FURMAN CENTER FOR REAL ESTATE & URBAN POLICY

NEW YORK UNIVERSITY SCHOOL OF LAW • WAGNER SCHOOL OF PUBLIC SERVICE

The External Effects of Place-Based Subsidized Housing

WORKING PAPER 05–02

Amy Ellen Schwartz · Ingrid Gould Ellen

Michael H. Schill · Ioan Voicu

The External Effects of Place-Based Subsidized Housing

Amy Ellen Schwartz*
Wagner School
New York University

Ingrid Gould Ellen
Wagner School
New York University

Ioan Voicu
Furman Center for Real Estate and Urban Policy
New York University

Michael H. Schill
School of Law
University of California, Los Angeles

Abstract

Prior research has provided little evidence that subsidized housing investments generate significant external benefits to their neighborhoods. This paper revisits the external effects of subsidized housing, exploring the case of New York City. Relying on geocoded administrative data, we estimate a difference-in-difference specification of a hedonic regression model.

We find significant and sustained external benefits. Spillovers increase with project size, and decrease with distance from the project sites and with the proportion of units in multi-family, rental buildings. Our results are robust to alternative specifications. Some of the benefit appears due to the effect of the replacement of existing disamenity.

(JEL H23, H43, R0)

Keywords: Development/Revitalization; Externalities; Housing; Neighborhood

* Corresponding author. Robert F. Wagner Graduate School of Public Service, New York University, 295 Lafayette Street, Room 3096, New York, NY 10012. Tel.: (212) 998 – 7461; Fax: (212) 995 - 3890.
Email address: amy.schwartz@nyu.edu

A strong consensus in the economics literature holds that place-based housing investment is an inefficient way to address failures in the housing market and, more specifically, to provide housing for low-income families. As Olsen (2003) writes, "The most important finding of the empirical literature … is that tenant-based vouchers and certificates provide equally good housing at much lower cost than any type of project-based assistance that has been studied." Yet the analyses of costs and benefits of housing programs have been based solely on the benefits delivered to the individual households who actually live in the subsidized housing. If externalities exist and are economically important, then place-based housing investment by governments may well be warranted – indeed critical - for efficient housing markets.

This paper examines such external effects. While prior research has left economists skeptical, estimating the external effects of subsidized housing investment – and disentangling the direction of causality in complex urban housing markets – is difficult, requiring plentiful geographically detailed data, a large range of housing investments, and a plausibly exogenous set of site choices. Earlier research has not had access to these data or circumstances. In this paper, we make use of the opportunity afforded by New York City's unparalleled investment in subsidized housing between 1987 and 2000, which resulted in the creation of 66,000 new subsidized housing units. We exploit a geo-coded administrative data set that includes detailed information on 294,000 residential property sales in New York City between 1980 and 1999. The results challenge what has become the conventional wisdom regarding place-based housing investment. We find significant, positive spillover effects of subsidized housing investment, which are robust to alternative specifications. We believe these results provide a persuasive justification for place-based housing investment.

To be more specific, we estimate a hedonic regression model with a difference-in-difference specification. Intuitively, impacts are estimated as the difference between property values in the vicinity of housing investment before and after the completion of a new unit relative to price changes of comparable properties farther away, but still in the same neighborhood. Our regressions include census tract fixed effects to control for idiosyncratic neighborhood characteristics and also control for pre-existing price trends in the vicinity of the new housing investment.

We find that while prices of properties surrounding city sites are lower than comparable properties in the same neighborhood prior to construction, the gap narrows considerably after the new housing is built. That is, price appreciation in the immediate vicinity of newly built subsidized housing exceeds appreciation in areas just beyond, and the magnitude of the external effects decreases with distance from the housing investment sites. The evidence suggests that the relationship is causal. While there are plausible alternative explanations, the evidence simply does not support them. As an example, although city officials may have wanted to pick winning sites where prices were going to appreciate, they had little latitude in their selection. By the end of our study period, virtually all available sites in the city had been developed. Moreover, the results are robust to specifications that include controls for prior trends in the value of properties surrounding assisted housing. Further estimates based upon a repeat sales model are nearly identical to those obtained in the hedonic analyses, suggesting that results are not driven by an increase in unmeasured quality among transacting properties.

As for the magnitude of the external benefits, we find that they are substantial. A simple cost-benefit analysis suggests that New York City's housing investments delivered a tax benefit to the city that exceeded the cost of the city subsidies provided.

Thus, these estimates suggest that place-based housing investment may well be warranted to correct for market failures in urban housing markets. As described in greater detail below, New York City explicitly targeted housing subsidies to properties that were, in effect, local disamenities, generating negative externalities in their vicinity. Thus, one interpretation of the positive external effects of the housing investment is that it reflects the removal of existing negative externalities and not the impact of the housing *per se*. Although fully disentangling the disamenity removal effect from any amenity offered by the housing itself is beyond the scope of this paper, our exploration of the timing of the impacts suggests that both effects are important.

The paper is organized as follows. Section I provides some theoretical background and a review of relevant literature and section II describes the model and empirical strategy. Section III gives an overview of the New York City's municipally supported housing programs. Section IV provides a description of the data. Section V presents results. The paper ends with a summary of the key findings and implications for public policy.

I. Background

How housing investment might generate external effects

Subsidized housing built in urban neighborhoods often replaces a disamenity, such as an abandoned boarded-up building or a littered vacant lot. As in the case of environmental disamenities, a dilapidated building or other eyesore can reduce the value of neighboring homes because it is visually unappealing or because it invites unwelcome activities like vandalism and crime. Thus, housing investment can increase property values simply because of what it removes. Further, while the impact of the disamenity may be largest in adjacent properties, it may well extend some distance, with diminishing impact. Research on environmental disamenities typically finds a negative impact of adverse land uses on nearby property values that declines with distance from the disamenity.[1] And, several studies have found that the effects of actual or proposed adverse land uses diminish or disappear after the clean up of sites (Kohlhase 1991) or cancellation of proposed facilities (Smollen et al. 1991). These studies find that the house price gradient vanishes when the disamenity that caused it is removed.

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).[2] Alternatively, public housing investment may yield spillover effects because it captures the benefits of collective action in large-scale investments. While small investments in a blighted neighborhood may not have been profitable, public subsidies may serve to provide just the marginal investment needed to put the neighborhood over the threshold and make private investment worthwhile.

Finally, new housing may yield external benefits as a result 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 that the impact of this added population may depend upon the characteristics of the inhabitants – their incomes, for example, or their housing tenure. In particular, homeowners may contribute to neighborhood stability by remaining in their homes for longer. Plus, they may have stronger economic incentives to maintain their homes properly and to become active in neighborhood

---

[1] 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.

organizations and political affairs (Ellen et al 2002).[3] Higher income residents may also be viewed by some as more desirable neighbors.

Note that the impact of a particular housing investment may vary across neighborhoods with different characteristics. As an example, a middle-class neighborhood dominated by homeowners may have a less positive reaction to a new, subsidized rental housing development than a lower income area.

The Timing of Impacts

While fully disentangling these different mechanisms is quite difficult, some inferences may be drawn by observing differences in the timing of impacts and the variation in impacts across projects of differing types. 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).

Figure 1 shows a possible time-line of effects of one hypothetical, city-assisted housing development built at a blighted location. First, an increase in local property values may occur at the time that the project is announced. Second, a further jump in values may occur when the construction actually starts on the project. At this point, the initial source of blight may be removed or sealed-off and the uncertainty about whether the announced project would actually be built is resolved. Third, property values could increase upon project completion when

---

[2] It is also possible that such government attention would prove stigmatizing.
[3] 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 community than residents of larger buildings (Glaeser and Sacerdote 2000).

neighbors see the finished project and new occupants begin to move in. Finally, property values may continue to increase in the years after completion, as population increase spurs further neighborhood changes.

Notice that these mechanisms all suggest to some degree that larger projects should have larger impacts - if, as seems intuitive, a larger number of units typically replaces a larger source of blight. 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.

Prior Evidence on the Externalities of Housing Investment

Until recently, empirical research has failed to provide persuasive evidence that investments in affordable housing can generate positive spillover effects. While Nourse (1963) and Rabiega, Lin, and Robinson (1984) find that newly developed public housing can have modest, positive impacts on neighboring property values, more recent studies, including Lyons and Loveridge (1993), Goetz, Lam, and Heitlinger (1996), and Lee, Culhane, and Wachter (1999), find small negative effects, associated with certain types of federally-subsidized housing. Further, even if these studies told a more consistent story, they share data limitations that make it difficult to pinpoint the direction of causality. Few studies, for example, have access to information on the date of construction for the projects examined. Thus, even if these studies find statistically significant relationships, they cannot establish whether subsidized sites are systematically located in weak/strong neighborhoods, or whether subsidized housing actually leads to neighborhood decline/improvement (Goetz, Lam ,and Heitlinger 1996; Lee, Culhane, and Wachter 1999; Lyons and Loveridge 1993).

Of the studies that have access to completion dates, some lack comparison areas, making it impossible to distinguish effects of subsidized housing from larger market conditions (see Rabiega, Lin, and Robinson 1984). Others use a test/control area methodology which compares price trends in neighborhoods with subsidized housing to those in neighborhoods without such housing. Unfortunately, however, it is difficult to know whether the two groups of neighborhoods are truly comparable. Nourse (1963), for example, chooses control neighborhoods on the basis of average rents, land use, population, and personal knowledge, which leaves open the possibility of many relevant, unmeasured differences.

Cummings, DiPasquale, and Kahn (2000) use a design that combines the test/control and pre/post methods, and enhance the comparability of their test and control neighborhoods by means of regression analysis. They examine the impact of two place-based homeownership developments in Philadelphia, by comparing the price increase in the two census tracts with homeownership developments to (1) price increases in similarly distressed tracts elsewhere in the city and (2) price increases in neighboring census tracts, while including a series of community attributes as additional controls. However, census tract characteristics may be poor proxies for conditions in the micro-neighborhood around the subsidized site, especially in a city like Philadelphia, where census tracts are relatively large. Additionally, given the census tract size, it seems very unlikely that the effects of subsidized housing would extend from one end of a census tract to another.

A few more recent analyses employ more geographically detailed data that allow researchers to compare price changes of properties within a smaller area of newly developed housing to price changes citywide, while controlling for neighborhood (census tract) fixed effects. Briggs, Darden, and Aidala (1999), for instance, use such a design and find that seven scattered-site public housing developments in Yonkers, New York have 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 as well as price levels after completion.[4] Their results suggest that proximity to public housing units can, in certain circumstances, be associated with a modest increase in the prices of single-family homes.

An additional drawback of these studies (with the exception of Santiago, Galster, and Tatian 2001) is that they do not allow subsidized housing effects to vary with time since project completion – an important caveat in the light of the above discussion on the timing of impacts.

Ellen et al. (2002) address some of these methodological problems in their study of the impact of affordable homeownership programs in New York City. They estimate a difference-in-difference hedonic regression model – with the impact of housing investment estimated as the difference between the change in prices of properties near the new owner-occupied homes before and after completion and the price appreciation experienced by properties outside the ring but still in the same neighborhood. They find evidence of significant, positive spillover effects. But the analysis is limited to measuring the impact of investments in new, owner-occupied homes, which we would expect to generate more positive neighborhood impacts.

As detailed below, our data permit us to improve on the models used in earlier papers in several respects. We have access to data on a far larger set of housing investments than earlier

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

papers and are thus able to arrive at more precise estimates of impacts and have room to test for differences in program impacts across neighborhoods and types of housing investments. Most fundamentally, the New York City experience offers an unusually good test of the impacts of subsidized housing. While sites were surely not chosen randomly, city officials were essentially given a set number of pre-determined sites in the city. By the end of our study period, virtually all were developed.

II. The Model

Our central approach involves a hedonic regression model with a difference-in-difference specification. In brief, we compare the prices of properties within a certain "ring" of subsidized housing with the prices of comparable properties that are outside this ring but still in the same neighborhood. We then compare the magnitude of this difference before and after the completion of housing.[5] Our model builds on that of Santiago, Galster, and Tatian (2001), but we introduce several meaningful extensions. First, while Santiago, Galster, and Tatian (2001) implicitly assume that neighborhood fixed effects are constant over time and therefore that prices in all neighborhoods increase at the citywide rate, we allow for neighborhood-specific trends in prices by including a set of quarter-specific, neighborhood fixed effects. Thus we are better able to distinguish the effects of housing investment from the effect of other changes in the local neighborhood.

Second, none of these earlier studies accounts for the possibility that large projects might be systematically located in more distressed neighborhoods than smaller projects. This is a likely scenario given that the extent of blight to be removed is larger in more dilapidated

[5] Ellen, Schill, Susin, and Schwartz (2002) employs a similar methodology to evaluate the impact of investments in selected homeownership developments.

neighborhoods. If so, then the estimated impact of project scale is likely to be downward biased. We control for these selection effects by allowing the initial prices in the rings surrounding subsidized sites to vary with the number of assisted units that are ultimately built on a site.

Third, previous studies of the impact of subsidized housing on sales prices use a single dummy variable to capture proximity to subsidized housing (typically location in the same census tract as subsidized housing), which can lead to misleading impact estimates. If, for example, most of the properties that sell within a certain ring of subsidized units are located at the edge of the ring, the average estimated impact within the ring is likely to underestimate actual impacts. Our model uses a distance gradient approach, which allows the effects of subsidized housing to vary with distance from the housing and generates more precise estimates in turn.[6]

Finally, the reliance of earlier studies on hedonic equations may raise concern about potential biases in the impact estimates. In this paper, we estimate a repeat sales specification to supplement the hedonic models. And, we explore alternative specifications of the distance function, specifications which allow for the impacts to vary across submarkets with different characteristics and which allow us to estimate separate impacts at the start of the projects and at completion.

Baseline Model

To be concrete, the centerpiece of our empirical work is a hedonic model of the price of property:

[6] Galster, Tatian, and Smith (1999) and Santiago, Galster and Tatian (2001) make the first steps towards a more elaborate treatment of distance by including several dummy variables corresponding to different distance ranges.

$$(1)\ \ \ln P_{icdt} = \alpha + \beta X_{it} + \delta_c W_c + \gamma R_{it} + \theta R_{it} D_i + \lambda R_{it} D_i^2 + \rho_{dt} I_{dt} + \varepsilon_{it}$$

where $\ln P_{icdt}$ is the log of the sales price per unit 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), D_i is the Euclidean distance between property i and the nearest project site, 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, θ, λ, and ρ, and ε is an error term.

Property related characteristics, X_{it}, include structural characteristics of the properties, such as building age, square footage, the number of buildings on the lot, and dummy variables distinguishing 18 different building classifications such as 'single-family detached' or 'two family home,' and so on. Census tract fixed effects (W_c) control for unobserved, time-invariant features of different neighborhoods.[7]

The ring variables (R_{it}) capture the impact of proximity to housing units created with city assistance. To be specific, "In Ring" is a dummy variable that takes a value of one if the property is located within 2,000 feet of a site on which there is or will be at least one subsidized housing unit.[8] Thus, "In Ring" captures baseline differences in sales prices between properties located within a 2,000-foot ring of subsidized housing sites and those outside. Because baseline

---

[7] The Census Bureau originally defined census tracts to capture cohesive neighborhoods, and researchers typically use tracts to proxy for neighborhoods. Although census tracts in New York City are relatively small (due to high population density), they are large enough that there may still be significant within-tract variation, potentially biasing the results. In the end, only a model with individual property-specific fixed effects would fully eliminate this possibility, which is essentially a repeat sales model. The potential drawback of repeat sales analyses is selection bias -- 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.

[8] 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 the impact of the initial disamenity (blighted site before construction of city-assisted housing) extends to roughly 2,000-feet. Results are substantially similar when using smaller rings.

property values are also associated with both the size of the site and the nature of the project to be built, six separate "In Ring" variables are included, distinguished by the scale of the project to be built (more or less than 100 units) and whether the units provide for homeownership, rental, or a mix of tenures.

A "Post Ring" dummy variable takes a value of one if the sale is within the ring of some number of *completed* city-assisted units; its coefficient provides the simplest impact estimate.[9] The number of completed units within the ring of the sale (and its square) offers a measure of the marginal effects of additional subsidized units, and the proportion of assisted units in the ring that are in multifamily, rental projects capture differences in property prices due to differences in tenure or structure.[10] 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.[11]

Interactions between distance, D_i, and the set of ring variables R_{it} allows for a pre-project distance gradient within the 2,000-foot ring, a post-completion distance gradient (again within the ring) and allows this gradient to change over time post-completion. D_i is interacted with each of the six in-ring variables to allow the gradients to vary with the size and composition of the new housing units. We also interact the Post Ring variable with distance to allow impacts to vary with distance in a similar fashion. In addition, by interacting distance with Tpost and number of units, we explore how that gradient changes over time and with project scale.

Potential non-linearities in distance are captured in two ways. First, we include a single

---

[9] If a sale was within 2,000 feet of more than one project, we use the completion date of the first completed.

[10] Although it would be desirable to test separately for the effects of the proportion of rental housing and the proportion of multifamily housing, the high correlation between the two precludes using separate variables.

[11] 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 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 (2002), for a useful discussion.

squared distance term for all ring types.[12] (Note that in the specifications shown below, we exclude the interactions of the Post Ring variable and the Tpost variable with the square of distance because these coefficients were found to be consistently insignificant.) Second, in an alternative specification, we interact the Post Ring, number of units and Tpost variables with a set of dummy variables corresponding to four distance intervals: 0-500 feet, 501-1000 feet, 1001-1500 feet and 1501-2000 feet. This model allows impacts to vary with distance in a nonparametric fashion. Taken together, the coefficients on these variables capture the pre-completion and post-completion distance gradients, 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.

A set of variables control for proximity to other subsidized housing such as occupied units that received renovation subsidies through various city programs and projects sponsored by the federal government (such as Section 202 and Section 8 units). In each case, a set of ring variables controls for both selection effects 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 assumed that price changes were constant across the city, this seems particularly inappropriate in a city as large and diverse as New York. Indeed, Schwartz, Susin, and Voicu (2003) find considerable variation in price trends across community districts in New York City. Including these community district-specific time trends helps us to control for the other neighborhood investments or changes that may have occurred at the same time as the housing investments.

[12] In results available from the authors, we also estimated models in which different quadratic terms were estimated or each ring type - an F-test could not reject the hypothesis that the coefficients on the six quadratic terms were

While specifying the time dummies using an even 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.[13] Moreover, there is little variation in the time dummies within the community districts – an F-test could not reject the hypothesis that census tract-quarter dummy variables were the same within a community district.[14]

As described below, we estimate several alternative models to the baseline model in equation (3). First, we estimate a model in which we control for trends in prices in the 2,000-foot ring around subsidized housing prior to the completion of the housing. Second, we estimate a repeat sales specification, using only properties which sold at least twice and fully controlling for time invariant property characteristics, albeit at the potential cost of selection bias. A third alternative specification allows impacts to vary across neighborhood types. Finally, we adapt the model to include information on the start and completion of construction on a project in an effort to gain insight into the nature of the externality.

Prior Trends

To help mitigate concerns about selection bias, we estimate a specification that includes controls for trends in the relative price of housing in the vicinity of subsidized housing sites prior

---

insignificantly different from one another across ring types.

[13] Plus, using census tract-quarter dummies would have made a repeat-sales specification infeasible, since the vast majority of the tract-quarter cells in our sample do not include multiple houses that sold repeatedly during the study period.

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

to the construction of the housing. This specification provides an estimate in which the counterfactual is that the price gap between properties in the vicinity of subsidized housing sites and properties in the larger neighborhood would have continued to shrink (or grow) at the pre-completion rate, had no subsidized housing been built.

Repeat Sales

Previous analyses have relied almost exclusively on hedonic analyses, which may yield biased impact estimates if there are relevant property characteristics that are unmeasured or omitted from the regression equation.[15] In this case in particular, it seems quite possible that the mix of properties that sells in the vicinity of new, city-assisted housing changes after the construction of the subsidized housing in ways that are not captured by the hedonic variables. Perhaps the "better" properties sell once the city-assisted housing is constructed. If so, then hedonic analysis may overstate the spillover benefits.

As discussed above, a repeat sales model relies solely on properties that sold multiple times over the study period, and is 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).

To arrive at the repeat sales estimating equation, we start with an equation for the logarithm of the price of the i^{th} house at time t' following (1):

---

[15] We are aware of only a few studies that have employed both hedonic and repeat sales methods to estimate the impact of various neighborhood quality measures on house prices: Kohlhase (1991), Downes and Zabel (1997), and

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

Subtracting (2) from (1) gives an equation for the change in the log price of housing:

$$(3)\quad \Delta_{t,t'} \ln P_{icd} = \beta \Delta_{t,t'} X_i + \gamma \Delta_{t,t'} R_i + \theta \Delta_{t,t'} R_i D_i + \lambda \Delta_{t,t'} R_i D_i^2 + \rho_{dt} I_{dt} - \rho_{dt'} I_{dt'} + \Delta_{t,t'} \varepsilon_i$$

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

Equation (3) 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 specific 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 omitted variables were time invariant, then (3) 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:

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

This is the repeat sales equation that we estimate in this paper. Estimation of (4) yields project impact estimates that are free of any bias due to the omission of time-invariant characteristics of

---

Schwartz, Susin, and Voicu (2002).

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

We next explore the extent to which the impacts of housing investments vary with income levels in a neighborhood. While Michaels and Smith (1990) find significant differences in the impact of hazardous waste sites across submarkets, few have explored such variation in the context of housing investments, despite its clear policy interest.[16] We test for heterogeneity in impacts between low- and high-income areas by interacting all of our ring variables, ring-distance interaction variables, and hedonic variables with a dummy variable indicating neighborhood income level.[17]

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. That is, in addition to the existing post-completion ring variables, we include a similar set of post-start ring variables - Post Ring and

[16] Santiago, Galster, and Tatian (2001) is an exception, exploring whether the effects of scattered-site public housing vary with neighborhood characteristics, but the authors do not explore interactions with project scale or type.
[17] In earlier models, an F-test rejected the hypothesis that the coefficients on property characteristics are similar across neighborhoods.

Tpost variables defined by the start of construction, the number of started units within the ring of the sale and its square, the proportion of started units in the ring that are in multifamily, rental projects, and variables interacting Post Ring (start), Tpost (start), and number of started units with distance. 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. 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 in the city. The initiative, which later came to be known 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, uninhabitable buildings).[18] We do not study the impact of the roughly 116,000 occupied housing units that received renovation subsidies through the program because these investments do not increase population and may not be visible outside of the building and thus are not likely to generate a similarly large impact.[19]

Certainly, a principal objective of the Ten Year Plan was to create additional housing

[18] Note that while 66,000 is a very large number of housing units, it is not large relative to the total stock of housing. This 66,000 represents just 2 percent of the 3 million housing units in New York City (Wallin et al 2002, 9).

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 the city's Department of Housing Preservation and Development (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 have 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 largely dictated by where the city owned property. During the late 1970s, as a result of large population losses, rising landlord costs and stagnant tenant incomes, New York had taken ownership through tax foreclosure of over 60,000 units in vacant buildings and another 40,000 units in occupied buildings. This so-called *in rem* housing, named after the legal action that vested title in the city, would provide the raw material for most of the program. Through the course of the Ten Year Plan, the city developed virtually all of these properties as well as its vacant land. By the time Mayor Michael Bloomberg announced his housing plan in 2002, the City's inventory of vacant housing units had fallen to just over 3,000 vacant units, just 5 percent of the original 60,000 that it owned in the early 1980s (Park 2003). So while HPD might have targeted more promising sites at the start, the agency ultimately subsidized the development of virtually all available sites.

The Ten Year Plan comprised a wide range of programs, some of which provided subsidies to nonprofit organizations and some of which provided subsidies to for-profit

---

[19] Investments in occupied buildings do not increase population and may not be very visible to outsiders.

developers. In general, land or buildings from the city's inventory of *in rem* property (both vacant and occupied) were conveyed at little or no cost to a developer. Capital subsidies were provided in the form of below market interest rate mortgage loans. Interviews with city officials suggest that the market was so depressed in these neighborhoods that private developers were unwilling to develop housing without such subsidies, even if given land or buildings for free.

A request for proposals was the first public announcement of a planned redevelopment, followed by formal community board consideration, the closing of financing and the commencement of construction, which was typically completed within two to three years of the RFP issuance.

IV. Summary of Data

To undertake the analysis outlined above, we obtained data from New York City's Department of Housing Preservation and Development (HPD) describing all of the new and gut-rehabilitated, 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.

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.[20] 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.[21] Our final sample includes 293,786 property sales, spread across 1,606 census tracts.[22] Both because of the long time span of the data and New York City's size, this is a very 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).[23] Nonetheless, these building characteristics explain variations in prices surprisingly well, suggesting the data are rich enough for estimating hedonic price equations.

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

[20] Note that sales of cooperative apartments are not considered to be sales of real property and are not included in the data set. Note also that most of the apartment buildings in our sample are rent stabilized. Given that legally allowable rents were typically *above* market rents outside of affluent neighborhoods in Manhattan and Brooklyn during the period of our study, we do not believe that their inclusion biases our results (see Pollakowski 1997).

[21] 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.

[22] 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.

[23] 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

our database to all Ten Year Plan and other housing sites and, from these distance measures, created a variable that identified properties within 2,000 feet of housing investments of different types. A continuous distance variable indicates the distance from the property sale to the closest city-assisted housing site.[24]

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 were located or at some point would 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.

---

merged RPAD data from 1990 and 1999, we found that characteristics changed very rarely (Ellen et al 2001). Even among these apparent changes, we suspect that a majority are corrections, rather than true changes.

[24] 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 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.

Properties located within the 2,000-foot ring were more likely to be in Brooklyn and less likely to be in Staten Island and Queens. Properties in the 2,000-foot ring were also older, less likely to be single-family homes, more likely to be walk-up apartments, and consistent with these differences, less likely to have garages.

Table 2 shows the distribution of property sales by proximity to scale and type of city-assisted housing. The table shows that of the properties located within 2,000 feet of a Ten Year Plan site, 29 percent were located in rings that included only homeownership projects, 27.9 percent were in rings that included only rental projects, and the remainder were located in rings with both types of housing. In terms of size, just over half (54 percent) of the properties within 2,000 feet of a Ten Year Plan site were located in rings where fewer than 100 units would ultimately be built.

As noted, we identified two submarkets (defined by community districts) based on household income information from the 1990 Decennial Census: a low-income submarket, consisting 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. On average, 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 2-3 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.[25]

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

V. Results

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 relatively high R^2 (0.86), together with the fact that the coefficients on the structural variables are consistent with expectations, suggest that these variables provide adequate controls for the characteristics of the houses sold.

Table 3 shows that the coefficients on all six in ring dummy variables are negative and statistically significant. In particular, prior to completion, properties located right next to a city-assisted housing site (D =0) sold for 17 to 36 percent less than comparable properties located outside the 2,000-foot ring. This is consistent with the presence of disamenities at the sites upon which city-assisted housing was eventually built, not surprising, perhaps, given that 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.

Coefficients on the In Ring*Distance variables are consistently positive indicating a sharp price gradient such that the pre-completion price-depressing effects of the site (the disamenity) decline with distance. For sales adjacent to sites that will ultimately hold more than 100 homeowner units, for example, estimated prices are initially 21 percent lower than in the surrounding neighborhood. At a distance of 2,000 feet, prices are only about 3 percent lower. At an average city block of 500 feet, the price differential falls by about 4.5 percentage points per city block.[26] Gradients are steeper for sites that will hold rental units.

[26] 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.

Turning to impact estimates, the positive and statistically significant coefficient on Post Ring indicates that, on average, the construction of subsidized housing units generated significant and positive external benefits. Building more units appears to bring a greater benefit, though this marginal effect declines as the number of units increases. Further, impacts decline with the share of units in rental, multifamily buildings.

As for changes over time and space, the positive, significant coefficient on Tpost implies that impacts grow over time, perhaps as families move in and the population rises. And, impacts shrink with distance from the new housing. Further, 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 2 shows estimated impacts for the average project in our sample.[27] The thick line shows the percentage difference between prices at the given distance and prices in the surrounding neighborhood, 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. Before project construction, 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, the 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

[27] 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

at 2,000 feet, impacts are approximately zero. Figure 3 shows how impact varies with scale. The figure shows that a 250-unit project has a substantially larger effect than a 50-unit project. This differential shrinks, however, with distance from the project site.

Table 4 presents the results of estimating the model with an alternative treatment of distance from subsidized housing. Here, in place of a continuous distance function, four discrete impacts are estimated -- for properties within 500 feet of subsidized housing and for those within 501-1000 feet, 1001-1500 feet and 1501-2000 feet of new housing. The first three coefficients in Table 4 can be interpreted as impacts on properties within 500 feet of subsidized housing; the other ring coefficients capture the differences in impacts in the outer rings relative to that in the 500-foot ring. These results are consistent with expectations and with the results from the baseline model in Table 3. Impacts are largest in the 500-foot ring, are smaller, but insignificantly so in the next ring, and drop significantly in the outermost rings. The impact decreases by almost 2.9 percentage points in the 1501-2000-foot ring relative to the 500-foot ring.

Turning again to estimates of the impact of an average project, as above, Figure 4 shows that one year after completion, the impact of the average project is one percentage point lower in the 501-1,000 foot ring than in the 500 foot ring, three percentage points lower in the 1,001-1,500 foot ring than in the 501-1000 foot ring, and seven percentage points lower in the 1,501-2,000 foot ring as compared to the 1,001-1,500 foot ring. Consistent with earlier results, the difference in the impact in the 500-foot ring and that in the outer rings grows over time.

In summary, we find that city-assisted housing was built on sites that acted as local disamenities within their communities. After construction, subsidized housing units have a

units) and tenure-structure mix (55.5 percent rental units in multifamily structures), are averages over all sales in a 2000 foot ring.

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 greater for larger developments and for those containing fewer units in multifamily rental buildings.

Alternative Explanations

There are several alternative explanations for the observed rise in property values in the vicinity of subsidized housing sites after their completion. First, subsidized housing developments are typically built in some of the most distressed sites in the city and it is possible that the value of the most distressed areas will naturally rise as prices bottom out and private developers begin to invest. A second, related possibility is that in choosing sites for new housing, the City simply picked winners. Third, the City may have strategically targeted other investments to the same neighborhoods where subsidized housing was built, and it was these added investments, rather than the housing itself, that produced the gains in value. Fourth, the quality of the properties on the market may have improved after the completion of the housing in ways that are not captured by the hedonic variables. Finally, the housing investment may have pushed disamenities to neighboring areas (that is, to the larger neighborhood surrounding the 2000-foot ring) or drew demand away from them. We address each of these possibilities in turn.

To start, we examine the possibility that the worst neighborhoods simply tend to get better regardless of whether subsidized housing is built there. If this is so, we should see prices in the ring of subsidized housing sites converging to price levels in their neighborhood, even before the housing built. After all, most of these sites had been distressed since the 1960s and 1970s when the original buildings were abandoned by their owners. To test for this, we examine the trend in housing prices in the ring of subsidized housing before the subsidized housing is completed. The data do suggest a slight upward trend in the years prior to the completion of the housing, but it is driven by an increase in prices in the ring of subsidized housing during the years immediately preceding completion, an increase which may have been caused by the projects themselves. (As noted above, community residents were involved in the planning process and often knew about these projects several years before the start of construction.)

Nonetheless, to be conservative, we assume that the pre-completion trend is independent of project impacts and include the trend in the regression. Even after controlling for these prior trends, we still find significant positive impacts upon project completion.[28] Thus, the positive impacts we find are not explained by a general upward trend in prices in the ring.

This exercise also dismisses the possibility that the City successfully picked winners. In fact, city officials had little latitude to pick winners. As noted above, the housing built through the Ten Year Plan was constructed on vacant city-owned property and virtually all of the city's stock of properties and land had been developed by the end of the time period observed here. Plus, interviews with city officials provide no indication that the city was aiming to choose sites where values were likely to appreciate.

As for the possibility that the City made investments in the vicinity of subsidized sites, concurrently with the construction of the housing, the inclusion of neighborhood*quarter fixed effects in the regression should largely capture the impacts of any other investments simultaneously targeted to these neighborhoods. Note that we also explicitly control for the effects of other types of subsidized housing investments, such as renovation subsidies offered to private landlords.

We estimate a repeat sales specification to test for the possibility that our results simply capture the fact that higher quality properties were sold after the completion of these developments. Repeat sales estimates for the baseline model are shown in Table 5, alongside the hedonic estimates from Table 3. The similarity of these estimates is striking. The models yield estimates that are of the same sign and general magnitude in all cases. The only exception is that the coefficient on Tpost * Distance, while still negative, is insignificant in the repeat-sales regression, but the magnitude of the coefficient is quite small in both regressions. Given the remarkable similarity between hedonic and repeat sales estimates, there is little reason to believe that the hedonic regressions are biased by failing to capture any critical quality measures.[29]

Finally, proving that the positive effects observed were not due to housing investment driving disamenities to neighboring areas or drawing demand away from them (that is, to the larger neighborhood surrounding the 2000-foot ring) is difficult. However, statistics from Schwartz, Susin, and Voicu (2003) suggest that this possibility is highly unlikely. Specifically, using police precincts to define neighborhoods, they show that, during the 1990's, property values rose and crime fell more sharply in the city's poorest neighborhoods than they did citywide. Such a pronounced improvement in the poorest neighborhoods would not likely have occurred if housing investments led to declines in property values in neighboring areas. Moreover, Schwartz, Susin, and Voicu (2003) find significant spillover benefits from the Ten-year Plan housing even at the level of community district - a considerably larger area than the 2000 foot ring. This suggests that displacement is not occurring, or if it is, crime/blight is being displaced to some area much further away.

Neighborhood Heterogeneity

Table 6 reports the results of models in which impacts are allowed to vary between lower

[28] These results are available from the authors upon request.

and higher income submarkets. Coefficients in column (1) capture impacts in high-income submarkets; coefficients in column (2) capture the difference between the impacts in high and low income markets.

The estimates tell a mixed story. The Post Ring coefficient is smaller in low-income areas, suggesting smaller effects. But the results also show that the marginal effects of additional units are significantly larger in low-income areas. For our average project of 250 units, the estimated effect is larger in a low-income area. In particular, we estimate that the price differential between a property located right next to an average project and a comparable property located outside the ring falls by 13.7 percentage points after project completion in low-income submarkets and by 10.6 percentage points in high-income submarkets. In contrast, the predicted impact of an otherwise identical 50-unit project is actually larger in the high-income submarket (14.3 percentage points versus 6.7 percentage points). This is consistent with the notion that in a poorer neighborhood with many distressed properties, a single, small project may be insufficient to have a significant impact since the remaining blight is too pervasive. In this situation, a critical mass of investment might be required to make a difference.

The coefficients on the share of multifamily units at the time of sale results suggest that the depressing effect of a larger proportion of units in multifamily, rental buildings is smaller in lower income submarkets. In other words, the difference in the impact of building owner-occupied, single-family homes and that of building rental apartments is not as large in lower income submarkets. One possible explanation is that residents in lower income submarkets are not as sensitive to the differences in incomes of the occupants living in these two sorts of developments. In higher income submarkets, residents may be more concerned about lower-

[29] 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.

income residents moving into their neighborhoods. In the end, these results are intriguing, if only suggestive, but worthy of further study.

Finally, the Tpost coefficients suggest that while positive impacts are likely to grow over time in the low-income submarket, they are likely to fall somewhat in the higher income areas. These temporal variations, however, are found to diminish with distance, as indicated by the coefficients on the Tpost*Distance interaction terms.

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

Table 7 presents the results of estimating a model that includes variables distinguishing the start and completion of construction. As shown, the coefficients on Post Ring (start) are statistically significant and very close to those based on completion dates. Moreover, the coefficients on Post Ring (completion) are not statistically significant. Thus, much of the effect is felt as soon as construction starts and there is little 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 (or reduction) of a disamenity.[30]

Notice, however, that 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 may be occupancy effects,

[30] The positive and 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. 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 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.

i.e., effects that occur through the number and characteristics of its inhabitants. Further, it suggests that housing market agents do not exhibit perfect foresight, at least with respect to the future occupants of the new subsidized housing.

Comparing Costs and Benefits

Although a fully satisfying cost-benefit analysis is outside the scope of this paper, we have made some effort to compare the magnitude of the potential increases in property tax revenues generated by these housing investments to their approximate costs. On the cost side, we rely on HPD estimates of subsidies provided for some of the more representative programs.[31] We use high cost estimates in order to be conservative.[32] However, we do not include the value of federal rent subsidies that some residents receive since these are portable benefits, nor do we include an estimate of the federal taxes foregone due to the low-income housing tax credits that were used in some of the rental projects.

To simulate the tax revenue gains, we estimate the aggregate increase in property values generated by each Ten Year Plan project (within a 2,000-foot radius) and then apply a standard tax assessment formula to these benefits.[33] To estimate the aggregate benefits, we undertake a three-step process. First, we use the RPAD database[34] to identify all the residential properties (whether sold or not) that were located within 2,000 feet of a Ten Year Plan housing site and

[31] Costs are based on HPD estimates of average subsidy per unit built for some of the more representative programs. For programs missing cost information, cost is computed as the average costs of programs in the corresponding program category (i.e., rental new construction, homeownership new construction) for which data are available. Since the HPD cost estimates pertain to different points in time, we express all costs in constant 1999 dollars to make them comparable with the benefits.

[32] Clearly, some costs may be under-estimated because of these exclusions. At the same time, although virtually all of the city subsidies came in the form of long-term loans, it is unclear whether the city will require repayment of these loans when they mature. Thus, we treat all subsidies as grants rather than loans.

[33] Since all Ten Year Plan new construction receives property tax exemptions under the 421-a or 421-b program, we assume the new subsidized units do not pay any property taxes over the period of the analysis, and we base our tax revenue calculation only on properties surrounding the projects.

thus should have benefited from the construction of the housing.

Second, we assign an approximate initial or "pre-completion" price to each of these properties. To do this, we allocated these properties to one of six mutually exclusive ring types (defined above by our "In Ring" variables). For each of these six ring types, and for each year in our data, we calculated the average per unit sales price for all properties that sold prior to the completion of any subsidized housing.[35] We then assigned a pre-completion price to each property (sold or not) equal to the mean pre-completion price estimated for its ring type and year of earliest project completion.[36]

Third, we use the pre-completion price and number of units of each property, the size and structure of the subsidized projects within 2,000 feet of each property, the distance from each property to the nearest project site, and our baseline post-completion coefficients, to estimate the increase in property values that should have occurred immediately following project completion. The sum of these gains is an estimate of the total benefit that all the Ten Year Plan housing delivered to residential properties within a 2,000-foot radius.[37]

Using this three-step method, we estimate that the benefits generated in the 2,000-foot ring total about $6.8 billion.[38] To measure the corresponding tax revenue gains, we first discount total benefits by a factor of 0.7 since the appraised market values appraised by the New York

---

[34] The RPAD database is an annual census of all New Yo rk City properties, described above.

[35] For some ring types there were no pre-completion sales in one or more years at the end of the study period. The average price for a year with no sales is computed via extrapolation, based on the average annual growth rate for the available years.

[36] In preliminary work, we assigned to each property a pre-completion price equal to the 1990 mean pre-completion price for its ring type and the results were similar to those presented in the paper.

[37] To account for the time it takes to complete a project (and the time between when costs are incurred and benefits accrue), we compute the present value of benefits at project start using a discount rate of 7.28%, (the average annual interest rate for the 30-year treasury bond over the study period). The reported estimates assume a 1.7 year construction period which is the average in our data. Similar results obtain with different discount rates and/or alternative assumptions about the duration of project construction.

[38] A comparison between the 1998 assessed values of properties in the 2,000-foot ring that sold in 1999 and the assessed values of *all* properties in the 2,000-foot ring suggests that transacting properties may have higher values than other properties. We this correct for this selection bias when estimating total benefits.

City Department of Finance for tax assessment purposes are, on average, about 70% below sales prices (on which our measured benefits are based). We then estimate the increase in tax revenues in the first year after project completion by applying the New York City assessment ratios and tax rates to the discounted benefits.[39] Finally, we compute the total tax revenue gains as the present value of the stream of annual tax benefits over the amortization period of the debt issued by the city to provide the construction subsidy, based on assumptions suggested by the staff of the New York City Independent Budget Office.[40]

In the end, we estimate that the city tax benefit amounts to about $2.8 billion due to the new housing built through the Ten Year Plan housing investment. While this exceeds the city's own investment of $2.4 billion, it does not exceed the total public investment, which includes state and federal dollars, and amounts to $3.7 billion. However, these estimated benefits are substantial, especially given that we have not considered the benefits enjoyed by the households that actually get to live in the new subsidized housing.

VI. Conclusion

Our results suggest that New York City's investment in new, subsidized housing generated significant external benefits and that these benefits have been 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. Further, spillover effects diminish

[39] Assessment ratios and tax rates vary by property type. The Department of Finance groups residential properties into three classes - class 1 (1-3 family houses), class 2 (4-6 family buildings), and class 2A (all the others) - and sets an assessment ratio and a tax rate for each class. The assessment ratio for class 1 is 0.08, for class 2 is 0.25 and for class 2A is 0.45. The tax rate for class1 is 0.116, and for the other two classes is 0.108. Due to this variation in assessment and tax rates, we also estimate the total benefits from subsidized housing separately for each of the three building classes and then apply the corresponding rates to the benefit for each class.

[40] In particular, we assume a debt amortization period of 20 years, an annual growth of 2.5 percent in property taxes over 20 years, based on average increases in assessments, and a discount rate of 5.35 percent (the average for recent city general obligation debt issues).

with distance from the housing investment sites. Our results are not sensitive to alternative specifications of the distance function, and impact estimates remain positive even after controlling for existing price trends prior to housing construction. In addition, estimates based upon a repeat sales methodology are very similar to those obtained in the hedonic regressions.

The paper also sheds some light on the kinds of investments likely to generate the largest spillover effects, by investigating how the impact of different projects varies across different submarkets. 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 consider the relative importance of two potential sources of spillovers – disamenity removal and amenity provision - by including both start dates and completion dates within a single regression. We 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, driven by the number and characteristics of its inhabitants. Finally, our cost-benefit estimates suggests that the gain in tax revenue generated in the 2000-foot ring exceeded the subsidies provided by the city.

The implication is that the conventional wisdom that place-based housing subsidies hold no advantages over people-based housing subsidies needs to be reconsidered. Housing investment, used strategically, may well deliver considerable positive externalities in the form of increases in property values in the vicinity of the investment. Thus, cities may be able to use housing subsidies to serve two purposes – create new, affordable housing units for qualified recipients and revitalize urban neighborhoods. Further, the rise in property values in the vicinity of the new housing offers the prospect of using tax increment financing, or a similar policy instrument, to finance the subsidies required. Perhaps equally important from a policy perspective, is the possibility that a more effective deployment of housing investments can be achieved by directing larger projects towards more distressed communities.

Acknowledgments

The authors gratefully acknowledge the financial support of the Lincoln Institute for Land Policy and thank Scott Susin for his intellectual contributions to this research. They would also like to thank Jeffrey Liebman and other participants in the NBER Conference on Government Expenditures for useful comments on a previous draft. Finally, they would like to thank Kathleen Dunn, Ken Lowenstein, Jerry Salama, and Marc Willis for sharing their insights about New York City's housing policies and to express their gratitude to Jerilyn Perine, Richard Roberts, Harold Shultz, Calvin Parker, Ilene Popkin, and Harry Denny of the New York City Department of Housing.

References

Aaron, Henry J. 1972. *Shelter and Subsidies: Who Benefits from Federal Housing Policies?* Washington, DC: The Brookings Institution.

Briggs, Xavier de Souza, Joe T. Darden, and Angela Aidala. 1999. "In the Wake of Desegregation: Early Impacts of Scattered-Site Public Housing on Neighborhoods in Yonkers, New York." *Journal of the American Planning Association*. 65(1):27-49.

Caplin, Andrew and John Leahy. 1998. "Miracle on Sixth Avenue: Information Externalities and Search." *The Economic Journal.* 108(January):60-74.

Cummings, Jean L., Denise DiPasquale and Matthew E. Kahn. 2000. "Inner City

Homeownership Opportunities and Black Community Choice" (unpublished paper presented at the American Real Estate and Urban Economics Annual Meetings, 2001).

Daniels, Glynis and Michael H. Schill. 2001. *State of New York City's Housing and Neighborhoods 2001*. New York: New York University, Center for Real Estate and Urban Policy.

Downes, Thomas and Jeffrey Zabel (2002) "The Impact of School Characteristics on House Prices: Chicago 1987-1991" *Journal of Urban Economics* Vol. 52. 1-25.

DeSalvo, Joseph S. 1974. "Neighborhood Upgrading Effects of Middle-Income Housing Projects in New York City." *Journal of Urban Economics*. 1:269-277.

Ellen, Ingrid Gould and Margery Austin Turner. 1997. "Does Neighborhood Matter? Assessing Recent Evidence." *Housing Policy Debate* 8(4): 833-866.

Ellen, Ingrid Gould, Michael H. Schill, Scott Susin, and Amy Ellen Schwartz. 2002."Building Homes, Reviving Neighborhoods: Spillovers from Subsidized Construction of Owner-Occupied Housing in New York City." *Journal of Housing Research* 12(2): 185–216.

Galster, George C., Peter Tatian, and Robin Smith. 1999. "The Impact of Neighbors Who Use Section 8 Certificates on Property Values," *Housing Policy Debate*.10(4): 879-917.

Galster, George C., Anna M. Santiago, Peter Tatian, and Robin Smith. 1999. "Assessing Property Value Impacts of Dispersed Housing Subsidy Programs." Report to the U.S. Department of Housing and Urban Development, Washington, DC.

Goetz, Edward, Hin Kin Lam, and Anne Heitlinger. 1996. "There Goes the Neighborhood? The Impact of Subsidized Multi-Family Housing on Urban Neighborhoods." Minneapolis: Center for Urban and Regional Affairs Working Paper 96-1.

Hirsch, Arnold. 1983. *Making the Second Ghetto: Race and Housing in Chicago 1940-1960*.

Cambridge: Cambridge University Press.

Kiel, K. and J. Zabel (2001) “Estimating the Economic Benefits of Cleaning Up Superfund Sites: The Case of Woburn, Massachusetts”*Journal of Real Estate Finance and Economics* 22:2/3, 163-184

Koch, Edward I. 1985. "The State of the City: Housing Initiatives." (Undated text of speech dated January 30, 1985).

Kohlhase, Janet E. 1991. "The Impact of Toxic Waste Sites on Housing Values," Journal of Urban Economics, 30: 1-26.

Lee, Chang-Moo, Dennis P. Culhane, and Susan M. Wachter. 1999. "The Differential Impacts of Federally Assisted Housing Programs on Nearby Property Values: A Philadelphia Case Study." *Housing Policy Debate.* 10(1):75-93.

Lyons, Robert F. and Scott Loveridge. 1993. “An Hedonic Estimation of the Effect of Federally Subsidized Housing on Nearby Residential Property Values” Staff Paper P93-6. St. Paul, MN: Department of Agriculture and Applied Economics, University of Minnesota.

Matulef, Mark. 1988. "The Effects of Subsidized Housing on Property Values." *Journal of Housing*. November/December:286-287.

McClure, Kirk. 1998. “Housing Vouchers versus Housing Production: Assessing Long-Term Costs.” *Housing Policy Debate.* 9(2): 355-71.

Mills, Edwin S. and Bruce W. Hamilton. 1994. *Urban Economics*. 5th ed. New York, N.Y.: HarperCollins College Publishers.

Newman, Sandra J. and Ann B. Schnare. 1997. “...And a Suitable Living Environment”: The Failure of Housing Programs to Deliver on Neighborhood Quality.” *Housing Policy Debate.* 8(4): 703-741.

New York City, Office of the Mayor. 1988. Press Release dated May 4, 1988.

New York City Department of Housing Preservation and Development. 1989. *The 10 Year Plan*. New York: Author.

-----. 2000. *Building on a Solid Foundation: Conference Proceedings Summary*. New York: Author.

Nourse, Hugh O. 1963. "The Effects of Public Housing on Property Values in St. Louis." *Land Economics.* 39: 433-441.

Olsen, Edgar O. 2003. "Housing Programs for Low-Income Households," in *Means-Tested Transfer Programs in the U.S.*, ed., Robert Moffitt, National Bureau of Economic Research (Chicago: University of Chicago Press).

Ondrich, Jan, Alex Stricker and John Yinger. 1999. "Do Landlords Discriminate? The Incidence and Causes of Racial Discrimination in Rental Housing Markets." *Journal of Housing Economics* 8(3):185-204.

Park, Molly Wasow. 2003. "Mayor Bloomberg's Housing Plan: Downpayment for the Future," New York City Independent Budget Office Fiscal Brief.

Polinsky, Mitchell A. and Ellwood T. David. 1979. "An Empirical Reconciliation of Micro and Grouped Estimates of the Demand for Housing." *Review of Economics and Statistics* 61(2):199-205.

Pollakowski, Henry O. 1997. "The Effects of Rent Deregulation in New York City." Cambridge, Mass.: MIT Center for Real Estate.

Poterba, James M. 1984. "Tax Subsidies to Owner-Occupied Housing: An Asset Market Approach." *Quarterly Journal of Economics* 99(4): 729-752.

Rabiega, William A., Ta-Win Lin, and Linda Robinson. 1984. "The Property Value Impacts of

Public Housing Projects in Low and Moderate Density Residential Neighborhoods." *Land Economics*. 60: 174-79.

Santiago, Anna M., George C. Galster, and Peter Tatian. 2001. "Assessing the Property Value Impacts of the Dispersed Housing Subsidy Program in Denver." *Journal of Policy Analysis and Management.* 20(1): 65-88.

Schill, Michael H. 1990. "Privatizing Federal Low Income Housing Assistance: The Case of Public Housing." *Cornell Law Review* 75: 878-948.

Schill, Michael H. 1993. "Distressed Public Housing: Where Do We Go From Here?" *The University of Chicago Law Review* 60(2): 497-554.

Schwartz, Amy Ellen, Scott J. Susin, and Ioan Voicu. 2003. "Has Falling Crime Driven New York City's Real Estate Boom?" *Journal of Housing Research*. 14(1): 101-135.

Shroder, Mark and Arthur Reiger. 2000. "Vouchers versus Production Revisited." *Journal of Housing Research*. 11(1): 91-107.

Wallin, Denise, Michael H. Schill, and Glynis Daniels. 2002. *The State of New York City's Housing and Neighborhoods, 2002.* New York: Furman Center for Real Estate and Urban Policy.

Table 1: Characteristics of Properties Sold

	Percentage of all property sales	Percentage of sales within 2,000 feet of Ten Year Plan new 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 Ten Year Plan units were built or rehabilitated.

Table 2: Distribution of Properties Sold within 2000 Feet of Any Ten Year Plan New 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

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 4. Step-Function Distance Specification

Post Ring variables		
Post Ring, 0-2000 ft.	0.0682	***
	(0.0091)	
Number of completed units at the time of sale, 0-2000 ft.	2.1E-04	***
	(2.1E-05)	
Tpost, 0-2000 ft.	7.9E-03	***
	(0.0014)	
Post Ring, 501-1000 ft.	-0.0093	
	(0.0096)	
Number of completed units at the time of sale, 501-1000ft.	3.6E-05	**
	(2.8E-05)	
Tpost , 501-1000ft.	-9.1E-03	***
	(0.0016)	
Post Ring , 1001-1500 ft.	-0.0169	*
	(0.0100)	
Number of completed units at the time of sale, 1001-1500ft.	-6.7E-05	*
	(3.7E-05)	
Tpost , 1001-1500ft.	-6.0E-03	***
	(0.0017)	
Post Ring , 1501-2000 ft.	-0.0293	***
	(0.0100)	
Number of completed units at the time of sale, 1501-2000ft.	-2.9E-04	***
	(5.6E-05)	
Tpost , 1501-2000ft.	-6.3E-03	***
	(0.0018)	
(Number of completed units at the time of sale)^2, 0-2000 ft.	-8.6E-08	***
	(9.0E-09)	
Share of completed renter-multi-family units at the time of sale, 0-2000 ft.	-0.0348	***
	(0.0053)	
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

	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	293,786
R^2	0.7403	0.8575

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 Neighborhood Heterogeneity

	High Income Submarket	Low Income - High Income Differential
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)
(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. Start Date vs. Completion Date as the Critical Event

Post start date ring variables		
Post Ring (start)	0.1114	***
	(0.0098)	
Post Ring (start) * D	-5.9E-05	***
	(6.6E-06)	
Number of started units at the time of sale	1.0E-04	**
	(4.2E-05)	
Number of started units at the time of sale * D	-4.4E-08	
	(3.5E-08)	
(Number of started units at the time of sale)^2	-2.5E-09	
	(2.1E-08)	
Share of started renter-multi-family units at the time of sale	-0.0108	
	(0.0069)	
Tpost (start)	6.3E-03	**
	(0.0025)	
Tpost (start) * D	8.1E-07	
	(1.7E-06)	
Post completion date ring variables		
Post Ring (completion) * D	-0.0117	
	(0.0112)	
Post Ring (completion) * D	1.5E-05	**
	(7.3E-06)	
Number of completed units at the time of sale	2.1E-04	***
	(4.6E-05)	
Number of completed units at the time of sale * D	-1.4E-07	***
	(3.9E-08)	
(Number of completed units at the time of sale)^2	-9.7E-08	***
	(2.2E-08)	
Share of completed renter-multi-family units at the time of sale	-0.0231	***
	(0.0073)	
Tpost (completion)	-1.3E-03	
	(0.0029)	
Tpost (completion) * D	-3.5E-06	*
	(2.0E-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.

Figure 1
Hypothetical Timeline of Project Impact

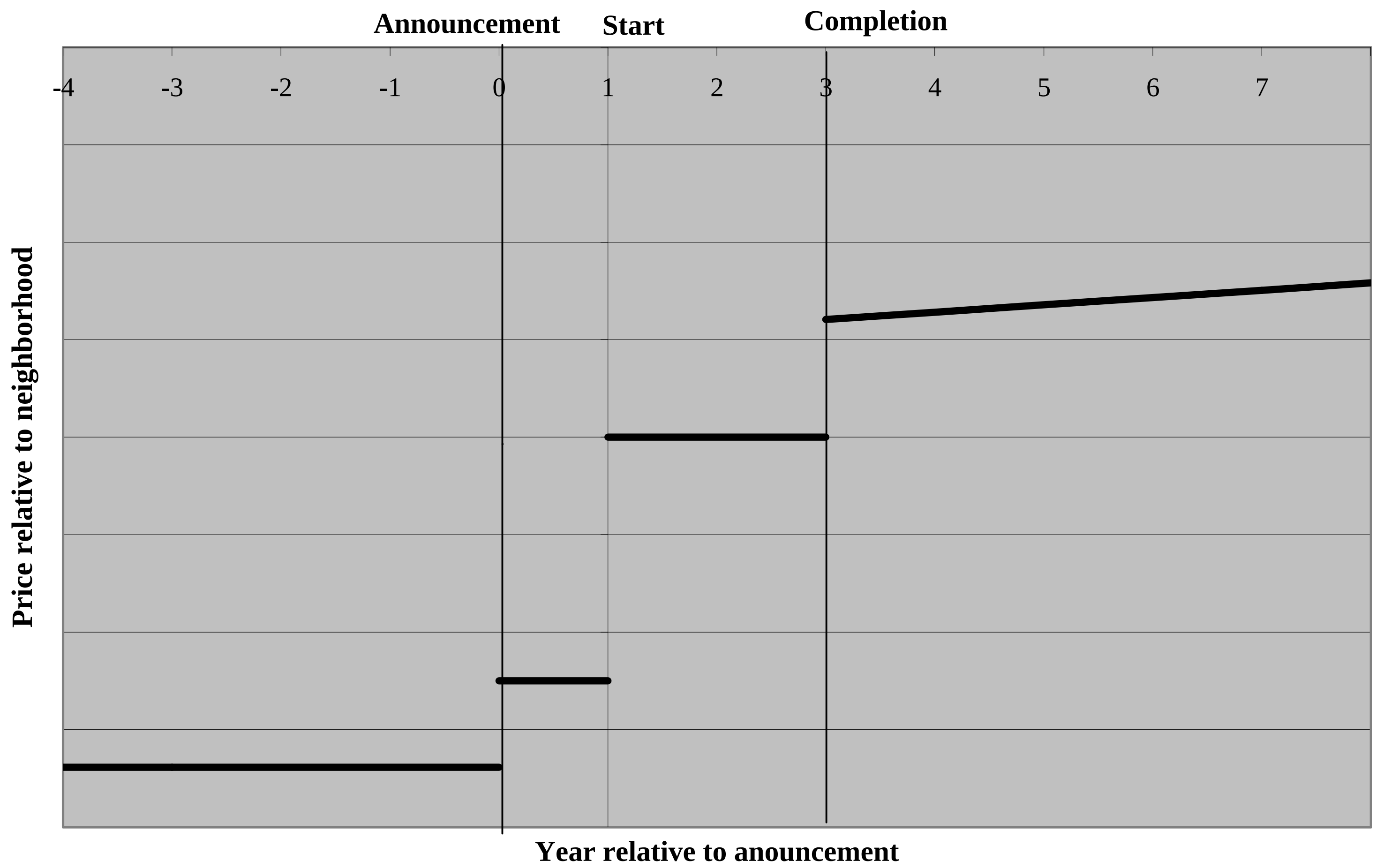
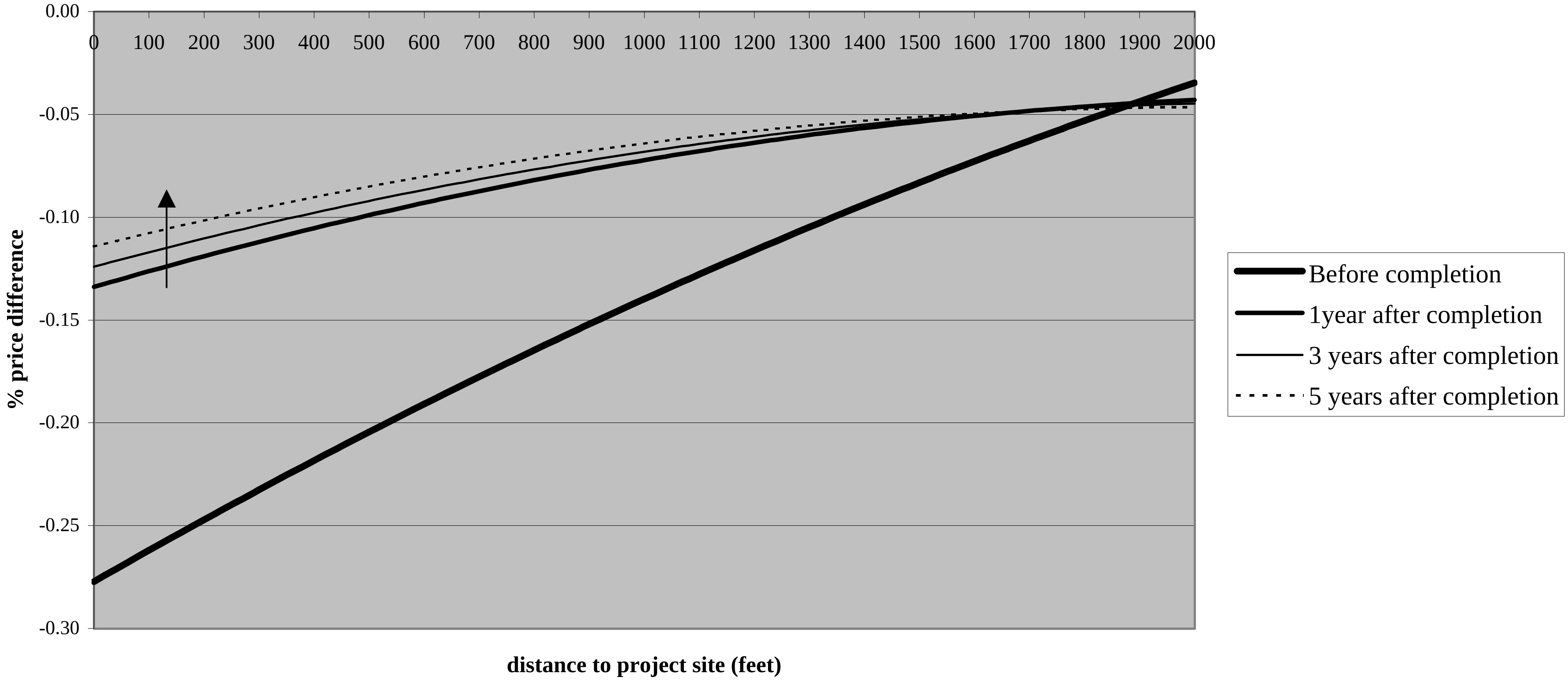


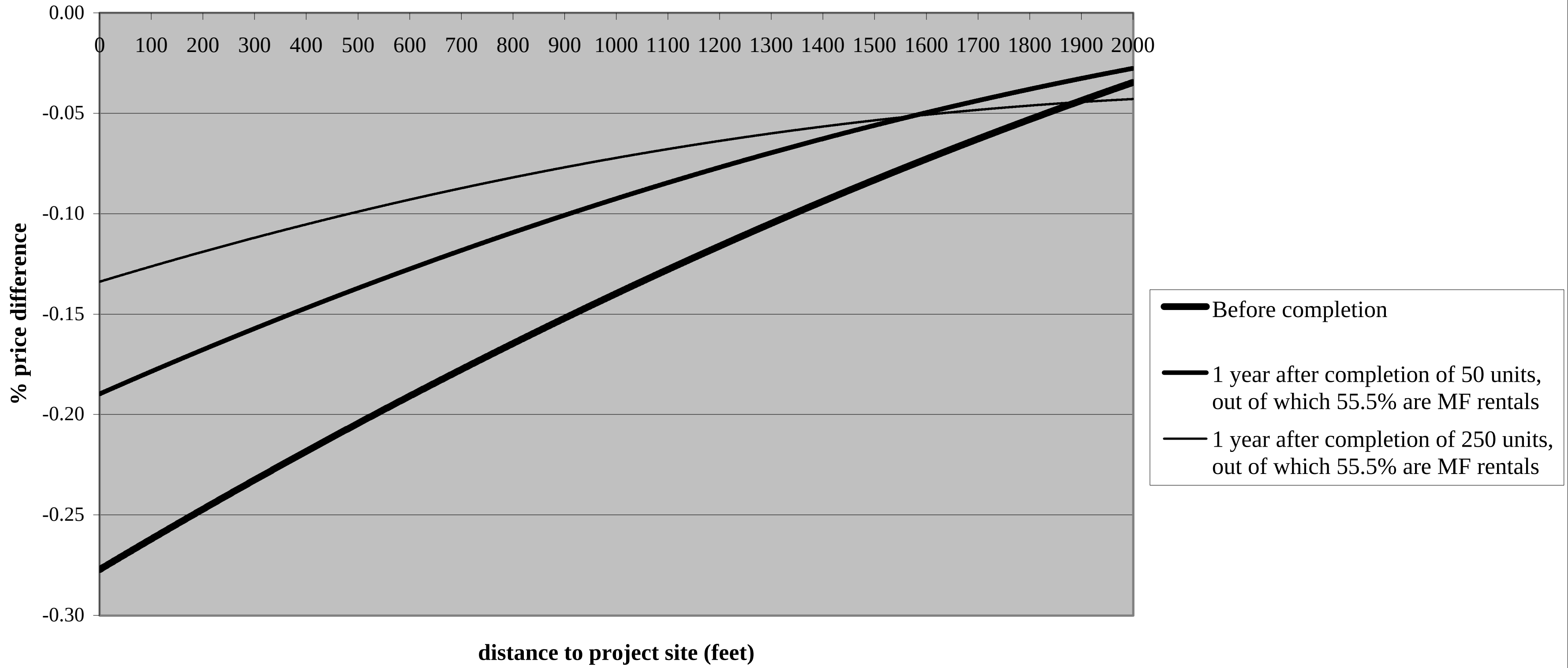
Figure 2

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 Time Since Completion



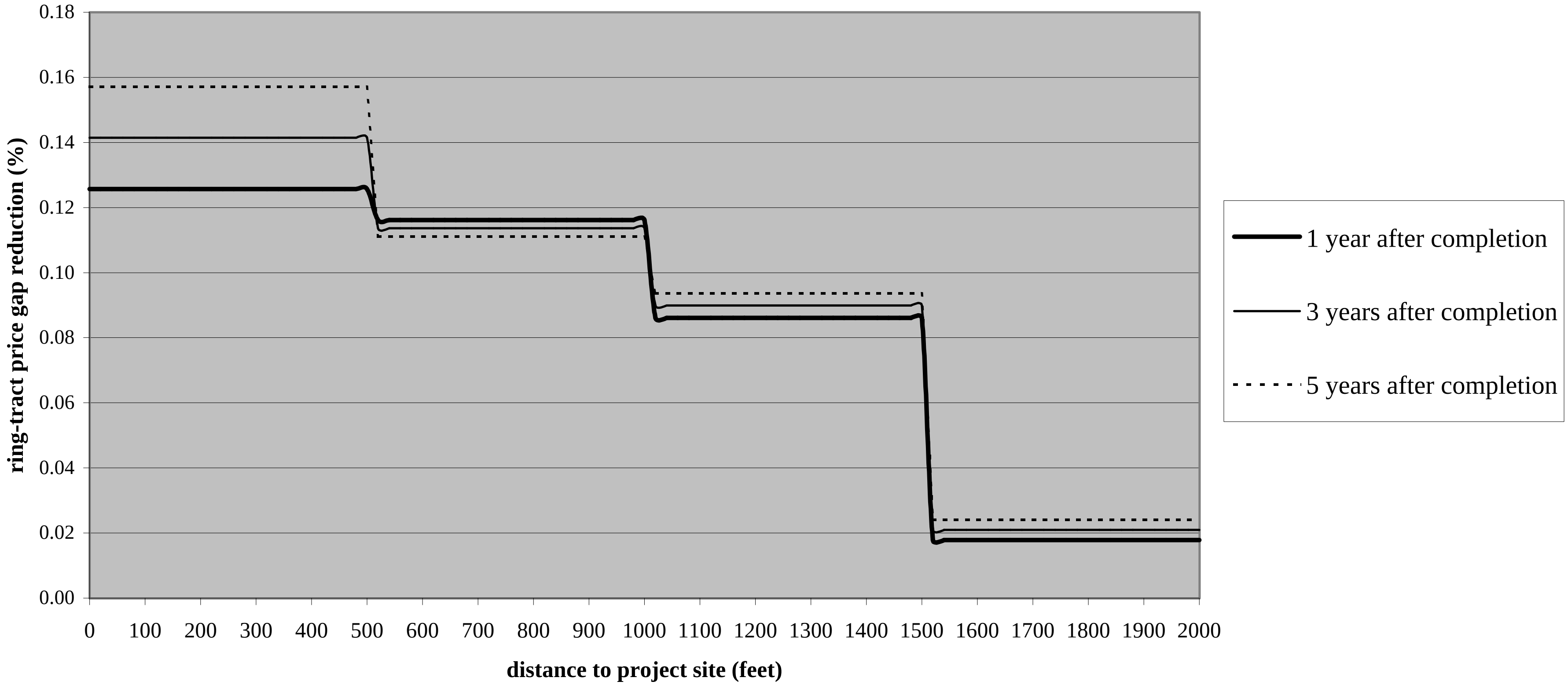
Note :The "average" project is defined as the project in the vecinity of the average sale in a 2000 foot ring. Thus, the percentage price gap before its completion is a weighted average of the (average) price gaps for the 6 ring types, with weights given by the number of sales in each ring type; and 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 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 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 4
Impact 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.

APPENDIX
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	-0.1745 ***
	(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)
Ring variables for Ten Year Plan housing rehabilitation programs	
In Ring	-0.0411 ***
	(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	-0.0030
	(0.0042)
Post Ring	0.0166 ***
	(0.0034)
Number of units at the time of sale	1.3E-05 **
	(5.7E-06)

Complete Regression Results for Baseline Model (continued)

Characteristics of properties sold		
Vandalized	-0.1285	***
	(0.0318)	
Other abandoned	-0.0827	***
	(0.0173)	
Odd shape	0.0149	***
	(0.0024)	
Garage	0.0440	***
	(0.0018)	
Extension	0.0472	***
	(0.0024)	
Corner	0.0281	***
	(0.0026)	
Major alteration prior to sale	-0.0681	***
	(0.0042)	
Age of unit	-0.0056	***
	(0.0001)	
(Age of unit)2	3.1E-05	***
	(1.2E-06)	
Age of unit missing	-0.0558	***
	(0.0193)	
Log square feet per unit	0.5360	***
	(0.0021)	
Number of buildings on same lot	-0.0139	***
	(0.0050)	
Includes commercial space	0.0358	***
	(0.0051)	
Square feet missing	3.8065	***
	(0.0229)	
Condo and square feet missing	-0.1947	***
	(0.0180)	
Single-family detached	0.0963	***
	(0.0026)	
Two-family home	-0.2656	***
	(0.0025)	
Three-family home	-0.4854	***
	(0.0033)	
Four-family home	-0.6378	***
	(0.0051)	
Five/six-family home	-0.9658	***
	(0.0053)	
More than six families, no elevator	-1.3934	***
	(0.0052)	
Walkup, units not specified	-1.1023	***
	(0.0061)	
Elevator apartment building, cooperatives	-1.3134	***
	(0.0158)	
Elevator apartment building, not cooperatives	-1.4028	***
	(0.0078)	
Loft building	-0.6485	***
	(0.0232)	
Condominium, single-family attached	0.0180	
	(0.0226)	
Condominium, walk-up apartments	-0.2009	***
	(0.0205)	

Complete Regression Results for Baseline Model (continued)

Condominium, elevator building	-0.4129 ***
	(0.0204)
Condominium, miscellaneous	-0.3681 ***
	(0.0213)
Multi-use, single family with store	-0.0799 ***
	(0.0098)
Multi-use, two-family with store	-0.4825 ***
	(0.0079)
Multi-use, three-family with store	-0.7154 ***
	(0.0120)
Multi-use, four or more family with store	-0.8799 ***
	(0.0087)
N	293,786
R^2	0.8575

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.