Homeownership, Labor Supply, and Neighborhood Quality*

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Abstract

This paper provides new evidence on the external neighborhood benefits of homeownership among low-income populations using a natural experiment in Israel that generated large changes in neighborhood homeownership rates while holding fixed the residents and housing stock, two primary sources of bias in traditional estimates of homeownership effects. When public housing tenants are given the opportunity to buy their units, eventual buyers significantly increase their labor supply on both the extensive and intensive margins. And the effects are felt in the neighborhood: when homeownership rates rise by 10 percentage points due to sales of these public housing units to sitting tenants, prices of neighborhood homes rise by 1.5-2%, reflecting an improvement in neighborhood quality. To address endogeneity in tenants' decisions to buy their homes, we take advantage of intertemporal and cross-sectional variation in purchase price discounts exogenously set by the government, and we find similar results. We discuss the conditions under which these effects may be interpreted as homeownership effects, per se, versus privatization effects. Evidence supports the general relevance of our results for policies that increase homeownership among low income populations using financial incentives.

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1 Introduction

Does homeownership promote household and neighborhood success? Governments of many developed countries engage in costly policy to incentivize homeownership through tax subsidies that primarily benefit the rich (Poterba and Sinai (2008)), home purchase subsidies to the poor (e.g. FHFA), and public housing privatizations (Sodini et al. (2016)). They do so with the view that homeownership has financial benefits for the owners themselves and has positive externalities for communities.[1] Yet causal evidence on the external benefits of homeownership is limited. As homeownership rates change across neighborhoods and over time, so do the observable and unobservable characteristics of residents and the housing stock, making ownership effects difficult to identify.[2]

This paper isolates the effects of homeownership – holding residents and housing stock fixed – using a natural experiment in Israel in which public housing units were sold to tenants, generating large changes in homeownership rates in certain localities. We use variation in government-set sale price discounts to identify the effects of these changes on neighborhood quality, as measured by nearby private housing prices. Seeking to understand the extent to which changes in neighborhood quality are the result of homeownership, per se, we drill down to measure tenants behavioral changes in their transition into ownership, estimating effects on household labor supply when discounts suddenly rise.

Homeownership might affect labor supply if new owners work more to finance their purchase or because they feel a greater sense of investment in their futures, with an asset they can bequeath to their offspring. Alternatively, in the privatization context, freedom from the means testing of public housing may release a labor supply constraint, or a wealth effect from the discounted sale could actually lead to a reduction in work. Changes in work behavior may in turn affect neighborhood character and trajectory. In addition, homeowners are thought to be more geographically stable and have greater incentives to invest in their property and its surroundings, including the neighborhood's appearance, amenities, and social capital (DiPasquale and Glaeser (1999); Hoff and Sen (2005)).

From 2000-2012, the Israeli government sold a third of its large public housing stock to sitting tenants. Neighborhoods with initially high densities of public housing had

[1]While pro-homeownership policies are often said to be justifiable only by benefits external to the homeowner, one could also imagine justifications based on market failures that lead individuals to make internally suboptimal decisions or to fail to fully internalize the consequences for their children.

[2]Coulson and Li (2013) discuss the identification challenges at length and propose panel data techniqus to address them. Engelhardt et al. (2010) use randomly assigned home purchase saving subsidies among a low income population in Tulsa, OK to identify homeownership effects on political engagement and home maintenance. Aaronson (2000) shows that homeownership is highly correlated with residential stability, which seems to drive homeownership effects on children's achievement.

the potential to experience significant changes in homeownership rates due only to these mass sales. Meanwhile, the residents of the neighborhood and the housing stock remained constant whether or not units were sold; resale of a unit within five years of purchase had prohibitive financial consequences, and non-buyers continued in their units as renters. Furthermore, units were priced at a discount set by the government, such that similar tenants faced different prices based on small differences in features such as tenure in public housing. Discounts were changed sporadically without warning, and tenants could not anticipate large declines in the effective sale price they faced (Arbel et al. (2014)).

A typical challenge in studies of homeownership is that buyers are selected. In our setting, tenants who bought their units may in principle be more motivated or of higher ability than other public housing dwellers. At the aggregate level, neighborhoods with higher purchase rates may be on faster price growth trajectories that are anticipated by tenants. We thus take several approaches to identifying effects at each the household and neighborhood levels. In a difference-in-differences framework, we use sharp intertemporal variation in discounts in 2005 as an event that increases purchases and may induce changes in labor supply and other behaviors that may affect the neighborhood. In a subset of analyses, we additionally use variation across households in discounts to instrument for the purchase decision, generating plausibly exogenous variation both in household level homeownership and, aggregated up, in neighborhood level homeownership rates. Because local amenities should be capitalized into housing prices, we approximate changes in neighborhood quality using transaction prices of private housing sales in the vicinity of public housing clusters.

Administrative data from the public housing authorities, the National Insurance Institute (NII), and the Israeli Tax Authority (ITA) enable precise measurement of these labor supply and neighborhood changes. We match tenants across data sources to track their histories in public housing, the details of their home purchases, their labor supply, and their various sources of income. While many studies of homeownership measure aggregate housing prices in neighborhoods, we observe private transaction prices, addresses, and hedonic characteristics for individual units, greatly reducing both omitted variables bias and measurement error due to unobserved features of the housing stock. The address-level detail further enables a variety of neighborhood definitions at both official statistical levels of aggregation and arbitrary radii around private transactions, which is important given evidence that housing externalities can be highly localized (Harding et al. (2009); Rossi-Hansberg et al. (2010); Campbell et al. (2011); Autor et al. (2014)).

We first show, at the household level, that tenants facing larger discounts on the market price of their unit are significantly more likely to buy. To understand potential behavioral changes that may accompany the transition into homeownership, we study a particular sale period in which discounts increased substantially. When the new sale terms were announced, buyers increased their labor supply relative to comparable non-buyers on both the extensive and intensive margins. This effect holds up under a variety of identification strategies, including instrumental variables (using discounts) and matching techniques in addition to a basic OLS fixed effects model. Our analysis indicates that disincentives to work in Israeli public housing were weak, such that the effect is not likely to be due to exiting a means tested system. Buyers' increased connection to the labor market suggests that a rise in homeownership rates may drive improvements in these low income, public housing neighborhoods as they transition from being pockets of disadvantage and welfare dependence to working class. Ownership of a significant asset may also have increased these tenants' engagement with the success of their neighborhoods as well as with their own financial futures (Jha and Shayo (2019)).

Aggregating households to the neighborhood level, we find that in neighborhoods with a high initial share of public housing and thus a high-dosage experiment, increases in homeownership indeed cause a significant increase in nearby private housing prices, reflecting an improvement in neighborhood quality. IV estimates are remarkably similar to OLS estimates, ranging from 1.5 to 2.2% per 10 percentage point increase in homeownership rates. Effects hold across a variety of measures of a "neighborhood" and with flexible geographic area controls. Difference-in-difference estimates comparing a reduced set of similar neighborhoods in the same town suggest similar, though slightly larger effects. This strategy focuses on the same sale event studied at the individual level, showing that just as the larger discounts are announced in 2005 and buyers increase their labor supply, private property values nearby rise.

Identifying the effects of homeownership on neighborhoods is a fundamentally difficult task because many observable and unobservable aspects of neighborhoods vary with homeownership rates. Because homeowners tend to be wealthier and more educated than renters, neighborhoods with high homeownership rates are likely to differ from others in the cross-section on a variety of dimensions that correlate with these characteristics, including ability, family characteristics, and features of the housing stock. Panel studies that observe neighborhoods over time are able to control for many time-invariant differences across neighborhoods but suffer, of course, from the problem that neighborhoods whose homeownership rates change are likely also to experience simultaneous changes in other features, including, importantly, the composition of residents and changes to the housing stock. Such changes could actually lead to greater bias in differenced estimates than in cross-sectional estimates.[3] This paper addresses these identification challenges with the combination of a natural experiment and highly detailed administrative data that, together, enable us to isolate the effect of homeownership and generate precise, well-controlled measures of its effect on household labor supply and neighborhoods.

We provide several additional pieces of evidence in support of our conclusions. First, we consider the possibility that buyers' labor supply changes could be specific to the public housing context if buying their units were to free tenants from strong work disincentives built into the public housing system. We conduct both a theoretical analysis of public housing rules and an empirical analysis of rents, showing that the means testing in Israeli public houisng was unlikely to have provided strong enough work disincentives to matter: public housing tenants could not lose their units by working more, rents were extremely low (usually near zero), rents increased only moderately with income and only for some subsets of tenants, and there was no discontinuous change in work incentives that coincided with the beginning of the sale event we study. This evidence suggests that our results are likely to be relevant to other policies that increase homeownership among low-income populations, for instance through subsidized loans or savings plans.

Second, we provide evidence that the improvement in neighborhood quality is unlikely to be due to the increased expected lifetime wealth of new homeowners, who received deep discounts from the government on the market price of their units. While the new owners renovate more than renters, they primarily renovate their homes' interiors and don't spend more than does the average homeowner outside of public housing. Third, we zoom in on the timing of buyers' labor supply changes to verify that they are unlikely to be due to other national policy changes that occurred during the period of study and could have affected all low income households. Finally, we show that the neighborhood price effect we estimate is robust to multiple empirical strategies, a variety of neighborhood definitions, flexible temporal controls, alternative assumptions on pre-treatment trends, alternate lag structures, exclusion of major cities, and event windows that exclude the rapid price appreciation experienced in Israel from 2008 to 2012.

The paper's results are informative for government policy world-wide. Governments encourage households to become homeowners in various ways at public expense. Many schemes to support homeownership in OECD countries target low-income (often first-time) homebuyers, granting financial assistance, as in Australia, Great Britain, Norway, and the U.S. (FHFA subsidies). Other schemes in the U.S. and the Netherlands use tax relief through mortgage interest deductions, property tax deductions, and exclusion of

[3]This greater bias in fixed effects estimates can occur because the endogenous part of the variation becomes a bigger component of the total variation used. See Ashenfelter and Rouse (1998) for a discussion.

implicit rental income; public spending to encourage homeownership in these countries in 2012 amounted to 0.5% of GDP and 2.3% of GDP, respectively (Salvi del Pero et al. (2016))). The homeownership effect we measure is the one relevant to informing policies – like the first set above – that increase ownership on the margin using financial incentives.

The substantial cost to governments of subsidizing homeownership begs justification on the basis of positive externalities. Homeowners may generate positive spillovers because of their increased financial discipline (Arbel et al. (2017); Sodini et al. (2016)), because of their incentives to invest in local amenities and social capital (DiPasquale and Glaeser (1999)), or because their greater residential stability creates a better environment for neighborhood children (Aaronson (2000); Green and White (1997); Gibbons et al. (2017)). Previous evidence from urban Peru indicates that ownership rights increase labor supply, which could potentially spur a neighborhood transition from poor to working class (Field (2007)). There is a growing wealth of evidence, more generally, that neighborhoods carry huge importance for children's long-run outcomes, suggesting potentially long-run and dynamic implications of homeownership policies if they indeed impact neighborhood character (Cutler and Glaeser (1997); Chetty et al. (2016); Chetty and Hendren (2018); Chyn (2018)).

Sodini et al. (2016), who study a natural experiment in Sweden, provide strong causal evidence that ownership has important effects on individual owners' financial outcomes, but they do not weigh in on the potential external benefits. This paper focuses specifically on neighborhood externalities, contributing to the long literature on homeownership with a new source of plausibly causal variation and careful measurement that traces effects through the behavior of individual buyers to their neighborhoods.

The paper is organized as follows. Section 2 discusses the theoretical background for homeownership effects on the quality of neighborhoods and briefly reviews the relevant literature. Section 3 describes the institutional background for our research – i.e. public housing in Israel and the process of its sale. Section 4 illustrates the effect of discounts on public housing tenants' propensity to buy their units and analyzes their labor supply response to a 2005 increase in these discounts. Section 5 gets to the heart of neighborhood externalities, describing how we employ our natural experiment at the aggregate, neighrbohood level, and the data we use to implement the strategy. Section 6 presents the results on neighborhood home price effects. Section 7 concludes.

2 Theory and Evidence on Homeownership Effects

External benefits of homeownership are thought to derive from two sources (DiPasquale and Glaeser (1999); Hoff and Sen (2005)). First, owner-occupiers are residual claimants on their home, which provides them with the incentive to invest in their property and its surroundings in ways that will increase their property's value. Second, due to transaction costs associated with selling a house, owner-occupiers are likely to be more residentially stable.[4] Increased stability both reinforces incentives to make long-run investments, lengthening the horizon over which they can be realized, and generates several other effects on the homeowners themselves, their children, and other neighborhood children.

Facing a longer horizon in a location, adults are more likely to engage in their communities, building social and civic capital. DiPasquale and Glaeser (1999) show that homeowners are more likely to know who their political representatives are, to vote in local elections, and to be members of non-professional organizations. Engelhardt et al. (2010) use randomized saving subsidies among a low-income population and find that the positive correlation between politicial involvement and ownership disappears in the causal estimates. Hilber (2010) and Ahlfeldt and Maennig (2015) support the first view, that homeownership affects political behavior and social capital investment.

Increased locational stability may also affect children of homeowners, encouraging stronger connections to neighborhood and school peers, and sparing them the administrative and emotional adjustment costs of switching schools. Green and White (1997) and Haurin et al. (2002) provide evidence that children who grow up in owner-occupied homes have better cognitive and high school graduation outcomes, and Aaronson (2000) and Galster et al. (2007) show that this effect is likely due to their reduced mobility. To the extent that parents internalize effects on their children when making home purchase decisions – and that extent is debatable – any own-children effects of homeownership are not externalities. But reduced mobility has been shown to have positive external effects on other neighborhood children, as well (Gibbons et al. (2017)).[5]

[4]Empirically, owner-occupiers are less mobile than are renters, but this difference in mobility may reflect a number of other empirical differences between the average owner and renter in addition to the different moving costs they face.

[5]It is worth discussing the repeated finding that much of the correlation between homeownership and better outcomes for children seems to be due to the increased stability that comes with homeownership, rather than to homeownership, per se. Strictly speaking, such a finding implies that policies should target residential stability rather than homeownership (when considering these particular outcomes). In practice, policymakers may have limited tools, and it's possible that encouraging homeownership is the most efficient way to encourage residential stability, given constraints. It also may be worthwhile to study more carefully policies that encourage long term rental relationships to understand better how they perform on a variety of objectives relative to homeownership policies.

Facing incentives to boost property values, homeowners may invest more in the physical structure and appearance of the home (Henderson and Ioannides (1983)). One of the most robust findings of DiPasquale and Glaeser (1999) is homeowners' increased propensity to garden. Owner-occupied properties are typically better-maintained than rental properties, potentially increasing nearby property values via amenity capitalization (Galster (1983); Harding et al. (2000)). These physical externalities are extremely localized, halving every 1000 feet (Rossi-Hansberg et al. (2010)). Numerous other pieces of research have supported the existence and highly localized nature of housing externalities – some component of which is likely to be due to physical appearance – in the contexts of foreclosure (Harding et al. (2009); Campbell et al. (2011)), low-income housing (Diamond and McQuade (2019)), and the elimination of rent control (Autor et al. (2014)).

While theory suggests that incentives provided by homeownership may lead to any of the behaviors described above, a finding that one particular behavioral effect is absent does not constitute evidence that homeownership has no spillovers. The direct approach of measuring effects on nearby home prices, which should capitalize externalities, avoids this limitation and captures the sum of all these induced behaviors. Coulson and Li (2013) take this approach and find that a 9 percentage point increase in homeownership rates within neighborhood over time leads to a 4.5% increase in prices; the result holds in OLS estimates and when instrumenting using lagged features of the neighborhood. Kortelainen and Saarimaa (2015), on the other hand, find no effects of homeownership on neighborhood prices in an urban setting in Finland when using the number of housing units per building as an instrument for homeownership.[6] If homeownership policy depends on the existence of positive externalities, then there is value to estimating directly the price parameter that summarizes the magnitude of these externalities.

This paper builds on the existing literature that estimates effects of homeownership on neighborhood house prices by exploiting a natural experiment providing quasi-random variation in neighborhood homeownership rates, providing a new source of identification. Most similar to our paper in terms of the variation used is work by Sodini et al. (2016), who take advantage of a public housing privatization in Sweden to study the private financial and consumption outcomes of new homeowners.[7] In their paper, identification comes from arbitrariness in a political process that prevented some buildings in Stockholm

[6]Their basic, non-instrumented estimates are negative, which they interpret as specific to the large co-op setting, in which more homeowners constitutes a negative externality because of the additional opinions to reconcile when making building-wide decisions.

[7]As noted in Sodini et al. (2016), the policies to increase homeownership that tend to be considered incorporate financial incentives – for example loan subsidies or assisted savings plans – and thus closely resemble the policies considered in both their paper and ours.

from privatizing. They find that new homeowners are more likely to move up the housing ladder, increase their labor supply, and save more. The extent of labor market attachment is a less-documented channel from the internal financial to the external neighborhood benefits of homeownership. Meanwhile, the degree to which residents work versus rely on welfare fundamentally affects the character and trajectory of a neighborhood. Our paper directly engages this connection, providing evidence consistent with Sodini et al. (2016) on the labor supply effects of homeownership and connecting those effects to neighborhood externalities, as measured by house prices.

3 Setting and Natural Experiment

3.1 Israeli Public Housing

As in many European countries, public housing in Israel comprised a large share of the total housing stock following World War II and the establishment of the Israeli state in 1948. Public housing in Israel was not initially targeted at low earning-ability households but, rather, was used to absorb mass migrations that required State housing solutions because of their sheer size relative to the population.[8] Housing was allocated throughout the country to a diverse set of households, comprising a quarter of the general housing stock in the 1960s and creating a public housing landscape that looked quite different from the concentrations of urban poverty and social problems typical of the modern U.S. experience.[9]

[FIGURE 1 HERE]

Since then and until 2000, the State steadily privatized public housing through sales to tenants at moderate discounts – up to 25% of the market price. In accordance with economic theory on the potential benefits of homeownership, an expressed goal of the policy was to incentivize residents' cooparation with neighborhood renewal plans, especially in the 1980s (Weinstein (2014)). Remaining available units were increasingly allocated to disadvantaged populations such as the low income and disabled (Carmon (2001)). In 2000, at the begining of the research period, these units were still located in a wide range

[8] For example, the nation's population more than doubled between 1948 and 1952, to 1.63 million from $806,000$, primarily due to immigration from Europe, the Middle East, and North Africa. In the mid-1950s and early 1960s, there were several waves from Africa and the former Soviet Union that comprised 10% of the population within 3-4 years; the largest influx from the former Soviet Union added another 7% to the population in two years alone, from 1991-92.

[9] David Ben Gurion, the first Prime Minister of Israel, expressed the goal of distributing the population throughout the country "quickly and evenly" (Weinstein (2014)).

of neighborhoods around the country, as can be seen in the map in Figure A1a. Although public housing was of course disproportionately located in low SES neighborhoods, it existed at fairly high rates in average and even above average SES neighborhoods (Figure A1b). Thus, where public housing remained by 2000, one should imagine pockets of relatively poor housing and tenants within neighborhoods that may be more or less poor as a whole.

3.2 Privatization: Variation from Discounts and their Timing

In late 1998, Parliament decided to speed up public housing privatization and passed a law granting discounts up to 85% of market value for tenants who satisfied certain criteria.[10,11] But rather than implementing the law, as passed, the government instated new sales terms at each annual budget meeting, changing discounts suddenly and meaningfully in a way that could not be anticipated by tenants (Arbel et al. (2014)).[12]

[FIGURE 2 HERE]

The terms of the implemented sales were not announced ex-ante and varied across households in ways that were quasi-random with respect to unobservables likely to affect labor market and neighborhood outcomes. The formula that determined discount sizes depended at times on region, marital status, number of children, type of rental contract, and disability, and it always depended on tenure in public housing.[13] While some of these factors are likely endogenously related to outcomes – and we control for them in our IV specifications – there are discontinuities in discount size across margins of these variables that are plausibly exogenous to outcomes. For example, discounts jump at the 2-3 child margin and at 6 and 12 years of tenure in public housing, although families on either side of these margins are otherwise arguably identical with respect to unobservables that could affect outcomes. Because each discontinuity of this sort, on its own, is not sufficiently powerful as an instrument, we use the union of them by residualizing the discount for smooth changes in the potentially endogenous determinants.

We take advantage first of the timing of discount changes, focusing on changes in outcomes when discounts rise. Figure 1a illustrates the substantial increase in discounts faced by many public housing tenants in 2005, when the "Buy Your Home" sale period

[10] Public housing law (purchasing rights), October 1998.

[11] Tenants opting not to buy their units under this plan could remain in their units as renters.

[12] Arbel et al. (2014) show, using standard time series tests, that the pattern of discounts over time follows a random walk.

[13] Details on the formulas and discounts by period are presented in Appendix Table B1.

ended and the “This is my Home” sale period began, shifting the distribution of discounts to the right. The modal discount during the “Buy Your Home” period, 2000-2004, was 7.5%. In 2005, the modal discount suddenly and unexpectedly increased to 85%, while the median discount rose from 17% to 50% (Appendix Table B2).[14]

Two additional features of the period we study (1998-2012) are helpful relative to previous privatizations: (1) there were essentially no simultaneous or trailing new additions to the public housing stock, which would move ownership rates in the opposite direction, and (2) price discounts were larger (during parts of the period) and more clearly announced to the general population of tenants. As before, tenants who chose not to buy their units remained eligible to stay in their units as renters.

4 New Homeowners and Changes in Labor Supply

This setting provides us with a natural experiment in which public housing units, previously rented to tenants, become privately owned units. Before measuring how these changes in ownership may affect neighborhoods, we first assess the experiment at the household level, measuring the extent to which the government-set discounts affect purchases and households' labor market behavior, which could importantly affect neighborhood character. In Section 5 below, we then discuss how we aggregate these household level purchase decisions into changes in neighborhood homeownership rates.

4.1 Effects of Discounts on Public Housing Sales

We illustrate the effects of discounts on household purchases in two ways. Figure 2 is a binned scatter plot showing the relationship between discount sizes and the predicted probability of buying one's unit, conditional on household characteristics (including those that enter the discount formula), such as age, marital status, number of children, region, and disability. Discount size strongly and positively predicts buying, with a coefficient of 0.51. This evidence suggests the possibility of using these discounts as an instrument for homeownership changes that occurred as a result of the large scale privatization of public housing.

[FIGURE 3 HERE]

[14] We don't conduct analogous analyses of the first and third sale periods, “Buy Your Home” and “My Own Apartment” because we don't have data from before the first period, and because there was no significant increase in discounts and buying between the second and third periods.

Second, as a preliminary step in the labor supply analysis, below, we estimate the probability of becoming a homeowner during any of the "This is My Home" sale period years (2005-2008) as a function of the discount, residualized for smooth changes in household covariates. The estimation is described in detail in Section 4.2 and estimates from the probability models are presented in Appendix Table A1. In all specifications, discounts are a strong and significant predictor of purchase, with a t-stat over 14 when discount is included linearly.

4.2 Effects on Buyers' Labor Supply

We take four approaches to comparing the extensive and intensive margin labor supply behavior of buyers and non-buyers. In each case, we focus on the years just before and after the 2005 beginning of the "This is My Home" sale event, exploiting the sharp increase in discounts in this sale (Figure 1a).[15] First, we compare raw averages of employment and labor income in each year. Second, we estimate OLS difference-in-differences models, explicitly comparing the employment and labor income of buyers to those of non-buyers, after the beginning of the sale event relative to before, and including household fixed effects to control for time-invariant features of a household that may affect labor market behavior.

$$y_{ht} = \gamma + \pi(I_t^{post} \times I_h^{buyer}) + X_{ht}\psi + \theta_h + \delta_t + \varepsilon_{ht} \tag{1}$$

In this specification, y_{ht} represents an outcome (employment or log labor income) for household h in year t, I_t^{post} is an indicator equalling 1 after the beginning of the sale event (2005 and later), I_h^{buyer} is an indicator equalling one for households that bought their unit during this sale event, X_{ht} are time-varying household characteristics, θ_h are household fixed effects, δ_t are year fixed effects, and ε_{ht} is an idiosyncratic error term. Because a household's purchase date is in principle endogenous, while the beginning of the sale event is exogenous to the household's labor supply, the latter is used as the event date. The coefficient of interest, π, reflects buyers' changes in labor supply relative to never-buyers when the sale period begins, where $\pi > 0$ implies a positive impact of homeownership on labor supply.

Of course, because buying one's home is endogenous, this estimate may be biased even with household fixed effects to control for time-invariant unobservables. Our third

[15] The "after" period is defined relative to the beginning of the sale period and not relative to the date of purchase, since purchase date varies across households, is endogenous, and is undefined for non-buyer households. We cannot use this methodology to study the "Buy Your Home" sale, which started in 2000, since we don't observe employment and income information prior.

approach to identifying the homeownership effect is to use the natural experiment provided by discounts to public housing units' sale prices in an instrumental variables framework.[16] As noted in Angrist and Pischke (2009), the second stage of a two-stage instrumental variables procedure is inconsistent when the first stage is estimated non-linearly, as may be desired in cases like ours with a binary endogenous regressor. Their proposed solution is to conduct a preliminary step ("step zero") in which predicted values of the endogenous regressor are generated from a non-linear estimation. These predicted values are then used as instruments in the normal 2sls procedure.

Since our endogenous regressor in equation 1 is $I_t^{post} \times I_h^{buyer}$, we first estimate the probability of buying between 2005 and 2008 as a function of the average discount a household faces during that period, as shown in column (5) of Appendix Table A1.[17] Using the covariate-residualized discount coefficients, we calculate the predicted probability of buying during the "This is My Home" sale period, $\widehat{I_h^{buyer}}^{step0}$ and proceed with a standard 2SLS procedure in which this predicted probability is our instrument for being a buyer in the first stage:

$$I_t^{post} \times I_h^{buyer} = \gamma_1 + \pi_1 \left(I_t^{post} \times \widehat{I_h^{buyer}}^{step0} \right) + X_{1ht}\psi_1 + \theta_{1h} + \delta_{1t} + \varepsilon_{1ht}$$

and the second-stage equation is:

$$y_{ht} = \gamma_2 + \pi_2 \left(\widehat{I_t^{post} \times I_h^{buyer}} \right) + X_{2ht}\psi_2 + \theta_{2h} + \delta_{2t} + \varepsilon_{2ht} \tag{2}$$

where the labor market outcome y_{ht} is estimated as a function of the instrumented interaction between the indicator variables for buyer and post sale period start.[18] As in equation 1, both stages of the 2SLS include controls for time invariant household characteristics in the household fixed effect, θ_h, time varying household characteristics, X_{ht}, and year fixed effects, δ_t. π_2 reflects the differential labor supply after the beginning of the sale period relative to before, for buyers relative to non-buyers, and it is again expected to positively predict employment and labor income if homeownership has positive effects

[16]If households have quasi-linear utility, then the variation in discounts will not directly affect their tradeoff between consumption and leisure.

[17]We have estimated this equation by both a linear probability model and by probit, and the final IV estimates are similar across specifications. Note that the predicted purchase probability computed uses only remaining variation in discounts after having residualized for these covariates.

[18]Household fixed effects absorb the main effect of being a homeowner. The step 0 predictions, $\widehat{I_h^{buyer}}^{step0}$, are interacted with a dummy variable equalling 1 after the beginning of the sale period to instrument for the post x buyer interaction in the equation of interest.

on labor supply.

The fourth estimation approach uses matching estimators (nearest neighbor) to calculate treatment effects by comparing buyers and non-buyers that are ex-ante similar (in 2004) on observables. Matching is conducted on available demographics, including age, marital status, number of kids, immigration year, disability status, and geographic area. The limitation of this method is that buyers and non-buyers may of course differ on unobservables as well as on the obervables that generate matched comparisons. This approach is meant to complement the others we take, such that we can provide a collage of evidence on the labor supply effects of homeownership. We follow Ichino et al. (2017) in implementing the matching estimation by generating the matched controls and then estimating equation 1 with a fixed effect for each matched set.

4.3 Data

4.3.1 Public Housing Records (Amidar and Amigur)

Amidar and Amigur, Israel's public housing authorities, provide us with data for the years 1960-2012 on more than 90% of all public housing units in Israel, their physical characteristics (including address), their tenants in each year, their rental rates and payments received, and the details of their sales to tenants – privatizations. From the data sets provided, we construct a panel of public housing units by detailed location, with time of sale (if any), linked to the identity of the tenants/owners in each year.

4.3.2 Ministry of Housing Memos

Numerous archived memos from the Ministry of Housing contain the complex rules in each year that determined the sale price discounts tenants faced for their units. Additional memos (along with email clarifications) detail the rules determining rents in public housing, discussed in more detail in Section 4.5.1.

4.3.3 Social Security Data from the National Insurance Institute (NII)

Because the public housing data include national identity numbers of tenants and their spouses and kids, we are able to match tenants to social security data. These confidential data, accessible only from the NII's research room, contain information on individuals' employment and labor income in addition to containing demographic information, such as disability status, date of birth, and date of immigration. As such, the NII data – available annually from 2000-2012 – facilitate the calculation of the discounts faced by each tenant in each year, given the government formula, and facilitate our analysis of the labor supply behavior of tenants offered the opportunity to buy their units.

[TABLE 1 HERE]

4.4 Results: Employment and Labor Income

We begin by examining buyers' and non-buyers' observables to get a sense of the extent to which buyers may be selected. A comparison of these two groups' demographics indicates that they differ along a number of dimensions (Table 1, Panel A). To reduce selection inherent in our comparisons, we first restrict our analyses to subsamples that are more homogeneous – albeit not identical – on observables. We generate these subsamples using a standard propensity score matching procedure following Rosenbaum and Rubin (1984) and using ex-ante household demographics as inputs.[19]

[FIGURE 4 HERE]

While in the full, balanced sample of working age tenants (25-60), buyers appear considerably stronger than non-buyers (Panel A), the two groups are more similar in the restricted samples we use for estimation (Panels B and C). In our primary estimation sample – the 25th-75th propensity score percentile common support sample described in Panel C – there are no remaining differences in age, immigration status, disability, or ex-ante employment. Buyers are slightly more likely to be single parents, and they face higher sale discounts as a result of having spent more years in public housing and having slightly more kids on average. Within the restricted samples, we implement the four identification strategies described in Section 4.2. We use the most conservative subsample in our main analysis and present results using additional, larger subsamples in Appendix A.

Importantly, we observe significant numbers and similar proportions of buyers and non-buyers at each propensity score included in the restricted samples, as shown in Figure A3. A sample restricted to the 25th to 75th percentiles of propensity to buy excludes tenants who, based on observables, are near-certain buyers or non-buyers, leaving a substantial proportion of each group at each propensity score in the common support. At the extreme low end propensity score of 0.3 are 4.5% of the sample's buyers and 5.2% of the sample's non-buyers, while at the extreme high end propensity score of nearly 0.4 are 5.5% of the sample's buyers and 4.6% of the sample's non-buyers.

[19] We match on age, marital status, number of children, year of immigration, disability, and geographic region as of 2004, the year before the start of the "This is My Home" sale event.

[TABLE 2 HERE]

We first examine the extensive margin of labor supply, estimating equations 1 and 2 with an indicator for employment in each year on the left hand side. Table 2 presents four estimates of the effect of homeownership on long-term employment, defined as employment for at least 6 months. Estimates indicate a significant increase in long-term employment for new homeowners of 4.4-7.6%, depending on the specification, relative to non-buyers. In columns (1) and (2), the regressions include household fixed effects and thus control for unobservable time-invariant differences between households in addition to controlling for observable time-varying differences, such as number of kids under 18, marital status, years since immigration, disability rating, and the regional unemployment rate.[20] Columns (3) and (4) replace household fixed effects with matched set fixed effects, ensuring that new homeowners are compared to similar non-buyers.

[TABLE 3 HERE]

Table 3 presents analogous results for the intensive margin of labor supply, estimating effects on log labor income.[21] Estimates suggest a positive and significant effect on the intensive margin as well. New homeowners experience an increase in labor income of 12-13% relative to non-buyers after the beginning of the sale period.[22]

[FIGURE 5 HERE]

Figures 3 and 4 detail the timing of these effects more clearly. The figures each present the evolution of the treatment effect by year under each of the four methodoligies used. Reassuringly, a similar picture emerges with each methodology: treatment effects for both employment and labor income are relatively flat around zero before 2005 when the sale event begins, they rise quickly at the start of the event for several years and then flatten out. Panel (a) of each figure shows the year-by-year average employment and labor income separately for buyers and non-buyers in the 25th-75th propensity score percentile common support sample with no controls. In Figure 3a, buyers and non-buyers have the same average long-term employment rates before the start of the sale event, but they diverge thereafter, with buyers' long-term employment rates rising faster and to a higher

[20] First stage estimates from the IV specification in column (2) are presented in Appendix Table A2.

[21] Israeli data sets generally lack information on hours worked, so one must rely on either months worked or labor income to measure intensive margin labor supply. Effects on months worked are substanstively similar to those for labor income but don't capture the key aspect of intensivity of work within a period.

[22] Estimates using a more relaxed nearest neighbor strategy with 3 control units for each treated unit generate larger estimates, around 17%. Extensive and intensive margin results for the 10th-90th percentile common support sample are presented in Appendix Tables A3 and A4.

steady-state level several years after the event start. This pattern holds up under the more controlled comparisons in Panels 3b- 3d using OLS fixed effects, IV, and matching estimators. While the household labor income of eventual buyers starts out higher than that of non-buyers in the raw averages (Figure 4a), the two groups' trends are parallel until the event start, at which point buyers' labor income again rises relative to that of non-buyers. This pattern strengthens in the more controlled comparisons in Panels 4b-4d. These patterns are not particular to this sample; similar and even stronger effects are apparent for both employment and labor income in the 10th to 90th propensity score percentile common support sample (Appendix Figures A4 and A5).

[FIGURE 6 HERE]

The absence of pre-treatment trends lends confidence to the notion that these labor supply effects are due to the new homeownership caused by the sale event rather than to pre-existing differences between the households that eventually bought their homes and those that didn't. In addition to the standard statistical assessment that treatment effects before the event date cannot be distinguished from zero, we implement the robustness procedure recommended in Rambachan and Roth (2019) to allow for the possibility that the assumption of parallel pre-treatment trends may not hold *exactly*. Choosing a graph that is closest to exhibiting a slightly positive pre-treatment trend, and allowing for a non-linear pre-treatment trend, we estimate confidence intervals for post-treatment coefficients in the OLS fixed effect model for employment. Results are presented in Appendix Figure A10a; the treatment effect remains significantly positive two years after treatment even allowing for some degree of differential non-linear pre-treatment trends. Choosing a graph with quite a flat pre-treatment trend but with larger standard errors, and again allowing for a non-linear pre-treatment trend, we estimate confidence intervals for post treatmenet coefficients in the NNM employment model. Results in Appendix Figure A10b indicate the treatment effect in 2006 is robust to non-linearity of differential trends equal to up to 0.005, which is equal to the greatest deviation from average slope observed before treatment.

Because labor supply effects appear immediately after the beginning of the sale event, while buyers in the "This is My Home" sale event buy all the way through 2008, it is unlikely that the change is due to sudden, unexpected financial pressure from new mortgage payments and other home maintenance expenses. The two more logical – and very closely related – interpretations paint homeownership in a much more positive light. One is that, presented with a newly feasible opportunity to become homeowners, some public housing tenants are motivated to increase their labor supply in hopes of being able to afford the purchase. The second explanation is that the possibility of becoming homeowners gives these households a greater sense of agency over their lives and makes a bright future seem more attainable. In that view, working and earning more now has higher returns: in addition to the monetary gains, which were always available, working now has the potential to open the door to an alternative future for buyers and their children. This sort of soft incentive, or "new outlook" view of what makes some disadvantaged families capitalize on opportunities to advance has drawn increasing research attention; Bergman et al. (2019) show in a field experiment in Seattle that making an opportunity *feel* more attainable is even more effective than financial incentives or informational interventions in driving take-up.

4.5 Evaluating Work Incentives in Public Housing

The evidence on labor supply strongly supports the notion that these new homeowners, relative to similar non-buyers, increase their labor supply on both the extensive and intensive margins. Because many social programs worldwide disincentivize work by conditioning benefit receipt on non-employment or low income, one question that naturally arises is whether these changes in labor market behavior may have occurred due to buyers' new freedom from public housing rules rather than to homeownership, per se. This section evaluates this possibility by examining both the official public housing rules and the de facto benefit receipt and rent level outcomes for households who increased their labor supply.

4.5.1 Public Housing Rules and Rent Setting

Tenants are not removed from public housing in Israel except in extreme cases, such as non-payment of rent for an extended period. This fact is known and acknowledged by public sector economists and practitioners, including those at the Bank of Israel, the Ministry of Finance, and the Ministry of Housing.[23] In addition, not once in the many pages of rules on public housing rent and discount eligibility is there a statement that a tenant could lose his eligibility to remain in his unit. Tenants would thus not realistically fear losing their public housing if they were to work more. Nevertheless, these tenants do pay monthly rent to their public housing administrator (company), so to understand the

[23] In Israel, the legal environment makes it exceedingly difficult to evict tenants even from non-public rental housing. Evictions from public housing are extraordinarily rare, and tenants would be aware of that.

incentives faced, we must evaluate the official determinants of public housing rent.[24]

Rent determination during the research period was based officially on a "market rent" and a set of conditions that determine the size of the discount a household receives from this market rent. Market rents were assessed once in the 1980s and then again in 2005, when assessed values rose, and we refer to these simply as "benchmark rents," since they were in fact subsidized – below market both before and after the updated assessments. The remainder of this section discusses the conditions for receiving either the maximum discount, which was 91.5%, a smaller discount, or no discount at all, such that the household would pay the benchmark rent. There are two challenges in inferring from the official rules the incentives tenants faced: first, the rules are complex (incomprehensible, in places), and second, the Ministry of Housing has acknowledged that the rules were not enforced with the frequency or stringency delineated. For these reasons, we provide an empirical assessment below in Section 4.5.2 to clarify the reality on the ground.

For determination of rent discounts, tenants are divided into two groups: those who receive the maximum discount automatically due to their status with the National Insurance Institute (NII), and those who have to pass both an income test and an employment test in order to receive a discount. The first group consists of households having either a member with a disability rating of at least 75% or a member who recieves the maximum income support benefit.[25,26] The second group consists of households with lower or no disability ratings or income support; households in this group could get some discount so long as they both "fulfill their earning capacity" and have income no higher than 125% of the maximum income threshold for income support. These thresholds are relatively high: the income threshold for a single parent with two children was 150% of the minimum monthly wage in 2003 (National Insurance Institute of Israel (2004)). Importantly, a "low" income is not enough to qualify households for a rent discount; in addition, they must "fulfill their earning capacity," either by having a full-time job at minimum or higher wage or by indicating official inability to work (receiving disability, income support, holocaust survivor allowance, or alimony from the NII). In other words, tenants who did not

[24] This section is based on several official documents from the Ministry of Housing and the Knesset Research Center (Ministry of Construction and Housing (2002, 2011); Mei Ami (2005)), in addition to numerous email exhanges, phone conversations, and in-person meetings with members of the Ministry of Housing, the Ministry of Finance, and Amidar.

[25] Of course, receipt of income support, or welfare payments, requires its own income and employment tests – candidates have to have income below some threshold and prove either having a job, undertaking a bona fide job search, or having a job with low pay.

[26] A number of conditions could disqualify a tenant from receiving the maximum discount, though even then he could receive a smaller discount: owning a car or house, not making proper use of the apartment, living in an apartment that is "too large" and having refused two offers to trade, non-cooperation with the public housing inspector, and not filling out the annual discount request form.

qualify for welfare were actually *required* to work in order to receive rent discounts.

As of 2005, 67% of tenants received the maximum rent discount, 16% received a smaller discount, and the remaining 17% paid the benchmark rent. The Ministry of Housing has been unable to provide the discount schedule used for discounts between 0% and 90% pre-2005 except to say that sub-maximum discounts were mostly between 68-83%.

In November 2005, market values were re-assessed for the first time in 20 years, and a set of rules was determined for how rents would adjust to the new, higher market rates.[27] For those who were highly disabled and welfare-dependent, rent would not adjust to the new market rates.[28] More able, higher earning tenants who received some rent discount, though not the maximal one, could experience *annual* rent increases of 50 NIS per month (~$11 USD). Average family labor income for public housing tenants who worked at least one hour during the year, meanwhile, was $950 per month and average welfare income was $330 per month. As with the rules above, these adjustments to benchmark would primarily affect tenent incentives on the intensive margin, since tenants in the affected portion of the distribution would have had to pass an employment test. During the years studied, 50 NIS constituted between 2 and 3 hours of work at minimum wage. At the high end, the most able, highest earning tenants could experience one large upward rent adjustment to the new benchmark of at most 350 NIS (~$78 USD). Of course, as was the case before 2006, if the tenant was found not to be fulfulling his maximum earning capacity, he could also lose eligibility for a discount.

The implication of these rent determination rules is that disincentives to work on the extensive margin were non-existent, and disincentives to increase earnings existed but were weak. Those with a highly disabled household member would not see rent increases under any circumstances, while those without would have to experience very substantial increases in household income to experience modest increases in rent; net income would rise after about 2.5 hours of additional minimum wage work per month. In practice, as we describe in the next section, the loose implementation of the rules likely weakened any existing disincentives further.

4.5.2 Rent Increases in Public Housing Data

Because we observe actual rents paid by tenants, we can empirically assess the extent to which rent determination was likely to affect labor supply decisions. In doing so, we consider absolute rent levels, the significance of rent paid relative to both labor income

[27]Note that only incumbent tenants that have been in public housing since at least 2000 are in our analysis since we have a balanced panel. Thus, rules relevant to tenants entering public housing post November 2005 are not relevant for us.

[28]A complete description of the rules pre- and post-2005 is presented in Appendix C.

and income from all sources, year-to-year changes in monthly rents over the period, and the extent to which rent seems to increase for a given household when labor income rises.

[FIGURE 7 HERE]

Figure A6 presents kernel density plots of the rent distrbution for all sample households in several different years of the sample. Panel A6a shows the distribution in the first year of the sample and the last year of the "This is My Home" sale period, and panel A6b shows the years around the November 2005 change in the rent rules. The vast majority of households pay near-zero rent: the distributions peak in all years just above 0, between 50 and 150 NIS (~$11-32 USD). 85-90% of the sample in all years has a monthly rent below 500 NIS (~$109 USD). The highest rents seen with any frequency reach 1,500 NIS per month, or $326 USD. These amounts are exceedingly small even relative to the low incomes of public housing households, which average $950 USD per month *before* transfers.[29] There is a slight rightward shift of the distribution over the whole period from 2000 to 2008 (panel A6a), and the distribution only barely changes just after the new benchmarks and associated adjusment rules were announced in November 2005 (panel A6b).[30]

[FIGURE 8 HERE]

To understand the labor supply incentives tenants faced, we next examine how rents increased with income and how this relationship may have changed over time. Since households with a disabled member received the maximum discount regardless of employment or earnings, we take a conservative approach and first consider non-disabled households who in theory face stronger disincentives. For this group, a 1,000 NIS ($217) increase in monthly income generated on average a 2.5 NIS ($0.54) monthly rent increase. The elasticity of rent with respect to income increased somewhat over time, as can be seen in Figure 5a, which presents the differential log rent-log labor income relationship in each year. Next consider a typical public housing family with 3 or more children. For this group, the same 1,000 NIS ($217) increase in monthly income generated on average a 1.9 NIS ($0.41) monthly rent increase. The cross-year average elasticity of rent with respect to income for this typical family type was 6.6%. Figure 5b shows a graph analogous to that next to it, again indicating a nearly perfectly linear and relatively flat pattern over time, suggesting a slow and stable strengthening of the relationship between incomes and rents

[29] Welfare payments average another $330/mo but of course negatively covary with labor income.

[30] In addition, 2006 is early enough such that buyers have not yet attrited from the sample of rent payers in meaningful numbers.

for both instructive groups. Importantly, there is no sharp change in this relationship in 2005, when the "This is My Home" sale period began.

The empirical evidence suggests that work disincentives from the system of public housing rent determination were extremely weak. Rent levels were low in both absolute terms and relative to income, rents increased only slightly with income, and the rent-income gradient did not change sharply in the event window. Neither the rent determination rules nor their de facto implementation seemed to discourage employment or earnings in any meaningful way.

4.6 Timing of Effects and Other Policy Changes

The event studies shown in Figures 3, 4, A4, and A5 show clearly that the increase in labor supply began in 2005, the first year of the sale event we study. Because the government made changes to aspects of Israel's National Insurance Institute (NII) benefits structure between 2002 and June 2003, we dig deeper into the timing of effects to be sure that the labor supply effects we measure are most likely to be due to homeownership rather than to these other policy changes. To explain the effect we measure, the policy changes would have to differentially affect buyers relative to non-buyers and they would do so when the policy changes took effect in 2003, prior to the beginning of the 2005 sale event.

For the purposes of understanding labor supply incentives, the most relevant set of changes from the 2002-2003 NII reform were those related to income support benefits. For certain groups, employment bureau check-in requirements were changed, and for many, the maximum benefit amounts were reduced. We summarize the relevant changes in Appendix D and test here for the possibility that this reform could confound our estimates by differentially affecting buyers and non-buyers.[31] We follow the form of the analysis delineated in Section 4.2 and focus on the timing of the labor supply effects, estimating effects in shorter windows around the real event date, 2005, and a placebo event date coinciding with the NII reform in 2003.

[TABLE 4 HERE]

[TABLE 5 HERE]

Table 4 shows extensive and intensive margin labor supply results estimated in two different event windows. Panel A shows estimates of effects around the "placebo" event

[31] A complete description of the reform can be found in National Insurance Institute of Israel (2004).

of the welfare reform of 2002-2003, examining the years 2001-2004 and defining the post-treatment years to be 2003 and 2004.[32] Effects on employment and log labor income are each estimated in both the 10th-90th propensity score percentile sample (columns (1) and (3)) and the 25th-75th percentile sample (columns (2) and (4)). Estimates in all four columns of Panel A are near zero and not significant, indicating that new homeowners did not increase their labor supply on either the extensive or intensive margin relative to non-buyers just after the welfare reform. Meanwhile, Panel B shows results of the same regression for a window of the same length around the 2005 beginning of the "This is My Home" sale event (2003-2006), defining the post-treatment years to be 2005 and 2006. In contrast to the results in Panel A, these results indicate a positive and significant increase in both employment (2.96%, column (2)) and labor income (11.9%, column (4)) for new homeowners relative to non-buyers just after the beginning of the event. The magnitudes are similar across samples, suggesting the robustness of the result to slight changes in degree of comparability of buyers and non-buyers included in the analysis.

Table 5 zooms in further on a one-year window around placebo (2003) and actual (2005) event dates. To study the placebo event, we compare 2004 to 2002 since elements of the reform may have been implemented with staggered timing between January and June 2003. This short window vastly reduces our sample size and power, especially in the reduced 25th-75th percentile sample, but we nevertheless find positive and significant effects on the extensive and intensive margins for the real event. Around the welfare reform date, we again find near-zero and insignificant effects.

These results complement those in Figures 3, 4, A4, and A5 in pinning down the timing with which these labor supply changes occurred differentially for new homeowners. Both sets of results indicate a significant change after 2005 but not after 2003.

5 Measuring Neighborhood Effects

Having illustrated the workings of our natural experiment on the micro, household level, we now turn to aggregating these individual home purchase decisions into neighborhood level changes in homeownership rates.

[32]Some of the relevant changes began in January 2003, while others were implemented in June 2003; many were announced in 2002 legislation.

5.1 Empirical Strategy

Homeownership is likely to be correlated with unobservable features of residents and of the housing stock that also affect neighborhood quality, and *changes* in homeownership within a neighborhood are likely to be accompanied (and perhaps caused) by more general neighborhood change. The privatization of Israeli public housing in the 2000s generates ownership changes while holding constant the residents and housing for which tenure status changes, addressing these primary sources of bias in traditional estimates of external homeownership effects.[33] In addition, these changes often occur in clusters because of the initial location of public housing units, thus treating nearby always-private units with meaningful changes in local homeownership rates. Measuring effects on the value of homes that were always privately held effectively isolates effects external to new buyers.[34]

The dispersal of public housing across Israel, discussed in detail in Section 3, created a landscape in which many neighborhoods at various SES levels across the country contained public housing units at the beginning of our research period in 2000. Neighborhoods that had significant public housing shares in 2000 could experience large increases in homeownership rates by 2012 due only to privatizations of public units. In 2000, 16% of statistical areas (SAs) with public housing in Israel had a public housing share of at least 15%, and 10% of SAs had a share of at least 25%. Many of these areas indeed experienced large increases in homeownership rates by 2012; zooming in geographically on the clusters of public units within these neighborhoods of course generates even larger initial shares and thus potential treatment dosages in the clusters' vicinity.

[FIGURE 9 HERE]

To illustrate the types of changes we observe, Figure 6a depicts a map of Be'er Sheva, a large city in southern Israel, home to a relatively large amount of public housing in 2000. Zooming in on a particular SA in Figures 6c and 6d, in which solid pentagons represent public housing units and hollow pentagons represent privatized public housing units, one can see the significant change in homeownership experienced by this SA over the period from 2000 to 2012. In the vicinity of these public housing units, of course, there are also units that were always privately held and that may be affected by the new private ownership of previously rented nearby public units. Figure 6b shows, with green triangles, the locations of these private units that were sold during the period such that

[33] Sodini et al. (2016) study a similar privatization in Sweden, but they examine private financial and labor supply outcomes only. They also find positive labor supply effects of homeownership.

[34] This partitioning should actually provide a lower bound on the external effects, since each unit privatized can affect all other public housing units as well as nearby private units.

their transaction prices can be observed. The red lines and circles indicate the various ways in which we can define neighborhoods to estimate effects – within the larger Statistical Areas set by the Central Bureau of Statistics (external borders), at the block level also set by the CBS (internal borders), or at abitrary radii around private transactions, which we refer to as buffers (red circles) and use in our analysis at radius 100 meters. In all of our analyses, the variation used comes from the increase in homeownership rate in the neighborhood as the local public units are purchased by tenants, and the outcomes are measured exclusively by prices of the always-private units represented by green triangles.

Figure 7 shows the substantial increase in public housing homeownership rates over the research period across statistical areas. One can clearly see the effects of increased discounts near the beginnig of the first two sale events, "Buy Your Home" and "This is My Home." Homeownership rates of initially-public housing rose over the period from 0% to 35% on average, exposing their always-private neighbors to substantial changes in nearby homeownership.

[FIGURE 10 HERE]

We take several empirical approaches to leveraging this natural experiment. First, we estimate neighborhood fixed effects models by OLS, using the following equation:

$$p_{int} = \alpha + \beta HR_{n,t-2} + \eta_n + \rho s_{2n,t-2} + Z_i\phi + \delta_t + \varepsilon_{int} \tag{3}$$

where p_{int} is the log transaction price of always-private units i transacted in quarter t and neighborhood n, $HR_{n,(t-2)}$ is the two-quarter lagged homeownership rate in neighborhood n, η_n are neighborhood fixed effects, $s_{2n,t-2}$ reflects lagged building starts, Z_i is a vector of hedonic characteristics of the private housing unit sold, δ_t are quarter fixed effects, and ε_{int} is an idiosyncratic error term. The homeownership rate is lagged to allow time for it to affect prices.[35] Because the specification includes neighborhood fixed effects, the coefficient β estimates the effect of a change in homeownership rate on prices of nearby always-private homes, residualized for differences in their physical features. β is expected to be positive if increases in the homeownership rate improve neighborhood quality.

Since there may be selection among public housing tenants into buying, and since this selection could potentially vary by neighborhood, changes in homeownership rates may be correlated with other features of the residents and location that may also affect price growth. To address this potential endogeneity, we take advantage of the discounts set by government formula that reduced the purchase price tenants faced. As described in

[35] In practice, results are not sensitive to the exact timing.

detail in Section 3, these discounts varied substantially over time without prior notice, as the government altered its formula, and across households at any point in time according in large part to their tenure in public housing, number of kids, and region, for example. We control for smooth changes in these discount-determining demographics and take advantage of discontinuous jumps in discounts along specific margins – such as those from 2 to 3 kids or 11 to 12 years of tenure – that are plausibly exogenous to neighborhood outcomes and, together, predict changes in neighborhood homeownership rates.[36]

Formally, we proceed in three steps. Since discounts operate at the household level, and since a given household's purchase is not independent across periods, we first estimate a household level hazard model of the probability to purchase in each period, where discounts affect this probability:

$$\lambda_{hant}(t) = \lambda_0(t)exp(\gamma_1 D_{hnt} + \theta_{1n} + X_h\gamma_2 + Z_a\gamma_3 + \delta_{1t}) \tag{4}$$

Here, $\lambda_{hant}(t)$ is the hazard that the public housing unit a of household h in neighborhood n will be sold in any quarter t, D_{hnt} is the discount faced by that household in that period, θ_{1n} is a geographic area fixed effect, X_h are household characteristics (age, family status, number of kids, disability status), Z_a are hedonic characteristics of the public housing unit (floor space, number of rooms, floor of building, building age) and δ_{1t} are quarter fixed effects. We calculate the discount faced using the government formula in each period.[37]

The coefficient estimates of this hazard model are presented in Appendix Table A5. Discount size strongly and positively affects buying; the t-statistic in specification (2), used for calculating predicted probabilities of sale, is 27.8. The unit's hedonic characteristics and many of the household's characteristics also significantly affect the log hazard ratio, though the effect of the discount remains highly important across specifications. This result supports the strength of the discount as an instrument.

Having used the demographics-residualized discount coefficients in this model to calculate predicted probabilities of sale for each unit by each quarter t of the sample period, we aggregate these probabilities to generate predicted homeownership rates for each neighborhood in each quarter:

[36]See Appendix B for more details on the discount formulas and the resulting discounts.

[37]We observe the discount given to each "buyer" household in practice, but (1) non-buyers, of course, have no such implemented discount, and (2) actual discounts differ slightly from our formula-calculated discounts, and we have no way of knowing whether these differences constitute random errors or endogenous deviations. We thus use the discounts we calculate using the relevant formula for each period.

$$\Lambda_{n,t} = \frac{\sum_{h \in n} \left(1 - e^{-\sum_{j=0}^{t} \lambda(j)}\right)}{phstock_{n,t=0}} \tag{5}$$

where $phstock_{n,t=0}$ is the number of public housing units in the neighborhood at the beginning of the period, and the numerator is the sum across public housing units h in neighborhood n of the cumulative probability through period t of each unit being sold. The fraction $\Lambda_{n,t}$ represents the predicted share of initially public housing that is owned in period t due to discounts. We then use this predicted value as the excluded instrument in a standard 2SLS procedure, in which the first stage predicts potentially endogenous homeownership rates:

$$HR_{in,t-2} = \alpha_1 + \beta_1 \Lambda_{n,t-2} + \eta_{1n} + \rho_1 s_{1n,t-2} + Z_{1i}\phi_1 + \delta_{1t} + \nu_{in,t-2}$$

and the second-stage equation predicting log transaction price is:

$$p_{int} = \alpha_2 + \beta_2 \widehat{HR}_{n,t-2} + \eta_{2n} + \rho_2 s_{2n,t-2} + Z_{2i}\phi_2 + \delta_{2t} + \varepsilon_{int} \tag{6}$$

Both the first and second stages control for the hedonic characteristics, Z_i, of the private unit i transacted as well as neighborhood and quarter fixed effects (η_n and δ_t), such that β_2 is identified off within-neighborhood changes in homeownership rates. At the statistical area level, we measure and control for lagged building starts, $s_{n,t-2}$, to account for supply side changes. Note the different indexing of units in the hazard model versus the (log) price equations; the hazard model relates to sales of public housing units, h, while the price equation relates to always-private housing units, i. Again, β_2 is expected to be positive if increases in homeownership rates improve neighborhood quality. If the OLS estimates are biased upwards due to selection of the highest quality tenants into buying, or because tenants are more likely to buy in places where they expect the highest price growth, one might expect $\beta_2 < \beta$.

We take one additional empirical approach. Neighborhoods that looked similar prior to the sale periods of the 2000s experienced, in some cases, very different changes in homeownership rates over the decade. In this analysis, we consider only localities (towns) that contain both a neighborhood that experienced a large increase in homeownership and one that experienced a small increase in homeownership, despite beginning with similar concentrations of public housing, sharing similar observables, and being located nearby one another. We conduct a difference-in-differences analysis comparing a town's "large change" to "small change" places at the end of the period relative to the beginning, using the beginning of the "This is My Home" sale as an event study, just as we did in Section 4, and relying on just the time variation in discounts. We expect private housing prices to rise more in the "large change" (but otherwise similar) places if homeownership rates positively affect neighborhood quality ($\pi_2 > 0$, below). We estimate:

$$p_{ilnt} = \alpha_0 + \pi_1 I_n^{lgchg} + \pi_2 \big(I_t^{post} \times I_n^{lgchg}\big) + \eta_l + Z_i \phi_3 + \delta_t + \varepsilon_{ilnt} \tag{7}$$

This specification includes town fixed effects η_l, quarter fixed effects δ_t, and controls for the transacted private home's hedonic characteristics, Z_i.

Although the estimates from this specification will not be directly comparable in magnitude to the OLS and IV estimates from equations 3 and 6, they represent a useful alternative approach that relies on time variation in discounts and need not rely on cross-household variation in discounts. Under the assumption that large change and small change neighborhoods in the same town followed parallel price trends before discounts were raised in 2005, this strategy identifies the price effects of experiencing a large change in homeownership rates when estimated by an OLS fixed effects model.

As an instructive comparison, we additionally estimate this specification explicitly using discounts to instrument for I_n^{lgchg}, a neighborhood experiencing a large increase in homeownership rates. Since these estimates use both cross sectional and time variation in discounts, while the OLS estimates use only time variation, the comparison between estimates can be informative about the importance of the cross sectional variation.

5.2 Data

To measure external neighbohood effects, we use two data sources in addition to those described in Section 4.3.

5.2.1 Israeli Tax Authority (ITA) Data on Housing Transactions

The confidential Carmen data of the Israeli Tax Authority contains administrative information on all housing transactions in Israel since 1998. Since the data are of higher quality since 2000, we focus on the period from 2000-2012 and use transactions in localities with public housing. Records include transaction prices and dates, units' physical characteristics (floor space, number of rooms, age of building, etc.), and detailed geographic location (addresses).[38] These data serve as our measure of neighborhood quality, since they allow us to observe price changes of private homes in public housing neighborhoods that may or may not have significant privatization. Although it is rare that we observe repeat sales, we are able to control for units' hedonic characteristics such that changes in the residual over time relfect changes in average quality of the neighborhood.

Transactions are frequent in the neighborhoods we study. At the statistical area (SA) level, we observe on average 24.75 transactions per neighborhood-year (median: 21), while at the block level we observe 19 on average (median: 10).[39]

5.2.2 Israeli Census Data

Israeli Census data contain a 20% random sample of the population in the years 1995 and 2008, providing characteristics of neighborhoods in periods near the endpoints of our sample. These data are primarily useful for understanding potential changes in homeownership rates in the general (non-public) housing stock in the places where we observe changes in homeownership rates of public housing units. They also allow us to describe the public housing areas in our sample according to features such as socioeconomic status and education levels. The Israeli Central Bureau of Statistics (CBS) provides us, in addition, with a time series database of the nation's general housing inventory; these data are available to us via the Bank of Israel.

5.2.3 GIS Layer Data

To place public and private housing units on a map, and to aggregate them into neighborhoods of various definitions, we use Geographic Information Systems software along with shapefiles from the CBS and the Survey of Israel (MAPI).

6 Results: Private Home Price Effects

6.1 First-Stage Effects of Discounts on Neighborhood Homeownership Rate

Sections 3.2, 4.1, and 4.4 present a comprehensive picture of the effect of the public housing sale price discounts on tenants' purchase behavior. Results of hazard models

[38] Note that transactions are not censored; these are confidential administrative data.

[39] Since, at the buffer zone level, neighborhoods and their associated homeownership rate are defined specifically around each transaction, there is by definition one transaction per neighborhood-year at that level. Strictly speaking, centering neighborhoods around each always-private unit is the most accurate way of calculating the exposure of each private unit to changes in the local homeownership rate.

using the discount to predict tenant purchase indicate the extremely strong predictive power of the discount, even when controlling for a large number of covariates (Table A5). Household-level models predicting the probability of becoming a homeowner during the "This is My Home" sale indicate a strong effect of discounts when estimated either by probit or linear probability models that include flexible public housing tenure controls (Appendix Table A1).

[FIGURE 11 HERE]

One would expect, then, that the aggregation of these instrumented purchase decisions would also strongly predict actual public housing homeownership rates at the neighborhood level, and it does. Figure A7 presents a binscatter plot of the actual homeownership rate against the discount-predicted homeownership rate at the statistical area level. The relationship is nearly perfectly linear, with a slope of 0.86, significant at the 1% level.

[TABLE 6 HERE]

Formal IV first stage results and F-statistics at the SA level further support the instrument's strength; they are presented in Table 6. All estimates are strongly positive and significant, with Kleibergen-Paap F-statistics well above standard thresholds. We proceed to use this instrument to analyze the neighborhood price effects of changes in homeownership rates.

6.2 Price Effects: OLS and Second Stage IV Results

Table 7 presents OLS and IV second stage estimates of the effect of the public housing homeownership rate on nearby always-private home prices at the statistical area (SA) level (Panel A) and at the smaller block level (Panel B). An observation in these regressions is an always-private home transaction, and the regressions control for homes' hedonic characteristics, year effects, season effects, and neighborhood effects. As such, any national price appreciation over the period is removed from the estimates, as are any underlying, time-invariant differences in neighborhood quality. Inclusion of neighborhood fixed effects means, in addition, that identification is coming from within-neighborhood *changes* in homeownership rates.

[TABLE 7 HERE]

Columns (1) and (4) of each panel include the entire sample of neighborhoods, while the remaining columns partition that sample into above- and below-median initial public housing share. Neighborhoods with high initial public housing shares are exposed to the natural experiment since a larger share of their initial housing stock can potentially switch from rented to owned in this relatively short period due to sale discounts. It is reassuring, then, that we find near-zero effects in the neighborhoods with low initial public housing shares, since they were essentially untreated. While the overall price effect at the SA level per 10 percentage point increase in homeownership rate is about 0.7%, the effect in high initial public housing share neighborhoods is an order of magnitude higher. The OLS estimate is 4.0% (column (2)) and the IV estimate is just slightly smaller at 3.67% per 10pp increase in the homeownership rate (column (5)). Since our prior was that OLS estimates would be upward biased, if at all, due to possible selection into homeownership on either buyer characteristics or expected price appreciation, the slightly lower but very similar IV estimates are reassuring. The measured effect is essentially 0 and not significant in low initial share neighborhoods, just as one would expect where the experiment is low dosage (columns (3) and (6)). Estimates at the slightly-zoomed-in block level follow the same pattern but are slightly larger (Panel B).

[TABLE 8 HERE]

Regressions with neighborhood fixed effects essentially treat the neighborhood as the unit at which the experiment occurs and uses all private home transactions in the neighborhood to measure outcomes for that unit. Strictly speaking, though, each always-private housing unit is subject to a unique experiment depending on the nearby public housing to which it's exposed. To implement this experiment in regression form, we draw a 100 meter radius circle – or "buffer zone" – around each transation and measure the public housing homeownership rate to which the private unit is exposed in each period in its individual buffer zone. Because repeat sales can be identified for only a small subset of the sample (discussed below), we cannot in the full sample control for fixed effects of each transaction's "neighborhood." Instead, we include fixed effects at a larger geographic level, the town, and a flexible geographic control for the location of each transacted unit: polynomials in the unit's latitude and longitude.

Table 8, Panel A presents OLS and IV estimates from these regressions. Again, columns (1) and (4) present estimates for the full sample, while the remaining columns partition the sample into above- and below-median initial public housing share buffer zones. The more flexible geographic controls seem, indeed, to absorb more of the unobserved geographic variation in prices. IV estimates – slightly lower than the corresponding OLS estimates, as before – indicate a 0.6% price increase per 10pp homeownership rate increase on average across all buffers and a 1.47% price increase in initially high public housing share buffers (columns (4) and (5)).

As we move from the SA/block levels of analysis to the buffer zone level, there are two competing effects. On one hand, we control more flexibly for neighborhood geographical differences, as disucssed above, putting downward pressure on price estimates. On the other hand, we zoom in more closely on the experiment – especially in initially high share buffer zones – which should put upward pressure on the coefficient of interest. To confirm that the change in geographical controls is what drives the reduced price estimate we find, we re-estimate our SA and block level analyses, now with flexible geographical controls that mirror those used in the buffer zone estimation. Panels B and C of Table 8 present estimates at the SA and block levels with town fixed effects and polynomials in the latitude and longitude of the unit's centroid. In this case, the polynomial centroid control will absorb more geographical variation the *smaller* is the geographic unit. Indeed, estimates at the block level are now slightly smaller than those at the SA level, the opposite of their relative sizes in Table 7 where the only difference between the estimates was the geographical zooming-in as we moved from SA to block. In addition, estimates in initially high public housing share neighborhoods are now more similar to those estimated with the buffer zone methodology: 2.18% and 1.95% price increases per 10pp increase in instrumented homeownership rates (column (5) of Panels B and C).

Finally, we take advantage of information on building ids and repeat sales in a subset of our sample to gauge the importance of unobserved heterogeneity in the housing stock. Table 9 presents results of estimations in high initial public housing share neighborhoods with HR measured at each of the three geographic levels: 100m buffer zone around transactions, SAs, and blocks. Columns (1) and (4) of each panel present OLS and IV estimates with neighborhood fixed effects, except for Panel A, which includes the flexible polynomial control in latitude and longitude of the transaction, along with town fixed effects. (Estimates in these columns differ from those in the analogous columns of Tables 7 and 8 because they come from the reduced sample in which repeat sales are identifiable.) The following columns include building and then apartment fixed effects, indicating remarkably similar magnitudes to those with neighborhood-level and polynomial-coordinate controls. In each case, the apartment fixed effect estimate is slightly smaller than that with building effects; at the buffer zone level, the IV estimates in the reduced sample are 1.65% per 10pp increase in HR with the coordinate controls, 1.79% with building effects, and 1.43% with apartment effects. At most, unobserverd heterogeneity in the housing stock may account for 0.2pp of the measured price increase.

Given the potential endogeneity of homeownership rates, the unobserved heterogeneity across neighborhoods, and the fact that the public housing homeownership "experiment" is most potent in initially high public housing share neighborhoods, we consider the IV estimates at the buffer zone and block levels, with flexible geographic controls, to be our best estimates of the magnitude of the homeownership effect. Our conclusion from these estimates is that the price effect is most likely to be between 1.47% and 1.95% per 10pp increase in the homeownership rate.

6.3 Magnitudes

The existing state-of-the-art estimate in the literature of external homeownership effects comes from Coulson and Li (2013), who use a neighborhood fixed effects OLS model and find a 4.5% increase in housing prices per 10pp increase in homeownership rates. Our best estimates of the price effect, 1.47% and 1.95% per 10pp increase in homeownership rates are 33%-43% as large. Given that the previous estimates allow homeownership rates to evolve endogenously over time, whereas our estimates use plausibly exogenous variation and more flexible geographic controls, the smaller estimate may reflect the removal of some remaining upward bias in previous estimates.

Our estimates are similar in magnitude to effects of other moderately sized changes in neighborhood amenities. Black (1999) finds that higher local school quality, as measured by a 5% increase in test scores, leads to a 2% increase in house prices. Direct housing externalities have been measured in the contexts of foreclosures and of rent control. Campbell et al. (2011) compare houses closest to forclosures to those slightly further away, controlling for common neighborhood shocks, and find a 1.5-6% reduction in prices from foreclosures. Autor et al. (2014) find a 16% increase in home prices over ten years from rent decontrol. The magnitude of our estimate suggests that changes in homeownership rates among low income populations are of similar importance to changes in these other central features of neighborhoods.

6.4 Difference-in-Differences Results

While the IV results generate a useful comparison across all public housing neighborhoods, with identifying variation coming from the size of discounts faced by tenants, we here present a complementary analysis that can identify effects using only time – and not cross-sectional – variation in discounts using a subset of neighborhoods that are very similar to each other ex-ante. Table 10 presents estimates from a difference-in-differences specification in which neighborhoods with large homeownership changes are compared to other same-town neighborhoods that experienced small changes, as in Equation 7. Only neighborhoods with large initial public housing shares are in the sample.

[TABLE 9 HERE]

Regressions define neighborhoods as statistical areas in Panel A and as blocks in Panel B. Leveraging the substantial, sudden increase in discounts in 2005, at the start of the "This is My Home" sale period, we focus on the years 2000-2008 as an event study. In each panel, the first row of estimates defines a large homeownership change as above-median relative to below-median, while the second row defines treatment as an above-75th-percentile change, while control neighborhoods are those with a below 25th percentile change. Accordingly, there are fewer observations in the latter set of estimates in both panels, and the estimated treatment effects are larger. Focusing on the Panel B estimates at the block level, where the unit of analysis is smaller and the experiment more powerful, we find a 12.4% increase in prices after 2005, relative to before, for an above-median relative to below-median increase in homeownership rates (column (1)). Given that the typical above-median change is 52.46 percentage points and the typical below-median change is 29.78 percentage points, scaling this 12.4% by that difference indicates an approximately 5.47% price increase per 10 percentage point increase in homeownership rate, which is in the same ballpark as the previous estimates, though a bit larger. The treatment effect looks slightly larger when treatment is defined as a larger change, as one would expect: prices increase by 22.1% after 2005 relative to before for an above 75th percentile change relative to one below 25th percentile (column (3)). Scaling again by the typical treatment and control changes in homeownership rate over the period, this effect amounts to a 5.73% increase in prices per 10 percentage point increase in homeownership rate.

[FIGURE 12 HERE]

Estimates in columns (1) and (3) use only time variation in discounts, treating same-town neighborhoods with similar initial shares of public housing and similar pre-treatment price trends as valid controls for large homeownership increase neighborhoods. In columns (2) and (4), we additionally take advantage of cross sectional variation in discounts and instrument for "large increase" neighborhoods. Results are highly similar to those estimated via OLS, suggesting the validity of the within-town comparison between neighborhoods.

To see that this difference-in-differences methodology is appropriate in this context, we visualize the treatment effects over time to see that treated and control neighborhoods within the same town were indeed on parallel trends before the event date in 2005. Figure 8 graphs the coefficients on a series of Treatment x Year interactions in regressions analogous to those in Table 10, where panel 8a shows OLS estimates and panel 8b shows

IV estimates. The graphs are relatively flat (with some noise) around zero before 2005 and rise thereafter.[40] This event study is reassuring that treatment and control units were not differentially trending in prices before the sale event began, and that the effects we find are most likely to be due to the homeownership changes resulting from the suddenly increased discounts tenants faced.

6.5 Interpretation and Potential Mechanisms

When discounts were raised sharply in 2005, the government signalled its intention to privatize public housing. Residents and potential residents in the vicinity of public housing could expect that the majority of units that were public in 2005 would be private several years later; by 2018, the majority had been privatized. To the extent that potential residents believed public housing was generally privatizing, the difference between initially high public housing share neighborhoods that had large versus small increases in the homeownership rate was really that: a difference in homeownership only, and not in residents, the housing stock, or expectations of the future public housing rate. The price effects in that case are attributable to the change in homeownership and any associated behavioral or neighborhood changes. On the other hand, to the extent that residents of a neighborhood knew which initially public units had already been bought by tenants – despite the absence of resident turnover – and interpreted these sales as determining the future public housing share of the neighborhood, the effects we measure may be specific to the privatization context. This section provides evidence on neighborhood and behavioral changes that occurred in neighborhoods with large homeownership rate increases and may be mechanisms of the price effect.

Civic Engagement and Voter Turnout: The literature on homeownership has long hypothesized that homeowners, more invested in the quality of their locations, may become more civicly engaged, often measured by voting (DiPasquale and Glaeser (1999); Engelhardt et al. (2010)). We use elections data for Israel's 6 parliamentary elections from 1999 to 2015 at the polling station level, matched to SAs, and find that SAs with larger increases in homeownership rate experience increases in voter turnout. Table 11 shows an increase in voter turnout of 5.4 percentage points per 1pp increase in HR in an IV specification (column 2) analogous to those with flexible geographic controls in Table 8. The large effect suggests the likelihood that new homeowners not only vote more themselves, but also draw incoming neighbors that are more civicly engaged. Figure 9

[40] Again, as recommended in Rambachan and Roth (2019), we conduct sensitivity analysis, allowing for potential non-linearities in pre-treatment differential trends; results are presented in Appendix Figure A11.

illustrates that this increase in civic engagement occurs after 2005 in high homeownership rate increase SAs relative to same-town SAs with similar initial characteristics but only small homeownership increases.

Welfare to Work: We measure a significant increase in labor supply among new homeowners. This effect is likely to be felt in the neighborhood, especially given the small neighborhoods we use in our analysis, which can include just a few blocks or, at their smallest, a 100 meter radius. The value of having a next door neighbor who works rather than sitting at home all day and collecting welfare – especially for families with kids, who are sensitive to role models – is an amenity that would be capitalized into housing prices and could drive the price effect.

Young Population and School Quality: The measured increase in new homeowners' labor supply may begin to alter neighborhood character, increasing its suitability for children, who are likely to benefit from working adult role models. We provide two pieces of evidence on the likely increase in these neighborhoods' value to families with kids.

First, we use annual population data extracted from the Central Bureau of Statistics to investigate changes in population composition in neighborhoods that experienced large homeownership rate increases. Using our difference-in-differences strategy to compare initially similar neighborhoods within the same town, we find that the share of population aged 0-17 increased after 2005 in high relative to low homeownership change areas. Appendix Figure A8 indicates that, while the young population shares of these neighborhoods were similar before 2005, the share of youngsters began to increase in following few years in places that experienced the largest increases in homeownership.

Second, we use data on elementary school "Meitzav" test scores, aggregated to the SA level, to measure local school quality. To reduce censoring in small neighborhoods, we use a weighted average of standardized 5th and 8th grade scores.[41] Table 11 shows an increase of a quarter of a standard deviation increase in test scores per 1pp HR increase, suggesting an improvement in school and average peer quality as rising homeownership rates may attract increasingly motivated young families to the neighborhood.

Resident Stability: The evidence suggests that the mechanism of the neighborhood price effect is unlikely to work through increased stability of residents, a mechanism that has been frequently studied in the literature. In our setting, public housing renters exhibit similar geographic mobility to new owners: 4% leave their units after five years relative to 8% of owners.[42]

[41]High schools often draw from multiple neighborhoods and their outcomes are thus less relevant for this neighborhood-specific analysis.

[42]New homeowners are allowed to rent out their units, but these results clearly indicate that renting

Renovations and Home Care: A frequently cited mechanism of homeownership externalities is the extra care owners are likely to put into their homes with renovations, upkeep, and gardening (Henderson and Ioannides (1983); DiPasquale and Glaeser (1999). We take advantage of a Ministry of Housing commissioned study of public housing tenants which finds that buyers (85%) did indeed renovate more than non-buyers (67%), and, conditional on renovating, spent an average of $740 USD more than non-buyers (3,402 in 2005 NIS). The most common types of renovation undertaken by survey respondents who renovated were painting (59%), kitchen or bathroom upgrading (45%), plumbing (40%), and flooring (39%), while a smaller set engaged in electrical work (28%), moving of walls (19%), and closing of balconies (15%). These renovations are mostly indoors and – perhaps with the exception of plumbing, which can have substantial within-building externalities due to leaks and clogs – are unlikely to have a huge effect on the value of neighborhood homes. 15% of respondents indicated that purchases likely improved the state of public housing buildings, but the vast majority perceived no great improvement. While there may have been some effect of homeowner renovations and home care on building and neighborhood prices, this mechanism seems unlikely to account for the bulk of the effect.

Wealth Effect: Price increases could be due to a wealth effect if tenants' increase in expected wealth due to the government discount (average value, $47,650 USD) drives them to spend extravagantly on external home improvements, visibly changing the face of the neighborhood.[43] In addition to the evidence presented above suggesting the new homeowners spent on generally modest renovations, we compare buyers' renovation expenditures to those in the general population. New owners are likely to spend more on improvements than would the average incumbent homeowner, who would not necessarily understake improvements in any particular year. Nevertheless, if the new public housing homeowners have a much higher marginal propensity to consume on home improvements than does the average homeowner, one might imagine that part of the increase in neighborhood quality could be due to these (extra) physical improvements.

A 2012 Dun & Bradstreet survey of firms in the Israeli home renovations industry indicates that 220,000 households, or 2.8% of the population, do some sort of renovations of their home each year. On average, they spent 41,661 NIS in 2012, which is $7,603 in

could have occurred within the first five years *at most* in 8% of newly owned units.

[43]These wealth increases were not – and could not have been – realized during the years in which we measure labor supply and neighborhood price effects: those that used discounts to buy their units would have to repay the discount received – a prohibitive financial cost. According to the MOH, selling within five years was extremely rare. In practice, 96% of non-buyers and 92% of buyers in our sample remained in their units through 2010.

2005 USD.[44] The average renovator in the general population – whether a homeowner or not – spends about $2,500 dollars, or 45%, more on the renovation than does the average new public housing homeowner in our setting. Thus, even considering that public housing tenants are drawn disproportionately from the lower half of the income distribution, it seems unlikely, given the nature of their renovations and their relative spending on renovations, that neighborhood changes are being driven by a wealth effect.[45]

6.6 Heterogeneity and Robustness

Many supplemental analyses assess the robustness of our results to model assumptions and potential measurement concerns.

Measurement of Homeownership Rates: Due to data limitations, we observe tenure status in always-private housing only near the two endpoints of the sample, in Israeli Census years, 1995 and 2008. Since our experiment uses homeownership rate changes in concentrations of public housing, this data limitation is immaterial so long as homeownership rates in always-private housing don't change systematically with our instrument.

[FIGURE 13 HERE]

Figure A9 plots the general housing stock homeownership rate change between the two censuses, by statistical area, against the change in the instrument (discount-predicted homeownership rates). The slope is near zero and not statistically significant, suggesting orthogonality of the instrument to changes in always-private tenure status.

Neighborhood Definition: The external neighborhood effects measured in Section 6 are robust to a variety of neighborhood definitions and sizes. For neighborhoods with above-median initial densities of public housing, effects range from a 1.47% to a 2.18% increase in prices per 10 percentage point increase in homeownership rate in our preferred specification with flexible geographic controls. The smallest neighborhood definition is a buffer zone with radius 100 meters (area $\pi 0.1^2 \approx 0.0314$ square km), while the largest definition used is a Statistical Area, where the median SA with public housing is 0.34

[44] Globes report, "Renovation Sector in Israel Rolls 15 Billion Shekels per Year," August 15, 2012.

[45] Note, also, that this result on renovations also suggests that the price effects found are unlikely to be due to changes in expectations of investment in the physical appearance of homes, since homes' exteriors tend not to be upgraded by the new owners, and since these new owners exhibit high locational stability (92% of new buyers remained in their homes by 2010, while 96% of non-buyers remained in their rental units). This result contrasts with that in Autor et al. (2014), where the end of rent control generated a substantial increase in investment, such that the physical external appearance of homes improved and drove neighborhood prices up.

square km. Blocks, our third neighborhood definition, are sized between the other two and also generate similar price effects (1.95% in our preferred specification).

Geographical Controls: We estimate price effects with three alternative specifications of geographic controls. The first specification is based on equation 6 and includes neighborhood fixed effects (either SA or block). The second specification is also based on equation 6 but, instead of including neighborhood fixed effects, it includes fixed effects for the town and a polynomial in the neighborhood centroid's lattitude and longitude (and, in the case of the buffer zone, the neighborhood centers on the location of the always-private unit transacted in that observation). The third specification, based on equation 7, uses a different strategy that compares ex ante similar neighborhoods with large public housing shares in the same town, and it contains only town fixed effects.

As one might expect, the most flexible geographic controls seem to soak up the most unobserved geographic variation, and price estimates are smallest using those (second specification), at 1.47–2.18% per 10pp increase in homeownership rate. The largest estimates come from the difference-in-dfferences specification with only town level fixed effects; implied price effects per 10pp increase in homeownership rate are 5.47–5.60%. In between are the estimates from the first specification with neighborhood fixed effects, at 3.67–4.38% per 10pp increase in homeownership rates. The magnitude of the effects remains within a fairly tight range across specifications, especially considering the significant differences in geographic controls. The pattern of the estimates fits the extent of unobserved geographic variation that can be picked up by each set of controls.

Exogeneity of Discounts: The paper employs multiple empirical strategies to estimate effects both at the household level and at the neighborhood level. The IV strategy using discounts to the sale price tenants face relies on the conditional exogeneity of these discounts to tenants' labor supply decisions and neighborhood price appreciation. We have provided a number of pieces of evidence in support of this assumption.

First, Arbel et al. (2014) show that the timing and size of the discounts were unpredictable to tenants – they follow a random walk – which supports using the timing of discount changes as an exogenous shock to tenants' behavior. The beginning of the "This is My Home" sale event represents an unexpected, large increase in discounts that spurred an increase in homeownership.

Second, discount variation across households in the cross-section is plausibly exogenous conditional on smooth changes in household covariates. We take advantage of discontinuous jumps in discounts along specific margins – such as those from 2 to 3 kids or 5 to 6 and 11 to 12 years of tenure – that are plausibly exogenous to labor supply and neighborhood outcomes. While each of these discontinuities on its own does not constitute a powerful enough experiment to estimate effects, the union of the discontinuities, as summarized in the residualized discount, is an important predictor of purchase behavior. Tables A1 and A5 exhibit the instrument's strength (t-stat>20 in all cases) even when controlling flexibly for endogenous household characteristics, such as disability status or the smooth difference between households with 8 versus 30 years of tenure in public housing.

Third, a glance at the discount determination rules – discussed in Section 3.2 and summarized in Appendix Table B1 – indicates they're unlikely to be biased towards particular families who may be more motivated to work, buy their homes, and improve their neighborhoods. Larger discounts were granted to older households with higher disability ratings (all of which are residualized from the instrument). Any geographic variation in discounts is controlled for with geographic fixed effects in all specifications, such that the estimation uses only variation in discounts within small geographic areas.

Fourth, the natural experiment in public housing is not systematically correlated, geographically, with general housing stock homeownership rate changes, as shown in Figure A9. Instrumented homeownership rate changes seem to have been allocated randomly among otherwise similar neighborhoods with initially high densities of public housing.

Fifth, among initially high public housing share neighborhoods, large instrumented homeownership rate changes are not correlated with access to employment centers (Table A10). Flexibly controlling in the price equations for differential importance of market access over time minimally affects coefficients (Tables A11 and A12), suggesting that differential market access does not drive both changes in homeownership rates and prices.

Parallel Trends Assumption: The estimation strategy delineated in equation 7 relies on the assumption that large and small homeownership change neighborhoods in the same town were on parallel price trajectories before treatment. The event study shown in Figure 8 suggests that this assumption holds; treatment coefficients before 2005 are statistically indistinguishable from zero and do not appear to be trending either up or down. Nevertheless, we estimate robust confidence intervals for post-treatment coefficients under a variety of assumptions for the degree of non-linearity allowed in differential pre-treatment trends. Results for the 2007 coefficient are presented in Appendix Figure A11. The treatment effect remains significantly positive under differential linear pre-treatment trends ($M = 0$) and with non-linearities in trends up to twice the average slope observed in the pre-period. Given that there doesn't even appear to be a *linear* differential trend, robustness to that degree of nonlinearity is substantial.

Estimation Window: Israel experienced rapid housing price appreciation during the period from 2008-2012, at the end of our sample window. The difference-in-difference results presented in Table 10 are generated by a sample that ends in 2008 and indicate large, positive effects, implying that our results are not driven by the rapid price growth from 2008-2012. In addition, we re-estimate external price effects shown in Tables 7 and 8 using only the years 2000-2008. Results are presented in Appendix Tables A6 and A7. Estimates are slightly smaller than, but not statistically significantly different from those estimated on the full sample period.

Exclusion of Major Cities: The results are not driven by Israel's major cities, Tel Aviv and Jerusalem. Table A8 presents results analogous to those in initially high public housing share SAs in columns (2) and (5) of Table 7. Estimates excluding these two major cities are slightly larger than, but statistically inditinguishable from the main estimates.

Alternative Lag Structures: The homeownership rate is included with a two quarter lag in the main specification. Table A9 shows that the results are not sensitive to the particular lag structure chosen. As one might expect, the magnitude of the effect declines slightly as the length of the lag increases, but the effect remains positive, significant, and economically meaningful at least up to a six quarter lag.

7 Discussion

This paper is the first, to our knowledge, to use a natural experiment to estimate external neighborhood quality effects – as measured by home prices – of changes in homeownership rates. Other natural experiments in Sweden and in Tulsa, Oklahoma have been employed to estimate effects on homeowners' labor market and financial outcomes (Sodini et al. (2016)) and on homeowners' community involvement and home repairs (Engelhardt et al. (2010)). Previous estimates of homeownership effects on neighborhood quality using cross-sections or panel data techniques could be biased by endogenous changes in residents or housing stock that are correlated with changes in homeownership rates. This paper uses plausibly exogenous variation in public housing privatizations, along with high quality administrative data on tenants, purchases, and prices of nearby homes, to identify positive effects of increases in homeownership rates on neighborhood quality.

The effects are economically meaningful, at 1.5–2% per 10 percentage point increase in homeownership rates. Estimates of the value of other neighborhood amenities are in the same ballpark for moderately sized changes in the amenity: higher local school quality, as measured by 5% higher tests scores, leads to a 2% increase in home prices Black (1999), and foreclosures in close proximity relative to slightly further away lead to 1.5-6%

reductions in prices Campbell et al. (2011). Homeownership rates vary from 55-85% in most geographical areas of the U.S, such that a 10 percentage point increase covers a third of the range from low to high homeownership neighborhoods. The magnitude of our estimate suggests that changes in homeownership rates among low income populations are of similar importance to changes in these other central features of neighborhoods.

Increases in homeownership rates among lower income populations are especially worthy of study since low income households are more likely to be the marginal owners in response to policy; higher income populations have much higher homeownership rates. In the U.S. in 2019, 78% of above-median income households are homeowners, while only 50% of below-median income households are homeowners (U.S. Census Bureau (2019)). Our finding that relatively disadvantaged, lower socioeconomic status neighborhoods experience quality improvements in response to increased homeownership rates is a hopeful one for policies – like savings plans or mortgage assistance – targeting this population. Like the policy we study, these frequently considered policies include financial incentives and thus net transfers to the target population.

Moreover, in-line with Sodini et al. (2016), we find strong evidence that new homeowners increase their labor supply on both the extensive and intensive margins, suggesting one possible mechanism for the estimated neighborhood effects. Working class neighborhoods have a fundamentally different feel from welfare-dependent ones and expose local children to more working adult role models. The opportunity to own their homes may give many public housing tenants just the sense of agency over their lives – and their children's inheritance – that they need to motivate them to work. Field (2007) finds that provision of property rights in urban areas of Peru encouraged squatters to increase their labor supply. Other recent work on neighborhood mobility has provided evidence that soft incentives may be at least as important as financial incentives in encouraging disadvantaged families to improve their own and their children's opportunities (Bergman et al. (2019)). This paper suggests that homeownership may spur lower income households to be more active in improving their own futures and, as a result, the quality of their neighborhoods.

The results have two types of policy implications. First, many historically socialist countries have large public housing stocks, some of which can become traps of social immobility. This paper suggests that privatizations are likely to improve outcomes for both the new homeowners and their neighborhoods. Second, and more generally, policies that encourage homeownership among lower income populations may have important effects on the nature of the communities in which relatively disadvantaged children grow up. A wealth of evidence in recent years has emphasized the importance of neighborhoods for children's long run outcomes (Chetty et al. (2016); Chetty and Hendren (2018); Chyn

(2018)). This paper suggests that homeownership is one means by which the quality of children's environments may be improved on the margin, with more nearby working adults and higher overall neighborhood quality.

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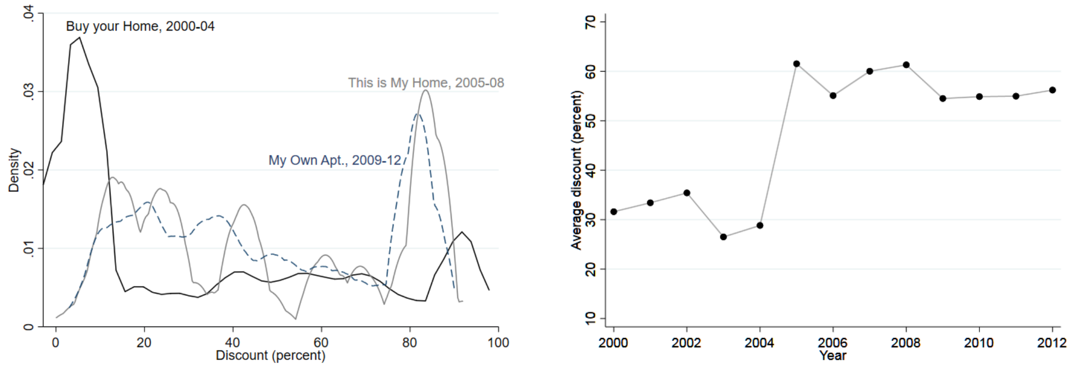
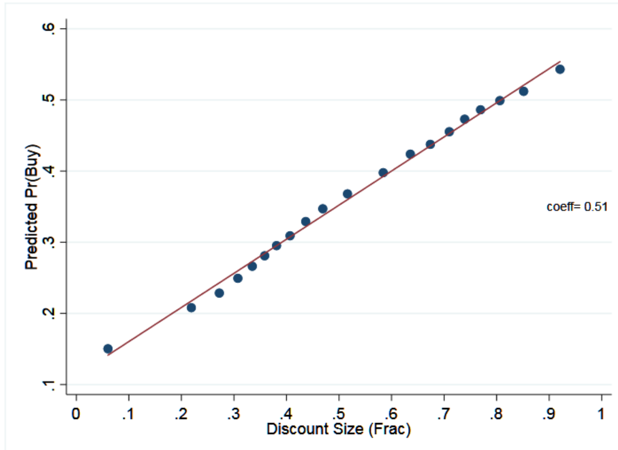
Figure 1: 2005 Increase in Discounts



(a) Discount Distribution by Sale Period (b) Avg. Discount, Family w/ 3+ Kids

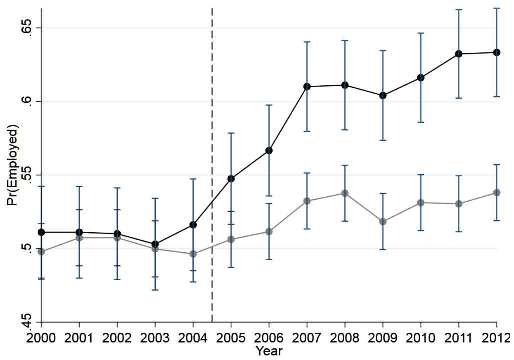
Notes: Figure shows the change in discounts over time. Panel (a) shows the distribution of discounts faced by tenants during each sale period, with mode shift in 2005. Panel (b) shows the average discount faced in each year by a public housing family with 3 or more children. The “Buy Your Home” sale was in place from 2000-2004; the “This is My Home” sale was in place from 2005-2008; the “My Own Apartment” sale was in place from 2009-2012.

Figure 2: Discount Sizes and Public Housing Sales

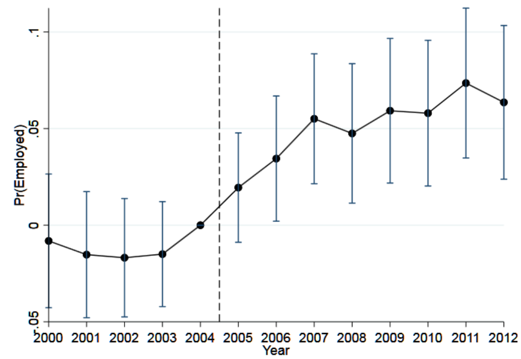


Notes: Points represent binscatter means within each discount size bin of the predicted probability a tenant household buys its unit. Means are residualized for household characteristics including household head age, marital status, number of children, region, and disability. Years included in the data are those of the "This is My Home" sale event, 2005-2008. Predicted probabilities are generated as described in Section 4.2.

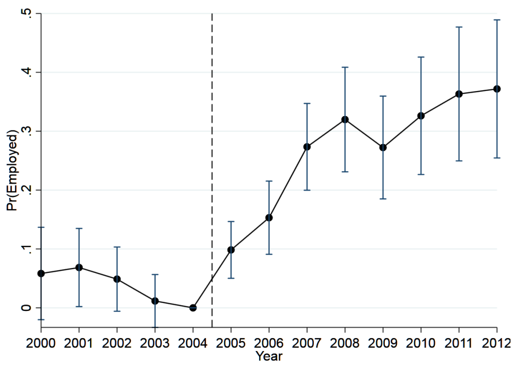
Figure 3: Employment Probability Effects: Four Methods of Comparison, 25th-75th Percentile Common Support



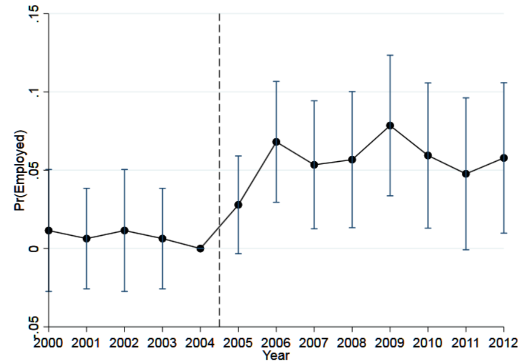
(a) Raw Data



(b) OLS Fixed Effects



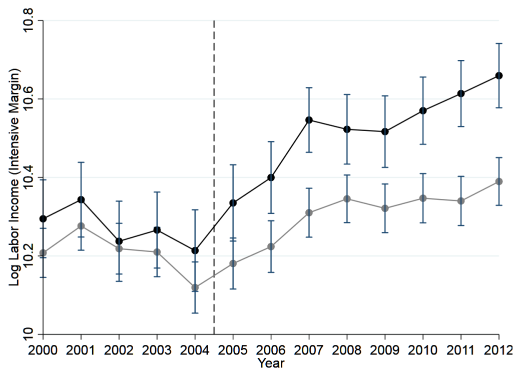
(c) IV



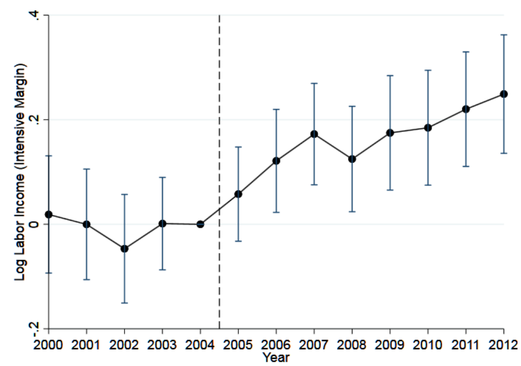
(d) Nearest Neighbor Matching

Notes: Points on the graphs in panels (b), (c), and (d) represent coefficients on treatment times year interactions, where treatment is defined as becoming a homeowner during the "This is My Home" sale period (2005-2008), in regressions predicting employment and including an indicator for being an ever-buyer, year effects, and time-varying demographic controls. Points on the graph in panel (a) represent coefficients on group times year interactions, where groups are buyers or non-buyers, in separate regressions predicting employment and including year effects and time varying demographic controls. Sample includes all households in the 25th to 75th percentile propensity score common support, where propensity scores are predicted using ex-ante demographics. The "This is My Home" sale event began in 2005. In panel (c), buying is instrumented with discounts, as described in Section 4.2 of the text. Standard errors are clustered at the household level. Error bars represent 95% confidence intervals.

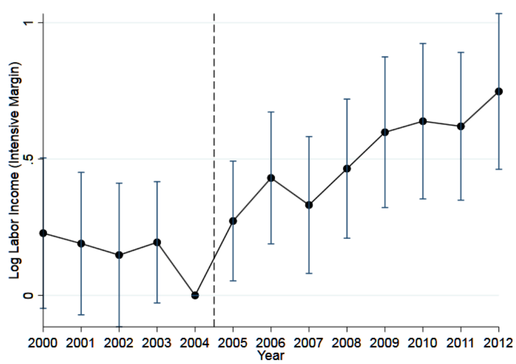
Figure 4: Labor Income Effects (Intensive Margin): Four Methods of Comparison, 25th-75th Percentile Common Support



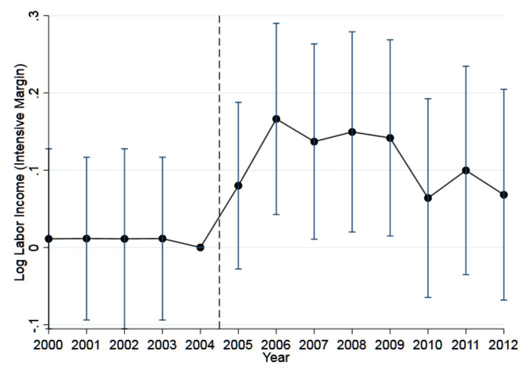
(a) Raw Data



(b) OLS Fixed Effects



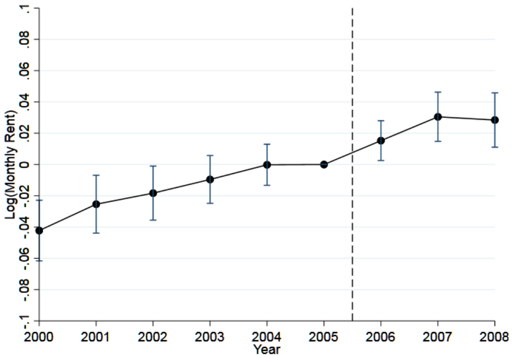
(c) IV



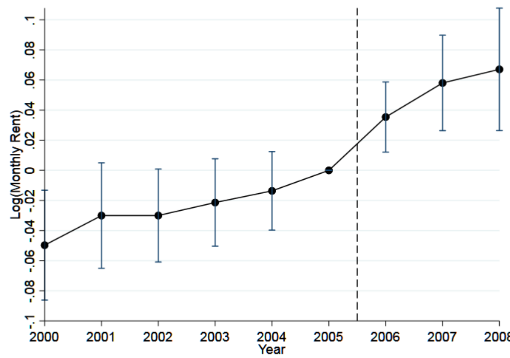
(d) Nearest Neighbor Matching

Notes: Points on the graphs in panels (b), (c), and (d) represent coefficients on treatment times year interactions, where treatment is defined as becoming a homeowner during the "This is My Home" sale period (2005-2008), in regressions predicting log labor income and including an indicator for being an ever-buyer, year effects, and time-varying demographic controls. Points on the graph in panel (a) represent coefficients on group times year interactions, where groups are buyers or non-buyers, in separate regressions predicting log labor income and including year effects and time varying demographic controls. Sample includes all households in the 25th to 75th percentile propensity score common support, where propensity scores are predicted using ex-ante demographics. The "This is My Home" sale event began in 2005. In panel (c), buying is instrumented with discounts, as described in Section 4.2 of the text. Standard errors are clustered at the household level. Error bars represent 95% confidence intervals.

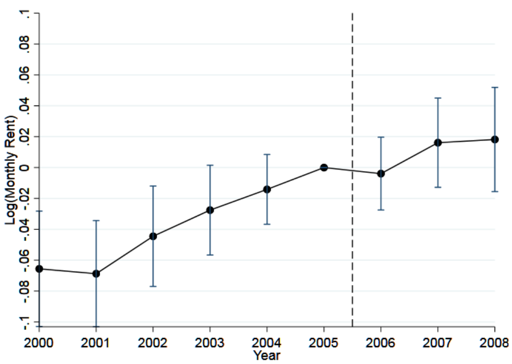
Figure 5: Rent-Income Gradients by Year



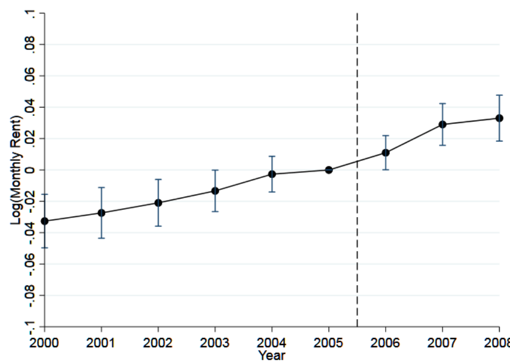
(a) Non-Disabled



(b) Families with 3+ Kids



(c) Post-1989 Immigrants

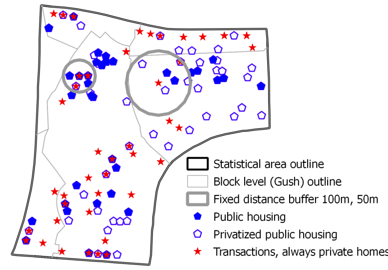


(d) All Public Housing Families

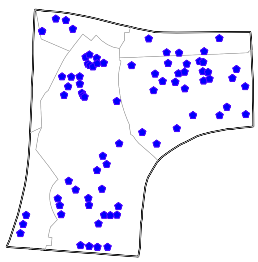
Notes: Points represent coefficients on Year x Log Labor Income interaction terms in a regression predicting Log Rent and including main effects of log labor income and year, household fixed effects, and time-varying household characteristics. In Panel (a), the sample is restricted to non-disabled households. In Panel (b), the sample is restricted to families with 3 or more children. In Panel (c), the sample is restricted to post-1989 immigrants. Panel (d) shows the average effects across all public housing households. The vertical line marks the implementation of a new set of rules for public housing rent determination, which coincided with new housing value assessments. Error bars represent 95% confidence intervals.

Figure 6: Example Public Housing Neighborhood in Be'er Sheva

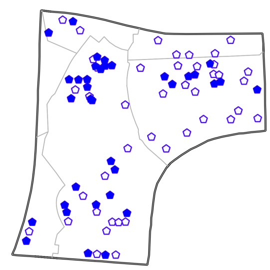
(a) City View



(b) Geographic Levels



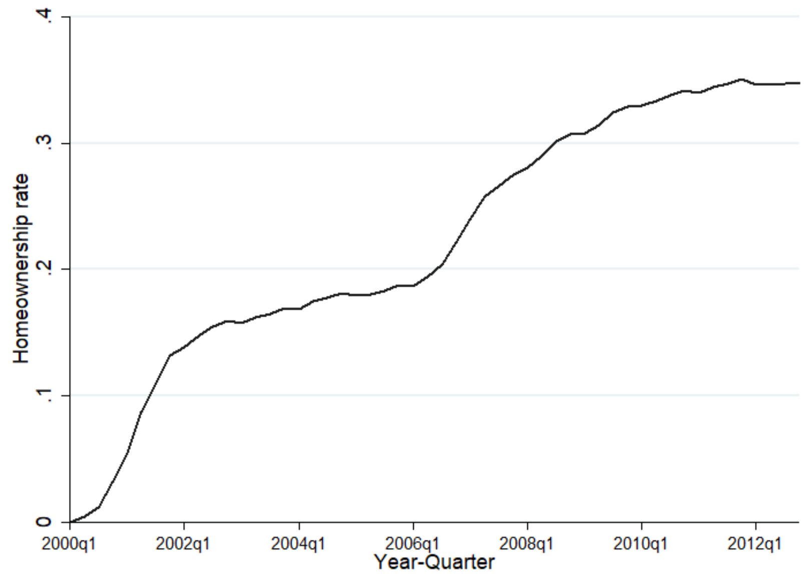
(c) 2000



(d) 2012

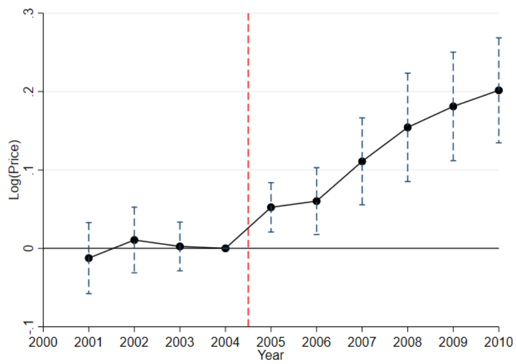
Notes: Figure maps public housing units in the city of Beer Sheva as a whole and in a particular neighborhood. Panels (c) and (d) show the neighborhood's public housing units at the beginning and end of the research period, indicating which units were sold to tenants. Panel (b) maps public housing units that remained public, public housing units that were privatized, transactions of units that were always privately held, and the various geographic levels at which we analyze neighborhoods that contain these various types of homes. Circles ("buffer zones") center on each private transaction.

Figure 7: Public Housing Homeownership Rate, 2000-2012

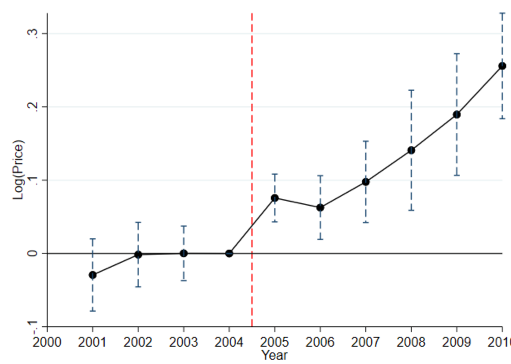


Notes: Figure presents the public housing homeownership rate by statistical area over the period of study, 2000-2012. Data are quarterly.

Figure 8: Within-Locality Difference-in-Differences Estimates of Price Effects,
"This is My Home" Sale Event



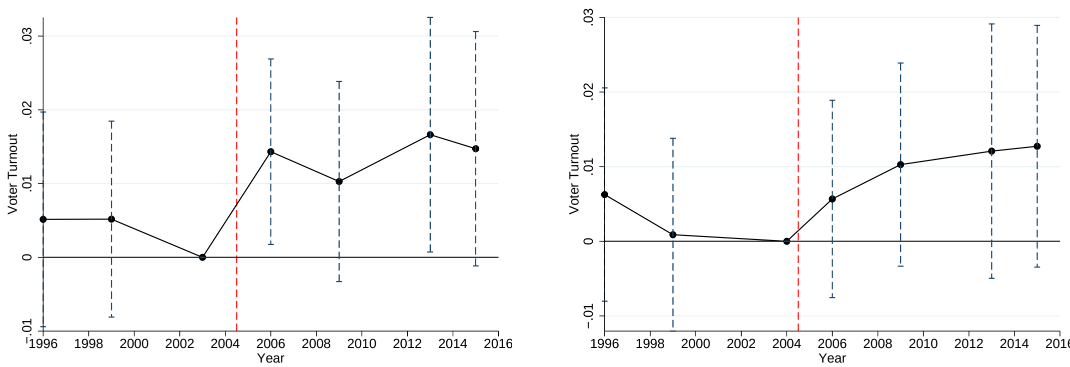
(a) OLS



(b) IV

Notes: Points on the graphs represent coefficients on treatment times year interactions, where treatment is defined as blocks with above-75th percentile increases in homeownership rates and control SAs have below 25th percentile increases in homeownership rates. The sample includes only blocks with high initial public housing shares, and only towns with both treated and control blocks. The "This is My Home" sale event began in 2005. The y-axis measures log transaction prices of always-private homes, and the regressions include quarter effects, town effects, hedonic controls for these units' physical characteristics as well as two-year lagged building starts. Panel (a) shows OLS estimates, while panel (b) shows IV estimates. Standard errors are clustered at the block level. Error bars represent 95% confidence intervals.

Figure 9: Within-Town Difference-in-Differences Estimates of Voter Turnout Effects, "This is My Home" Sale Event



(a) OLS (b) IV

Notes: Points on the graphs represent coefficients on treatment times year interactions, where treatment is defined as SAs with above-75th percentile increases in homeownership rates and control SAs have below 25th percentile increases in homeownership rates. The sample includes only SAs with high initial public housing shares, and only towns with both treated and control SAs. The "This is My Home" sale event began in 2005. The y-axis measures voter turnout rates in parliamentary elections, which took place in 1996, 1999, 2003, 2006, 2009, 2013, and 2015. Regressions include year effects, town effects, two-year lagged building starts, and the USSR immigrant share. Panel (a) shows OLS estimates, while panel (b) shows IV estimates. Standard errors are clustered at the SA level. Error bars represent 95% confidence intervals.

Table 1: Buyer and Non-Buyer Characteristics by Sample

Panel A: Full Balanced Sample

	Buyer	Non-Buyer	Difference	T-Stat
Head of household's age	43.67	47.22	-3.54	-31.98
Number of children under 18	2.27	1.21	1.06	45.81
Single parent	0.42	0.26	0.16	24.91
Post 1989 Immigrant	0.33	0.22	0.11	17.79
Public Housing Tenure	23.34	14.65	8.69	61.84
Disabled	0.20	0.31	-0.11	-17.84
Employed 6+ months	0.58	0.38	0.20	29.77
N	8,129	15,528		

Panel B: 10th-90th Percentile Common Support

	Buyer	Non-Buyer	Difference	T-Stat
Head of household's age	44.33	45.54	-1.21	-8.54
Number of children under 18	1.77	1.35	0.42	17.50
Single parent	0.46	0.35	0.11	12.84
Post 1989 Immigrant	0.30	0.31	-0.01	-1.19
Public Housing Tenure	23.73	13.04	10.69	58.53
Disabled	0.22	0.24	-0.02	-2.82
Employed 6+ months	0.54	0.49	0.05	5.27
N	4,726	8,421		

Panel C: 25th-75th Percentile Common Support

	Buyer	Non-Buyer	Difference	T-Stat
Head of household's age	44.66	44.72	-0.06	-0.20
Number of children under 18	1.59	1.45	0.14	3.33
Single parent	0.46	0.41	0.04	2.45
Post 1989 Immigrant	0.30	0.31	-0.01	-0.86
Public Housing Tenure	24.21	12.64	11.57	33.70
Disabled	0.24	0.23	0.01	0.86
Employed 6+ months	0.49	0.51	-0.01	-0.68
N	1,333	2,300		

Notes: Table presents descriptive statistics for (A) the full, balanced panel of working age tenants (25-60), (B) the balanced sample of 10th-90th propensity-to-buy percentiles, and (C) the balanced sample of 25th-75th propensity-to-buy percentiles. Post-1989 immigrants are distinguished from those that came earlier because of the large wave that came from former Soviet Union countries beginning in that year. Sample C underlies the main labor supply analyses presented.

Table 2: Estimates of the Homeownership Effect on Long-Term Employment, 25th-75th Percentile Common Support

Dependent Variable: Employment	(1) OLS	(2) IV	(3) NNM-1	(4) NNM-3
I{year≥2005} x I{Homeowner}	0.0503***	0.0764*	0.0443***	0.0530**
	(0.0121)	(0.0442)	(0.0150)	(0.0259)
Observations	32,697	32,697	17,721	17,217
R-squared	0.648	0.648	0.644	0.595
Num. Clusters	3,633	3633	1969	1436
1st Stage KP F-Stat		343.6		

An observation in the sample is a household-year for the years 2000-2008 and households living in public housing at the beginning of the period. The sample includes households in the 25th-75th percentile common support of propensity to buy their units during the sale period "This is My Home." Regressions are fixed effects specifications of long-term employment on the interaction of a homeowner indicator with an indicator for after the start of the sale period, including, year effects, household effects, and controls for number of kids under 18, marital status, years since immigration, having a disabled household member, and the regional unemployment rate. Long-term employment is defined as employment for at least 6 months in a row. In column (2), the homeownership x after interaction is instrumented for using sale discounts, as described in Section 4.2 of the text. Columns (3) and (4) implement nearest neighbor matching estimators with one match (column 3) and 3 matches (column 4) for each treated household; in these regressions, match group fixed effects replace household fixed effects, and not all control units serve as matches. Standard errors are clustered by household. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 3: Estimates of the Homeownership Effect on Labor Income, 25th-75th Percentile Common Support

Dependent Variable: Log(Labor Inc.)	(1) OLS	(2) IV	(3) NNM-1	(4) NNM-3
I{year≥2005} x I{Homeowner}	0.135***	0.125	0.120**	0.171***
	(0.033)	(0.111)	(0.049)	(0.070)
Observations	21,265	21,265	10,978	10,999
R-squared	0.705	0.705	0.675	0.633
Num. Clusters	3,080	3,080	1,560	1,176
1st Stage KP F-Stat		275.5		

Notes: An observation in the sample is a household-year for the years 2000-2008 and households living in public housing at the beginning of the period. The sample includes households in the 25th-75th percentile common support of propensity to buy their units during the sale period "This is My Home." Regressions in columns (1) and (2) are fixed effects specifications of log income on the interaction of a homeowner indicator with an indicator for after the start of the sale period, including, year effects, household effects, and controls for number of kids under 18, marital status, years since immigration, having a disabled household member, and the regional unemployment rate. In column (2), the homeownership x after interaction is instrumented for using sale discounts, as described in Section 4.2 of the text. Columns (3) and (4) implement nearest neighbor matching estimators with one match (column 3) and 3 matches (column 4) for each treated household; in these regressions, match group fixed effects replace household fixed effects, and not all control units serve as matches. Standard errors are clustered by household. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 4: Placebo vs. Actual Event Dates

Panel A: Placebo Event – 2003

Dependent Variable:	Employed		Log(Labor Inc)	
	(1)	(2)	(3)	(4)
I{year≥2003} x I{Homeowner}	0.0041	0.0084	0.0150	0.0090
	(0.0064)	(0.0118)	(0.0181)	(0.0348)
Observations	52,588	14,532	32,659	9,059
R-squared	0.773	0.770	0.783	0.787
Years Included	2001-2004	2001-2004	2001-2004	2001-2004
Common Support PS Pctiles	10th-90th	25th-75th	10th-90th	25th-75th

Panel B: Actual Event – 2005

Dependent Variable:	Employed		Log(Labor Inc)	
	(1)	(2)	(3)	(4)
I{year≥2005} x I{Homeowner}	0.0366**	0.0296**	0.110***	0.119***
	(0.0069)	(0.0135)	(0.020)	(0.036)
Observations	52,588	14,532	33,239	9,224
R-squared	0.759	0.752	0.806	0.816
Years Included	2003-2006	2003-2006	2003-2006	2003-2006
Common Support PS Pctiles	10th-90th	25th-75th	10th-90th	25th-75th

Notes: An observation in the sample is a household-year for the years indicated in each regression and for households living in public housing at the beginning of the period. The sample includes households in the indicated common support of propensity to buy their units during the sale period "This is My Home." Regressions in columns (1) and (2) are OLS fixed effects and IV specifications of long-term employment on the interaction of a homeowner indicator with an indicator for after the start of the placebo or actual sale period, including year effects, household effects, and controls for number of kids under 18, marital status, years since immigration, having a disabled household member, and the regional unemployment rate. Regressions in columns (3) and (4) are analogous to those in columns (1) and (2) but with log of labor income as the dependent variable. Long-term employment is defined as employment for at least 6 consecutive months. Standard errors are clustered by household. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 5: Placebo vs. Actual Event Dates, Shorter Event Windows

Panel A: Placebo Event – 2003

Dependent Variable:	Employed		Log(Labor Inc)	
	(1)	(2)	(3)	(4)
I{year≥2003} x I{Homeowner}	0.0025	0.0155	0.0304	0.0332
	(0.0082)	(0.0158)	(0.0232)	(0.0461)
Observations	26,294	7,266	14,968	4,124
R-squared	0.833	0.825	0.825	0.828
Years Included	2002, 2004	2002, 2004	2002, 2004	2002, 2004
Common Support PS Pctiles	10th-90th	25th-75th	10th-90th	25th-75th

Panel B: Actual Event – 2005

Dependent Variable:	Employed		Log(Labor Inc)	
	(1)	(2)	(3)	(4)
I{year≥2005} x I{Homeowner}	0.0289**	0.0210	0.0769**	0.0737*
	(0.0076)	(0.0145)	(0.0223)	(0.0386)
Observations	26,294	7,266	15,590	4,286
R-squared	0.860	0.856	0.866	0.886
Years Included	2004-2005	2004-2005	2004-2005	2004-2005
Common Support PS Pctiles	10th-90th	25th-75th	10th-90th	25th-75th

Notes: An observation in the sample is a household-year for the years indicated in each regression and for households living in public housing at the beginning of the period. The sample includes households in the indicated common support of propensity to buy their units during the sale period "This is My Home." Regressions in columns (1) and (2) are OLS fixed effects and IV specifications of long-term employment on the interaction of a homeowner indicator with an indicator for after the start of the placebo or actual sale period, including year effects, household effects, and controls for number of kids under 18, marital status, years since immigration, having a disabled household member, and the regional unemployment rate. Regressions in columns (3) and (4) are analogous to those in columns (1) and (2) but with log of labor income as the dependent variable. Long-term employment is defined as employment for at least 6 consecutive months. Standard errors are clustered by household. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 6: First Stage IV Estimates - Neighborhood Fixed Effects Price Models

Dependent Variable:	Actual Homeownership Rate$_{t-2}$		
	All (1)	High Share (2)	Low Share (3)
$\widehat{HomeownershipRate}_{t-2}$	0.793***	0.793***	0.792***
(Discount-Predicted)	(0.029)	(0.035)	(0.034)
N	288,516	163,148	125,368
N Clusters	977	521	456
KP F-Stat	745.11	519.08	551.81

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2012. The sample includes neighborhoods that had public housing in 2000; column (2) includes only neighborhoods with an above-median share of public housing in 2000, and column (3) includes only neighborhoods with a below-median share of public housing in 2000. All regressions are first stage estimates from IV fixed effects specifications of log transaction price on (two quarter lagged) homeownership rate that include neighborhood effects, year effects, and quarter (season) effects, as well as building starts in the neighborhood and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. The homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by statistical area. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 7: OLS and IV Price Estimates – Neighborhood Fixed Effects

Panel A: Statistical Area Level

	Dependent Variable: ln($Price$)					
	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.074***	0.407***	0.006	0.072***	0.367***	0.011
	(0.022)	(0.068)	(0.020)	(0.024)	(0.063)	(0.022)
N	288,516	163,148	125,368	288,516	163,148	125,368
N Clusters	977	521	456	977	521	456
R-Sq	0.840	0.822	0.831			
R-Sq Within	0.215	0.209	0.233	0.215	0.209	0.233
1st Stage KP F-Stat				745.11	519.08	551.81
Public Housing Share	All	High	Low	All	High	Low

Panel B: Block Level

	Dependent Variable: ln($Price$)					
	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.091***	0.494***	0.018	0.076***	0.438***	0.010
	(0.024)	(0.118)	(0.019)	(0.024)	(0.095)	(0.022)
N	262,445	120,038	142,407	262,445	120,038	142,407
N Clusters	1351	718	633	1351	718	633
R-Sq	0.851	0.822	0.837			
R-Sq Within	0.195	0.196	0.207	0.195	0.196	0.207
1st Stage KP F-Stat				685.64	282.92	537.00
Public Housing Share	All	High	Low	All	High	Low

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2012. The sample includes neighborhoods that had public housing in 2000; columns (2) and (5) include only neighborhoods with an above-median share of public housing in 2000, and columns (3) and (6) include only neighborhoods with a below-median share of public housing in 2000. All regressions are fixed effects specifications of log transaction price on (two quarter lagged) homeownership rate that include neighborhood effects, year effects, and quarter (season) effects, as well as building starts in the neighborhood and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. Homeownership rate is scaled from 0 to 1. In columns (3)-(6), the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by the geographic area at which fixed effects are included (noted in the table). * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 8: OLS and IV Price Estimates – Flexible Geographic Controls

Panel A: Buffer Zone Centered on Each Private Transaction

	Dependent Variable: ln($Price$)					
	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.062*** (0.014)	0.161*** (0.025)	0.025** (0.010)	0.060*** (0.014)	0.147*** (0.024)	0.021* (0.011)
N	144,696	77,662	67,034	144,696	77,662	67,034
1st Stage KP F-Stat				2974.73	5551.88	2345.15
Public Housing Share	All	High	Low	All	High	Low

Panel B: Statistical Area Level (Centroid Polynomial)

	Dependent Variable: ln($Price$)					
	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.093*** (0.023)	0.216*** (0.046)	0.043* (0.025)	0.092*** (0.027)	0.218*** (0.046)	0.035 (0.031)
N	288,519	163,148	125,371	288,519	163,148	125,371
1st Stage KP F-Stat				621.96	2159.43	439.68
Public Housing Share	All	High	Low	All	High	Low

Panel C: Block Level (Centroid Polynomial)

	Dependent Variable: ln($Price$)					
	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.072*** (0.024)	0.192*** (0.049)	0.008 (0.023)	0.074** (0.030)	0.195*** (0.050)	–0.007 (0.029)
N	262,469	120,054	142,415	262,469	120,054	142,415
1st Stage KP F-Stat				1272.68	2264.78	1025.09
Public Housing Share	All	High	Low	All	High	Low

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2012. The sample includes neighborhoods that had public housing in 2000; columns (2) and (5) include only neighborhoods with an above-median share of public housing in 2000, and columns (3) and (6) include only neighborhoods with a below-median share of public housing in 2000. All regressions predict log transaction price and include (two quarter lagged) homeownership rate, polynomial controls in latitude and longitude of the neighborhood centroid, locality effects, year effects, and quarter (season) effects, as well as building starts in the neighborhood and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. Homeownership rate is scaled from 0 to 1. In columns (3)-(6), the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by Town (panel A), Statistical Area (panel B), Block (panel C). * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 9: OLS and IV Price Estimates – Building and Apt Fixed Effects
Repeat Sales Sample

Panel A: Buffer Zone Centered on Each Private Transaction

Dependent Variable: ln($Price$)	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.187***	0.218***	0.199***	0.165***	0.179***	0.143***
	(0.015)	(0.027)	(0.036)	(0.016)	(0.029)	(0.040)
N	22,278	22,278	22,276	22,278	22,278	22,276
1st Stage KP F-Stat				19292.14	7994.06	4360.01
Geo Control	Lat-Lon	Building	Apt	Lat-Lon	Building	Apt

Panel B: Statistical Area Level

Dependent Variable: ln($Price$)	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.372***	0.374***	0.353***	0.297***	0.306***	0.286***
	(0.071)	(0.072)	(0.094)	(0.078)	(0.073)	(0.096)
N	44,116	44,116	44,099	44,116	44,116	44,099
1st Stage KP F-Stat				99.63	175.86	73.75
Geo Control	SA	Building	Apt	SA	Building	Apt

Panel C: Block Level

Dependent Variable: ln($Price$)	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.464***	0.475***	0.444***	0.351***	0.380***	0.329***
	(0.104)	(0.110)	(0.111)	(0.109)	(0.105)	(0.102)
N	32,829	32,829	32,817	32,829	32,829	32,817
1st Stage KP F-Stat				46.64	79.42	65.23
Geo Control	Block	Building	Apt	Block	Building	Apt

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2012. The sample includes transactions for which repeat sales information is available in neighborhoods that had high initial shares of public housing in 2000. All regressions predict log transaction price and include (two quarter lagged) homeownership rate, year effects, and quarter (season) effects, as well as building starts in the neighborhood and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. Homeownership rate is measured at the geographic level indicated in each panel. In columns (1) and (4), the geographic control included varies by panel (indicated); in columns (2) and (5), building fixed effects are included; in columns (3) and (6), apartment fixed effects are included. Homeownership rate is scaled from 0 to 1. In columns (3)-(6), the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by building (Panel A), Statistical Area (Panel B), Block (Panel C). * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 10: Within-Town Difference-in-Differences Estimates
Large vs. Small Homeownership Rate Increases

Panel A: Statistical Area Level

	(1)	(2)	(3)	(4)
Dependent Variable: ln(p)	OLS	IV	OLS	IV
I{year≥2005} x I{>p50 HRchg}	0.075***	0.069***		
	(0.017)	(0.017)		
I{year≥2005} x I{>p75 HRchg}			0.081***	0.058***
			(0.026)	(0.021)
Observations	87,287	87,287	25,186	25,186
R-squared	0.724	0.723	0.743	0.733

Panel B: Block Level

	(1)	(2)	(3)	(4)
Dependent Variable: ln(p)	OLS	IV	OLS	IV
I{year≥2005} x I{>p50 HRchg}	0.124***	0.124***		
	(0.021)	(0.022)		
I{year≥2005} x I{>p75 HRchg}			0.221***	0.216***
			(0.037)	(0.044)
Observations	68,558	68,558	25,642	25,642
R-squared	0.749	0.748	0.764	0.784

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2008, the years just before and after the beginning of the "This is My Home" sale event. The sample includes only Statistical Areas that had above-median public housing in 2000 and only towns that include neighborhoods with both large and small homeownership changes. In the first row, a large increase is defined as above-median (relative to below-median). In the second row, a large increase is defined as above 75th percentile (relative to below 25th percentile). All regressions are fixed effects specifications of log transaction price that include town effects, year effects, and quarter (season) effects, as well as the main effect of Large Change, building starts in the neighborhood and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. In columns (2) and (4), treatment status (large increase in homeownership rate) is determined using the sale-discount predicted homeownership rate, as described in Section 5.1 of the text. Standard errors are clustered by neighborhood. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table 11: OLS and IV Price Estimates of Voter Turnout and School Quality

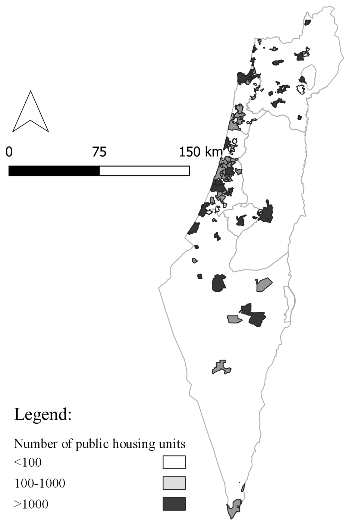
Dependent Variable:	Voter Turnout		School Quality	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Homeownership Rate	0.045**	0.054**	0.217**	0.248**
	(0.021)	(0.023)	(0.099)	(0.109)
N	2,252	2,252	1,570	1,570
N Clusters	418	418	468	468
1st Stage KP F-Stat		702.30		639.30

Notes: An observation in the sample is a Statistical-Area-year for the years 1999-2015 in columns (1) and (2), and for the years 2002-2012 in columns (3) and (4). The sample includes neighborhoods that had high public housing shares in 2000. The dependent variable in columns (1) and (2) is voter turnout in parliamentary elections, as a share of eligible voters. The dependent variable in columns (3) and (4) is elementary school quality, as measured by the average standardized test score for 5th and 8th graders on the Meitzav tests. All regressions include year fixed effects, town fixed effects, and polynomial controls for latitude and longitude of SA centroids. Regressions (3) and (4) also control for SA population and employment rates. In columns (2) and (4), the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by SA. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

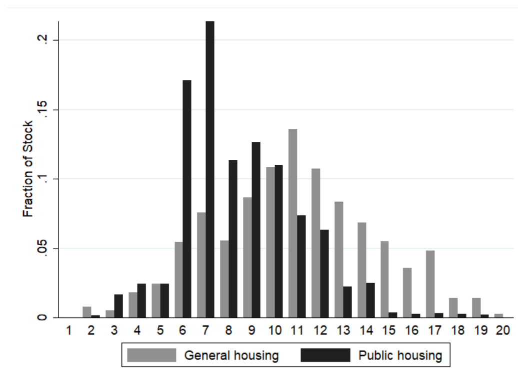
Appendix for Online Publication

A Appendix Tables and Figures

Figure A1: Geographic Distribution of Public Housing, 2000



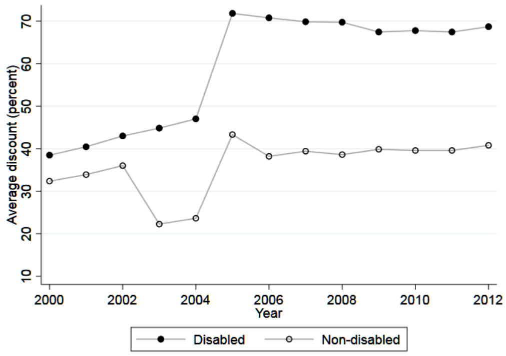
(a) Map of Public Housing



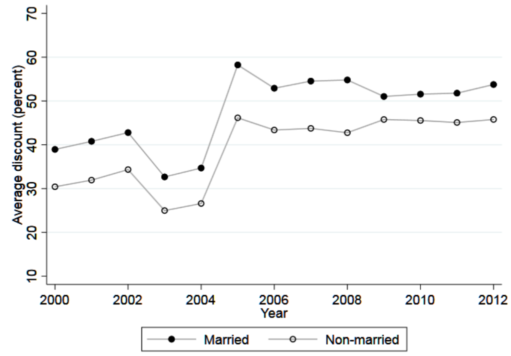
(b) Public and Private Housing by Neighborhood SES

Notes: Panel (a) reflects only localities with more than 5000 residents. The SES grade in Panel (b) is scaled from 1 (lowest) to 20 (highest). Areas of SES level 1 are almost exclusively Arab localities and are excluded from the sample.

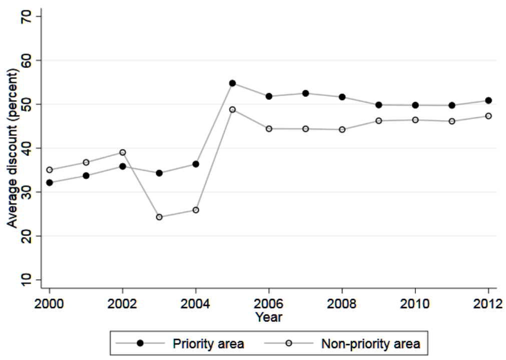
Figure A2: 2005 Increase in Discounts – Additional Sub-Groups



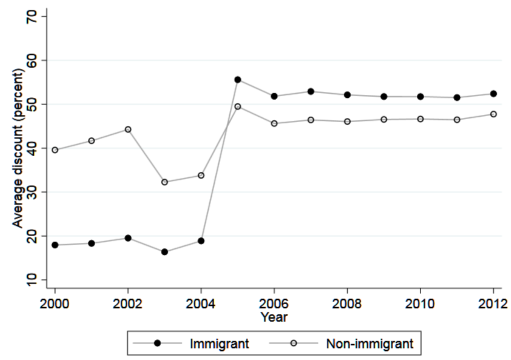
(a) By Disability Status



(b) By Marital Status



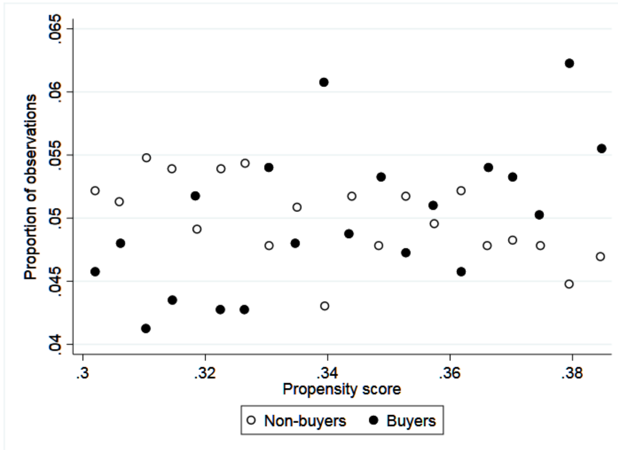
(c) By Geographic Priority Area



(d) By Recent Immigrant Status

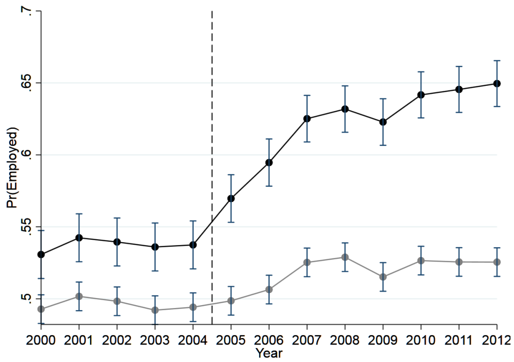
Notes: Figure shows the average discount faced in each year by public housing tenant characteristics that affect discount size. Panel (a) shows average discounts by disability status of a family member. Panel (b) shows average discounts by marital status. Panel (c) shows average discounts by geographic priority area. Panel (d) shows average discounts by recent (post-1989) immigrant status. Discount determination rules are summarized in Appendix Table B1 The "Buy Your Home" sale was in place from 2000-2004; the "This is My Home" sale was in place from 2005-2008; the "My Own Apartment" sale was in place from 2009-2012.

Figure A3: Propensity Scores of Buyers and Non-Buyers
25th to 75th Percentile Common Support Sample

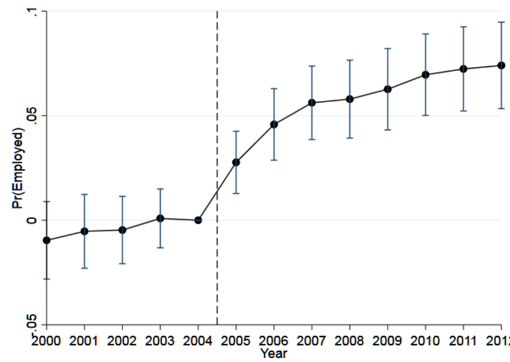


Notes: Graph shows the proportion of observations of buyers and non-buyers at each propensity score. The sample includes observations between the 25th and 75th percentiles of propensity scores.

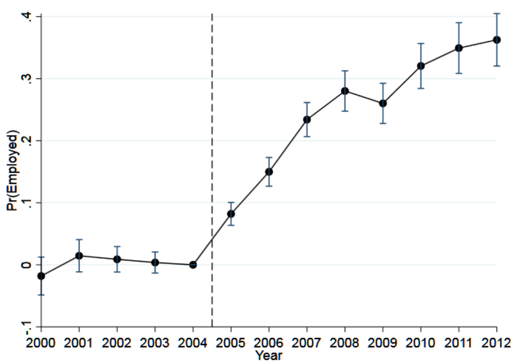
Figure A4: Employment Probability Effects: Four Methods of Comparison, 10th-90th Percentile Common Support



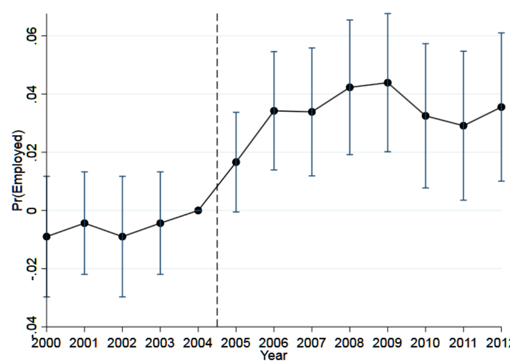
(a) Raw Data



(b) OLS Fixed Effects



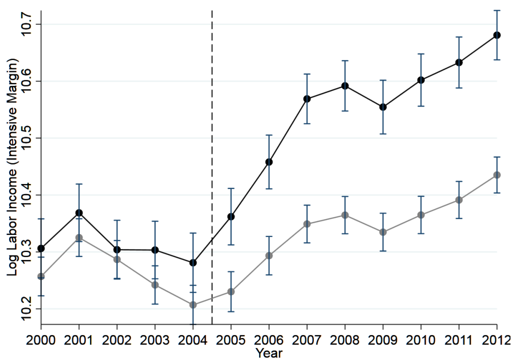
(c) IV



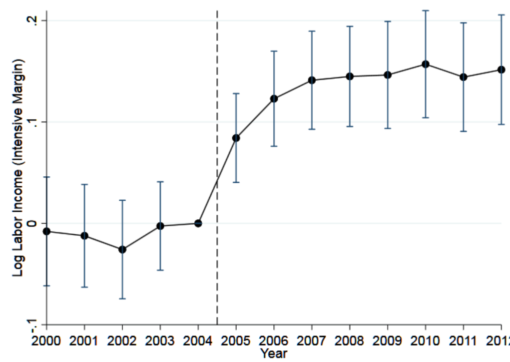
(d) Nearest Neighbor Matching

Notes: Points on the graphs in panels (b), (c), and (d) represent coefficients on treatment times year interactions, where treatment is defined as becoming a homeowner during the "This is My Home" sale period (2005-2008), in regressions predicting employment and including an indicator for being an ever-buyer, year effects, and time-varying demographic controls. Points on the graph in panel (a) represent coefficients on group times year interactions, where groups are buyers or non-buyers, in separate regressions predicting employment and including year effects and time varying demographic controls. Sample includes all households in the 25th to 75th percentile propensity score common support, where propensity scores are predicted using ex-ante demographics. The "This is My Home" sale event began in 2005. In panel (c), buying is instrumented with discounts, as described in Section 4.2 of the text. Standard errors are clustered at the household level. Error bars represent 95% confidence intervals.

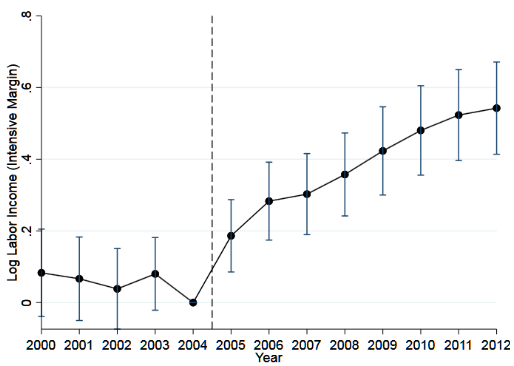
Figure A5: Labor Income Effects (Intensive Margin): Four Methods of Comparison, 10th-90th Percentile Common Support



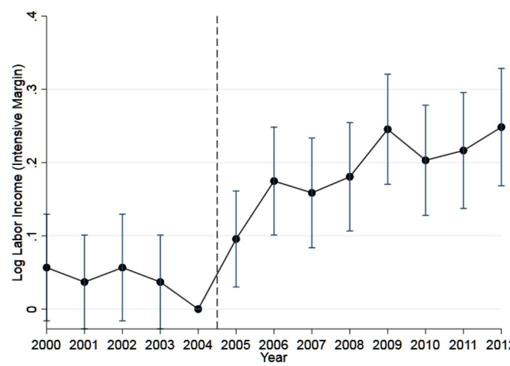
(a) Raw Data



(b) OLS Fixed Effects



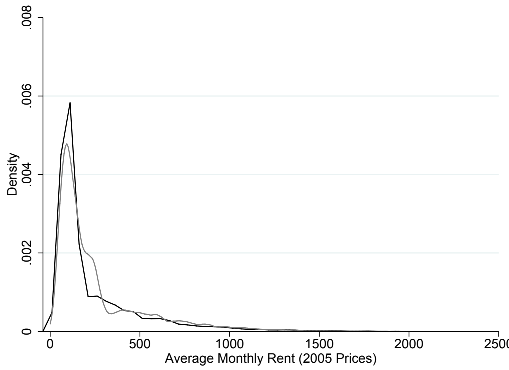
(c) IV



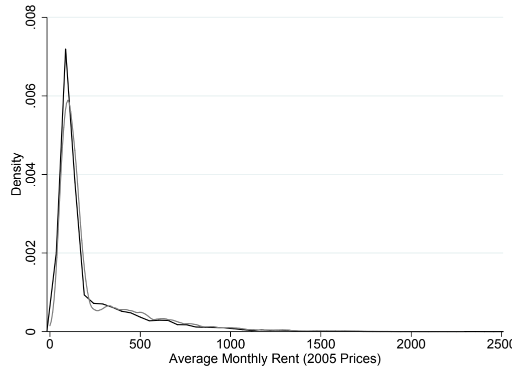
(d) Nearest Neighbor Matching

Notes: Points on the graphs in panels (b), (c), and (d) represent coefficients on treatment times year interactions, where treatment is defined as becoming a homeowner during the "This is My Home" sale period (2005-2008), in regressions predicting log labor income and including an indicator for being an ever-buyer, year effects, and time-varying demographic controls. Points on the graph in panel (a) represent coefficients on group times year interactions, where groups are buyers or non-buyers, in separate regressions predicting log labor income and including year effects and time varying demographic controls. Sample includes all households in the 10th to 90th percentile propensity score common support, where propensity scores are predicted using ex-ante demographics. The "This is My Home" sale event began in 2005. In panel (c), buying is instrumented with discounts, as described in Section 4.2 of the text. Standard errors are clustered at the household level. Error bars represent 95% confidence intervals.

Figure A6: Distribution of Rent Levels, by Year



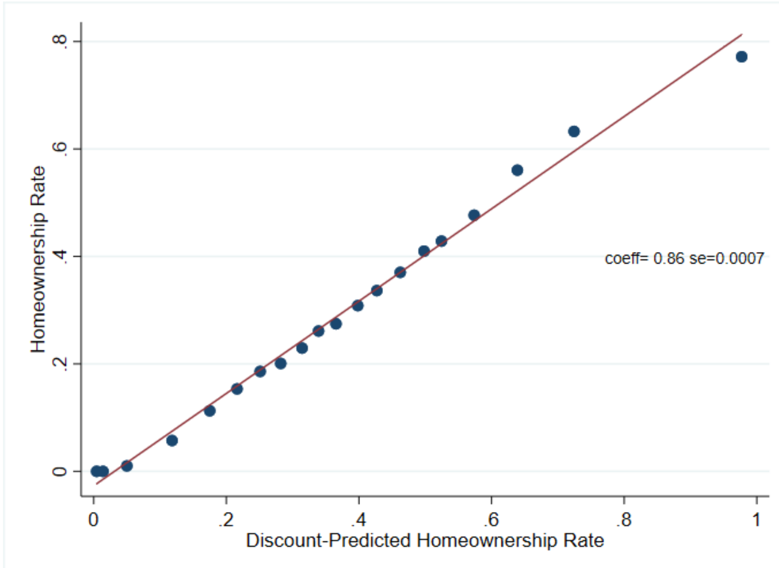
(a) 2000 vs. 2008



(b) 2004 vs. 2006

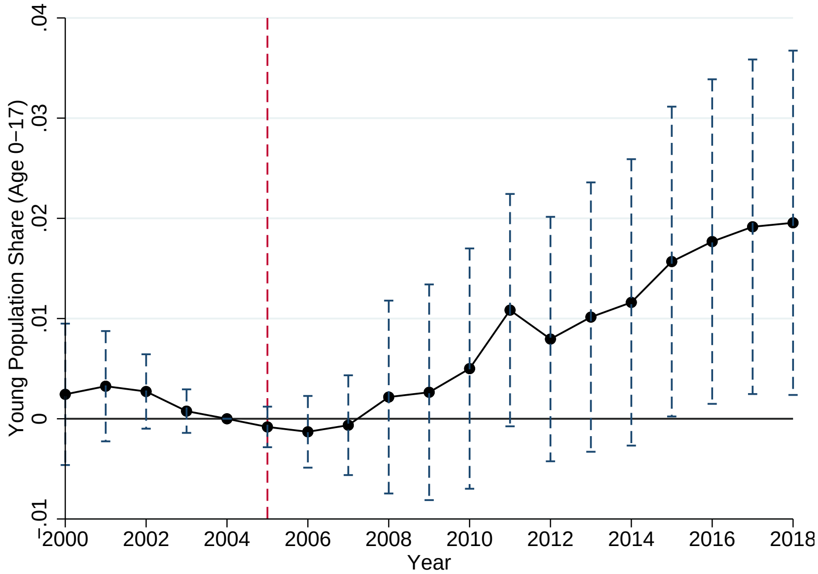
Notes: Kernel density plots show the distribution of monthly net (after discount) rent levels for all sample households. The gray curve reflects the later year in each panel. Panel (a) shows the distributions at the beginning and end of the period spanning the "This is My Home" sale event that we study. Panel (b) shows the distributions just before and just after the November 2005 change in rent determination and housing value assessments. All values shown are in 2005 NIS, where $1 USD=4.6 NIS in 2005.

Figure A7: Predictive Power of Aggregated Instrument



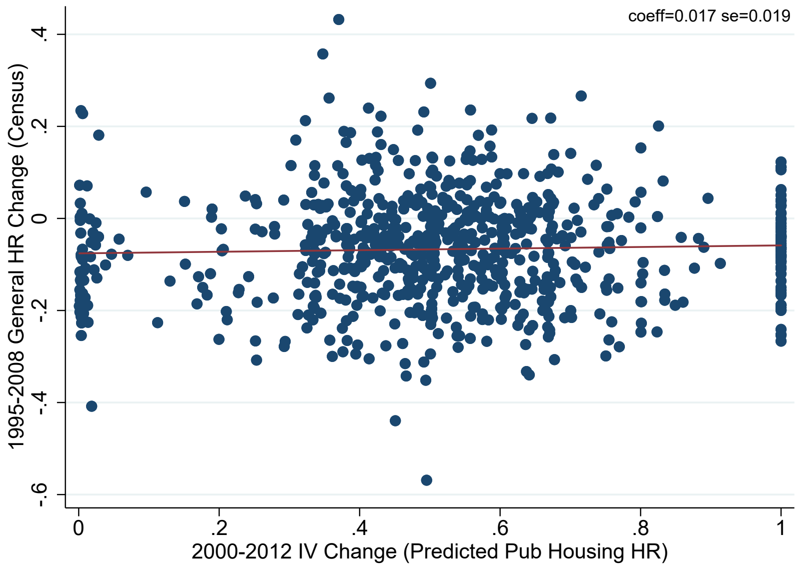
Notes: The figure presents a binscatter plot of the relationship between the actual homeownership rate in each statistical area and the aggregated instrument: the homeownership rate predicted by discounts to the sale prices of individual units and the resulting predicted probability of sale. The sample includes the years 2000-2012.

Figure A8: Within-Town Difference-in-Differences
Effect on Young Population Share in Neighborhood



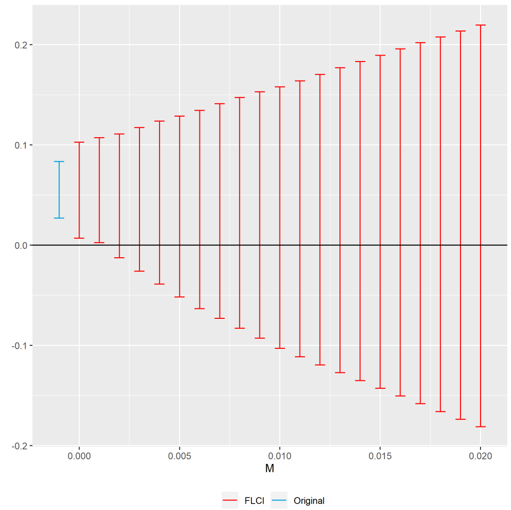
Notes: Points on the graphs represent coefficients on treatment times year interactions, where treatment is defined as SAs with above-75th percentile increases in homeownership rates and control SAs have below 25th percentile increases in homeownership rates. The sample includes only SAs with high initial public housing shares, and only towns with both treated and control SAs. The "This is My Home" sale event began in 2005. The y-axis measures share of local population aged 0-17. Regressions include year effects and town effects. Standard errors are clustered at the SA level. Error bars represent 95% confidence intervals.

Figure A9: Orthogonality of Instrument to Change in General Homeownership Rates

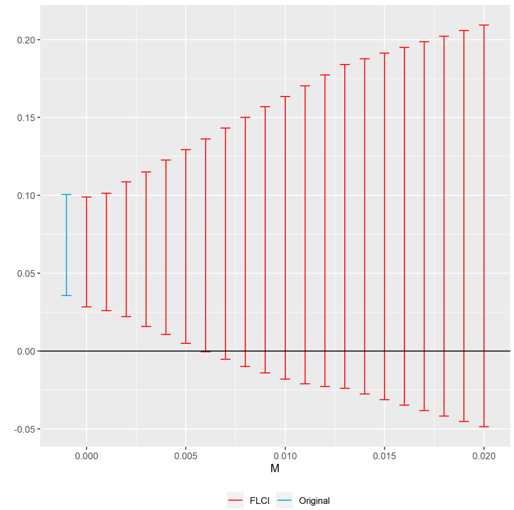


Notes: Points on the graph represent a scatter plot at the statistical area level of the 1995-2008 change in general (non-public) housing stock homeownership rates, as measured by the Israeli Census, against the 2000-2012 change in the IV-predicted public housing homeownership rate. 1995 and 2008 are the Census years closest to the years spanning our sample. The slope coefficient and standard error of the best fit linear prediction are shown in the upper right hand corner.

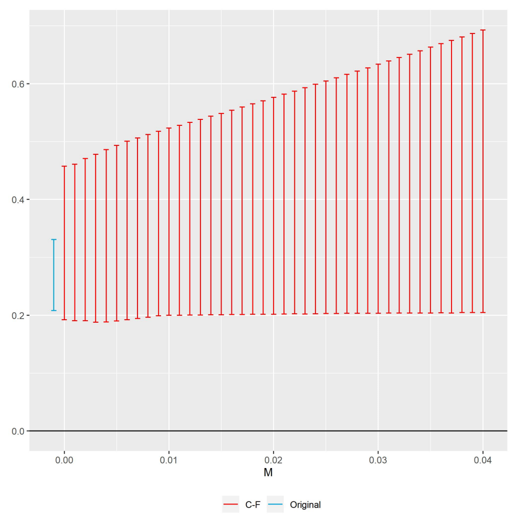
Figure A10: Robustness to Non-linear Differential Pre-treatment Trends
OLS and NNM-1 Long-Term Employment Effects



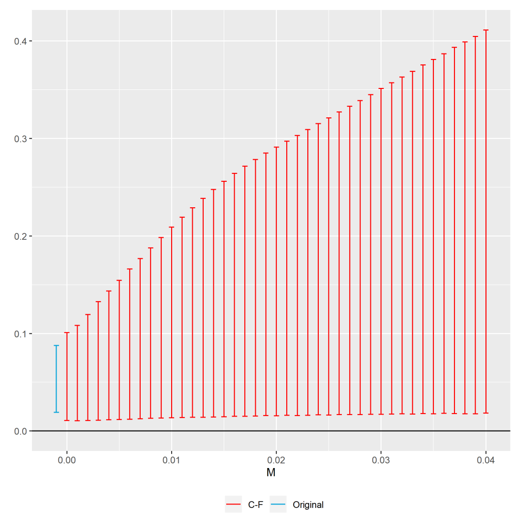
(a) OLS, Non-Linear Trends



(b) NNM, Non-Linear Trends



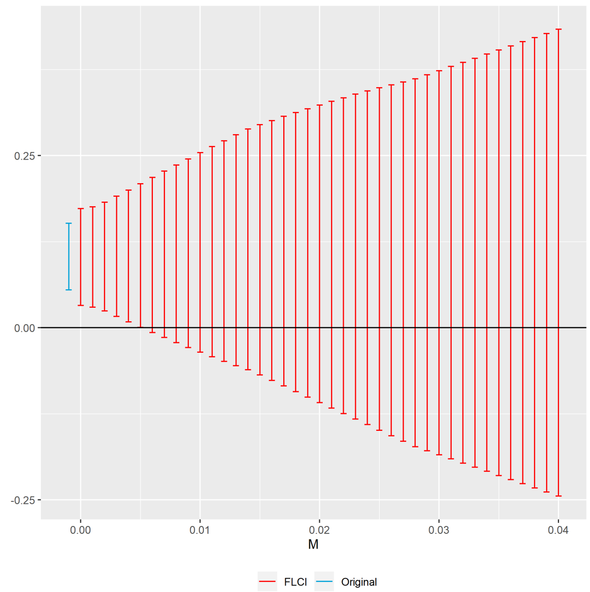
(c) IV, Monotonic Trends



(d) NNM, Monotonic Trends

Notes: Graphs present robust confidence intervals for the 2007 treatment coefficient under alternative assumptions regarding differential pre-treatment trends across treatment and control groups, following Rambachan and Roth (2019). The blue CI reflects the original CI under the parallel trends assumption. The first red CI is robust under the assumption of differential but linear pre-treatment trends. Subsequent CIs are robust allowing for increasing degrees of non-linearity, M, of differential pre-treatment trends. All panels allow for non-linearity in differential trends. Panels A10c and A10d allow specifically for monotonically decreasing trends, according to the direction of pre-treatment coefficients observed.

Figure A11: Robustness to Non-linear Differential Pre-treatment Trends
OLS Price Difference-in-Differences Effect



Notes: Graph presents robust confidence intervals for the 2007 treatment coefficient under alternative assumptions regarding differential pre-treatment trends across treatment and control groups, following Rambachan and Roth (2019). The blue CI reflects the original CI under the parallel trends assumption. The first red CI is robust under the assumption of differential but linear pre-treatment trends. Subsequent CIs are robust allowing for increasing degrees of non-linearity, M, of differential pre-treatment trends.

Table A1: Probability of Becoming a Homeowner during "This is My Home," as a Function of Discounts

Dependent Variable:	Bought during "This is My Home" Sale, 2005-2008					
	(1)	(2)	(3)	(4)	(5)	(6)
Discount_t	1.212***	0.681***	0.709***	−0.267	−0.245	−0.347
	(0.086)	(0.108)	(0.109)	(0.350)	(0.331)	(0.330)
Discount_t^2				2.704***	2.170***	2.440***
				(0.889)	(0.836)	(0.834)
Discount_t^3				−2.375***	−2.035***	−2.206***
				(0.636)	(0.597)	(0.597)
Tenure (Cts.)		0.043***			0.014***	
		(0.002)			(0.001)	
0-5 Yrs. Tenure			−1.408***			−0.444***
			(0.090)			(0.023)
6-10 Yrs. Tenure			−1.167***			−0.389***
			(0.063)			(0.020)
11-15 Yrs. Tenure			−1.054***			−0.356***
			(0.055)			(0.018)
16-20 Yrs. Tenure			−0.856***			−0.303***
			(0.051)			(0.017)
21-25 Yrs. Tenure			−0.661***			−0.243***
			(0.045)			(0.016)
Disabled		−0.103*	−0.112**		−0.023	−0.024
		(0.055)	(0.055)		(0.016)	(0.016)
Married		0.234***	0.224***		0.064***	0.057***
		(0.049)	(0.049)		(0.014)	(0.014)
Num. Children		0.052***	0.062***		0.017***	0.020***
		(0.019)	(0.019)		(0.006)	(0.006)
HH age		0.005	0.002		0.001	−0.000
		(0.003)	(0.003)		(0.001)	(0.001)
Apt Characteristics		Yes	Yes		Yes	Yes
Geo FE		Yes	Yes		Yes	Yes
Model	Probit	Probit	Probit	Non-Par	Non-Par	Non-Par
Num. Households	3,633	3,633	3,633	3,633	3,633	3,633

Notes: Table presents probit and flexible non-parametric estimates of entry in to homeownership as a function of sale discounts, scaled as a rate between 0 and 1. Regressions are at the household-year level and the dependent variable is an indicator for buying during the "This is My Home" Sale between 2005 and 2008. Regressions in columns (2), (3), (5), and (6) include geographic area fixed effects and controls for regional unemployment, household demographics, and apartment characteristics. The omitted tenure group is tenure>25yrs. Sample includes buyers in the "This is My Home" sale event and never-buyer public housing tenants. Standard errors are clustered at the household level. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A2: IV First Stage Estimates for Labor Supply Regressions

	Dependent Variable: I{Homeowner} x I{year≥2005}			
	(1)	(2)	(3)	(4)
I{year≥2005} x Discount IV	0.770***	0.834***	0.812***	0.925***
	(0.031)	(0.015)	(0.038)	(0.018)
Observations	32,697	118,323	20,567	74,137
R-squared	0.463	0.481	0.472	0.522
Num. Clusters	3,633	13,147	2,855	10,288
1st Stage KP F-Stat	636.2	3129	457.3	2614
2nd Stage Outcome	Employed	Employed	Log(Inc)	Log(Inc)
Common Support PS Pctiles	25th-75th	10th-90th	25th-75th	10th-90th

Notes: An observation in the sample is a household-year for the years 2000-2008 and households living in public housing at the beginning of the period. The sample in columns (1) and (3) includes households in the 25thth-75th percentile common support of propensity to buy their units during the sale period "This is My Home," while the sample in columns (2) and (4) includes households in the 10th-90th percentile common support. Estimates in each column represent first stage IV results corresponding to the second stages presented in column (2) of Tables 2, 3, A3, and A4. All regressions are fixed effects specifications including year effects, household effects, and controls for number of kids under 18, marital status, years since immigration, having a disabled household member, and the regional unemployment rate. Second stage outcomes are long-term employment (columns 1 and 2) and log labor income (cols 3 and 4). The instrument is constructed as described in Section 4.2 of the text. Standard errors are clustered by household. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A3: Estimates of the Homeownership Effect on Long-Term Employment, 10th-90th Percentile Common Support

Dependent Variable: Employment	(1) OLS	(2) IV	(3) NNM-1	(4) NNM-3
I{year≥2005} x I{Homeowner}	0.0504***	0.141***	0.0367***	0.0285**
	(0.0065)	(0.021)	(0.0081)	(0.0122)
Observations	118,323	118,323	61,866	61,506
R-squared	0.656	0.654	0.479	0.456
Num. Clusters	13,147	13,147	6,874	5,073
1st Stage KP F-Stat		1564		

An observation in the sample is a household-year for the years 2000-2008 and households living in public housing at the beginning of the period. The sample includes households in the 10th-90th percentile common support of propensity to buy their units during the sale period "This is My Home." Regressions are fixed effects specifications of long-term employment on the interaction of a homeowner indicator with an indicator for after the start of the sale period, including, year effects, household effects, and controls for number of kids under 18, marital status, years since immigration, having a disabled household member, and the regional unemployment rate. Long-term employment is defined as employment for at least 6 months in a row. In column (2), the homeownership x after interaction is instrumented for using sale discounts, as described in Section 4.2 of the text. Columns (3) and (4) implement nearest neighbor matching estimators with one match (column 3) and 3 matches (column 4) for each treated household; in these regressions, match group fixed effects replace household fixed effects, and not all control units serve as matches. Standard errors are clustered by household. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A4: Estimates of the Homeownership Effect on Labor Income, 10th-90th Percentile Common Support

Dependent Variable: Log(Labor Inc.)	(1) OLS	(2) IV	(3) NNM-1	(4) NNM-3
I{year≥2005} x I{Homeowner}	0.160***	0.359***	0.115***	0.0756**
	(0.018)	(0.052)	(0.026)	(0.0350)
Observations	76,634	76,634	39,866	39,705
R-squared	0.699	0.698	0.535	0.506
Num. Clusters	10,444	10,444	5,591	4,277
1st Stage KP F-Stat		1287		

Notes: An observation in the sample is a household-year for the years 2000-2008 and households living in public housing at the beginning of the period. The sample includes households in the 10th-90th percentile common support of propensity to buy their units during the sale period "This is My Home." Regressions in columns (1) and (2) are fixed effects specifications of log income on the interaction of a homeowner indicator with an indicator for after the start of the sale period, including, year effects, household effects, and controls for number of kids under 18, marital status, years since immigration, having a disabled household member, and the regional unemployment rate. In column (2), the homeownership x after interaction is instrumented for using sale discounts, as described in Section 4.2 of the text. Columns (3) and (4) implement nearest neighbor matching estimators with one match (column 3) and 3 matches (column 4) for each treated household; in these regressions, match group fixed effects replace household fixed effects, and not all control units serve as matches. Standard errors are clustered by household. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A5: Hazard Model of Public Housing Sales to Tenants

Dependent Variable:		Sale(t)	
	(1)	(2)	(3)
Discount	0.0199***	0.0139***	0.0112***
	(0.0004)	(0.0005)	(0.0005)
Tenure in Public Housing		0.0729***	0.0810***
		(0.0014)	(0.0014)
Household member with disabilities		-0.764***	-0.698***
		(0.031)	(0.032)
Married		0.0333*	0.0307
		(0.0244)	(0.0247)
Num. Children		0.123***	0.509***
		(0.007)	(0.027)
(Num. Children)^2			-0.0955***
			(0.0067)
(Num. Children)^3			0.00514***
			(0.0004)
Post 1989 Immigrant		0.468***	0.551***
		(0.026)	(0.026)
Household Head Age		-0.0390***	-0.211**
		(0.0014)	(0.083)
(Household Head Age)^2			0.0071***
			(0.0018)
(Household Head Age)^3			-7.24e-05***
			(1.30e-05)
Floor of Building	-0.0136*	-0.0448***	-0.0444***
	0.0072	(0.0077)	(0.0077)
Year Built	0.2880***	0.0570***	0.0593***
	(0.0013)	(0.0017)	(0.0017)
Number of Rooms	0.139***	-0.0217	-0.0400
	(0.029)	(0.0292)	(0.0292)
Floor Space	0.0084***	0.0165***	0.0178***
	(0.0014)	(0.0014)	(0.00142)
Geo FE	Yes	Yes	Yes
Constant	-63.92***	-120.0***	-124.8***
	(2.640)	(3.310)	(3.538)
Observations	238,822	238,822	238,822

Notes: Estimates presented are coefficients from a hazard model predicting the sale of public housing units and including geographic area fixed effects. Sample includes buyers in the "This is My Home" sale event and never-buyer public housing tenants. Bootstrapped standard errors are reported. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A6: OLS and IV Price Estimates – Neighborhood Fixed Effects
Shorter Estimation Window, 2000-2008

Panel A: Statistical Area Level

	Dependent Variable: ln($Price$)					
	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.089***	0.393***	0.024	0.082***	0.330***	0.028
	(0.023)	(0.067)	(0.022)	(0.025)	(0.068)	(0.024)
N	170,208	94,081	76,127	170,208	94,081	76,127
1st Stage KP F-Stat				752.75	188.78	614.16
Public Housing Share	All	High	Low	All	High	Low

Panel B: Block Level

	Dependent Variable: ln($Price$)					
	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.098***	0.480***	0.020	0.090***	0.421***	0.021
	(0.026)	(0.144)	(0.022)	(0.026)	(0.120)	(0.024)
N	154,907	67,837	87,070	154,907	67,837	87,070
1st Stage KP F-Stat				590.85	86.85	508.91
Public Housing Share	All	High	Low	All	High	Low

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2008. The sample includes neighborhoods that had public housing in 2000; columns (2) and (5) include only neighborhoods with an above-median share of public housing in 2000, and columns (3) and (6) include only neighborhoods with a below-median share of public housing in 2000. All regressions are fixed effects specifications of log transaction price on (two quarter lagged) homeownership rate that include neighborhood effects, year effects, and quarter (season) effects, as well as building starts in the neighborhood and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. Homeownership rate is scaled from 0 to 1. In columns (3)-(6), the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by the geographic area at which fixed effects are included (noted in the table). * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A7: OLS and IV Price Estimates – Flexible Geographic Controls
Shorter Estimation Window, 2000-2008

Panel A: Buffer Zone Centered on Each Private Transaction

	Dependent Variable: ln($Price$)					
	OLS	OLS	OLS	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Homeownership Rate$_{t-2}$	0.055***	0.145***	0.023**	0.050***	0.120***	0.024**
	(0.015)	(0.028)	(0.010)	(0.015)	(0.024)	(0.011)
N	82,240	41,072	41,168	82,240	41,072	41,168
1st Stage KP F-Stat				2385	3720	1895
Public Housing Share	All	High	Low	All	High	Low

Panel B: Statistical Area Level (Centroid Polynomial)

	Dependent Variable: ln($Price$)					
	OLS	OLS	OLS	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Homeownership Rate$_{t-2}$	0.085***	0.171***	0.051*	0.077***	0.151***	0.045
	(0.025)	(0.055)	(0.026)	(0.028)	(0.056)	(0.030)
N	170,215	94,085	76,130	170,215	94,085	76,130
1st Stage KP F-Stat				659	1678	476.2
Public Housing Share	All	High	Low	All	High	Low

Panel C: Block Level (Centroid Polynomial)

	Dependent Variable: ln($Price$)					
	OLS	OLS	OLS	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Homeownership Rate$_{t-2}$	0.092***	0.149**	0.029	0.089***	0.116*	0.013
	(0.028)	(0.059)	(0.026)	(0.034)	(0.060)	(0.032)
N	154,937	67,853	87,084	154,937	67,853	87,084
1st Stage KP F-Stat				916.8	1373	878.8
Public Housing Share	All	High	Low	All	High	Low

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2008. The sample includes neighborhoods that had public housing in 2000; columns (2) and (5) include only neighborhoods with an above-median share of public housing in 2000, and columns (3) and (6) include only neighborhoods with a below-median share of public housing in 2000. All regressions predict log transaction price and include (two quarter lagged) homeownership rate, polynomial controls in latitude and longitude of the neighborhood centroid, locality effects, year effects, and quarter (season) effects, as well as building starts in the neighborhood and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. Homeownership rate is scaled from 0 to 1. In columns (3)-(6), the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by Town (panel A), Statistical Area (panel B), Block (panel C). * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A8: OLS and IV Price Estimates – Excluding TLV and JLM

	Statistical Area		Block	
	OLS	IV	OLS	IV
Dependent Variable: ln(Price)	(1)	(2)	(3)	(4)
Homeownership Rate$_{t-2}$	0.416***	0.390***	0.499***	0.455***
	(0.069)	(0.065)	(0.121)	(0.101)
Observations	148,116	148,116	108,001	108,001
R-Sq	0.820		0.818	
R-Sq Within	0.213	0.213	0.198	0.198
1st Stage KP F-Stat		463.10		239.62
Public Housing Share	High	High	High	High

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2012. The sample includes neighborhoods that had public housing in 2000; all columns include only neighborhoods with an above-median share of public housing in 2000 and are thus analogous to columns (2) and (5) of Table 7. All regressions are fixed effects specifications of log transaction price on (two quarter lagged) homeownership rate that include neighborhood effects, year effects, and quarter (season) effects, as well as building starts in the neighborhood (cols 1-2 only) and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. Homeownership rate is scaled from 0 to 1. In columns (2) and (4), the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by the geographic area at which fixed effects are included (indicated). * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A9: OLS and IV Price Estimates – Alternative Lag Structures

Dependent Variable: ln(Price)	OLS (1)	IV (2)
Homeownership Rate$_t$	0.439***	0.405***
	(0.070)	(0.065)
Homeownership Rate$_{t-1}$	0.427***	0.385***
	(0.070)	(0.065)
Homeownership Rate$_{t-2}$	0.407***	0.367***
	(0.068)	(0.063)
Homeownership Rate$_{t-3}$	0.387***	0.350***
	(0.068)	(0.065)
Homeownership Rate$_{t-4}$	0.380***	0.336***
	(0.067)	(0.064)
Homeownership Rate$_{t-5}$	0.331***	0.286***
	(0.064)	(0.062)
Homeownership Rate$_{t-6}$	0.279***	0.230***
	(0.061)	(0.060)

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2012. The sample includes neighborhoods that had an above-median share of public housing in 2000; column (1) presents OLS estimates, and column (2) presents IV estimates in which the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. All regressions are fixed effects specifications of log transaction price on (two quarter lagged) homeownership rate that include neighborhood effects, year effects, and quarter (season) effects, as well as building starts in the neighborhood and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. Standard errors are clustered by Statistical Area. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A10: Reduced Form Correlations of Market Access Measures with Instrument

Dependent Variable:	Peripherality Rating (1)	Peripherality Rating (2)	Distance from Tel Aviv (km) (3)	Market Access (4)
Δ HR, IV Prediction	0.124	0.079	–0.303	0.008
	(0.122)	(0.117)	(0.210)	(0.124)
N	1,022	1,951	994	1,022
N Clusters	929	929	901	929
R-Sq	0.867	0.870	0.999	0.842
Geo FE	Sub-district	Sub-district	Town	Sub-district
Year of Access Measure	2015	2004, 2015	2015	2015

Notes: An observation in the sample is a Statistical Area in columns (1), (3), and (4), and a Statistical Area-year (2004, 2015) in column (2). The sample includes neighborhoods that had public housing in 2000. All regressions are fixed effects specifications of the indicated market access measure on the IV-predicted 12-year change in homeownership rate and sub-district fixed effects. Market access measure reflects 2015 access and varies at the locality level; the peripherality rating is measured in 2004 and 2015, varying at the locality level. Distance from Tel Aviv is measured at the Statistical Area level. Standard errors are clustered by Statistical Area. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A11: Within-Town Difference-in-Differences Estimates
with Market Access-by-Year Interaction Controls

Panel A: Statistical Area Level

	(1)	(2)	(3)	(4)
Dependent Variable: ln(p)	OLS	IV	OLS	IV
I{year≥2005} x I{>p50 HRchg}	0.064***	0.0587***		
	(0.014)	(0.014)		
I{year≥2005} x I{>p75 HRchg}			0.066***	0.066***
			(0.017)	(0.019)
Observations	87,287	87,287	25,906	25,906
R-squared	0.736	0.735	0.756	0.737

Panel B: Block Level

	(1)	(2)	(3)	(4)
Dependent Variable: ln(p)	OLS	IV	OLS	IV
I{year≥2005} x I{>p50 HRchg}	0.089***	0.074***		
	(0.016)	(0.017)		
I{year≥2005} x I{>p75 HRchg}			0.147***	0.125***
			(0.032)	(0.032)
Observations	67,904	67,904	25,400	25,400
R-squared	0.764	0.763	0.776	0.801

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2008, the years just before and after the beginning of the "This is My Home" sale event. The sample includes only neighborhoods that had above-median public housing in 2000 and only towns that include neighborhoods with both large and small homeownership changes. In the first row of each panel, a large increase is defined as above-median (relative to below-median). In the second row, a large increase is defined as above 75th percentile (relative to below 25th percentile). All regressions are fixed effects specifications of log transaction price that include town effects, year effects, and quarter (season) effects, as well as the main effect of Large Change, a series of distance to Tel Aviv times year dummy interactions, a series of labor market accessibility times year dummy interactions, building starts in the neighborhood, and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. In columns (2) and (4), treatment status (large increase in homeownership rate) is determined using the sale-discount predicted homeownership rate, as described in Section 5.1 of the text. Standard errors are clustered by neighborhood. * denotes significance at the 10% level, ** at 5%, and ***at 1%.

Table A12: OLS and IV Price Estimates – Flexible Geographic Controls with Distance-to-TLV-Year Interaction Controls

Panel A: Buffer Zone Centered on Each Private Transaction

Dependent Variable: ln($Price$)	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.055***	0.135***	0.023**	0.055***	0.117***	0.023**
	(0.013)	(0.024)	(0.009)	(0.013)	(0.022)	(0.010)
N	140,675	75,666	65,009	140,675	75,666	65,009
1st Stage KP F-Stat				3135.44	5428.34	2470.79
Public Housing Share	All	High	Low	All	High	Low

Panel B: Statistical Area Level (Centroid Polynomial)

Dependent Variable: ln($Price$)	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.064***	0.179***	0.013	0.068***	0.173***	0.016
	(0.021)	(0.046)	(0.022)	(0.025)	(0.045)	(0.024)
N	280,508	161,798	118,710	280,508	161,798	118,710
1st Stage KP F-Stat				704.51	2327.00	516.77
Public Housing Share	All	High	Low	All	High	Low

Panel C: Block Level (Centroid Polynomial)

Dependent Variable: ln($Price$)	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Homeownership Rate$_{t-2}$	0.065***	0.167***	0.008	0.067**	0.169***	–0.004
	(0.024)	(0.043)	(0.023)	(0.029)	(0.045)	(0.028)
N	258,847	118,997	139,850	258,847	118,997	139,850
1st Stage KP F-Stat				1758.10	2787.55	1330.31
Public Housing Share	All	High	Low	All	High	Low

Notes: An observation in the sample is a transaction (sale) of an always-private house in the years 2000-2012. The sample includes neighborhoods that had public housing in 2000; columns (2) and (5) include only neighborhoods with an above-median share of public housing in 2000, and columns (3) and (6) include only neighborhoods with a below-median share of public housing in 2000. All regressions predict log transaction price and include (two quarter lagged) homeownership rate, polynomial controls in latitude and longitude of the neighborhood centroid, locality effects, year effects, and quarter (season) effects, as well as a series of distance to Tel Aviv times year dummy interactions, building starts in the neighborhood, and hedonic characteristics of the transacted house: floor space, a series of indicators for number of rooms, an indicator for a multi-unit dwelling, floor, and building age. Homeownership rate is scaled from 0 to 1. In columns (3)-(6), the homeownership rate is instrumented for using sale discounts, as described in Section 5.1 of the text. Standard errors are clustered by Town (panel A), Statistical Area (panel B), Block (panel C). * denotes significance at the 10% level, ** at 5%, and ***at 1%.

B Public Housing Sale Discount Rules

Table B1: Sale Discount Determination Rules by Sale Event

Sale period name	Dates	General description	Discounts Increase With	Discount formula
Buy your home I *(Kne Beitcha I)*	5.2000-8.2003	Discounts determined by disability and public housing tenure as of Jan 2000, with four tenure groups: (1) tenure>=12 years; (2) tenure in [7,12); (3) tenure in [2,7); (4) tenure<2 years.	Public housing tenure, disability.	Tenure<2:No special discount. Tenure∈[2,7):7.5% of the price Tenure∈[7,12): Discount effective at tenure of 12 years: 12*3% (4% for disabled) Tenure≥12: (tenure(2000)*3% (4% if disabled)+additional discount per year following 2000.
Buy your home II *(Kne Beitcha II)*	9.2003-12.2004	Same as above, but with additional variation by geographic priority area. Discount for priority area "A" higher than for "B" and "C".	Public housing tenure, disability, geographic area.	
This is my home I *(Kan Beiti I)*	1.2005-8.2005	Discount formula changes, and discounts rise substantially. Discount eligibility requires tenure of at least 4 years, with a substantial increase in discount for those with tenure of at least 12 years. Discounts granted only for geographic priority areas "A" and "B".	Tenure, marital status, number of children, disability, rent discount level, type of housing assistance certificate.	Tenure<4: 0% Tenure>4, by RDG and CG within family type, tenure≥12 get doubled discounts (first two entries in each row): Single (25%, 10%, 12.5%, 5%) Married (50%, 30%, 25%, 15%) Married, 1 child (70%, 50%, 35%, 25%) Married, 2+ children (85%, 60%, 42.5%, 30%) Family with disabled member (85%, 60%, 42.5%, 30%)
This is my home II *(Kan Beiti II)*	9.2005-12.2006	Formula for geographic priority areas "A" and "B" remains the same. Priority areas "C" households with tenure of at least 6 years newly eligible, though for lower discounts. Discounts depend also on rent-discount group (RDG), type of housing assistance certificate (CG), disability, and wheelchair confinement.	Tenure, marital status, number of children, disability rating, rent discount group, type of housing assistance certificate, geographic location.	Tenure≥12 get doubled discounts (first two entries in each row): Family type* - Priority areas "A"+"B": Single (25%, 10%, 12.5%, 5%) Couple (50%, 30%, 25%, 15%) Family with 1 child (70%, 50%, 35%, 25%) Couple with 2+ children (85%, 60%, 42.5%, 30%) Fam. w/ wheelchair-disabled (85%, 60%, 42.5%, 30%) Fam. w/ non-wheelchair disabled (additional 10% for any of the types above, not exceeding wheelchair amount) Family type* - Priority area "C": Single (15%, 10%, 7.5%, 5%) Couple (40%, 15%, 20%, 7.5%) Family with 1 child (70%, 20%, 35%, 10%) Couple with 2+ children (85%, 25%, 42.5%, 12.5%) Fam. w/disabled member (85%, 60%, 42.5%, 30%) Fam. w/non-wheelchair disabled (Receive additional 10% on any category above, not exceeding wheelchair amount)
This is my home III *(Kan Beiti III)*	2.2007-8.2008	Minimum required tenure reduced to 2 years in areas "A" and "B" and 3 years in area "C". Formula otherwise continued to depend on the same parameters (family type, rent discount group, type of housing certificate, disability rating).	Tenure, marital status, number of children, disability, confinement to wheelchair, rent discount group, type of housing assistance certificate, geographic location.	Tenure≥12 get doubled discounts (first two entries in each row): Family type* - Priority areas "A"+"B": Single (25%, 13%, 12.5%, 5%) Couple (46%, 28%, 23%, 14%) Family with 1 child (69%, 49%, 35%, 25%) Couple with 2+ children (92%, 65%, 46%, 32%) Fam. w/ wheelchair disabled (85%, 60%, 42.5%, 30%) Fam. w/ non-wheelchair disabled (Receive additional 10% for any of the types above, not exceeding wheelchair amount) Family type* - Priority area "C": Single (20%, 13%, 10%, 5%) Couple (40%, 15%, 20%, 7.5%) Family with 1 child (60%, 17%, 30%, 8.5%) Couple with 2+ children (80%, 23.5%, 40%, 12%) Fam. w/ disabled member: (80%, 60%, 42.5%, 30%) Fam. w/ non-wheelchair-disabled: (Receive additional 10% for any of the types above, not exceeding wheelchair amount)
My own apartment *(Dira misheli)*	9.2008-12.2010	Minimum tenure for discount raised to 5 years, with discounts a step function of tenure. Additional discount increments granted based on family type, disability, rent discount group (RDG), type of housing assistance certificate (CG), and geographic priority area.	Tenure, marital status, number of children, disability, confinement to wheelchair, rent discount group, type of housing assistance certificate, geographic location.	Tenure-based step function; percentage points per year (non-wheelchair disabled get additional 25pp): 0-5: 0.5pp/yr; 6-15: 0.75pp/yr; 16-25: 1pp/yr; 26-30: 1.4pp/yr; 31-35: 2pp/yr Then add discount points for family type and region**: Priority areas "A" and "B": Single: 25% (CG=1) , 12.5% (CG=2) Couple: 45% (CG=1) , 23% (CG=2) Family with 1 child: 55% (CG=1) , 28% (CG=2) Family with 2+ children: 65% (CG=1) , 33% (CG=2) Fam. w/ wheelchair-disabled: 85% (CG=1) , 42.5% (CG=2) Priority area "C": Single: 20% (CG=1) , 10% (CG=2) Couple: 40% (CG=1) , 20% (CG=2) Family with 1 child: 50% (CG=1) , 25% (CG=2) Family with 2+ children: 60% (CG=1) , 30% (CG=2) Fam. w/ wheelchair-disabled: 80% (CG=1) , 40% (CG=2)

Notes: Formulas are based Ministry of Housing memos. * Numbers in parentheses are ordered as follows: Rent Discount Group (RDG)=1 and Certificate Group (CG)=0, RDG=0 and CG=0, RDG=1 and CG=1, RDG=0 and CG=1. ** Households with no rent discount are eligible for 30% of the amounts listed.

Table B2: Empirical Discount Rates by Sale Period

		Discount				
Sale Name	Years (Approx.)	Mean	Min	Max	Median	Mode
Buy Your Home I	2000-2002	36.5	0	95	7.5	7.5
Buy Your Home II	2003-2004	30.0	0	95	17	7.5
This is My Home I&II	2005-2006	52.5	0	85	50	85
This is My Home III	2007-2008	52.0	5	92	46	80
My Own Apartment	2009-2012	52.5	4.7	90	51.8	80

Notes: Discount statistics reflect the balanced sample of tenants analyzed in Section 4 of the text. Years correspond approximately to cutoffs of sale event periods, which often occurred mid-year.

C Public Housing Rent Discount Rules

Table C1: Rent Discount Determination Rules

Pre November 2005	**Post November 2005**
Discounts determined by income and family type. Discount is a percentage of assessed market rent (indexed to CPI) as follows:	Discount determined by income, family type, and pre-Nov 2005 discount. Discount is a percentage of assessed market rent (indexed to CPI) as follows:
(1) *Discount Center Rent (DCR)*: 90% discount; granted to households that either (a) have income only from NII subsistence allowances (mostly: high disability rating, alimony, full income support), or (b) fulfill their earning capacity but earn less than 125% of the maximum allowed income for income support (*MaxAllowedInc*). (2) *Social Rent*: 68-83% discount. Granted to families with special needs (e.g. single parents or many children) who fulfill their earning capacity. (3) *Full market rent*: Those who either (a) have AGI > *MaxAllowedInc* or (b) do not fulfill their earning capacity and are not eligible for *Social Rent*. Last mark to market was in the early 1980s.	(1) *High discount*, 91.5%: Those who (a) have income only from NII subsistence allowances or (b) had previous entitlement to DCR. (2) *Regular discount:* For those who fulfill earning capacity and AGI<*MaxAllowedInc*, tenants divided into incumbent vs. new. (a) Incumbents: pre-Nov 2005 rent plus additional 50 NIS+CPI per year, up to a max increase of NIS 350+ CPI or the difference between the old and new full market rent. (b) New tenants: rent = 0.085**MktRent* + 0.915*(*AGI* - *MaxIncSupportAllowance*) / (*MaxAllowedInc* - *MaxIncSupportAllowance*) (3) *"Full Market Rent":* New tenants who (a) do not fulfill earning capacity or(b) have *AGI* > *MaxAllowedInc* have no discount. Incumbents have a one-time increase not larger than NIS 350 (CPI-indexed) and thus never get to full market rent.

Notes: AGI – income considered by the Ministry of Housing for rent discount determination – is the 3 month average (pre-discount request) of labor income, NII allowances of the main tenant and spouse, and 13 of of income of other cohabitating adults. Pre-Nov2005, incomes were meant to be examined every year; post-Nov2005, incomes were to be examined every two years. Note that new tenants post Nov 2005, for whom the discount formula in (2)b of the right panel is relevant, are excluded from our labor supply analysis because we use a balanced panel across years. *MaxAllowedInc* is the income threshhold above which non-disabled, non single-parent tenants were supposed to pay full market rent in the old system, though if they were found to be earning less than that income and not fulfilling their earning capacity, they would also have to pay full market rent. This threshold amount is 125% of the maximum allowed income for receipt of NII income support (varies by family type). *MaxIncSupportAllowance* is the maximum NII income support benefit granted (varies by family type). The following qualify as "full" NII substistence allowances: (1) full income support or partial support plus another allowance, (2) high disability benefits (rating 75% or more), (3) special old age or holocaust survivor allowance, and (4) [Pre-Nov2005 only] single mother alimony allowance. To "fulfill earning capacity" according to the MoH, a household had to have either (1) at least one family member working full time at at least minimum wage, (2) have earnings below minimum wage but an NII-documented partial inability to work. Sources for the table are MoH memos, emails with the MoH, and Mei Ami (2005).

D Summary of 2003 Income Support Changes

For the purposes of understanding labor supply incentives, the most relevant set of changes from the 2002-2003 NII reform were those related to income support benefits.[46] Income support receipt depends on both an employment test and and earnings test. The employment test requires beneficiaries to prove that they are either unable to work (generally, disabled), engaging in a bona fide effort to find a job, or employed with low income. The earnings test requires that earnings are below some threshold that varies by marital status and number of kids; the threshold for a single parent with two children in 2003 after the change, for example, was 150% of the minimum wage.

The employment test rules changed in 2002-2003 in two ways: (1) check-in frequency at the Employment Bureau would now be determined by law, rather than by Employment Bureau branch discretion,[47] (2) mothers of kids aged 2-7 and widows with children newly had to check in at the Employment Bureau to pass the employment test.

Three types of changes were made to the earnings test rules: (1) the earnings disregard was reduced for most families from $263 USD in 2002 to $108 USD in 2003, (2) the maximum income thresholds were reduced somewhat, and (3) the slope of the phase-out was reduced to 0.675 (in some cases from 100%, in others from 0.7-0.8) for singles and married couples with no kids, and to 0.625 (from 0.90, implied) for a married couple with at least two kids.

Finally, maximum benefit amounts were reduced for eligibles, for example to 39.0% of the minimum wage from 49.5% of the minimum wage for a married couple with at least two kids, or to 33.5% of minimum wage from 37.5% of minimum wage for a single adult with a child. All changes applied to both buyers and non-buyers.

[46] A complete description of the reform can be found in National Insurance Institute of Israel (2004).

[47] It is unclear whether de-facto check-in frequency changed.