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Estimating the External Effects of Subsidized Housing Investment on Property Values[1]

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Although housing advocates and urban policymakers promote housing investment as a tool for neighborhood improvement, there is relatively little empirical evidence suggesting that significant, much less substantial, external effects of subsidized housing investment exist. Previous research investigating the impact of subsidized housing investment aimed at blighted properties has been mixed, and the analyses have typically been hampered by limitations in both the data and the number of housing units or projects ("experiments") available for study. This paper makes use of an extraordinarily rich and detailed data set to investigate the external effects of a large number of subsidized housing investment projects on the value of surrounding properties. In particular, we estimate the impacts of 66,000 new, publicly assisted housing units produced in New York City between 1987 and 2000, as part of an ambitious program (the "Ten Year Plan") that ultimately cost more than $5 billion and involved the construction or rehabilitation of over 182,000 units (Schill *et al,* 2002).

Our empirical work relies on a unique, geo-coded administrative data set that includes detailed information on 293,786 sales of apartment buildings, condominium apartments, and single-family homes in New York City between 1980 and 1999. These data allow us to estimate a difference-in-difference specification of a hedonic regression model. census tract fixed effects control for idiosyncratic characteristics of the micro-neighborhoods in which subsidized housing is sited. Intuitively, impact estimates are formed as the difference between price changes of properties immediately surrounding city-assisted housing sites following their completion and price changes of comparable properties in the same neighborhood, but beyond the vicinity of the new housing. Unlike previous work in this area, we estimate distance gradients to allow impacts to vary with distance from city-assisted housing. We also investigate how effects vary across different sub-markets — a question that has received little attention in the literature, despite its implications for public policy.

The results are striking and robust. Prior to project construction, prices of properties surrounding city sites are lower than other properties in the same neighborhood, with price differentials larger for larger projects. After construction, the gap narrows considerably — price appreciation in the vicinity of new housing exceeds that in areas just beyond. The magnitudes of the external effects are found to increase with project size and to decrease with the proportion of units in multi-family, rental buildings. Consistent with expectations, we find that spillover effects diminish with distance from the housing investment sites. In fact, the effects typically fall to zero at a distance of roughly 2,000 feet. As for neighborhood conditions, spillovers are typically larger in the more distressed neighborhoods, but smaller projects appear to generate larger spillovers in neighborhoods that are more prosperous. The results are not sensitive to alternative specifications. In particular, impact estimates based upon a repeat sales specification are nearly identical to those obtained in the hedonic analyses.

In short, the extraordinarily rich data and the size and variety of New York City's subsidized housing investments combined with the difference-in-difference specification offers surprisingly robust and persuasive evidence that subsidized housing investments deliver significant external benefits to urban neighborhoods.

The paper is organized as follows. Section I provides some theoretical background. It reviews the existing literature, highlighting the specific contribution this paper makes to the area, and section II describes the model. Section III provides a description of the data used for the study. Section IV presents our findings on the impacts of subsidized housing and sensitivity analyses results. The paper ends with a summary of the key findings and implications for public policy.

I. Background

Why might housing investment generate external effects? Externalities can be generated both by what is eliminated to construct the housing and by what is created. The former process is a familiar one to students of public economics. Since housing is fixed in space, its value may be influenced by the condition of neighboring properties and a dilapidated building or other eyesore can reduce the value of neighboring homes because it is visually (or otherwise) unappealing. Thus, housing investment can increase property values simply because of what it removes. In fact, new, assisted housing often replaces a disamenity (such as an abandoned boarded-up building or a littered vacant lot) in urban neighborhoods.

Research on the valuation of environmental disamenities provides some evidence that suggests large impacts on nearby property values. The research typically estimates a negative impact of adverse land uses on nearby property values that declines with distance from the disamenity. (See, for example, Kiel and Zabel 2001; Hamilton and Viscusi 1999; Kiel and McClain 1995; Kohlhase 1991; or Michaels and Smith 1990. Farber (1998) provides a summary.) Further, several studies also found that the distance effects of actual or proposed adverse land uses diminish or disappear after the cleanup of sites (Kohlhase 1991) or cancellation of proposed facilities (Smollen *et al,* 1991). Thus, these studies find that the house price gradient 'flattens out' — that is, the location premium variation with distance from the adverse land use vanishes — when the disamenity that caused it is removed.

Of course, housing investment can also create externalities because of what it creates. Property values may rise not simply because the blight has been removed, but also because the new buildings are clean, new, and attractively designed. Or, housing investment can generate spillovers because of a 'demonstration effect' — if the new housing project is successful, it demonstrates that a residential project can be viable in an area, and may attract other investors (Caplin and Leahy 1998). Finally, new housing may yield external benefits if it because of its inhabitants. Increasing the population might improve neighborhood safety by increasing street traffic and providing volunteers for community watches. More residents can also fuel demand for retail services and promote economic development. Thus there may be a 'population growth effect.' Notice, however, that the size and importance of externalities may depend upon the characteristics of the inhabitants — their incomes, for example, or their housing tenure. In particular, bringing homeowners into a neighborhood may increase neighborhood stability and bring in new residents who have strong economic incentives to maintain their homes properly and to become active in neighborhood organizations and political affairs (Ellen et al 2001a).[2] Higher income residents may also be viewed as more desirable neighbors. This might be called a 'population mix effect.'

While fully disentangling these different mechanisms empirically is quite difficult, some insight can be gained by noticing the different implications these suggest about the timing of impacts, the variation in impacts across projects of differing types, etc. As an example, the benefits of blight removal should be felt almost immediately, while other effects (such as those related to occupancy) may take longer to unfold. Of course, if housing markets are characterized by perfect foresight, all project impacts, including occupancy effects, will be capitalized into prices immediately at the time that the project is announced (Poterba 1984).

Further, these mechanisms all suggest to some degree that larger projects should have larger impacts - if, as seems intuitive, a larger number of units is likely to replace a larger source of blight. Perhaps more important, however, is the relationship between project size (number of units) and the growth in population. That is, the population growth effect should be most directly related to project size. In addition, if bringing homeowners and wealthier residents into a community creates larger spillover effects, then we would expect the construction of owner-occupied homes to generate larger spillovers than rental housing. Again, unless there is perfect foresight in the housing market, these population mix and population growth effects should be more closely linked to project completion while the blight removal effect should be more closely tied to the project start. We use data on project start, project completion and project size and tenure mix to try to gain some insight into the relative importance of these.

Prior studies provide mixed evidence about the nature of spillover effects generated by investments in affordable housing. Nourse (1963) and Rabiega, Lin, and Robinson (1984), for instance, find that newly developed public housing can have modest, positive impacts on neighboring property values, while Lyons and Loveridge (1993), Goetz, Lam, and Heitlinger (1996), and Lee, Culhane, and Wachter (1999) find

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[2] The behavior of residents may depend upon the size (or structure) of the buildings - residents of smaller buildings may be more invested in the community and serve as more effective monitors of street life in a

small negative effects, at least associated with certain types of federally subsidized housing. Cummings, DiPasquale, and Kahn (2000) find no effects, when examining a pair of affordable homeownership projects in Philadelphia. Further, even if these studies told a more consistent story, they share data limitations that make it difficult to pinpoint the direction of causality. Are subsidized sites systematically located in weak (strong) neighborhoods, or does subsidized housing lead to neighborhood decline (improvement)?

A few more recent analyses have made strides to overcome this causality problem by using models that compare price changes of properties within certain rings of newly developed housing to price changes citywide, while also controlling for neighborhood (census tract) fixed effects. Briggs, Darden, and Aidala (1999), for instance, use such a design to examine the early effects of seven scattered-site public housing developments in Yonkers, New York and find little effect on their surrounding areas. In studying the impact of a scattered-site public housing program in Denver, Santiago, Galster, and Tatian (2001) supplement this basic design by adding trend variables for the rings surrounding subsidized housing sites to test for changes in price trends after completion.[3] Their results suggest that proximity to dispersed public housing units is, if anything, associated with an increase in the prices of single-family homes. The difficulty here is that it does not allow individual neighborhoods to have a price trend that differs from the citywide pattern; thus, price trends in rings surrounding housing are compared to citywide trends rather than to trends in their own neighborhoods.

Finally, in recent work [Ellen *et al* (2001a, 2001b), Schill *et al* (2002)], we, along with other co-authors, adapt this model to examine the impacts of various housing

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community than residents of larger buildings (Glaeser and Sacerdote 2000).

[3] This method is first presented in Galster, Tatian, and Smith (1999).

programs in New York City. In these studies, we estimate hedonic equations that include a set of quarter-specific, neighborhood fixed effects — that is, there is a unique fixed effect specified for every neighborhood for every quarter in the study period, allowing for a unique trend in prices for each neighborhood in the sample. And, we use a difference-in-difference specification - the impact of housing investment is estimated as the difference between the prices of properties near subsidized housing units (typically defined as within a 500-foot ring) before and after the completion of those units, relative to the price appreciation experienced by properties outside the ring but still in the same neighborhood. We find evidence of significant, positive spillover effects.

In all of these previous studies, however, the treatment of distance has been relatively naïve — captured simply by a dummy variable indicating proximity. None of these has explicitly incorporated distance into their analyses. Yet urban blight and dilapidated housing — usual features of sites slated for publicly subsidized housing — may well create the same sort of price gradients generated by environmental disamenities, implying that housing investment may yield spillover benefits that decline with distance.   And, omitting distance in these models could lead to biased impacts estimates. It could be, for example, that most of the properties that sell within a 500-foot ring of city-assisted projects are located at the edge of the 500-foot ring of impact. If so, and if impacts are larger closer in, then estimates of average impacts within the 500-foot ring will be downwardly biased.

The implication is that including more precise measures of distance will yield more nuanced and precise impact estimates, a task we undertake in this paper. In particular, we apply the distance gradient approach used in research on environmental disamenities to the analysis of the effects of blighted sites and their subsequent replacement with new housing. Thus, we incorporate distance explicitly into the model, estimating the price gradient both before the housing investment and after, yielding estimates of the impact of housing investment that vary with distance. Note, however, that the cleanup of a toxic waste site implies the removal of the disamenity, while building new housing on blighted sites implies the replacement of the disamenity with (possibly) an amenity, which may affect housing values itself. Thus, we might expect not only a flattening of the distance gradient due to the removal of the disamenity but there may also be a change in its sign, *i.e.*, from positive to negative. Alternatively, it is also possible that the new housing is itself a disamenity. In the end, disentangling these is an empirical matter.

We should note that the use of hedonic analyses to estimate impacts may yield biased estimates if there are relevant property characteristics that are unmeasured or omitted from the regression equation. It is, in fact, quite possible that the mix of properties that sells in the vicinity of new, city-assisted housing changes in ways uncaptured by the hedonic variables after the construction of the housing. Perhaps the "better" properties sell once the city-assisted housing is constructed. If so, then hedonic analysis may overstate the spillover benefits. To investigate this potential bias, we estimate impact using a repeat sales specification, which uses only the sample of properties that sell more than once and purges the regression equation of any bias due to omitted (or poorly measured) time-invariant, property-specific characteristics.

A third extension in this paper is to explore whether, and to what extent, the impact of housing investment varies with the characteristics of the neighborhood. Does subsidized housing investment yield larger impacts in poor neighborhoods or rich ones? Michaels and Smith (1990) find significant differences in the impact of hazardous waste sites across submarkets, but few have explored such variation in the context of housing investments. Santiago, Galster, and Tatian (2001) is an exception — it explores whether the effects of scattered-site public housing differ depending on neighborhood characteristics — but it does not explore interactions with project scale. Note that this question is of particular policy interest. If the impact of subsidized housing investment varies with neighborhood characteristics, and it is possible to identify the types of housing investment that will be particularly effective in neighborhoods with particular characteristics, then these results may provide guidance to more effectively deploy future investments.

Optimally targeting projects to neighborhoods may well require considering the interaction of scale and neighborhood characteristics. Consider the impact of building a single new unit. In a relatively high-income neighborhood with little existing blight, the construction of a single new unit may effectively remove all of the blight in the surrounding area, suggesting a relatively big impact. In a poorer neighborhood with many distressed properties, a single unit may, in contrast, be only "a drop in the bucket," with a concomitant small impact - even after project completion, blight remains too high to invite any additional investment. Larger projects, however, may be more successful as a critical mass is reached. Whether (and to what extent) these differences are significant is an empirical matter that we consider.

Note that while the focus of this paper is the impact of units built with city-subsidies, we also explore the impacts of federally assisted housing built during this period. This helps us to generalize results beyond the particular features of the housing built through New York's Ten Year Plan.

Finally, in an effort to disentangle competing explanations about the sources of positive spillover effects we also separately analyze effects upon project start and completion. If all effects are felt upon project start, this suggests either perfect foresight or that the bulk of the effect is driven by the removal of previous blight.

II. The Model

As discussed, researchers have used a variety of strategies to identify the neighborhood impact of subsidized housing. The key challenge is to find a realistic counterfactual against which to compare price changes. Our access to geo-coded data allows us to estimate a difference-in-difference model.

More specifically, our empirical work is based on a hedonic model of the price of property:

$$(1)\ \ln P_{icdt} = \alpha + \beta X_{it} + \delta_c W_c + \gamma R_{it} + \rho_{dt} I_{dt} + \varepsilon_{it}$$

where $\ln P_{icdt}$ is the log of the sales price of property i in census tract c, in community district d, and in quarter t, X_{it} is a vector of property-related characteristics, including age and structural characteristics, W_c are a series of census tract fixed effects, R_{it} is a vector of ring variables (described below), and I_{dt} are a series of dummy variables indicating the quarter and community district of the sale. The coefficients to be estimated are α, β, δ, γ, and ρ, and ε is an error term.

Property related characteristics, X_{it}, include structural characteristics of the properties, including building age, square footage, the number of buildings on the lot, and dummy variables distinguishing eighteen different building classifications such as 'single-family detached' or 'two family home', and so on. We include census tract fixed effects (W_c) to control for unobserved, time-invariant features of different neighborhoods. The Census Bureau originally defined census tracts to capture cohesive neighborhoods, and researchers typically use tracts to proxy for neighborhoods.[4]

The ring variables (R_{it}) capture the impact of proximity to housing units created with city assistance. To be specific, we include "In Ring," a dummy variable that takes a value of one if the property is ever located within 2,000 feet of a subsidized housing unit completed during the study period.[5] Thus, "In Ring" captures baseline differences in sales prices in the ring and outside. Because baseline property values are also associated with both the size of the site and the nature of the project to be built there, we include six separate "In Ring" variables, distinguished by the scale of the project to be built (more or less than 100 units) and whether it the units provide for homeownership, rental, or a mix.

A "Post Ring" dummy variable takes a value of one if the sale is within the ring

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[4] Although census tracts in New York City are relatively small (due to the high population density), they are large enough that there may still be significant within-tract variation in time-invariant characteristics that census tract level fixed effects would not capture, potentially biasing the results. In the end, only individual property-specific fixed effects would fully eliminate this possibility. Notice that a specification with property-specific fixed effects specification is, essentially, a repeat sales model (since property-specific fixed effects can only be estimated in a model with more than one observation for each property) and potentially plagued by the selection issues inherent in repeat sales analysis. That is, properties that sell multiple times may be systematically different from those that do not. Nonetheless, we also estimate a repeat sales model and find the results substantially unchanged.

[5] On average, city blocks in New York City are about 500 feet long. Thus, the 2,000 foot ring allows for impacts extending up to roughly four blocks away from the housing investment. Exploratory work suggested that , we found that the impact of the initial disamenity (blighted site before construction of city-assisted housing) does not appear to extend beyond 2,000-feet. With this said, results are substantially similar when using smaller rings.

of some number of *completed* city-assisted units.[6] The coefficient on Post Ring provides, in the simplest model, the impact estimate. We also include the number of completed units within the ring of the sale and the number of units squared, which allows us to measure the marginal effects of additional subsidized units. We also include a variable indicating the proportion of assisted units in the ring that are in multifamily, rental projects, to capture differences in property prices due to differences in tenure or structure.[7] Finally, "Tpost" equals the number of years between the date of sale and the project completion date for properties in the 2,000 foot ring and allows the impact to vary over time.[8] The Tpost coefficient will be positive if after completion, prices in the rings continue to rise relative to prices in the census tract.

We also include a set of variables that control for proximity to other subsidized housing, since it is possible that the location of these other types of units is correlated with that of the new, city-assisted units. These include occupied units that received renovation subsidies through the city's Ten Year Plan, projects sponsored by the federal government (such as Section 202 and Section 8 units), and units that received city subsidies prior to 1987. In each case, we control for both selection effects (with a set of in ring variables) and post-completion effects.

The final set of variables (I_{dt}) allow separate sets of time dummies (one for each quarter in each year of the study period) for each of the 48 community districts used in the analysis. As noted above, while previous research by other authors has, essentially,

[6] In cases where a sale was within 2,000 feet of more than one unit, we use the completion date of the first completed.
[7] While we would ideally like to distinguish between rental and multifamily effects, they are simply too correlated. The best we can do is to identify a joint effect.
[8] To be clear, Tpost equals 1/365 if a sale is located within the ring of a city-assisted unit and occurs the day after its completion; it equals one if the sale occurs one year after the unit completion; and so on. We

assumed that price changes were relatively constant across the city, this seems particularly inappropriate in a city as large and diverse as New York. Indeed, Schwartz, Susin, and Voicu (2002) find considerable variation in price trends across community districts in New York City.

While specifying the time dummies using a smaller geographic area — say a city block or a census tract — may seem preferable to the community districts, doing so comes at a considerable cost and adds little explanatory power. Put simply, census-tract specific time dummies would add approximately 80,000 more dummy variables to the specification, significantly increasing the number of parameters to be estimated, and greatly reducing degrees of freedom. Moreover, there seems to be little variation in the time dummies within the community districts. An F-test could not reject the joint hypothesis that census-tract specific time dummies are insignificantly different from one another within a community district.[9]

We extend this basic model by introducing interactions between distance, D_i, measured as the Euclidean distance between property i and the nearest project site, and the set of ring variables R_{it}.

$$(2)\ \ln P_{icdt} = \alpha + \beta X_{it} + \delta_c W_c + \gamma R_{it} + \theta R_{it} D_i + \lambda R_{it} D_i^2 + \rho_{dt} I_{dt} + \varepsilon_{it}$$

where θ and λ are additional parameters to be estimated. This specification allows for a pre-project distance gradient within the 2,000-foot ring, a post-completion distance

should note that the environmental disamenities literature has explored alternative ways to specify the decay or acceleration of impacts over time. See Kiel and Zabel (2001), for a useful discussion.

[9] While census tracts sometimes cross community districts, the vast majority are fully contained within a single community district.

gradient (again within the ring) and allows this gradient to change over time post-completion.

We allow for six different in-ring distance interactions, so that the gradients can vary across rings distinguished by the size and composition of their new housing units. At the same time, however, we include only a single squared distance term, which applies to all ring types, in the models presented below. In results available from the authors, we also estimated models in which different quadratic terms were also estimated - an F-test could not reject the hypothesis that the coefficients on the six quadratic terms were insignificantly different from one another across ring types. Taken together, the coefficients on these variables capture the pre-completion distance gradient, inside the 2,000 foot ring, estimating the change in the gap between prices inside and outside the ring as distance to the housing site increases.

We also interact the Post Ring variable with distance to allow impacts to vary with distance in a similar fashion. This produces a post-completion gradient that shows the ring-tract price gap at different distances from the city-assisted housing. By interacting distance with Tpost and number of units, we explore how that gradient changes over time and with project scale. Note that in the specifications shown below, we excluded the interactions of the Post Ring variable and the Tpost variable with the square of distance because these were found to be statistically insignificant in earlier specifications.

Repeat Sales

As discussed above, we also estimate a repeat sales specification of the model.

This method relies solely on properties that sold multiple times over the study period, and is essentially equivalent to including fixed effects at the level of the individual property. Thus, the repeat sales methodology is attractive because it effectively controls for any time invariant characteristics of properties that may be unobserved (or omitted) in the hedonic equations including not only characteristics of neighborhoods but also housing. It may, however, be less desirable than the hedonic approach due to its inherent selection bias problems (only properties that sell multiple times are included). We are aware of only a few studies that have employed both hedonic and repeat-sales to estimate the impact of various neighborhood quality measures on house prices: Kohlhase (1991), Downes and Zabel (2002), and Schwartz, Susin, and Voicu (2002).

To arrive at the repeat sales estimating equation, write an equation for the logarithm of the price of the i^{th} house at time t' following (2):

$$(3)\quad \ln P_{icdt'} = \alpha + \beta X_{it'} + \delta_c W_c + \gamma R_{it'} + \theta R_{it'} D_i + \lambda R_{it'} D_i^2 + \rho_{dt'} I_{dt'} + \varepsilon_{it'}.$$

Subtracting (3) from (2) gives an equation for the change in the log price of housing:

$$(4)\quad \Delta_{t,t'} \ln P_{icd} = \beta \Delta_{t,t'} X_i + \gamma \Delta_{t,t'} R_i + \theta \Delta_{t,t'} R_i D_i + \lambda \Delta_{t,t'} R_i D_i^2 + \rho_{dt} I_{dt} - \rho_{dt'} I_{dt'} + \Delta_{t,t'} \varepsilon_i$$

where $\Delta_{t,t'}$ indicates change between time t and time t'.

Equation (4) can be estimated directly using only information on the changes in the price for houses that sold multiple times, changes in the time-varying characteristics of the house and the ring variables, community district – quarter dummies for the time t and t', and a data set that includes a sufficient number of repeat sales. Notice that if all of the unmeasured (omitted) variables were time invariant, then (4) will not suffer from omitted variable bias, since it is only the time-varying characteristics that remain in the equation.

If the structural characteristics of the house remain essentially unchanged (or the sample of houses includes only those with constant structural characteristics) then (4) can be rewritten as:

$$(5) \quad \Delta_{t,t'} \ln P_{icd} = \gamma\Delta_{t,t'}R_i + \theta\Delta_{t,t'}R_iD_i + \lambda\Delta_{t,t'}R_iD_i^2 + \rho_{dt}\, I_{dt} - \rho_{dt'}\, I_{dt'} + \Delta_{t,t'}\varepsilon_i$$

This is the repeat sales equation that we estimate in this paper. Estimation of (5) yields project impact estimates that are free of any bias due to the omission of time-invariant characteristics of housing or neighborhood. One of the costs of this repeat sales approach, however, is that the model yields no estimates of the coefficients on variables which are time-invariant or that change only rarely or slowly, such as the In Ring variables or the fixed structural characteristics of housing.

Testing for Neighborhood Heterogeneity

To test for heterogeneity in impacts across neighborhoods, we interact all of our ring variables (and the ring-distance interaction variables) with a dummy variable indicating neighborhood type. We test for differences in impacts between low- and high-income areas. An F-test rejected the hypothesis that the coefficients on property characteristics are similar across neighborhoods. Therefore, in this specification, the hedonic variables are also interacted with the neighborhood type dummy.

Eliminating Disamenities vs. Providing Amenities — The Timing of the Critical Event

Our final analysis is an attempt to shed light on the underlying causes of spillover effects. As discussed above, providing subsidized housing can create external effects in two ways. First, by eliminating an eyesore or blight that is creating a disamenity and second, by creating new housing that provides amenities. We try to disentangle these effects by including both start dates and completion dates within a single regression. This gives us some insight into whether impacts are felt prior to completion and whether the actual completion and occupancy of the new housing delivers any additional benefits.

III. Summary of Data and Housing Investment in New York City

In 1985, Mayor Edward I. Koch made a commitment of over $4 billion to build or rehabilitate more than 100,000 housing units over a period of five years. The initiative, commonly referred to as the "Ten Year Capital Plan," or the "Ten Year Plan" ultimately resulted in the expenditure of more than $5 billion and the construction or rehabilitation of over 182,000 units over a period of more than fifteen years, making it the largest municipally supported housing program in the history of the United States. The Ten year Plan encompasses a wide variety of programs to stimulate the production and rehabilitation of housing (see Schill *et al,* 2002, for more detail). Our focus is on estimating the spillover effects of the 66,000 new units that have been produced through the program (either through new construction or the gut rehabilitation of vacant buildings).

Certainly, a principal objective of the Ten Year Plan was to create additional housing opportunities for low- and moderate-income families as well as the homeless. In addition, a focus on neighborhood revitalization was evident from the beginning of the Ten Year Plan. According to the Mayor, "[f]irst, we intend to undertake a major effort to rebuild entire neighborhoods of, perhaps 15 to 25 square blocks throughout the City...[i]t is anticipated that such concentrated revitalization would provide the hub for further development." (Koch 1985, 11.) A document produced by HPD in 1989 made the point even more explicitly: "We're creating more than just apartments — we're re-creating neighborhoods. We're revitalizing parts of the city that over the past two decades had been decimated by disinvestment, abandonment, and arson." (NYC Department of Housing Preservation and Development 1989.)

The location of the Ten Year Plan housing investments was, to some extent, dictated by where the city owned property. During the late 1970s, the city had taken ownership of over 100,000 vacant and occupied apartments as a result of tax foreclosure. This so-called *in rem* housing, named after the legal action that vested title in the city, would provide the raw material for the lion's share of the program. The bulk of housing created through the Ten Year Plan were rental units, created from the gut rehabilitation of formerly vacant buildings.

To undertake the analysis outlined above, we first obtained data from New York City's Department of Housing Preservation and Development (HPD) describing all of the new, city-assisted housing completed between January 1987 and June 2000. For each housing project, this data set indicates the city block on which it was built, the year the project was completed, the type of building structure, the number of units that were built or rehabilitated, and if units are rental or owner-occupied. We also obtained data on the location of occupied units that received renovation subsidies through the city's Ten Year Plan, of federally assisted housing built since 1980 and of other city-subsidized housing projects, completed prior to 1987. The location of federal and older city-sponsored projects is, however, less accurately recorded in the HPD files and their coverage has been less systematic than in the case of the Ten Year Plan housing. Thus, we chose to focus on the impact of the Ten Year Plan units and only control for proximity to the other types of subsidized housing.

We supplement our data on housing investments with geocoded data from two other city sources. First, through an arrangement with the New York City Department of Finance, we obtained a confidential database that contains sales transaction prices for all apartment buildings, condominium apartments and single-family homes over the period 1980–1999.[10] In order to insure that we did not include the sales of Ten Year Plan developments themselves, we attempted to exclude any sales that could potentially be part of a development. Unfortunately, the RPAD and homes sales data do not identify whether a particular property received city subsidies, so we excluded any sale that occurred on the same block as a Ten Year Plan development if the sale was of a building that was constructed after the Ten Year Plan units had been completed.[11] Our final

[10] Because sales of cooperative apartments are not considered to be sales of real property, they are not included in the data set. We should also note that most of the apartment buildings in our sample are rent stabilized. Given that legally allowable rents are typically *above* market rents outside of affluent neighborhoods in Manhattan and Brooklyn, we do not think that their inclusion biases our results (see Pollakowski 1997.)

[11] To provide a margin of error with respect to the construction dates in RPAD, we also excluded sales of buildings on the same block as a Ten Year Plan unit that were built up to two years before the Ten Year Plan units.

sample includes 293,786 property sales, spread across 1,606 census tracts.[12] Because of the long time span of the data, and New York City's size, this is a large sample size compared with much of the literature.

Second, data on building characteristics were obtained from an administrative data set gathered for the purpose of assessing property taxes (the RPAD file). Unfortunately, the RPAD data contains little information about the characteristics of individual units in apartment buildings (except in the case of condominiums).[13] Nonetheless, these building characteristics explain variations in prices surprisingly well, suggesting the data are rich enough for estimating hedonic price equations.[14]

As mentioned above, identifying properties in the vicinity of housing investment sites was critical to our analyses. We used GIS techniques to measure the distance from each sale in our database to all Ten Year Plan and other housing sites and, from these distance measures, created a variable that identified properties within 2000 feet of housing investments of different types. We also created a continuous distance variable (for the models with distance) equal to the distance from property to the closest project Site.[15]

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[12] We limited the analysis to properties that are located within the 48 community districts (of the total 59) where there were more than 100 Ten Year Plan units developed that were either (1) rehabilitation of occupied, *in rem* buildings, (2) rehabilitation of vacant buildings, or (3) new construction.

[13] Note that most of the RPAD data we use were collected in 1999, and it is conceivable that some building characteristics may have changed between the time of sale and 1999. However, most of the characteristics that we use in the regressions are fairly immutable (*e.g.*, corner location, square feet, presence of garage), and when we merged RPAD data from 1990 and 1999, we found that characteristics changed very rarely (Ellen et al 2001a). Even among these apparent changes, we suspect that a majority are corrections, rather than true changes.

[14] See (Ellen et al 2001a) for more detail on the data and parameter estimates on the building characteristics in a similar model.

[15] Since all buildings in New York City have been geocoded by the New York City Department of City Planning we used a "cross-walk" (the "Geosupport File") which associates each tax lot with an x,y coordinate (*i.e.* latitude, longitude using the US State Plane 1927 projection), police precinct, community district and census tract. A tax lot is usually a building and is an identifier available to the homes sales and RPAD data. We are able to assign x,y coordinates and other geographic variables to over 98 percent of the

Table 1 shows summary statistics. The first column shows the characteristics of our full sample of property sales; the second column shows the characteristics of transacting properties that are located or at point will be located within 2,000 feet of a new city-assisted unit. As shown, most of the sales in our sample were located in Brooklyn and Queens, largely because those boroughs include a relatively large share of smaller properties, which sell more frequently than apartment buildings. Nearly two thirds of all buildings sold were either one- or two-family homes, and 81 percent were single-family homes, two-family homes, or small apartments. Roughly one third of the transacting properties had garages and more than three quarters were built before the Second World War. Only a handful of buildings were vandalized or otherwise abandoned. Finally, 43.7 percent of the transacting properties were located within 2,000 feet of a city-assisted housing site. 17.6 percent of the properties sold were within 2,000 feet of a *completed* city unit.

The second column of the table reveals some systematic differences between the transacting properties located close to city-assisted housing sites and those that are not. Properties located within the 2,000-foot ring are far more likely to be in Brooklyn and far less likely to be in Staten Island and Queens. Properties in the 2,000-foot ring are also much older, much less likely to be single-family homes, more likely to be walk-up apartments, and consistent with these differences, much less likely to have garages.

Table 2 shows the distribution of property sales by proximity to scale and type of city-assisted housing. Using the six mutually exclusive ring types described above, the

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sales using this method. For most of the HPD units, we had both tax block and tax lot. If the tax lost was unavailable, then we collapsed the Geosupport file to the tax block level (*i.e.* calculating the center of each block) in order to assign x.y coordinates. We were unable to assign an x,y coordinate to 6 percent of the HPD units, largely due to missing block information.

table shows that properties in the ring of city-assisted units are fairly evenly distributed among rings with newly built owner units (but no new rental units) and new rental units (but no new owner units). A somewhat larger share (43 percent) of sales was located in rings that contained both owner and rental units. In terms of size, just over half (54 percent) of the properties within 2,000 feet of a Ten Year Plan site are located in rings where fewer than 100 units will ultimately be built.

To test for heterogeneity in project impacts across different types of neighborhoods, we identified two submarkets (defined by community districts) based on household income information from the 1990 Decennial Census: a low-income submarket which consists of community districts with an average household income less than 80 percent of the MSA mean household income, and a high-income submarket including all the remaining districts. A comparison of key socioeconomic characteristics reveal that the low-income submarket is considerably more distressed than the high-income submarket. For example, the mean household income for the high-income submarket was \$60,893, compared with just \$29,490 for the low-income submarket; the poverty rate and minority presence were two to three times higher in the low-income districts; the unemployment rate was almost twice as high in the low-income submarket than it was in the high-income submarket; and fewer than 20 percent of the households in low-income areas own their homes, compared with almost 35 percent in high-income areas.[16]

[16] To create submarkets, we matched census tract-level data to community districts. More detailed submarket characteristics are available upon request from the authors.

IV. Results

The Baseline Model

Table 3 shows the key coefficients and their standard errors for our baseline model in equation (1). Coefficients for structural variables are shown in the appendix. The consistency of the coefficients on the structural variables with expectations and the relatively high R^2 (0.86) suggest that these variables provide adequate controls for the characteristics of the houses sold.

To begin, notice that all six of the In Ring coefficients are negative and statistically significant. Prior to completion, properties located within 2,000 feet of a city-assisted housing site sold for 6 to 15 percent less than comparable properties located elsewhere in the census tract, outside the 2,000 foot ring. This is consistent with the existence of disamenities at the sites upon which city-assisted housing was eventually built. (This is, perhaps, unsurprising since the city-assisted housing was typically built on abandoned properties that had been taken over for nonpayment of taxes.) With the exception of rental-only projects, this disamenity effect was typically larger for larger sites.

Turning to impact estimates, the positive and statistically significant coefficient on Post Ring indicates that, on average, the construction of city-assisted housing units generated significant external benefits. Moreover, building more units appears to bring a greater benefit, though this marginal effect declines as the number of units increases. Further, impacts decline (significantly) with the share of units in rental, multifamily buildings. Finally, the positive, significant coefficient on Tpost implies that impacts actually grow over time, perhaps as families move in and the population rises.

We also included a set of variables to control for proximity to other types of subsidized housing—occupied units that received renovation subsidies through the Ten Year Plan (rehab projects), and housing units sponsored by the federal government and pre-1987 city-sponsored projects, taken together. Estimated coefficients indicate that rehab projects were located in relatively distressed neighborhoods (although less so than the new housing investments). Impacts were small, but positive, which is unsurprising given the limited scope of these projects. Moreover, unlike new housing, rehabilitation of occupied units involves neither blight removal nor occupancy effects. Interestingly, federal and older city-sponsored units, taken together, were not located in worse-than-average neighborhoods and their impact was small, too. Again, the small impacts may reflect the importance of rehab projects, in contrast to new construction, in this category.[17]

Table 4 shows the key coefficients from the model that incorporates distance as in equation (2). Results are, again, consistent with expectations. Positive coefficients on the in ring distance interaction variables are consistently positive, suggesting a sharp price gradient such that the price-depressing effects of the site prior to completion (the initial disamenity) decline with distance. For sales adjacent to sites that will ultimately house more than 100 homeowner units, for example, we estimate that prices are initially 21 percent lower than in the surrounding neighborhood. At 2,000 feet away prices are only

[17] To investigate whether the aggregation of federal and old city-sponsored units masks differences in impacts between the two program types, we re-estimated the baseline model with separate sets of ring variables for each program type. The results are available from the authors. Perhaps most interesting among these is that the Post ring coefficient for federally sponsored units (0.0380) is almost identical to that for Ten Year Plan new housing (0.0397). This similarity suggests that significant positive spillover effects estimated for the Ten Year Plan housing investments may well be generalizable for a much larger array of programs. Finally, note that the Post ring coefficient for old city programs is only about one third as large, likely due to the low share of new construction, compared to nearly half of the federally sponsored units (*e.g.*, Section 8 project-based units, Section 202 new construction).

about 3 percent lower. At 500 feet per city block, the price differential falls by about 4.5 percentage points per city block.[18] Gradients are even steeper for sites that will house rental units.

Impacts change across the ring too, shrinking with distance from the new housing. And, the negative and significant coefficient on the interaction between Tpost and distance suggests that impacts do not increase with time further away from the project.

Figure 1 shows estimated impacts for the average project in our sample.[19] The thick line shows the percentage difference between prices at the given distance and prices in surrounding neighborhoods, prior to completion. As noted, this pre-completion gradient is fairly steep, climbing at a rate of 1.5 to 0.9 percentage points per 100 feet.

The thinner lines above show price gradients one, three, and five years after completion and suggest substantial impacts, especially for projects close by. Thus, while, prices are initially 28 percent lower in the immediate vicinity of the housing site; after completion this gap falls to 13 percent. Five years after completion the gap falls further to 11 percent. The implication is that although new housing does not completely eliminate the price gradient, price gradient is significantly flattened by the housing investment. This is consistent with the new housing partially, but not fully, eliminating an existing disamenity at the site. Note that at 2,000 feet, impacts are approximately zero.

Figures 2 and 3 show how impact varies with scale and project mix (proportion of units that are rental units in multifamily structures). In figure 2, we see that a 250-unit

[18] Note that this is an average change per block; blocks closer to the site will experience steeper declines than those farther away due to the non-linear gradient specification.
[19] The "average" project is defined as the project in the proximity of the average sale in a 2000 foot ring. Thus, the percentage price gap before its completion is a weighted average of the (average) price gaps for the 6 ring types, with weights given by the number of sales in each ring type; and its other relevant characteristics, *i.e.*, size (250 units) and tenure-structure mix (55.5 percent rental units in multifamily structures), are averages over all sales in a 2000 foot ring.

project has a substantially larger effect than a 50 unit project. This differential shrinks, however, with distance from the project site. Figure 3 indicates that projects that include a smaller share of rental units in multifamily structures average greater impacts.[20]

In summary, we find that city-assisted housing was built on sites that acted as local disamenities within their communities. After construction, city-assisted housing units have a positive impact on surrounding property values, which is sustained over time. Impacts are quite large, especially for properties close to the project sites. Impacts are larger for larger projects and for those with fewer units that are in multifamily rental buildings.

Repeat Sales

We estimate the baseline model and the model with distance using the repeat sales method. The repeat sales estimates are shown in Table 5, alongside the hedonic estimates from Table 4. The similarity of these estimates is striking. The only (somewhat) notable difference is that the coefficient on the Tpost-distance interaction term, which, while still negative, becomes insignificant in the repeat-sales regression. This implies that time patterns of impacts do not vary significantly with distance from project site. The similarity of the hedonic and repeat sales estimates seem to suggest that, at least in our analysis, omitted variable bias is not a problem in the hedonic regressions nor is selection bias a problem in the repeat sales estimation.[21]

[20] The regression specification does not allow the impact of multifamily rental share to vary with distance since we have little theoretical reason to justify such variation.
[21] Of course, another possibility would be that the hedonic and repeat-sales methods induce similar biases in the coefficients of interest, but this seems less likely than the “no bias” alternative.

Neighborhood Heterogeneity

Table 6 reports the results of models estimated to examine differences in impacts between lower and higher income community districts, or submarkets. The first column shows the coefficients for properties sold in high-income submarkets. The second column shows the coefficients of the variables interacted with a dummy variable indicating that the property is sold in a low-income submarket.

In general, the estimates suggest a somewhat smaller initial disamenity effect in low-income submarkets, which may not be surprising since this variable indicates prices *relative* to the rest of the neighborhood. Absolute prices are likely to be lower in rings located in low-income submarkets, but the differential between prices in the ring of a housing site and outside appears to be smaller. As for impacts, the story is mixed. The coefficient on Post Ring is smaller in lower income areas, suggesting smaller effects. But the marginal effects of additional units appear to be larger in lower income areas.

As an example, we estimate that the ring/neighborhood price differential for our average project falls by 13.7 percentage points in low-income submarkets and by 10.6 percentage points in high-income submarkets. One possible explanation is that the income of the households moving into the new housing is relatively high compared to the incomes of others living in the low-income submarket. Alternatively, the population was already at a reasonably high level, and thus the new residents generate little in the way of economic development.

The story is different for smaller projects. For a 50-unit project (that otherwise has the same characteristics), the predicted impact is actually larger in the high-income submarket (14.3 percentage points versus 6.7 percentage points). This is consistent with the idea that smaller housing investments will generate little in the way of spillovers if surrounded by high levels of blight.

Figure 4 shows the before/after picture for the average project, located in different submarkets. As shown, effects for a project of this size are predicted to be larger in lower income submarkets. The figure also shows that while positive impacts are predicted to grow over time in low-income submarket, they are predicted to fall somewhat in the higher income areas.

Eliminating Disamenities vs. Providing Amenities — The Timing of the Critical Event

As shown in Table 7 we also estimated a set of models that include variables capturing the timing of both the start and completion of construction. Both the baseline model and the model with distance are re-estimated with a post-ring variable defined by the start of construction in addition to the post-ring for completion, and companion variables interacting Post Ring with number of units. There are several findings in this table that are worth noting. First, in this specification, the coefficients on Post Ring (start) are statistically significant and very close to the ones based on completion dates and the coefficients on Post Ring (completion) are not statistically significant. These results suggest that the effects are felt as soon as construction starts, and that there is no additional fixed effect associated with project completion. This is consistent both with the immediate capitalization of future benefits and, in the absence of perfect foresight, with the elimination (at least to some extent) of a disamenity.[22] Second, the positive and

[22] As mentioned above, with perfect foresight, all project effects would be capitalized into prices as soon as construction starts. This would make it impossible to distinguish disamenity removal effects from amenity

significant coefficient on the number of started units indicates that, as expected, a larger project is likely to eliminate a larger source of disamenities and thus has a larger impact. Third, the completion of one more unit has a positive and significant effect over and beyond the marginal effect of its start (though this additional marginal effect declines as the number of completed units and distance to site increase); and the coefficient on the share of completed units in rental, multifamily buildings is negative and significant. These findings suggest that some of the external benefits of new housing are occupancy effects, *i.e.*, effects that occur through the number and characteristics of its inhabitants. They also imply that housing market agents do not exhibit perfect foresight, at least with respect to the future occupants of the new subsidized housing**.**

V. Conclusion

With the benefit of a unique dataset, this paper provides a detailed analysis of the external effects of subsidized housing on the value of surrounding properties. In particular, we use a difference-in-difference specification of a hedonic regression model to estimate the spillover effects of the new, publicly assisted housing units produced in New York City as part of the Ten Year Plan program.

This paper improves on previous research in several ways. First, we estimate distance gradients to allow impacts to vary with distance from subsidized housing sites. Second, we investigate how effects vary across different submarkets and identify the types of housing investment that will be particularly effective in neighborhoods with particular characteristics. Third, we attempt to shed light on the relative importance of

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provision effects. However, our subsequent findings on the marginal impact of completed units and the impact of completed units in rental, multifamily buildings cast doubt on the perfect foresight assumption.

two potential sources of spillovers — disamenity removal and amenity provision — by including both start dates and completion dates within a single regression. Last, but not least, we perform a sensitivity check of the results by re-estimating our model with the repeat sales methodology.

Our results suggest that the city's investment in new housing generated significant external benefits and that these benefits are sustained over time. The magnitudes of the external effects are found to increase with project size and to decrease with the proportion of units in multi-family, rental buildings. Consistent with expectations, we find that spillover effects diminish with distance from the housing investment sites. The submarket results suggest that spillovers are typically larger in the more distressed neighborhoods, and that smaller projects are likely to be less effective if surrounded by high levels of blight. We also find that the spillover benefits reflect, at least to some extent, the elimination of a disamenity. In addition, some of the external benefits of new housing seem to be occupancy effects, occurring through the number and characteristics of its inhabitants. Impact estimates based upon a repeat sales methodology are very similar to those obtained in the hedonic regressions, suggesting that our results are not sensitive to alternative specifications.

Regarding policy implications, this paper suggests that owners of properties in the relevant neighborhoods will benefit from the increase of values apparently associated with the city-subsidized new housing. In addition, the reassessment of the properties in these neighborhoods may generate additional tax revenues for the City. On the other hand, higher property values are likely to result in higher rents and, despite rent regulation, some tenants may experience difficulties as a result. Perhaps more important from a policy perspective, however, is the idea that a more effective deployment of housing investments can be achieved by directing larger projects with larger shares of owner-occupied units towards more distressed communities.

Table 1: Characteristics of Properties Sold

	Percentage of all property sales	Percentage of sales within 2,000 feet of new, city-assisted housing
Borough		
Manhattan	14.6	20.7
Bronx	13.1	13.0
Brooklyn	29.6	40.1
Queens	31.0	21.3
Staten Island	11.8	4.8
Any borough	100.0	100.0
Building Class		
Single-family detached	25.0	15.6
Single-family attached	11.1	7.3
Two-family	27.6	29.2
Walk-up apartments	17.5	26.1
Elevator apartments	1.2	1.8
Loft buildings	0.1	0.1
Condominiums	14.4	15.5
Mixed-use, multifamily (includes store or office plus residential units)	3.0	4.3
Any building type	100.0	100.0
Other Structural Characteristics		
Built pre-World War II	77.0	89.2
Vandalized	0.0	0.1
Other abandoned	0.1	0.2
Garage	31.1	20.2
Corner location	7.1	7.0
Major alteration prior to sale	3.3	5.0
N	293,786	128,445

Note: Universe = all sales in community districts where at least 100 city-assisted units were built or rehabilitated.

Table 2: Distribution of Properties Sold within 2000 Feet of Any New City-Assisted Housing, by Ring Type

	N	% of sales
Ring contains:		
Homeownership units only	**37,264**	**29.0**
100 units or less	33,588	26.1
101 units or more	3,676	2.9
Rental units only	**35,805**	**27.9**
100 units or less	22,381	17.4
101 units or more	13,424	10.5
Homeownership and rental units	**55,376**	**43.1**
100 units or less	13,596	10.6
101 units or more	41,780	32.5
Total	**128,445**	**100.0**

Table 3. Baseline Model

Ring variables for Ten Year Plan new housing projects		
In Ring variables		
Homeownership units only		
100 units or less	-0.0879	***
	(0.0059)	
101 units or more	-0.1123	***
	(0.0104)	
Rental units only		
100 units or less	-0.1085	***
	(0.0061)	
101 units or more	-0.0591	***
	(0.0085)	
Homeownership and rental units		
100 units or less	-0.1128	***
	(0.0076)	
101 units or more	-0.1543	***
	(0.0076)	
Post Ring variables		
Post Ring	0.0415	***
	(0.0047)	
Number of units at the time of sale	2.4E-04	***
	(1.7E-05)	
(Number of units at the time of sale)^2	-9.1E-08	***
	(8.2E-09)	
Share of renter-multi-family units at the time of sale	-0.0387	***
	(0.0052)	
Tpost	2.5E-03	***
	(0.0007)	
Ring variables for Ten Year Plan housing rehabilitation projects		
In Ring	-0.0380	***
	(0.0044)	
Post Ring	0.0083	***
	(0.0030)	
Number of units at the time of sale	1.3E-05	***
	(2.9E-06)	
Ring variables for federal and pre-1987 city-sponsored projects		
In Ring	-0.0047	
	(0.0042)	
Post Ring	0.0167	***
	(0.0034)	
Number of units at the time of sale	9.1E-06	
	(5.7E-06)	
N	293,786	
R^2	0.8569	

Note: The regression includes census tract and CD-quarter dummies and the full set of building controls, as in the appendix. Standard errors in parentheses. *** denotes 1% significance level; ** denotes 5% significance level; * denotes 10% significance level.

Table 4. Model with Distance

In Ring variables	
Homeownership units only	
100 units or less	-0.1745 ***
	(0.0098)
100 units or less * D	7.9E-05 ***
	(1.1E-05)
101 units or more	-0.2051 ***
	(0.0183)
101 units or more * D	8.5E-05 ***
	(1.7E-05)
Rental units only	
100 units or less	-0.3227 ***
	(0.0131)
100 units or less * D	1.6E-04 ***
	(1.3E-05)
101 units or more	-0.2506 ***
	(0.0152)
101 units or more * D	1.7E-04 ***
	(1.4E-05)
Homeownership and rental units	
100 units or less	-0.2530 ***
	(0.0116)
100 units or less * D	1.3E-04 ***
	(1.2E-05)
101 units or more	-0.3582 ***
	(0.0101)
101 units or more * D	2.2E-04 ***
	(1.0E-05)
Any Units * D^2	-1.6E-08 ***
	(4.8E-09)
Post Ring variables	
Post Ring	0.0870 ***
	(0.0087)
Post Ring * D	-2.8E-05 ***
	(5.9E-06)
Number of units at the time of sale	3.1E-04 ***
	(2.3E-05)
Number of units at the time of sale * D	-1.8E-07 ***
	(2.2E-08)
$(\text{Number of units at the time of sale})^2$	-1.1E-07 ***
	(9.1E-09)
Share of renter-multi-family units at the time of sale	-0.0355 ***
	(0.0052)
Tpost	0.0049 ***
	(0.0014)
Tpost * D	-2.9E-06 ***
	(1.1E-06)
N	293,786
R^2	0.8575

Note: This table shows only the ring variables for the Ten Year Plan new housing projects. The regression includes ring variables for other types of subsidized housing, census tract and CD-quarter dummies and the full set of building controls, as in the appendix. Standard errors in parentheses. *** denotes 1% significance level; ** denotes 5% significance level; * denotes 10% significance level.

Table 5. Repeat Sales Estimation

	Model with Distance	
	Repeat Sales Estimates	Hedonic Estimates
Post Ring variables		
Post Ring	0.0788 ***	0.0870 ***
	(0.0120)	(0.0087)
Post Ring * D	-2.7E-05 ***	-2.8E-05 ***
	(8.2E-06)	(5.9E-06)
Number of units at the time of sale	4.5E-04 ***	3.1E-04 ***
	(3.9E-05)	(2.3E-05)
Number of units at the time of sale * D	-2.2E-07 ***	-1.8E-07 ***
	(3.6E-08)	(2.2E-08)
(Number of units at the time of sale)^2	-1.4E-07 ***	-1.1E-07 ***
	(1.6E-08)	(9.1E-09)
Share of renter-multi-family units at the time of sale	-0.0350 ***	-0.0355 ***
	(0.0074)	(0.0052)
Tpost	0.0040 **	0.0049 ***
	(0.0019)	(0.0014)
Tpost * D	-3.7E-07	-2.9E-06 ***
	(1.5E-06)	(1.1E-06)
N	65,367	65,367
R^2	0.7403	0.7399

Note: This table shows only the ring variables for the Ten Year Plan new housing projects. The repeat sales regression includes Post ring variables for other types of subsidized housing and differenced CD-quarter dummies. Standard errors in parentheses. *** denotes 1% significance level; ** denotes 5% significance level; * denotes 10% significance level.

Table 6. Model with Distance and Neighborhood Heterogeneity

	High Income Submarket	Low Income - High Income Differential
In Ring variables		
Homeownership units only		
100 units or less	-0.3258 ***	0.2314 ***
	(0.0203)	(0.0256)
100 units or less * D	1.7E-04 ***	-9.6E-05 ***
	(2.4E-05)	(2.7E-05)
101 units or more	-0.1934 ***	0.0623
	(0.0335)	(0.0414)
101 units or more * D	8.6E-05 ***	-7.9E-06
	(3.1E-05)	(3.7E-05)
Rental units only		
100 units or less	-0.4285 ***	0.2137 ***
	(0.0215)	(0.0292)
100 units or less * D	2.4E-04 ***	-9.3E-05 ***
	(2.5E-05)	(2.9E-05)
101 units or more	-0.3009 ***	0.1877 ***
	(0.0216)	(0.0327)
101 units or more * D	2.2E-04 ***	-1.5E-04 ***
	(2.3E-05)	(3.2E-05)
Homeownership and rental units		
100 units or less	-0.2871 ***	0.1013 ***
	(0.0356)	(0.0393)
100 units or less * D	0.0002 ***	-3.6E-05
	(3.2E-05)	(3.5E-05)
101 units or more	-0.3882 ***	0.0935 ***
	(0.0185)	(0.0246)
101 units or more * D	2.8E-04 ***	-8.4E-05 ***
	(2.2E-05)	(2.4E-05)
Any Units * D^2	-2.7E-08 ***	5.8E-09
	(9.2E-09)	(1.1E-08)
Post Ring variables		
Post Ring	0.1942 ***	-0.1410 ***
	(0.0171)	(0.0198)
Post Ring * D	-8.0E-05 ***	7.1E-05 ***
	(1.1E-05)	(1.3E-05)
Number of units at the time of sale	-2.1E-04 ***	6.0E-04 ***
	(6.0E-05)	(6.5E-05)
Number of units at the time of sale * D	-4.3E-08	-4.2E-08
	(4.9E-08)	(5.5E-08)
$(\text{Number of units at the time of sale})^2$	6.7E-08 ***	-2.0E-07 ***
	(2.2E-08)	(2.4E-08)
Share of renter-multi-family units at the time of sale	-0.0608 ***	0.0287 **
	(0.0098)	(0.0117)
Tpost	-0.0071 ***	0.0194 ***
	(0.0026)	(0.0031)
Tpost * D	3.4E-06 *	-1.0E-05 ***
	(1.9E-06)	(2.3E-06)
N	293,786	
R^2	0.8598	

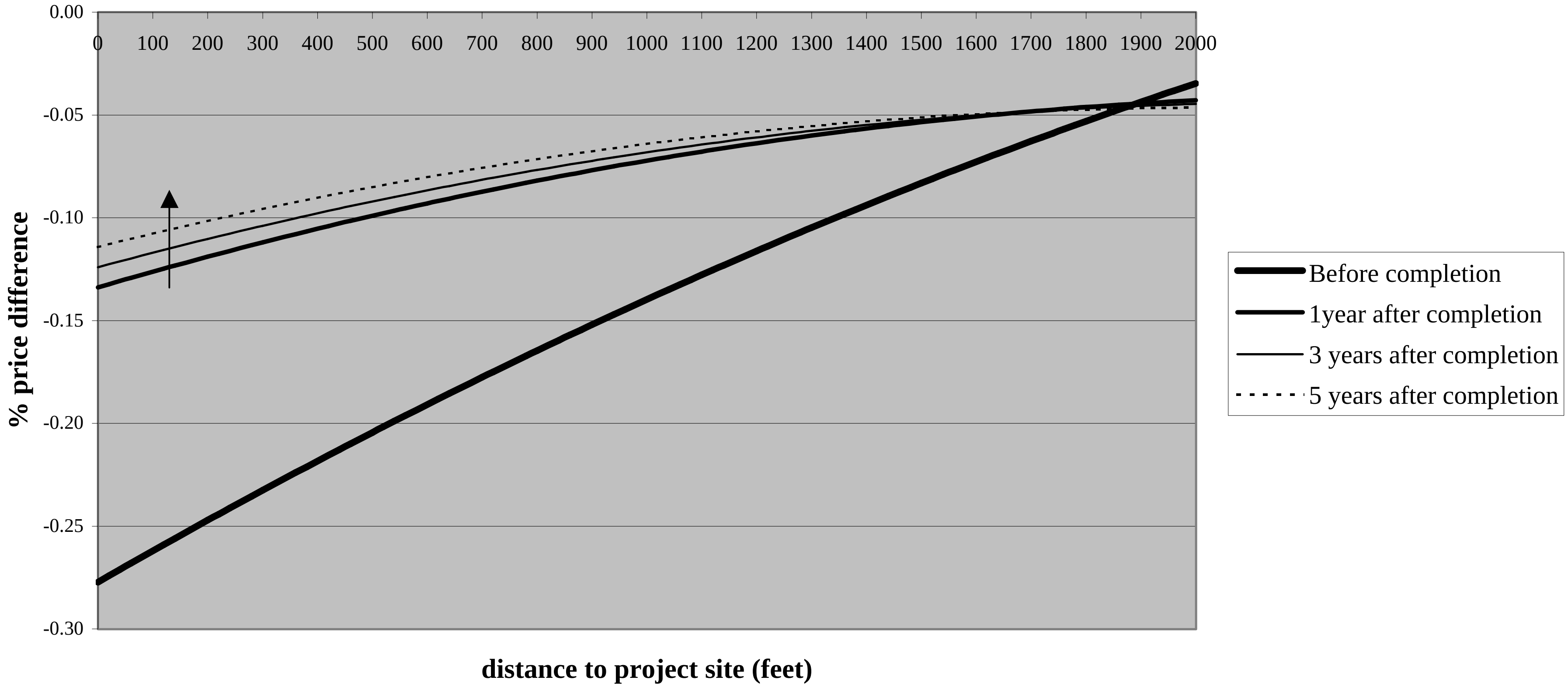
Note: This table shows only the ring variables for the Ten Year Plan new housing projects. Coefficients in column 2 correpond to a set of interactions between the ring variables and a dummy which is equal to 1 for the low income submarket and 0 otherwise. The low income submarket comprises community districts for which the CD/MSA mean household income ratio is smaller than 0.8 (and the high income submarket includes all the other community districts). The regression includes ring variables for other types of subsidized housing, census tract and CD-quarter dummies, and the full set of building controls and their interactions with the low income submarket dummy. Standard errors in parentheses. *** denotes 1% significance level; ** denotes 5% significance level; * denotes 10% significance level.

Table 7. Starting Date vs. Completion Date as the Critical Event

	Baseline Model	Model with Distance
In Ring variables		
Homeownership units only		
100 units or less	-0.0907 ***	-0.1691 ***
	(0.0061)	(0.0096)
100 units or less * D		0.000085 ***
		(1.2E-05)
101 units or more	-0.1216 ***	-0.2166 ***
	(0.0105)	(0.0184)
101 units or more * D		0.000101 ***
		(1.7E-05)
Rental units only		
100 units or less	-0.1101 ***	-0.3295 ***
	(0.0060)	(0.0127)
100 units or less * D		0.000181 ***
		(1.3E-05)
101 units or more	-0.0704 ***	-0.2819 ***
	(0.0083)	(0.0150)
101 units or more * D		0.000196 ***
		(1.4E-05)
Homeownership and rental units		
100 units or less	-0.1098 ***	-0.2573 ***
	(0.0076)	(0.0118)
100 units or less * D		0.0001 ***
		(1.3E-05)
101 units or more	-0.1603 ***	-0.3678 ***
	(0.0076)	(0.0101)
101 units or more * D		0.0002 ***
		(1.0E-05)
Any Units * D^2		-2.2E-08 ***
		(5.0E-09)
Post start date ring variables		
Post Ring (start)	0.0309 ***	0.1114 ***
	(0.0053)	(0.0098)
Post Ring (start) * D		-5.9E-05 ***
		(6.6E-06)
Number of started units at the time of sale	1.3E-04 ***	1.0E-04 **
	(3.1E-05)	(4.2E-05)
Number of started units at the time of sale * D		-4.4E-08
		(3.5E-08)
$(\text{Number of started units at the time of sale})^2$	-9.3E-09	-2.5E-09
	(1.9E-08)	(2.1E-08)
Share of started renter-multi-family units at the time of sale	-0.0097	-0.0108
	(0.0068)	(0.0069)
Tpost (start)	6.7E-03 ***	6.3E-03 **
	(0.0019)	(0.0025)
Tpost (start) * D		8.1E-07
		(1.7E-06)
Post completion date ring variables		
Post Ring (completion)	0.0066	-0.0117
	(0.0062)	(0.0112)
Post Ring (completion) * D		1.5E-05 **
		(7.3E-06)
Number of completed units at the time of sale	1.2E-04 ***	2.1E-04 ***
	(3.3E-05)	(4.6E-05)
Number of completed units at the time of sale * D		-1.4E-07 ***
		(3.9E-08)
$(\text{Number of completed units at the time of sale})^2$	-7.3E-08 ***	-9.7E-08 ***
	(2.0E-08)	(2.2E-08)
Share of completed renter-multi-family units at the time of sale	-0.0279 ***	-0.0231 ***
	(0.0073)	(0.0073)
Tpost (completion)	-3.8E-03 *	-1.3E-03
	(0.0021)	(0.0029)
Tpost (completion) * D		-3.5E-06 *
		(2.0E-06)
N	293,786	293,786
R^2	0.8569	0.8575

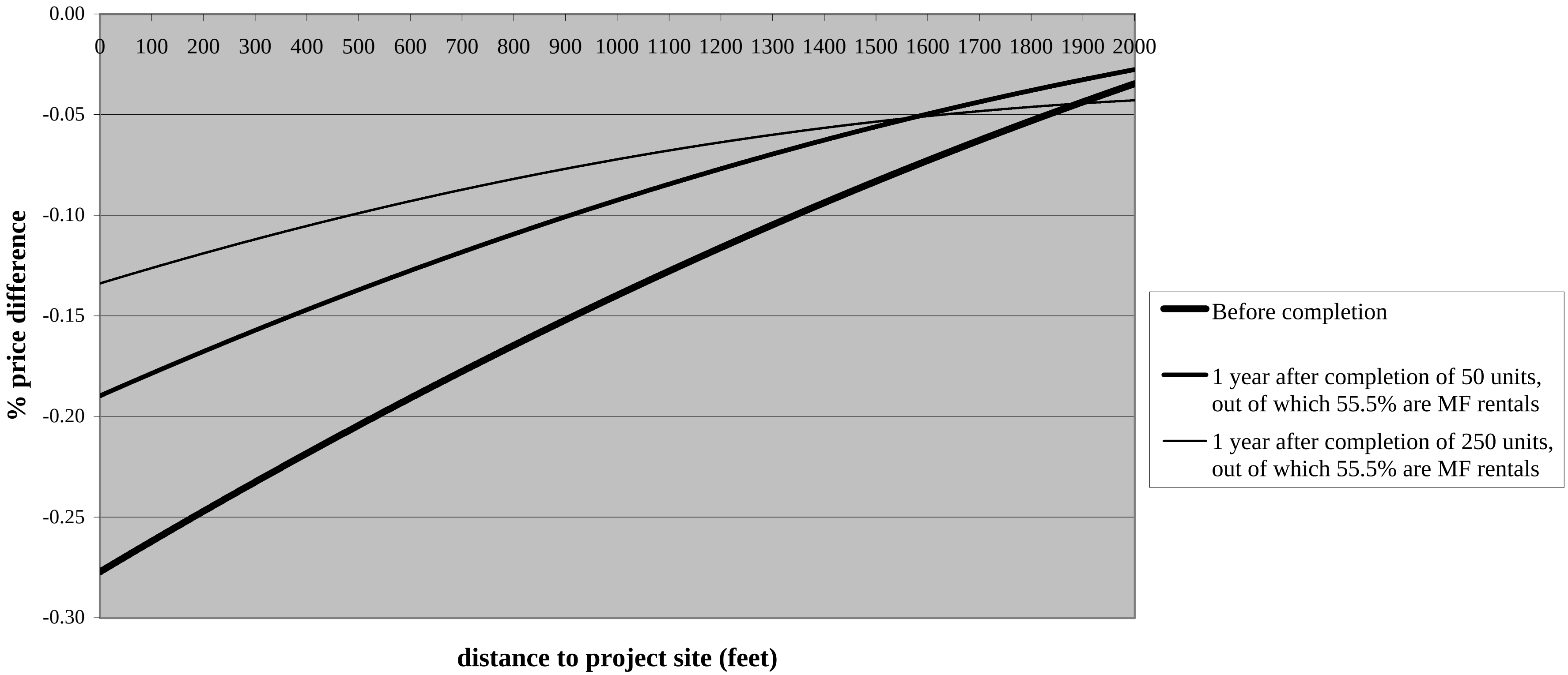
Note: This table shows only the ring variables for the Ten Year Plan new housing projects. Both regressions include ring variables for other types of subsidized housing, census tract and CD-quarter dummies and the full set of building controls, as in the appendix. Standard errors in parentheses. *** denotes 1% significance level; ** denotes 5% significance level; * denotes 10% significance level.

Figure 1
Percent Differerence between Prices in 2,000-Foot Ring and Surrounding Neighborhood, Before and After Completion of "Average" Project, by Distance to Project Site and Time Since Completion



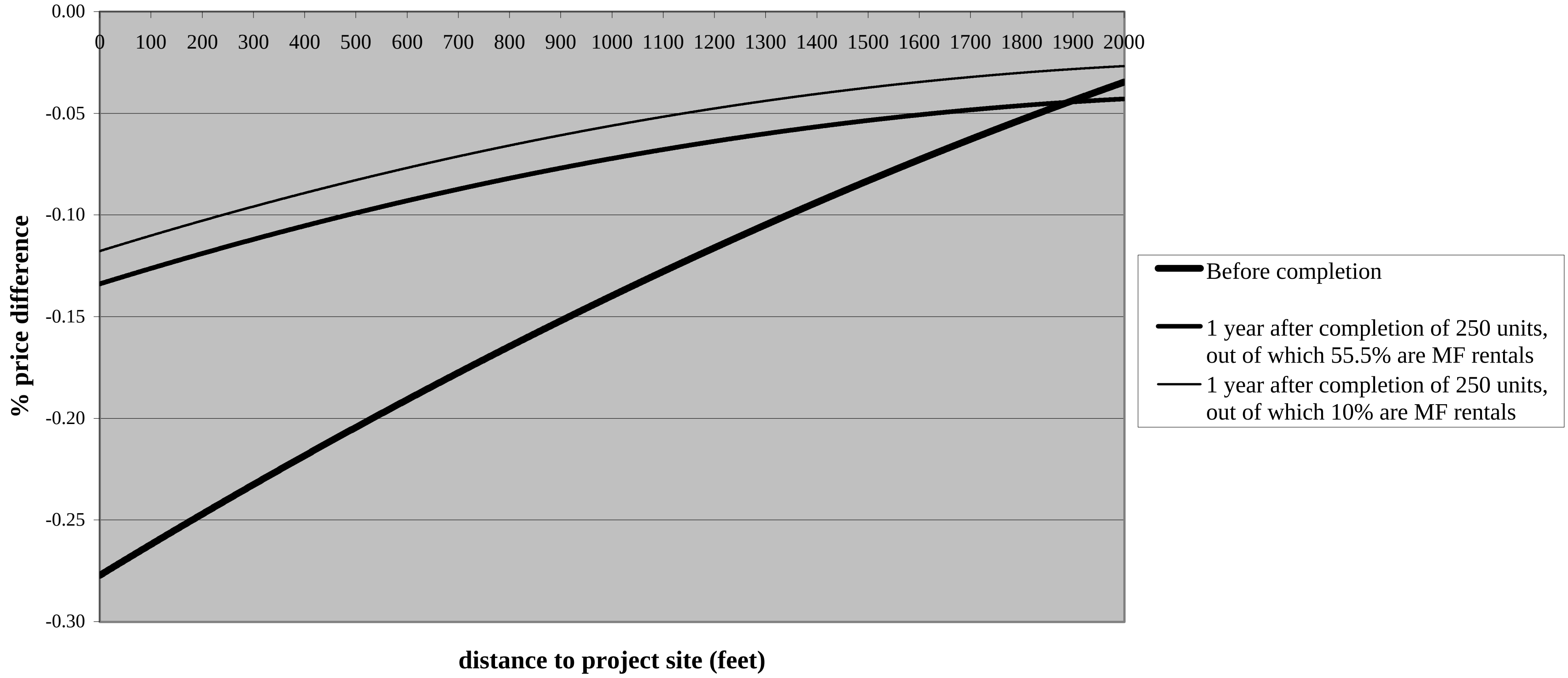
Note :The "average" project is defined as the project in the vecinity of the average sale in a 2000 foot ring. Thus, the percentage price gap before its completion is a weighted average of the (average) price gaps for the 6 ring types, with weights given by the number of sales in each ring type; and its other relevant characteristics, i.e., size (250 units) and tenure-structure mix (55.5 percent renter-multi-family units), are averages over all sales in a 2000 foot ring.

Figure 2
Percent Differerence between Prices in 2,000-Foot Ring and Surrounding Neighborhood, Before and After Completion of "Average" Project, by Distance to Project Site and Number of Units Completed



Note :The "average" project is defined as the project in the vecinity of the average sale in a 2000 foot ring. Thus, the percentage price gap before its completion is a weighted average of the (average) price gaps for the 6 ring types, with weights given by the number of sales in each ring type; and its other relevant characteristics, i.e., size (250 units) and tenure-structure mix (55.5 percent renter-multi-family units), are averages over all sales in a 2000 foot ring.

Figure 3
Percent Differerence between Prices in 2,000-Foot Ring and Surrounding Neighborhood, Before and After Completion of "Average" Project, by Distance to Project Site and Tenure-Structure Mix

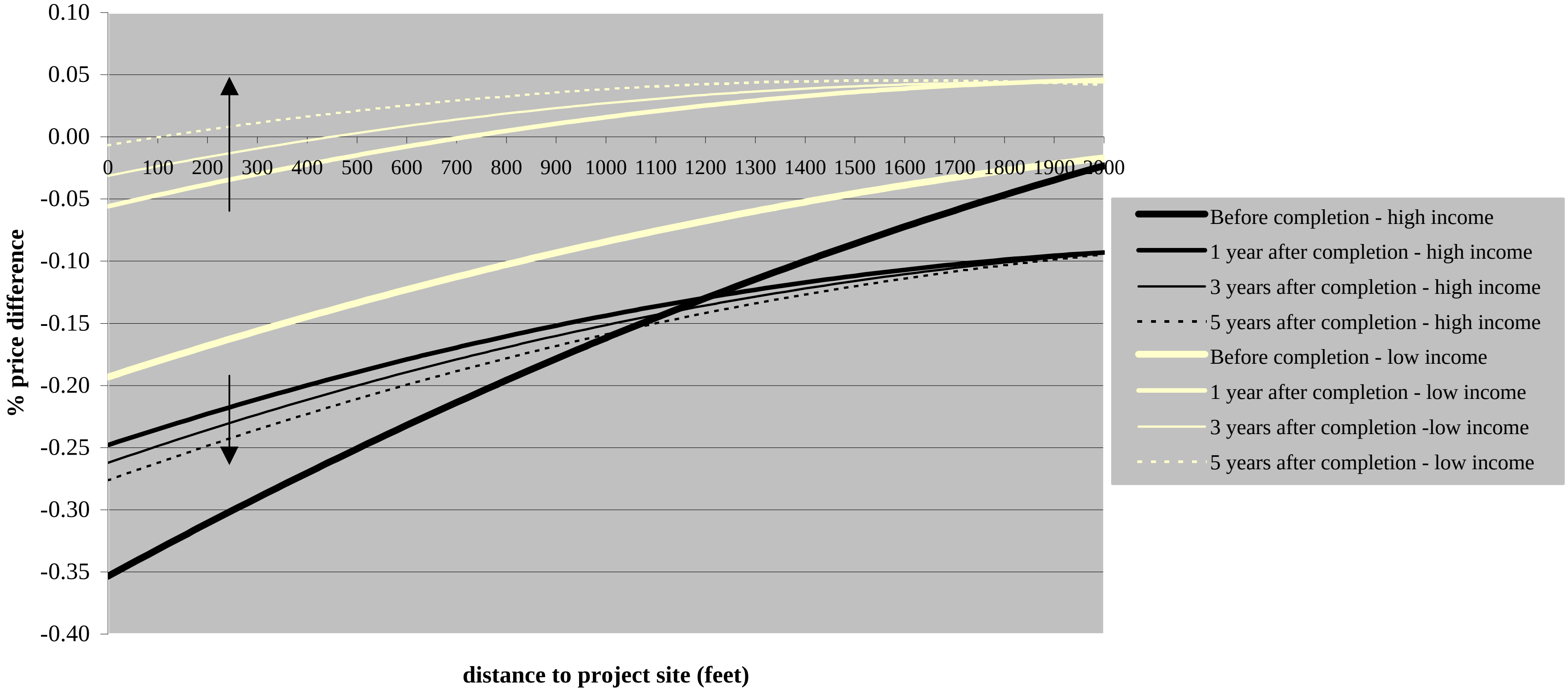


Note :The "average" project is defined as the project in the vecinity of the average sale in a 2000 foot ring. Thus, the percentage price gap before its completion is a weighted average of the (average) price gaps for the 6 ring types, with weights given by the number of sales in each ring type; and its other relevant characteristics, i.e., size (250 units) and tenure-structure mix (55.5 percent renter-multi-family units), are averages over all sales in a 2000 foot ring.

Figure 4

Percent Differerence between Prices in 2,000-Foot Ring and Surrounding Neighborhood, Before and After Completion of "Average" Project, by Distance to Project Site and Time Since Completion

- Results by Submarket -



Note:The "average" project is defined as the project in the vecinity of the average sale in a 2000 foot ring. Thus, the percentage price gap before its completion is a weighted average of the (average) price gaps for the 6 ring types, with weights given by the number of sales in each ring type; and its other relevant characteristics, i.e., size (250 units) and tenure-structure mix (55.5 percent renter-multi-family units), are averages over all sales in a 2000 foot ring.

Table A1. Complete Regression Results for Baseline Model

Ring variables for Ten Year Plan new housing programs	
In Ring variables	
Homeownership units only	
100 units or less	-0.0879 ***
	(0.0059)
101 units or more	-0.1123 ***
	(0.0104)
Rental units only	
100 units or less	-0.1085 ***
	(0.0061)
101 units or more	-0.0591 ***
	(0.0085)
Homeownership and rental units	
100 units or less	-0.1128 ***
	(0.0076)
101 units or more	-0.1543 ***
	(0.0076)
Post Ring variables	
Post Ring	0.0415 ***
	(0.0047)
Number of units at the time of sale	2.4E-04 ***
	(1.7E-05)
(Number of units at the time of sale)^2	-9.1E-08 ***
	(8.2E-09)
Share of renter-multi-family units at the time of sale	-0.0387 ***
	(0.0052)
Tpost	2.5E-03 ***
	(0.0007)
Ring variables for Ten Year Plan housing rehabilitation programs	
In Ring	-0.0380 ***
	(0.0044)
Post Ring	0.0083 ***
	(0.0030)
Number of units at the time of sale	1.3E-05 ***
	(2.9E-06)
Ring variables for federal and pre-1987 city-sponsored programs	
In Ring	-0.0047
	(0.0042)
Post Ring	0.0167 ***
	(0.0034)
Number of units at the time of sale	9.1E-06
	(5.7E-06)
Characteristics of properties sold	
Vandalized	-0.1343 ***
	(0.0318)
Other abandoned	-0.0860 ***
	(0.0173)
Odd shape	0.0147 ***
	(0.0024)

Table A1. Complete Regression Results for Baseline Model (continued)

Garage	0.0444	***
	(0.0018)	
Extension	0.0479	***
	(0.0024)	
Corner	0.0282	***
	(0.0026)	
Major alteration prior to sale	-0.0677	***
	(0.0042)	
Age of unit	-0.0056	***
	(0.0001)	
(Age of unit)2	3.1E-05	***
	(1.2E-06)	
Age of unit missing	-0.0587	***
	(0.0194)	
Log square feet per unit	0.5367	***
	(0.0021)	
Number of buildings on same lot	-0.0139	***
	(0.0050)	
Includes commercial space	0.0361	***
	(0.0051)	
Square feet missing	3.8118	***
	(0.0229)	
Condo and square feet missing	-0.1952	***
	(0.0180)	
Single-family detached	0.0962	***
	(0.0026)	
Two-family home	-0.2660	***
	(0.0025)	
Three-family home	-0.4869	***
	(0.0033)	
Four-family home	-0.6401	***
	(0.0051)	
Five/six-family home	-0.9675	***
	(0.0053)	
More than six families, no elevator	-1.3953	***
	(0.0052)	
Walkup, units not specified	-1.1052	***
	(0.0061)	
Elevator apartment building, cooperatives	-1.3115	***
	(0.0158)	
Elevator apartment building, not cooperatives	-1.4012	***
	(0.0078)	
Loft building	-0.6470	***
	(0.0232)	
Condominium, single-family attached	0.0192	
	(0.0227)	
Condominium, walk-up apartments	-0.2005	***
	(0.0206)	
Condominium, elevator building	-0.4132	***
	(0.0204)	
Condominium, miscellaneous	-0.3677	***
	(0.0213)	

Table A1. Complete Regression Results for Baseline Model (continued)

Multi-use, single family with store	-0.0803 ***
	(0.0098)
Multi-use, two-family with store	-0.4855 ***
	(0.0079)
Multi-use, three-family with store	-0.7195 ***
	(0.0120)
Multi-use, four or more family with store	-0.8819 ***
	(0.0087)
N	293,786
R^2	0.8569

Note: The regression includes census tract and CD-quarter dummies. Standard errors in parentheses. *** denotes 1% significance level; ** denotes 5% significance level; * denotes 10% significance level.

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