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Working Paper

There Goes the Neighborhood? The Local Impacts of State Policies That Override Municipal Zoning

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There Goes the Neighborhood? The Local Impacts of State Policies that Override Municipal Zoning*

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Abstract

Prior research shows that restrictive zoning regulations are major drivers of rising housing costs and residential segregation in the United States. In response, a growing number of state and local governments are passing laws to allow for denser housing in strictly zoned municipalities, despite entrenched opposition from incumbent residents. This paper examines whether incumbent residents' responses undermine the success of these policies by studying new construction permitted under Massachusetts Chapter 40B; one of the longest-standing and most productive examples of a housing policy that bypasses local zoning laws. Exploiting hyperlocal variation in residents' proximity to new 40B buildings, we find that only a subset of larger 40B developments cause property values to decrease, and that this effect is both highly localized and only emerges in the longer term, many years after these developments are proposed. Focusing on these larger developments that are more likely to elicit resident reactions, we find that only a fraction of incumbent residents move out after their approval and that the magnitude of these migration responses is insufficient to undermine policymakers' desegregation goals. We also do not find evidence that incumbent residents become more politically active against future development, as they are no more likely to vote in local or general elections nor are they more likely to vote for repealing Chapter 40B after 40B developments are proposed near their homes.

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1 Introduction

Housing distributes much more than shelter; it also distributes access to neighborhoods, which in turn shape opportunities for health, social, and economic mobility (Chetty et al., 2014; Chetty and Hendren, 2018a,b; Chyn, 2018; Chyn et al., 2022). Yet housing in the United States has become increasingly expensive and remains highly segregated along economic and racial lines. Other research has shown that this is partly because municipalities with more favorable opportunities employ strict zoning regulations that limit new housing construction and drive up prices (Glaeser and Gyourko, 2002; Wu and Cho, 2007; Glaeser and Ward, 2009; Kok et al., 2014; Gyourko and Krimmel, 2021), making them unaffordable for low- and moderate-income households who are more likely to identify as Black or Hispanic (Resseger, 2022; Trounstein, 2020).

As a result, state and local governments are increasingly introducing laws that override these ‘exclusionary zoning’ practices to allow for denser housing, citing both the need to make housing more affordable region-wide and the need to open opportunities in these strictly zoned municipalities to all income groups.¹ A major concern with these preemptive approaches, however, is the entrenched opposition they draw from the residents of affected municipalities, who claim that these policies will harm their communities by, for example, reducing property values, changing the ‘neighborhood character’, and lowering the quality of local public services such as schools (Monkkonen, 2016; Been et al., 2019).

While a large body of literature has documented the negative consequences on housing market outcomes that come from incumbent residents’ strong opposition to new housing proposals (Ortalo-Magné and Prat, 2014; Marble and Nall, 2021; Mast, 2022), little is known about the ways those residents react in the medium to longer term and the extent to which those reactions undermine the success of these (or future) policy efforts. For instance, if enough wealthy incumbent residents leave in response to the in-migration of lower-income households, this outmigration could undermine the economic desegregation goals held by many of these policies. Similarly, residents may also respond by voting in ways that undercut future housing policy efforts at the state or local level. Measuring

¹For example, between 2018 and 2024, several states and cities—including California, Oregon, Washington, Minneapolis, and Florida (SB 102)—have enacted policies to override or eliminate single-family zoning. Others, like Montana, reformed housing approval processes to minimize opposition to denser development. Other common policies include legalizing accessory dwelling units, upzoning near transit, and removing parking minimums.

these responses is thus essential to evaluating these housing reforms, yet research has historically been limited by two main factors. First, many of these reforms are too recent to evaluate in any capacity beyond permits filed. Second, though a few older zoning override policies exist, they tend to be missing accurate housing records that can be linked to data on individuals' outcomes.

In this paper, we overcome these barriers and provide new evidence on the effects of new, dense housing construction on neighboring properties, outmigration responses, and political engagement among incumbent residents. Specifically, we study incumbent residents' reactions to housing produced under Massachusetts Chapter 40B (henceforth '40B'); a major, longstanding example of a policy that bypasses zoning to increase housing density in high-income municipalities that offer greater opportunities for social mobility. Since it was enacted in 1969, 40B has produced over 53,500 market-rate and 17,500 affordable units, largely in Boston's high-income and strictly zoned suburbs that offer some of the greatest opportunities for health and social mobility in the state (Sportiche et al., 2025). Chapter 40B incentivizes new housing construction by requiring all local governments to maintain at least 10 percent of their housing as affordable and allowing developers to bypass local zoning regulations in municipalities that do not meet this minimum standard if they build affordable housing in the new buildings. Though 40B's ability to bypass zoning laws has drawn strong resident opposition to its projects, the fact that it has successfully produced so many units in Boston's high-income, low-density suburbs over a long period of time make it a model test case from which to examine residents' responses to new construction.

We estimate the effects of new 40B developments using a difference-in-differences design that exploits hyperlocal variation in the proximity of housing units and residents to new 40B developments, similar to that used in previous work (Diamond and McQuade, 2019; Asquith et al., 2023; Blanco and Neri, 2025). To do so, we leverage a novel dataset containing the exact locations of buildings produced under 40B and linking these locations to a variety of granular datasets containing information on residential property transactions, resident address histories, and voting records that allow us to understand the migration and voting behavior of incumbent residents over a twelve-year period. Using these rich, linked data, we compare the change in house prices and resident outcomes within an 'inner' ring surrounding a new 40B development to the change in house prices and resident outcomes within an 'outer' ring of the same fixed width that immediately surrounds this inner ring around the time when the 40B permit is approved. This approach is well-suited to

capturing effects that depend on proximity to new 40B developments, such as changes in neighborhood character, exposure to low-income households, or increased housing supply in the micro-neighborhood around the 40B development. If incumbent residents' reactions are driven by such factors, we should expect greater opposition among those living closest to 40B developments. We therefore begin by testing whether new 40B developments affect nearby density and housing prices as these are among the most common concerns raised by incumbent residents. It is important to note, however, that this design does not capture broader, municipality-wide effects on housing markets and local public goods such as school quality and public safety.

Using the ring method, we first show that the average 40B development leads to a substantial increase in nearby housing density but does not affect local housing prices. Density increases by 50 percent within 0.1 miles and by 16 percent within 0.2 miles from a 40B development. The null effect on housing prices is precisely estimated: we can reject price reductions larger than 0.4 percent and price increases above 2.4 percent within 0.1 miles—compared to sales 0.5 to 0.6 miles away from these developments—, both smaller in magnitude than the 2.5 percent price drop that Diamond and McQuade (2019) estimate within the same distance from mixed-income developments in high-income neighborhoods. However, this overall null effect masks important heterogeneity: smaller 40B developments (fewer than 50 units) increase nearby prices by about 5 percent, while larger developments (50 or more units) reduce prices by roughly 9 percent. These price effects appear about eight years after permit approval and are concentrated within 0.1 miles of the developments. We estimate that property value losses generated by larger 40B developments are roughly equal to 3 percent of the value they generate. Given their greater potential to elicit reactions from residents, we focus on these larger 40B developments in subsequent analyses.

Our findings focused on these larger developments show that only a small fraction of incumbent residents move out in response to the new, denser housing. Residents living in their immediate vicinity—within 0.1 miles—are about 83 percent more likely to move out after their approval. Effects then dissipate with distance: we observe a similar though much smaller effect for residents living within 0.1 to 0.2 miles, and no effects beyond this distance. However, because of the low density of these areas, a back-of-the-envelope calculation suggests that this effect is substantively small: the average larger 40B development only induces 1.6 percent of the long-term, incumbent residents (2.28 out of 140) that we detect living within 0.2 miles to move out within the first twelve

years after the permit is approved. These patterns are consistent with the highly localized property value decreases near larger 40B developments. We observe similar results for move-out rates among the sample of all local residents, that is, including new residents moving into housing within those distances from a 40B after its approval. An examination of treatment effect heterogeneity by development type reveals that effects are stronger for residents living near 40B developments that differ the most from the existing community (i.e., 40B developments that are rental and larger in size) and that the impact is concentrated on residents that we do not identify as homeowners.

In contrast, we do not find any evidence that incumbent residents are more likely to influence future housing construction by becoming more politically active. We show that incumbent residents are not more likely to vote in local elections, where affected residents may be more inclined to take action against the new construction, following the approval of larger 40B developments. However, our ability to precisely estimate these effects in the long run, where negative price effects fully realize, is limited by the shorter period for which we have turnout data for local elections. Nonetheless, we also show that there are no significant effects on voter turnout for general elections (both at the state and federal level), even in the long run. Finally, we examine whether residents become more politically engaged in housing decisions by studying a 2010 ballot measure in Massachusetts that asked voters whether to repeal Chapter 40B altogether. A cross-sectional regression that compares precincts with 40B developments approved before and after 2010 shows that neighborhoods with existing 40B developments—which have directly experienced the effects of the policy—are not more likely to favor repealing Chapter 40B.

Overall, our results suggest that incumbent residents' responses to new, denser construction in high-income, low-density municipalities do not meaningfully undermine at least some of the policy goals of zoning override policies. Changes in the move-out rates of incumbent residents are small relative to the number of incoming 40B residents and to prior estimates of resident responses to much smaller shocks (Bayer et al., 2022). Moreover, migration responses of incumbent homeowners—who have higher incomes than renters—are nonexistent. These facts indicate that the outflow of high-income residents is unlikely to hinder 40B's income mixing goal. Chapter 40B being a statewide policy may explain such limited responses, since close housing substitutes may also be vulnerable to 40B construction through the same policy. The null effects on political participation at any level suggest that residents are not more likely to impact the approval of future

regulations restricting housing supply.

This paper makes three contributions to the literature. First, our findings build on an established body of research showing that stricter land use regulations are associated with reduced supply (Wu and Cho, 2007), higher housing prices (Glaeser and Gyourko, 2002; Glaeser and Ward, 2009; Kok et al., 2014; Gyourko and Krimmel, 2021), more segregated neighborhoods (Resseger, 2022; Trounstein, 2020), and slower economic growth (Hsieh and Moretti, 2019). Recent work on specific zoning restrictions such as minimum lot sizes finds similar results (Kulka, 2019; Anagol et al., 2021; Kulka et al., 2023; Krimmel and Wang, 2023; Liao, 2023; Peng, 2023; Song, 2024). Likewise, existing work on inclusionary zoning suggests that affordable housing requirements are associated with higher housing prices without significantly increasing housing production (Bento et al., 2009; Schuetz et al., 2011). The literature also highlights that local control over housing decisions can lead to such stricter zoning regulations in whiter, higher-income municipalities due to stronger local opposition and the failure to internalize regional housing affordability problems at the local level (Mast, 2022; Favilukis and Song, 2023). To the best of our knowledge, this is the first paper that examines the local effects of a statewide law aimed at bypassing strict local zoning regulations, not only on housing market outcomes but also on incumbent residents' responses, for which there is little empirical evidence despite their potential impact on the goals of such policies.² Importantly, Chapter 40B also differs from affordable housing requirements set by traditional inclusionary zoning policies, which tend to be voluntarily adopted at the local level.

Second, we expand on the literature on the effects of new construction on local housing markets. Most prior research shows that market-rate construction leads to lower local housing prices, potentially through a supply effect (Li, 2021; Pennington, 2021; Asquith et al., 2023), though Schwartz et al. (2006) suggests that new subsidized housing can increase local property values by replacing existing disamenities. More relevant to our paper, Diamond and McQuade (2019) finds that Low-Income Housing Tax Credit (LIHTC) developments lead to property value decreases in relatively higher-income neighborhoods. Motivated by current state and local government efforts to bypass exclusionary zoning, we extend this literature by examining the effects of mixed-income housing introduced specifically into *high-income, low-density* neighborhoods. In contrast, most prior work

²Prior work on Chapter 40B focuses on its siting patterns (Goetz and Wang, 2020; Sportiche et al., 2025), and the effects of moving to 40B housing on beneficiaries' health and well-being (Sportiche, 2023).

focuses on the effects of new construction in dense, urban areas, where the key arguments against new development center on gentrification and displacement of low-income residents.

Finally, we contribute to the literature on residential sorting by examining migration and political responses to a large shock triggering neighborhood change. Tiebout (1956) argued that preferences for public goods can be revealed by ‘voting with your feet’ in decentralized systems, which may lead to residential segregation (Oates, 1972; Epple and Romer, 1991; Banzhaf and Walsh, 2008; Bayer and McMillan, 2012). In our context, higher move-out rates near 40B developments suggest incumbent residents’ dislike for new construction that bypasses zoning regulations. This concern is particularly relevant for affordable housing, as prior studies show that high-income households strongly prefer living closer to other high-income households (Bayer et al., 2007; Bousttan, 2013; Guerrieri et al., 2013; Diamond, 2016). Several seminal papers document similar patterns with respect to race, showing that white residents are more likely to move out of neighborhoods after Black households move in (Schelling, 1971; Card et al., 2008; Bousttan, 2010; Bayer et al., 2022). In addition to migration responses, we study the political behavior of incumbent residents, highlighting a plausibly important mechanism of housing supply shortages, as political participation can affect future housing development.³

2 Background: Massachusetts Chapter 40B

2.1 Historical Context and Permitting Process

In 1968, a court-mandated study completed by the Massachusetts Legislative Research Council concluded that local governments used zoning regulations to exclude residents from living in their communities on the basis of race and income. The study further concluded that regional or state oversight was a necessary remedy (The Commonwealth of Massachusetts, 1968). The following year, Chapter 40B was enacted with the explicit goal of bypassing these exclusionary zoning laws.

Also known as the ‘Comprehensive Permit Law’, Chapter 40B introduced three key sets of provisions to the state’s legislative landscape. First, 40B began requiring that all municipalities main-

³Prior research studies the political influence of homeowners in stricter zoning laws and limited development (Fischel, 1987, 2005; Dehring et al., 2008; Ortalo-Magné and Prat, 2014; Marble and Nall, 2021). In Massachusetts, Einstein et al. (2019b) documents that homeowners are much more likely to participate in local meetings and overwhelmingly oppose new construction. In contrast, Hankinson et al. (2022) shows that homeowners are more likely to vote in favor of new affordable housing when it replaces blight and is more likely to increase property values.

tain at least 10 percent of their housing stock or 1.5 percent of their land area as affordable, which it enforces by allowing developers to bypass local zoning regulations under certain conditions—described below—in municipalities that do not meet either minimum standard. In practice, the state and local governments almost exclusively rely on the 10 percent standard. The state Executive Office of Housing and Livable Communities (EOHLC) monitors municipal compliance with these standards and counts all housing units that meet the Department of Housing and Urban Development’s definition of affordable housing towards these minimum thresholds.

Second, 40B created the ‘Comprehensive (40B) Permit’, a simplified, streamlined process which aims to entice qualified developers (public agencies, non-profit organizations, and ‘limited-dividend organizations’) to produce more affordable housing in municipalities with restrictive zoning (Krefetz, 2001). To obtain a Comprehensive Permit, developers must apply to one of three programs managed by either EOHLC, MassHousing, or MassHousing Partnership, each of which has slightly different requirements. For example, EOHLC’s Low Initiative Program requires formal municipal support but does not provide project financing, while MassHousing’s program may provide financing and does not require formal support. Then, the Comprehensive Permit allows developers to apply to a single local authority (the Zoning Board of Appeals or ZBA) to receive approval rather than needing to obtain approval from various bodies, substantially streamlining the approvals process at the local level. In exchange, 40B requires that developers propose either rental or ownership projects where at least 20 to 25 percent of the units are affordable for low- and moderate-income households.⁴

Developers can then apply for qualifying mixed-income projects in any municipality through the Comprehensive 40B Permit, even if that municipality exceeds 40B’s minimum affordability standards.⁵ In cases where local governments meet 40B’s minimum standard (e.g., have at least 10 percent affordable housing), developers must obtain municipal support as the local ZBA may deny a developer’s application without repercussions under 40B. However, in cases where local governments do not meet 40B’s minimum standards and the ZBA denies a developer’s Comprehensive Permit application, 40B allows developers to appeal these decisions to a statewide Housing

⁴Either at least 20 percent of a development’s units must be affordable for households with an income of less than 50 percent of the area median income (AMI) or 25 percent of units must be affordable for households with an income of less than 80 percent of the AMI.

⁵The 10 percent standard ensures that all municipalities contribute to affordable housing, regardless of whether projects use a Comprehensive 40B Permit. Other types of housing, such as LIHTC or public housing units, also count toward this threshold even though they are rarely built under 40B.

Appeals Committee (HAC)—the third central feature introduced by 40B—which has historically sided almost entirely with developers in these disputes (Bratt and Vladeck, 2014; Reid et al., 2017).

In practice, most 40B housing has been built in municipalities that fall below the 10 percent affordability threshold. This pattern reflects, in part, the fact that many municipalities have consistently remained below the threshold: 94 percent were noncompliant in 1990, and 78 percent still were in 2020. Appendix Table G.1 shows that 93 percent of 40B developments approved between 1995 and 2019 occurred in noncompliant municipalities. The table also reports approvals by monitoring agency, which serves as a proxy for how contentious a project is, since EOHLIC monitors developments with formal municipal support and MassHousing more often oversees those facing stronger opposition. Both agencies account for a similar number of developments, suggesting that 40B is used under both cooperative and potentially adversarial circumstances. Still, the concentration of production in noncompliant municipalities indicates that the ability to override local zoning—rather than municipal support—is likely a key driver of 40B development (Sportiche et al., 2025). That is, in cooperative cases, municipalities may approve 40B developments preemptively, knowing that the developer can proceed via the appeals process under Chapter 40B otherwise.

2.2 Housing Production and Local Opposition

Comprehensive 40B Permits are one of the most important means of increasing both rental and ownership affordable housing in the low-density, high-income suburbs in Greater Boston. Today, approximately 17,500 affordable units and 53,500 market-rate units have been permitted under 40B, accounting for one in five affordable units in Massachusetts. Though these counts make 40B the second largest affordable housing program in the state after the Low-Income Housing Tax Credit (LIHTC), which has produced 58,000 affordable units and 25,500 market-rate units as of 2022, it remains essentially the only means of producing any new housing in Boston’s affluent and predominantly white suburbs (CHAPA, 2018). Indeed, recent work by Sportiche et al. (2025) shows that neighborhoods with 40B developments tend to be whiter and wealthier than the typical neighborhood in Massachusetts—differences that are even more pronounced when compared to the neighborhoods available to the beneficiaries of the LIHTC or even the tenant-based voucher program. Moreover, most 40B developments are built in municipalities that require overriding strict zoning laws: over 50 percent of affordable 40B units are in neighborhoods zoned for single-family

residences, most of which have features such as large minimum lot sizes that are characteristic of exclusionary zoning. In contrast, only three percent are in multifamily-zoned areas (Sportiche et al., 2025).⁶

However, Chapter 40B’s ability to bypass local zoning regulations has been very contentious, and makes it an extremely unpopular policy tool among the residents of affected municipalities. As a result, proposals for 40B developments often draw enormous opposition from local residents who campaign against the new construction, raising a long list of concerns in public meetings (Einstein et al., 2019a), such as decreased property values, changes to the neighborhood character, increased traffic, and increased congestion of public services.⁷ The contentious nature of 40B was particularly evident when it was introduced as a ballot measure in 2010, asking Massachusetts residents whether they wished to repeal the policy. While the measure was rejected with 58 percent of the votes, suburban areas in Greater Boston showed the greatest support for repealing 40B.

Overall, the longevity of Chapter 40B, its substantial housing production in the high-income, low-density suburbs of Greater Boston, and its contentious nature make it an excellent setting for studying residents’ responses to construction resulting from policies overriding local zoning laws.

3 Empirical Strategy

We estimate the impact of Chapter 40B on local housing markets and residents using a difference-in-differences design that compares housing very close to 40B developments to housing located farther away. To implement this approach, we gather a rich set of data on 40B developments, residential sales, residents’ address histories, and voter registration records.

3.1 Data

We link a validated 40B address and permit dataset with data on property sales, residents’ migration patterns, and resident voting behavior to obtain a comprehensive picture of the evolution of housing markets and local residents’ responses to 40B construction.

⁶The remaining fraction of affordable units are in areas zoned for the following uses: mixed-use (3 percent), non-residential business (12 percent), non-residential other (7 percent), industrial (22 percent) and not zoned (1 percent).

⁷These instances are often covered by the media (e.g., see articles in *The Boston Globe*; 2015, 2016, 2017).

40B developments. Our first dataset was introduced by Sportiche et al. (2025)⁸ and contains the precise geocoded addresses for 1,209 developments (representing 5,288 buildings, an estimated 17,689 subsidized units, and about 71,000 total units) permitted using a Comprehensive 40B permit. Though any affordable housing development can count towards a municipality’s 10 percent threshold under the first of Chapter 40B’s key provisions whether or not it was permitted using a Comprehensive Permit, we focus on only developments built using a Comprehensive Permit to ensure that we are isolating the effects of housing made possible only under Chapter 40B.

In addition to addresses, these data include the number of subsidized units in each development; whether the development is rental or ownership; and the dates the Comprehensive Permit, building permit, and occupancy permit were filed. Because these data recover the precise geocoded addresses of 40B buildings, we overcome a major shortcoming of prior studies, which focus on municipality-level effects of Chapter 40B due to inaccuracies in publicly available 40B addresses. An important note is that our data only contain the addresses of affordable units in the case of ownership 40B developments. This limitation makes it challenging to interpret the results in specifications that include residents moving in after the approval of a 40B development, since we cannot exclude owners of 40B market-rate units. However, our main results refer to the house prices of preexisting housing and responses of incumbent residents and are not affected by this limitation.

We focus on the 899 developments which began the permitting process between 1995 and 2019. We consider the date when the Comprehensive Permit—the earliest permit which marks the process of planning a 40B development—was approved as the treatment event. For the 12 percent of developments where this date is missing, we use the minimum of the building permit approval date, the date when the 40B was included in the SHI, and the occupancy permit approval date.

House prices. To measure changes in housing values, we use data from Zillow’s ZTRAX on real estate transactions. This dataset contains information regarding the universe of residential sales in Massachusetts and its coverage is consistent starting in 1987 (Appendix Figure F.1). For each house sale, the transaction dataset contains a transaction id, address, sale date, sale price, mortgage

⁸This study obtains the data from the Executive Office of Housing and Livable Communities’ (EOHLC) Subsidized Housing Inventory (SHI). The SHI does not have accurate street addresses as these are collected at the time when the original Comprehensive Permits are filed, when complete and full addresses are not always known. Sportiche et al. (2025) validate 40B addresses using a combination of online real estate listings (e.g., Zillow or Apartments.com), Google Maps, Street View and local news articles. For more details, please see Sportiche et al. (2025).

information, foreclosure status, and other information collected by the local tax assessor. We merge this with other property characteristics that Zillow obtained from local assessors' offices, such as property type, lot size, number of bedrooms and bathrooms, and year of construction.

Our final house price data includes only residential arms-length transactions of single units. More specifically, we restrict the sample to single-family residences and condominiums, dropping those signaled as intra-family transactions, and removing outliers.⁹

Infutor data. We use Infutor data to track migration responses among residents living near 40B developments. This dataset, collected by Infutor Data Solutions from a number of private and public sources and introduced to the economics literature by Diamond et al. (2019), contains information on the address history of most adult individuals in the United States since the 1980s. These data include information on resident names, dates of birth, gender, precise street addresses, and move in month and year for each address. We exploit these data to construct two novel, longitudinal datasets. The first dataset includes 'all local residents': individuals who ever lived at an address within 0.6 miles of any 40B development. The second dataset is restricted to 'incumbent residents', which we define as the subset of local residents who were already living at that address in years 7 and 8 years prior to 40B permit approval and, thus, are likely to better capture long-term residents.

Voter files. We use data from the Massachusetts voter file, collected by L2 and previously used in studies in political science and economics (Velez and Newman, 2019; Enamorado et al., 2019; Chyn, 2018), to examine the effects of 40B on residents' political behavior. These data contain voter registration dates and the individual histories of federal and state election turnout since 1996 and local election turnout since 2008. These data also include information on voter names, date of birth, gender, and current address. Relying on names and addresses, we link voter data to our longitudinal panel of Infutor residents using a probabilistic linkage method described in detail in Appendix A.1. This method allows us to match voter data to 45 and 59 percent of all local residents and incumbent residents, respectively.

⁹We drop transactions with sale prices below \$25,000 or above \$10 million, and winsorize sale prices at the top and bottom 1 percentiles of the yearly price distribution.

Other data sources. Finally, we complement these rich individual panel data with demographic, socioeconomic and housing characteristics at the census tract level from the decennial census for years 1990, 2000, 2010; as well as zoning data from MassGIS, compiled from maps sent by each municipality in the mid-2000s and previously used in Resseger (2022). This zoning dataset allows us to identify basic zoning characteristics at each point location.¹⁰

3.2 Descriptive Statistics: The Location of 40B Development

As Figure 1 illustrates, 40B developments are mostly concentrated in suburban municipalities in Greater Boston and along the Cape. While Boston has no 40B units as it has long been well above the 10 percent affordable housing threshold that makes the policy binding, the municipalities closest to Boston have the highest levels of 40B production. The approval of 40B developments was spread over our sample period, though there was a notable surge in approvals in the early 2000s, as Appendix Figure F.2 shows.

Table 1 describes the characteristics of our final sample of 40B developments and the census tracts and nearby sales characteristics (within 0.6 miles) that surround them. The first three columns focus on samples of all 40B developments while the next three focus on samples of 40B developments with a total of 50 units or more. Within each set of columns, we provide summary statistics for the full sample (columns 1 and 4), 40Bs approved between 1995 and 2019 (columns 2 and 5)—which we use in our main analysis—and a balanced sample of 40Bs approved between 1995 and 2007 (column 3 and 6)—for which we can observe at least twelve post-approval years.

Overall, Table 1 shows that 40B developments are located in census tracts that have higher-income households, higher-ownership shares, a more expensive housing stock, and that are lower-density when compared to the average Massachusetts tract. As the last column shows, 40B developments also tend to be in tracts with larger white populations and a higher share of college-educated residents. These statistics suggest that studying the effects of 40B on house prices and resident responses is particularly relevant as the higher share of owner-occupiers near 40Bs and more expensive housing imply that potential home value losses could be larger than in other areas. Finally, slightly less than half of 40B developments are rentals and the average 40B development is large,

¹⁰Census data and shapefiles were obtained from Steven Manson, Jonathan Schroeder, David Van Riper, Tracy Kugler, and Steven Ruggles. IPUMS National Historical Geographic Information System: Version 15.0 [dataset]. Minneapolis, MN: IPUMS. 2020. <http://doi.org/10.18128/D050.V15.0>. We thank Matthew Resseger for sharing the original shapefiles, which are no longer publicly available.

containing around 58 units. Though large 40Bs are more likely to be rental, the characteristics of the census tracts where they are located are similar to the average 40B developments.

3.3 Empirical Specification: the Ring Method

A key issue in studying the impact of 40B developments on surrounding neighborhoods is the endogenous selection of 40B development sites, which may be correlated with factors that also affect house prices and residents' responses. For instance, developers may strategically decide to build in neighborhoods where house prices are rising faster or where local opposition is minimal. Thus, a simple comparison between neighborhoods with and without 40B developments would likely be confounded by unobserved neighborhood characteristics.

We tackle this issue by using a stacked difference-in-differences design that compares the change in house prices (resident outcomes) within an inner ring of a certain radius around 40B developments to the change in house prices (resident outcomes) within an outer ring surrounding the inner ring around the time of 40B approval. This method, known as the ring method, has been used in previous studies on the effects of housing policies on neighborhoods (Diamond and McQuade, 2019; Li, 2021; Asquith et al., 2023). In this paper, we closely follow the methodology outlined by Blanco and Neri (2025). The intuition of this approach is that treatment intensity is determined by proximity to 40B developments. Hence, the outermost ring should not be as affected by 40B construction and can serve as a comparison group. The identifying assumption is that, in the absence of 40B construction, the outcome of interest would have changed in parallel in the inner and outer rings. Because the distances we will consider are small, it is plausible that both rings belong to the same micro-neighborhood, making distance to 40B developments the only difference between housing (residents) in the inner and outermost rings after controlling for observable characteristics.

We construct the dataset for our analyses by including all residential sales (residents) within 0.6 miles from 40B developments. We do this separately for every 40B development and then 'stack' all these sub-datasets, which we index by the corresponding development d .¹¹ We create an event year variable defined as the difference between the calendar year t when the transaction occurred and the year E_d when the 40B permit was approved for the associated development d . Since the

¹¹ As a result of this process, some sales (residents) may show up for multiple times in the stacked dataset if they are within 0.6 miles of multiple 40B developments (i.e., rings for different 40Bs may overlap). Sections 4 and 5 provide robustness checks with similar results in cases where we drop sales (residents) in overlapping rings.

number of sales around each development can be low—because neighborhoods with 40B housing are usually low-density—, we group event years into event year-pairs τ to minimize the number of event periods without sales. We limit the period to ten years before and twelve years after permit approval. Note that we exclude sales (residents) in newly constructed 40Bs because our objective is to examine the impact of 40B developments on nearby housing (residents).

Our estimating equation is an event study at the housing unit (resident) h , 40B development d , and year t level:

$$Y_{hdt} = \alpha_{dt} + \kappa_{d,r(h,d)} + \sum_{\tau=-10}^{12} \sum_{r \in R} \beta_{\tau,r} \mathbb{1}(t - E_d = \tau, r(h, d) = r) + \gamma' \mathbf{X}_{hdt} + \epsilon_{hdt} \quad (1)$$

We are interested in $\beta_{\tau,r}$, the coefficient on the interaction between event year-pair τ indicators and indicators for the inner ring $r(h, d)$ where housing unit (resident) h is located with respect to 40B development d . For instance, $\beta_{\tau,r}$ captures the change in house prices in inner ring r compared to the outer ring in year-pair τ relative to the 40B approval year. We include indicator variables only for ‘treated’ rings, $r \in R = \{0-0.1\text{mi}, 0.1-0.2\text{mi}, \dots, 0.4-0.5\text{mi}\}$. Thus, housing (residents) located between 0.5 and 0.6 miles is the omitted group, i.e., ‘comparison ring’.

Given the included 40B-specific controls, the coefficient $\beta_{\tau,r}$ represents the difference in outcomes between the inner rings and the outermost ring around each 40B development. 40B-calendar year FE (α_{dt}) and 40B-ring FE ($\kappa_{d,r(d,h)}$) control for time patterns across all rings around each 40B development and baseline differences across rings, respectively. Additionally, we interact 40B development indicators with housing characteristics (\mathbf{X}_{het}).¹²

We also estimate a version of Equation (1) that combines post-treatment years into three periods: 0-4, 5-8 and 9-12 years. This specification is intended to separate the early stages after the approval of a 40B and the later stages when the development is already completed—on average, 40B residents

¹²We include indicator variables for month-of-sale, property type, decade-of-construction, number of bedrooms, bathrooms, and stories, and lot size deciles. We winsorize these variables to limit outliers.

move in four years after the 40B permit is approved.¹³ We estimate the following equation:

$$Y_{hdt} = \alpha_{dt} + \kappa_{d,r(h,d)} + \sum_{r \in R} (\theta_{1,r} \text{Post}_{dt}^{0-4} + \theta_{2,r} \text{Post}_{dt}^{5-8} + \theta_{3,r} \text{Post}_{dt}^{9-12}) \times \mathbb{1}(r(h, d) = r) + \gamma' \mathbf{X}_{hdt} + \epsilon_{hdt} \quad (2)$$

In our main analyses, we estimate the equations above for the sample of 40B developments approved between 1995 and 2019 and cluster standard errors at the 40B development level.

Our stacked difference-in-differences strategy is robust to negative weighting issues in staggered adoption designs with treatment effect heterogeneity (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfœuille, 2020; Baker et al., 2022; Gardner, 2022) and has been widely used in prior work (e.g., see Cengiz et al., 2019; Deshpande and Li, 2019). Each sub-dataset, which in our case includes housing within 0.6 miles of a specific 40B development, is a sub-experiment with treated units (inner rings) and control units (outermost ring). Stacking these sub-datasets and estimating a regression that interacts fixed effects and control variables with an indicator for each sub-dataset, we recover a weighted average of 40B development-specific treatment effects. We use the corrective sample weights proposed by Wing et al. (2024) to interpret $\beta_{\tau,r}$ as the average treatment effect across 40B developments.¹⁴ In Appendix E.1, we estimate the main results using the method proposed by Gardner (2022), also robust to treatment effect heterogeneity, and obtain similar estimates.

4 How Do 40B Developments Affect Nearby Properties?

4.1 The Impact on Neighborhood Outcomes

We begin by examining how 40B developments affect their immediate surroundings to understand the magnitude and type of shock 40B developments represent in receiving neighborhoods. First, we show that 40B developments substantially increase density in their surrounding neighborhoods. Panel (a) of Figure 2 plots the estimates of a Poisson regression based on Equation (1) and using the cumulative count of residents living within several distances from 40B developments per square mile. In this analysis, we use cumulative distances and include 40B residents to better

¹³Given that information on completion dates is unavailable, we use Infutor data to obtain estimated occupancy dates, which should be close to completion. Appendix A.3 provides the details.

¹⁴Specifically, we weight each observation h by $1/(N_{r(h,d)}/N_r)$ within each event year. The numerator indicates the number of observations in ring r of 40B development d to which h belongs, while the denominator is the total number of observations in ring that same ring r across all 40B developments.

understand how experienced density changes for nearby residents. The comparison group is the cumulative count of residents living within 0.6 miles of the development. After approval, density increases by up to 50 percent ($e^{0.4} - 1 = 0.49$) within 0.1 miles, by 16 percent within 0.2 miles, and does not change much beyond that distance. Larger 40B developments lead to even bigger changes: Appendix Figure F.3 shows that 40Bs with 50 or more units more than double density within 0.1 miles (110 percent), while smaller 40Bs increase density by one fifth within the same distance.

We also show that 40B developments change neighborhood composition by bringing in residents coming from slightly less educated neighborhoods with lower housing values, mostly from other areas within Massachusetts. Panel (b) of Figure 2 shows the origin neighborhood characteristics of individuals moving into these neighborhoods within twelve years of the approval of a 40B. We report the results separately for individuals moving into 40B units and those moving into units either up to 0.2 miles or between 0.2 and 0.4 miles away from a 40B. The comparison group consists of individuals moving into units located 0.4 to 0.6 miles away. Compared to this group, 40B residents are more likely to come from census tracts that are less dense, have slightly lower levels of educational attainment, and have lower housing values. There is also some suggestive, though not statistically significant, evidence that 40B residents may come from lower-income areas. The small differences may be due to affordable units typically representing only 25 percent of all units 40B buildings, while our sample also includes the 75 percent of individuals in market-rate units.

While this paper focuses on the localized effects of 40B developments, our next set of analyses also present suggestive evidence of impacts at broader levels. We first examine the effect of 40B developments on schools, given that overcrowding is a common concern among opponents of new housing in low-density areas. Using data from the Massachusetts Department of Elementary and Secondary Education (DESE) and a staggered difference-in-differences design that compares school districts that approved 40B developments earlier to those that approved 40B developments later (Callaway and Sant’Anna, 2021), we find that school enrollment increases by roughly 8 percent beginning about six years after approval (see Appendix Figure F.4). This increase rises to about 12 percent when focusing on districts that approved larger 40B developments with at least 50 units in a given period. While these results indicate that 40B developments do lead to higher school enrollment, assessing whether such effect results in overcrowding is more challenging, as we do not have data on school capacity. In addition, we find no meaningful change in the share of students who do

not classify as non-Hispanic white, suggesting that—at least at a high level—40B developments do not alter the racial composition of the student body.

Our final municipality-wide analyses aim to understand the impact of new 40B developments on municipal budgets. Using data from the Department of Revenue and a similar strategy to the aforementioned school analyses at the municipality level, we find that 40B developments are associated with comparable increases in both local expenditures and revenues (see Appendix Figures F.5 and F.6). About six years after approval, spending on education and public safety—municipalities’ two largest budget categories, accounting for roughly 50 and 11 percent of expenditures, respectively—increases by 10 percent. While tax revenues remain largely unchanged—and make up about 70 percent of local revenue—, other sources of revenue rise by 20 percent. These patterns hold for both any 40B development and those with more than 50 units. On net, we find no significant effect on local budget balances for the average 40B development, though larger developments may lead to a slight reduction. These fiscal estimates should be interpreted with caution, as standard errors widen considerably in post-treatment periods and most estimates are not statistically significant.

4.2 The Impact on Local Property Values

4.2.1 Main Results

We find that, *on average*, 40B developments do not affect local housing prices. Figure 3 plots the effect of 40B developments on the logarithm of sale prices of nearby preexisting housing, defined as units built at least 10 years before the associated 40B development approval, for the entire sample of 40B developments (the left two panels) and 40B developments with 50 or more units (the right two panels). Panels (a) and (b) report the event study results, and panels (c) and (d) report the results of estimating Equation (2), where we pool post-treatment event years into three periods.

Panels (a) and (c) show that the average 40B development has no significant effect on local property values at any time or distance. This null effect is precisely estimated: we can reject price *reductions* larger than 0.4 percent and price *increases* larger than 2.4 percent within 0.1 miles at the 5 percent significance level when aggregating all post-treatment periods, both of which are—in absolute value—below the 2.5 percent drop in prices that Diamond and McQuade (2019) estimate within this same distance of LIHTC developments in higher-income areas. This comparison is noteworthy given that 40B developments in Massachusetts are located in higher-income, higher-

priced areas than LIHTC developments (Sportiche et al., 2025).

However, the null average effect masks important heterogeneity. Panels (b) and (d) show that larger 40B developments do lead to reductions in local property values eight years after permit approval. While we find no evidence that larger 40B developments change prices in the short or medium term, our results suggest that permitting larger developments reduces prices by up to 9 percent within 0.1 miles in the long run (Table 2, Column 1 shows the coefficients that correspond to panel (d)). In contrast, developments with fewer than 50 units increase prices by about 5 percent over the same distance and time frame, likely explaining why the average effect across all 40B developments is close to zero (Appendix Figure F.7). As we show in Appendix C, this positive impact is driven by the smallest developments (fewer than 12 units) built in areas not zoned for single-family homes. Given our interest in understanding local opposition to new housing and the fact that larger 40B developments drive the negative price effects, the remainder of the section focuses on these larger developments.

The delayed response of prices for larger 40B developments, which has also been found in other contexts (Blanco and Neri, 2025), roughly corresponds to the period when 40B residents are increasingly moving in (Figure 2) and school enrollment is rising (Appendix Figure F.4). While we cannot fully disentangle them, two explanations align with this timing. If supply-side forces are mostly at play, the timing may reflect when new 40B units enter the market and exert downward pressure on prices, particularly if nearby homes are close substitutes. Alternatively, if the price decline is demand-driven, it may suggest that homebuyers either fail to anticipate neighborhood changes or respond primarily to realized—rather than expected—conditions, such as increased density or the arrival of lower-income households. We further explore these mechanisms in Section 4.2.2.

We also find that 40B developments lead to a sharp increase in the number of nearby housing sales, driven primarily by the sale of new 40B units, with a modest 8 percent rise in sales of nearby preexisting housing (Appendix Table G.2). When including new 40B units, the total number of sales within 0.1 miles roughly doubles, and the mix of transacted properties shifts notably, with a sharp decline in the share of single-family home sales. In contrast, the characteristics of transacted preexisting housing, the focus of our price regressions, do not change following 40B approval. This suggests that our earlier price effects are not driven by changes in the quality of transacted housing, but instead reflect changes in how buyers value proximity to a 40B development.

While our main analysis focuses on highly localized effects, we also examine broader housing market impacts at the census tract level in Appendix Table G.3. Using data for all census tracts in Massachusetts, we regress the change between 1990 and 2019 in several outcomes on an indicator for whether a tract approved any 40B units during this period (columns 1-4), and add a second indicator for tracts with 50 or more 40B units (columns 5-8). By including municipality fixed effects, these regressions compare tracts with 40Bs to other tracts in the same municipality that did not approve any. We find that tracts with 40Bs expanded their housing stock by 9 percent more and had homeownership rates about 2 percentage points lower than comparable tracts. Consistent with our highly localized results, home values did not change significantly. However, rents increased by 4.5 percent, particularly in tracts with larger 40B developments. This finding may be explained by new 40B market-rate rental units, which can constitute a large share of the rental housing in these predominantly homeowner areas and may be of higher quality than the existing rental stock.

A caveat of the results is that we ignore general equilibrium effects because we focus on the very localized impact on incumbent residents, who have been argued to bear the largest property value decreases. Since 40B developments are usually located in low-density suburban municipalities, a 50-unit development can lead to a big shock that extends beyond the neighborhood surrounding these developments to the municipality more broadly. For instance, it may lower house prices across the entire municipality due to increased supply, especially in a small municipality. Large 40B developments may also decrease the attractiveness of the neighborhood relative to the rest of the municipality (e.g., by increasing local traffic patterns or noise), which may increase relative demand for other neighborhoods.

4.2.2 Mechanisms: Supply and Disamenity Effects

There are several potential explanations for the negative price effects near larger 40B developments. 40B developments shift the housing supply curve outwards, exerting downward pressure on prices. This supply channel may explain the highly localized results if there is strong market segmentation and new 40B housing is a close substitute to nearby housing, otherwise any supply effect should dissipate across the entire housing market (Kristof, 1965; Sweeney, 1974; Braid, 1981). 40B developments also affect housing demand. For instance, 40B housing may present a negative physical externality to local residents, such as a perceived loss of ‘neighborhood character’, and in-

cumbent and prospective residents may negatively value the increase in low- and moderate-income residents moving into new 40B housing. Other potential explanations such as negative effects on the provision of public goods (e.g., congestion in public schools or traffic) are less plausible because the effect of larger 40B developments on prices are highly localized.

In Table 3, we find suggestive evidence supporting both the increased supply and the negative demand shock hypotheses through various sets of heterogeneity analysis (see Appendix C for an extended analysis). Regarding the former, we show that price decreases within 0.1 miles are stronger for ownership 40B developments, which directly compete with nearby for-sale houses. As evidence for demand-side mechanisms, we observe that the largest 40B developments (96 or more units) lead to greater price reductions when compared to smaller developments (between 50 and 95 units), suggesting that larger 40B developments generate additional disamenity effects within 0.1 miles. Two additional results highlight the potential significance of the ‘neighborhood character’ argument: 40B approvals only negatively impact the sale prices of single-family houses, and price decreases are more pronounced around 40B developments in single-family zoned areas.

4.2.3 Discussion

The magnitude of the price effects we estimate for larger 40B developments is larger but qualitatively consistent with prior research on new construction in high-income neighborhoods. As noted above, Diamond and McQuade (2019) finds price decline of 2.5 percent for LIHTC developments in relatively higher-income, whiter neighborhoods. Other studies also report smaller, localized price reductions for nearby housing in dense urban settings (Pennington, 2021; Li, 2021). Our estimates are likely larger due to three reasons. First, our negative effect applies to a subset of larger 40B developments. Second, 40B developments are disproportionately located in higher-income and whiter neighborhoods than LIHTC developments (Sportiche et al., 2025). Third, we only focus on single-family residences and condominiums, which may be more sensitive to nearby multifamily construction than other housing types.

A comparison with the local effects of similarly sized developments *not* permitted under Chapter 40B further highlights the importance of 40B’s mechanism of bypassing zoning laws in explaining the results. Using tax assessment data from Zillow, we identify 295 non-40B developments with 50 or more units built outside of Boston between 1995 and 2012. Given that these developments

are typically in denser municipalities closer to Boston, we estimate their effects on local housing prices using inverse probability weighting to align the tract-level characteristics of neighborhoods around non-40B developments with those around large 40Bs. As shown in Appendix B, we find that non-40B developments lead to spatially broader declines in sale prices—ranging from 3 to 7 percent up to 0.4 miles away—compared to the highly localized effects of 40B developments. This difference may reflect a larger role of increased competition in communities where non-40Bs is built, given that nearby preexisting housing may be more similar to units in the new development, while 40Bs tend to be surrounded by single-family homes in lower-density, strictly zoned areas.

Finally, we compare the property value losses experienced by households near larger 40B developments to the overall housing value produced under Chapter 40B. To estimate the average value of a 40B unit, we focus on developments approved between 1995 and 2012—allowing sufficient time for construction—and convert their tax assessment values into market values using year-specific adjustment factors derived from the relationship between sale prices and assessment values (see Appendix D for details). Importantly, this calculation does not incorporate the property value gains observed near smaller 40B developments, resulting in a conservative estimate of the net value generated by 40B housing. Similarly, we estimate nearby property value losses by focusing on single-family housing within 0.2 miles of larger 40B developments, which is the type of housing affected by 40B according to our heterogeneity analysis in Table 3.

We estimate that Chapter 40B has generated approximately \$17.4 billion (in 2019 dollars) in housing value since its inception, with developments of 50 or more units accounting for \$12.88 billion (74 percent) of this total. Between 1995 and 2019, these figures are \$12.77 and \$9.31 billion for all 40B developments and large developments alone, respectively. In contrast, the estimated property value losses for the roughly 10,000 single-family homes located within 0.2 miles of larger 40B developments approved between 1995 and 2019 total \$0.29 billion, only 3 percent of the aggregate value created by these larger developments.

4.2.4 Robustness

Our price results are robust to using different samples, an alternative identification strategy, and a placebo test. First, our result is not sensitive to the choice of distance to define the comparison group, as shown in Figure F.8, which uses housing within 0.8 to 1 mile away as the comparison

group. In Appendix Table G.4, which shows price effects in years 9 to 12 after 40B approval in the first five rings for all 40B developments and larger 40B developments, we run the analysis on alternative samples and change the definition of ‘treatment event’. The first columns of each 40B type report our main estimates, while the second columns show that results are very similar when using a balanced panel of 40Bs approved between 1995 and 2007, for which we can observe the full post-treatment period (columns 2 and 7). Since a potential concern is that our definition of permit approval year may incorporate some noise because we define it as the minimum of several permit dates due to missing information, columns 3 and 8 show that our results are similar using 40B developments for which the Comprehensive 40B Permit year is not missing. Finally, columns 4 and 9 show that our findings are robust to dropping duplicated sales across developments.

Appendix E.2 further shows that the interpretation of our results does not change when we use an alternative empirical strategy that exploits the quasi-exogenous timing of 40B approval to find a plausible comparison group. In this strategy, we compare housing within a certain distance of a 40B currently being approved to housing within the same distance of a 40B that will be built in the future. Using this method, we find no effects near the average 40B development and long-run prices decreases of roughly 13 percent within 0.1 miles of larger 40B developments.

Finally, a placebo test also supports our findings. We run the event study specification again by setting the treatment period nine years before the actual 40B approval year. This choice is grounded on the trade-off between losing some early 40Bs as the placebo year takes place earlier when data is not available and testing whether price decreases eight years after 40B approval are spurious. As a result, we use 40Bs actually approved between 2002 and 2012. Appendix Figure F.9 shows that the post-treatment coefficients of this regression are not statistically significant.

5 How Do Incumbent Residents Respond to 40B Developments?

We study whether incumbent residents react to new developments built under Chapter 40B, focusing on larger 40B developments where negative price effects are observed. We find that incumbent residents do respond to these lower property values, but to a limited extent: only a small fraction of residents move out in response to the approval of 40B developments, and we find no compelling evidence that they change their political behavior.

5.1 Do Incumbent Residents Move Away?

New 40B developments may prompt incumbent residents to move away, potentially undermining 40B's goals of racial and income desegregation. Given that opponents of denser housing often argue that construction negatively affects neighborhood character together with the observed reduction in property values, local residents may relocate to areas where the perceived risk of such changes is lower. These moves may be to either other areas within the same municipality where new construction is less feasible or to municipalities where regulations better protect low-density housing, consistent with Tiebout (1956)'s model of 'voting with your feet'. If these moves disproportionately involve wealthier, whiter residents, move-out responses could undermine 40B's main objective of desegregation (Oates, 1972; Epple and Romer, 1991).

To explore this hypothesis, we study move-out rates among local residents using Infutor data. We focus on larger 40B developments throughout the rest of the paper, since any resident responses should be most salient where we observe the highly localized, negative price effects. We will present the results of estimating Equations (1) and (2) using an indicator variable for whether the resident moves out of their address near a 40B development in a given event period as the outcome.¹⁵

We start by showing that the approval of a 40B development increases move-out rates among the sample of all local residents. Column 2 of Table 2 shows that larger 40B developments raise move-out rates by 8 percentage points (double from the baseline) within 0.1 miles and by nearly 3 percentage points (one third) within 0.1-0.2 miles over a twelve-year period. The event study in panel (a) of Figure 4 (and the pooled DID in panel (c)) illustrates that, unlike prices, move-out rates jump immediately after 40B approval. However, these higher move-out rates may capture two facts different than direct responses to 40B developments. One is that we may be capturing residents moving out of pre-40B buildings when 40B replaces preexisting housing. Another possibility is that incoming households after the approval of 40B developments live in their units for shorter periods of time. In addition, the sample of all local residents includes individuals residing in market-rate units within 40B *ownership* developments, as SHI data does not contain their addresses and thus we cannot exclude them from the sample.

¹⁵In our analyses, we only include resident-year observations when local residents were living within 0.6 miles from the 40B developments, that is, we drop out their stays in addresses before and after their stay near a 40B. As a result, we capture the move-out rates for residents remaining in their original unit at each point in time.

Given these challenges, we focus on move-out responses among incumbent residents, defined as those already residing near a 40B development seven to eight years before approval. Column 5 of Table 2 shows that larger 40B developments also increase move-out rates among incumbent residents within 0.1 miles by up to 83 percent. Move-out rates go up by 4 percentage points within 0.1 miles (from a 4.8 percent baseline) and by 1.6 percentage points between 0.1 and 0.2 miles (26 percent from a 6.2 percent baseline) over a twelve-year period. The event study in panel (b) of Figure 4 shows that incumbent residents begin moving out of the area gradually after the approval of 40B developments. Appendix Figure F.10 shows that move-out responses to 40B developments with less than 50 units are always close to zero regardless of distance to the development, consistent with the lack of an effect on prices for these smaller developments.

Column 6 of Table 2 uses the cumulative move-out rate for incumbent residents, i.e., an indicator variable that takes the value 1 after a resident moves out of their pre-40B address and keeps that value until the end of the sample period we consider, as the outcome.¹⁶ We estimate a 3.2 percentage-point increase within 0.1 miles in the long run and a 1 percentage-point increase between 0.1 and 0.2 miles, though this second result is not statistically significant. The first number implies that the average large 40B development would induce 1.28 of the 40 incumbent residents we detect living within the first 0.1 mile-wide ring to move out by year 12 after 40B permit approval. Similarly, 1 of the 100 residents living within 0.1 and 0.2 miles move out in response to such shock, which makes a total of 2.28 out of 140 incumbent residents (1.6 percent) within 0.2 miles moving out in the long run in response to the approval of larger 40B developments. This number is low when compared to the 35 percent increase in population within 0.2 miles achieved by the construction of these larger 40B developments, as shown in Appendix Figure F.3(b).

Finally, we show that the increase in move-out rates among incumbent residents does not result in a decline in population. To examine this, we construct a stacked dataset tracking the number of move-ins and move-outs at the addresses of incumbent residents for all rings around each 40B development. Appendix Figure F.11 presents event study results using these two measures, along with their net difference, as outcome variables. While both move-ins and move-outs increase within 0.2 miles following development—mirroring the patterns in Figure 4—we find no significant change

¹⁶That is, we include all resident-years between event years -6 and 12 relative to 40B permit approval in this analysis, even in the case where the incumbent resident no longer lives within 0.6 miles of a 40B development.

in the net difference between them. This suggests that although turnover increases, the number of people moving in offsets the number moving out, resulting in no overall population loss.

Heterogeneity. In Table 3, we conduct several sets of heterogeneity analyses for incumbent residents using several development and neighborhood characteristics to split the sample of larger 40B developments in two, converting any non-binary variables into indicators denoting whether a 40B development is above or below the median value in the sample. In Appendix Table G.6, we extend this analysis to a broader set of heterogeneity variables and the sample of all local residents.

Move-out rate responses are larger for residents near the types of 40B developments that are the most different to low-density housing. Rental 40Bs and 40Bs above the median size lead to move-out rate increases of roughly 5.1-6.4 percentage points among incumbent residents, while ownership and smaller 40Bs show lower increases, around 2-2.6 percentage points (columns 1-4).

We find that move-out responses among incumbent residents are driven by non-homeowners. Linking Zillow records on homeownership with Infutor data, we identify property owners near 40B developments.¹⁷ Only incumbent residents who we do *not* identify as homeowners show a significantly positive effect on move-out rates (columns 7 and 8), which is consistent with homeowners being less mobile due to high psychological, social, and monetary costs associated with changing homes (DiPasquale and Glaeser, 1999; Haurin and Gill, 2002; Andersson and Mayock, 2014). This result may also explain why 40B developments in single-family zoned areas, which have more homeowners, are less likely to induce move-outs among incumbent residents (columns 5 and 6).

Destination neighborhoods. We find suggestive evidence that incumbent residents tend to relocate to lower-density neighborhoods, though our results have low statistical power due to the limited number of residents moving out at close distances. Appendix Table G.5 indicates that, while most characteristics of destination tracts remain unaffected following 40B approval, incumbent residents move to tracts with densities that are 30 percent lower in the long run (column 2). This finding

¹⁷This process is described in detail in Appendix A.1. Appendix A.4 demonstrates that our imputed ownership status is highly correlated with census data on ownership rates and further describes ownership rates around 40B developments. We consider that an individual is an ‘owner’ if it satisfies the following criteria. First, the individual must own the property corresponding to the address where they lived near a 40B (i.e., a ‘neighboring address’). After using the fuzzy matching algorithm, we restrict the two addresses (living address and ownership address) to have the same street name. Second, this individual is considered an owner if they either (1) show up in 2020 tax assessment data and did not buy the property after the 40B permit associated to their neighboring address was approved, or (2) do not show up in 2020 tax assessment data and sold the property 12 years before 40B permit was approved.

offers some limited evidence consistent with Tiebout (1956)’s model, since some residents seem to move to lower-density tracts that better fit their preferred ‘neighborhood character’.

Overall, while the new, denser construction in high-income, low-density municipalities through 40B induces additional move-outs, it is unlikely that these migration responses can substantially undermine the policy’s goal of reducing racial and economic segregation for two reasons. First, homeowners, who are plausibly higher-income than renters, are much less likely to move out. Second, the magnitude of our results is relatively small when we scale it and compare it to smaller shocks in the literature: Bayer et al. (2022) find a 6 to 8 percent increase in homeowners’ move-out rates in the two years after a neighbor of a different race moves into the same block, while our estimate corresponds to a much larger shock. Given that larger 40B developments have an average of 37 affordable units and 147 total units (Table 1, column 5), our main estimate of 83 percent increase in move-out rates for incumbent residents in years 9 to 12 after approval translates into a modest 2.2 and 0.6 percent increase in move-out rates per 40B affordable unit and 40B unit, respectively.

5.2 Do Incumbent Residents Become More Politically Active?

Other than by moving out, incumbent residents may react to 40B development by changing how they engage in state and local politics, which may affect future housing supply. Prior research shows that Massachusetts homeowners who oppose new construction are overwhelmingly more likely to participate in local meetings than the general public (Einstein et al., 2019a,b). Fischel (2005) argues that homeowners have the incentive to work towards restricting undesired development to protect their property values. In this line, perceived decreases in property values from larger 40B developments may trigger greater political participation among incumbent residents and, as a result, stricter regulations on new construction may be more likely to succeed.

In contrast to this hypothesis, we find no evidence that larger 40B developments affect voter turnout in general (state and federal) elections among the samples of all local residents and incumbent residents, as shown in columns 3 and 7 of Table 2, respectively. These columns estimate the pooled DID specification in Equation (2) using an indicator variable for whether an individual voted in a general election (usually held every two years) as the outcome variable, separately for each sample of residents. Panels (a) and (c) of Figure 5 plot the event study estimates.¹⁸ We further

¹⁸We restrict our sample to 40B developments approved between 2000 and 2019 because voter registration data

restrict our sample by including only voters who are registered to vote in the fourth or fifth year before 40B approval and who lived near the development during its approval year or the subsequent year, thus examining voter turnout among registered voters directly affected by 40B. In both the pooled DID and event study specifications, we find that coefficients are not statistically different from zero at any distance from the 40B development. Panel C of Table 3 show that there is little heterogeneity in these effects.

Despite the null results on general elections, we should expect the largest impact on local elections. Residents are more likely to influence the construction of future development in the high-income suburban communities that we study by participating in local meetings (see Einstein et al. (2019b) for anecdotal evidence in Massachusetts). There are three main challenges in examining voting patterns in local elections, however. First, voter data for such elections only starts in 2008 in our sample. Thus, we limit the analyses to 40B developments that are approved between 2010 and 2017, which decreases the precision of our estimates. Second, there is no publicly available dataset gathering the election dates for all local elections. We tackle this issue with a data-driven approach: by counting the number of residents who voted in a local election at a specific date for each municipality in Massachusetts, we can infer that the dates with the highest participation were dates where a local election was taking place. We describe this process in detail in Appendix A.2. Third, we cannot observe the municipality where residents are registered to vote. Thus, we include all residents that we see living in the same municipality as the 40B development, regardless of whether they were registered to vote. That is, our outcome of interest is voter turnout among the entire population of residents.

From our results, we cannot conclude that Chapter 40B significantly impacts participation in local politics over an eight-year period (columns 4 and 5 of Table 2). While 40B developments do not affect voter turnout in local elections for the average resident living within a 0.1-mile distance, column 5 suggests that this effect may be positive for the subset of incumbent residents. However, the corresponding event studies in panels (b) and (d) of Figure 5 show that estimates are not precisely estimated for the last two years of the sample period due to the small number of 40B developments with data for this post-treatment event period.

for general elections starts in 1996. In voter turnout regressions, we use development-resident fixed effects instead of development-ring fixed effects to control for individual-level unobserved characteristics. In addition, we eliminate living spells in addresses outside of Massachusetts, given that residents would not be able to vote in the state.

5.3 Does 40B Lead to Stronger Opposition to New Housing?

While we find only null or weak effects on the political responses of incumbent residents in general and local elections, exposure to 40B housing may still significantly shift their political stance on housing-related issues.

To study this issue, we take advantage of a 2010 Massachusetts ballot measure asking to repeal Chapter 40B. Our empirical approach consists of comparing ballot results for precincts that had permitted or were about to permit at least one 40B development at the time when the ballot measure was proposed in 2010 to precincts that would permit their first 40B development soon after 2010. This strategy is grounded in the argument that residents already affected by a 40B development may be more opposed to Chapter 40B because they were already exposed to these developments.¹⁹

To implement this strategy, we gather data containing the ballot measure results at the precinct level from the Secretary of the Commonwealth of Massachusetts and then we compute the earliest calendar year in which a 40B was approved for each precinct. Due to the small sample, we also include smaller 40B developments in this analysis. Using these measures, we divide precincts into five groups by 40B approval period g : precincts with no 40Bs ($N=1,564$, $g = 1$), and precincts with their first 40B approved before 1995 (142, $g = 2$), 1995-2005 (254, $g = 3$), 2006-2012 (95, $g = 4$), and 2013-2019 (51, $g = 5$). Because groups 3 and 4 are undergoing their first 40B approval, their responses should be strongest if exposure to 40B development affects voting on this ballot measure, while the responses for group 5 should not be affected. We estimate the following equation:²⁰

$$\text{Yes share}_p = \alpha + \sum_{g=1}^5 \beta_g \mathbb{1}(g(p) = g) + \omega' \mathbf{X}_p + u_p \quad (3)$$

where Yes share_p is the ratio of the number of residents voting ‘Yes’ over the number of residents voting either ‘Yes’ or ‘No’. We omit group 3 and include the following standardized control variables at the municipality level: population density (which is our precinct-level variable), the share

¹⁹This approach is reasonable if the year when a 40B development is approved is plausibly exogenous. Appendix E.2 assesses the validity of this assumption by showing that the census tracts with 40B developments approved earlier are generally similar to census tracts with 40B developments that are approved later in the period. Some differences do exist—the latter group do have a higher share of the population with a college degree and lower rental prices—, however, given that we will control for these variables, our assumption is that, controlling for neighborhood characteristics, the time when a 40B is approved is as good as random.

²⁰Note that we match 40Bs with precincts by using precinct shapefiles. Out of 2,173 precincts, we are unable to match 67 precincts in the shapefile with precincts in the ballot measure dataset.

of residents aged 65 or older, the share of residents that are not white, the share of residents with at least a college degree, the owner-occupied share, the median home value, median household income, and an indicator for whether the precinct is in the Greater Boston area.

While the coefficient magnitudes imply slightly more support for repealing Chapter 40B in precincts with at least one 40B development, we cannot reject that this effect is in fact null. Figure 6 shows the results without (panel (a)) and with controls (panel (b)). When we do not include controls, precincts with future 40Bs (group 5) are 3.4 percentage points less likely to vote in favor of repealing 40B compared to precincts whose first 40B was approved in 1995-2005. This result is statistically significant and there is no difference among precincts with their first 40B being approved at any time before 2010. However, the difference in positive votes between precincts exposed earlier versus later goes down to 1.5 percentage points when we account for neighborhood characteristics in Equation (3) and is no longer statistically significant. Overall, we cannot conclude that 40Bs lead affected residents to vote more strongly against housing policies that bypass zoning regulations.

5.4 Robustness

The move-out and vote turnout rate results hold under the robustness checks in Section 4. First, Tables G.7-G.10 in the Appendix reproduce Table G.4 for these two outcomes. The tables include one additional specification: for households showing up as local residents for multiple 40B developments, we keep the observations corresponding to the nearest 40B ('Near'). For both move-out rates and general elections, results are qualitatively similar across all columns specifications. Second, the estimates are qualitatively similar when using 40B developments approved later in the period as the comparison group (Appendix Figure E.5). While the identifying assumption does not seem to hold for voter turnout, the results are consistent with an increase in move-out rates and no change in voter turnout within 0.1 miles when compared to slightly farther away areas. Third, the placebo test using 40B approval dates taking place nine years before the actual date yields estimates that are close to zero for the two outcomes (Appendix Figure F.12).

6 Conclusion

This paper provides new evidence on the effects of a major state policy that bypasses local zoning regulations in high-income, low-density neighborhoods on local housing prices and incumbent

residents' migration and political responses. First, we document that 40B developments that local zoning would have otherwise not allowed lead to a substantial increase in housing density but on average do not affect local property values. Focusing on a subset of larger 40B developments that do lead to reductions in the sale price of nearby houses, we show that only a small fraction of incumbent residents respond to these losses by leaving their homes and that they do not respond by participating more in general and local elections, or by voting to repeal 40B.

Our findings suggest that move-out rates are likely not large enough to substantially undermine 40B's goal of reducing racial and economic segregation. Changes in move-out rates are lower than other estimates in the literature, despite the fact that the shock of a large 40B development to local housing markets is greater than the housing market shocks studied in other similar papers (Bayer et al., 2022). Moreover, we find that this impact on move-out rates is concentrated among non-homeowners, which suggests that the policy may improve income diversity while mitigating concerns about large outflows of high-income households after an inflow of low-to-moderate income households. This result is in contrast to prior studies in racial contexts that document 'white flight' (Card et al., 2008; Boustan, 2010).

Finally, we do not find evidence that bypassing zoning regulations shifts political participation, which could impact future housing supply. That is, the results alleviate the concerns that opposition to future new construction in affected neighborhoods by local residents, which are most affected by new construction, will be stronger. This finding is especially relevant given that prior literature finds that homeowners are politically more active in these high-income areas and tend to oppose such policies (Einstein et al., 2019b).

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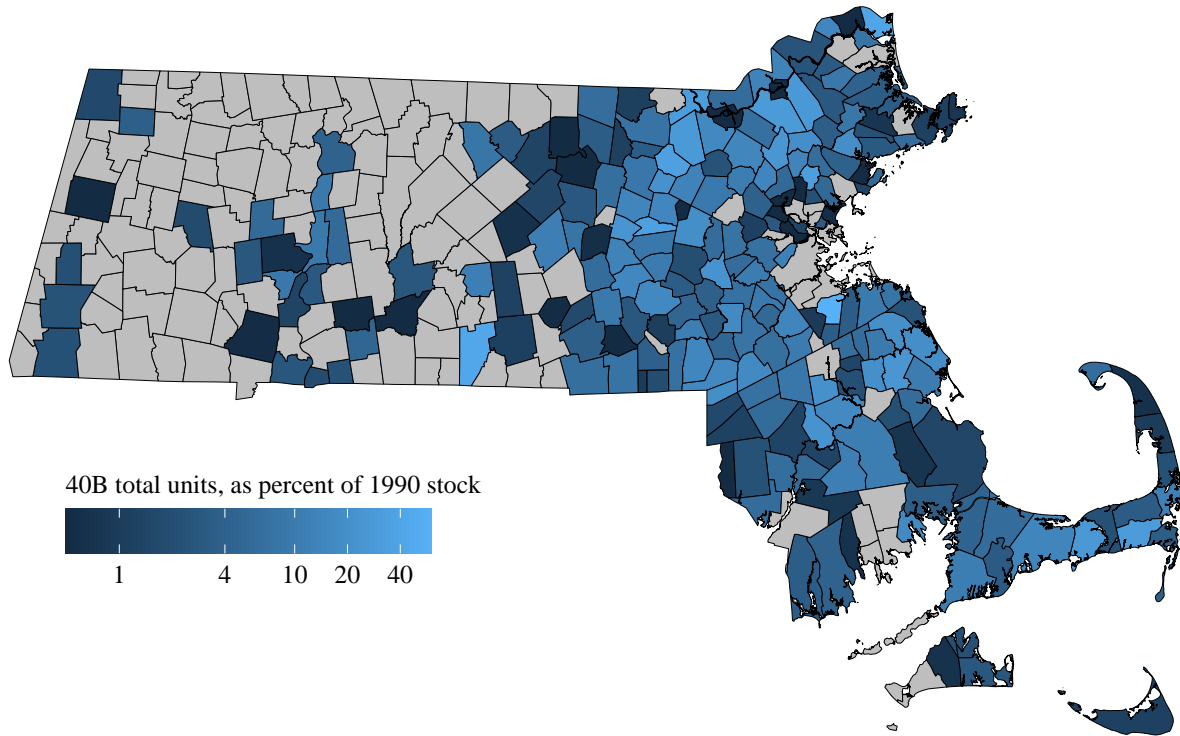
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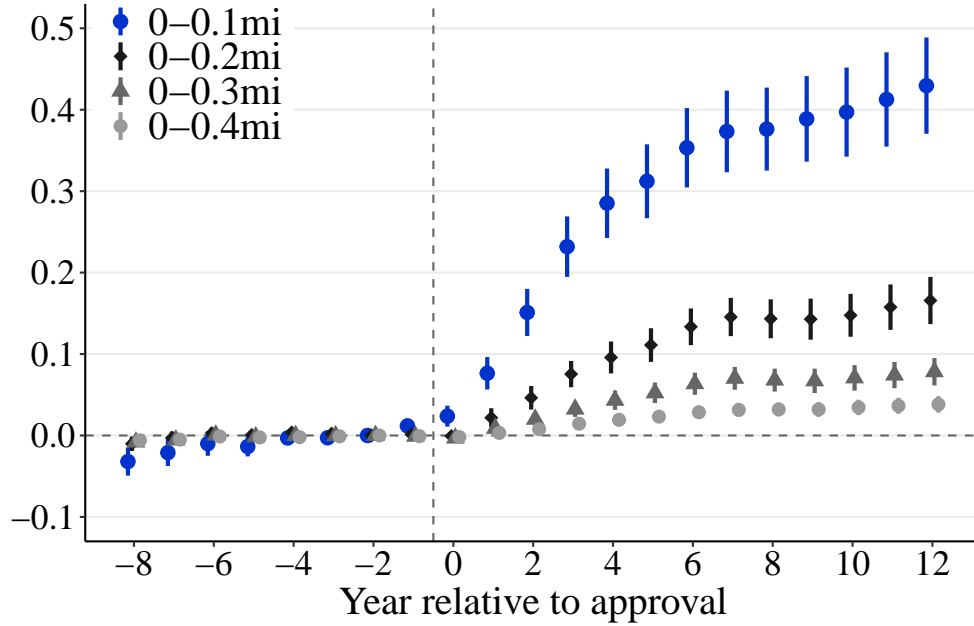
Figures

Figure 1: Number of approved 40B units between 1995 and 2019 by municipality

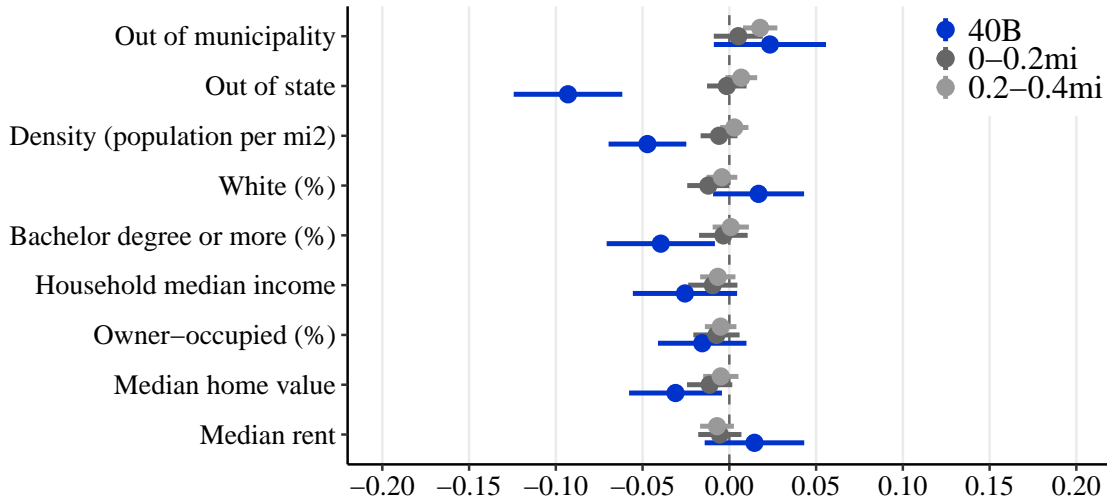


Note: The map shows the total number of units (includes both market and affordable units) approved under a Comprehensive 40B Permit between 1995 and 2019 by municipality as a share of 1990 housing units in the decennial Census. We identify 40B units using the Subsidized Housing Inventory (SHI) from the Executive Office of Housing and Livable Communities (EOHLC) in Massachusetts. Each 40B address has been verified in the procedure detailed in Sportiche et al. (2025). Municipalities in gray did not approve any 40B units as reported in SHI. The legend coloring is log-transformed for visual purposes.

Figure 2: Population density around 40B developments and origin neighborhood characteristics of 40B residents



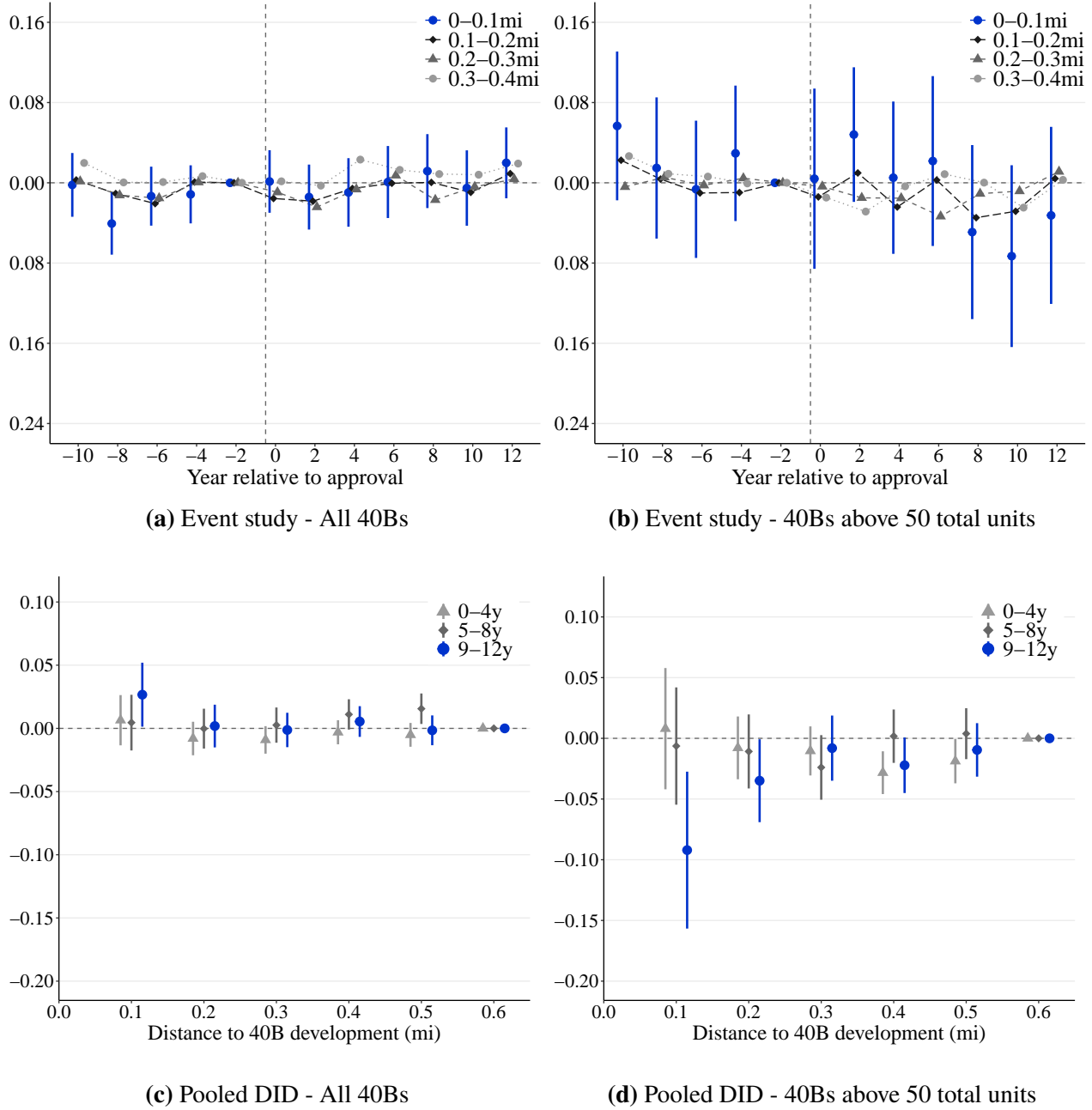
(a) Population density by distance to 40Bs



(b) 40B residents: Origin neighborhood characteristics

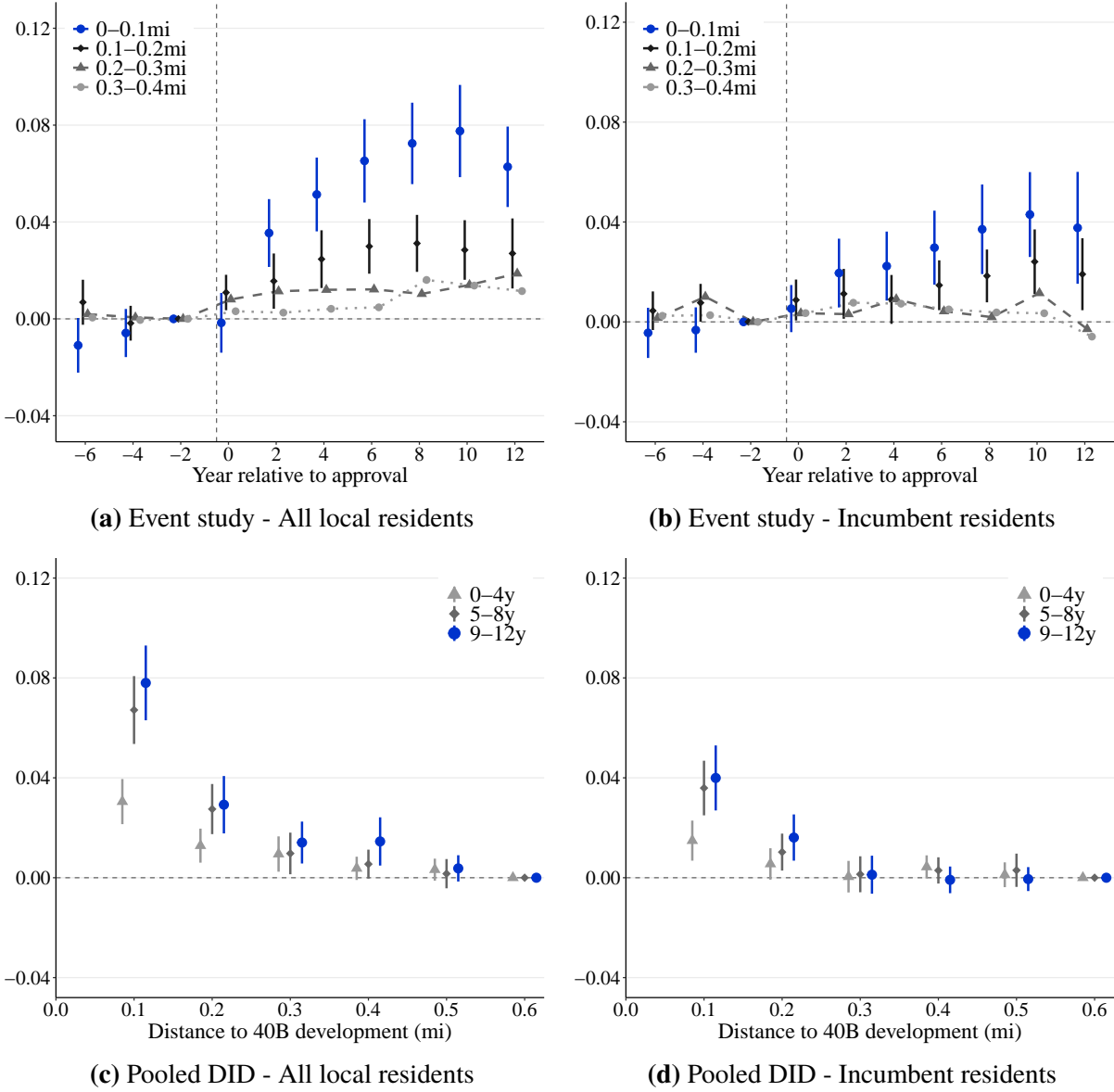
Note: Panel (a) plots the estimated coefficients $\beta_{\tau,r}$ of a Poisson regression that is analogous to Equation (1) using the average number of individuals per square mile that Infutor reports as living within each (cumulative) ring around 40B developments as an outcome. This population count includes 40B residents. Panel (b) plots the coefficients of separately regressing each of the origin neighborhood characteristics on the left-hand side of the plot on three mutually exclusive indicator variables denoting whether an individual lives in a 40B development, or within 0-0.2 miles, or within 0.2-0.4 miles of the development. Individuals living within 0.4-0.6 miles are the omitted group. The sample includes individuals moving into a 40B or within each of the rings from 0 to 12 years after the approval of the 40B. Outcome variables are standardized (except for the first two variables) and the regressions include development-by-year fixed effects. We weight each 40B development equally and standard errors are clustered at the 40B development level. Both panels use the sample of 40B developments approved between 1995 and 2019. Bars represent 95% confidence intervals.

Figure 3: DID estimates of the impact of 40B developments on sale prices of preexisting housing



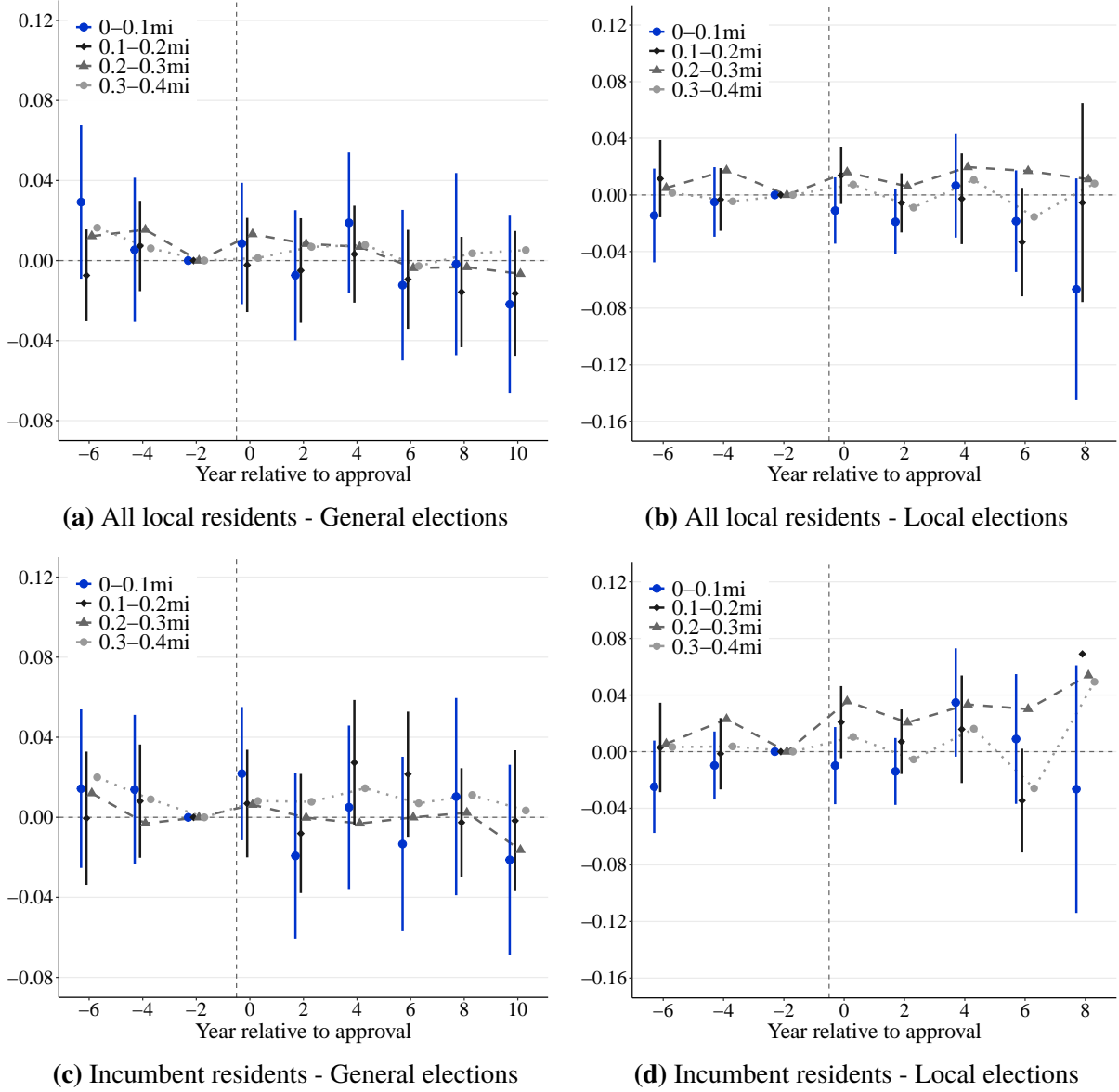
Note: The figures plot the estimated coefficients $\beta_{t,r}$ in Equation (1) (panels a and b) and $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ (panels c and d) in Equation (2) using 0.1 mile-wide rings and the logarithm of sale prices as the outcome. We restrict the sample to 40B developments approved in 1995-2019 and sales of preexisting units. Panels (a) and (c) include the entire sample of 40Bs in this period; panels (b) and (d) include only 40Bs with a total of 50 units or more.

Figure 4: DID estimates of the impact of large 40B developments on residents' move-out rates



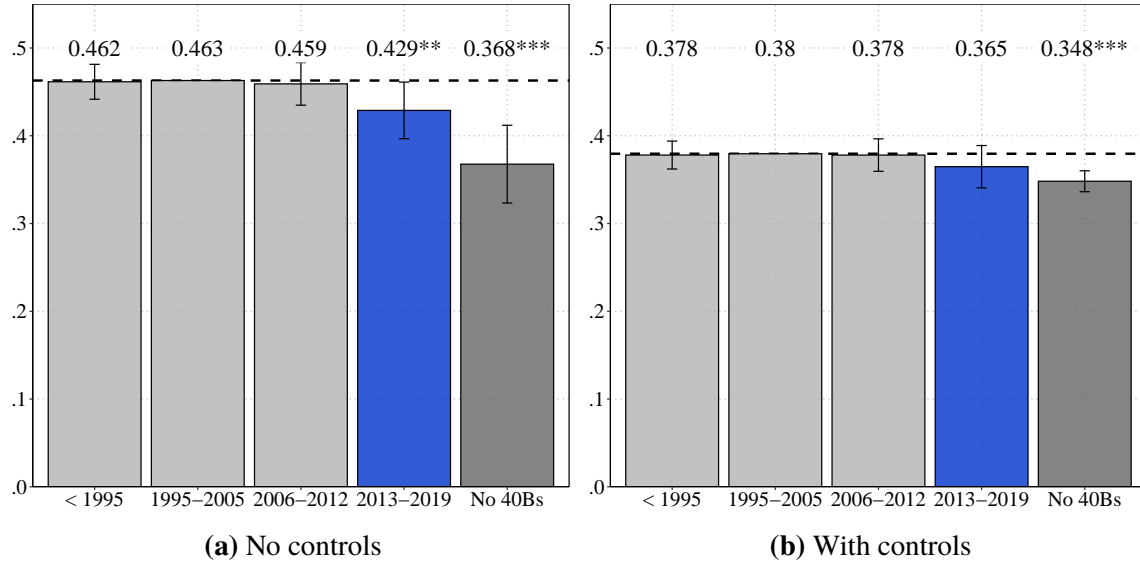
Note: The figures plot the estimated coefficients $\beta_{\tau,r}$ in Equation (1) (panels a and b) and $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ (panels c and d) in Equation (2) using 0.1 mile-wide rings and an indicator variable for whether the individual moved out from the address neighboring a 40B as the outcome. We restrict the sample to 40B developments approved in 1995-2019 with 50 or more units. Panels (a) and (c) uses the sample of 'all local residents', while panels (b) and (d) use the sample of 'incumbent residents' who were living near 40B developments in years 7 and 8 before permit approval, as defined in Section 3.1.

Figure 5: Event studies of the impact of large 40B developments on residents' voter turnout rates



Note: The figures plot estimated coefficients $\beta_{\tau,r}$ in Equation (1) using 0.1 mile-wide rings and an indicator variable for whether an individual voted in a general (panels a and c) or local (panels b and d) election as the outcome. We restrict the sample to 40B developments approved in 2000-2019 with 50 or more units. Panels (a) and (b) uses the sample of 'all local residents', while panels (c) and (d) use the sample of 'incumbent residents' who were living near 40B developments in years 7 and 8 before permit approval, as defined in Section 3.1.

Figure 6: Share voting ‘Yes’ to repealing Chapter 40B by precinct type



Note: The bars show the estimates of coefficients β_g in Equation (3) after adding back the estimate of constant α . 95% confidence intervals are shown in the error bars (standard errors clustered at the municipality level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Tables

Table 1: Summary statistics of 40B developments

Characteristic	All developments			50 units or more			MA
	Full	95-19	95-07	Full	95-19	95-07	
	(1)	(2)	(3)	(4)	(5)	(6)	
Panel A: Development							
Total units	58	56	52	134	147	137	
Affordable units	15	14	13	34	37	34	
Number of buildings	3.7	3.8	4.3	5.2	5.6	7.0	
Permit approval year (min)	2,003	2,007	2,003	2,002	2,007	2,003	
Tenure: ownership	0.49	0.58	0.69	0.26	0.30	0.41	
Tenure: rental	0.48	0.39	0.29	0.70	0.64	0.55	
Agency: DHCD	0.49	0.41	0.35	0.40	0.29	0.24	
Agency: MassHousing	0.33	0.39	0.37	0.43	0.49	0.48	
Panel B: 2010 census tract							
Population: total	5,282	5,273	5,283	5,533	5,605	5,557	4,430
Population: density (mi2)	2,173	2,031	2,094	2,500	2,046	2,108	6,904
Population: 65 or more	0.15	0.15	0.14	0.15	0.15	0.15	0.14
Race: white	0.88	0.88	0.87	0.88	0.89	0.88	0.79
Race: black	0.023	0.022	0.023	0.026	0.024	0.027	0.07
Education: High school or less	0.08	0.08	0.09	0.07	0.06	0.07	0.12
Education: Bachelor or more	0.45	0.46	0.43	0.45	0.47	0.45	0.39
Household median income	86,900	88,843	85,096	87,315	90,868	88,140	70,357
Housing: owner-occupied	0.74	0.75	0.74	0.72	0.75	0.73	0.60
Housing: median rent	1,014	1,031	1,028	1,046	1,096	1,114	981
Housing: median home value	411,359	420,010	400,512	401,518	411,243	402,774	354,834
Distance to CBD (mi)	34	34	36	28	27	28	31
Panel C: 2010 sales (< 0.6mi)							
Sale price (dollars)	352,661	360,695	342,647	344,049	349,340	343,110	
Number of sales	29	29	27	32	31	32	
Year built	1,964	1,965	1,965	1,966	1,969	1,968	
Stories	1.73	1.73	1.74	1.72	1.71	1.71	
Bedrooms	2.92	2.93	2.91	2.87	2.88	2.89	
Single family (share)	0.77	0.76	0.77	0.75	0.74	0.74	
Condo (share)	0.23	0.24	0.23	0.25	0.26	0.26	
Number of developments	1,209	899	598	416	270	174	

Note: Panel A uses data from the Subsidized Housing Inventory (SHI), processed as described in Sportiche et al. (2025). We define permit approval year as the minimum of the Comprehensive 40B Permit year, the building permit year, the occupancy permit year, and the year when 40B was added to the SHI. Panel B uses 2010 census data. Panel C uses data from Zillow ZTRAX for residential sales taking place within 0.6mi of 40B developments in 2010. The first three columns include the full sample of 40B developments, the next three columns restrict it to those with 50 units or more. Within each group, the first column reports the average for the entire sample; the second, for 40Bs approved between 1995 and 2019; the third, for 40Bs approved between 1995 and 2007 (balanced sample for which we can report results up to 12 years after approval). The last column shows the characteristics of the average census tract in MA.

Table 2: DID estimates for house prices, move-out rates, and voter turnout for large 40B developments

	Log(price)	All neighbors			Incumbent neighbors			
		Move-out	Turnout (General)	Turnout (Local)	Move-out	Move-out (cumulative)	Turnout (General)	Turnout (Local)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Yrs. 0-4: 0-0.1mi	0.008 (0.025)	0.030*** (0.005)	-0.001 (0.011)	-0.009 (0.008)	0.015*** (0.004)	-0.029*** (0.008)	-0.001 (0.011)	-0.000 (0.010)
Yrs. 0-4: 0.1-0.2mi	-0.008 (0.013)	0.013*** (0.003)	-0.001 (0.008)	0.003 (0.007)	0.006* (0.003)	0.000 (0.006)	0.004 (0.009)	0.014 (0.009)
Yrs. 5-8: 0-0.1mi	-0.006 (0.024)	0.067*** (0.007)	-0.024 (0.015)	0.000 (0.012)	0.036*** (0.006)	-0.002 (0.013)	-0.023 (0.018)	0.039** (0.016)
Yrs. 5-8: 0.1-0.2mi	-0.011 (0.015)	0.028*** (0.005)	-0.013 (0.009)	-0.008 (0.017)	0.010*** (0.004)	-0.004 (0.008)	0.010 (0.010)	0.007 (0.018)
Yrs. 9-12: 0-0.1mi	-0.092*** (0.033)	0.078*** (0.008)	-0.029 (0.019)		0.040*** (0.007)	0.032** (0.015)	-0.011 (0.021)	
Yrs. 9-12: 0.1-0.2mi	-0.035** (0.017)	0.029*** (0.006)	-0.009 (0.014)		0.016*** (0.005)	0.010 (0.010)	0.009 (0.014)	
Unique N per 40B (0-0.1mi)	14	82	19	38	40	40	12	22
Unique N per 40B (0.1-0.2mi)	30	212	51	64	100	100	30	40
Y mean (0-0.1mi)	249,361	0.078	0.734	0.093	0.048	0.151	0.760	0.094
Y mean (0.1-0.2mi)	264,307	0.088	0.756	0.105	0.062	0.192	0.771	0.120
Observations	145,693	6,659,850	974,710	405,092	2,923,192	4,728,334	605,179	250,694
R ²	0.868	0.205	0.479	0.580	0.042	0.190	0.488	0.600

Note: The table reports estimates for coefficients $\theta_{1,r}$, $\theta_{2,r}$, and $\theta_{3,r}$ in Equation (2) for the first two rings and several outcomes: logarithm of sale prices (column 1), an indicator for whether the resident moved out from an address within 0.6mi from a 40B development (columns 2 and 5, we drop residents after they move out, i.e., this variable represents the move-out rate among remaining residents), an indicator that takes the value 1 for all periods after the previous variable turns 1 (column 6, we keep residents throughout the entire sample period), and an indicator for whether the resident voted in a general (columns 3 and 7) or a local (columns 4 and 8) election. In the first two rows after the estimates, we report the average number of sales per 40B in the first two rings (column 1) and the average number of unique residents in each ring in the move-out and turnout rate regressions (columns 2-8). We also report the outcome average in the pre-treatment period. Standard errors in parenthesis (clustered at the 40B level). Columns 3, 4, 7, and 8 include development-by-individual fixed effects. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Heterogeneity results: long-run price effects on sale prices, move-out and voter turnout rates of incumbent residents for large 40B developments

	Tenure		Development size		40B Zoning		Sale type	
	Ownership (1)	Rental (2)	<median (3)	>median (4)	Other (5)	SFR (6)	Condo (7)	SFR (8)
<i>Panel A: Sale prices</i>								
0-0.1mi	-0.104** (0.052)	-0.075** (0.035)	-0.067* (0.037)	-0.153*** (0.058)	-0.035 (0.044)	-0.124*** (0.043)	-0.010 (0.036)	-0.126* (0.067)
0.1-0.2mi	-0.053* (0.028)	-0.020 (0.021)	-0.032 (0.023)	-0.040 (0.025)	-0.022 (0.029)	-0.046** (0.021)	0.033 (0.036)	-0.048** (0.022)
Developments	95	175	131	139	144	126	181	270
Observations	48,659	97,034	88,006	57,687	80,547	65,146	64,698	80,995
R ²	0.851	0.875	0.850	0.884	0.870	0.865	0.921	0.869
	Tenure		Development size		40B Zoning		Owner	
	Ownership (1)	Rental (2)	<median (3)	>median (4)	Other (5)	SFR (6)	No (7)	Yes (8)
<i>Panel B: Move-out rates - incumbent residents</i>								
0-0.1mi	0.026*** (0.007)	0.051*** (0.010)	0.020*** (0.006)	0.064*** (0.012)	0.044*** (0.010)	0.036*** (0.009)	0.046*** (0.008)	0.009 (0.007)
0.1-0.2mi	0.008 (0.007)	0.022*** (0.006)	-0.002 (0.005)	0.038*** (0.008)	0.030*** (0.007)	0.002 (0.005)	0.015*** (0.005)	0.006 (0.004)
Developments	95	175	131	139	144	126	270	269
Observations	980,935	1,942,257	1,672,699	1,250,493	1,552,352	1,370,840	2,033,342	889,850
R ²	0.042	0.043	0.041	0.043	0.050	0.034	0.045	0.074
<i>Panel C: Voter turnout in general elections - incumbent residents</i>								
Yrs. 9-12: 0-0.1mi	0.020 (0.034)	-0.030 (0.029)	-0.007 (0.024)	-0.023 (0.037)	0.005 (0.031)	-0.030 (0.029)	0.004 (0.025)	-0.015 (0.028)
Yrs. 9-12: 0.1-0.2mi	0.028 (0.022)	-0.005 (0.019)	0.029* (0.016)	-0.016 (0.027)	0.006 (0.026)	0.014 (0.014)	0.025 (0.017)	0.006 (0.019)
Developments	86	165	121	130	138	113	251	250
Observations	206,369	398,810	339,569	265,610	322,132	283,047	322,726	282,453
R ²	0.474	0.498	0.492	0.486	0.484	0.493	0.482	0.511

Note: The table reports estimates for coefficient $\theta_{3,r}$ in Equation (2)—i.e., 9-12 years after 40B approval—using 0.1 mile-wide rings for several outcome variables (sale prices, move-out rates, and voter turnout). Each column uses a different sample of 40B developments depending on: tenure (ownership or rental 40Bs, columns 1-2), 40B development size (above or below median in the sample, 3-4), whether the 40B development is in an area zoned as single-family residence (5-6), whether the transacted housing is a condominium or a single-family home (7-8 in panel A), and whether the incumbent resident is a homeowner (7-8 in panels B and C). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Supplementary Appendix

‘Local Effects of Bypassing Zoning Regulations’

Hector Blanco (Rutgers University) and Noémie Sportiche (Mathematica, Harvard University)

A Data Appendix

A.1 Linking 40B Developments to Infutor and Voter Records

We link address data on 40B developments to information about 40B beneficiaries and local residents from Infutor and the Massachusetts voter file in five steps. First, we standardize all linkage fields. Second, we identify 40B beneficiaries by linking 40B building addresses with individuals’ residential addresses in Infutor. Third, we create a dataset containing all Infutor individuals ever living within 0.6 miles of a 40B address, excluding 40B beneficiaries. Fourth, we link these individuals to voter records using a combination of individual’s names, birth dates, and addresses. Finally, we also link Infutor individuals to tax assessment and deed records from Zillow. We use probabilistic linkage methods for the second, fourth, and fifth steps.

Step 1. Standardizing Linkage Fields. We use the same protocol laid out by Sportiche (2023), which is based on the steps recommended in previous work (Winkler, 2014; Abramitzky et al., 2012; Enamorado and Imai, 2019) to standardize linkage fields.

These steps include removing all punctuation, converting all street suffixes to the USPS standard (e.g., ‘route’ becomes ‘route’ or ‘street’ becomes ‘st’), and parsing addresses into street number, street name, city, five-digit zip code, and—where applicable—unit numbers. We also standardize all Massachusetts municipalities to those listed in the 2010 census using a crosswalk of historical municipality and neighborhood names drawn from the Massachusetts Secretary of State’s database of archaic community and neighborhood names.²¹

We follow a similar protocol to standardize names and birth dates. First, we remove all punctuation. Then we split individuals’ full names into first name, middle initial (where applicable), and last name, removing all prefixes and suffixes (e.g., JR, junior, II, III) such that a name like James R. Jones III becomes James R Jones. Finally, we also remove all special characters from birth dates and convert them into a YYYYMM format.

Step 2. Identifying 40B Beneficiaries. We identify 40B beneficiaries by determining whether each address in Infutor is a 40B development address. We do so by linking the precise geocoded addresses of 5,288 buildings (1,209 developments) permitted under 40B in 1995 or after to addresses in Infutor based on street number, street name, city, and state. We use the `fastLink` package in R

²¹<https://www.sec.state.ma.us/cis/cisuno/unoidx.htm>

to complete this linkage using slightly more conservative parameters than those recommended in supplementary material provided by (Enamorado et al., 2019). Using this method, we are able to identify at least one address in Infutor for 93 percent of 40B developments. The vast majority of these are exact matches. In Infutor, this represents about 29,064 unique address IDs and 112,789 individuals.

Step 3. Identifying Non-Beneficiary Residents Living Within 0.6 Miles from a 40B Building.

We identify non-beneficiary residents living near 40B addresses in three steps. First, we create a dataset of all Infutor individuals who ever lived in Massachusetts excluding 40B beneficiaries as identified in the previous section. Second, we identify 1,067,388 unique address IDs in Infutor which are within 0.6 miles of the 5,288 buildings permitted under 40B. Third, we find 4,585,542 unique individuals included in the first step dataset who ever lived in an address identified in the second step ('40B non-beneficiary individuals').

We build our main dataset of *all local residents* by restricting this sample to 40B developments with a permit approval in 1995-2019. We are left with 768,137 unique address IDs and 3,456,180 unique individuals. Next, we discard individuals who started living in the address near a 40B development too late (12 or more years after the approval of the 40B) or finished living in that address too early (6 or more years before the approval of the 40B). This dataset of local residents contains 2,491,682 unique individuals.

We also build a separate dataset for *incumbent residents*. To do this, we only keep 40B non-beneficiary individuals who lived in the address neighboring the 40B building in years 8 and 7 before the year when the 40B permit was approved for that building. There are 605,004 such individuals. For these individuals, we create a panel dataset going from year 6 before approval to year 12 after approval or the year when the individual is last seen in Infutor, whichever takes place first.

Step 4. Linking Voter Records to Infutor Individuals. We link data on 40B non-beneficiary individuals to voter records based on first and last name, year of birth, street number, street name and municipality in Massachusetts. As described in the Step 2, we link individuals using slightly more conservative parameters via the `fastLink` package in R. We begin with about 5,185,067 million individuals registered to vote since 2012 and about 4.7 million 40B non-beneficiary individuals. Following recommendations from Abramitzky et al. (2020) and Winkler (2014), we block on fixed variables where mistakes are unlikely to expedite the matching process. Specifically, we restrict our comparisons to pairs of individuals who have last names starting with the same letter (e.g., we only compare records for people whose last names begin with "A" in both datasets). To maximize the matching rate, we include all addresses for each individual as observed in Infutor. In case of multiple matches to different individual ids in the voter file, we keep the match with the highest

probability. Using this method, we are able to match a total of 1,910,430 40B non-beneficiary individuals in the voter files.

Given that voter history records for general elections only start in 1996, we restrict our sample to 40Bs that were approved between 2000 and 2019 in the regressions involving voter turnout outcomes. With this in mind, we first match the linked dataset to the dataset of *all local residents* and restrict the sample to individual-address stays corresponding to Massachusetts. Out of 2,365,959 local residents, 1,073,152 of them show up in voter records (45.4% matching rate). We do the same for the dataset of *incumbent residents* (this time we do not discard addresses outside of MA): out of 605,004 incumbent residents, 358,565 of them show up in voter records (59.3% matching rate). In our empirical analysis, we further restrict the two samples to individuals who were registered to vote in year four or five before the 40B approval and lived near the 40B in year zero or one after its approval, which leaves a total of 319,049 local residents and 200,271 incumbent residents.

A potential concern for our analysis is that we may have a more incomplete coverage for earlier 40B developments because we only observe voter histories for individuals who are registered to vote in any year after 2012 (included). Figure A.1 shows the average 40B development-level matching rates by permit approval year for the sample of incumbent residents. Despite our data limitations, matching rates for this sample of individuals living within 0.6 miles of 40B developments is remarkably consistent during the entire study period.

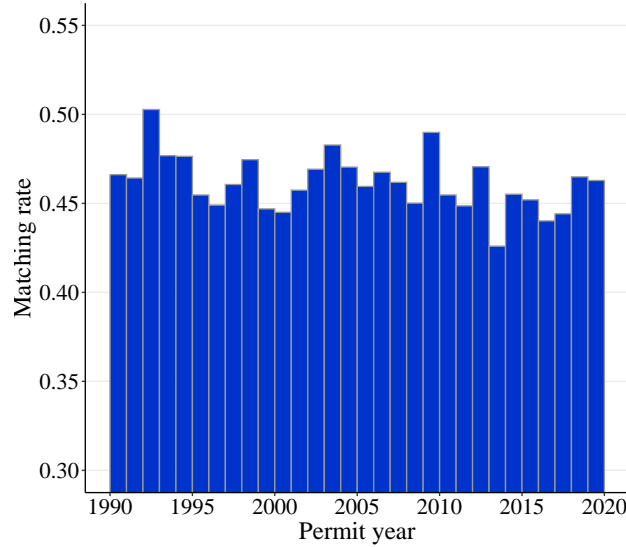
Step 5. Linking Zillow Assessment and Transaction Data to Infutor Individuals. We follow a similar approach to Step 4. We link data on 40B non-beneficiary individuals to tax assessment data in 2020 and residential transactions since 1987 based on first and last name, street number, street name and municipality in Massachusetts. Tax assessment records allows us to detect individuals that are homeowners in 2020, while residential transactions allow us to keep track of who used to be an owner before 2020 (using buyers' names) and when they become an owner (using sellers' names, if they bought a property after 1986). We also use the `fastLink` package in R and restrict comparisons to pairs of individuals who have last names starting with the same letter.

We are able to match 822,721 unique non-beneficiary individuals who own a property in 2020, 944,579 who bought a property before 2020, and 785,345 who sold a property before 2020. In total, these numbers capture 1,605,454 unique individuals. When we focus on the final sample of *all local residents* in Step 3, 21% of them show up in tax assessment records (529,861), 24% bought a home after 1986 (593,670) and 19% sold a home after 1986 (465,508). In total, these numbers cover 35% (870,242) of unique local residents.

When we focus on *incumbent residents*, 30% of them show up in tax assessment records (183,936), 28% bought a home after 1986 (171,466) and 28% sold a home after 1986 (170,672). In total, these numbers cover 55% (334,855) of unique incumbent residents. As in the previous step, we test how

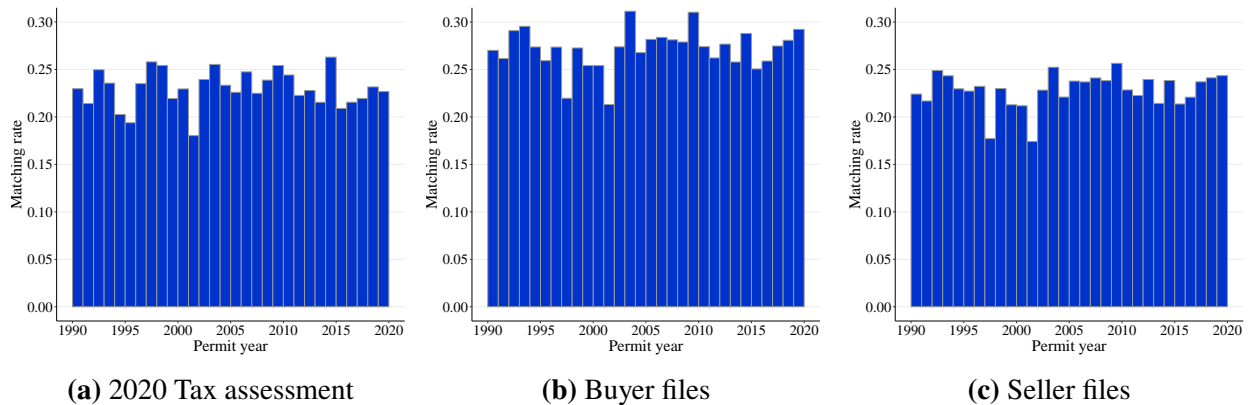
consistent our matching rates are for 40B non-beneficiary individuals throughout our period in Figure A.2. The figure shows the share of residents that we can identify in each of the three files mentioned above. Matching rates are consistent across the three files and across all periods.

Figure A.1: Histogram of Infutor-voter file matching rates 40B permit approval



Note: The plot shows the average matching rate defined as the percentage of incumbent residents in Infutor who are present in the MA voter file by permit approval year.

Figure A.2: Histogram of Infutor-Zillow files matching rates 40B permit approval



Note: The plots show the average matching rate defined as the percentage of incumbent residents in Infutor who are present in three different Zillow datasets by permit approval year.

A.2 Local Elections

The first step to study the effect of Chapter 40B on local election outcomes is to build a dataset containing the list of dates when local elections were held in all 292 towns and 59 cities in Mas-

sachusetts. This task is challenging for two reasons. First, there is no such dataset that is publicly available. Second, the type and frequency of local elections held by towns and cities differ.²² For instance, most towns have town meetings, which is a term used both for an event that gathers all eligible voters and for the legislative body of a town. In contrast, cities are governed by the city council or a board of aldermen, i.e., citizens do not directly govern.

The paragraphs below describe how we overcome the issues above and how we build the main analysis sample to study turnout in local elections.

Identifying Local Election Dates. We adopt a data-driven approach to identify local election dates. L2 data's information on local election turnout contains the list of individuals who *actually* voted in a local election starting in 2008. That is, we can observe the individuals who vote and the dates when they voted. Since L2 data also includes their addresses from 2012 to 2019, we can also infer the municipality where these individuals voted.²³

Using this information—and after standardizing municipality names as in Step 1 of the previous section—we generated a dataset that contains a list of municipality-election date pairs along with the number of individuals who show up as having voted in the corresponding municipality and election date. This process yields 34,906 municipality-election date pairs covering 347 municipalities. We further restrict this dataset as follows. First, we drop all municipality-election date pairs with less than 100 voters, which gets rid of irrelevant dates due to mistakes in allocating the right municipality where voters are actually voting in. Second, we drop municipality-election date pairs where the turnout was less than 25 percent of the turnout in the election date with the highest turnout within the same municipality. This second restriction should get rid of smaller elections that should not involve participation numbers that compare to most important local elections. We are left with 3,813 municipality-date pairs for 334 municipalities in the period 2008-2019.

To make sure that the identified election dates capture relevant local elections, we manually checked the nature of a subset of these elections. In the case of towns with town meetings, elections dates mostly refer to annual and special town meetings. In the case of cities, election dates mostly refer to mayoral and at-large elections.

Building the Final Sample of Residents for Local Elections. We start by obtaining the sample of 40B developments that were approved between 2010 and 2017. We impose this restriction because turnout data for local elections only starts in 2008. For this sample of 40Bs, we match voter records on local elections with the two Infutor datasets of non-beneficiary residents living within 0.6 miles

²²By law, municipalities under 12,000 inhabitants cannot adopt a city form of government.

²³For years 2008-2011, we assume that the individual voted in the municipality corresponding to the first ever observed address for that individual.

of these 40B developments described in the previous section (*all local residents and incumbent residents*).

To do so, we follow the same steps as in Step 4 of that section with two differences. First, we restrict the Infutor sample to individual-address pairs that correspond to the municipality from which the 40B development they live near is located—hence, individual-address pairs outside of Massachusetts are dropped. Second, we do *not* restrict the sample to Infutor individuals who we are able to match to voter records and that were registered to vote in year four or five before the 40B approval: our sample contains any individual who lives within 0.6 miles of the 40B in the municipality where the 40B is located. These restrictions are motivated by the fact that we do not observe the municipality where individuals are registered to vote. Applying the restrictions, we end up with a sample of 153,805 local residents and 94,106 incumbent residents.

A.3 Identification of 40B Occupancy Dates

Our main dataset has limited information regarding the completion dates for developments permitted under 40B. This section explores the timing of completion by focusing on the sample of 40B developments that were approved between 1995 and 2012, which may have plausibly been completed by the end of our sample period. One option is using the date when the occupancy permit is issued as reported in the Subsidized Housing Inventory (SHI) as a proxy for completion. However, only 264 out of these 733 developments (36 percent) have a non-missing value for occupancy dates. Panel (a) of Figure A.3 shows a histogram of the difference between the reported occupancy permit date and the 40B permit approval date. On average, 40B buildings the occupancy permit is approved around 3.9 years after the 40B permit is approved.

Given the incomplete data on occupancy dates, we also use Infutor data to better understand the timing of construction. Next, we describe the procedure to proxy occupancy dates from observed population inflows into 40B developments.

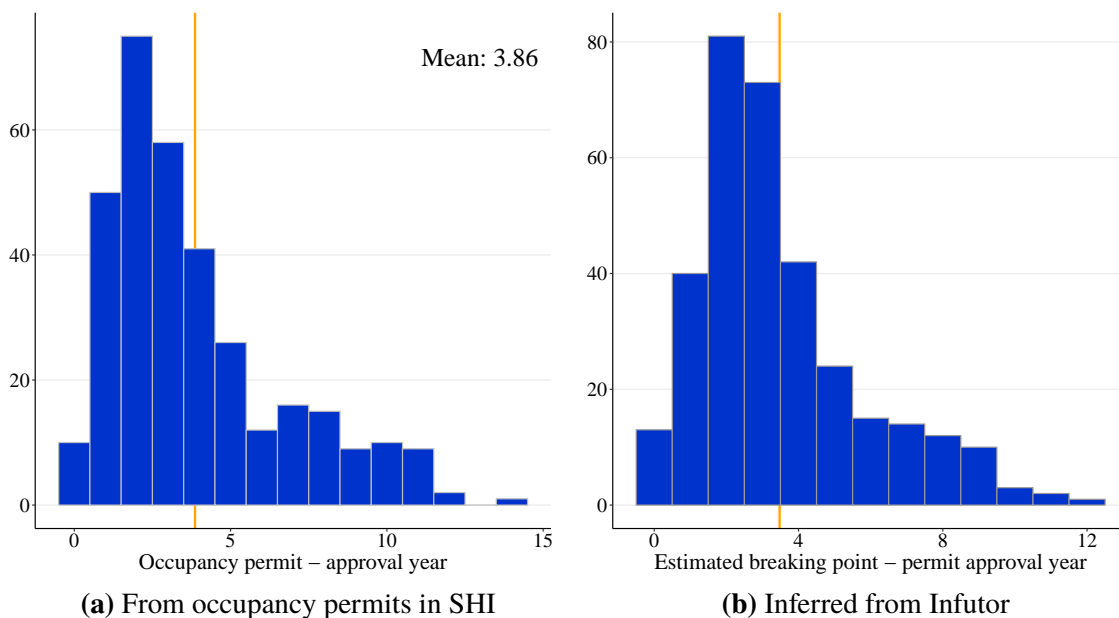
We identify all individuals who ever lived in a 40B development, as well as the time period during which they lived in that address, using Infutor data as described in the Step 2 of Section A.1. We restrict the sample to living spells that started in 1985 or later given that coverage in Infutor starts being meaningful around that year. Next, we create a dataset at the 40B development-year level indicating the number of individuals who we observe living in any 40B address belonging to that development in each year. In total, we find a positive number of individuals living in a 40B address for 684 developments approved between 1995 and 2012.

Next, we use a structural break detection algorithm to infer spikes in the number of individuals living in each 40B development. Intuitively, a discrete, big jump in the individual count of a development in a particular year should mean the start of occupancy. We follow several steps. First, we restrict the exercise to addresses in which we observe a minimum of 10 or more individuals to

reduce noise in our estimation. This step decreases the number of developments to 355. Second, we follow the structural break detection literature to identify the shift in population counts (Bai, 1994, 1997a,b; Bai and Perron, 1998, 2003; Zeileis et al., 2003) using the package `strucchange` in R. For each 40B development, we implement this method for the regression of its population count over time on a constant limiting the number of structural breaks to one. This technique yields the year before the population count first jumps; hence, we add one year to this number to obtain the (start of) occupancy year.

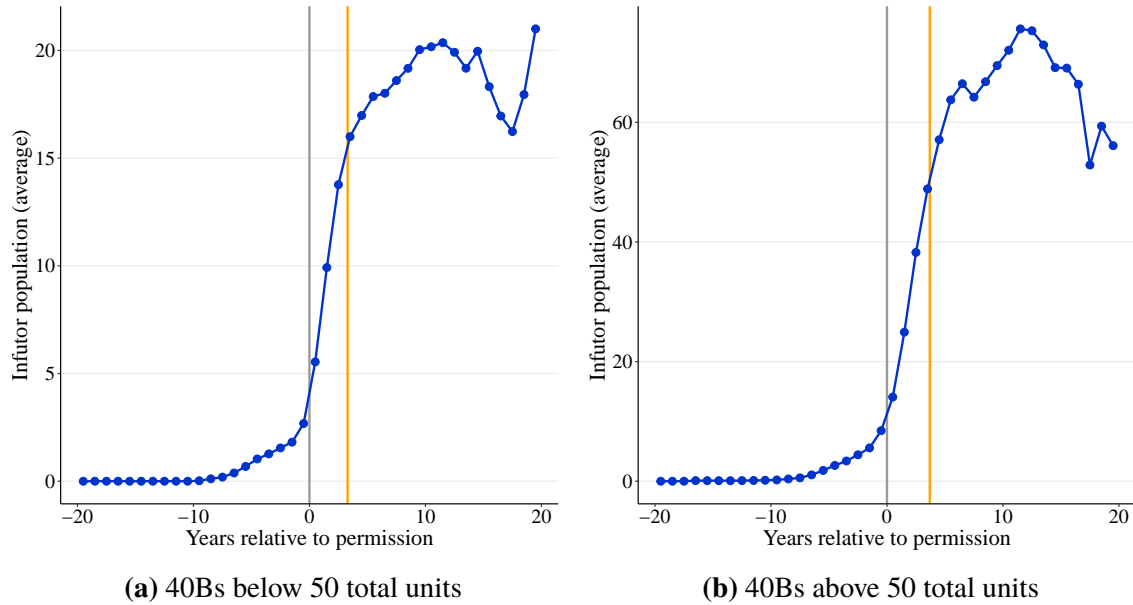
Using this method, we are able to estimate the start of occupancy year for 355 developments. However, the estimated occupancy year is lower than the 40B permit approval year for 25 developments. Panel (b) of Figure A.3 shows a histogram of the difference between our estimated occupancy year and 40B permit approval year for those with an estimated occupancy year greater than their approval year. There is some variation in the timing of occupancy: 40 percent are occupied within 2 years of approval and 75 percent within 4 years, while almost 20 percent take 6 or more years. Finally, Figure A.4 shows the evolution of Infutor population of the average 40B development for which we are able to apply the structural break algorithm around permit approval by development size. For small 40Bs, it takes an average of 3.29 years to go from approval to occupancy; for large 40Bs, it takes 3.69 years—closer to the average using occupancy permit dates from SHI.

Figure A.3: Histogram of estimated occupancy years relative to 40B permit approval



Note: The figure shows two histograms of the number of 40B developments by estimated occupancy year relative to 40B permit approval. Panel (a) uses occupancy permit dates from the SHI; panel (b) infers occupancy dates using population flows in Infutor. The vertical orange line indicates the mean. In panel (b), we drop 25 developments with an estimated breaking point taking place before permit approval year.

Figure A.4: Raw count of the average number of individuals living in a 40B development



Note: The plots show the average population count for 40B developments for which we are able to estimate occupancy years. The vertical gray line denotes the 40B permit approval year, the vertical orange line denotes the mean of the estimated occupancy year. Panel (a) focuses on the sample of 40Bs with less than 50 units, panel (b) reproduces the analysis for 40B developments with 50 units or more. We drop 19 developments with an estimated breaking point taking place before permit approval year.

A.4 Ownership near 40B Developments

This section provides a description of ownership around 40B developments and assesses the extent to which our imputed ownership measure is a good approximation. For this analysis, we use ownership rates in 2000 at the census block group level and limit our sample to 40B developments approved in 2000 or after. We impose this sample restriction to avoid counting 40B units towards ownership rates in the census.

Figure A.5 illustrates the variation in ownership rates in census block groups with 40B developments. Most areas surrounding 40Bs have high ownership rates. This finding is consistent with 40Bs being approved in areas restricted to single-family residences, which are more likely to be owned by their residents. However, there are still many other 40B areas with low ownership rates, as indicated by the histogram's long right-tail. Given the potentially large population of renters, our heterogeneity analyses based on ownership status are less sensitive to error in the matching process (i.e., non-owners are less likely to be non-matched owners).

Next, we compare our imputed ownership measure to census ownership rates. Our measure is obtained by linking Infutur individuals that we observe living in 2000 in census block groups with 40Bs to Zillow tax assessment and transaction records. In particular, we consider that an individual is an 'owner' if they either (1) show up in 2020 tax assessment data and they did not buy the property

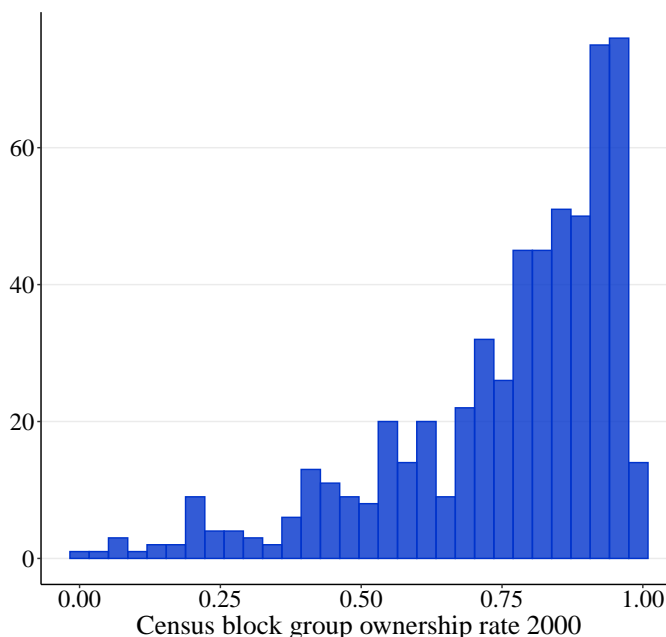
after 2000, or (2) do not show up in 2020 tax assessment data and they sold the property after 2000. Then, we compute the average ownership rates for each block group.

Note that there are two main sources of error in our imputed measure—i.e., reasons why we would not be able to identify an individual as an owner. First, Infutor coverage is not complete. Diamond et al. (2019), which introduced this dataset to the economics literature, reports that Infutor has a 44% coverage in 1990 and increases to explain 90% of census tract variation in 2000 for the city of San Francisco. Second, households are usually composed of multiple adult individuals (Infutor only covers adults) but it is possible that only one of them is the owner of the household. In that case, individuals living in the same unit as an owner would count as non-owners.

Figure A.6 plots our imputed measure against the census measure. As expected, imputed rates are much lower than census ownership rates. However, there is a strong positive relationship between measures across block groups with 40Bs, suggesting that our fuzzy matching algorithm linking Infutor to Zillow records is a reasonable proxy for ownership status.

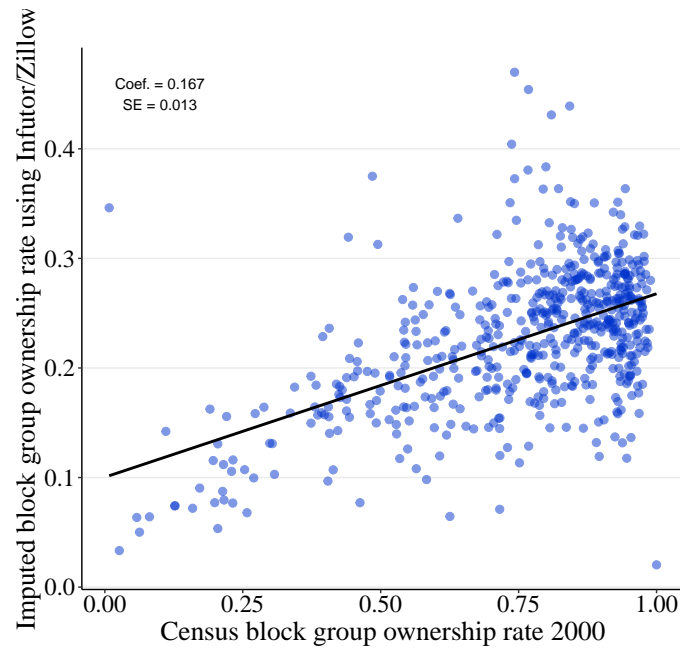
Finally, Figure A.7 uses imputed ownership status to calculate ownership rates for the rings used in our empirical specifications. The estimated ownership rates are around 23 percent beyond 0.2 miles away and seem to be slightly lower in areas closer to 40B developments, reaching a 20 percent ownership rate within 0.1 miles. The fact that we do not observe sharp changes across distances from 40Bs suggests that our heterogeneity estimates are not substantially biased by measurement error in closer distances.

Figure A.5: Histogram of 2000 ownership rates in census block groups with 40Bs



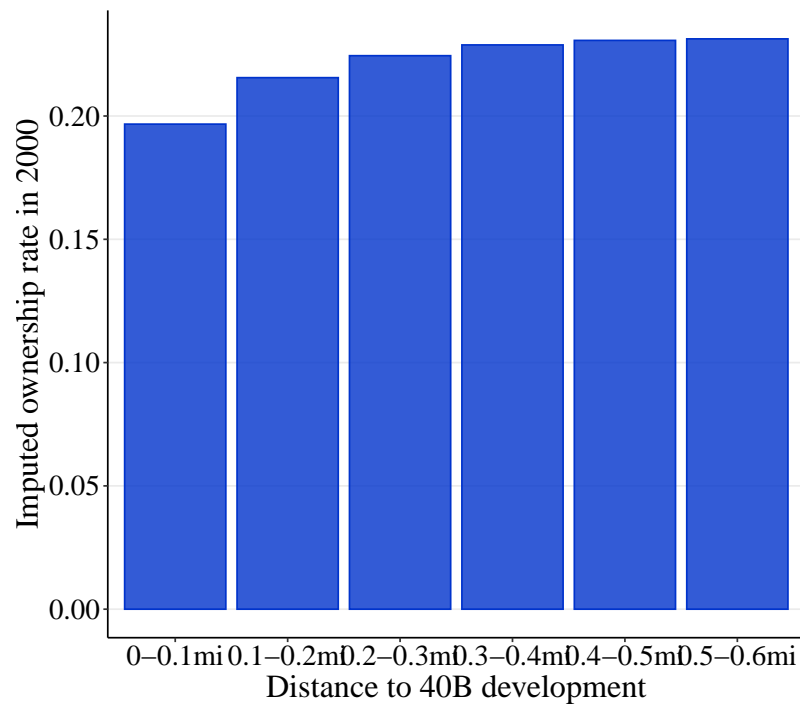
Note: The plot shows a histogram of ownership rates in 2000. The sample is census block groups with any 40B units that were approved in 2000 or after.

Figure A.6: Comparison of ownership rates: imputation (from Infutor/Zillow) vs census



Note: The figure is a scatter plot where every dot represents a 2000 census block group with any 40B units that were approved in 2000 or after. The y-axis is our imputed ownership rate calculated by matching Infutor individuals to Zillow's tax assessment and transaction records. The x-axis is the ownership rate as reported by the census. The line represents an ordinary least squares fit of the relationship between two variables. The coefficient and standard error of the regression are shown in the top left corner.

Figure A.7: Ownership rates by ring



Note: The plot shows ownership rates by 0.1 mile-wide rings around 40B developments using our imputed ownership measure. The sample includes 40B developments that were approved in 2000 or after.

B Comparison to Effects of Non-40B Large Developments

We compare the impact of 40B developments on housing prices and residents' responses with that of developments with similar characteristics and in similar locations that were not permitted under Chapter 40B ('non-40Bs'). This comparison should be informative of the impact of bypassing zoning laws given that, presumably, comparable non-40B developments should have been built in accordance to local zoning restrictions. Consistent with our main findings, we will focus on developments with 50 or more units.

We identify large developments built outside the scope of the 40B program using tax assessment data from Zillow. First, we identify and exclude all 40B developments in tax assessment records by using the fuzzy matching described in Section A.1 based on street number, street name, street type, and city. We block matches based on an exact match on street numbers and city names. Second, we restrict the sample to properties with 50 or more units and that were built between 1995 and 2012. Third, we exclude any buildings located in Boston to make our sample more comparable to 40B developments given that Chapter 40B is not binding in Boston. We are able to identify 295 non-40B developments. Note that approval dates for non-40B developments and completion dates for non-40B developments are not available: a comparison of the exact timing of the effects between both development types is not possible. However, the comparison is still informative, since the price effects of 40Bs only show up in the long run, when they are likely completed.

Despite Boston is excluded from the sample, non-40B developments are still slightly different from 40B developments approved during the same period. Figure B.1 shows that both types of developments are concentrated in the Greater Boston area. However, non-40Bs have a higher presence in municipalities closer to Boston, such as Cambridge, Somerville or Medford; while 40Bs have a greater presence in other areas such as Cape Cod. The first two columns of Table B.1 compare the two development types. Non-40Bs are slightly smaller and are located in census tracts that are denser, with lower white population and ownership shares, slightly higher housing prices and closer to the Central Business District (CBD). Despite this, census tracts where the two development types are located are remarkably similar along other dimensions, such as demographic composition, education, unemployment, and income.

We estimate the effects of non-40B developments using inverse probability weighting to deal with the selection problem. More specifically, we run a logistic regression of an indicator variable for whether the development is a 40B as the outcome variable on the number of units and census tract characteristics. We then construct a propensity score \hat{p}_d for every non-40B development d . Column (3) of Table B.1 reports the same descriptive statistics for non-40Bs weighting each of them by $1/(1 - \hat{p})$. That is, non-40Bs that are more similar to 40Bs are weighted more. As a result, the (weighted) average non-40B development is more similar to 40Bs, though some differences

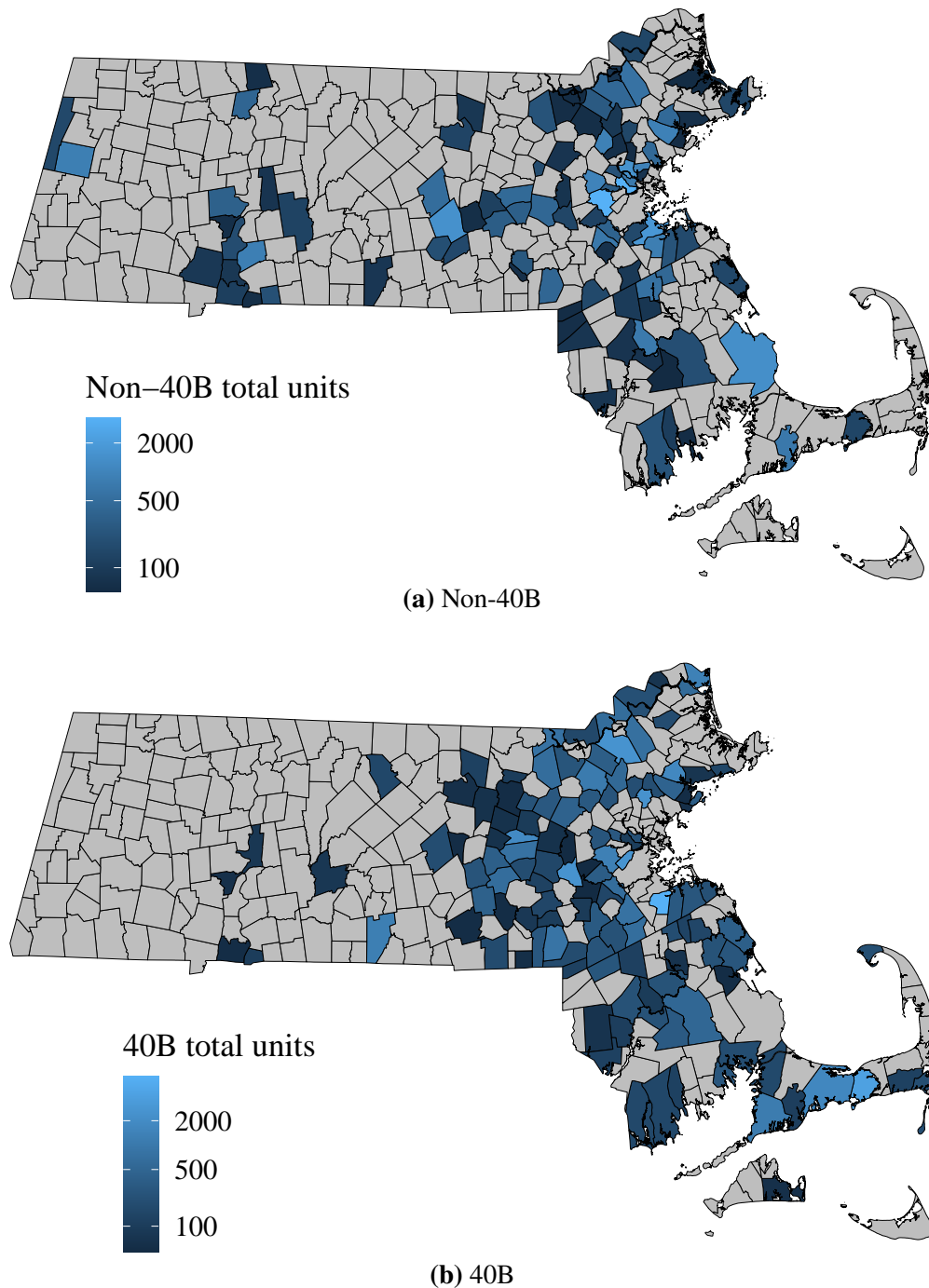
still persist. For instance, non-40B are still in denser census tracts and slightly closer to the CBD, consistent with the fact that they should be less likely to be in areas with stricter zoning laws.

Non-40B developments lead to negative effects on local housing prices beyond the distances where 40B lowers property values. To estimate these effects, we use the same methodology as in Section 3.3. Figure B.2 reports the event study and the pooled DID results for non-40Bs. The event study shows that sale prices temporarily go up around the completion year for preexisting housing located within 0.1 miles and then decrease by 20 percent in the long-run. This result must be interpreted with caution, given that pre-trends are relatively unstable—partly due to the fact that sale prices should react at the time when the building is approved and not when it is completed. However, there are no pre-treatment trends for distances beyond 0.1 miles. Overall, the pooled DID results show that sale prices go down by roughly 6-7 percent within 0.3 miles and by 3 percent within 0.3-0.4 miles after completion. There are no effects beyond 0.4 miles.

These findings contrast with the impact of large 40Bs on local housing prices. While 40B's negative price effects are restricted to the first 0.1 miles, non-40Bs negatively affects prices up to 0.4 miles away. Such result may be explained by a larger role of the supply effect in communities where non-40Bs is built, given that nearby existing housing may be more similar to units in the new building (as opposed to 40B neighborhoods, mostly composed of single-family residences). Another difference with our 40B findings is that non-40B developments seem to increase nearby sale prices in the very short run. A potential explanation is that non-40Bs may replace an eyesore, which is valued by local residents and prospective buyers, a result that has been previously linked to LIHTC developments Hankinson et al. (2022). Thus, the value of properties immediately adjacent to non-40Bs increases temporarily until the new units become available, when the new residents move in and the supply effect kicks in.

Non-40B developments trigger the outmigration of incumbent residents to a limited extent and does not generate responses in voter participation in general elections. Figure B.3 shows the event study results for move-out rates and voter turnout rates in general elections. Move-out rates increase within 0.1mi after completion, though the effect is smaller for the average local resident and more irregular for incumbent residents than that of 40Bs. Some of this variation may be due to the difference in the 'treatment event', given that in our 40B specification we use the approval year. Thus, the results for non-40Bs may be missing some of these effects. Furthermore, we do not observe a meaningful effect within 0.1-0.2mi for incumbent residents, even though 40Bs have a significant effect at these distances in the long run. These results suggest that move-out responses are not as strong as for 40Bs. This finding is consistent with a stronger negative preference of local residents for 40Bs in low-density neighborhoods. Finally, we do not observe a clear pattern of effects of non-40B developments on political behavior.

Figure B.1: Comparison of non-40B and 40B units in large developments between 1995 and 2012 by municipality (except Boston)



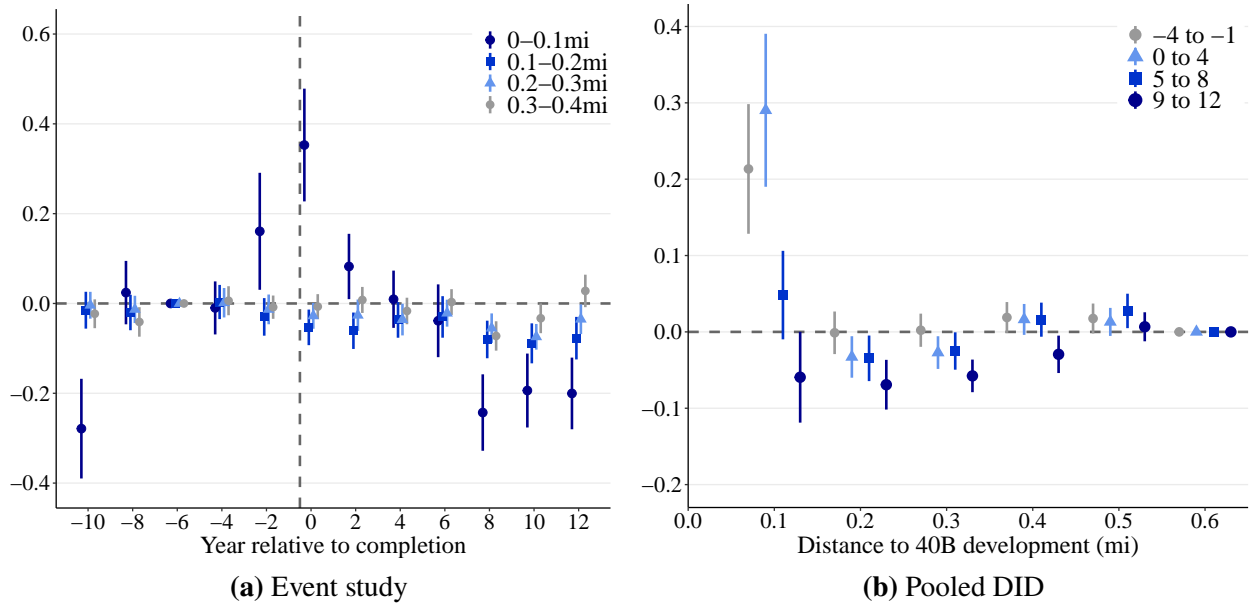
Note: The map shows the total number of units (includes both market and affordable units for 40B) in developments with 50 or more units by 40B status for each municipality. Panel (a) shows units completed in 1995-2012 without a 40B permit. We identify these developments using property tax assessments in the Zillow data. Panel (b) shows units approved under a 40B permit in the same period. We identify 40B units using the Subsidized Housing Inventory (SHI) from the Executive Office of Housing and Livable Communities (EOHLC) in Massachusetts. Each 40B address has been verified in the procedure detailed in Sportiche et al. (2025). Municipalities in gray did not approve any units except for Boston, which we exclude from our analysis. The legend coloring is log-transformed for visual purposes.

Table B.1: Summary statistics of non-40B developments

Characteristic	40B	No 40B	No 40B - IPW
<i>Panel A: Development</i>			
Total units	140	123	137
<i>Panel B: 2010 census tract</i>			
Population: total	5,530	5,273	5,379
Population: density (mi2)	2,027	3,810	2,989
Population: under 18	0.23	0.20	0.21
Population: 65 or more	0.15	0.17	0.16
Race: white	0.89	0.82	0.85
Race: black	0.025	0.04	0.035
Education: High school or less	0.07	0.07	0.07
Education: Bachelor or more	0.46	0.50	0.48
Unemployment rate	0.073	0.067	0.068
Household median income	89,746	86,663	86,491
Below poverty rate	0.06	0.08	0.07
Housing: owner-occupied	0.74	0.66	0.69
Housing: median year built	1,969	1,964	1,966
Housing: median rent	1,102	1,296	1,214
Housing: median home value	408,087	441,085	416,923
Distance to CBD (mi)	28	23	26
Number of developments	216	295	295

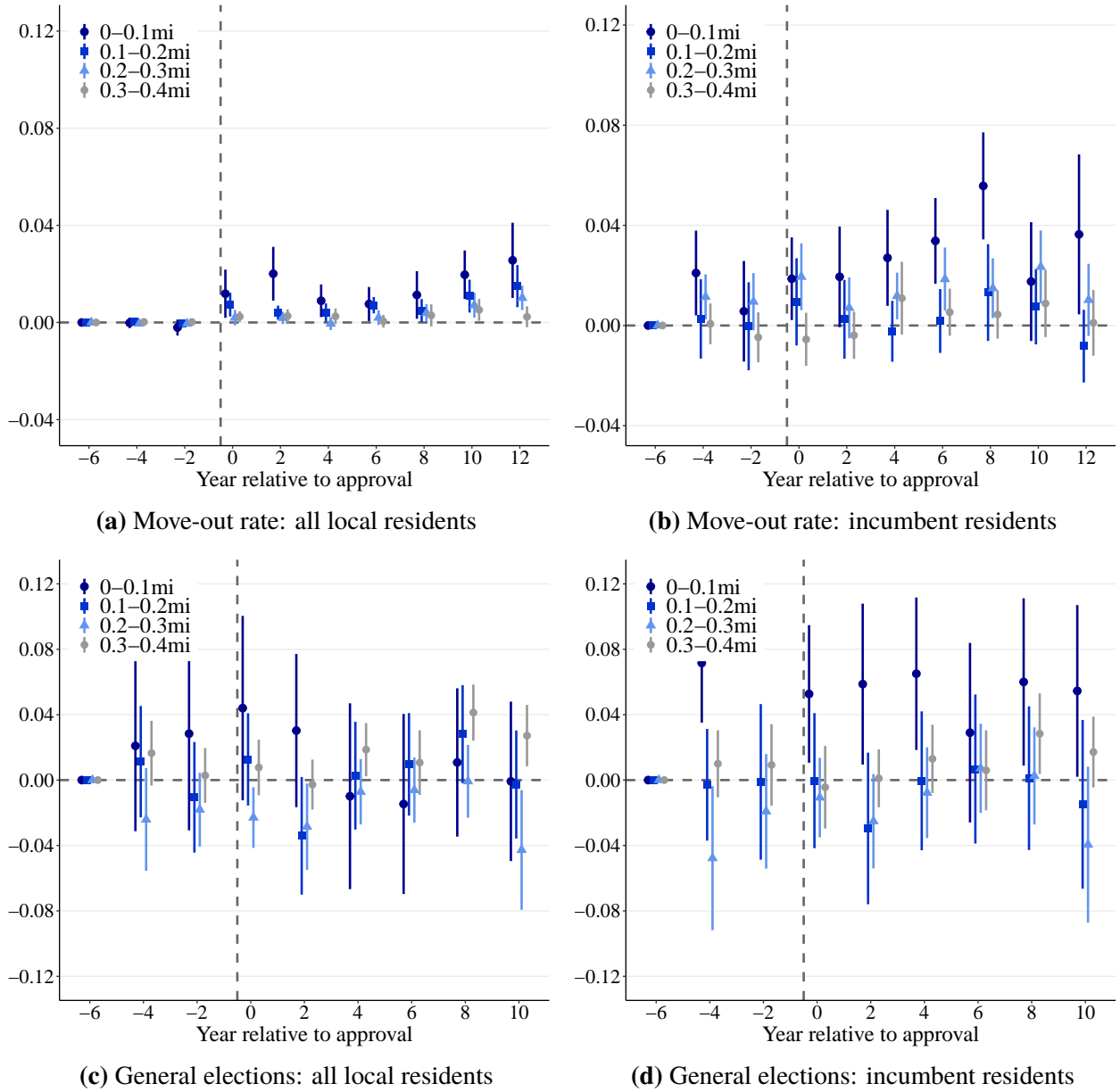
Note: Column (1) provides summary statistics for 40B developments with 50 or more units approved between 1995 and 2012, while columns (2) and (3) do the same for other non-40B developments with 50 or more units built anywhere in Massachusetts except for Boston. Column (3) weights each non-40B development by their propensity score as described in the text above. Panel A uses data from the Subsidized Housing Inventory processed as described in Sportiche et al. (2025) for column (1) and from property tax assessments in Zillow data for columns (2) and (3). Panel B uses 2010 census data.

Figure B.2: DID: sale prices of preexisting housing for large non-40Bs developments



Note: The figures plot estimated coefficients $\beta_{\tau,r}$ in Eq. (1) (panel a) and $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ (including one extra period, panel b) in Equation (2) using 0.1 mile-wide rings and the logarithm of sale prices as the outcome. The sample includes non-40B developments of 50 or more units that were completed in 1995-2012 and sales of old houses.

Figure B.3: Event study: move-out and turnout rates for large non-40B developments



Note: The figures show estimated coefficients $\beta_{\tau,r}$ in Eq. (1) using 0.1 mile-wide rings. The outcome variables are indicators of whether the individual moved out from the address neighboring a 40B (panels a and b) and whether they voted in a general election (panels c and d). We restrict the analysis to non-40B developments completed in 1995-2012 in panels (a) and (b) and in 2000-2012 in panels (c) and (d). Panels (a) and (c) run the analyses for the sample of all local residents, while panels (b) and (d) use the sample of incumbent residents of non-40B developments.

C Heterogeneity of the Price Effects of Chapter 40B

This section presents heterogeneity analyses of the price effects of 40B developments.

We start by focusing on *larger 40B developments*. There are several potential explanations for the negative price effect near larger 40B developments found on Section 4. One potential explanation is that by adding new units, 40B developments shift the housing supply curve outwards, which should put a downward pressure on prices. This supply channel may explain the highly localized results if there is strong market segmentation and new 40B housing is a close substitute to nearby housing, otherwise any supply effect should dissipate across the entire housing market (Kristof, 1965; Sweeney, 1974; Braid, 1981). 40B developments may also affect housing demand by introducing a disamenity. For example, nearby residents may view 40B housing as a negative physical externality (e.g., loss of ‘neighborhood character’) or they may negatively value the increase in low- and moderate-income residents moving into the new 40B units. Other potential explanations such as negative effects on the provision of public goods (e.g., congestion in public schools or traffic) are less plausible because the effect of larger 40B developments on prices that we estimate are highly localized rather than affecting the entire municipality.

To explore the relative importance of these supply and disamenity mechanisms, we conduct several sets of heterogeneity analyses based on development characteristics—such as tenure, monitoring agency, development size—and characteristics of the surrounding neighborhood such as local zoning and median household income. To do so, we begin by splitting our sample of larger 40B developments in two, converting any non-binary variables into indicators denoting whether a 40B development is above or below the median value in the sample. Table C.1 reports the results of this exercise.

Columns 1 and 2 in these results show that negative effects on property values are greater among ownership 40Bs. When compared to rentals, 40B ownership developments generate reductions in local sale prices that are roughly 3 percentage points larger for preexisting housing located within 0.1 miles of 40B housing. 40B ownership developments also decrease sale prices by 5 percent within 0.1-0.2 miles compared to rentals, which do not significantly affect prices at that distance. Since ownership 40B units compete directly with nearby for-sale houses, we interpret these results as suggestive evidence that the supply effect may be relevant in explaining negative effects.

We also find suggestive evidence that demand-side mechanisms play a role in our findings. Columns 5 and 6 show that the largest 40B developments (i.e., with 96 or more units) lead to larger price reductions within 0.1 miles than smaller large developments (i.e., between 50 and 95 units). These larger effect may be due either to a physical externality effect or to the more intense exposure to 40B residents. Two results suggest that the ‘neighborhood character’ argument may be an important factor explaining the results. In columns 7 and 8, we find that 40B developments

placed in single-family zoned areas concentrate most of the price decreases within 0.2 miles (12.4 and 4.6 percent decrease within 0.1 and 0.1-0.2 miles, respectively). In addition, the results from columns 11 and 12 showing that price reductions only affect the sale of single-family housing further supports this argument. Because we expect condos to be closer substitutes to new market-rate 40B units (and thus, the supply effect should be larger), these results for single-family houses indicate that households searching for single-family housing may negatively discount locations near such large developments, potentially due to the loss of neighborhood character or higher exposure to low- and moderate-income households.

Finally, we also explore the sources of the positive price effects associated with smaller 40B developments, as shown in Appendix Figure F.7. Table C.2 presents a heterogeneity analysis similar to the one above for this sample of smaller 40B developments. These results show that 40B developments with fewer units than the median smaller development (fewer than 12 units) and those built in areas not zoned for single-family residences largest actually lead to increases in sale prices for nearby housing beginning in years 5 to 8.²⁴ These results suggest that smaller 40B developments may generate positive spillovers, for example by replacing deteriorated buildings or vacant lots that previously imposed negative externalities on local residents. As prior literature shows, replacing these blighted spaces with new housing can raise nearby property values (Schwartz et al., 2006; Hankinson et al., 2022). Additionally, 40B developments may attract complementary public investments, such as repaved streets or new green spaces, which can further improve the neighborhood's appeal.

²⁴While most developments are in single-family zoned areas, about 5 percent of this sample are built in industrial areas.

Table C.1: Heterogeneity of price effects

Heterogeneity var.	Tenure		Agency		Development size		40B Zoning		Household income		Sale type	
	Ownership	Rental	MassHous.	Others	<median	>median	Other	SFR	<median	>median	Condo	SFR
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Yrs. 0-4: 0-0.1mi	-0.022 (0.038)	0.043 (0.027)	0.061* (0.034)	-0.026 (0.034)	-0.034 (0.030)	0.097** (0.043)	0.002 (0.035)	0.012 (0.033)	0.027 (0.029)	-0.020 (0.042)	0.008 (0.026)	-0.004 (0.033)
Yrs. 0-4: 0.1-0.2mi	-0.034 (0.022)	0.011 (0.016)	-0.025 (0.020)	0.005 (0.018)	-0.011 (0.017)	-0.003 (0.021)	0.013 (0.019)	-0.023 (0.018)	-0.006 (0.019)	-0.010 (0.018)	0.018 (0.019)	-0.009 (0.016)
Yrs. 5-8: 0-0.1mi	-0.021 (0.034)	0.005 (0.033)	-0.032 (0.043)	0.017 (0.029)	-0.012 (0.027)	0.008 (0.048)	0.011 (0.040)	-0.018 (0.029)	0.006 (0.037)	-0.018 (0.030)	0.024 (0.039)	0.002 (0.033)
Yrs. 5-8: 0.1-0.2mi	-0.012 (0.023)	-0.008 (0.021)	0.002 (0.023)	-0.018 (0.021)	-0.013 (0.021)	-0.009 (0.023)	-0.021 (0.023)	-0.001 (0.021)	-0.009 (0.023)	-0.014 (0.021)	0.025 (0.032)	-0.021 (0.018)
Yrs. 9-12: 0-0.1mi	-0.104** (0.052)	-0.075** (0.035)	-0.093** (0.044)	-0.082* (0.045)	-0.067* (0.037)	-0.153*** (0.058)	-0.035 (0.044)	-0.124*** (0.043)	-0.099** (0.047)	-0.077* (0.043)	-0.010 (0.036)	-0.126* (0.067)
Yrs. 9-12: 0.1-0.2mi	-0.053* (0.028)	-0.020 (0.021)	-0.021 (0.023)	-0.045* (0.025)	-0.032 (0.023)	-0.040 (0.025)	-0.022 (0.029)	-0.046** (0.021)	-0.039 (0.025)	-0.030 (0.024)	0.033 (0.036)	-0.048** (0.022)
Developments	95	175	133	137	131	139	144	126	135	135	181	270
Observations	48,659	97,034	64,617	81,076	88,006	57,687	80,547	65,146	87,193	58,500	64,698	80,995
R ²	0.851	0.875	0.880	0.854	0.850	0.884	0.870	0.865	0.849	0.868	0.921	0.869

Note: The table reports estimates for coefficients $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings and the logarithm of house prices as an outcome. Each column uses a different sample depending on: tenure (ownership or rental 40Bs, columns 1-2), monitoring agency (MassHousing or other, columns 3-4), 40B development size (above or below the median in the sample, 5-6), whether the 40B development is in an area zoned as single-family residence (7-8), household income in the 40B's census tract in 1990 (above or below the median in the sample, 9-10), and whether the sold property is a condo or a SFR (11-12). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Heterogeneity of price effects

Heterogeneity var.	Tenure		Agency		Development size		40B SFR Zoning		40B Industrial		Household income		Sale type	
	Ownership (1)	Rental (2)	MassHous. (3)	Others (4)	<median (5)	>median (6)	Other (7)	SFR (8)	Other (9)	Industrial (10)	<median (11)	>median (12)	Condo (13)	SFR (14)
Yrs. 0-4: 0-0.1mi	0.005 (0.013)	0.011 (0.018)	0.023 (0.021)	0.0010 (0.013)	0.004 (0.015)	0.012 (0.017)	-0.002 (0.015)	0.007 (0.014)	0.007 (0.011)	0.010 (0.034)	-0.002 (0.017)	0.016 (0.014)	-0.013 (0.020)	0.0007 (0.014)
Yrs. 0-4: 0.1-0.2mi	-0.007 (0.009)	-0.013 (0.015)	-0.004 (0.016)	-0.009 (0.009)	-0.013 (0.011)	-0.002 (0.011)	-0.026* (0.013)	0.0008 (0.010)	-0.009 (0.008)	-0.0002 (0.029)	-0.014 (0.011)	-0.004 (0.011)	-0.011 (0.017)	-0.012 (0.009)
Yrs. 5-8: 0-0.1mi	0.0003 (0.014)	0.030 (0.028)	-0.0003 (0.024)	0.010 (0.015)	0.042*** (0.016)	-0.023 (0.019)	-0.010 (0.021)	0.012 (0.016)	0.007 (0.013)	0.081 (0.064)	-0.011 (0.020)	0.018 (0.017)	-0.004 (0.032)	0.018 (0.015)
Yrs. 5-8: 0.1-0.2mi	0.005 (0.011)	0.011 (0.018)	-0.014 (0.017)	0.014 (0.011)	0.017 (0.015)	-0.004 (0.012)	-0.014 (0.017)	0.014 (0.011)	0.005 (0.010)	0.028 (0.033)	-0.006 (0.013)	0.015 (0.013)	-0.029 (0.020)	0.002 (0.010)
Yrs. 9-12: 0-0.1mi	0.054*** (0.015)	0.051* (0.030)	0.042 (0.028)	0.061*** (0.015)	0.081*** (0.019)	0.033* (0.020)	0.080*** (0.021)	0.040** (0.017)	0.055*** (0.014)	0.078 (0.055)	0.056*** (0.019)	0.054*** (0.019)	-0.014 (0.028)	0.048*** (0.016)
Yrs. 9-12: 0.1-0.2mi	0.014 (0.011)	0.017 (0.021)	0.032* (0.016)	0.007 (0.012)	0.016 (0.015)	0.016 (0.013)	-0.009 (0.018)	0.026** (0.012)	0.012 (0.010)	0.059** (0.022)	-0.021 (0.014)	0.051*** (0.013)	-0.018 (0.024)	0.007 (0.010)
Developments	449	180	215	414	267	362	217	412	594	31	300	329	484	629
Observations	209,704	116,418	98,021	228,101	153,197	172,925	128,695	197,427	308,507	16,471	175,914	150,208	95,334	230,788
R ²	0.889	0.869	0.874	0.885	0.904	0.859	0.889	0.866	0.886	0.845	0.861	0.881	0.953	0.883

Note: The table reports estimates for coefficients $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings and the logarithm of house prices as an outcome. Each column uses a different sample depending on: tenure (ownership or rental 40Bs, columns 1-2), monitoring agency (MassHousing or other, columns 3-4), 40B development size (above or below the median in the sample, 5-6), whether the 40B development is in an area zoned as single-family residence (7-8), whether the 40B development is in an area zoned as industrial (9-10), household income in the 40B's census tract in 1990 (above or below the median in the sample, 11-12), and whether the sold property is a condo or a SFR (13-14). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D The Aggregate Value of Chapter 40B

We estimate the housing value created by Chapter 40B and the property value losses borne by homes located near larger 40B developments using residential transactions and tax assessment data.

We compute the aggregate value of housing produced under Chapter 40B as follows. First, we identify 40B units in the 2020 tax assessment data as the sample of residential buildings located within 0.1 miles from 40B developments approved between 1995 and 2012 and constructed within twelve years of the approval year of the associated 40B development.²⁵ We limit the sample to developments approved by 2012 to ensure sufficient time for the 40B units to be constructed by the end of our sample. We identify at least one unit for 525 of the 733 40B developments approved in this period and further restrict the sample to developments for which we observe at least one-third of the units in the assessment records, yielding a final sample of 435 developments. Figure D.1 compares the number of 40B units identified in the assessment data with official unit counts for each 40B development. The close alignment with the 45-degree line suggests that our identification strategy is a reasonable proxy for actual 40B units.

Second, we estimate the market value of these units, acknowledging that assessment values may differ from market values. To do so, we generate year built-specific adjustment factors that convert assessment values to market values using transactions data on single-family residences. We start by converting sale prices in transaction data to 2019 dollars using FRED's All-Transactions House Price Index for Massachusetts.²⁶ Next, we regress sale prices in this dataset on their 2020 assessment value interacted with the year when the unit was built to obtain the built-year specific adjustment factors.²⁷ We apply these conversion factors to the assessment values of 40B units to obtain their estimated market value. For multifamily buildings, we divide the estimated building value by the number of units to obtain per-unit values.

Third, we estimate the aggregate value of Chapter 40B, shown in Table D.1. To do this, we compute the average market value of a 40B unit for each 40B development and then compute the average market value across all developments by tenure (rental, ownership, mix), weighting each development by their number of units. We then extrapolate this average market value by tenure to compute the aggregate value of two samples of 40B developments: all 40B developments (column 1) and those approved between 1995 and 2019 (column 2), which correspond to the main sample in our analysis. Columns 3 and 4 present the results for 40B developments with 50 or more units. The table shows aggregate values (in 2019 billions), average per-unit values, and the total number of 40B units in each sample.

²⁵A caveat is that this approach may include non-40B units that were built near 40Bs around the same time.

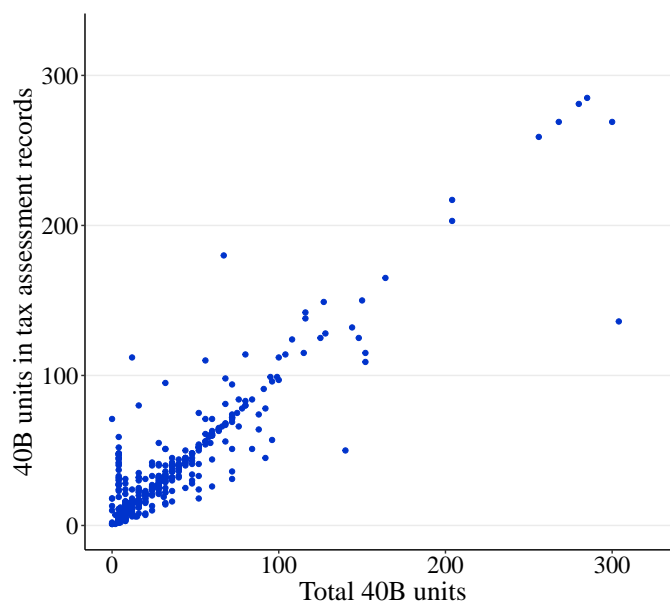
²⁶<https://fred.stlouisfed.org/series/MASTHPI>.

²⁷Properties built before 1899 are grouped into a single category due to small sample sizes, and we include a separate category for properties missing year-built information (1.7 percent of observations).

To estimate the property value losses for housing near larger 40B developments, columns 5–7 of Table D.1 focus on single-family residences located within 0.2 miles of such developments approved between 1995 and 2019. These are the properties for which we observe negative price effects, based on our heterogeneity analyses in Table C.1. As before, we use tax assessment records and apply our conversion methodology to estimate market values in 2019 dollars. We then multiply market values by the estimated price effects of larger 40B developments—12.6 percent for housing within 0.1 miles and 4.8 percent for housing between 0.1 and 0.2 miles—to calculate total property value losses. Column 5 reports the aggregate losses within 0.2 miles, while columns 6 and 7 separate the results for the 0.1 and 0.1–0.2 mile bands, respectively. The last row reports the number of affected single-family residences at each distance.

Overall, we estimate that Chapter 40B has produced approximately \$17.4 billion in housing value since its inception, with an average market value of \$248,055 per unit. Developments with 50 or more units, which represent 80 percent of 40B units, account for \$12.88 billion (74 percent) of this total. In contrast, the estimated property value losses for roughly 10,000 single-family residences located within 0.2 miles from these larger 40B developments approved between 1995 and 2019 total \$0.29 billion. These property value losses represent just 3.11 percent of the aggregate value generated by these larger 40B developments (\$9.31 billion).

Figure D.1: Comparison of 40B units in assessment data and official number of 40B units



Note: The x-axis represents the official number of units in a given 40B development as reported in the Subsidized Housing Inventory (SHI) and the y-axis shows the number of 40B units that we are able to identify in tax assessment records for that 40B development. The sample is restricted to 40B developments approved between 1995 and 2012 for which we find at least one third of the official number of 40B units in the development.

Table D.1: Aggregate value of Chapter 40B, by period and size

	Aggregate value of Chapter 40B housing				Property value losses		
	Full sample		50 units or more		50 units or more		
	All 40Bs	1995-2019	All 40Bs	1995-2019	0-0.2mi	0-0.1mi	0-0.2mi
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total value (in 2019 billions of dollars)	17.40	12.77	12.88	9.31	0.29	0.11	0.17
Per unit value/loss (in 2019 dollars)	248,055	253,386	231,180	234,483	30,143	56,422	23,039
Number of units	70,149	50,407	55,733	39,719	9,634	2,082	7,552

Note: Columns 1-4 report the estimated aggregate value of 40B housing by sample (first two columns, full sample; last two columns, 40B developments with 50 units or more) and by approval period (in odd columns, all periods since the inception of the program; in even columns, 40B developments approved between 1995 and 2019). Columns 5-7 estimate property value losses for single-family residences near larger 40B developments approved between 1995 and 2019 by distance. The first two rows report the total value, which adds up the value across all units produced in the period, and the per-unit value added (columns 1-4) or lost (columns 5-7). The third row reports the total number of 40B units in Columns 1-4, and the total number of single-family residences within a given distance of the 40B as reported by tax assessment records in Columns 5-7.

E Robustness: An Alternative Estimator and an Alternative Identification Strategy

E.1 Estimator: Gardner (2022)

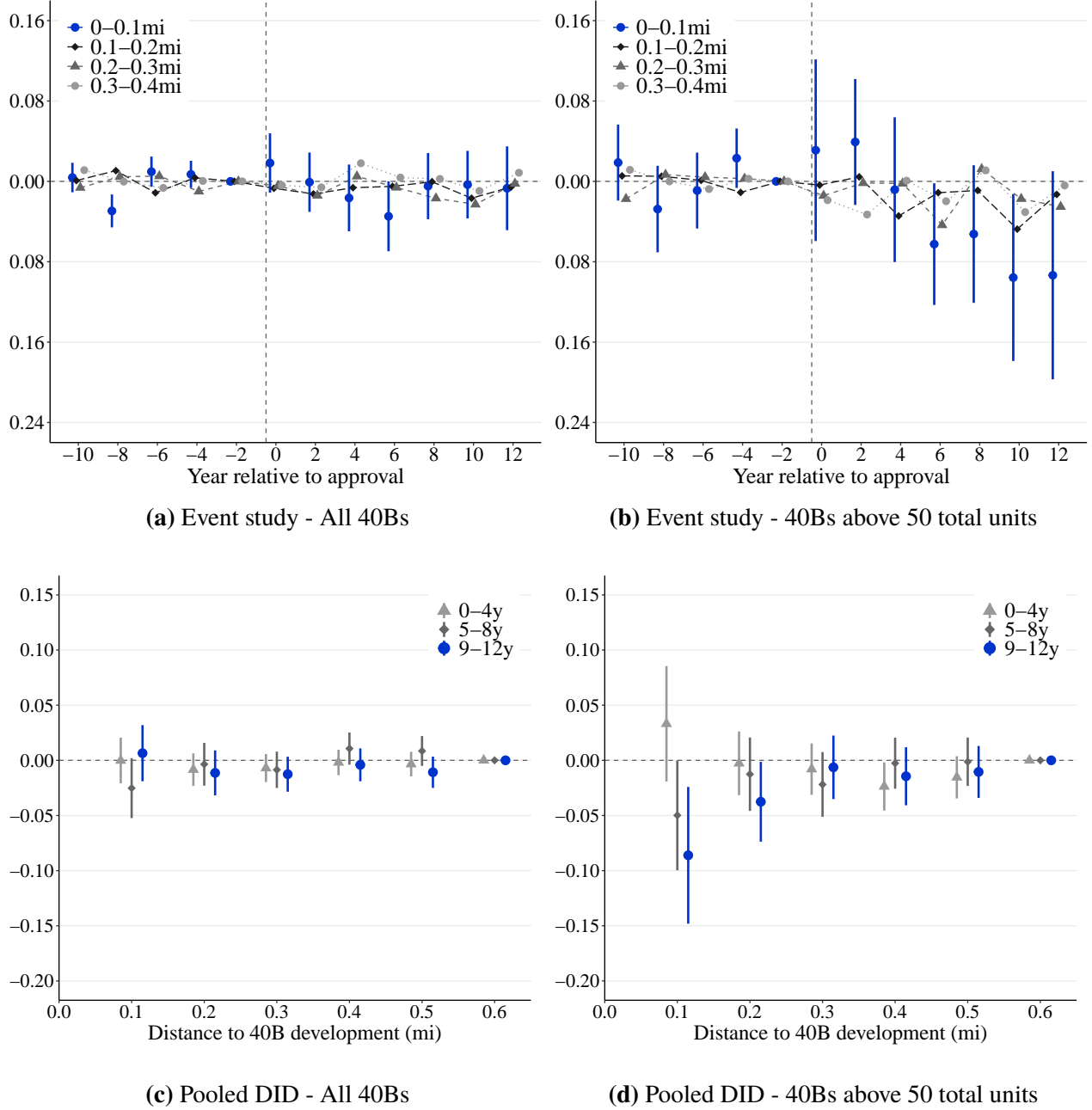
We estimate our main results using an alternative difference-in-differences estimation method proposed by Gardner (2022), which is also robust to treatment effect heterogeneity in staggered treatment adoption settings. This estimator proceeds in two steps. In the first step, it estimates group and time fixed effects using only untreated observations. In our case, we use observations belonging to either the pre-treatment period or the outermost ring as untreated observations. In the second step, the difference between the observed outcome and the predicted fixed effects is regressed on the treatment variables, which include interactions of event year-pair indicators with indicator variables for each treated ring. Importantly, this estimator allows us to accommodate several treatments (inner rings) in the same specification.

More specifically, we estimate the following regression using the R package `did2s`:

$$Y_{hdt} = \alpha_{dt} + \kappa_{r(h,d)} + \sum_{\tau=-10}^{12} \sum_{r \in R} \beta_{\tau,r} \mathbb{1}(t - E_d = \tau, r(h, d) = r) + \gamma' \mathbf{X}_{ht} + \epsilon_{hdt} \quad (4)$$

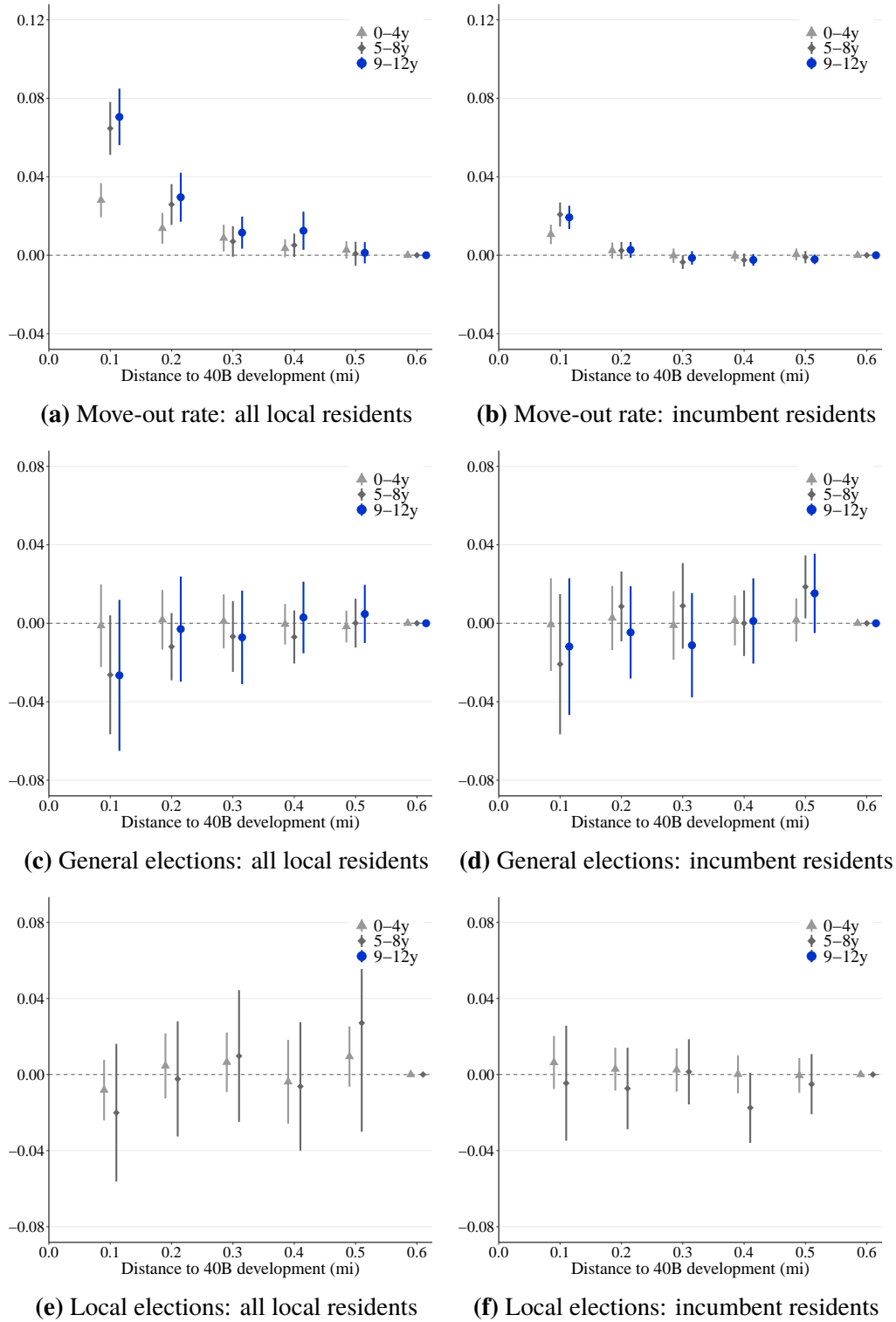
We weight every 40B development-ring-year combination equally, so that we can interpret the coefficients as the average of 40B development-specific treatment effects. Figures E.1 and E.2 show that our main results are robust to this alternative estimator.

Figure E.1: DID estimates of the impact of 40Bs on sale prices following Gardner (2022)



Note: The figures plot the estimated coefficients $\beta_{\tau,r}$ in Equation 4 (panels a and b) and $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ (panels c and d) in Equation (2) using 0.1 mile-wide rings and the logarithm of sale prices as the outcome, following Gardner (2022). We restrict the sample to 40B developments approved in 1995-2019 and sales of preexisting houses. Panels (a) and (c) include the entire sample of 40Bs in this period; panels (b) and (d) include only 40Bs with a total of 50 units or more.

Figure E.2: DID estimates for move-out and turnout rates for large 40Bs following Gardner (2022)



Note: The figures show estimated coefficients $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ in the version of Equation (4) that is analogous to Equation (2) using 0.1 mile-wide rings, following Gardner (2022). The outcome variables are indicators of whether the individual moved out from the address neighboring a 40B (panels a and b) and whether they voted in a general/local election (panels c to f). We restrict the analysis to 40B developments approved in 1995-2019 in panels (a) and (b) and in 2000-2019 in panels (c) to (f). Panels (a), (c) and (e) run the analyses for the sample of all local residents, while panels (b), (d) and (f) use the sample of incumbent residents. All panels only include 40Bs with a total of 50 units or more.

E.2 Identification Strategy: Quasi-Random Timing

Following prior literature (Asquith et al., 2023; Blanco and Neri, 2025), we implement an alternative empirical strategy leveraging quasi-exogenous variation in the timing of 40B development. The identifying assumption is that areas very close to different 40B developments should be similar and the reason why some areas build 40B developments earlier than others is as good as random. This randomness could result from idiosyncratic factors such as negotiations between local governments and developers or land assembly, as argued by prior research.

This ‘timing’ approach is complementary to our main specification in Section 3.3—which uses proximity to 40B developments to choose the comparison group—, though it has several limitations. The proximity approach assumes parallel trends between the inner and the outer ring, which offers two main advantages: (1) it controls for time patterns in the micro-neighborhood surrounding each 40B development by leveraging variation in the evolution of outcomes between near and farther away areas, and (2) it usually contains a positive number of observations in the comparison group (areas between 0.8 and 1 mile away) for each 40B development-year combination due to the larger area covered. In contrast, the timing approach cannot control for time patterns around each development and it may lack a valid control group within short distances for some developments due to the low number of sales in such small areas.

When we test for the ‘quasi-exogenous timing’ assumption, we find that the timing of 40B development correlates with certain 40B characteristics. Developments that are rental, located in more educated, lower-rent tracts tend to be built later. We show this by regressing the year of 40B permit approval on several development and neighborhood characteristics, with results shown in Figure E.3. The fact that some estimates are statistically significant at the 95% level supports the choice of the proximity approach as our main specification. Nonetheless, we will still estimate the timing approach controlling for such time-invariant characteristics by including development fixed effects.

We implement the timing approach using a stacked approach, similar to our main specification. We create a separate dataset s for each 40B development approved between 1995 and 2012, pairing each treated 40B development (which has $\text{Treated}_{ds} = 1$) with similar 40B developments to be built after 2012 ($\text{Treated}_{ds} = 0$) that meet four conditions: (1) they are built at least two years later than the treated development, (2) they are not in the same municipality, (3) they are the same tenure type (rental/ownership), and (4) the number of units in the control development is within 20 percent of the treated development. Next, we estimate the following equation, *separately for each ring*, following Gardner (2022):

$$Y_{hds} = \omega_{ts} + \phi_{ds} + \sum_{\tau=-6}^{10} \beta_{\tau} \mathbb{1}(t - E_s = \tau) \times \text{Treated}_{ds} + \gamma' \mathbf{X}_{hds} + \varepsilon_{hds} \quad (5)$$

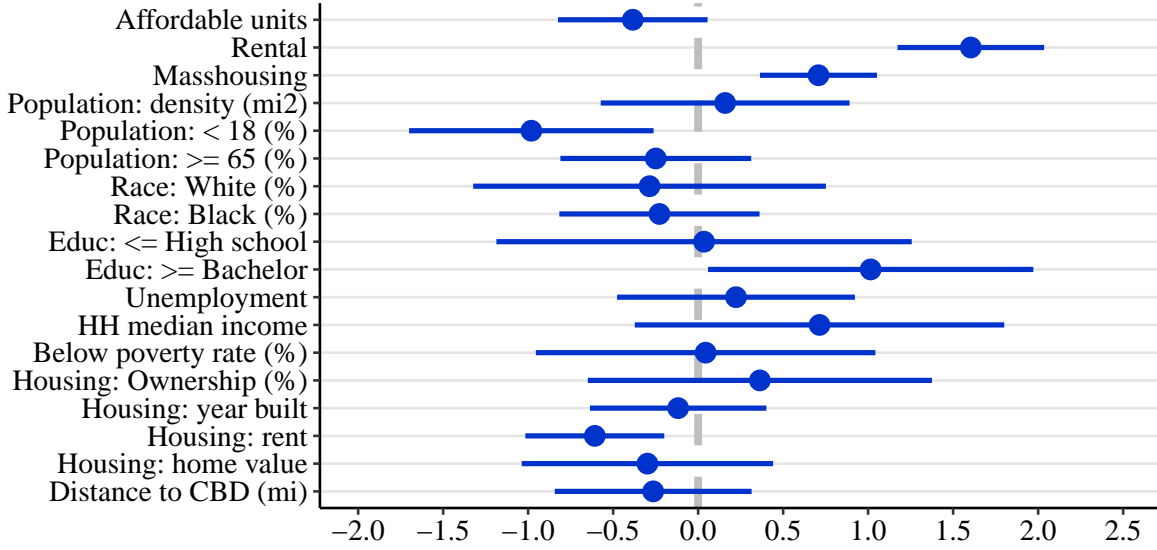
Here β_τ represents the effect of interest, i.e., evolution of the outcome for housing near current 40B developments compared to those near future 40B developments. ω_{ts} and ϕ_{ds} are calendar year-by-dataset and 40B development-by-dataset fixed effects, respectively. All fixed effects and control variables are interacted with indicator variables for each dataset s .²⁸ We weight each dataset-treated 40B development-year equally and each control 40B development equally within each dataset. Standard errors are clustered at the dataset level to account for 40B developments appearing in multiple comparison groups across datasets. In practice, we will estimate the pooled DID version that collapses relative years into three periods as in Equation (2).

Using this method, we obtain results that are consistent with our main specification. Figure E.4 plots the results for the pooled DID specification by ring. The average 40B development leads to moderate price decreases of about 4 percent within 0.1 miles (panel a). In contrast, there is a clear reduction in sale prices—of about 13 percent—for properties located within 0.1 miles of larger 40B developments in the long run (panel b), which is not present in farther away rings and aligns with our main results.

We also find similar results for move-out and voter turnout rates of residents around large 40B developments in Figure E.5. For these outcomes, we drop dataset-treated-year combinations with less than 10 observations to avoid over-weighting these combinations and obtaining noisier estimates. Effects on move-out rates within 0.2 miles align our main specification. However, estimates become unstable beyond 0.3 miles, possibly due to the identifying assumption being less plausible at these distances. Voter turnout is negatively affected but relatively constant within 0.6 miles, suggesting residents near earlier 40B developments experience a downward trend in voter turnout compared to those near later developments.

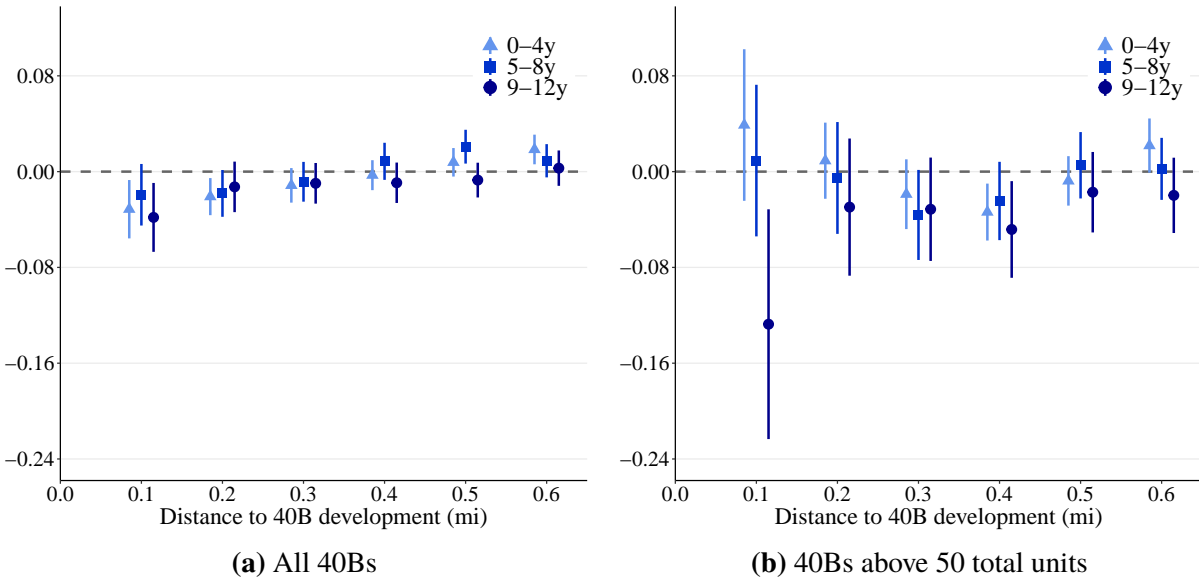
²⁸When using sale prices as the outcome, we cannot include calendar year-by-dataset fixed effects due to the low number of yearly sales in closer rings. Thus, we only include year fixed effects for this outcome. We also apply this strategy to voter turnout in general elections for incumbent residents, but only for the first ring.

Figure E.3: Coefficients of the regression of permit approval year on 40B characteristics



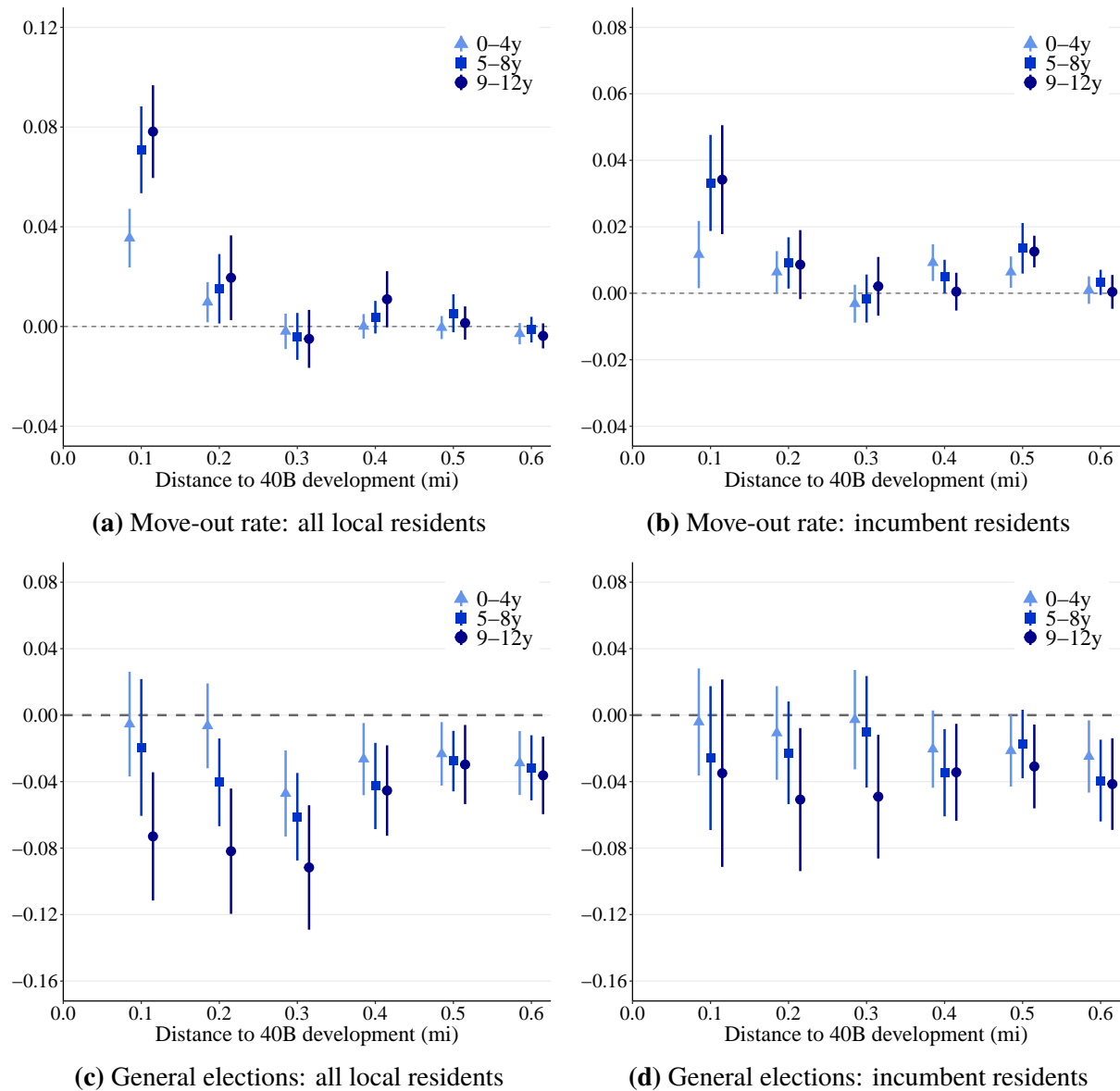
Note: The plot shows result of regressing the permit approval year on several 40B development and neighborhood characteristics. We use the sample of 40B developments approved between 1995 and 2019. All explanatory variables are standardized. Horizontal bars indicate 95% confidence intervals.

Figure E.4: DID estimates for sale prices of preexisting housing using quasi-exogenous timing



Note: The figures show estimated coefficients $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ in the version of Equation (5) that is analogous to Equation (2) using 0.1 mile-wide rings and the logarithm of sale prices as the outcome, following Gardner (2022). We restrict the analysis to 40B developments approved in 1995-2012 and sales of old houses. Panel (a) includes the entire sample of 40Bs; panel (b) includes only 40Bs with a total of 50 units or more.

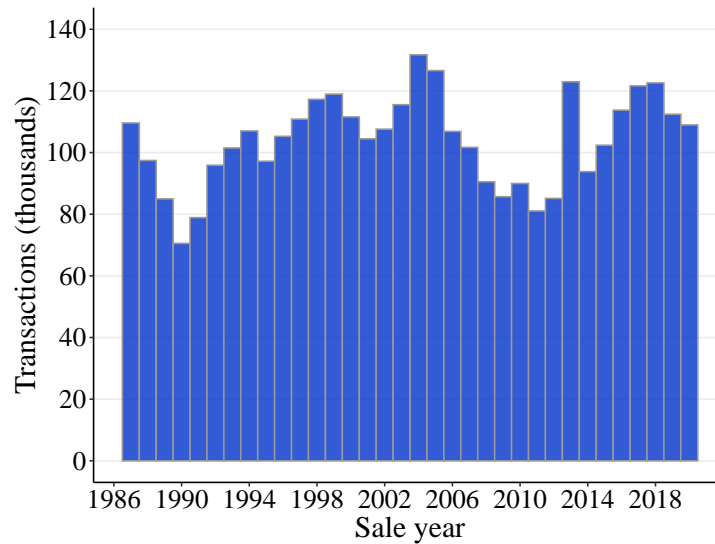
Figure E.5: DID estimates for move-out and turnout rates using quasi-exogenous timing for large 40Bs



Note: The figures show estimated coefficients $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ in the version of Equation (5) that is analogous to Equation (2) using 0.1 mile-wide rings, following Gardner (2022). The outcome variables are indicators of whether the individual moved out from the address neighboring a 40B (panels a and b) and whether they voted in a general election (panels c and d). We restrict the analysis to 40B developments approved in 1995-2012 in panels (a) and (b) and in 2000-2012 in panels (c) and (d). Panels (a) and (c) run the analyses for the sample of all local residents, while panels (b) and (d) use the sample of incumbent residents. All panels only include 40Bs with a total of 50 units or more.

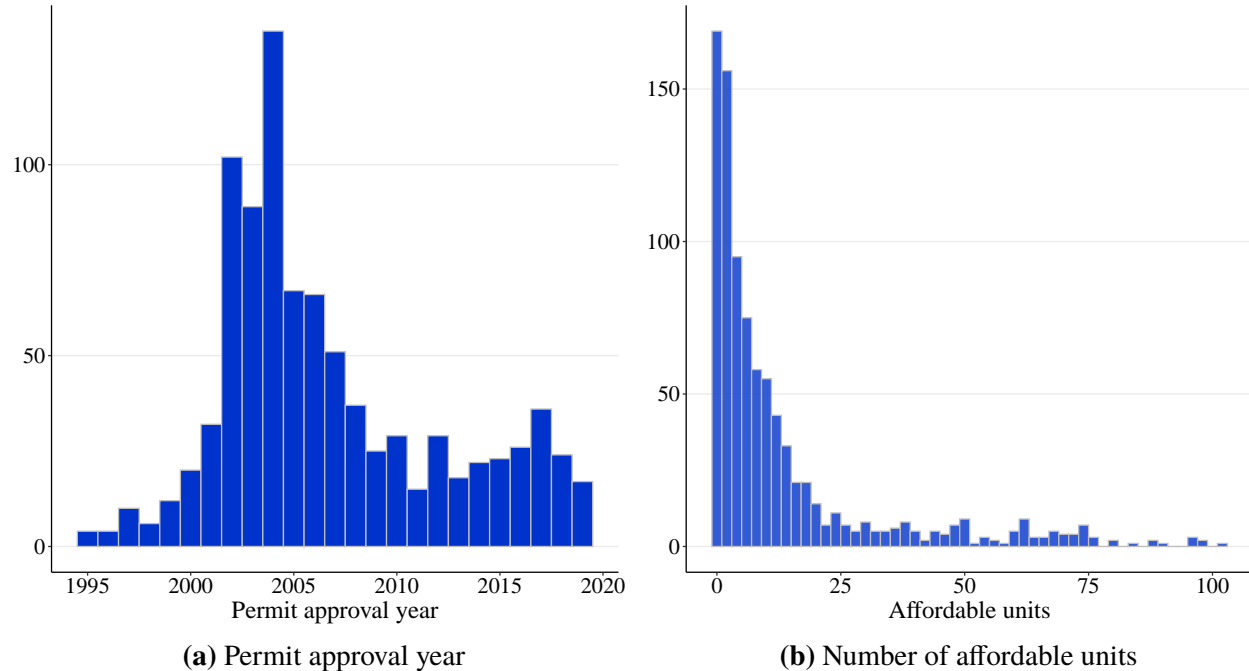
F Figures

Figure F.1: Histogram of Zillow ZTRAX transactions per year



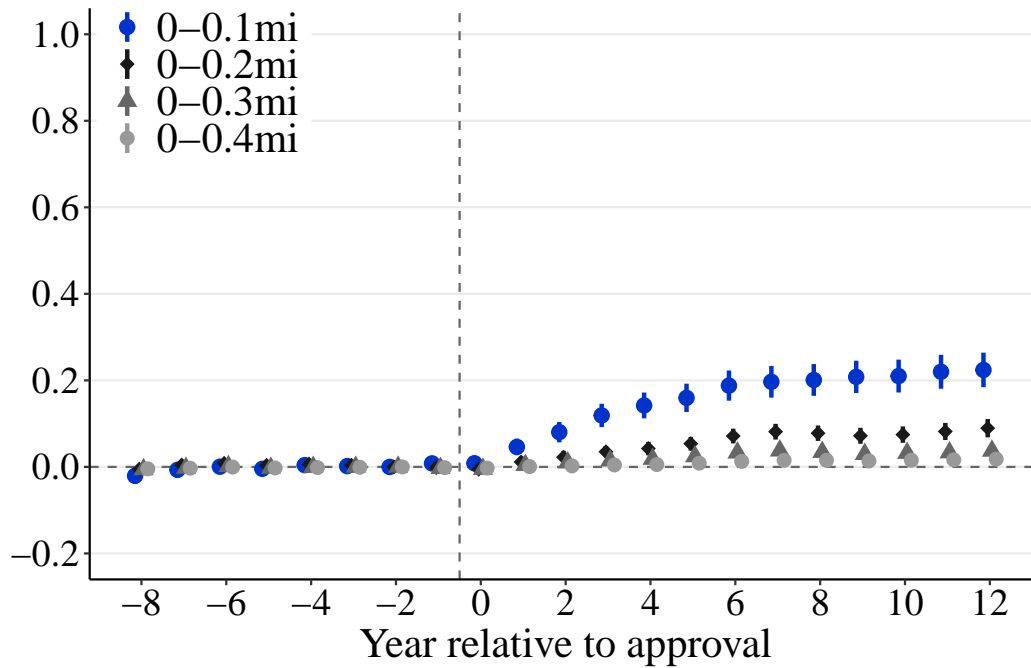
Note: The plot shows a histogram of the number of residential sales taking place each year between 1987 and 2020. Data was obtained from Zillow Ztrax.

Figure F.2: Histogram of permit approval year and number of affordable units

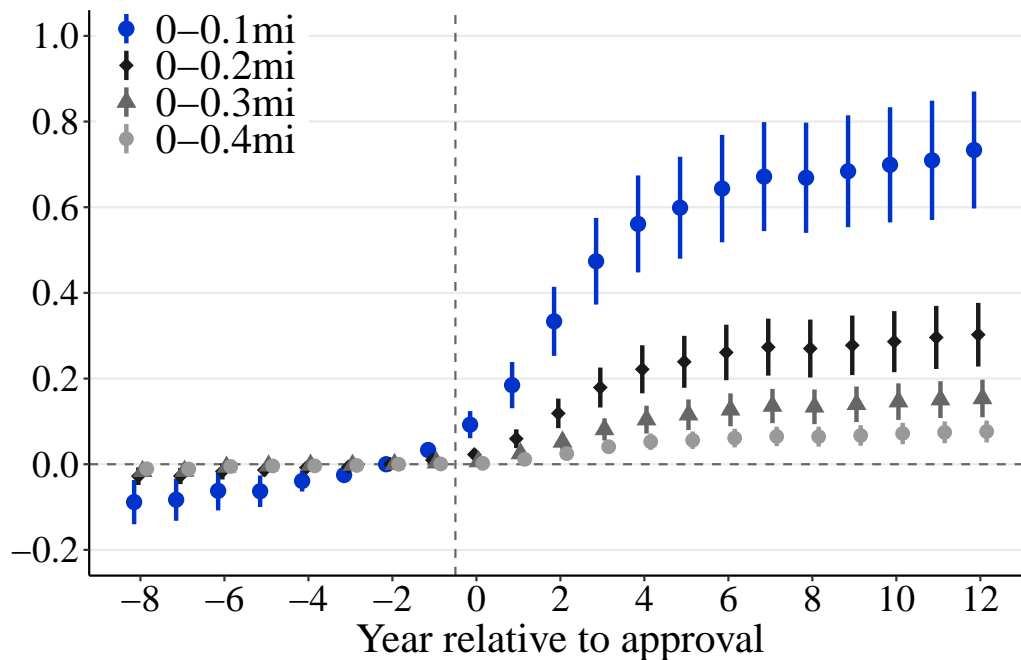


Note: Panel (a) plots the number of 40B developments by the year of permit approval. We define permit approval as the minimum of the following: Comprehensive 40B Permit year, building permit year, occupancy permit year, and year when 40B was added to the Subsidized Housing Inventory. Panel (b) plots a histogram of affordable units (25% of total units). In both panels, we use the sample of 40Bs approved between 1995 and 2019.

Figure F.3: Evolution of population density around 40B developments and characteristics of origin neighborhoods for residents of 40B units



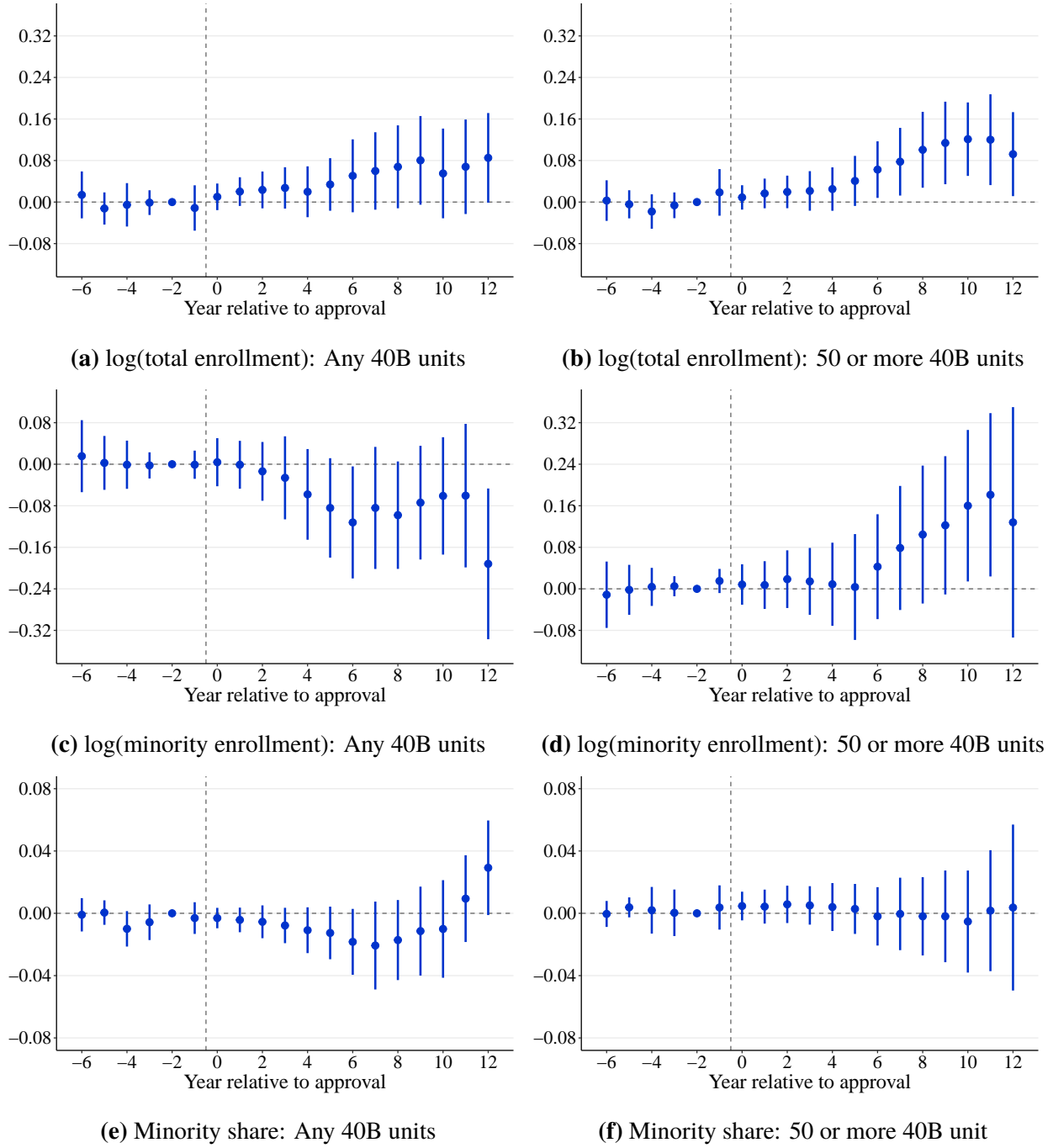
(a) 40Bs with less than 50 units



(b) 40Bs with 50 or more units

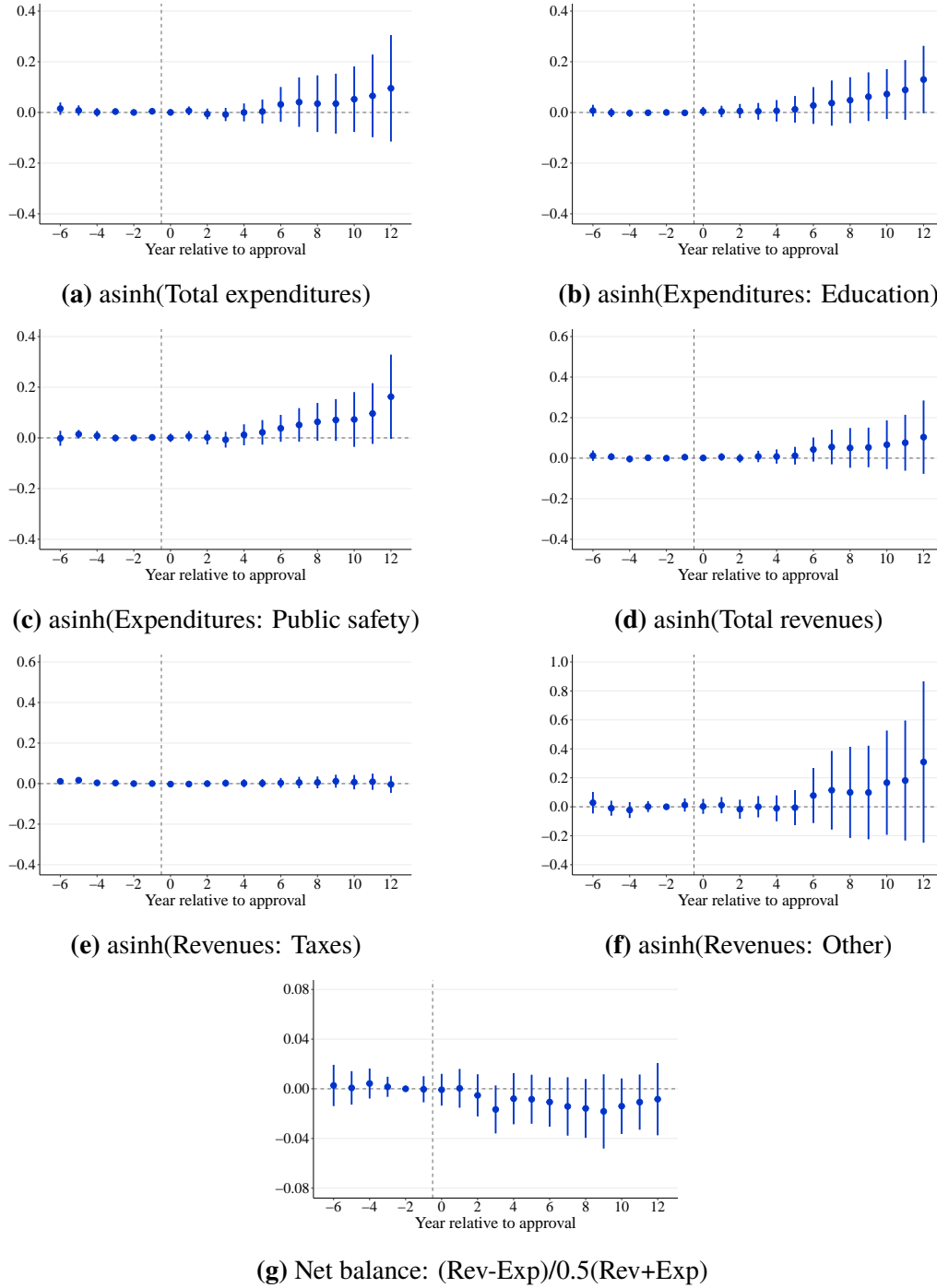
Note: Both panels plot the average number of individuals per square mile that Infutor reports as living within each of four cumulative rings around 40B developments. This population count includes 40B residents. The horizontal axis denotes years relative to the approval of the corresponding 40B development. Both panels use the sample of 40B developments that were approved between 1995 and 2019. Panel (a) restricts the sample to 40B developments with less than 50 units, panel (b) restricts the sample to 40B developments with 50 or more units.

Figure F.4: Event study of the impact of Chapter 40B on school district enrollment



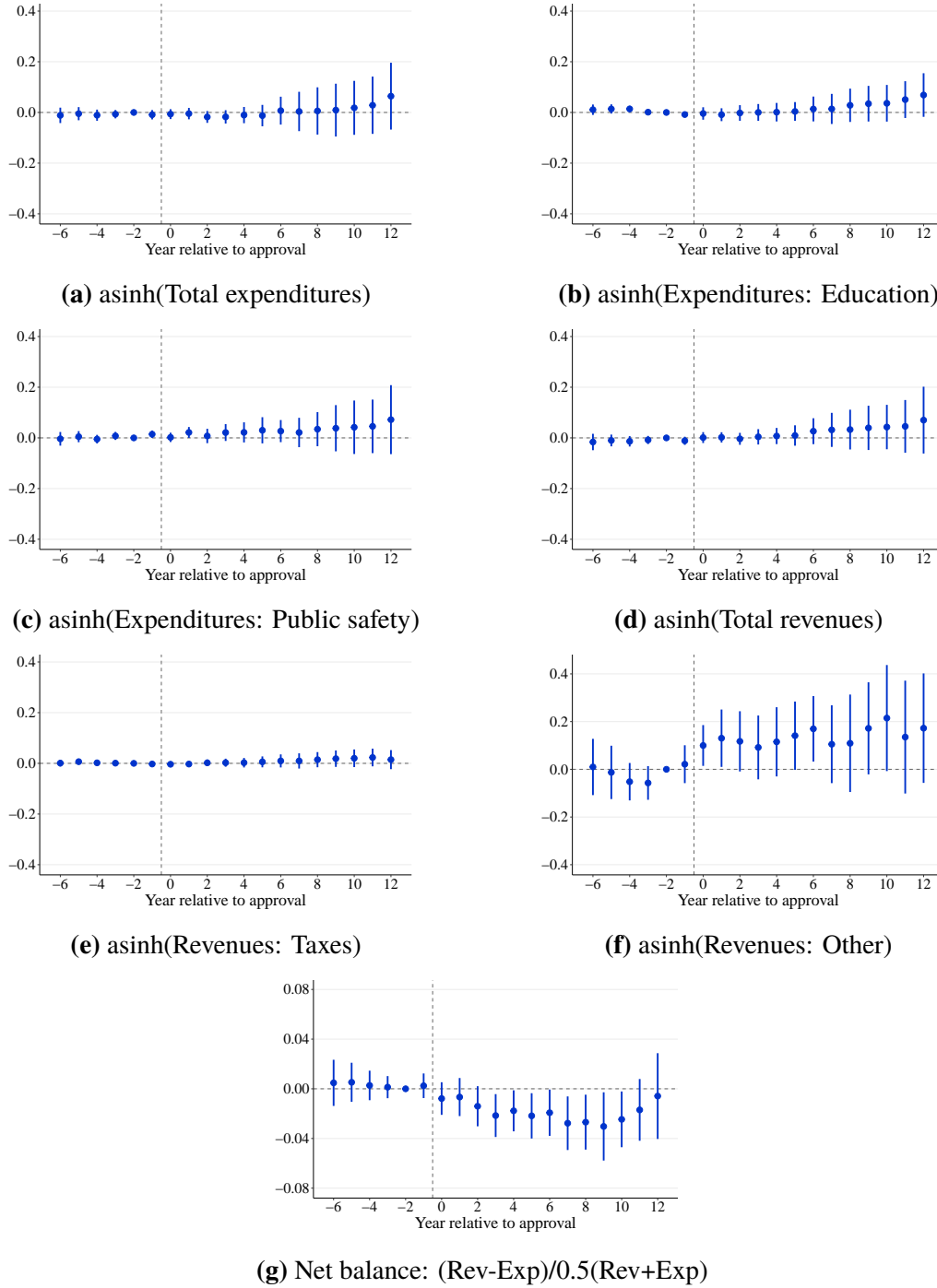
Note: The figures plot estimated coefficients of a staggered difference-in-differences regression following Callaway and Sant'Anna (2021) at the school district level using the logarithm of the total number of enrolled students (panels a, b), the logarithm of the number of minority enrolled students (panels c, d) and the share of minority students (panels e, f) as dependent variables. Minority students are defined as the total students minus non-Hispanic white students. The treatment period is defined as either the first year during which any 40B units were approved in the school district (left panels, including 169 districts) or during which 50 or more 40B units were approved (right panels, including 118 districts). We use publicly available data on school enrollment from 2002 to 2024 from the Massachusetts' Department of Elementary and Secondary Education (DESE). Vertical bars represent 95% confidence intervals. Standard errors are clustered at the school district level.

Figure F.5: Event study of the impact of Chapter 40B on local public finances (any 40B units)



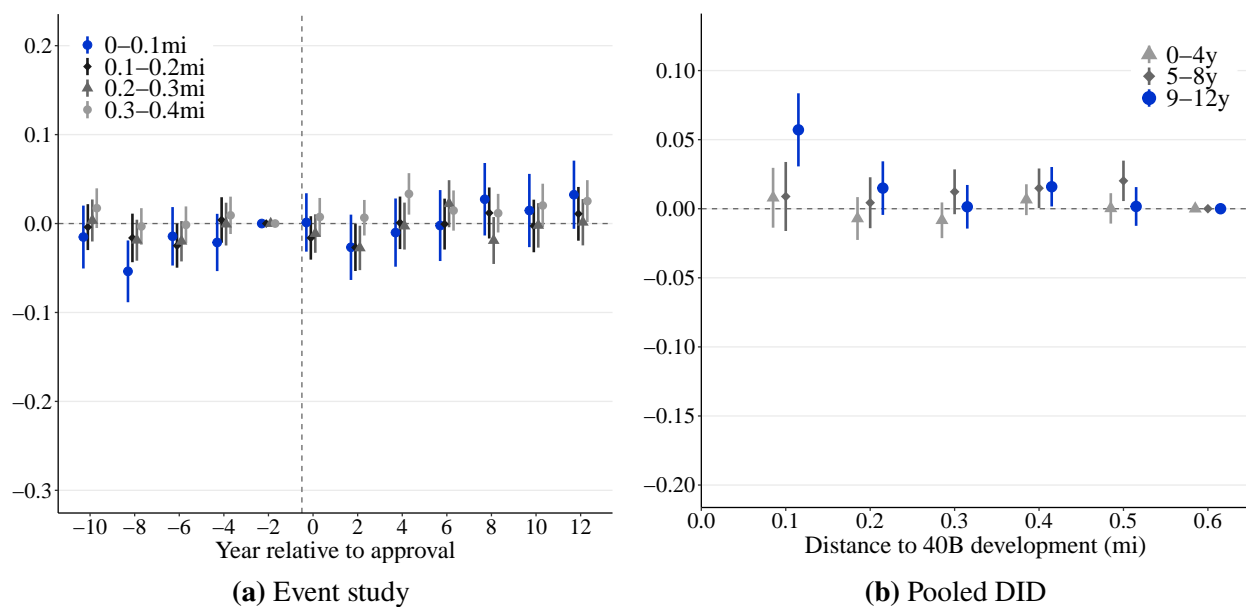
Note: The figures plot estimated coefficients of a staggered difference-in-differences regression following Callaway and Sant’Anna (2021) at the municipality level using the inverse hyperbolic sign—since a few municipality-years have zero values in the outcome—of several expenditure and revenue categories as dependent variables. The category “Revenue: Other” includes all other categories that are not taxes (e.g., federal/state transfers, permits and licensing, service charges). The net balance is computed as the difference between total revenues and expenditures divided by the average of the two. The treatment period is defined as the first year during which any 40B units were approved. We use publicly available data on local public finances from 2002 to 2024 from the Massachusetts’ Division of Local Services at the Department of Revenue. The number of treated municipalities throughout this period is 171. Vertical bars represent 95% confidence intervals. Standard errors are clustered at the municipality level.

Figure F.6: Event study of the impact of Chapter 40B on local public finances (≥ 50 40B units)



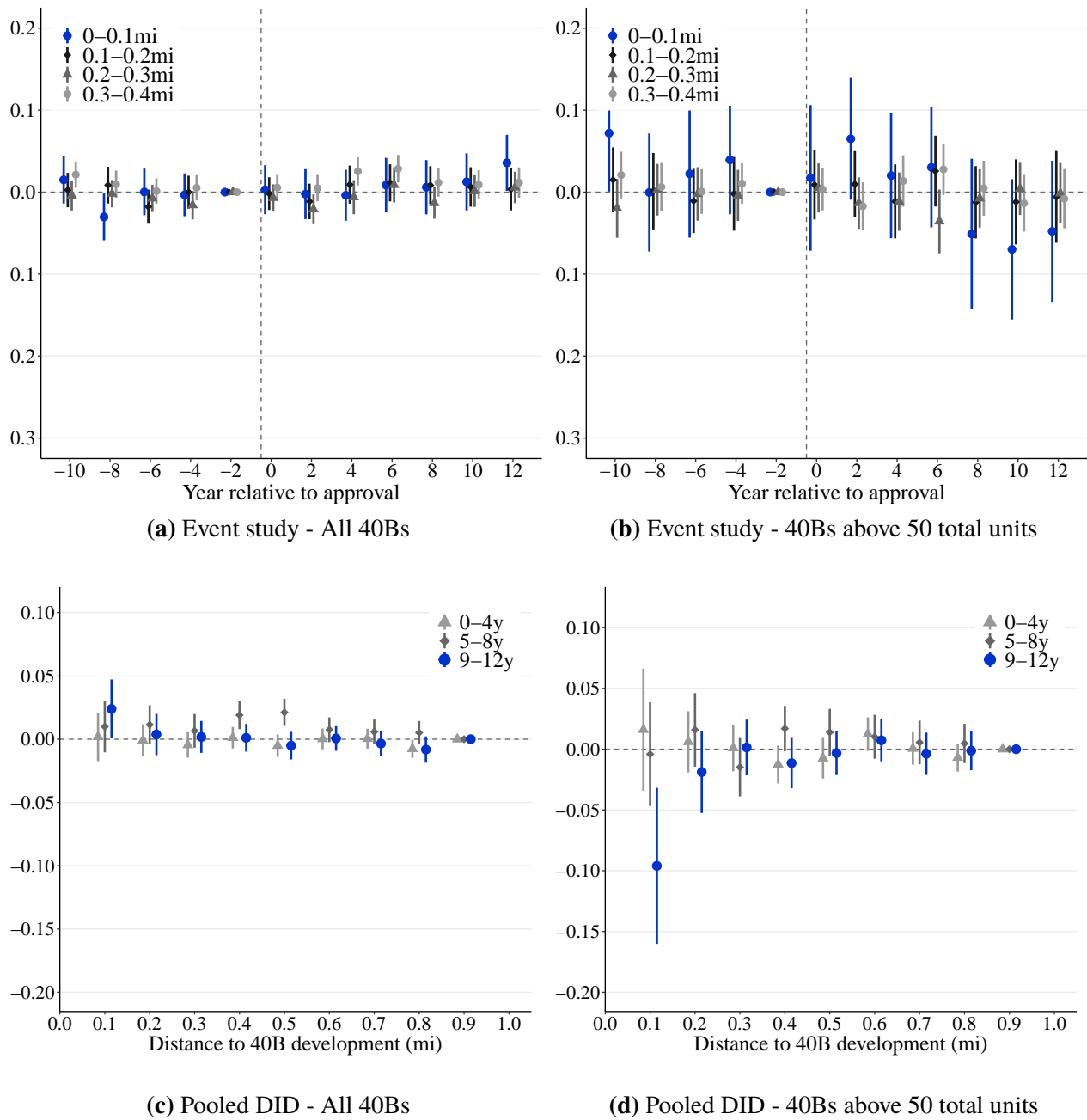
Note: The figures plot estimated coefficients of a staggered difference-in-differences regression following Callaway and Sant’Anna (2021) at the municipality level using the inverse hyperbolic sign—since a few municipality-years have zero values in the outcome—of several expenditure and revenue categories as dependent variables. The category “Revenue: Other” includes all other categories that are not taxes (e.g., federal/state transfers, permits and licensing, service charges). The net balance is computed as the difference between total revenues and expenditures divided by the average of the two. The treatment period is defined as the first year during which 50 or more 40B units were approved. We use publicly available data on local public finances from 2002 to 2024 from the Massachusetts’ Division of Local Services at the Department of Revenue. The number of treated municipalities throughout this period is 120. Vertical bars represent 95% confidence intervals. Standard errors are clustered at the municipality level.

Figure F.7: DID estimates of the impact of small 40Bs on sale prices of preexisting housing



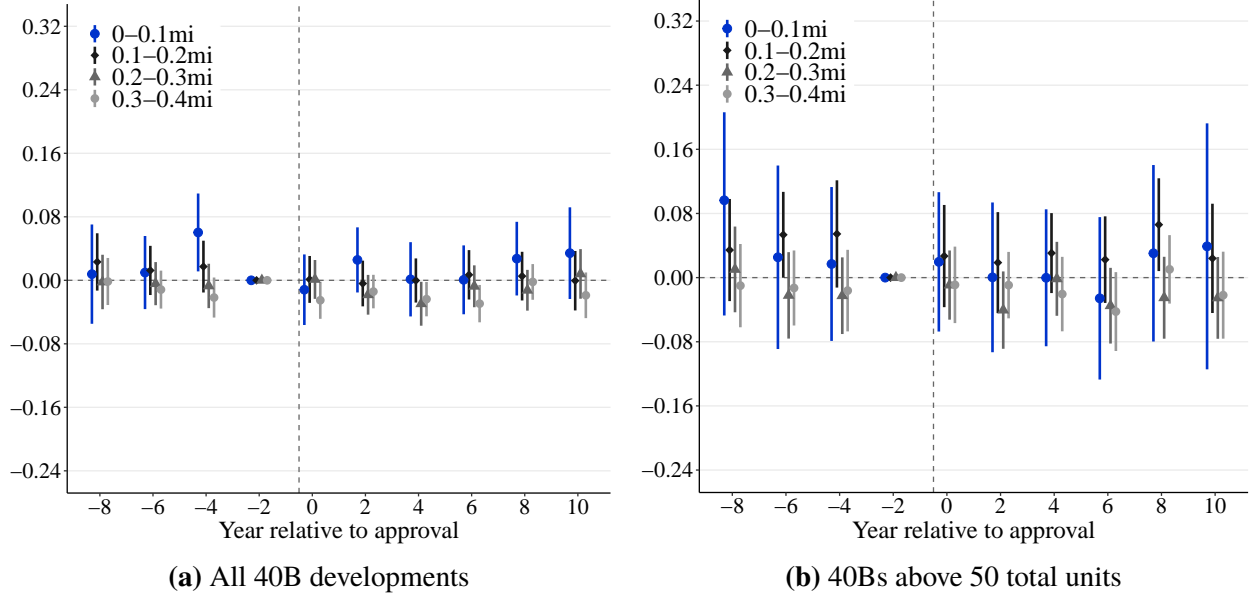
Note: This figure reproduces Figure 3 for the sample of 40B developments approved in 1995-2019 with less than 50 units.

Figure F.8: DID estimates of the impact of 40Bs on sale prices of preexisting housing within 1 mile



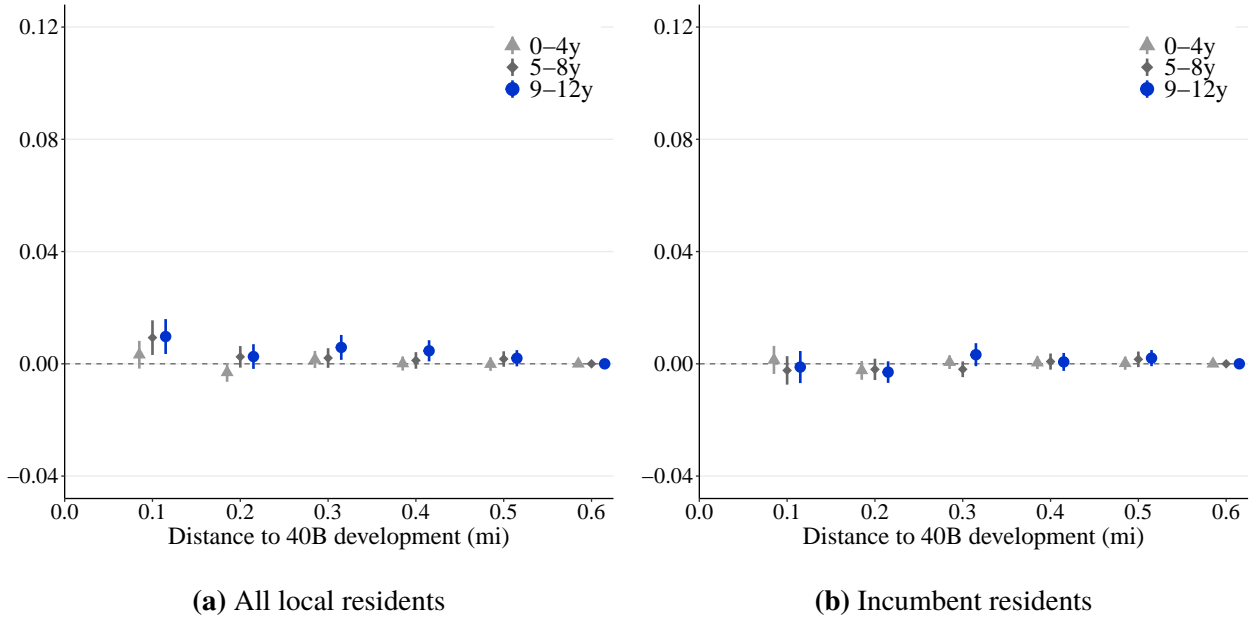
Note: This figure reproduces Figure 3 including all housing within 1 mile in the sample and using housing within 0.8 to 1 mile away as the comparison group.

Figure F.9: Placebo DID: impact of 40Bs on sale prices of preexisting housing



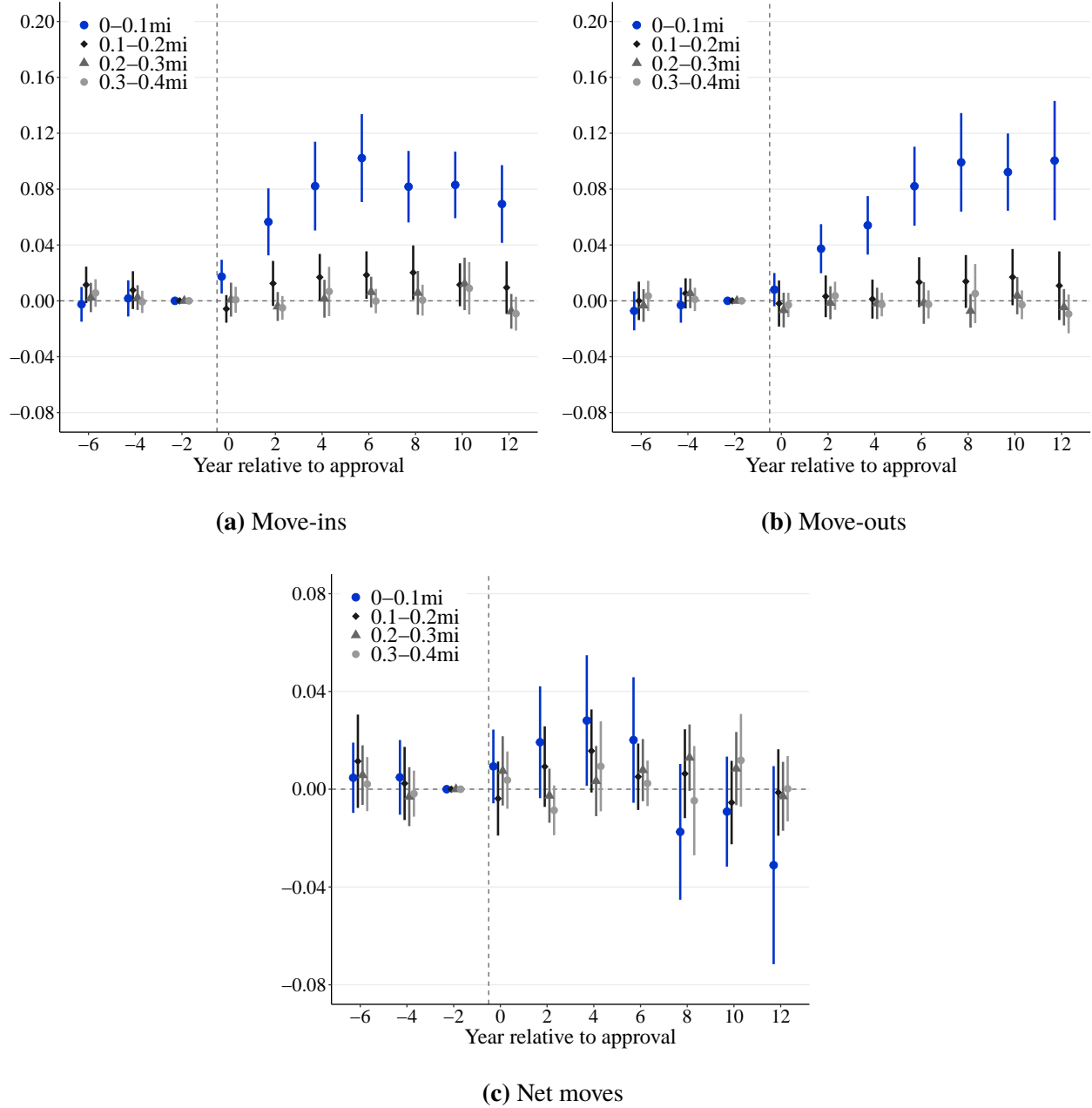
Note: The figures plot estimated coefficients $\beta_{\tau,r}$ in Equation (1) using 0.1 mile-wide rings and the logarithm of sale prices as the outcome. We define the 40B approval year as taking place nine years before announcement. We restrict the sample to all 40B developments (panel a) and 40B developments approved in 2002-2012 with 50 or more units (panel b); and sales of preexisting housing units.

Figure F.10: DID estimates of the impact of small 40Bs (less than 50 units) on residents' move-out rates



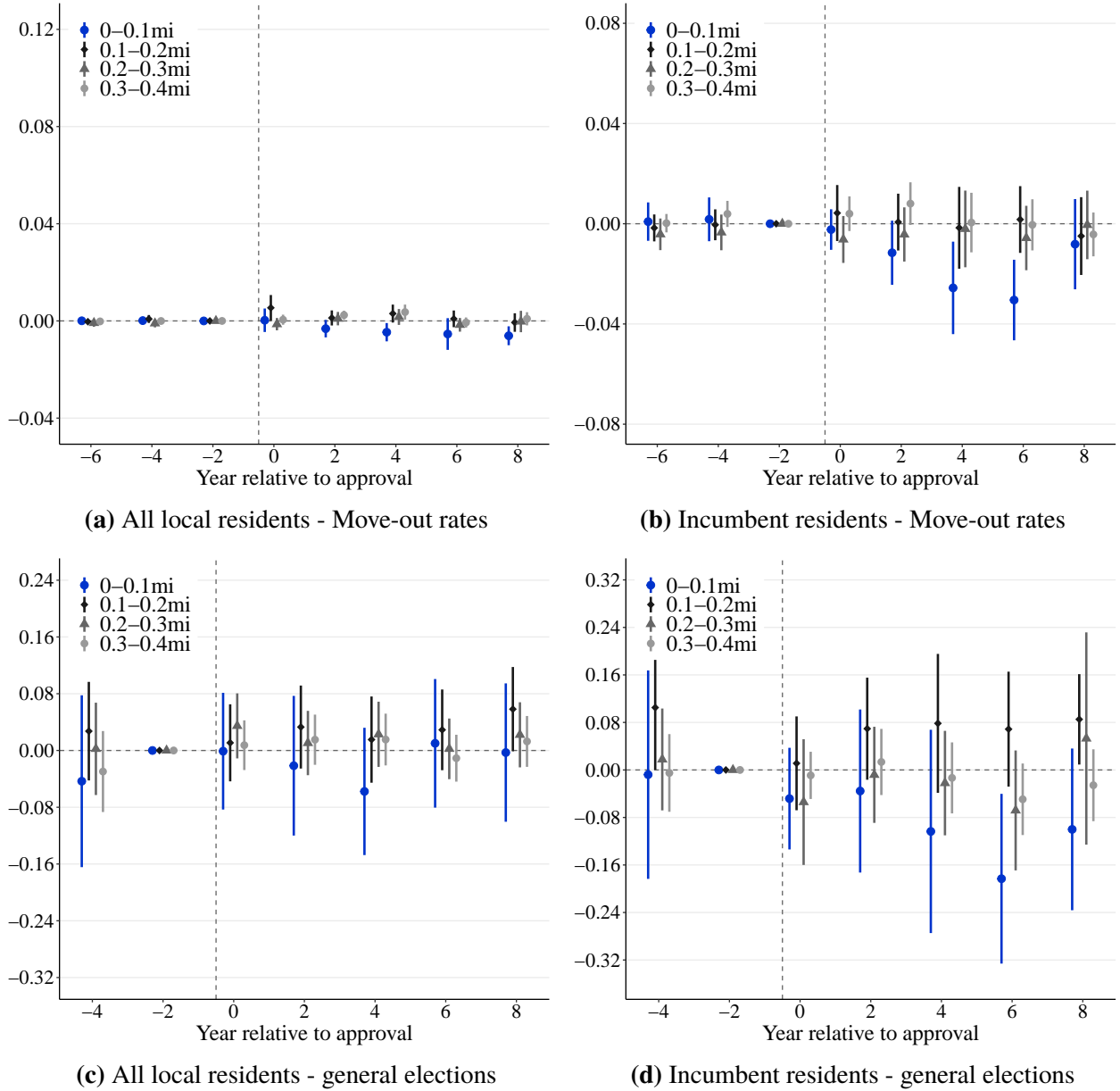
Note: The figure reproduces the pooled DID results in Figure 4 for 40B developments with less than 50 units.

Figure F.11: Event study for migration flows at incumbent residents' addresses for large 40Bs



Note: The figures show the estimated coefficients $\beta_{\tau,\tau}$ in Equation (1) using 0.1 mile-wide rings and incumbent residents' addresses within 0.5-0.6 miles from a 40B as a comparison group. All panels use the sample of individuals who ever lived at addresses where incumbent residents lived in year 7 before the corresponding 40B development is approved. The dependent variables are the number of residents moving into an incumbent address (panel a), moving out of an incumbent address (panel b), and the difference between these (panel c); all divided by the number of incumbent residents. The sample includes 40B developments with 50 or more units approved between 1995 and 2019. Vertical bars indicate 95% confidence intervals.

Figure F.12: Placebo DID: impact of 40Bs on move-out and turnout rates for large 40Bs



Note: The figures show estimated coefficients $\beta_{\tau,r}$ in Equation (1) using 0.1 mile-wide rings. The outcome variables are indicator variables for whether the individual moved out from the address neighboring a 40B as the outcome (panels a and b) and for whether an individual voted in a general election (panels c and d). We define the 40B approval year as taking place 9 years before announcement. We restrict the analysis to 40B developments approved in 2002-2012 with 50 or more units. Panels (a) and (c) uses the sample of ‘all local residents’, while panels (b) and (d) use the sample of ‘incumbent residents’ who were living near 40B developments in years 7 and 8 before permit approval—both of them are described in more detail in Section 3.1.

G Tables

Table G.1: Number of 40B developments by monitoring agency and municipality's Subsidized Housing Inventory (SHI)

	Above 10%	Below 10%
EOHLC	16	288
MassHousing	8	286
MassHousing and Other (non-EOHLC)	0	32
MassHousing Partnership	1	26
Other	45	197
Total	70	829

Note: This table shows counts of 40B developments approved between 1995 and 2019 based on a combination of their monitoring agency (rows) and whether the municipality where they are located is above or below the 10 percent affordable housing threshold. Data on municipalities' SHI percentages was obtained from EOHLC for years 1990, 1993, 1997, 2000, 2002-2014, 2017, 2020.

Table G.2: DID estimates of the quantity and quality of sold properties

Variable	Total numbers			All sales chars.				Old sales chars.			
	All	New	Old	SFR	Yr built	Beds	Baths	SFR	Yr built	Beds	Baths
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Yrs. 0-4: 0-0.1mi	42.3*** (3.37)	40.5*** (3.38)	-0.210 (0.732)	-0.134*** (0.012)	9.90*** (0.976)	-0.152*** (0.022)	0.038* (0.020)	-0.005 (0.006)	-0.492 (0.747)	-0.029 (0.023)	-0.024 (0.020)
Yrs. 0-4: 0.1-0.2mi	2.81*** (0.757)	1.23** (0.554)	0.660 (0.523)	-0.005 (0.006)	1.90*** (0.645)	0.024 (0.017)	0.047*** (0.015)	-0.002 (0.005)	-0.244 (0.565)	0.019 (0.017)	0.017 (0.015)
Yrs. 5-8: 0-0.1mi	22.5*** (2.69)	19.2*** (2.41)	-3.14*** (0.908)	-0.162*** (0.015)	11.4*** (1.10)	-0.145*** (0.029)	0.070*** (0.022)	0.012* (0.007)	-0.676 (0.836)	0.011 (0.031)	-0.006 (0.026)
Yrs. 5-8: 0.1-0.2mi	0.212 (0.978)	1.04 (0.758)	-1.93*** (0.632)	-0.0004 (0.008)	2.58*** (0.805)	0.018 (0.022)	0.057*** (0.021)	0.009 (0.006)	0.144 (0.731)	0.024 (0.021)	0.029 (0.020)
Yrs. 9-12: 0-0.1mi	24.6*** (2.96)	5.07** (2.17)	-2.24** (0.999)	-0.176*** (0.015)	10.9*** (1.12)	-0.169*** (0.028)	0.032 (0.023)	-0.005 (0.007)	-0.162 (0.823)	-0.010 (0.030)	-0.038 (0.025)
Yrs. 9-12: 0.1-0.2mi	1.41 (1.21)	0.121 (1.08)	-0.481 (0.568)	0.004 (0.008)	2.58*** (0.835)	0.037 (0.024)	0.027 (0.019)	0.009 (0.007)	-0.008 (0.725)	0.061** (0.025)	-0.002 (0.018)
Y mean (0-0.1mi)	34.3	8.43	26.4	0.772	1,957.4	2.96	1.98	0.785	1,950.2	2.93	1.89
Y mean (0.1-0.2mi)	30.0	6.00	24.2	0.810	1,957.1	3.03	2.02	0.830	1,950.5	3.00	1.93
Observations	166,108	166,108	166,108	142,677	141,637	140,385	141,610	133,174	133,174	131,407	132,721
R ²	0.649	0.287	0.850	0.777	0.682	0.542	0.609	0.828	0.709	0.545	0.602

Note: The table reports estimates for coefficients $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings for several outcomes. Columns 1-3 use the number of all sales, new house sales and old house sales per mi², respectively. Columns 4-7 use the following dependent variables for the sample of all sales: single-family residence indicator, year built, number of beds, and number of bathrooms. Columns 8-11 report these estimates restricting the sample to old houses. Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G.3: Long-differences regression of housing outcomes at the census tract level (1990-2019)

	Ln(units) (1)	Ownership (2)	Ln(home value) (3)	Ln(rent) (4)	Ln(units) (5)	Ownership (6)	Ln(home value) (7)	Ln(rent) (8)
40B	0.088*** (0.025)	-0.021** (0.009)	0.0008 (0.011)	0.045** (0.022)	0.064** (0.031)	-0.015 (0.011)	-0.012 (0.016)	0.0001 (0.029)
40B \times 50 units or more					0.034 (0.030)	-0.009 (0.011)	0.017 (0.015)	0.063** (0.030)
Y mean (40B tracts, 1990)	1,864	0.730	188,655	578	1,864	0.730	188,655	578
Observations	1,376	1,376	1,376	1,376	1,376	1,376	1,376	1,376
R ²	0.317	0.263	0.642	0.493	0.318	0.263	0.642	0.495

Note: The table reports estimates of regressions at the census tract level where the dependent variable is the difference in the outcome between 2019 and 1990, as measured by 2015-2019 5-year ACS and 1990 decennial Census data. As an independent variable, Columns 1-4 use an indicator for whether there is any 40B approval within that period. Columns 5-8 also include an interaction of this indicator with another indicator for whether 50 or more 40B units were built. All regressions include municipality fixed effects. The third to last row shows the mean of the dependent variable in 1990 for census tracts that approve at least one 40B unit. Standard errors in parenthesis (clustered at the census tract level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G.4: Robustness of price effects in years 9-12

Sample	Full sample				50 units or more			
	Full	Bal	Comp year	Dups	Full	Bal	Comp year	Dups
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0-0.1mi	0.027** (0.013)	0.029** (0.013)	0.022 (0.014)	-0.032 (0.019)	-0.092*** (0.033)	-0.075** (0.032)	-0.074** (0.031)	-0.100** (0.041)
0.1-0.2mi	0.002 (0.009)	0.004 (0.009)	-0.016* (0.009)	0.0003 (0.011)	-0.035** (0.017)	-0.015 (0.018)	-0.043** (0.018)	-0.012 (0.020)
0.2-0.3mi	-0.001 (0.007)	0.007 (0.007)	-0.013* (0.007)	-0.016 (0.011)	-0.008 (0.014)	0.002 (0.014)	-0.019 (0.014)	-0.030* (0.018)
0.3-0.4mi	0.005 (0.006)	0.011* (0.006)	-0.003 (0.007)	-0.018* (0.009)	-0.022* (0.012)	-0.007 (0.012)	-0.020 (0.012)	-0.032** (0.016)
0.4-0.5mi	-0.002 (0.006)	0.005 (0.006)	-0.008 (0.006)	-0.005 (0.008)	-0.010 (0.011)	0.003 (0.012)	-0.010 (0.012)	-0.004 (0.014)
Observations	471,815	296,629	449,224	242,358	145,693	94,165	136,132	92,533
R ²	0.881	0.873	0.881	0.894	0.868	0.856	0.866	0.888

Note: The table reports estimates for coefficient $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings and the logarithm of house prices as an outcome. The first five columns includes all 40Bs regardless of size, the last five columns only include 40Bs with 50 or more units. Within each group, each column represents a different sample or design choice. In order: sample of 40Bs approved between 1995-2019 (full sample), sample of 40Bs approved between 1995-2007 (balanced sample), full sample using only Comprehensive 40B Permit as the treatment event (comprehensive year), and full sample dropping sales within 1 mile of more than one 40B (duplicates). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G.5: Characteristics of destination census tracts of incumbent movers

	Out of state	log(density)	White	HH income	Ownership	log(value)	SFR zoning
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Yrs. 0-4: 0-0.1mi	-0.030 (0.024)	0.008 (0.082)	0.0005 (0.006)	0.006 (0.018)	0.009 (0.010)	0.003 (0.023)	0.007 (0.017)
Yrs. 0-4: 0.1-0.2mi	0.006 (0.019)	0.032 (0.057)	-0.007* (0.003)	-0.007 (0.011)	0.0004 (0.007)	-0.004 (0.016)	-0.003 (0.013)
Yrs. 5-8: 0-0.1mi	-0.009 (0.031)	-0.015 (0.101)	0.001 (0.008)	-0.011 (0.021)	0.009 (0.013)	-0.030 (0.027)	-0.001 (0.022)
Yrs. 5-8: 0.1-0.2mi	0.035 (0.023)	0.058 (0.072)	-0.007 (0.004)	-0.038** (0.016)	-0.009 (0.008)	-0.025 (0.017)	-0.024 (0.015)
Yrs. 9-12: 0-0.1mi	-0.015 (0.041)	-0.291** (0.127)	-0.004 (0.011)	0.038 (0.033)	0.025 (0.019)	0.013 (0.040)	0.045 (0.030)
Yrs. 9-12: 0.1-0.2mi	0.040 (0.032)	-0.123 (0.091)	-0.003 (0.005)	-0.005 (0.022)	0.003 (0.011)	-0.016 (0.029)	0.024 (0.023)
Y mean (0-0.1mi)	0.251	7,541.3	0.907	42,954.0	0.608	197,559.6	0.634
Y mean (0.1-0.2mi)	0.226	6,138.0	0.920	43,740.0	0.637	193,449.0	0.663
Observations	156,927	154,763	154,763	154,760	154,761	154,663	120,914
R ²	0.183	0.271	0.172	0.281	0.210	0.273	0.269

Note: The table reports estimates for coefficients $\theta_{1,r}$, $\theta_{2,r}$ and $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings on the sample of incumbent residents moving out from housing within 0.6 miles of 40Bs. We use several characteristics of their destination tracts as an outcome. In order: out-of-state dummy (from Infutor), logarithm of density (population per square mile), share of white population, logarithm of household income, share of owner-occupied housing units, logarithm of median home value, and share of the tract zoned as single-family (from MassGIS). Data on columns 2-6 comes from the 1990 decennial census. Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G.6: Heterogeneity results: long-run price effects on sale prices, move-out and voter turnout rates of incumbent residents for large 40B developments

Heterogeneity var.	Tenure		Agency		Development size		40B Zoning		Household income		Age		Owner		Party	
	Ownership (1)	Rental (2)	Masshous. (3)	Others (4)	<median (5)	>median (6)	Other (7)	SFR (8)	<median (9)	>median (10)	<50 (11)	>=50 (12)	No (13)	Yes (14)	Other (15)	Rep (16)
<i>Panel A: Move-out rates - all local residents</i>																
0-0.1mi	0.047*** (0.008)	0.102*** (0.011)	0.093*** (0.012)	0.064*** (0.009)	0.044*** (0.008)	0.118*** (0.013)	0.093*** (0.010)	0.062*** (0.012)	0.067*** (0.009)	0.090*** (0.012)	0.073*** (0.008)	0.061*** (0.008)	0.066*** (0.007)	0.041*** (0.010)	0.030*** (0.009)	0.025** (0.012)
0.1-0.2mi	0.004 (0.005)	0.047*** (0.009)	0.036*** (0.009)	0.023*** (0.007)	0.009 (0.006)	0.053*** (0.010)	0.046*** (0.010)	0.011* (0.006)	0.034*** (0.009)	0.024*** (0.007)	0.024*** (0.007)	0.024*** (0.007)	0.021*** (0.006)	0.001 (0.006)	0.017** (0.007)	0.012 (0.008)
<i>Panel B: Move-out rates - incumbent residents</i>																
0-0.1mi	0.026*** (0.007)	0.051*** (0.010)	0.044*** (0.010)	0.035*** (0.008)	0.020*** (0.006)	0.064*** (0.012)	0.044*** (0.010)	0.036*** (0.009)	0.029*** (0.007)	0.052*** (0.011)	0.063*** (0.010)	0.033*** (0.006)	0.046*** (0.008)	0.009 (0.007)	0.040*** (0.010)	0.040*** (0.010)
0.1-0.2mi	0.008 (0.007)	0.022*** (0.006)	0.017*** (0.006)	0.015** (0.007)	-0.002 (0.005)	0.038*** (0.008)	0.030*** (0.007)	0.002 (0.005)	0.013** (0.006)	0.020*** (0.007)	0.022*** (0.006)	0.018*** (0.006)	0.015*** (0.005)	0.006 (0.004)	0.016** (0.007)	0.024*** (0.008)
<i>Panel C: Voter turnout in general elections - all local residents</i>																
0-0.1mi	-0.001 (0.029)	-0.057** (0.026)	-0.045 (0.029)	-0.013 (0.022)	-0.027 (0.023)	-0.039 (0.030)	-0.048* (0.027)	-0.009 (0.025)	-0.038 (0.029)	-0.020 (0.023)	-0.037 (0.026)	-0.010 (0.021)	-0.003 (0.021)	-0.006 (0.023)	-0.003 (0.023)	-0.073*** (0.026)
0.1-0.2mi	-0.022 (0.019)	-0.005 (0.019)	-0.015 (0.024)	-0.009 (0.014)	0.014 (0.017)	-0.033 (0.023)	-0.007 (0.022)	-0.011 (0.016)	0.007 (0.019)	-0.026 (0.021)	-0.040** (0.017)	0.026 (0.017)	0.012 (0.015)	-0.017 (0.015)	-0.023 (0.015)	-0.009 (0.019)
<i>Panel D: Voter turnout in general elections - incumbent residents</i>																
0-0.1mi	0.020 (0.034)	-0.030 (0.029)	-0.033 (0.034)	0.006 (0.027)	-0.007 (0.024)	-0.023 (0.037)	0.005 (0.031)	-0.030 (0.029)	-0.013 (0.030)	-0.009 (0.029)	0.024 (0.027)	-0.017 (0.025)	0.004 (0.025)	-0.015 (0.028)	0.0002 (0.026)	-0.040 (0.028)
0.1-0.2mi	0.028 (0.022)	-0.005 (0.019)	0.012 (0.027)	0.007 (0.014)	0.029* (0.016)	-0.016 (0.027)	0.006 (0.026)	0.014 (0.014)	0.009 (0.017)	0.010 (0.024)	-0.013 (0.019)	0.017 (0.018)	0.025 (0.017)	0.006 (0.019)	-0.008 (0.020)	-0.0002 (0.020)

Note: The table reports estimates for coefficient $\theta_{3,r}$ in Equation (2)—i.e., 9-12 years after 40B approval—using 0.1 mile-wide rings for several outcome variables in each panel. Each column uses a different sample depending on: tenure (ownership or rental 40Bs, columns 1-2), monitoring agency (Masshousing or other, columns 3-4), 40B development size (above or below median in the sample, 5-6), whether the 40B development is in an area zoned as single-family residence (7-8), median household income in the 40B's census tract in 1990 (above or below the median in the sample, 9-10), the individual's age (below or above 50, columns 11-12), whether the individual owns the unit near the associated 40B (13-14), and their political affiliation (15-16). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G.7: Robustness of move-out rate estimates for all local residents in years 9-12

Sample	Full sample					50 units or more				
	Full	Bal	Comp year	Dups	Near	Full	Bal	Comp year	Dups	Near
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
0-0.1mi	0.029*** (0.003)	0.026*** (0.004)	0.034*** (0.004)	0.050*** (0.007)	0.029*** (0.003)	0.078*** (0.008)	0.075*** (0.008)	0.083*** (0.008)	0.078*** (0.013)	0.076*** (0.008)
0.1-0.2mi	0.010*** (0.002)	0.009*** (0.003)	0.013*** (0.003)	0.015** (0.006)	0.009*** (0.002)	0.029*** (0.006)	0.028*** (0.006)	0.032*** (0.006)	0.020* (0.012)	0.028*** (0.006)
0.2-0.3mi	0.008*** (0.002)	0.008*** (0.002)	0.010*** (0.002)	0.002 (0.005)	0.008*** (0.002)	0.014*** (0.004)	0.014*** (0.005)	0.015*** (0.005)	0.004 (0.009)	0.013*** (0.004)
0.3-0.4mi	0.008*** (0.002)	0.008*** (0.002)	0.008*** (0.002)	0.003 (0.005)	0.007*** (0.002)	0.014*** (0.005)	0.017*** (0.005)	0.017*** (0.006)	0.003 (0.010)	0.014*** (0.005)
0.4-0.5mi	0.003* (0.001)	0.003* (0.001)	0.003** (0.001)	0.002 (0.004)	0.003* (0.001)	0.004 (0.003)	0.003 (0.003)	0.006** (0.003)	-0.001 (0.009)	0.003 (0.003)
Observations	22,364,128	17,304,841	19,497,043	8,639,114	20,363,736	6,659,850	5,178,831	5,578,066	3,458,155	6,123,451
R ²	0.193	0.070	0.203	0.310	0.196	0.205	0.080	0.212	0.300	0.207

Note: The table reports estimates for coefficient $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings and an indicator variable for whether the local resident moved out from an address within 0.6mi within a 40B development. The first five columns includes all 40Bs regardless of size, the last five columns only include 40Bs with 50 or more units. Within each group, each column represents a different sample or design choice. In order: sample of 40Bs approved between 1995-2019 (full sample), sample of 40Bs approved between 1995-2007 (balanced sample), full sample using only Comprehensive 40B Permit as the treatment event (comprehensive year), full sample dropping local residents who lived within 0.6 miles of more than one 40B (duplicates), and full sample only keeping the nearest address for those individuals who show up as local residents of multiple 40B addresses (near). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G.8: Robustness of move-out rate estimates for incumbent residents in years 9-12

Sample	Full sample					50 units or more				
	Full	Bal	Comp year	Dups	Near	Full	Bal	Comp year	Dups	Near
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
0-0.1mi	0.011*** (0.003)	0.007** (0.003)	0.016*** (0.003)	0.022*** (0.004)	0.009*** (0.003)	0.040*** (0.007)	0.035*** (0.007)	0.047*** (0.007)	0.042*** (0.008)	0.038*** (0.007)
0.1-0.2mi	0.003 (0.002)	0.002 (0.002)	0.005** (0.002)	0.005 (0.003)	0.002 (0.002)	0.016*** (0.005)	0.018*** (0.005)	0.018*** (0.005)	0.014** (0.007)	0.014*** (0.005)
0.2-0.3mi	0.003 (0.002)	0.002 (0.002)	0.004* (0.002)	-0.0006 (0.003)	0.003 (0.002)	0.001 (0.004)	0.0004 (0.004)	0.003 (0.004)	-0.005 (0.005)	0.0004 (0.004)
0.3-0.4mi	0.0002 (0.001)	0.0005 (0.002)	0.002 (0.001)	-0.002 (0.002)	0.0004 (0.001)	-0.0009 (0.003)	0.000 (0.003)	0.001 (0.003)	-0.003 (0.003)	-0.002 (0.003)
0.4-0.5mi	0.001 (0.001)	0.0010 (0.001)	0.002* (0.001)	0.001 (0.002)	0.002 (0.001)	-0.0005 (0.002)	0.000 (0.003)	-0.0008 (0.002)	-0.002 (0.003)	-0.001 (0.002)
Observations	9,690,853	6,473,469	8,690,418	4,994,521	8,960,685	2,923,192	2,009,452	2,504,947	1,849,663	2,706,842
R ²	0.036	0.038	0.035	0.092	0.036	0.042	0.046	0.041	0.084	0.042

Note: The table reports estimates for coefficient $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings and an indicator variable for whether the incumbent resident moved out from an address within 0.6mi within a 40B development. The first five columns includes all 40Bs regardless of size, the last five columns only include 40Bs with 50 or more units. Within each group, each column represents a different sample or design choice. In order: sample of 40Bs approved between 1995-2019 (full sample), sample of 40Bs approved between 1995-2007 (balanced sample), full sample using only Comprehensive 40B Permit as the treatment event (comprehensive year), full sample dropping incumbent residents who lived within 0.6 miles of more than one 40B (duplicates), and full sample only keeping the nearest address for those individuals who show up as incumbents of multiple 40B addresses (near). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G.9: Robustness of general election turnout estimates for all local residents in years 9-12

Sample	Full sample					50 units or more				
	Full	Bal	Comp year	Dups	Near	Full	Bal	Comp year	Dups	Near
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
0-0.1mi	-0.0005 (0.008)	0.009 (0.009)	-0.009 (0.009)	-0.012 (0.012)	-0.0004 (0.009)	-0.029 (0.019)	-0.015 (0.020)	-0.035* (0.020)	-0.024 (0.024)	-0.031 (0.019)
0.1-0.2mi	0.009 (0.006)	0.019*** (0.006)	0.002 (0.006)	0.002 (0.010)	0.005 (0.006)	-0.009 (0.014)	-0.003 (0.015)	-0.011 (0.015)	-0.003 (0.016)	-0.013 (0.014)
0.2-0.3mi	-0.006 (0.006)	-0.002 (0.006)	-0.002 (0.006)	0.0002 (0.008)	-0.007 (0.006)	-0.011 (0.012)	-0.006 (0.013)	-0.010 (0.012)	-0.006 (0.014)	-0.013 (0.012)
0.3-0.4mi	0.000 (0.004)	0.0004 (0.005)	0.004 (0.004)	0.005 (0.007)	-0.001 (0.004)	0.0002 (0.009)	-0.004 (0.010)	-0.0006 (0.009)	0.002 (0.011)	-0.004 (0.009)
0.4-0.5mi	0.003 (0.004)	0.004 (0.004)	0.001 (0.004)	-0.007 (0.006)	0.002 (0.004)	-0.0002 (0.007)	0.006 (0.008)	-0.0002 (0.008)	-0.015 (0.012)	-0.002 (0.007)
Observations	3,350,176	2,279,497	3,047,650	1,705,325	2,999,788	974,710	687,759	846,425	631,379	878,374
R ²	0.496	0.488	0.498	0.504	0.495	0.479	0.467	0.480	0.486	0.480

Note: The table reports estimates for coefficient $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings and an indicator variable for whether the local resident voted in a general election. The first five columns includes all 40Bs regardless of size, the last five columns only include 40Bs with 50 or more units. Within each group, each column represents a different sample or design choice. In order: sample of 40Bs approved between 1995-2019 (full sample), sample of 40Bs approved between 1995-2007 (balanced sample), full sample using only Comprehensive 40B Permit as the treatment event (comprehensive year), full sample dropping local residents who lived within 0.6 miles of more than one 40B (duplicates), and full sample only keeping the nearest address for those individuals who show up as local residents of multiple 40B addresses (near). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table G.10: Robustness of general election turnout estimates for incumbent in years 9-12

Sample	Full sample					50 units or more				
	Full	Bal	Comp year	Dups	Near	Full	Bal	Comp year	Dups	Near
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
0-0.1mi	0.008 (0.010)	0.017 (0.010)	-0.004 (0.010)	0.006 (0.014)	0.007 (0.010)	-0.011 (0.021)	-0.001 (0.022)	-0.008 (0.022)	0.006 (0.025)	-0.020 (0.021)
0.1-0.2mi	0.004 (0.007)	0.009 (0.008)	-0.0003 (0.008)	0.004 (0.011)	0.002 (0.007)	0.009 (0.014)	0.018 (0.016)	0.005 (0.015)	0.026 (0.020)	0.005 (0.015)
0.2-0.3mi	-0.004 (0.006)	-0.004 (0.007)	0.003 (0.007)	0.007 (0.010)	-0.006 (0.007)	-0.010 (0.012)	-0.012 (0.013)	-0.011 (0.013)	0.011 (0.016)	-0.014 (0.013)
0.3-0.4mi	-0.004 (0.006)	-0.005 (0.007)	0.002 (0.006)	0.009 (0.009)	-0.006 (0.006)	0.004 (0.011)	0.003 (0.012)	0.007 (0.011)	0.023 (0.015)	-0.001 (0.011)
0.4-0.5mi	0.001 (0.005)	0.004 (0.006)	0.0005 (0.005)	-0.001 (0.008)	-0.001 (0.005)	0.010 (0.009)	0.021** (0.010)	0.013 (0.009)	0.005 (0.012)	0.006 (0.009)
Observations	2,084,395	1,321,169	1,899,663	1,156,401	1,910,805	605,179	404,279	526,779	409,186	555,254
R ²	0.496	0.488	0.498	0.505	0.496	0.488	0.480	0.486	0.495	0.490

Note: The table reports estimates for coefficient $\theta_{3,r}$ in Equation (2) using 0.1 mile-wide rings and an indicator variable for whether the incumbent resident voted in a general election. The first five columns includes all 40Bs regardless of size, the last five columns only include 40Bs with 50 or more units. Within each group, each column represents a different sample or design choice. In order: sample of 40Bs approved between 1995-2019 (full sample), sample of 40Bs approved between 1995-2007 (balanced sample), full sample using only Comprehensive 40B Permit as the treatment event (comprehensive year), full sample dropping incumbent residents who lived within 0.6 miles of more than one 40B (duplicates), and full sample only keeping the nearest address for those individuals who show up as incumbents of multiple 40B addresses (near). Standard errors in parenthesis (clustered at the 40B level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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