**Low Income Housing Tax Credit Housing Developments
And Property Values**

By

Richard K. Green, Stephen Malpezzi and Kiat-Ying Seah

June 14, 2002

The Center for Urban Land Economics Research
The University of Wisconsin
975 University Avenue
Madison, WI 53706-1323
rgreen@bus.wisc.edu
smalpezzi@bus.wisc.edu
www.bus.wisc.edu/realestate

Richard K. Green is professor and Wangard Faculty Scholar in the Department of Real Estate and Urban Land Economics of the University of Wisconsin-Madison. Stephen Malpezzi is associate professor and Wangard Faculty Scholar in the Department of Real Estate and Urban Land Economics, and an associate member of the Department of Urban and Regional Planning. Kiat-Ying Seah is a Ph.D. student in the Department of Real Estate and Urban Land Economics.

This research was funded by the Wisconsin Housing and Economic Development Authority. We thank David Ginger, Mary Zins and Chris Laurent for their contributions to this research. We also think Patty Stuard of the Multiple Listing Service of South Central Wisconsin and Peter Shuttleworth of the Metropolitan Multiple Listring Service for data. However, opinions in this paper are those of the authors, and emphatically do not reflect the views of any other persons or institutions.

Comments and criticisms are particularly welcome.

EXECUTIVE SUMMARY

- Few causes will mobilize American citizens, at least the 68 percent who own their homes, faster or more effectively than a perceived threat to the value of their property. It is common for at least some neighbors to object to low income housing developments, whether traditional public housing, or privately (for-profit or nonprofit) developed housing under the Section 42 Low Income Housing Tax Credit (LIHTC) program. This phenomenon is not limited to LIHTC developments, of course; for example, waste disposal facilities, power lines, community care facilities, and even churches are among nonresidential uses that at least some homeowners have objected to in recent times, giving rise to the well-known rallying cry, "Not In My Backyard."

- The Low Income Housing Tax Credit was originated in conjunction with the Tax Reform Act of 1986 (TRA 86) to provide incentives for private sector production of low-to-moderate income housing. The credits provide a mechanism for funding a wide range of developments including new construction, substantial rehabilitation, moderate rehabilitation, acquisition, and repair by existing owners. Over the initial three years of the program, about $6 billion worth of funding, aiding 300,000 units of low-to-moderate income housing, was made available. Program activity then increased, as the non-subsidized multifamily market declined. Lately tax credit units have comprised about 40-50 percent of total multifamily construction.

- Many papers have studied the localized effects of housing externalities, whether negative "bads" like environmental problems, traffic congestion, or nonconforming uses; or positive "goods" like high-performing schools or other amenities. The question before us is whether Section 42 developments actually create "bads" that translate into lower property values. A review of eight past studies on the issue of the effect of low-income housing on property values generally does not support the proposition that such housing diminished property values. Often it is the case that low-income housing developments cause surrounding property values to *increase*. Interestingly enough, past authors have generally found that such developments have a more positive impact in higher income areas. It seems to be the case that it is only when low-income housing developments are located in areas that already have concentrated poverty that they have a negative impact on property values.

- Our method for examining the influence of Section 42 developments on property values is to use repeat sales techniques. Specifically, we gather data on properties that have sold more than once in Madison and Milwaukee Metropolitan areas, and determine whether differences in appreciation can be explained by proximity to Section 42 developments.

- The repeat sales technique is a statistically correct manifestation of what appraisers call a "paired-sales" technique. Because each observation in a repeat sales data set follows the same house across time, it controls for many things, including things that are easy to measure, such as size and number of bathrooms, and things that are difficult to measure, including design and "curb appeal." In our view, this leads the repeat sales setup to be superior to the alternative "hedonic" design. One deficiency with repeat sales is that it can only explain price changes, rather than price levels. But this is not an issue in our context, because we are examining how Section 42 development influence changes in house prices.

- We specified a number of mechanisms by which Section 42 developments might influence surrounding property values. We performed regressions that included linear, quadratic (i.e., squared) and gravity measures of distance to determine the influence of the developments on property values. We also ran regressions that included neighborhood controls, such as poverty rates, education levels, marriage rates, income levels, and age distribution of the population.

- Our data set on property values for Madison was based on every sale in the Multiple Listing Service of South Central Wisconsin database over the period 1991-2000. This gave us 3193 repeat sales observations to work with. We have also obtained the MetroMLS's database of property sales for the Metropolitan Milwaukee area (Waukesha, Washington, Ozaukee and Milwaukee Counties) and used that data to look at the impact of the developments in those areas. We were able to generate 2258 observations for Milwaukee County, 367 for Waukesha County, and 425 for Ozaukee County.

- Our dataset on the size and location of Section 42 developments was provided by the Wisconsin Housing and Economic Development Authority, and contains the universe of such developments in Wisconsin.

- To measure proximity of Section 42 developments to each single-family house, we used a Euclidian distance measure, which we calculated based upon the latitudes and longitudes of the developments and the houses. We also develop a "gravity measure" that combines the effects of magnitudes and distances on values.

- To this point, our results for Wisconsin are generally consistent with results in other studies: we have not been able to find evidence that Section 42 developments cause property values to deteriorate. The exception is Milwaukee County, where properties that are distant from the developments seem to appreciate more rapidly, although the magnitude of the effect is small. We have found no evidence of an impact in Waukesha and Ozaukee, and find evidence that properties in Madison near Section 42 developments appreciate *more* rapidly.

- In our view, the key policy implication of our results is that Section 42 developments are best placed in relatively affluent communities, where there is no evidence that that developments cause property values to deteriorate. This phenomenon is consistent with findings from past literature.

Low Income Housing Tax Credit Housing Developments
And Property Values

Introduction

Few causes will mobilize American citizens, at least the 68 percent who own their homes, faster or more effectively than a perceived threat to the value of their property. It is common for at least some neighbors to object to low income housing developments, whether traditional public housing, or privately (for-profit or nonprofit) developed housing under the Section 42 Low Income Housing Tax Credit (LIHTC) program.[1] This phenomenon is not limited to LIHTC developments, of course; for example, waste disposal facilities, power lines, community care facilities, and even churches are among nonresidential uses that at least some homeowners have objected to in recent times, giving rise to the well-known rallying cry, "Not In My Backyard."[2] Even during the recent California electricity crisis, neighborhood associations continued to enforce prohibitions against air-drying clothes outside, citing potential reductions in housing values.

But are these perceptions of lowered property values correct? An emerging literature (to be surveyed below) suggests that quite a few NIMBY concerns are unfounded. As Fischel (2000) has elegantly pointed out, even if it is unlikely that a given activity actually reduces values, merely a low probability is sufficient to engender opposition, given the stakes involved for an individual homeowner. On the one hand, this suggests that if LIHTC developments do not lower nearby property values, solid and convincing evidence will be required in order to assuage NIMBY fears. On the other hand, if it turns out that LIHTC developments do lower neighbors' property values significantly, knowledge of such potential losses could be used to revisit development design so as to remedy such problems and reduce opposition to developments.

[1] Add some references, including newspaper articles.
[2] For example, Farber (1986), Michaels and Smith (1990), Hughes and Sirmans (1992), Thibodeau (1990).

The Low Income Housing Tax Credit Program

The Low Income Housing Tax Credit was originated in conjunction with the Tax Reform Act of 1986 (TRA 86) to provide incentives for private sector production of low-to-moderate housing. The credits provide a mechanism for funding a wide range of developments including new construction, substantial rehabilitation, moderate rehabilitation, acquisition, and repair by existing owners. Over the initial three years of the program, about $6 billion worth of funding, aiding 300,000 units of low-to-moderate income housing, was made available. Program activity then increased, as the non-subsidized multifamily market declined. Lately tax credit units have comprised about 40-50 percent of total multifamily construction.

The Low Income Housing Tax Credit provides up to 70 percent[3] of the cost of new construction or 30 percent of the cost of acquisition of existing low income housing in return for limits on rents charged. The credit is paid as an annuity over ten years. The credits are allocated over a ten-year period based on the "Applicable Federal Rate" (AFR). Nominally the value of the credit is 9 percent annually for the 70 percent credit and 4 percent annually for the 30 percent credit. For acquisition of existing rental housing, the applicable credit is also 4 percent.

The developer must decide between two options for the unit. Either 20 percent of available rental units must be rented to households with income less than 50 percent of the county median income (adjusted for family size), or 40 percent of the units must be set aside for households with income less than 60 percent of the county median income. (The rent can be adjusted in future years as median incomes change). The maximum gross rent, including utilities, paid by households in qualifying units may not exceed 30 percent of maximum qualifying income. The federal program mandates a fifteen-year period for maintaining the unit as a low-income unit. If the rent restrictions are not followed, there are provisions for recapturing the tax credits used. For more on the mechanics of this program, see Guggenheim (1989).

[3] When the credits are "sold" in a secondary market, however, they generally sell for between 65 and 70 percent of face value.

In Wisconsin, the LIHTC program is administered by the Wisconsin Housing and Economic Development Authority (WHEDA). WHEDA sets local program rules, in line with Congressional and Treasury rules, collects and evaluates proposals for developments, and monitors development compliance and effectiveness.

Previous Research on Negative Housing Externalities

Many papers have studied the localized effects of housing externalities, whether negative "bads" such as environmental problems, traffic congestion, or nonconforming uses; or positive "goods" such as high-performing schools or other amenities.[4] In this brief review, we focus on studies of one kind of low-income housing development or another.

All such studies revolve around some kind of comparison of housing prices near and far away from housing developments, controlling for other locational features. The major methodological differences among studies revolve around how these comparisons are undertaken. More specifically, (1) how are two sets of "comparable" housing units with and without the "treatment effect"[5] of developments defined; and (2) how are prices compared?

Generally, there are two main methods of measuring the "treatment" to be found in this literature. First, and simplest, the analyst can construct some kind of price index, either in levels (dollar amounts) or changes (percentage growth in prices) for a "treatment group" of neighborhoods or units with developments, and a "control group" of units or neighborhoods without. The great difficulty in doing such a study well is in finding otherwise nearly-identical units and neighborhoods to compare, that differ more-or-less only in whether developments exist nearby.[6] The second method is to combine all units or neighborhoods in the study together, but rather than separating them into two distinct groups, study the effect of some continuous measure of distance to developments, usually using regression analysis to obtain a *coefficient* that quantifies the effect of distance from a development on some price measure. The regression also allows us to measure a

[4] See Follain and Malpezzi (1981) and Jud (1981) for "goods," and Gamble and Downing (1982), Hughes and Sirmans (1992) and Thibodeau (1990) for "bads." See Palmquist (1992) and Bartik (1986) more generally.
[5] In statistical jargon, the "treatment" refers to the phenomenon under study (here, being near public housing) and the "treatment group" is comprised of those nearby projects. The "control group" consists of otherwise similar units or neighborhoods farther away from the influence of projects.
[6] Part of that judgment is determining what exactly "nearby" means.

standard error around a coefficient. These standard errors allow us to determine the potential range of impacts within which we can have a certain degree of confidence. In another context, the standard errors in survey data underlie the “sampling error” referred to in media reports. When, for example, the media report that the president has a 65 percent approval rating with a sampling error of plus of minus three percent, the three percent arises from the standard error of the underlying survey. The standard error also allows us to determine whether the price effect measured by the coefficient is different from zero, or whether it is simply the product of randomness.

How are these house prices measured in these impact studies? Generally, there are three main methods of price construction found in this literature. The first is to work with some kind of average or median housing price for each group, treatment or control. These prices may be considered in levels or changes, but the problem comes in attributing any observed differences to true differences in price, as opposed to some unobserved difference in the quantity or quality of housing services obtained from typical units in one group, as opposed to the other.[7]

The second method is to regress sales “prices” or other measures of market value against characteristics of the units, such as the size of the unit, various quality variables, and neighborhood variables, including distance of the unit from the developments. These so-called “hedonic price indexes” are familiar to housing economists as well as real estate appraisers, although appraisers usually use another name. In effect, hedonic models are a statistical version of the comparable-sales approach to valuation.[8] Hedonic models work well when carefully implemented, and they can be constructed to work in either levels or changes; one problem with them, especially relevant to the present study, is that to do them well requires a lot of data on unit and neighborhood characteristics and location, which are often difficult to obtain.

[7] More detailed explanations of the problems involved in measuring housing prices, and the methods briefly described here to attach these problems, can be found in Green and Malpezzi (forthcoming).
[8] See Green and Malpezzi (2001) and Malpezzi, Ozanne and Thibodeau (1980) for more detailed discussion of these models.

The third method is to measure price changes for identical units by examining the price changes of units that have sold twice, or more often, during the study period. Because these are in effect comparisons of the same units, detailed data on unit and neighborhood characteristics are not needed (other than, in the case of our model below, distance to developments). Of course these so-called "repeat sales indexes" rely on several other assumptions, notably that there have been no major changes or renovations to units during the study period; and that there has been no significant physical depreciation or major change in neighborhood conditions. These are obviously strong assumptions, and we will return to them in our detailed discussion of our own repeat sales models. It should also be noted that repeat sales indexes only tell us about price changes (appreciation rates). They cannot, on their own, tell us about the level (dollar amount) of prices. Repeat sales models have been used in several influential previous studies of the effects of housing developments on nearby units, and we will make use of them in our own study.

We will return to the repeat sales model and other details of our own study later. Next we will briefly review previous studies that focus on one kind of public or low-income housing or another.[9]

In the discussions below, we will be referring to statistical significance. What we mean by significance is whether it is unlikely that a relationship that we observe is random. When a relationship is statistically significant, it is highly unlikely that it is random.

But significance is distinct from importance. We may observe in data a consistent, but small, relationship between two variables. When we work with large data sets, we will often observe statistically significant and economically unimportant relationships.

[9] We are of course aware that traditional public housing differs greatly from LIHTC projects. That is one of the motivations for the present study. Still, the general setup of the problem is the same. Also, since most observers would agree that the "negative externalities" of LIHTC units are less than those from public housing, a finding that public housing's negative externalities were small or insignificant would tend to suggest that LIHTC units would have little effect on their neighborhood. One counterargument might be if public housing units were typically located in "bad" neighborhoods with already-low prices, while LIHTC units were located in "better" neighborhoods.

One of the first, and one of the most often cited, studies of the effects of public housing developments on nearby private units is Hugh Nourse's (1963) study of St. Louis. Interestingly, the point of departure for Nourse's article was an investigation of claims by Congressional sponsors that public housing *raised*, rather than lowered, nearby property values. Nourse applied the then-new method of repeat sales to construct price indexes for each of three neighborhoods containing eight public housing developments, and to then construct price indexes for three control neighborhoods that were nearby and similar in housing and demographic characteristics. His data were from 1937 to 1959. Nourse found that, in two of his paired comparisons, the trends in prices between treatment and control neighborhoods were roughly the same. In the third paired comparison, the trend in prices seemed higher in the treatment neighborhood, i.e. the neighborhood with public housing; but the difference in trend was not statistically significant. Nourse examined each of the annual differences between price changes in the treatment neighborhood and its control neighborhood, using a procedure called a t-test for the significance of the differences between the two. In only one case in 65 could Nourse find a statistical difference between neighborhoods with public housing and neighborhoods that did not have such housing. Given the way we measure statistical significance[10], we would expect to see statistical differences in randomly generated data one time in 20, simply as a function of chance. Nourse thus concluded that his data provided no evidence that neighborhoods containing public housing appreciated at a higher or lower rate than neighborhoods without. We would expect Section 42 developments to be more beneficial to neighborhoods than public housing, because the market gives private developers better incentives to manage property than public-sector developers, who face no such market discipline.

Another early study that is often cited is Robert Schafer's (1972) study of Below Market Interest Rate (BMIR) housing in Los Angeles. Schafer compared two comparable neighborhoods, one with BMIR housing, one without, using data from 1958 to 1970. His methodology was essentially similar to Nourse's. One point of interest for our own study is that BMIR housing might be considered closer to LIHTC housing than traditional public housing. The earlier BMIR and the current LIHTC programs certainly differ in many respects, not least of which is their financing mechanism – BMIR

[10] We generaaly accept thatbgroups are statistically difference when we can do so with 95 percent confidence.

housing's subsidy consisted mainly in the program's concessionary interest rates, whereas the LIHTC program relies on a more complicated system based on the "sale" of tax credits. But both programs essentially subsidize privately developed and owned rental real estate targeted to lower middle income households. In the event, the area with the BMIR housing actually exhibited slightly higher appreciation than the control group, although the differences were again not statistically significant. So once again the analysis failed to support the hypothesis that low-income housing developments reduced nearby property values.

A third early study by Joseph DeSalvo (1974) found essentially similar results, examining New York City's Mitchell-Lama program, which subsidizes (initially lower) middle income private apartments. Assessed values near the developments appreciated faster than assessed values of control areas. The fact that this study was forced to rely on assessed values, rather than market transactions, is one possible shortcoming.

A (1985) study by Donald Guy, John Hysom and Stephen Ruth had somewhat different findings. Guy *et al*. examine housing located near two BMIR developments in newly constructed middle-income housing in Fairfax County, Virginia, using sales data from 1972 through 1980. The authors differed from the previously cited studies by relying on the hedonic regression approach, regressing sales prices against characteristics of the units, including distance to the nearest BMIR development. Their list of independent variables is a short one, but since they are limiting themselves to a fairly homogenous group of town homes in several adjacent developments, their specification seems reasonable.[11] They found that sales prices rose about $1.57 for every additional foot of distance away from the development.

A more recent study was undertaken by Chang-Moo Lee, Dennis Culhane and Susan Wachter (1999). Unlike previous studies, Lee et al. examined several different federally assisted housing programs and designs, denoted (1) high rise public housing, (2) large scale public housing, (3) homeownership public housing, (4) public housing built after 1980. These categories were not all mutually exclusive. Dummy variables were included for whether a given unit was within either a 1/8- or 1/4-mile radius of a development. Sales prices from 1989 through 1991 were the dependent variable, and other variables controlled for area demographic, housing, and amenity variables. Results

[11] See Butler (1982) and Ozanne and Malpezzi (1985) for discussion of the importance of a correct hedonic specification.

show that public housing developments exert a modest negative impact on property values. Scattered-site public housing and units rented with Section 8 certificates and vouchers have slight negative impacts. Federal Housing Administration-assisted units, public housing homeownership program units, and Section 8 New Construction and Rehabilitation units have modest positive impacts. Low-Income Housing Tax Credit sites have a slight negative effect in two of their four models, and no effect in their other two. Given that they had a sample size of over 18,000 observations, it is actually surprising that they could run models where the coefficients on LITC developments were not significant. When Lee et al. got significant coefficients, they were still trivially small. Results suggest that homeownership programs and new construction/rehabilitation programs have a more positive impact on property values.

Another study was carried out by George Galster, Peter Tatian, and Robin Smith (1999). Galster et.al examined the price effects on neighboring single family homes of Section 8 developments in Baltimore County, Maryland. Interestingly, they found that the effects of a development on neighboring properties were related to the type of neighborhood. In higher-valued, faster-appreciating, predominantly white tracts, developments actually were associated with higher prices in nearby locations. On the other hand, in lower valued tracts experiencing real declines in values, Section 8 developments were associated with adverse impacts on prices. These adverse impacts were highly localized, beginning to fall off significantly after 500 feet and virtually disappearing within 2,000 feet. Galster et al. also conducted focus groups with nearby home owners that suggested that the kind of effect the development had was determined at least partly by the management of the development.

Santiago, Galster and Tatian (2001) examined the effect on nearby properties of rehabilitation developments in Denver. Existing dilapidated properties were acquired by the Public Housing Authority, rehabilitated, and occupied by subsidized housing tenants. Using hedonic methods to control for characteristics of the neighborhood as well as the unit, Santiago et al. found that proximity to a subsidized housing site generally had an independent and positive effect on single-family home sales prices. There were exceptions; in neighborhoods that had high percentages of black residents, proximity to the sites were associated with lower growth in housing prices. Santiago *et al*. suggest there exists a threshold within “vulnerable” neighborhoods “whereby any potential gains associated with rehabilitating existing units are offset by the increased concentration of poor residents." Another study that suggests the impacts of developments on property values varies by the type of development was carried out by Goetz, Lam, and Heitlinger (1997). In their study of subsidized multifamily housing in Minneapolis, Goetz et al. found that units operated by non-profit community development corporations had slight positive impacts on property values, while large public housing developments and older Section 8 new construction developments had slightly negative effects on nearby property values. Briggs and Darden (1999) studied effects on property values on the introduction of scattered site public housing in Younkers, New York. A related issue, that the introduction of assisted housing leads to "tipping" and a high degree of racial turnover in local neighborhoods was studied by Freeman and Rohe (2000). Freeman and Rohe found that assisted housing had no such impact.

Problems shared by most or all of these studies include the following. First, many of the studies are based on limited numbers of observations, which reduces the power of the test, which means that it is difficult to distinguish between truly significant and insignificant results. The precision of our estimates and the "power" of our test generally rises as we add data, up to a point; many of the early studies, especially Nourse's and Schafer's, may suffer from having a modest number of sales to study.

Secondly, the nature of treatment-control is often problematic. In studies such as Nourse's, where the analyst chooses a treatment area and control area, there is art as well as science in matching such areas up; and of course the discrete nature of the categorization can cause problems. Consider two neighborhoods, one treatment and one control. Suppose that there are some units as far as half a mile from the development in the treatment neighborhood; suppose that there are some units just over half a mile away in the control neighborhood. The former units are lumped in with units literally on the doorstep of the development; the latter are lumped in with units perhaps a mile away. How and where do we draw this line?

On the other hand, models that include linear distance to the development have their own problems. Most such studies simply enter a linear distance. The dollar effect[12] of moving out from 50 feet away to 51 feet is constrained to be the same as that from

[12] "Dollar effect" assuming a linear hedonic, as in Guy et al. If a semilogrithmic specification is used, the effect will be approximately a percentage change effect. See Halvorsen and Pollakowski (1981), and Malpezzi, Ozanne and Thibodeau (1980).

moving 5000 feet away to 5001. Consider the fact that any such effects might in reality be nonlinear, e.g. the effect of moving out a short distance might be great when close in but small when farther away. Furthermore, consider that the analyst must also worry about other locational effects. For example, the "standard urban models" of Alonso, Muth and Mills, and more recent variants such as Cappoza and Helsley, all predict that percentage appreciation in housing prices will be greater as we move farther out from the center of the city.[13] If some of the control units are farther out from the center than corresponding treatment units, we may confuse this pure locational effect (slower rates of appreciation in the center of the city) with a negative externality. Similarly, if prices appreciate differently in high and low income areas, but developments are located in low income areas (perhaps because approvals are easier to obtain, or perhaps because LIHTC developers are particularly focused on lower land costs), then the location of the development is "endogenous," i.e. is determined partly by the very thing we want to study (price differences). Thus it is important to control for neighborhood and location attributes as well as the housing unit.

[13] In brief, this is because as long as transportation costs remain stable, as a city grows, rents and prices for a similar housing unit at different locations will grow by a similar dollar amount; but a given dollar increase translates into a larger percentage increase on the fringe of the city, where initial prices are lower due to lower land costs.

A Simple Model for Measuring External Effects of LIHTC Developments

In this section we describe the model we will use. The first part of the section describes repeat sales methods in some detail.[14] The second part elaborates on how we incorporate location vis a vis developments, and some other details of our particular variant of the model.

Repeat Sales indexes are estimated by analyzing data where all units have sold at least *twice*. Such data allow us to annualize the percentage growth in sales prices over time.[15] These are time series indexes in their purest form. They do not provide information on the value of individual house characteristics or on price levels. They have the advantage of being based on actual transactions prices, and they reduce mis-measurement arising from having an insufficient number of characteristics for explaining house price. However, units that sell are not necessarily representative of all units. Sometimes it's difficult to tell whether a unit retains the same characteristics across time. For example, remodeling could cause a house's characteristics to change.

The best way to understand how repeat sales indexes work is by example. Figure 1 shows a graph of seventeen properties which sold twice in the Shorewood Hills neighborhood of Madison, Wisconsin in the late 80s and early 90s. Each property is numbered with 1 to 17, and each property appears twice. The vertical axis is the logarithm of the selling price of the unit.

We can think of the repeat sales estimator as an attempt to measure the average slope of the lines in Figure 1, year by year. In a classic paper, Bailey, Muth and Nourse (1963) illustrated how to compute this using regression methods and a larger sample. The method was later refined by Case and Shiller (1987), who took steps toward mitigating the problems arising from the fact that as distance between sales increases, so too does the variability of price appreciation across houses.

Consider a house "A" that sells in periods 2 and 4 (period 0 is the base year). The physical characteristics of the house have likely not changed much over this time period; any change in price represents a change in land value and the change in cost of the construction labor and materials that would be needed to replace the house. Because

[14] This discussion draws heavily on Green and Malpezzi (forthcoming).
[15] Actually, as we will see later in this section, with large samples regression techniques are used, but it amounts to the same thing.

labor and materials costs are homogenous within a metropolitan area, differences in house price changes are a function of differences in changes in land values, which are in turn a function of how the market values neighborhood amenities.

Table 1 illustrates this with the sample data. The first two columns of Table 1 contain the first and second sales prices from our repeat sales sample. The third column is simply the difference in the natural logarithm of these prices (which is very similar to the percentage change in price). The next two columns record the dates.

Let us for simplicity consider time to be represented in years. 1986 is the base year. Then let us have zero-one ("dummy") variables represent 1987, 1988 and so on through 1992 (i.e. Notice the coefficient for, say β_{1988}, is negative if the unit is first sold in period 2 (i.e., 1988) and positive if it is last sold in period 2, but the magnitude of β_{1988} stays the same in either case. Thus we can simply construct a dummy variable which imposes this restriction upon the estimation. That is, we construct a dummy variable which takes on the value -1 if it is the first sale, +1 if it is the second sale, and 0 if no transaction took place during the period. Then we simply regress the difference in log prices (or, roughly, the percentage change in prices) against this matrix of rather unusual dummy variables.[16] Then the coefficients of each of these dummies yield an estimate of the changing price between the base period (here in 1986) and succeeding periods.

A key point about interpretation: a reasonably close estimate of the annual price change can be computed by subtracting one year's coefficient from the next period.[17]

Another possible refinement is to consider the fact that the variance of these housing prices will generally increase over time. In today's econometric parlance, such prices are not *stationary*. Case and Shiller (1987) suggest a refinement to the Bailey, Muth and Norse model to mitigate such problems.

[16] Econometricians will notice that we suppress the constant term in the regression because it drops out in the subtraction of the two characteristic vectors X.

[17] If we wish to interpret these as percentages, we should make the Halvorsen and Palmquist (1980) correction discussed above.

Repeat sales indexes are currently much discussed in the literature because they have the following advantages:

1. No information is required on the characteristics of the unit (other than that an individual unit has not significantly changed its characteristics between sales).

2. The method can be used on data sets which are potentially widely available and collected in a timely manner, with great geographic detail, but do not have detailed housing characteristics. For example, Case and Shiller's original work used data collected by the Society of Real Estate Appraisers. Much of the current research in this area has been undertaken by Fannie Mae and Freddie Mac, who have the advantage of large data sets with price data from a huge number of transactions nationwide.

The repeat sales method has a number of shortcomings as well. For example:

1. Even at its best, the method only yields estimates of price *changes*. No information on price levels, or place to place price index, is derivable from the repeat sales method. Of course, the repeat sales method can be combined with some other method; i.e., to update earlier estimates of price levels constructed using some other method.

2. Because only a few units transact twice over a given time period, the repeat sales method utilizes only a fraction of potential information on the housing market.

3. Units that transact frequently may be systematically different from units representative of the stock as a whole (Gatzlaff and Haurin, 1993). How big this problem is depends partly on the purpose of the index. It certainly would be less of a problem if the purpose of the index was to track the prices of units that transact.

4. The method implicitly assumes that there is no change in the quality or quantity of housing services produced by the unit between periods. Of course, this assumption is always violated to some degree. Those who construct these indexes spend a lot of time weeding out units which have been upgraded using, for example, collateral data on building permits, or the limited structural information that may exist in the data set in use.

5. The method also assumes that the coefficients on the underlying hedonic model remain constant: this is what allows the house characteristics to drop out of the model. But this assumption may also be questioned. For example, as families have gotten smaller, so too has the value of bedrooms, holding all else equal. Thus the hedonic coefficient for bedrooms in 1990 was almost certainly different from the coefficient in 1960, regardless of the particular market (see Gatzlaff, Green and Ling (1997) for a specific case).

Now that we have set the stage with a discussion of repeat sales models in general, let us discuss our particular specification. The first thing we note is that our data series are relatively short in length: ten years in the case of Madison, and five years in the case of Metropolitan Milwaukee. This means that it is unlikely that the relative value of housing attributes such as bedrooms have changed much, and that most of the differences in changes in property values across places arises from differences in land values. We therefore can be confident that only differences in major changes in neighborhood characteristics will lead to differences in changes in property values. An example of a major change might be the introduction of a Section 42 development.

We also note that urban economic theory and empirical observation tells us that land in the center of cities appreciates less rapidly than land on the periphery; we therefore must control for location relative to the central city if we wish to find the determinants of differences in appreciation rates.

Because properties that record very many sales are unusual and may be reflecting something other than normal transactions, we omit any properties that record more than four sales in five years. Properties that sell twice in one year are also omitted.

Our matrix of sales dates is comprised of years. Finer breakdowns are not possible because the number of observations in each date cell becomes sparse if we use quarterly or monthly dates as the columns of D. But a year is a long time; consider one property that sells in January of 1990 and later in December of 1991; we record the sale as one year apart, while the true distance is closer to two years. A pair of sales in December 1990 and January 1991 are also recorded as a year apart, even though they're roughly a month or two apart. To partially correct for this, we add a continuous variable m1 for the number of the month of the first sale (m1 = 1 if sale 1 is in January, m1 = 2 if sale 1 is in February, and so on), and an analogous variable m2 for the month of the second sale. This imposes a restriction that the percentage premium or discount over the average price change for that year is the same as we move a month forward or back a month, i.e. there are no seasonal effects in house prices.

Finally, so far our discussion assumes that the relevant measure of proximity to a development is the linear distance to the nearest development. Many prior studies, such as DeSalvo (1997), make this reasonable assumption. But it is certainly possible that the relationship is more complex. First of all, the relationship between distance and price could be nonlinear. It is at least as reasonable to assume that the effect of distance is stronger as we observe close in locations; moving from 100 feet away from a development to 200 feet away might have a different effect than moving from 5,000 feet away to 5,100. Second, distance to the nearest development fails to capture whether there are yet other developments nearby. Third, our simple distance variable does not account for the size of the project.

All three issues can be addressed rather neatly with the so-called "gravity" measure of distance.[18] This draws on the well-known Newton's law of gravitation and constructs a measure of "gravity" that is a function of size and squared distance: this specification allow large projects to have a larger effects than small projects, and for distance to become less important is it gets larger.

Data

For Madison, we obtained every sale of a single-family house recorded between 1990 and March 2001 from the Realtors Association of South Central Wisconsin. From these we culled a sample of repeat sales, which gave us 3138 observations. We also obtained data from the Wisconsin Housing and Economic Development Authority on Section 42 Low Income Housing Tax Credit buildings in Madison: we have a sample of 125 buildings.

The Department of Planning of the city of Madison provided us with a data set that matched tax key identification numbers for each parcel in the city to locations for each parcel as represented by latitude and longitude. We then measured the Euclidean distance from each repeat sale observation to (1) the state capital (to capture the "urban economics" effect described above) and to (2) each low-income housing tax credit development. After we performed step (2), we determined the minimum distance of any particular development to each observation, and use that minimum as our distance measure. We also constructed a gravity measure that took into account development size, the number of developments near each house in the data set, and squared distance.

[18] See Lowry (1964) for the classic formulation of this model, and see Isard (1999) for a discussion of the analogy between this model and Newton's use of it in physics.

For the Milwaukee Metropolitan area, we obtained every sale of a single-family house recorded between 1995 and March 2001 from the Metropolitan Multiple Listing Service[19]. From these we again culled a sample of repeat sales, which gave us 2258 observations for Milwaukee County, 367 observations for Waukesha County, and 425 observations for Ozaukee County.[20]

We should note that while Milwaukee, Waukesha and Ozaukee Counties lie within the same metropolitan area, the suburban counties are quite different demographically and economically from the central city county. Median Household Income in Milwaukee County in 1997—the most recent available year—was approximately $37,000, while in both Waukesha and Ozaukee Counties it was approximately $62,000. The poverty rate in Milwaukee County that year was 16.5 percent, while in the two suburban counties it was around three percent. Finally, the 2000 census reported that 24.6 percent of Milwaukee County's population was African-American, while African-Americans made up less than one percent of Waukesha and Ozaukee Counties populations.[21]

Although we think our repeat sales methodology allows us to control for neighborhood characteristics, we also ran regressions that include specific controls for neighborhood poverty rate, income, marital status, percentage African-American, percentage married-couple, and percentage of households headed by women. We obtained these data from the 1990 census, and neighborhoods are defined by zip codes. As we shall see below, these controls had little influence on our overall results.

Results

We report our results for Madison in Table 2. We have to this point specified five models: one that looks at the influence of linear distance on percentage change in price; one that looks at linear distance and linear distance squared, one that looks at the interaction of distance and year in which sales take place, one that uses a gravity measure, and one that includes neighborhood controls. The R^2 statistics reported in the table reflect the explanatory power of the variables *beyond* the year-dummy control variables. Note that these generally have small explanatory power.

---

[19] We thank Peter Shuttlesworth and the Metropolitan MLS for these data.

[20] We dropped observations from zip code 53235, because it did not exist in 1990, and was therefore bereft of census data we needed for our analysis.

[21] These data are from http://quickfacts.census.gov/qfd/states/55/55089.html

Our first specification suggests that being proximate to a low-income housing tax credit development does not diminish value—indeed, it appears to enhance value. But this may be a function of the specification. We next move to a specification with a quadratic, which is negative in both the linear term and in the squared term. This means that as one moves further away from a LIHTC development depreciation increases at a rising rate. This result should not be taken very seriously, however, because the coefficients on both the linear and the quadratic term have t-statistics of well under 2: they are not individually different from zero at the 90 percent level of confidence: they are not statistically significant.

For the interactive regression, we test the null hypothesis that all of the coefficients that interact distance with year sold are equal to zero.[22] The F-Statistic of this joint test is .71, which is well below the 90 percent critical value of 17.28—in short, the coefficients on proximity to a Section 42 unit add no explanatory power to changes in value.

The gravity regression gives us a similar result. The null hypothesis that the coefficients on the "gravitational pull" pull is different from zero produces an F-statistic of only 0.47! At the same time, the linear distance coefficient retains its negative sign, meaning again that if anything, the developments enhance value.

Finally, when we include controls for neighborhood poverty rate, income, marital status, percentage African-American, percentage married-couple, and percentage of households headed by women, the coefficient on linear distance between each single family house and Section 42 development is negative, and is even different from zero.

These five specifications leads us to the view that there is no evidence that proximity to low income tax credit developments diminishes value. Indeed, if anything, we find that proximity to such developments might *enhance* property values.

[22] Alone among the Madison regressions, this is not a residual regression: year dummies and interactive terms are included at the same time. This is why the R^2 is much higher in this regression than the others.

In Table 3 we report results for Milwaukee.[23] We get a very different result from Madison: now proximity to a development seems to matter, and seems to have a negative impact on appreciation rates. Table 3 shows that in three out of four regressions, the impact of nearest distance between a development and a repeat sales transaction is significantly different from zero at the 95 percent level of confidence (Regressions 1,2 and 4 have t-statistics that are substantially greater than 2). The gravity measure estimated in regression 2 is also different from zero at the 95 percent level of confidence. The regression with the coefficient that is not significant lacks our most sophisticated measure of the potential impact: the gravity measure. We should note that the magnitude of the impact is not large: a one standard deviation movement in distance away from the project increases the appreciation rate by .5 percent. Moreover, it is possible that the location of developments is correlated with unmeasured neighborhood characteristics that cause properties not to appreciate in value. Still, there is no denying that the Milwaukee result contrasts sharply with the results for Madison.

The Milwaukee result also contrasts with the results for Waukesha and Ozaukee (see Tables 4 and 5), where there is no evidence that the developments have an impact on value. The coefficients on our distance measures are not only not significant, they are extremely close to zero in magnitude. If there are two places where we may say with some confidence that Section 42 developments have no discernable impact on value, these two are they.

These results are consistent with the idea that Section 42 developments are best cited away from concentrations of poverty. At least in Wisconsin, the impact of the developments on surrounding property values in relatively affluent areas seems to range from neutral to positive, while this does not seem to be the case in the state's largest city within which there is a concentration of poverty. These results are also quite consistent with previous literature.

[23] In an earlier version of this paper, the explanatory variable we used for Milwaukee was not distance from the nearest Section 42 building, but rather the number of developments in the census tract. We had only data for the city of Milwaukee, which we obtained from the assessor's web page. The regression set-up was also slightly different from the Madison set-up: with the Madison regressions, the independent variables were explaining the variation in house prices after the "year effect" was removed. For Milwaukee, we reported both year effects and other effects. We found that income was positively associated with value growth, however, we found that there is no statistical evidence that the presence of Section 42 developments has an influence on appreciation rates. (see Appendix Table 1). On the other hand, because data in tracts containing Section 42 developments was so limited, we did not want to place too much weight on this result. Rather, we sought to develop better data that allowed us to use distance measures, and we succeeded.

Conclusions

In this report, we have investigated the impact of Section 42 developments on surrounding property values. Past work has suggested that low-income housing in general, and Section 42 developments in particular, do not generally have a negative influence on surrounding property values. We sought to find whether these results applied to Wisconsin cities.

To this point, we have indeed found that the findings apply to Wisconsin as well. In the cities of Madison and in Waukesha and Ozaukee Counties, we have been able to produce no evidence that Section 42 developments have a negative impact on property values. When we look at Milwaukee County, our story changes—there does indeed seem to be a negative—albeit small--impact on appreciation rates. If the results from this study suggest anything, it is that it may well be better to site Section 42 developments in areas that lack concentrations of poverty. This is consistent with the view that it is better for communities for housing developed for low to moderate income households to be scattered, rather than concentrated.

References

Alonso, William. *Location and Land Use*. Harvard University Press, 1964

Austin/Travis County Community Action Network. *Through the Roof: A Report on Affordable Homes in Austin*. Austin, TX: Working Group on Affordable Housing, August 1999

Bailey, Martin J., Richard F. Muth and Hugh O. Nourse. A Regression Method for Real Estate Price Index Construction. *Journal of the American Statistical Association*, 58, 1963, pp. 933-42.

Bartik, Timothy J. Neighborhood Revitalization's Effects on Tenants and the Benefit-Cost Analysis of Government Neighborhood Programs. *Journal of Urban Economics*, 19, 1986, pp. 234-48.

Bond, Eric W. and N. Edward Coulson. Externalities, Filtering and Neighborhood Change. *Journal of Urban Economics,* 26, September 1989, pp. 231-49.

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

Brown, H. James. Micro Neighborhood Externalities and Hedonic Housing Prices. *Land Economics*, 56, 1980, pp. 125-41.

Buchanan, James M. and William Craig Stubblebine. Externality. *Economic Journal*, 29, November 1962, pp. 371-84.

Butler, Richard V. The Specification of Hedonic Indexes for Urban Housing. *Land Economics*, 58(1), February 1982, pp. 96-108.

Capozza, Dennis R. and Robert W. Helsley. The Fundamentals of Land Prices and Urban Growth. *Journal of Urban Economics*, 26(3), November 1989, pp. 295-306.

Case, Karl E. and Robert J. Shiller. The Efficiency of the Market for Single Family Homes. *American Economic Review*, 79(1), March 1989, pp. 125-37.

Chinloy, Peter. The Effect of Maintenance Expenditures on the Measurement and Depreciation of Housing. *Journal of Urban Economics,* 8, 1980, pp. 86-107.

DeSalvo, Joseph S. Neighborhood Upgrading Effects of Middle Income Housing Projects in New York City. *Journal of Urban Economics*, 1(3), 1974, pp. 269-77.

Evans, William N., Wallace E. Oates and Robert M. Schwab. Measuring Peer Group Effects: A Study of Teenage Behavior. *Journal of Political Economy*, 100(5), 1992.

Farber, S. Market Segmentation and the Effects of Group Homes for the Handicapped on Residential Property Values. *Urban Studies*, 23, 1986, pp. 519-25.

Fischel, William A. Why Are There NIMBYs? *Land Economics*, 77(1), February 2001, pp. 144-52.

Follain, James R. and Stephen Malpezzi. The Flight to the Suburbs: Insight from an Analysis of Central City versus Suburban Housing Costs. *Journal of Urban Economics*, 9(3), May 1981, pp. 381-98.

Galster, George C. Empirical Evidence on Cross Tenure Differences in Home Maintenance and Condition. *Land Economics*, 59(1), February 1983, pp. 107-13.

Gamble, H.B. and R.H. Downing. Effects of Nuclear Power Plants on Residential Property Values. *Journal of Regional Science*, 22(4), November 1982, pp. 457-78.

Gatzlaff, Dean H. and Donald R. Haurin. Sample Selection Bias and Repeat-Sales Index Estimates. *Journal of Real Estate Finance and Economics*, 14, 1997, pp. 33-50.

Gatzlaff, Dean H., Richard K. Green and David L. Ling. Revisiting Cross-Tenure Differences in Housing Maintenance. *Land Economics*, 74(3), August 1998, pp. 328-42.

Goetz, Edward G., H.K. Lam and A. Heitlinger. *There Goes the Neighborhood? The Impact of Subsidized Multifamily Housing on Urban Neighborhoods*. Minneapolis: University of Minnesota Center for Urban and Regional Affairs, 1996.

Greater Baltimore Community Housing Resource Board, Inc. *The Impact of Group Homes on Residential Property Values in Baltimore County, MD.* Baltimore, 1993.

Green, Richard K. and Stephen Malpezzi. *A Primer on U.S. Housing Markets and Policies.* The Urban Institute Press for the American Real Estate and Urban Economics Assocation, 2001.

Grieson, R.E. and J.R. White. The Existence and Capitalization of Neighborhood Externalities: A Reassessment. *Journal of Urban Economics*, 25, 1989, pp. 68-76.

Guggenheim, Joseph. *Tax Credits for Low Income Housing*. Fourth Edition, Simon Publications, 1989.

Guy, Donald C., John L. Hysom, Stephen R. Ruth. The Effect of Subsidized Housing on Values of Adjacent Housing. *AREUEA Journal*, 13, Winter 1985, pp. 378-87.

Halverson, R. and Henry O. Pollakowski. Choice of Functional Form for Hedonic Price Equations. *Journal of Urban Economics*, 10(1), July 1981, pp. 37-49.

Hughes, William T. Jr. and C.F. Sirmans. Traffic Externalities and Single-Family House Prices. *Journal of Regional Science*, 32(4), 1992, pp. 487-500.

Ihlanfeldt, Keith Ray and Thomas P. Boehm. Government Intervention in the Housing Market: An Empirical Test of the Externalities Rationale. *Journal of Urban Economics*, 22, 1987.

Isard, Walter. Regional Science: Parallels from Physics and Chemistry. *Papers in Regional Science*, 78(1), January 1999, pp. 5-20

Jud, G. Donald and J.M. Watts. Schools and Housing Values. *Land Economics*, 57(3), August 1981, ppp. 459-70.

Knight, John R. and C.F. Sirmans. Depreciation, Maintenance and Housing Prices. *Journal of Housing Economics*, 5(4), December 1996, pp. 369-89.

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

Lowry, Ira S. *A Model of Metropolis*. Rand, 1964.

Malpezzi, Stephen, Larry Ozanne and Thomas Thibodeau. *Characteristic Prices of Housing in 59 SMSAs*. The Urban Institute, 1980.

Maxfield Research, Inc. *A Study of the Relationship Between Affordable Family Rental Housing and Home Values in the Twin Cities*. Minneapolis: The Family Housing Fund, 2000.

Meyer, William B. NIMBY Then and Now: Land-Use Conflict in Worcester, Massachussetts, 1876-1900. *Professional Geographer*, 47(3), August 1995, pp. 298-307.

Michaels, R.G. and V.K. Smith. Market Segmentation and Valuing Amenities with Hedonic Models: The Case of Hazardous Waste Sites. *Journal of Urban Economics*, 28, 1990, pp. 223-42.

Mills, Edwin S. *Studies in the Structure of the Urban Economy.* Johns Hopkins University Press, 1972.

Morris, Jane Anne. *Not in My Backyard: The Handbook*. San Diego: Silvercat Publications, 1994.

Muth, Richard F. *Cities and Housing*. University of Chicago Press, 1969.

National Low Income Housing Coalition. *The NIMBY Report: Smart Growth and Affordable Housing*. Washington, D.C.: 2001.

Nelson, A. C., Genereux and M. Genereux. Price Effects on Landfills on House Values. *Land Economics*, Vol. 68, No. 4, 1992, pp. 359-365.

Newman, Sandra J. and Joseph Harkness. Assisted Housing and the Educational Attainment of Children. *Journal of Housing Economics*, 9(1/2), March/June 2000, pp. 40-63.

Nourse, Hugh O. The Effect of Public Housing on Property Values in St Louis. *Land Economics*, 39, 1963, pp.434-41.

Ozanne, Larry and Stephen Malpezzi. The Efficacy of Hedonic Estimation with the Annual Housing Survey: Evidence from the Demand Experiment. *Journal of Economic and Social Measurement*, 13(2), July 1985, pp. 153-72.

Palmquist, Raymond B. Valuing Localized Externalities. *Journal of Urban Economics,* 31, 1992, pp. 59-68.

Palmquist, Raymond B., Fritz M. Roka and Tomislav Vukina. Hog Operations, Environmental Effects and Residential Property Values. *Land Economics,* 73(1), February 1997, pp. 114-24.

Polinsky, A.M. and S. Shavell. Amenities and Property Values in a Model of an Urban Area. *Journal of Public Economics*, 5, 1976, pp. 119-29.

Rabiega, William A., Ta-Win Lin and Linda Robinson. The Property Value Impacts of Public Housing Projects in Low and Moderate Density Residential Neighborhoods. *Land Economics*, 60, 1984, pp. 174-9.

Reichert, Alan K., Michael Small and Sunil Mohanty. The Impact of Landfills on Residential Property Values. *Journal of Real Estate Research*, 7(3), Summer 1992, pp. 297-314.

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

Shafer, Robert. The Effect of BMIR Housing on Property Values. *Land Economics*, 48, 1972, pp. 262-86.

Siegel, Joyce. *The House Next Door*. The Innovative Housing Institute, 2001.

Thibodeau, Thomas G. Estimating the Effect of High-Rise Office Buildings on Residential Property Values. *Land Economics,* 66(4), 1990, pp. 402-8.

Troy, Austin. *Assessing the Effects of Natural Hazard Disclosure on California Property Markets Using a Spatial Hedonic Analysis*. University of California, Berkeley, Department of Environmental Science, Policy and Management, n.d., circa 1999.

Wang, Ferdinand T. and Peter M. Zorn. Estimating House Price Growth with Repeat Sales Data: What's the Aim of the Game? *Journal of Housing Economics*, 6, 1997, pp. 93-118.

Weicher, John C. and R.H. Zerbst. The Externalities of Neighborhood Parks: An Empirical Investigation. *Land Economics,* 49, February 1973, pp. 99-105.

Zax, J.S. Compensation for Commutes in Labor and Housing Markets. *Journal of Urban Economics*, 30(2), 1991, pp.192-207.

Table 1: Repeat Sales Example from Shorewood Hills

Number	First Sale Price	Second Sale Price	Difference in Log Prices	First Sale Date	Second Sale Date	Matrix of Time Dummy Variables (1986 is Base Year) 1986	1987	1988	1989	1990	1991	1992
1	200,000	232,000	0.148	June 1991	Sept 1992	0	0	0	0	0	-1	1
2	131,000	180,000	0.318	May 1987	July 1989	0	-1	0	1	0	0	0
3	66,500	95,100	0.358	November 1988	August 1992	0	0	-1	0	0	0	1
4	85,000	90,000	0.057	March 1987	May 1998	0	-1	1	0	0	0	0
5	145,000	186,000	0.249	June 1988	November 1992	0	0	-1	0	1	0	0
6	175,000	215,000	0.206	June 1988	April 1989	0	0	-1	1	0	0	0
7	240,000	261,566	0.086	July 1987	Oct 1992	0	-1	1	0	0	0	0
8	186,250	244,500	0.272	August 1989	July 1992	0	0	0	-1	0	0	1
9	455,000	506,000	0.106	July 1988	March 1992	0	0	-1	0	0	0	1
10	58,200	69,000	0.170	July 1988	July 1991	0	0	-1	0	0	1	0
11	157,000	340,000	0.773	June 1987	August 1991	0	-1	0	0	0	1	0
12	85,000	127,500	0.406	Sept 1986	May 1991	-1	0	0	0	0	1	0
13	75,900	87,900	0.147	Sept 1987	January 1991	0	-1	0	0	0	1	0
14	149,800	195,000	0.264	June 1987	Sept 1992	0	-1	0	0	0	0	1
15	88,500	186,000	0.743	June 1988	Sept 1992	0	0	-1	0	0	0	1
16	144,000	302,000	0.741	April 1986	June 1991	-1	0	0	0	0	1	0
17	220,000	265,000	0.186	August 1989	December 1992	0	0	0	-1	0	0	1

Table 2

Regressions Explaining Impact of Section 42 Developments on changes in House Prices: Madison

	(1)	(2)	(3)	(4)	(5)
Dcenter	0.18 (0.03)	0.18 (0.03)	0.17 (0.03)	0.46 (0.06)	0.10 (0.05)
Dmin	-0.42 (0.19)	-0.36 (0.47)	-0.40 (0.19)	0.26 (0.40)	-0.60 (0.23)
$Dmin^2$		-1.66 (11.27)			
Controls ?	N	N	N	N	Y
Interactions ?	N	N	N	Y	N
Gravity Measures?	N	N	Y	N	N
Gravity F-Statistic			0.50		
R^2	0.02	0.02	0.02	0.71	0.03

N = 3193. Standard Errors are in parenthesis.

Table 3

Regressions Explaining Impact of Section 42 Developments on changes in House Prices: Milwaukee County

Variable	**(1)**	**(2)**	**(3)**	**(4)**
Distance to County Courthouse	-0.23 (0.13)	-0.26 (0.13)	-0.44 (0.30)	-0.29 (0.32)
Distance to nearest Development	1.68 (0.46)	1.73 (0.46)	0.42 (0.52)	0.41 (0.16)
Controls	N	N	Y	Y
Gravity Measures	N	Y	N	Y
Gravity F-Statistic	-	4.28	-	-
Year of Sale	N	N	N	Y
R-square	0.01	0.01	0.02	0.03

Number of Observations: 2258
Standard Errors are in parenthesis.

Table 4

Regressions Explaining Impact of Section 42 Developments on changes in House Prices: Waukesha County

Variable	(1)	(2)	(3)	(4)
Distance to County Courthouse	-0.06 (0.06)	-0.07 (0.06)	-0.18 (0.11)	-0.16 (0.14)
Distance to nearest Development	0.19 (0.16)	0.19 (0.16)	0.12 (0.16)	0.05 (0.18)
Controls	N	N	Y	Y
Gravity Measures	N	Y	N	Y
Gravity F-Statistic	-	1.66	-	-
Year of Sale	N	N	N	Y
R-square	0.00	0.01	0.04	0.21

Number of Observations: 367
Standard Errors are in parenthesis.

Table 5

Regressions Explaining Impact of Section 42 Developments on changes in House Prices: Ozaukee County

Variable	**(1)**	**(2)**	**(3)**	**(4)**
Distance to County Courthouse	0.01 (0.17)	0.01 (0.17)	0.41 (0.61)	0.41 (0.62)
Distance to nearest Development	0.01 (0.50)	0.01 (0.50)	-0.01 (0.62)	-0.06 (0.63)
Controls	N	N	Y	Y
Gravity Measures	N	Y	N	Y
Gravity F-Statistic	-	0.00	-	-
Year of Sale	N	N	N	Y
R-square	0.00	0.00	0.00	0.04

Number of Observations: 425
Standard Errors are in parenthesis.

Appendix Table 1

Regressions Explaining Impact of Section 42 Developments on changes in House Prices: Milwaukee

Variable	**Coefficient**	**Standard Errors**
Year in 1995	-0.02	0.04
Year in 1996	-0.02	0.04
Year in 1997	0.03	0.06
Year in 1998	0.10	0.06
Year in 1999	-0.02	0.02
Year in 2000	0.02	0.04
Number of Developments in census tract	-0.02	0.03
Percentage of Married Households	-0.25	0.12
Percentage of Women Headed Households	0.48	0.28
Percentage of Black Headed Households	0.01	0.12
Median Income	0.00	0.00
Poverty Rate	0.02	0.18
$\mathbf{R^2}$	0.47	