Supportive Housing and Neighborhood Property Value Externalities

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ABSTRACT

Supportive Housing and Neighborhood Property Value Externalities

We analyzed impacts on single-family home prices of eleven, small-scale supportive housing facilities announced in Denver during 1989-1995. Using a difference in differences econometric specification, we found that these facilities produced a *positive* impact on house prices within 1,001 to 2,000 feet. We attributed this effect to countervailing externalities (building rehabilitation vs. resident behaviors) that vary in their spatial extent. Supportive housing facilities were systematically sited in neighborhoods with declining relative prices compared to elsewhere in the census tract. This location bias led a conventional econometric specification to erroneously estimate a negative property value impact from supportive housing.

SUPPORTIVE HOUSING AND NEIGHBORHOOD PROPERTY VALUE EXTERNALITIES

For many years, economists have investigated the degree to which neighborhood amenities and disamenities have been capitalized into the values of properties located nearby (Polinsky and Shavell, 1976; Bartik, 1988; Grieson and White, 1989; Palmquist, 1992). The list of externality sources that has been studied has been long, ranging from the land use and building maintenance choices of property owners, to the racial composition of residents, to public infrastructure decisions involving highways and parks. Of more relevance to the present work, the list has also included a variety of subsidized housing programs, including public housing, privately owned subsidized rental complexes, and tenant-based rental vouchers.

Few types of assisted housing, however, bear the public stigma of negative externality generator as blatantly as "supportive housing," facilities developed to care for special needs populations who, in an earlier era, would have been confined in large, institutional settings. As part of the nation-wide "deinstitutionalization" movement beginning in the 1970s, chronically mentally ill, developmentally and physically disabled, non-violent offenders, recovering substance abusers, and frail elderly individuals have increasingly been domiciled in small-scale facilities located in residential neighborhoods. This "normalization" of residential environment, coupled with on-site and on-call specialized care, is seen as therapeutic by many policy makers (Newman, 1994).

Unfortunately, it is conventionally seen by the host neighborhood as something to be opposed because of its alleged harmful impact on property values. The particular negative externalities claimed vary with the specifics of the facility, but often include: increased noise and litter, poorly maintained property, infectious diseases (particularly in the case of HIV/AIDS care facilities), uncivil or bizarre behaviors, and criminal activities (Takahashi and Dear, 1997).

Certainly this has been true in the city investigated in this research, Denver, Colorado.[1] There in the early 1990s, a proposed site intended to provide supportive housing for people with physical disabilities encountered intense opposition when it was erroneously characterized in the media as a "halfway house for criminals." In another case, a single-family home converted for use by eight women with chronic mental illness and/or dual diagnosis with substance addiction resulted in protests from a nearby school, which argued that these new residents would pose a threat to school children walking past their facility. In response to such controversies, the Denver passed the Large Residential Care Use Ordinance in 1993 (City and County of Denver, 1998a, b). Among other things, the Ordinance specified minimum separation requirements among supportive housing facilities, and established a mechanism of consultation between the developer and the host neighborhood, mediated by city officials. The Ordinance gives Denver's Zoning Administrator the power to approve, approve with conditions, or deny a permit for supportive housing.

This study was designed to ascertain whether Denver supportive housing facilities indeed have the negative property value impacts that their critics contend. We analyze eleven, small-scale “Special Care Homes” developed during the early 1990s. As defined by Denver ordinances (City and County of Denver, 1998a, b), *Special Care Homes* are residential care facilities that are the primary residence of unrelated persons who live as a single housekeeping unit and receive more than 12 hours per day of on-premises treatment, supervision, custodial care or special care due to physical condition or illness, mental condition or illness, or behavioral or disciplinary problems.

We develop a new econometric specification that overcomes the location bias associated with supportive housing development that confounds assessing direction of causality. Contrary to conventional wisdom, we find that the development of these supportive housing facilities in Denver generally had a positive effect on property values of single-family homes nearby. On the contrary, a conventional econometric specification erroneously estimated a negative property value impact, leading us to conclude that using the correct specification is crucial.

I. PREVIOUS RESEARCH ON SUPPORTIVE HOUSING FACILITIES AND PROPERTY VALUES

By the end of the 1980s at least a dozen scholarly studies had investigated the question of whether supportive housing for renters with mental or physical handicaps generated a negative impact on neighboring single-family property values. The common conclusion reached by these studies was that there was no sizable or statistically significant impact. After reviewing "every available study," the Mental Health Law Project (1983: abstract) concluded that "[they] conclusively establish that a group home as community residential facility for mentally disabled people does not adversely affect neighbors' property values or destabilize a neighborhood." A few studies even concluded that there was a positive property value impact, especially in lower-valued neighborhoods (Dear, 1977; Wagner and Mitchell, 1980; Gabriel and Wolch, 1984; Farber, 1986; Boydell, Trainor and Pierri, 1989; Hargreaves, Callanan, and Maskell, 1998).

Only one study of this early period provided even a hint of dissension, and it could be discounted on methodological grounds. Gabriel and Wolch (1984) studied the relationship between the number of human service facilities per 1,000 residents of census tracts in Oakland and median home sales prices in the tract, using multiple regression analysis. When all tracts were included in the regression, larger numbers of residential facilities for both adult and children proved inversely related to median prices. When regressions were disaggregated by predominant race of occupancy, however, the only adverse impacts appeared to be from adult residential facilities located in predominantly black-occupied tracts. In any event, the lack of variables controlling for other aspects of census tracts that could affect prices besides human service facilities renders conclusions from this study suspect.

During the last decade, however, the conventional wisdom of no negative external impacts has been shaken by several statistical studies concluding that, with certain circumstances and kinds of developments, supportive housing can create severe effects on proximate property values. Lyons and Loverage (1993) investigated the impacts of four locations where federally supportive buildings housed handicapped tenants in St. Paul, MN. The apartment complexes ranged in size from 10 to 103 units. They found a negative impact from each handicapped unit but, surprisingly, the size of the negative impact diminished with marginal increases in the number of units in the facility. For example, an apartment with ten handicapped units within one-half mile of a single-family home reduced the assessed value of that home by a statistically significant $1670; within one mile it reduced it by $682. But, an apartment with one hundred handicapped units within one-half mile of a single-family home was estimated to *increase* the assessed value of that home by $1,300; although within one mile it reduced it by $1,600. The authors offered no explanation for these results.

Galster and Williams (1994) investigated the effects of dwellings occupied exclusively by severely mentally disabled tenants on sales prices of nearby homes in two small Ohio towns. Controlling for features of the dwelling and the neighborhood, proximity within two blocks of two small, newly constructed apartment buildings for the mentally ill resulted in a 40 percent decrease in sales prices. However, proximity to three similar, new apartment complexes or to three rehabilitated apartment buildings for the mentally ill had no impact on prices. The authors interpreted the results as suggesting that siting, building type, and tenant allocation procedures may matter more for potential neighborhood externality effects than occupancy by mentally ill tenants per se.

In the most methodologically sophisticated of these studies, Colwell, Dehring, and Lash (2000) analyzed seven group homes opening during the 1987-1994 period in seven communities in suburban Chicago. Each site housed between four and eight handicapped tenants. Controlling for neighborhood-specific housing price trends and levels, they considered whether there were any noticeable aggregate shifts in the overall home sales price gradients across these seven areas after a nearby group home was announced. They found a post-announcement negative impact within 200 feet, with a reduction in sales prices of 13 percent if the sales were within sight of the group home. Moreover, if a community protest arose after the announcement, and additional 7.7 percent price declination occurred, which the authors attributed to the negative "signaling" effect that such a protest had for the market evaluation of the area.

One possible explanation for why the foregoing analyses have come to such different conclusions is because they employ different methodologies. Unfortunately, each has serious, if somewhat different, shortcomings. The three alternative approaches can be termed: control area, pre/post, and econometric.

The *control area approach*, represented by Dear (1977), Wolpert (1978), Boeckh, Dear, and Taylor (1980), Lauber (1986), Iglhaut (1988) and Boydell, Trainor and Pierri (1989), selects neighborhoods that are otherwise comparable to one(s) that have supportive housing located within them and then compares property value levels or trends in both sets. The fundamental challenge here is identifying areas that are, indeed, identical in all respects save for supportive housing and that have no other forces or land developments which differentially affect them subsequent to the supportive housing development. Indeed, this challenge may be insurmountable. As amplified below, developers and occupants of supportive dwellings may choose certain neighborhoods precisely because they have attributes that are particularly attractive for their purposes.

The *pre/post approach*, represented by Wagner and Mitchell (1979; 1980), Lindauer, Tungt, and O'Donnell (1980), Ryan and Coyne (1985), District of Columbia Association for Retarded Citizens (1987), Iglhaut (1988), and Boydell, Trainor and Pierri (1989), compares levels and/or trends in property values in the same neighborhood(s) between periods preceding and then succeeding the introduction of a supportive housing development.[2] The difficulty here is ensuring that there are no additional forces coincident with opening the supportive housing that may affect values in the impact neighborhood, such as macroeconomic or local housing submarket pressures. For example, the entire metropolitan area's housing market may be in an area of deflationary prices, whereupon there will be a tendency for any pre/post comparison of values in any neighborhood to show a secular trend of decline, regardless of the presence of a supportive housing site.

The *econometric approach* has many variants, but typically it tries to ascertain whether there is an independent, cross-sectional variation in housing prices that can be associated with proximity to a supportive housing site. Although not an inherent flaw in the approach, virtually all previous econometric studies (except Galster and Williams 1994; Colwell, Dehring and Lash 2000; see below) have failed to control for the idiosyncratic characteristics of the neighborhood that surrounds (say, within a radius of a quarter mile) but is unrelated to the supportive housing site. Instead, most settle for variables that measure characteristics of the encompassing census tract, which may be poor proxies for conditions in the area near the supportive site. Thus, if these omitted, nearby-neighborhood variables were correlated with the location of supportive housing, apparently statistically significant proximity effects might erroneously be attributed to the latter instead of the former. One candidate for such an important omitted variable is the presence of a (possibly large) apartment building in the area, into which some special needs households are placed at a later date after the building is rehabilitated. In such a case the statistics could not distinguish between the impacts of proximity to an apartment building and proximity to a supportive housing facility.

This criticism takes on additional importance when considering a fundamental shortcoming of virtually all previous approaches: they do not convincingly distinguish the direction of causation between patterns of neighborhood property values and the siting of supportive housing. Put differently, they cannot ascertain whether supportive housing sites lead to neighborhood value decline or whether supportive sites are systematically located in areas having property values that are low and/or expected to depreciate in the future.

The latter causal pattern is likely because of several, not mutually exclusive behaviors of the developers of the supportive housing facility and the nature of the local real estate market. First, the public authority or non-profit organization developing supportive housing will be encouraged to husband its scare resources by acquiring properties (vacant land or existing structures) in the least-expensive neighborhoods available. Second, if new construction of supportive housing is contemplated, the location of vacant, appropriately zoned parcels will likely be constraining on choices. Neighborhoods with mixed-use or higher-density zoning are likely to have lower home values. Third, if rehabilitation of structures for use as supportive housing is contemplated, minimization of expected lifetime development costs of the structure implies choices of certain building types that likely are historically concentrated in certain types of lower-valued neighborhoods (Newman, Harkness, Galster, and Reschovsky, 1997). Fourth, developers often search in lower-value areas because they expect less public opposition there (Pendall, 1999). All these reasons imply that the particular locations in which supportive housing is developed are not likely to be representative, but rather will be biased toward lower/declining values. To obtain a clear portrait of the impact of supportive housing facilities, one must thus control for both the *level and trend of prices* in the neighborhoods surrounding the site(s), both *before and after* the site is developed as supportive housing.

Two of the aforementioned studies come close to achieving the appropriate level of control, but ultimately fall short. Both Galster and Williams (1994) and Colwell, Dehring, and Lash (2000) employ a "spatial fixed effects" specification wherein dummy variables denote the *level* of home prices associated with the neighborhood within a certain distance from a supportive housing site (either future or current). A corresponding set of dummy variables denotes whether these price *levels* differ significantly after the supportive housing is announced or begins operation. This econometric version of a pre/post method fails, however, to control for the *trend* in sales prices extant in this neighborhood *prior* to the introduction of the subsidized housing.[3] This omission could lead to erroneous conclusions if, for example, the given area were on a trajectory of steep depreciation prior to the introduction of supportive housing. Such an area would manifest a lower level of prices after the opening of the supportive site than before. But this would not be caused by the supportive site, rather to a continuation of pre-existing trends in this neighborhood. Thus, only an econometric specification that controls for pre-/post-occupancy changes in *both price levels and trends* in the neighborhood near the supportive housing site can yield unambiguous implications about the causal impact of that site.

Our approach overcomes shortcomings of prior approaches by combining elements of the pre/post and control area methods into an econometric model. As in the pre/post approach, it measures for various areas near supportive housing sites the level and trend in home prices before and after the facilities are announced. As in the control area approach, it compares these localized effects to property value trends in all neighborhoods to control for forces affecting the entire city's housing market. The complete specification of our model follows, along with a discussion of how it differs from conventional approaches.

II. A CONCEPTUAL FRAMEWORK FOR ANALYZING THE DETERMINANTS OF HOUSE PRICES

We adopt the conventional notion (Muellbauer 1974; Rosen 1974) that each house may be described as a package of various characteristics describing numerous attributes of the structure [*S*], neighborhood [*N*], and local public services [*L*]. Symbolically:

$$H = f([S], [N], [L]) \qquad [1]$$

where *H* can be thought of as the "quality" of that house or its "hedonic value" (Rothenberg et al., 1991: ch. 3). The sales price of the housing package is a function of its embodied quality:

$$P = g(H) \qquad [2]$$

As shown in equations [1] and [2], the sales price of a home will be affected by numerous neighborhood attributes (including physical and occupancy characteristics of neighboring properties, environmental conditions, and potentially the proximity of supportive housing sites) and attributes of the local public sector (including schools, police protection, taxes, and zoning). One challenge facing the analyst is to gather complete data on this array of neighborhood attributes so that results will not be tainted by omitted variable bias. This challenge has two facets: one must not only gather a comprehensive, dauntingly large set of attributes, but one also must ascertain the geographic area over which these attributes are most appropriately measured for each site.

The approach we have chosen responds to this challenge by specifying a spatial fixed effects model. That is, dummy variables were specified that denote mutually exclusive geographic areas of various scales. These variables control, in summary form, for the idiosyncratic bundle of attributes that are present in the corresponding space. The effect on sales prices of individual attributes in this spatial bundle cannot be determined, however.

Our procedure is distinguished by its specification of the "neighborhood." We employed a set of predetermined-boundary, mutually exclusive areas (census tracts) for defining one set of spatial fixed effect variables. However, to measure fixed effects in smaller-scale neighborhoods we relied on a different procedure. Essentially, we defined a series of "neighborhoods" centered on each supportive housing site, each one comprising one of several concentric rings within a range of 2,000 feet. Depending on the proximity of the supportive housing sites, these neighborhoods may overlap slightly. Our specification estimated a fixed effect for each of these neighborhood spaces.

III. MODEL SPECIFICATION

In overview, our approach measures the *level and trend* in home prices in two sorts of neighborhoods during two periods: in neighborhoods surrounding supportive housing *both before and after it* was developed, and in otherwise-similar neighborhoods where no supportive housing was developed. Our approach then compares home prices *after* such housing was developed to *what they would have been had pre-development trends in the neighborhood persisted*, adjusting for any area-wide changes in trends occurring during the post-development period (as evinced by neighborhoods where no such housing was developed). Were actual home prices to differ from the counterfactual prices, impact would be deduced. Ellen et al. (2001) equivalently framed this comparison in terms of "difference in differences:" the difference between target and control neighborhoods' price differences in the pre-development and post-development periods. Were these differences to vary between pre- and post-development periods, impact would be deduced.

More precisely, our specification of the hedonic price function [2] can be expressed symbolically as:

$$LnP = c + [Struct][b] + [Quarter][n] + [Tract][m] + [SpaceH][p]$$

$$d\,\mathcal{D}All_{500} + e\,\mathcal{D}All_{1k} + fDAll_{2k} + g\,\mathcal{D}Post_{500} + h\,\mathcal{D}Post_{1k} + j\,\mathcal{D}Post_{2k} +$$

$$q?Time_{500} + r?Time_{1k} + s?Time_{2k} + t?TrPost_{500} + u?TrPost_{1k} + v?TrPost_{2k} + ? \qquad [3]$$

Where the components of the models are defined as follows:

LnP	Log of the sales price[4]
c	Constant term
[Struct]	Vector of structural characteristics of home, including building and lot size, age, building materials and type, and numerous amenities; for details, see Annex A
[Quarter]	Vector of dummies indicating the time (year and quarter) of sale; a seasonal and Denver-wide intertemporal trend measure
[Tract]	Vector of census tract dummies indicating the location of home; a tract fixed-effect measure
[SpaceH]	Vector of latitude (X) and longitude (Y) coordinates, plus XY, X^2, and Y^2 ; spatial heterogeneity correction variables (see below)

$DAll_x$	Dummy for mutually exclusive distance ring *x*; equals 1 if sale occurs within area *x* proximate to current or future supportive housing site; zero otherwise; a localized fixed-effect measure where ring *x* can be either 0-500 feet, 501-1,001 feet, or 1,001-2,000 feet areas
$DPost_x$	Post-announcement dummy for mutually exclusive distance ring *x*; equals 1 if sale occurs within area *x* proximate to supportive housing site after announced; zero otherwise
$Time_x$	Trend variable for mutually exclusive distance ring *x;* equals 1 if sale occurs in first quarter of study period (1st quarter 1987) and sale occurs within area *x* proximate to current or future supportive housing site, equals 2 if sale occurs in second quarter of study period and sale occurs within area *x* proximate to current or future supportive housing site, etc.; zero otherwise; a localized trend measure
$TrPost_x$	Post-announcement trend variable for mutually exclusive distance ring *x;* if sale is post-announcement of site in ring *x*, then equals 1 if sale occurs in first quarter after site was announced, equals 2 if sale occurs in second quarter after site was announced, etc.; zero otherwise
?	A random error term with statistical properties discussed below.

All lower case letters in the equations (b, c, d, etc.) represent coefficients to be estimated. Descriptive statistics for these variables are available from the first author.

The model tests for both price level shift and price trend slope alteration effects in impact areas near supportive housing sites, while controlling for fixed spatial effects in the larger neighborhood and inter-temporal changes in the Denver housing market through the following:[5]

[Quarter] Measures quarterly changes in the overall Denver house price levels associated with seasonality and general market trends

[Tract] Measures the fixed effect on house prices due to location in the area defined by the Census tract

$DAll_x$ Measures the fixed effect throughout Denver of being in the area defined as within ring *x* proximate to one of the supportive housing sites studied, regardless of whether announced or operating yet, where ring *x* can be either 0-500 feet, 501-1,001 feet, or 1,001-2,000 feet mutually exclusive areas centered on the supportive housing site

$DPost_x$ Measures the fixed effect throughout Denver of being in the area defined as within ring *x* proximate to one or more supportive housing site(s) *after* announcement and, later, operation

$Time_x$ Measures the trend in house prices during the study period in the area throughout Denver defined as within ring *x* proximate to one of the supportive housing sites studied, regardless of whether announced or operating yet

$TrPost_x$ Measures the trend in house prices during the study period in the area throughout Denver defined as within ring *x* proximate to one or more supportive housing site(s) *after* announcement and, later, operation

The test for statistical significance of the post-announcement shift coefficients (g, h, j) of the $DPost_x$ variables is equivalent to testing that there is a discontinuous change in the price levels in the neighborhoods (defined for one of the mutually exclusive distance rings) around supportive housing sites post-announcement. The test for statistical significance of the post-announcement trend coefficients (t, u, v) of $TrPost_x$ is equivalent to testing that there is a change in the price trends in the area (again, defined for one of the mutually exclusive distance rings) around supportive housing sites post-announcement.[6] Should both the shift and trend post-announcement coefficients prove to not be significantly different from zero, it would reject the hypothesis of impact.

Should one or both be statistically significant, however, the magnitude of supportive housing impact across all sites involves assessing whether (d+qTime*) - (g+tTrPost), (e+rTime*) - (h+uTrPost), and/or (f+sTime*) - (j+vTrPost) ? 0, where Time* represents the latest quarter prior to announcement of the supportive housing development. Should the alterations in shift and trend terms yield contrary implications (such as a downward shift but increased slope in the price gradient), it will be necessary to calculate net effects at different quarters post-announcement.

We stress that the results of any regression model do not offer conclusive proof of causation, merely association. Nevertheless, our specification, by clearly comparing pre- and post-announcement differences in price levels and trends (adjusted for city-wide trends), provides exceptionally convincing evidence in this regard.

Our difference in differences approach overcomes the shortcomings of prior approaches noted above as follows. By measuring both level and trend of home prices in a small-scale surrounding area before an affordable or multifamily development is present, this method controls for the idiosyncratic local neighborhood characteristics that are reflected in these prices. By relating post-development home price trends and levels in the affected neighborhood nearby to those in larger, unaffected geographic areas, it controls for forces having area-wide affects, such as metropolitan-wide changes in the economy. By doing both, the model distinguishes the processes through which supportive housing is systematically located in weaker neighborhood submarkets from the ultimate consequences of such housing on these neighborhoods. In other words, by controlling what would have happened in the neighborhood had pre-supportive housing development trends persisted (adjusting for area-wide changes that would have altered all neighborhood home price trends), our method unambiguously measures effects attributable to the new development.

To get a better intuitive grasp of what our difference in differences approach does, refer to Figure 1, which illustrates several hypothetical alternatives. Our method essentially estimates home price trends and levels in two sorts of neighborhoods within the metro area under investigation: one "impact neighborhood" in the vicinity of supportive housing (shown by the variants of the dashed line) and other, "control neighborhoods" without such housing (shown by the solid line).

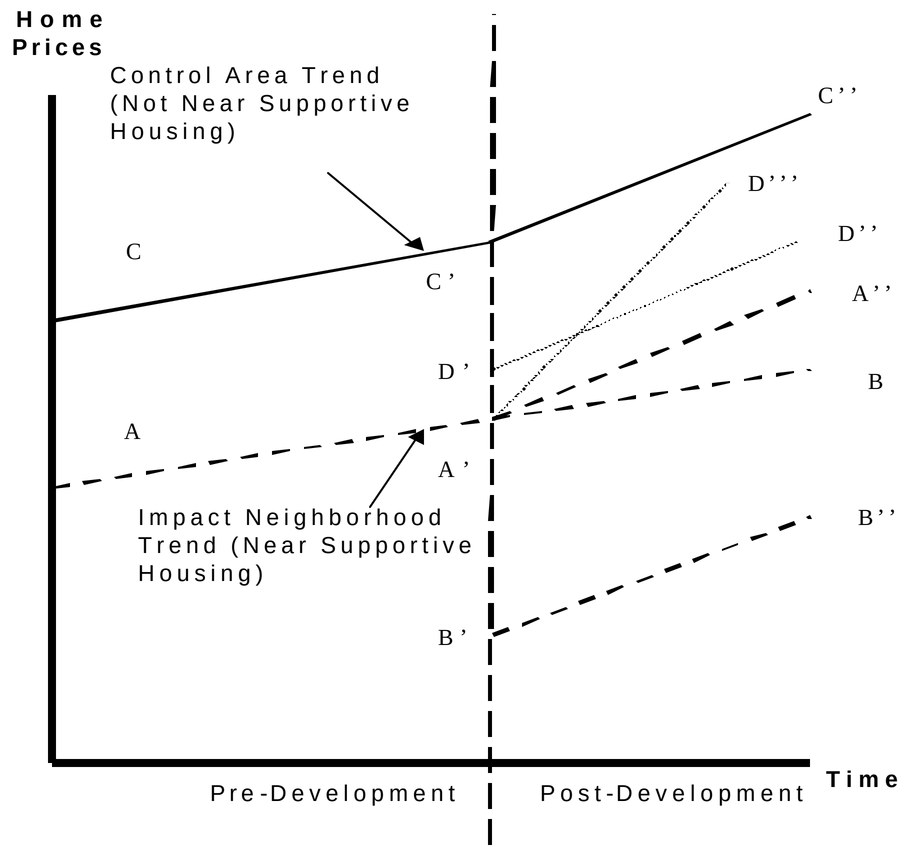
The correct test of whether the supportive housing development has an effect is whether there is a pre/post break in the trend (or shift in level) in the proximate impact neighborhood, which is different than what was observed in the other, control areas of the metropolitan housing market. Thus, were we to estimate empirically line A-A'-A'', this would signify no impact, because the price trend break after the supportive housing opened mirrored the trend break observed in control neighborhoods in the metro area (line C-C'-C'').[7] However, if prices in the impact neighborhood after the supportive housing opened were to shift up to a higher level (A-A'-D'-D'') and/or increase more rapidly than the control area trends (A-A'-D'''), this would signify a positive impact.[8] Conversely, if prices in the impact neighborhood after the supportive housing opened were to shift down to a lower level (A-A'-B'-B'') and/or increase less rapidly (decrease more rapidly) than the control area trends (A-A'-B), this would signify a negative impact.[9]

[figure 1 about here]

Contrast these conclusions to those following from the three traditional approaches. Were price profiles A-A'-B or A-A'-A'' to be observed, the pre/post approach would have erroneously concluded a positive impact because prices (trends or average levels) were higher after the development. Were any of the price profiles shown actually manifested, the cross-sectional approach would have erroneously concluded a negative impact because the observed prices were lower near areas where supportive housing opened than elsewhere. The traditional control area approach could not be operationalized in this scenario because of the inability to match impact areas having the supportive housing developments of interest with otherwise identical (e.g., same price) areas.

Figure 1

Illustration of Potential Types of Home Price Impacts from Supportive Housing



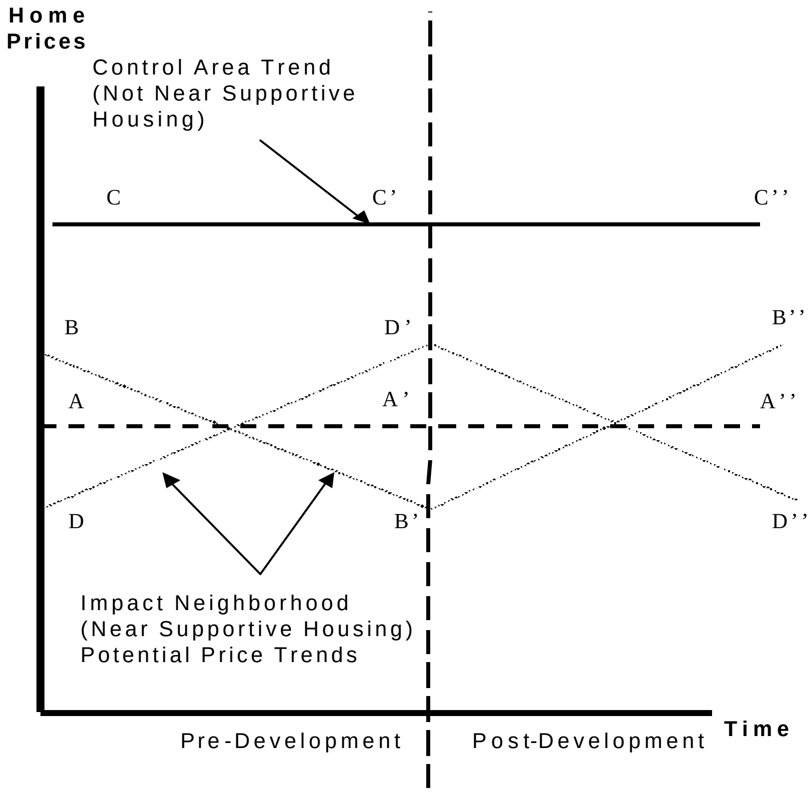
Note: Positive Impact, Absolute Increase in Trend: A-A'-D'''
Positive Impact, Absolute Upward Shift in Level: A-A'-D-D''
No Impact, No Relative Change in Trend from City-Wide Trend: A-A'-A''
Negative Impact, Relative Decrease in Trend: A-A'-B
Negative Impact, Absolute Downward Shift in Level: A-A'-B'-B''

The foregoing presentation of the difference in differences approach noted several times the importance of measuring both levels and trends in prices both before and after the development of supportive housing. This is more than a minor technicality, and as such deserves fuller explication. There are two examples in the relevant literature on supportive housing impacts of approaches that are similar to the one advocated here, but they fundamentally contrast only the pre- and (area-adjusted) post-development average *levels* of home prices in the impact neighborhoods (Galster and Williams, 1994; Colwell, Dehring and Lash, 2000). The approaches in these studies offer substantial improvements over the three traditional approaches in confronting the omitted variables/causation challenges, but nevertheless have weaknesses. Pre/post comparisons of price levels alone may obscure significantly different price trends pre- and post-development, thereby leading to erroneous conclusions. Our argument is illustrated with the help of Figure 2.

Assume for simplicity that during the period in question there is no change in home prices in control areas (line C-C'-C''). But suppose that we also observe no change between pre- and post-development periods in the *average* level of prices in impact neighborhoods proximate to the supportive housing development (i.e., average A-A' identical to average A'-A''). One might be tempted to deduce no impact from the development, but such might be seriously in error. As illustrated in Figure 2, such a comparison might well overlook pre- and post-development trend shifts suggesting either strong positive (B-B'-B'') or negative (D-D'-D'') impacts. Note that Colwell, Dehring and Lash (2000) indeed control for the average rate of price appreciation in the impact neighborhood measured over both pre- and post-development periods, but this clearly would not have yielded a valid conclusion in the situation shown in Figure 2. They would have estimated zero average appreciation and no pre/post differences in average levels of prices, thereby leading to the conclusion of no impact.

Figure 2

Illustration of Potential Types of Home Price Impacts from Supportive Housing (pre/post levels of prices method)



Note: Positive Impact, Absolute Increase in Trend: B -B'-B'''
Negative Impact, Absolute Decrease in Trend: D -D'-D''

IV. ECONOMETRIC ISSUES

Spatial autocorrelation is analogous to serial correlation and, if left uncorrected, would lead to biased parameter estimates and misleading t-tests. To test for this potential problem, we employed a specification that Can and Megbolugbe (1997) found to be robust. We calculated the *spatial lag* of the dependent variable (house price or crime rate) and included it in our model as an independent variable. The spatial lag is a spatially weighted average of all of the observations of the dependent variable within a certain distance from the reference observation. Consistent with the approach of Can and Megbolugbe, we used the inverse of the distance (1/d) as the spatial weight. The formula for the spatial lag is:

$$\text{Spatial Lag}(P_i) = \sum_j \left[(1/d_{ij}) / \sum_j 1/d_{ij} \right] P_j \qquad [3]$$

where P_i is the sale for which we are calculating the spatial lag, d_{ij} is the distance between sales i and j, and P_i is one of the set of all sales within distance D of P_j and that occurred within the six months prior to the date of P_i.

One of the key parameters is the selection of the cutoff distance D. The choice of D depends upon the researcher's knowledge and assumptions as to how far the supposed spatial dependence is likely to be felt, but can be tested by evaluating the effectiveness of different choices. We assumed that a minimum cutoff distance of 2,000 feet would be necessary to see a spatial effect. We calculated spatial lags at this distance, but also tested spatial lags with cutoffs of 5,000 and 10,000 feet to examine the possibility that spatial dependence may exist over a larger area.

Because of the large numbers of house sales, calculating the spatial lag is computationally intensive and very time consuming for the property value models. We therefore conducted several test cases before attempting to create spatial lags for the entire set of house sales. We calculated spatial lag variables for three census tracts and estimated one of our model specifications first without any spatial lag variable, and then trying each of the spatial lag variables in turn. The test was whether the addition of the spatial lag variable significantly improved the goodness of fit (R^2) of the model.[10] None of the spatial lag variables improved the model fit by any substantial amount, and we therefore excluded it from the model.

Spatial heterogeneity, sometimes known as spatial submarket segmentation, refers to the systematic variation in the behavior of a given process across space. Here, the issue is whether the parameters of the hedonic price equation are invariant across space or whether they assume different values according to the local socioeconomic, demographic, and/or physical contexts of the various neighborhoods across a metropolitan area. If such were the case, the error term ? would be heteroskedastic.

To deal with this issue we employed the "spatial contextual expansion with quadratic trend" specification as suggested by Can (1997). This method involves adding to the models the latitude (X) and longitude (Y) coordinates of each observation in the following variables (normalized so that zero values represent the center of the city): X, Y, XY, X^2, and Y^2. Higher numerical values of X (Y) signify increasing distance from the center of the city heading west (north). These variables typically proved statistically significant in our specifications (see Annex D for details), suggesting that our various controls for local fixed effects needed further supplementation from these spatial coordinates.

In addition to the aforementioned spatial econometric tests, standard heteroskedasticity tests using the Goldfeld-Quandt and other procedures were conducted (Intriligator, 1978: 156).

Though they proved inconclusive, to be conservative we used the White (1980) covariance matrix to estimate standard errors.

V. DATA SOURCES AND ISSUES

Single-Family Home Sales Data

The most complete and accurate source of home sales data available in Denver is the property tax rolls maintained by local property tax assessment offices. We purchased a complete set of property tax roll records for Denver from the private data vendor Experian. The data contain all of the information available from the tax rolls on the property itself (including address, number of rooms, square footage, and type of construction), as well as the dates and amounts of the last two sales for each property. A supplementary sales history database was also purchased from Experian to identify properties that sold more than twice during the study period.

Both the tax roll and sales history files were geocoded to match street addresses with latitude and longitude coordinates, Census geographic identifiers (*i.e.,* state, county, tract, and block), and US Postal Service ZIP+4 codes.[11] We were able to geocode 98 percent of property addresses in Denver to an exact street address or to a ZIP+4 centroid.[12] Sales records that could not be geocoded to at least this level of precision were excluded from the analysis.

From the geocoded set of sales data, we selected only sales of single-family homes. To obtain the most precise estimates in our models, we wished to exclude from our database sales that were recorded erroneously, highly idiosyncratic, and did not represent arms-length transactions. In this vein, we eliminated the top and bottom two percent of all observations according to sales price and land area.

Supportive Housing Facilities Data

Data on the location and characteristics of the supportive housing sites were obtained from the Denver Zoning Commission and the Colorado Department of Health and Environment. Their databases identified 146 supportive housing sites that were announced at some time during our study period: between 1987 and 1997. The databases included information on the program's address, the year the program started, program type, and number of beds (residents). We identified the supportive housing locations by geocoding the addresses of the sites. We were able to geocode 90 percent of the records to an exact street address and an additional 10 percent to a ZIP+4 centroid.

We conducted our econometric analysis of property value impacts on a subset of the 146 pre-1998 vintage supportive housing sites. We will refer to these subsets as *analysis sites*. These analysis sites were chosen because they: (1) were the first supportive housing facility within 2,000 feet of the site where they were approved (and thus had a "pre" period with sales uncontaminated by other such facilities nearby); and (2) had sufficient observations of housing sales in both the pre- and post-announcement periods to allow us to estimate impacts reliably in the immediate vicinity of the sites. Note that we had no information on when supportive housing facilities began construction/rehabilitation activities or opened for clients, only when they were officially approved by the Denver Zoning Board at public hearing. Thus, our "post" period measures the amalgam of announcement/approval, physical development, and occupancy/operation effects on property values, although for simplicity we will refer to it as "post-announcement" hereafter.[13]

In particular, to operationalize the pre/post econometric specifications described above for the property value impact model, we were restricted to those supportive locations having: (1)

no other supportive sites within 2,000 feet when it was announced; and (2) an announcement date that yields sufficient observations of sales prices for a minimum of two years both pre- and post-announcement. These restrictions reduced our sites to 29. To ensure the reliability of our estimates, we also imposed the restriction that a supportive site required an average annual rate of single-family homes sales of at least 2.0 in each of the concentric circle ranges of 0-500 feet, 501-1,000 feet, and 1,001-2,000 feet both prior to and subsequent to announcement date. This restriction produced our final sample of 11 analysis sites. They are described in Table 1; a map identifying their locations is available from the first author.

Note that all of the analysis sites are small-scale, Special Care facilities representing a wide range of clientele, including senior care, substance abuse rehabilitation, mental health, developmental disabilities, children with disabilities, and hospice. Thus, the property value model results should be interpreted as stemming from a set of small-scale facilities engaged in a wide range of supportive activities, but not qualifying as community corrections or homeless shelters/transitional facilities, which typically are more adamantly opposed by neighborhoods (Takahashi and Dear, 1997).

The subset of home sales to be used in the econometric analysis was chosen in relation to these analysis sites. We used all single-family home sales that either were: (1) not within 2,000 feet of any announced supportive site (or one for which we had no announcement date), or (2) within 2,000 feet of one (or more) of our analysis sites after announcement. We omitted sales that were within 2,000 feet of any other announced supportive site(s) but did not qualify as an analysis site(s). This yielded a sample of 45,601 sales and permitted unambiguous tests based on our pre/post principles of deciphering property value impacts.

Table 1 - Characteristics of Supportive Housing Sites for Property Value Impacts Analysis

Neighborhood	Program type	Zoning	Starting Year	Number of Beds
Berkeley #1	Senior Special Care	R2	1989	8
Clayton	Hospice	R2	1993	8
Hilltop	Developmental Disabilities	R0	1992	8
Montbello #1	Developmental Disabilities	R1	1990	4
Montbello #2	Children's Home	R1	1992	8
Montbello #3	Substance Rehabilitation	R1	1995	12
Montbello #4	Unknown	R1	1995	5
S. Park Hill	Mental Health	R1	1990	8
Speer #1	Mental Health	R3	1993	6
Virginia Village	Personal Care Boarding Home	R1	1992	4
Washington Virginia Vale	Substance Rehabilitation	R1	1989	8

VI. EMPIRICAL RESULTS

Overall, our house price impact models performed well; see results for price impact variables in Table 2.[14] Figure 3 summarizes the main findings graphically. It shows the relative percentage differences in single-family home sales prices in 1,001-2,000 foot proximity to supportive housing sites, compared to baseline prices for similar dwellings elsewhere in the same census tracts but not within 2,000 feet of any supportive sites. The vertical axis on the graph indicates the percentage differences in house prices over the baseline. The horizontal axis indicates time, starting with the beginning of our study period, the first quarter of 1987, and extending to the end of our study period. The first dotted vertical line indicates a representative starting date chosen as the point of first announcement of the archetypal supportive housing facility. Therefore, the section of the graph to the left of the dotted line is the estimated relative price pattern *before* the supportive housing site was announced, and the section to the right of the dotted line is the price pattern *after* the site was announced.

Sales Price Patterns Before Supportive Sites are Announced: Evidence of Location Bias

The results show that in Denver there was a systematic tendency for our sample of supportive housing sites developed during the early 1990s to be located in relatively lower-valued and/or declining-value pockets within census tracts. The negative coefficients on the distance-specific, fixed-effects dummy variables (DAll) indicate that home prices within 500 feet of areas that were to be acquired for supportive housing facilities were 6 percent lower, on average, than prices of comparable homes elsewhere in the census tract. The corresponding DAll estimates for the 501-1,000 feet and 1,001-2,000 feet distance rings were roughly 2 percent decrements, though of lower statistical significance. Moreover, in the 501-1,000 feet and 1,001-2,000 feet distance rings there was a statistically significant pattern of relative inter-temporal decline in value compared to comparable homes in other parts of the same census tract (see $Time_{1,000}$ and $Time_{2,000}$ results).

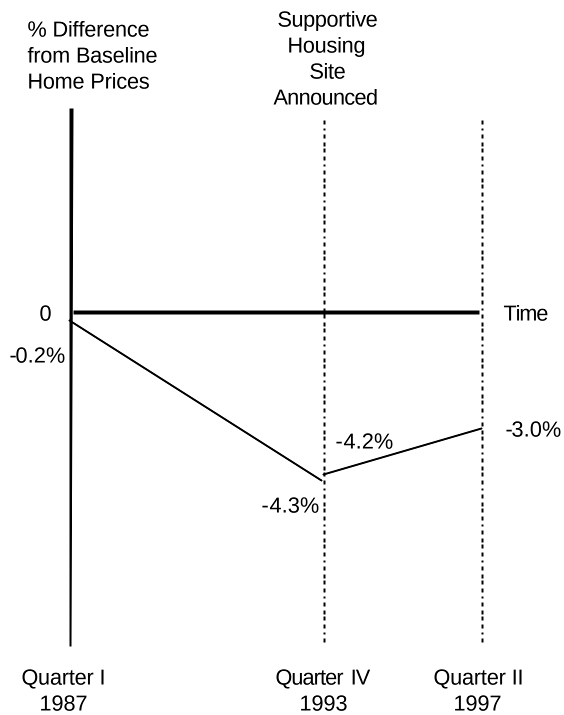
Table 2 Estimated Regression Parameters for Price Impact Variables
(dependent variable = ln[sales price single-family home])

Variable	Coefficient	White's Standard Error
Dall $_{500}$	-0.0626	[.024]**
Dall $_{1,000}$	-0.0195	[.016]
DAll $_{2,000}$	-0.0165	[.011]
DPost $_{500}$	0.0134	[0.033]
DPost $_{1,000}$	0.0308	[0.018]
DPost $_{2,000}$	0.0205	[.010]*
Time $_{500}$ (/100)	0.0016	[.0014]
Time $_{1,000}$ (/100)	-0.0022	[.0007]**
Time $_{2,000}$ (/100)	-0.0015	[.0006]**
TrPost $_{500}$ (/100)	-0.0005	[.0024]
TrPost $_{1,000}$ (/100)	0.0016	[0.0013]
TrPost $_{2,000}$ (/100)	0.0024	[0.0011]**

** = coefficient statistically significant at .01 level, two-tailed test
* = coefficient statistically significant at .05 level, two-tailed test

Note: Parameters for all other control variables found in Annex A

Figure 3
Estimated Relative Price Trends At 1,001- 2,000 Feet From Supportive Housing Site(s)*
(Relative to Baseline Areas of Same Tracts Not Within 2,000 Feet of Such Sites)**



* There were no statistically significant impacts of supportive housing observed at distances closer than 1,000 feet
** Baseline prices control for seasonal and county-wide quarterly trends, plus housing stock characteristics.

Interviews we conducted with key informants in Denver provided support for two of the aforementioned potential explanations of this finding. First, providers of supportive housing often acquired vacant, sometimes deteriorated property for their facilities. Insofar as these properties likely had been generating negative externalities for the surrounding neighborhood for oftentimes-considerable periods prior to their acquisition, these areas would tend to have lower values. Second, developers seemed to search more intensively for buildings for purchase in lower value/declining areas where "they could get the most building for the money," thereby stretching their scarce programmatic resources as far as possible. Whatever the reasons, it is clear that location biases are present in siting of supportive housing in Denver.

Home Price Impacts of Supportive Housing Proximity

The regression showed no statistically significant evidence of negative single-family home price impacts associated with proximity to our analysis set of eleven supportive housing facilities. On the contrary, there is evidence of a *positive* impact. During the early to mid-1990s, we observed relative increases in property value levels and trends as a result of 1,001-2,000 foot proximity to these supportive housing sites being announced and, later, operated. We reiterate that these results were produced by a set of small-scale, widely separated special care facilities, with no community correctional facilities or homeless shelters included. Because a wide range of other special care programs is represented among our analysis sites (Table 1), however, no conclusions should be drawn about the impacts of particular program types.

As shown in Figure 3, after a supportive housing facility was announced, sales prices reversed their previous relative downward trend evinced before announcement. Fourteen quarters after announcement, prices at this distance were only 3.0 percent less than the baseline within the census tract; immediately preceding announcement they were 4.3 percent less. Had the pre-announcement trend persisted, by second quarter 1997 the properties within this distance ring would have been 6.5 percent below baseline. Thus, on average across all eleven supportive sites announced during our study period, sales prices 3.5 years after announcement were about 3.5 percentage points higher within 1,001-2,000 feet of a supportive facility than they would have been in the facility's absence.

Why No Price Impacts at Closer Ranges?

What is curious about the foregoing results is that statistically significant impacts were observed only in the 1,001-2,000 feet distance ring, and not closer to the supportive housing sites. Smaller sample sizes of home sales within this smaller (within 1,000 feet) area may provide a partial answer. We believe that these results are also consistent, however, with the notion of "countervailing externalities" that we wish to advance for consideration. We hypothesize that supportive housing sites may (with particulars depending on neighborhood context, structure, clientele, and management) generate *several* distinct types of externalities, some positive and others negative, some extending impacts relatively short distances and others considerably farther. At certain distances where both positive and negative externalities are operating they can, in effect, cancel each other out, yielding no net effects on observed sales prices at that range. But, at other distances one sort of externality may predominate. In our case, the results are consistent with negative externalities that decay rapidly as one moves away from the supportive housing site (disappearing entirely at an approximate distance of

1,000 feet), superimposed on positive externalities that have a much more gradual distance-decay function.

Interviews with key informants in Denver and reviews of the literature suggest that several, potentially countervailing externalities may be at play when it comes to supportive housing, each with its own associated range of impact:

- Increased parking pressures and traffic congestion: negative externality, usually confined close to site
- Resident behavior (noise, littering, e.g.) on site: negative externality, usually confined close to site
- Resident behavior (pan-handling, crime, e.g.) off site: negative externality, may extend farther from the site.
- Rehabilitation or construction of supportive housing facility: positive externality signaling investment in area, spark to investor confidence, and possibly removing of blighting prior use of property, may extend far from the site.
- Upkeep of property once developed: could be positive or negative externality, depending on intensity and in comparison to others on the block-face, primarily operative within sight of facility but may extend further.

Here, the econometric results are consistent with the notion that positive externalities associated with improving the property before the supportive facility opens and/or comparatively superior maintenance of the facility during operation predominate in the 1,001-2,000 foot distance ring, whereas at closer proximity positive and negative externalities are countervailing to the point where no net impact is produced. Informants told us that rehabilitation of many of the analysis sites resulted in significant external improvements. Moreover, better-than-blockface average maintenance levels of the analysis sites were, indeed, revealed by researchers' personal inspections. A related study of Denver supportive housing was unable to identify any neighborhood crime impacts (Galster et al., 2002), so it is likely that only negative externalities with small spatial extent may be present. What this implies is that home buyers more than 1,000 feet away from a supportive housing site may have been unaware of its designation as such, instead perceiving only a well-maintained structure where an undermaintained one used to be.

Are the Results Robust to Sample Variations?

To ascertain the strength of the aforementioned results, we expanded our analysis sample to include 18 additional supportive facilities developed from 1989-1995 and their associated proximate home sales. Unlike our original eleven-site analysis sample, which required a minimum annual average of two home sales per distance ring during the study period, these additional sites did not satisfy any minimum sales criteria. Thus, while this enhanced sample provides a larger number (29) and greater geographic and programmatic diversity of supportive sites, over half of its site observations provide pre-/post-announcement estimates of price levels and trends that may be quite imprecise.

Nevertheless, the results produced by regression analysis of this enhanced sample generally correspond with those of the more precise sample. Supportive housing facilities tend to be developed in areas which are lower-valued than average for the census tract. Not surprisingly, the statistical significance of the impact variable coefficients is reduced, but the positive coefficient in the 1,001-2,000 feet range persists and no statistically significant coefficients suggest negative price impacts in any distance range.

Does Using the Right Specification Matter?

Earlier we claimed that, because supportive housing is located systematically in lower-valued/declining neighborhoods, traditional econometric specifications that merely ascertain whether there is any *ex post* cross-sectional association between property values and proximity to extant supportive housing will likely be biased toward finding a negative relationship. To test this explicitly, we estimated a simplified version of our model wherein only the $DPost_x$ variables were included as measures of proximity effects. This mimics most conventional specifications (excluding Galster and Williams, 1994 and Colwell, Dehring and Lash, 2000).

The only statistically significant coefficient proved to be that of the $DPost_{500}$ variable and it was *negative.* Using the conventional approach, the analyst thus would have erroneously deduced a negative externality impact within 500 feet of a supportive housing facility. This misleading result is exactly the opposite of what we found with a superior specification. Indeed, using the correct econometric specification matters greatly in this case.

VII. CONCLUSIONS, CAVEATS, AND IMPLICATIONS

In this study we analyzed statistically the single-family home property value impacts of eleven, small-scale supportive housing facilities that were announced in Denver during 1989-1995 and met certain requirements regarding sales data adequacy and minimum separation from any extant supportive facilities. These facilities comprised a wide range of supportive housing program types, but included no homeless shelters or community corrections facilities.

We employed an econometric specification based on a difference in differences method, which we argued provides a more accurate and precise measure of the causal impact than prior approaches. We found no evidence that the announcement and development of these supportive housing sites was associated with any *negative* impact on proximate house prices. On the contrary, the areas within 1,001 to 2,000 feet of these sites experienced both a post-announcement/operating *increase* in both general level and trend in house prices relative to the prices of similar homes in the same census tract not near such facilities. We hypothesized that this effect may have been produced by countervailing externalities (building rehabilitation vs. behaviors of residents) that varied in their spatial extent. This positive impact reversed a *relative decline* in house prices (compared to elsewhere in the census tract) that existed in these areas prior to the announcement of the supportive housing site, consistent with our expectations regarding site location bias.

We reiterate that our study was conducted for a particular set of supportive facilities in particular neighborhood contexts located in a city where developers of supportive housing were, for a substantial part of the study period, subject to stringent regulatory requirements regarding siting, design, size, and public notification. Thus, generalizations from the Denver experience should not be made cavalierly.

Were these empirical findings to have more general applicability, however, they would suggest the need for enhanced public education on the issue, because conventional fears about the impact of supportive housing do not appear to be justified in all cases. Our statistical results support opinion poll studies of other researchers in Denver and nationwide, which show that residents' actual experiences with supportive housing nearby are much more satisfactory than they had predicted (Wahl, 1993; Gould and O'Brien, 1997; and Cook, 1997). Other studies have documented the benefits of institutionalizing mechanisms of community education and participation in the siting approval process (Hogan, 1996: Ch. 7).

Regardless of the programmatic particulars that might be considered, the key policy lesson of this study should be kept in the forefront: *it **is** possible to develop supportive housing that generates positive externalities for the environs.* Neighbors, developers, operators, and regulators of supportive housing must be aware that if care is taken in siting, management, maintenance, community outreach, and oversight, a needed form of assisted housing can be provided in a fashion providing benefits to multiple stakeholders.

Our study offers important methodological implications for future research on measuring neighborhood externalities generated by not only supportive housing, but by a wide variety of facilities. We have shown that, when there are forces causing facilities to be developed in a group of neighborhoods with unrepresentative housing price profiles, the traditional econometric specification of examining cross sectional relationships between property values and proximity to operating facilities will likely yield biased estimates of externality effects. Indeed, in our case the traditional specification yielded the opposite conclusion from that produced from our difference in differences specification. Indeed, using the right specification matters.

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Annex A

Regression Estimates for Structural, Quarter, and Spatial Variables

(Dependent Variable: log of sale price)

Variable	Parameter Estimate	Standard Error	T for H0: Parameter=0	Prob > |T|
Has 1/1.5 bathrooms	.268045	.04283260	6.258	0.0001
Has 2/2.5 bathrooms	.376120	.04290340	8.767	0.0001
Has 3+ bathrooms	.411640	.04306480	9.559	0.0001
Brick exterior wall	.025661	.02277134	1.127	0.2598
Concrete ext. wall	-.072401	.02410473	-3.004	0.0027
Frame ext. wall	-.046042	.02274157	-2.025	0.0429
Masonry/frame wall	-.032806	.02322196	-1.413	0.1577
Stucco ext. wall	-.130872	.02362710	-5.539	0.0001
Has 1 fireplace	.078045	.00251876	30.985	0.0001
Has 2+ fireplaces	.126698	.00944012	13.421	0.0001
Building 1.5 story	.221291	.00560860	39.456	0.0001
Building 2+ stories	.246469	.00372370	66.189	0.0001
Built 1900-19	.082103	.00684210	12.000	0.0001
Built 1920-39	.132852	.00719429	18.466	0.0001
Built 1940-49	.096309	.00786387	12.247	0.0001
Built 1950-59	.093905	.00805138	11.663	0.0001
Built 1960-69	.086543	.00961658	8.999	0.0001
Built 1970-79	.131815	.00994627	13.253	0.0001

Built 1980-89	.248328	.01013783	24.495	0.0001
Built 1990 or later	.143075	.01136439	12.590	0.0001
base square feet	.000345	.0000045	75.370	0.0001
total square footage (1,000s)	.015284	.00078	19.705	0.0001
Yr/Quarter:87/2	-.001867	.01217414	-0.153	0.8781
Yr/Quarter:87/3	.020483	.01229689	-1.666	0.0958
Yr/Quarter:87/4	-.034536	.01165529	-2.963	0.0030
Yr/Quarter:88/1	-.044782	.01260926	-3.552	0.0004
Yr/Quarter:88/2	-.067888	.01153055	-5.888	0.0001
Yr/Quarter:88/3	-.063535	.01120969	-5.668	0.0001
Yr/Quarter:88/4	-.076593	.01150949	-6.655	0.0001
Yr/Quarter:89/1	-.110466	.01210390	-9.126	0.0001
Yr/Quarter:89/2	-.083423	.01123647	-7.424	0.0001
Yr/Quarter:89/3	-.099631	.01124780	-8.858	0.0001
Yr/Quarter:89/4	-.105340	.01156477	-9.109	0.0001
Yr/Quarter:90/1	-.126127	.01205072	-10.466	0.0001
Yr/Quarter:90/2	-.112382	.01119564	-10.038	0.0001
Yr/Quarter:90/3	-.104054	.01113754	-9.343	0.0001
Yr/Quarter:90/4	-.119418	.01142320	-10.454	0.0001
Yr/Quarter:91/1	-.130489	.01200363	-10.871	0.0001
Yr/Quarter:91/2	-.067835	.01120645	-6.053	0.0001
Yr/Quarter:91/3	-.055253	.01128423	-4.896	0.0001
Yr/Quarter:91/4	-.031890	.01141285	-2.794	0.0052
Yr/Quarter:92/1	-.025441	.01185791	-2.146	0.0319

Yr/Quarter:92/2	.032995	.01098232	3.004	0.0027
Yr/Quarter:92/3	.042634	.01118283	3.812	0.0001
Yr/Quarter:92/4	.046831	.01111663	4.213	0.0001
Yr/Quarter:93/1	.076378	.01165738	6.552	0.0001
Yr/Quarter:93/2	.128530	.01108247	11.598	0.0001
Yr/Quarter:93/3	.161046	.01112406	14.477	0.0001
Yr/Quarter:93/4	.174723	.01123956	15.545	0.0001
Yr/Quarter:94/1	.203386	.01152251	17.651	0.0001
Yr/Quarter:94/2	.266207	.01108233	24.021	0.0001
Yr/Quarter:94/3	.267098	.01138012	23.471	0.0001
Yr/Quarter:94/4	.283785	.01149518	24.687	0.0001
Yr/Quarter:95/1	.316987	.01185636	26.736	0.0001
Yr/Quarter:95/2	.362084	.01141081	31.732	0.0001
Yr/Quarter:95/3	.385029	.01138303	33.825	0.0001
Yr/Quarter:95/4	.392689	.01165871	33.682	0.0001
Yr/Quarter:96/1	.434453	.01185668	36.642	0.0001
Yr/Quarter:96/2	.454140	.01142199	39.760	0.0001
Yr/Quarter:96/3	.488965	.01155346	42.322	0.0001
Yr/Quarter:96/4	.492473	.01215815	40.506	0.0001
Yr/Quarter:97/1	.509503	.03864655	13.184	0.0001
Yr/Quarter:97/2	.548674	.03253930	16.862	0.0001
Yr/Quarter:97/3	.527354	.03997659	13.192	0.0001
X coordinate	-1.177196	.23948228	-4.916	0.0001
Y coodinate	-6.467223	.30442221	-21.244	0.0001
X*Y	5.068835	3.53596516	1.434	0.1517

X*X	-1.076674	1.14325896	-.942	0.3463
Y*Y	-56.013024	3.76862556	-14.863	0.0001

F = 951.463 Prob. >a = 0.0001

Adj R-sq = 0.8096

Endnotes

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[1] For those who are unfamiliar with this area, the City and County of Denver, with a population of roughly one-half million, are coterminous jurisdictions. Throughout, we will refer to the City and County of Denver as simply, "Denver."

[2] The comparison often is accomplished with the aid of multivariate statistical procedures to control for differences in the properties being sold pre- and post-announcement of the site.

[3] Colwell, Dehring and Lash (2000) estimate a neighborhood specific trend but it represents an average appreciation rate over both pre- and post-development periods. As explained below, this leaves room for misinterpretation in some cases.

[4] Based on previous work (Rothenberg et al., 1991: ch. 13), we will use a semi-log form, that is, expressing the logarithm of sales price as a linear function of the house and neighborhood characteristics and other dependent variables.

[5] A site refers to a unique street address for a single or multi-family property.

[6] The perceptive reader will recognize that our specification represents an extension of the "interrupted time series trend" specification, with the addition of an interruption in intercepts, as well as a control for interruptions that occur not only in the target areas but city-wide (Bloom and Ladd, 1982; Bloom, 2003).

[7] Or, expressed equivalently, the post-development difference C''-A'' is unchanged from the pre-development difference C'-A'.

[8] Or, expressed equivalently, the post-development difference C''-D'' (or C''-D''') is smaller than the pre-development difference C'-A'.

[9] Or, expressed equivalently, the post-development difference C''-B (or C''-B'') is smaller than the pre-development difference C'-A'.

[10] We actually calculated and tested six alternative specifications of spatial lag for each census tract. We created spatial lag variables for the sales price and for the log of sales price using 2,000, 5,000, and 10,000 foot cutoffs. To give some idea of the computationally intensive nature of determining spatial lag, calculating six spatial lag variables for each of six census tracts took over 32 hours on a Pentium computer.

[11] Geocoding was done using MapMarker software from MapInfo Corporation.

[12] ZIP+4 codes are roughly equivalent to a city block. The centroid of a ZIP+4 would be the geographical center of a block.

[13] Key informants indicated that physical development of sites typically began soon after approval, with first occupancy averaging a year after approval. Inspection revealed that all analysis sites were, in fact, developed.

[14] The adjusted R-square was 0.82. Not surprisingly given the exceptional sample sizes, virtually all of the [Struct], [Tract], and [Quarter] control variables evinced coefficients that were significantly different from zero; See Annex A. All the coefficients of the [Struct] characteristics of homes proved to have the expected signs.