliation and duplicate cleanup, the analytic market-rate file contains 136 parcel-resolved developments and 11,334 units.

Parcel footprints are built from the New Hampshire GRANIT statewide parcel mosaic. Developments were linked to parcels using three forms of evidence: geocoded latitude and longitude, parcel-address matching, and manual web search review where the automated evidence remained ambiguous. When a development occupied multiple contiguous or functionally related parcels, those parcels were grouped into a single project footprint. Some parcel boundaries did not cleanly identify the project footprint - either extending beyond or containing within-parcel cut-outs; in either case, project location accuracy was prioritized over precise development area coverage. The final spatial treatment measure is based on the physical parcel footprint of the development rather than on a single geocoded point, though point-based distances were also computed to confirm that this decision does not substantially alter any of the results.

The cleaned and geocoded statewide sales universe contains 484,230 residential transactions. The regression samples are defined by restricting to arm's-length single-family, condominium, and mobile or manufactured home sales; excluding low-precision geocodes and whole-building or lot transfers; trimming the upper tail of annual sale prices; resolving same-day duplicate records; and harmonizing the hedonic controls used in the models. The main analysis retains three geocode precision classes, rooftop, range_interpolation, and nearest_rooftop_match. A rooftop-only robustness sample of 54,302 sales yields the same substantive conclusion as the full kept sample, indicating that geocoding precision is not driving the headline result.

Appendix B. Full Results Reporting

This appendix reports the full set of estimates behind the results in the main text. Identification in every model rests on the same logic: comparing price changes for more-exposed and less-exposed homes within the same census tract and year, so the key assumption is that, absent nearby development, the two groups would have followed similar price paths. Each table notes its inference method; unless stated otherwise, estimates use the headline geometry (a 0.25-mile treatment ring with a 0.25-to-1.0-mile comparison ring) with Conley spatial standard errors, which allow unexplained price movements to be correlated across nearby transactions.

Figure B1: Power Analysis

Reports the statistical precision of the main and benchmark designs, including the one-sided 95% lower bound and the minimum detectable effect.

Panel A. Main and benchmark designs

Design	N	Scale	Standard error	MDE (80% power)
Lead sale-level per-unit, AH only	68,488	per 10 units	0.34%	0.95%

Companion binary (any AH nearby)	68,488	any nearby	1.57%	4.51%
Paired sale-development benchmark	121,297	any nearby	2.53%	7.35%

Panel B. Affordable minus market-rate difference (joint model)

Design	N	Scale	Standard error	MDE (80% power)
Sale-level contrast, full sample	119,792	pp per 10 units	0.36 pp	1.00 pp
Sale-level contrast, matched common support	117,922	pp per 10 units	0.36 pp	1.00 pp
Sale-level contrast, matched selected	94,054	pp per 10 units	0.44 pp	1.24 pp

Panel C. Distance and intensity

Design	N	Scale	Standard error	MDE (80% power)
Local control 0.25 / 0.5 mi	30,132	per 10 units	0.32%	0.89%
Broader 0.5 / 1.0 mi	68,488	per 10 units	0.15%	0.43%
Continuous distance decay	68,488	per 10 units	6.00%	18.31%
Adaptive-density radius	68,488	per 10 units	0.48%	1.36%
Proportional intensity	68,488	per 1 pp of stock	0.02%	0.05%

Panel D. Timing

Design	N	Scale	Standard error	MDE (80% power)
0-2 years after completion	2,012	per 10 units	0.35%	0.98%
3-5 years after completion	1,780	per 10 units	0.47%	1.32%
More than 5 years after completion	3,319	per 10 units	0.40%	1.11%
LIHTC: allocation to placed-in-service	55,062	per 10 units	2.70%	7.84%
LIHTC: 0-2 yr after placed-in-service	55,062	per 10 units	4.55%	13.60%
LIHTC: 3+ yr after placed-in-service	55,062	per 10 units	4.92%	14.78%

Panel E. Heterogeneity subgroups (interacted models)

Design	N	Standard error (0.25 mi)	MDE (0.25 mi, 80% power)	Standard error (0.5 mi)	MDE (0.5 mi, 80% power)
Income tercile: low	68,488	0.42%	1.17%	0.21%	0.58%
Income tercile: middle	68,488	0.53%	1.47%	0.26%	0.72%
Income tercile: high	68,488	0.50%	1.40%	0.30%	0.83%
Property type: general occupancy	68,488	0.31%	0.88%	0.18%	0.49%
Property type: age-restricted	68,488	0.77%	2.14%	0.41%	1.16%
Construction: new construction	68,488	0.35%	0.98%	0.16%	0.44%
Construction: rehabilitation	68,488	0.29%	0.81%	0.18%	0.49%
Development size: small	68,488	1.53%	4.30%	0.84%	2.34%
Development size: medium	68,488	0.71%	1.99%	0.30%	0.83%
Development size: large	68,488	0.29%	0.83%	0.20%	0.56%
Affordability: fully affordable	68,488	0.36%	1.01%	0.19%	0.53%
Affordability: mixed-income	68,488	0.53%	1.50%	0.25%	0.70%

Urban / rural: urban	68,488	0.38%	1.05%	0.15%	0.43%
Urban / rural: suburban	68,488	1.04%	2.90%	0.61%	1.71%
Urban / rural: rural	68,488	0.93%	2.59%	0.80%	2.24%

*Notes. All models use Conley spatial-HAC inference and are reported as the effect per 10 units. The minimum detectable effect (MDE) is the smallest true effect each design would detect with 80% power at the 5% two-sided significance level, computed from the estimated standard error as MDE = 2.8 x se on the displayed scale. The one-sided 95% lower bound is the most negative effect consistent with the data at the 95% confidence level. All lower bounds sit well inside the 5% to 10% declines often raised in public debate, which the design rules out. * p < 0.10, ** p < 0.05, *** p < 0.01.*

Figure B2: Event Study Results

Reports whether treated and comparison sales followed similar price trends before development completion.

Specification	Pre-trend Wald p	Verdict
Lead per-unit event study	0.324	Pass
Companion binary event study	0.680	Pass
Stacked staggered DiD event study	0.188	Pass
LIHTC allocation-anchored event study	0.651	Pass
AH vs MR joint event study, full sample	0.396	Pass
AH vs MR joint event study, matched common support	0.447	Pass
AH vs MR joint event study, matched selected	0.369	Pass
Pair-level DD event study	0.244	Pass
Pair-level per-unit DDD, full sample	0.013**	Fail
Pair-level per-unit DDD, matched	0.006***	Fail
Matched binary DDD event study	0.468	Pass

*Notes. Joint Wald test that the pre-period event-study coefficients are jointly zero over the five years before completion; rejection (p < 0.10, marked Fail) means treated and comparison sales were already on different price paths, so that design receives less weight. Inference is Conley spatial-HAC for sale-level rows, cluster bootstrap for pair-level rows, and sale-level clustering for the staggered and allocation-anchored rows. * p < 0.10, ** p < 0.05, *** p < 0.01.*

Figure B3: Full Results for Primary and Benchmark Models

Reports the main sale-level estimates and benchmark specifications for the main affordable housing analysis.

Design	N	Estimate	Scale	p-value	One-sided 95% lower bound
Lead sale-level per-unit, AH only	68,488	+0.17%	per 10 units	0.617	-0.38%
Companion binary (any AH within 0.25 mi)	68,488	+0.37%	any nearby	0.814	-2.19%
Hybrid: first-arrival term	68,488	-0.95%	any nearby	0.635	-4.15%
Hybrid: additional units	68,488	+0.32%	per 10 units	0.487	-0.44%
Pair-level DD benchmark	121,297	+0.66%	any nearby	0.796	-3.45%

Notes. Sale-level difference-in-differences at the headline geometry (0.25 / 1.0 mi) with tract-by-year fixed effects and Conley spatial-HAC inference. "Per 10 units" rows scale to 10 nearby completed affordable units. The two hybrid rows come from a single regression that separates the first nearby completion from additional units beyond it.

Figure B4: Distance, Intensity, and Timing Results

Reports alternative definitions of nearby exposure, proportional exposure, post-completion timing, and LIHTC milestone timing.

Panel A. Distance

Specification	N	Estimate	Scale	p-value
Headline 0.25 / 1.0 mi	68,488	+0.17%	per 10 units	0.617
Local control 0.25 / 0.5 mi	30,132	-0.16%	per 10 units	0.617
Broader 0.5 / 1.0 mi	68,488	+0.01%	per 10 units	0.928
Continuous distance decay	68,488	-2.58%	per 10 units	0.663
Adaptive-density radius	68,488	-0.57%	per 10 units	0.236

Panel B. Intensity

Specification	N	Estimate	Scale	p-value
Proportional intensity	68,488	+0.02%	per 1 pp of stock	0.353

Panel C. Time since first nearby completion

Specification	N	Estimate	Scale	p-value
0-2 years after completion	2,012	+0.14%	per 10 units	0.680
3-5 years after completion	1,780	-0.00%	per 10 units	0.998
More than 5 years after completion	3,319	+0.19%	per 10 units	0.630

Panel D. LIHTC subset: allocation vs. placed-in-service phases

Specification	N	Estimate	Scale	p-value
Allocation to placed-in-service	55,062	+2.27%	per 10 units	0.405
0-2 yr after placed-in-service	55,062	+1.33%	per 10 units	0.772
3+ yr after placed-in-service	55,062	+1.68%	per 10 units	0.735

Notes. Sale-level per-unit difference-in-differences models with Conley spatial-HAC inference unless otherwise stated. Panel A varies the distance definition; Panel B measures exposure as the affordable share of nearby housing stock; Panel C splits effects by years since completion; Panel D uses LIHTC allocation and placed-in-service dates to distinguish anticipation from post-completion phases. Joint tests do not reject equality across the Panel C timing bins or the Panel D LIHTC phases. Parallel-trends tests are reported in Figure B2.

Figure B5: Small-development Diagnostics

Panel A: The small-development estimate (interacted subgroup model)

Quantity	0.25 mi	0.5 mi
Estimate, per 10 nearby units	-2.29%	-2.21% ***
95% confidence interval	[-5.32, +0.69]	[-3.87, -0.59]
Two-sided p-value	0.131	0.008

Minimum detectable effect (80% power)	4.3%	2.34%
Treated sales	2,333	5,947

*Notes. The small arm holds 23 developments totaling 368 units. Panel A restates the small-arm coefficient from the main-text heterogeneity table, from the single interacted sale-level per-unit model with tract-by-year fixed effects, property and transaction controls, and Conley spatial-HAC inference; the minimum detectable effect is 2.8 x SE (80 percent power, 5 percent two-sided). * p < 0.10, ** p < 0.05, *** p < 0.01.*

Panel B: Price levels near small-development sites

Quantity	0.25 mi	0.5 mi
Pre-construction gap (SE)	-2.29%	-2.21% ***
Two-sided p-value	[-5.32, +0.69]	[-3.87, -0.59]
Post-completion gap (SE)	0.131	0.008
Two-sided p-value	4.3%	2.34%
Difference-in-difference at completion (development-sale pair model)		
DD estimate (pp), all post years vs. all pre years	-	-0.46
Two-sided p-value	-	0.687
DD estimate (pp), years 0-2 after vs. years 1-2 before	-	+0.29
Two-sided p-value	-	0.816

Notes: Panel B shows the adjusted price gaps between homes within the stated distance of a future or completed small-development site and comparable homes in the same model sample that are never within that distance of a small-development site, computed after removing tract-by-year fixed effects, property and transaction controls, and exposure to medium and large developments. The exposure coefficient in a model with no near-future-site term can absorb a standing gap of this size. The difference-in-differences rows are the standard DiD test, estimated on development-sale pairs: each compares the adjusted near-versus-comparison gap after completion with the same gap before completion, so a causal completion effect would move these contrasts away from zero. The all-post row averages every post-completion year against every pre-completion year; the narrow-window row compares the two years just after completion with the two years just before it, the sharpest test of a price change at the moment the developments opened.

Panel C: Small-arm event study (0.5 mi, per 10 units)

Event time	Estimate	95% CI	p	Treated sales
6+ years before completion	-2.44%	[-5.8, +1.0]	0.166	1,653
5-3 years before	-2.05%	[-5.0, +1.0]	0.181	1,460
2-1 years before	-3.23% *	[-6.8, +0.5]	0.085	1,160
0-2 years after	-2.95% **	[-5.2, -0.7]	0.011	1,745
3-5 years after	-3.09% ***	[-4.9, -1.3]	0.001	1,699
6+ years after	-1.02%	[-2.7, +0.7]	0.250	3,037

*Notes. Event study extending the specification with pre-completion indicators; each coefficient is relative to sales with no small-development exposure in the same tract and year. The pre-completion coefficients are as negative as the post-completion ones, so the price gap predates construction rather than opening at completion. * p < 0.10, ** p < 0.05, *** p < 0.01.*

Panel D: Placebo distributions (0.5 mi)

Quantity	Random completion year	Random location

Observed estimate	-2.07%	-2.07%
Placebo-draw mean	-1.86%	-0.15%
Placebo-draw median	-1.85%	-0.18%
5th-95th percentile of draws	[-2.85, -0.84]	[-2.80, +2.82]
Share of draws negative	100%	53%
Share of draws more extreme than observed	0.39	0.24
Draws	500	500

Notes. Summary of 500-draw placebo distributions. Assigning random completion years reproduces the negative estimate: all 500 random-timing draws are negative, and the observed estimate falls at the 39th percentile of the random-timing placebo distribution, well inside it. Relocating the developments to random sites instead collapses the estimate. An estimate that survives random timing but not random location measures a feature of the locations, not an effect of the developments. The observed estimate is the placebo harness's re-estimate of the small-development coefficient (Panel A).

Panel E: Inference on the 0.5 mi estimate

Method	Clusters	SE	p
Conley spatial HAC, 0.5-mile kernel	-	0.85%	0.009
Conley spatial HAC, 1.0-mile kernel	-	0.84%	0.008
Conley spatial HAC, 2.0-mile kernel	-	0.82%	0.007
Conley spatial HAC, 3.0-mile kernel	-	0.80%	0.006
Cluster by development	87	0.79%	0.005
Cluster by census tract	153	0.97%	0.022
Cluster by town	246	0.53%	< 0.0001
Cluster by county	11	0.46%	< 0.0001
Randomization inference (size labels permuted)	-	-	< 0.0005

Notes. Re-computed inference for the fixed 0.5-mile point estimate; randomization inference permutes the size labels across developments (2,000 draws) and is exact in finite samples, so it does not rely on the number of clusters and directly addresses the small number of developments in this arm. Leave-one-out re-estimation across 57 perturbations (each development, town, or county dropped in turn, plus the highest-sales developments) keeps the estimate between -2.52% and -1.76% (largest p 0.079).

Figure B6: Full Results for Affordable vs. Market-Rate Comparison Models

Reports results for the analysis comparing affordable and market-rate effects across sale-level, paired, and repeat-sales comparison design, including results from both full sample and matched samples.

Specification	Estimate	Scale	p-value	One-sided 95% lower bound	Parallel trends check
Sale-level contrast, full sample	-0.15 pp	pp per 10 units	0.666	-0.74 pp	Pass
Sale-level contrast, matched common support	-0.15 pp	pp per 10 units	0.677	-0.74 pp	Pass
Sale-level contrast, matched selected	-0.54 pp	pp per 10 units	0.220	-1.27 pp	Pass
Pair-level per-unit DDD, full sample	+0.28 pp**	pp per 10 units	0.038	+0.06 pp	Fail

Pair-level per-unit DDD, matched	+0.07 pp	pp per 10 units	0.715	-0.25 pp	Fail
Pair-level binary DDD, matched common support	-3.73%	any nearby	0.183	-8.15%	Pass
Repeat-sales DDD, matched	+2.34%	any nearby	0.247	-0.97%	NA
Repeat-sales per-unit DDD, matched	+0.22 pp	pp per 10 units	0.428	-0.23 pp	NA

*Notes. All models use the 0.25 / 1.0 mile geometry with property and transaction controls, estimated on full and matched development samples. Matching pairs each affordable development with market-rate developments completed in the same era and urbanization class, choosing the two nearest on tract income and project size: "matched common support" keeps every development in the era-by-urbanization groups, where both types are present, while "matched selected" keeps only each affordable development and its two selected market-rate matches. "pp per 10 units" rows report the affordable-minus-market-rate gap in percentage points per 10 nearby units; other rows are percent price effects. Sale-level rows use Conley spatial-HAC inference; pair-level rows cluster by development or sale; repeat-sales rows cluster by property. The parallel trends check reports each design's pre-period test from Figure B2; it does not apply to the repeat-sales design. * p < 0.10, ** p < 0.05, *** p < 0.01.*

Figure B7: Affordable vs. Market-Rate Covariate Balance

Characteristic	Affordable	Market-rate	SMD (raw)	SMD (matched)
Total units (mean)	34	83	-0.72	-0.34
Completion year (mean)	2016.0	2018.2	-0.55	-0.12
Tract median household income ($)	70,188	72,389	-0.09	+0.07
Urban tract (share)	51%	78%	-0.60	-0.08
Suburban tract (share)	26%	13%	+0.32	-0.05
Rural tract (share)	24%	9%	+0.41	+0.15
Pre-period local price trend	+0.001	+0.016	-0.18	-0.16

Notes. New-construction affordable developments (N = 93) versus market-rate multifamily (N = 136), parcel-resolved. SMD is the standardized mean difference; values within +/-0.25 are conventionally considered balanced. The matched column reports the SMD after nearest-neighbor matching within urbanization-by-era groups (65 market-rate developments carry weight). Market-rate multifamily in NH is built larger than affordable housing developments so full scale balance is not attainable; this does not affect the results significantly since they are reported as per-unit effects. The pre-period price trend is each development's local log-price slope over pre-completion sales within 0.25 miles.

Figure B8: Results from Robustness Suite

Reports modeling, data-quality, placebo, and inference checks for the main result.

Panel A. Modeling and data-quality checks

Specification	N	Estimate	Scale	p-value
Conditional trends	68,488	+0.08%	per 10 units	0.806
Stacked, staggered DiD	515,217	-0.54%	per 10 units	0.749
Geocode quality: all kept sales	68,488	+0.17%	per 10 units	0.617
Geocode quality: rooftop only	54,302	-0.02%	per 10 units	0.949
Lead model, repeat-sale homes only	22,613	-0.12%	per 10 units	0.773

Panel B. Placebo tests

Specification	N	Estimate	Scale	p-value
Temporal placebo: completion -5 yr	68,488	+0.18%	per 10 units	0.578

Temporal placebo: completion -3 yr	68,488	+0.13%	per 10 units	0.685
Temporal placebo: completion +3 yr	68,488	+0.12%	per 10 units	0.740
Temporal placebo: completion +5 yr	68,488	+0.03%	per 10 units	0.944
Spatial placebo: random relocation	383 perms	--	permutation test	0.853

Panel C. Inference stress tests

Specification	N	Estimate	Scale	p-value
Wild cluster bootstrap	68,488	+0.17%	per 10 units	0.608
HonestDiD post-average bound, M-bar = 0	--	-0.87% to -0.04%	bound	excludes 0
HonestDiD post-average bound, M-bar = 0.5	--	-1.97% to +1.07%	bound	includes 0

Notes. Headline geometry (0.25 / 1.0 mi) under Conley spatial-HAC inference unless noted; estimates are percent price effects per 10 nearby affordable units. Panel A varies modeling and data-quality choices; Panel B assigns placebo completion dates and placebo locations, where significant estimates would signal a spurious design; Panel C: the wild-cluster row reports the restricted bootstrap p, a finite-sample check on clustered inference. The HonestDiD rows report the post-average effect bound when post-completion trends are allowed to drift by M-bar times the drift observed before completion (Rambachan and Roth 2023). The M-bar = 0 row allows no drift at all and is the conventional confidence interval around the event-study post-completion average of -0.46% (SE 0.21%), a binary-exposure summary that is coarser than the lead per-unit model; it excludes zero marginally. Because the observed pre-period coefficients already drift by up to 0.37% per year, assuming that drift stops exactly at completion is the assumption the method exists to relax; once half the observed pre-drift is permitted (M-bar = 0.5) the bound includes zero, and larger allowances widen it further.