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Measuring Amenities and Disamenities in the Housing Market: Applications of the Hedonic Method

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Chapter 1
Measuring Amenities and Disamenities in the Housing Market: Applications of the Hedonic Method

Joshua Hall, Kerianne Lawson, and Jacob Shia

1.1 Introduction

1.2 Introduction

The hedonic method is an econometric technique used to measure the value of or demand for a good. By considering the characteristics of the good, the method allows for analysis of how each part contributes to the good's value (Lancaster, 1966; Rosen, 1974). Houses have many attributes that are not directly sold but which affect their value. This can include parts of a house – such as a pool or half bathroom – but also publicly-provided goods whose usage is associated with the home, such as public schools. This explains the widespread use of the hedonic method in economics. There are thousands of research articles that employ the hedonic model and there are various modifications and contexts studied (Ekeland et al., 2004; Goodman and Thibodeau, 2003; Maclennan, 1977; Malpezzi, 2002; Simons and Saginor, 2006).

The hedonic method is useful when researchers want to understand how some external factor that can not be directly purchased affects house prices. The external factors are often broken down into two categories, amenities and disamentities. In theory, amenities should be incorporated into the value of a home. An amenity has a positive relationship with house prices if it is desirable to a specific group, or groups, that have a higher willingness to pay for the home due to its relationship with or proximity to the amenity. A disamenity would have a negative relationship with house prices.

In many cases, distance to the amenity affects house prices, or the amenity is specific to a certain area or neighborhood. Therefore, the real estate economics literature has extended the general hedonic method to include spatial analysis (Anselin, 1998; Anselin and Lozano-Gracia, 2009; Basu and Thibodeau, 1998; Bourassa et al., 2007). Locational aspects of the home are related to an amenity and are therefore incorporated into the econometric model. First, these models may include

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a dummy variable to designate whether the observations are positioned in a geographic area. Additionally, models can use the relative positioning of an observation or distance from an observation to the amenity in question to understand the spatial relationships.

Aside from physical attributes of the house itself, there are many external factors that could affect an individual's willingness to pay for a house. Therefore, there are many opportunities for research that applies the hedonic method. The following chapters of this book identify various amenities and disamenities, discuss which houses would be impacted by their presence, and then measure the relationship between the amenity or disamenity with house values. This book's objective is to be a guide of the current real estate economics literature through applications of the hedonic method. It is our hope that readers can gain a better understanding of the hedonic method and feel inspired to contribute new research to the field.

1.2.1 Environment

Many articles in the hedonic literature focus on the relationship between environmental factors and real estate (Boyle and Kiel, 2001). This is because the environment can be affected by human activity or nature, and there are many components of the environment. To name a few, scholars study the effect of storms and wildfires (Cohen and Coughlin, 2009; Gourley, 2021; Hansen et al., 2014), air quality (Amini et al., 2021; Chattopadhyay, 1999; Chay and Greenstone, 2005; Lang, 2015; Nelson, 1978; Nourse, 1970; Zabel and Kiel, 2000), water quality (Walsh et al., 2011), and invasive species (Hansen and Naughton, 2013; Zhang and Boyle, 2010) on house prices.

Cohen and Coughlin (2009) examine the differences in the expectations of flooding (based on the FEMA flood zones) and actual flooding in New York City after Hurricane Sandy in 2012. For those who were further from the storm surge than expected, there was no price effect. However, for those who experienced a negative shock, which meant their homes were closer to the storm surge than expected, there was a significant difference in house prices depending on their distance from the storm surge, about 6-7% for every mile. With time this effect dissipates, as the shock of the storm wears off.

Liu et al. (2017) study the impact of water quality on house prices in Narragansett Bay on the north side of Rhode Island. They find that poor coastal water quality corresponds with negative house prices, and this effect diminishes with distance to the shoreline. They also find evidence that house prices are most impacted by extreme environmental conditions, which make the poor water quality more noticeable.

In Chapter 6 of this volume, Meder and Johnson (2021) measure the effect of the zebra mussel infestation of lakes in Wisconsin on local house prices. While zebra mussels are devastating to the native species, they are also associated with increased water clarity. Meder and Johnson (2021) find no measurable effect on lakefront properties on infested lakes. This finding may explain the difficulty around enforcing policies aimed at containing the spread of zebra mussel infestation.

When the environment is affected by an industry, or accident, it may be difficult to accurately measure the environmental impact. Yet, it is still possible to measure how this event has affected house prices. For example, after the Three Mile Island accident, proximity to the nuclear plant meltdown would be considered a disamenity (Nelson, 1981). Additionally, the environmental externalities associated with animal agriculture facilities has a negative impact on house prices (Kuethe and Keeney, 2012).

Noise is another externality associated with industry, construction, and development. There is a large literature that uses the hedonic method to measure the disamenity value of noise. For example, airports (Nelson, 1980), windmills (Jensen et al., 2014), and traffic (Swoboda et al., 2015; Theebe, 2004), are all sources of noise that can affect house prices.

Jensen et al. (2014) employ a dataset of over twelve thousand homes within 2,500 meters of a wind turbine and find that wind turbines pose significant and negative effects on house prices. They look at two different ways in which wind turbines may affect the desirability of a home. First, visual pollution, which means that the wind turbines are visible from the home. This could have a negative impact as it makes the surrounding land seem less rural and perhaps ruin a good view of the terrain. Next, they measure the impact of the noise pollution generated when the turbines run on house prices. They find that there is a negative impact of about 3% for visual pollution and 3-7% for noise pollution, depending on the specification.

In Chapter 4, Lawson (2021) measures the impact of an addition of a runway at the airport in Columbus, Ohio on house prices. The airport's runways only run east-west, making a cone of noise pollution projecting from each side of the airport, but very little noise pollution occurs to the north and south of the airport. This provided a control group of homes that are a similar distance to the airport, but are not experiencing the same noise pollution. Lawson finds that the airport's expansion led to an increase in air traffic and noise, which resulted in a decline in house prices by 2-10%, depending on the distance from the airport.

1.3 Crime

Many studies have shown that proximity to registered sex offenders is negatively related to house prices (Caudill et al., 2015; Linden and Rockoff, 2008; Pope, 2008). Criminal activity in general is also associated with lower house prices, and can even offset the value of other amenities, like parks (Troy and Grove, 2008). House prices are negatively associated with drug-related deaths (Ajzenman et al., 2015), violence (Bishop and Murphy, 2011), terrorism (Besley and Mueller, 2012), when they occur within a close distance to a house.

In Chapter 2, Bian et al. (2021) expand on the existing literature about the negative relationship between the proximity to registered sex offenders and house prices. Their contribution looks at the effects of clusters in a neighborhood and discuss the sorting mechanisms at work, as registered sex offenders would not be uniformly distributed across a city or neighborhood. The study finds that there is a discount associated with homes near a cluster of 4 or more registered sex offenders.

In Chapter 5, Alexander (2021) uses data from Baltimore to measure various impacts of crime on house prices. First, the overall level of crime, based on historical averages is not significantly related to house prices. However, when crimes occur near the house while the house is listed for sale do decrease house prices and this effect varies with the distance to the house. Additionally, there are differential effects on house prices depending on the type of crime that occurs nearby.

1.4 Location

There are many outdoor amenities and land-use that can positively affect the value of a home (Fischel, 2004; Heimlich and Anderson, 2001). In Chapter 1, Jauregui et al. (2021) contribute to this area of the hedonic literature by highlighting how open space amenities change over time. Their study shows that preferences surrounding land use are dynamic and the impact on land values depends on the use, but also the surrounding areas. There are a number of papers in the economics literature that support this finding. Open and green space can be an amenity, depending on quality. For example, the literature finds that people are willing to pay a premium for proximity to parks (Espey and Lopez, 2000; Hoshino and Kuriyama, 2010; Poudyal et al., 2009). This is also the case with golf courses (Do and Grudnitski, 1995), woodlands (Garrod and Willis, 1992; Tyrväinen and Miettinen, 2000), lakes (Lansford Jr and Jones, 1995a,b; Nelson, 2010).

In Chapter 7, Stephens (2021) discusses the possible amenity and disamenity effects of living near a lake by examining houses on or near Lake Erie in Ohio. The previous literature suggests that buyers are willing to pay a premium for proximity to bodies of water, but in the Great Lakes Region, living near the lake may mean living near industrial sites or factories. Stephens (2021) finds that there is a positive effect on house prices for homes located directly on Lake Erie, but as the homes get further away from the lakefront, the effects begin to shift, depending on the proximity to manufacturing facilities. These results are meaningful for the Great Lakes Region as residents may positively value less industrialization near the lake and view the lake as a natural and recreational amenity instead.

Depending on the preferences of the buyers, neighbors can also influence the value of a home. For example, purchasing a home near a foreclosed or vacant property is often viewed as a disamenity (Immergluck and Smith, 2005; Mikelbank, 2008). On the other hand, there are many other neighborhood attributes that can be viewed as amenities. For example, residential community associations (Grace and Hall, 2019), historic landmark designation (Noonan, 2007), and historical districts (Zhou, 2021), correspond with higher property values.

In Chapter 3, (Ursey, 2021) examines how the proximity of rental homes may affect the value of homes sold in a neighborhood. Utilizing an econometric approach that is new to the literature with this research question, Ursey (2021) finds that rentals within a quarter mile of a sold home had a negative impact on price, while rentals between a quarter and half mile had a positive impact on price.

1.5 Locally provided goods

There is also a substantial literature associated with proximity to establishments that provide goods and services to the community. Some establishments may be valuable to those living nearby, but the congestion and noise associated with the establishment may harm house prices. For example, proximity to places of worship, are associated with house price premiums (Carroll et al., 1996; Do et al., 1994; Simons and Seo, 2011; Makovi, 2019; Babawale and Adewunmi, 2011). The literature about living near hospitals is mixed. It may be desirable if individuals value being close to where they can receive medical treatment, but on the other hand, noise from ambulances and helicopters can be seen as a drawback (Peng and Chiang, 2015; Rivas et al., 2019).

In Chapter 10, Russell Kashian et al. (2021) study the relationship between the distance to the nearest hospital and house price. In a contribution the existing literature on this research question, the article focuses on homes in rural and exurban locations in Wisconsin. Their results suggest that in rural communities living near a hospital is an amenity.

For schools, there are various factors that determine if the school is viewed as an amenity or disamenity. The literature finds that quality, type, and distance to schools are positively related to real estate prices (Bayer et al., 2007; Black, 1999; Bogart and Cromwell, 2000; Downes and Zabel, 2002; Figlio and Lucas, 2004; Jud and Watts, 1981). Whereas school district consolidation contributes to a drop in house prices (Brasington, 2004).

In Chapter 8, Batson (2021) studies a policy in Rockford, Illinois in 1996. The school district implemented a Controlled Choice school assignment policy, which resulted in students moving schools. There was considerable variation in the quality of schooling prior to the policy, and Batson (2021) finds that when the neighborhood school was high quality, homes in that neighborhood lost an average of 9.3% of their value. Alternatively, there was a positive effect on price for homes previously assigned to poor quality schools.

In Chapter 9, Hearn (2021) discusses the relationship between public-school quality and house prices. Using a natural experiment from Charlotte, NC, Hearn (2021) finds that people were willing to pay a premium to live in areas with higher exam scores, especially in the areas that were experiencing large increases in the pass rates on the standardized examinations. And unlike the results in Batson (2021), there is no evidence that there is a negative impact on price for houses that were reassigned for lower quality public high schools.

1.6 Conclusion

The papers in this volume provide an overview of how the hedonic method can be used to measure a number of amenities and disamenties. While some of these topics have been studied before, it is important to remember that willingness to pay depends on the tastes and preferences of individuals living and purchasing homes in an area. Empirical estimates from hedonic studies will therefore vary across space and over time. For this reason, there will always be a need for well done empirical work measuring amenities and disamenties using the hedonic method.

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Chapter 2
Open Space at the Rural-Urban Fringe: A Joint Spatial Hedonic Model of Developed and Undeveloped Land Values

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Abstract We examine the impacts of different open space amenities on sales prices of developed and undeveloped land in two time periods ten years apart in a rapidly urbanizing county in central Ohio. Buffers within 0.25 and 0.5 miles are created that include percentages of agricultural, residential, park and golf course uses around each land sale and are used as explanatory variables. We contribute to the literature by estimating a unified model that accounts for spatial heterogeneity as well as for cross-correlations in developed and undeveloped land sales. Our findings suggest interactions between the land markets examined are complex and dynamic.

2.1 Introduction

There is widespread concern that the largely unplanned production of new housing stock at the urban frontier reduces open space and other amenities, and that externalities associated with development ultimately lead to a reduction in the value of suburban residences (Fischel, 1999). Given that most land use decisions that shift land from agricultural to developed uses are "irreversible," at least within a 20–50 year period, if developers make decisions that do not account for the spillovers from development, the effects are capitalized into land values for long periods of time (Heimlich and Anderson, 2001). Numerous studies (e.g., Irwin (2002); Ready and Abdalla (2003)) have used the traditional hedonic price model to estimate homeowners' valuations of environmental amenities surrounding their homes. Their findings suggest that development processes that cause too much development at the rural fringe can have negative consequences. However, they have not addressed the fundamental questions posed by Fischel (1999) and Heimlich and Anderson (2001): Do unplanned development processes accumulate over time, and reduce the value of other nearby houses and land, and if there are effects, are they irreversible?

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To test these hypotheses, we focus on jointly estimating and comparing values of local open space amenities on developed (land with houses) and undeveloped land (land without houses). In contrast, traditional hedonic models estimate the value of amenities on developed land alone. In comparing estimates for developed and undeveloped land, we provide insights into the extent of spillover capitalization into the housing and land markets. The land market, in fact, may provide a clearer picture of the value of environmental amenities to households because one does not need to account for the potential endogeneity between structural amenities in homes and the value of nearby amenities caused by sorting. By estimating hedonic equations for undeveloped land, we are able to assess whether open space amenities are incorporated into the value of land before it is developed. Showing that amenities are incorporated into land values before development occurs suggests that the development process accounts for environmental amenities. Further, if the estimates for developed and undeveloped sites are the same sign and scale, it would indicate that the buyers and sellers of bare land pay close attention to the amenities that currently drive homeowner decisions.

In addition to assessing both developed and undeveloped land, we also assess how amenity values change over time by comparing estimates in 1988 with those in 1998 for the same county. If unplanned development processes reduce welfare, one would expect that development near home sites would have stronger disamenity effects over time, and that remaining open space would have stronger benefits. Further, one would anticipate that if developed or undeveloped sites in 1998 were endowed with the larger percentages of surrounding open space amenities than those in 1988 had, then the overall value of the homes would be greater. We also examine the counterfactual by predicting how 1988 prices would have been affected had land surrounding undeveloped sites been developed to the extent of the 1998 data. By assessing changes over time, we can also address whether the irreversible nature of land use change leads to permanent reductions in land value.

To empirically test the questions raised above, we develop methods to jointly estimate hedonic equations for bare land sales and house and land sales in the same county of Ohio for two different time periods, 1988 and 1998. A two-step procedure is developed to account for both the spatial nature of the data, and the likely correlation between house sales and land sales. To account for unobserved heterogeneity, we first run regressions on the developed and undeveloped land equations separately, employing spatial econometric techniques to account for unobserved variables that may be spatially correlated with the error terms. To account for potential correlation between land sales and house sales, a second regression is conducted where the two hedonic equations are estimated jointly as a seemingly unrelated regression (SUR). The covariance matrix from the first-stage spatial estimates is used in the SUR to account for unobserved variables related to the location of the observation. By using this two step procedure, we are able to account both for potential correlation with unobserved spatial variables, and for likely correlation between house and land sales. In addition, we are able to decompose the effects of open space amenities on land and capital.

This is obviously not the first study to assess how development and environmental amenities affect home values with hedonic methods, however it is the first we are aware of to investigate whether environmental values and the development process affect home and land values similarly. Existing studies have by and large concluded that the value of open space to local landowners depends on the type and quality of the open space. Irwin (2002) suggests that open space provides benefits, but that the largest benefits accrue only if the land is preserved from future development. Ready and Abdalla (2003), Palmquist et al. (1997), and Irwin et al. (2003) find that some types of farming operations, even though they provide open space for neighbors, can have negative consequences. Garrod and Willis (1992), Tyrväinen and Miettinen (2000), Espey and Owusu-Edusei (2001), and Paterson and Boyle (2002) show that the value of open space depends on the quality of the open space. In the context of this study, we account for differences between permanent and temporary protection of open space, although we do not account for different types of land use (i.e. livestock versus crop operation, or forest versus agricultural land.[1] There have been some hedonic studies investigating the value of agricultural land (e.g., Boisvert et al. (1997); Roka and Palmquist (1997); Xu et al. (1993); Kennedy et al. (1997), but these studies did not consider local environmental amenities located around the site. Bastian et al. (2002) and Pearson et al. (2002) examined the influence of scenic views on farmland values, but these studies were conducted in locations with particularly scenic viewing opportunities.

Thus, this paper innovates in several ways. First, it innovates by considering how open space amenities change over time. Over the 10 year period in the study region (Delaware County, Ohio), population and per capita income increased by around 60%, and 80% respectively, and around 8,843 agricultural parcels were developed into residential uses (Hite et al., 2003). One would expect in this context that the value of environmental amenities would change. Not only would the nature of the landscape be altered, but new urban migrants with different sets of values would increasingly inhabit the area. Most studies to date have only considered a single time slice, but the results in this study show that environmental values are susceptible to potentially large changes over time. Second, the paper accounts for likely correlation between land sales and house and land sales that occur in the urban frontier. Third, the paper shows how open space values and local residential development influence the value of two different types of sales that are important in regions with substantial development-–land sales and house and land sales. Our results indicate that local amenities are capitalized into both types of sales in similar ways and that the amenities are about the same proportion of the total value of the sale. This implies that farmers not only provide open space, but also receive a share of the benefits of open space.

2.2 Analytical Framework

Following Rosen (1974) we apply the hedonic framework to the markets for undeveloped and developed land characteristics. The traditional (non-spatial) hedonic undeveloped price model is specified as:

$$lnV_i = L_i\alpha_1 + O_i\alpha_2 + N_i\alpha_3 + S_i\alpha_4 + \varepsilon_i \tag{2.1}$$

where lnV is a vector of natural logarithms of undeveloped land prices, L is a matrix of land characteristics, O is a matrix of neighboring land use variables, N is a matrix of neighborhood characteristics, S is a vector of structural and other environmental variables, and the s represent parameters to be estimated for properties $i = 1...M$.

The traditional hedonic developed land price model adds H, a matrix of house characteristics, to the land only equation:

[1] The agricultural land in the study area is primarily row crop. Examination of different types of agricultural land uses in the area uncovers very few livestock operations of any kind.

$$lnP_j = L_j\beta_1 + H_j\beta_2 + O_j\beta_3 + N_j\beta_4 + S_{ij}\beta_5 + \varepsilon_j \tag{2.2}$$

where O, N and S are as before, the βs are parameters, and lnP represents a vector of natural logarithms of house prices for houses $j = 1...N$. Both the traditional hedonic undeveloped and developed price models are most frequently estimated using Ordinary Least Squares (OLS), corrected for heteroskedasticity.

The majority of previous undeveloped and developed land hedonic studies do not account for the possible spatial nature of housing and land data. The literature on spatial econometrics focuses on two types of spatial effects that can arise when sample data has a locational component: (1) spatial dependence and (2) unobserved spatial heterogeneity (LeSage, 1999). Spatial dependence refers to the fact that one observation associated with a location depends on other observations in adjacent locations. For example, the price of a house in a particular location depends on the prices and characteristics of neighboring houses. Unobserved spatial heterogeneity refers to variation in relationships over space. Anselin (1988) suggests that spatial effects lack uniformity, that is, the impact of spatial characteristics on the spatial units vary from one region to another. For example, the impact of house characteristics on house prices located close to a forest is different from the impact of housing characteristics on house prices located close to a lake. Traditional hedonic price models that use Ordinary Least Squares fail to account for these effects which in turn may result in biased, inefficient and inconsistent parameter estimates (Anselin, 1988; Brasington and Hite, 2005). In order to incorporate spatial effects into a regression model we consider a series of spatial model specifications, all derived from a general spatial model.

2.2.1 General spatial hedonic price model

The general spatial model is considered a point of departure for a series of specific spatial models that are obtained by constraining some of its parameters (Anselin, 1988). The general spatial model takes the form:

$$\begin{aligned} lnV_i &= \rho W_1 lnV_i + L_i\alpha_1 + O_i\alpha_2 + N_i\alpha_3 + S_i\alpha_4 + \varepsilon_i \\ \varepsilon_i &= \lambda W_2 \varepsilon_i + \mu_i \end{aligned} \tag{2.3}$$

where ρ is a spatial lag coefficient, W_1 is a spatial autoregressive weight matrix, λ is a spatial error coefficient, W_2 is a spatial weight matrix of disturbances, μ_i is assumed to be a vector of i.i.d. errors, and all other variables and parameters are defined as before. In this paper we also consider the following model specifications: the spatial lag model, which is found by imposing $\lambda = 0$ into the general spatial model, the spatial error model, which is obtained by imposing $\rho = 0$, and the spatial Durbin model, which is specified as:

$$\begin{aligned} lnV_i = {} & \rho W_1 lnV_i + L_i\alpha_{11} + O_i\alpha_{21} + N_i\alpha_{31} + S_i\alpha_{41} \\ & + W_2 L_i\alpha_{12} + W_2 O_i\alpha_{22} + W_2 N_i\alpha_{32} + W_2 S_i\alpha_{42} + \varepsilon_i \end{aligned} \tag{2.4}$$

From all these model specifications, we perform a series likelihood ratio tests to determine the best fitting models in each year for both developed and undeveloped land transactions.

2.2.2 Spatial weight matrices

To capture spatial dependence in the house and land hedonic models, spatial weight matrices must be constructed. Each land and house transaction coordinates can be used to find the nearest neighbors to each property and construct spatial weight matrices. The matrices are then normalized to have row-sums of unity. Following Kim et al. (2003), we experimented with a series of different weights and report results from the best fitting models.

2.2.3 Seemingly Unrelated Regression (SUR) models

Previous empirical studies have assumed that the impact of transactions in the undeveloped land market is independent of transactions occurring in the developed land market and vice versa. It was argued earlier in this study that the literature fails to account for the link between these two markets. The main argument for assuming linkage of the two markets comes from the fact that land is continuous. For example, the price of land-only transactions may be affected by the price of house transactions if they share common boundaries. Therefore the impact of different land uses on house prices is not independent of the impact of land uses on land prices. Though the impact of land use variables is accounted for individually in both single equations, it is reasonable to believe contemporaneous correlations exist between the two markets through the unexplained portions of the equations. Given that both the undeveloped land price equation and the developed land price equation share similar characteristics that affect one another, as well as possible existence of common omitted factors that are not accounted for in each equation, we can argue that the errors between the two equations may be contemporaneously correlated. A model of this structure calls for a seemingly unrelated regression estimation (SURE) approach.

2.3 Data

The dataset used in this paper consists of undeveloped (land only) and developed (house included) land transactions that took place in two time periods in Delaware County, Ohio: first, a total of 705 undeveloped land transactions and 1,595 developed land transactions from July 1987 to June 1988, and second, a total of 1,236 undeveloped land transactions and 1,915 developed land transactions from July 1997 to June 1998. Delaware County is located north of the city of Columbus, and is a fast-growing part of Ohio. It contains not only high quality agricultural land, but also high value land for development (Sohngen et al., 2000). Table 2.1 presents variables definitions and sources, while Table 2.2 and Table 2.3 presents summary statistics by time period and type of transaction on structural housing characteristics, parcel characteristics, and neighborhood characteristics used in the hedonic regressions.

Table 2.1 Definitions and sources of hedonic regression variables

Name	Definition
PRICE	Price of undeveloped and developed land transactions in 1988 and 1998*
NOTINACITY	1 for properties not in a city, 0 otherwise
CORNYIELD	Yield of corn on land (bushels per acre)
SLOPE	Average percentage slope of property*
AREA	Property area in acres
SQFT	Square footage of house (1000s)
AGE_HOUSE	Age of house in years, up to the transaction year*
STORYHGT	Number of stories in house*
BASEMENT	Size of basement coverage*
ROOMS_TOT	Total number of rooms in house*
BATHS_TOT	Total number of bathrooms in house (half bath=0.5)*
GARAGE_CAP	Cars fitting in garage*
ATTIC	1 for houses with an attic, 0 otherwise*
POP_DENS	Population density in census block**
INCPRCAP	Income per capita in block, in dollars**
SOUTHBND	Distance to the southern boundary in miles***
PCT_AG	% of agricultural land in 0.25 and 0.50 radii buffer, in 1988 and 1998****
PCT_GOLF	% of golf courses land in 0.25 and 0.50 radii buffer, in 1988 and 1998****
PCT_PARK	% of park land in 0.25 and 0.50 radii buffer, in 1988 and 1998****
PCT_RES	% of residential land in 0.25 and 0.50 radii buffer, in 1988 and 1998****

Note: Prices is deflated by the average quarterly Ohio Housing Price Index (I QTR 1988=100) from Federal Housing Enterprise Oversight. Sources: * Delaware County Auditor, **US Census Bureau, *** Calculated using ArcGIS®.

It can be seen from Table 2.2 that the average price for undeveloped transactions increased by 45.38% over the ten years under study, while Table 2.3 shows the average price for developed transactions increased by 19.46%. The average parcel size for undeveloped transactions decreased by 78% while the average parcel size for developed transactions slightly decreased. All this suggests that considerable parcel fragmentation occurred over the ten years period, resulting in more transactions in a given year but highly reduced area transacted. Further, the percentage increase in per capita income at the census tract level over the ten years period of study (41.65% for undeveloped transactions and a 45.02% for developed transactions) is much higher than the average percentage increase at the state level.[2]

Several sources comprise the dataset. All house and land information comes from the Delaware County Auditor.[3] Housing characteristics include the total number of rooms in a house, the total number of bathrooms (the sum of full baths and half baths), the total garage capacity, and the age of a house in years, as well as dummy variables for the presence of an attic, basement, and central air conditioning.

The land characteristics include parcel area in acres and the percentage slope (measured by rise/run) and soil type of the parcel. Also included is a dummy variable for whether the parcel is located in a city area. Further, since most land price studies are concerned with the impact of land characteristics, such as productivity,

[2] Calculated at 20.21% for the 1989-99 period; data obtained from the US Census Bureau.

[3] The Delaware County Office operates a project named Delaware Appraisal Land Information System (DALIS) whose mission is to collect GIS data in Delaware County, OH. Special thanks go to Shoreh Elhami, GIS Director of DALIS Project, for her assistance with the data.

Table 2.2 Hedonic variable means for undeveloped land

	1988		1998	
Variable	Mean	Std. Dev.	Mean	Std. Dev
PRICE	$60,589.52	$118,729.09	$88,086.39	$230,599.47
NOTINACITY	0.33	0.47	0.77	0.42
CORNYIELD	93.72	11.01	94.58	16.67
SLOPE	3.61	1.72	3.38	4.03
AREA	7.16	19.82	1.78	5.87
SQFT	-	-	-	-
AGE-HOUSE	-	-	-	-
STORYHGT	-	-	-	-
BASEMENT	-	-	-	-
ROOMS_TOT	-	-	-	-
BATHS_TOT	-	-	-	-
GARAGE_CAP	-	-	-	-
POP_DENS	1,025.25	677.06	289.44	720.95
INCPRCAP	$14,377.72	$3,577.34	$20,308.83	$8,873.36
SOUTHBND	4.96	4.66	4.70	4.45
PCT_AG (0.25 mi buffer)	41.27	34.13	24.40	26.39
PCT_GOLF (0.25 mi buffer)	1.42	6.63	1.60	7.67
PCT_PARK (0.25 mi buffer)	3.39	8.94	2.40	6.91
PCT_RES (0.25 mi buffer)	34.53	24.93	51.31	23.45
PCT_AG (0.50 mi buffer)	41.26	29.91	31.20	25.57
PCT_GOLF (0.50 mi buffer)	1.39	4.06	1.76	6.54
PCT_PARK (0.50 mi buffer)	5.27	8.47	3.51	7.64
PCT_RES (0.50 mi buffer)	36.90	21.82	41.72	18.71
Number of observations	705	705	1,236	1,236

Table 2.3 Hedonic variable means for developed land

	1988		1998	
Variable	Mean	Std. Dev	Mean	Std. Dev.
PRICE	$101,498.89	$81,020.66	$121,249.74	$67,952.14
NOTINACITY	0.14	0.35	0.60	0.49
CORNYIELD	92.61	6.89	93.42	17.60
SLOPE	3.91	1.62	3.81	4.30
AREA	1.96	8.77	1.84	6.83
SQFT	2.22	0.87	2.20	0.74
AGE-HOUSE	16.62	29.34	17.05	29.09
STORYHGT	1.64	0.45	1.75	0.40
BASEMENT	0.85	0.36	0.90	0.29
ROOMS_TOT	7.26	1.87	7.34	1.49
BATHS_TOT	2.49	1.02	2.63	0.84
GARAGE_CAP	1.75	1.01	1.91	0.90
POP_DENS	1,293.02	494.59	894.76	1,795.07
INCPRCAP	$13,825.80	$1,883.41	$20,050.19	$9,133.43
SOUTHBND	4.65	4.35	4.63	4.15
PCT_AG (0.25 mi buffer)	26.84	28.69	18.65	21.38
PCT_GOLF (0.25 mi buffer)	1.91	6.77	1.72	7.09
PCT_PARK (0.25 mi buffer)	3.95	10.35	2.84	7.03
PCT_RES (0.25 mi buffer)	43.20	22.92	55.11	19.34
PCT_AG (0.50 mi buffer)	42.26	29.15	26.93	21.90
PCT_GOLF (0.50 mi buffer)	1.81	4.94	1.78	6.25
PCT_PARK (0.50 mi buffer)	4.96	8.30	4.55	8.36
PCT_RES (0.50 mi buffer)	36.64	20.96	43.18	16.46
Number of observations	1,595	1,595	1,915	1,915

on land prices, we include the potential average corn yield of the transacted parcel. Neighborhood demographic characteristics include income per capita and population density collected at the census block group level. These variables come from the US Census Bureau. Following the urban economics literature, a set of distance measures is included to capture proximity to urbanized areas. These variables, calculated using ArcGIS®, are the distances in miles from each transacted house and parcel to the city of Columbus and the city of Delaware.

2.3.1 Neighborhood land characteristic variables

The primary variables of interest in this study are the percentages of land use types in buffers around a transacted property. A review of the literature does not suggest a specific number or size of buffers to be included in the hedonic regressions. Irwin (2002) suggests that a visual inspection of the land use distribution could be a first indicator to determine the specification of the neighborhood extent; she uses a 0.25-mile radius buffer (400-meters); Patterson and Boyle (2002) use a 0.62-mile radius buffers (1-kilometer); Espey and Owusu-Eudsei (2001) use various buffer sizes for different park types. To avoid collinearity problems, the hedonic regressions in this essay do not include all of the land uses calculated within the buffers. Since it is of interest to determine the impact of open space lands and residential land on land and house prices, the regressions only include the percentage of agricultural land, residential land, parks, and golf courses within a buffer of 0.25 miles radii. A second buffer of 0.5 miles radii is also created. The percentages of land uses in the buffers are calculated using ArcGIS®.

2.4 Empirical Results

Table 2.4 and Table 2.5 presents the estimates of the spatial undeveloped and developed land price equations in 1988 and 1998. Table 2.4 presents the 0.25 mile buffer and Table 2.5 the 0.50 mile buffer. The undeveloped land equations in 1998 present higher adjusted R^2 than the undeveloped land equations in 1988; further, they comparatively have a greater number of explanatory variables that are statistically different from zero. The same result is observed in the developed land equations, yet the 1988 equations present several explanatory variables that are statistically different from zero. With respect to the land use variables, their impact on the undeveloped land prices is not statistically significant, yet significant on developed land prices (except in 1988).

Table 2.6 and Table 2.7 presents results for the undeveloped and developed land price equations in 1988 and 1998 treated as seemingly unrelated regressions. Table 2.6 presents the results for the 0.25 mile buffer and Table 2.7 for the 0.50 barrier. In order to estimate these coefficients, we proceed as follows: First, we obtain the vector of disturbances from a first stage spatial regression with undeveloped and developed land transactions pooled together for a given year. Second, we estimate the variance-covariance matrix of disturbances using Judge et al.'s (1985) procedure when each equation has different number of observations. This variance-covariance matrix of disturbances is used to re-estimate the equations as seemingly unrelated regressions without directly accounting for spatial effects.

Table 2.4 Results for the spatial hedonic models, 0.25 mile buffer

	Undeveloped				Developed			
	1988		1998		1988		1998	
Variable	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
INTERCEPT	4.48***	2.84	5.88***	3.94	11.11***	140.90	9.47***	512.92
NOTINACITY	-0.075	-0.43	0.294**	2.37	0.127	0.69	0.008	0.30
LN_CORNYIELD	0.023	0.90	0.008	0.71	-	-	-	-
LN_SLOPE	-0.160**	-2.00	0.044	1.06	0.106***	2.78	0.031***	2.57
AREA	0.008***	3.80	0.000	0.05	0.002	1.12	-0.005***	-3.41
SQFT	-	-	-	-	0.184***	4.68	0.217***	9.71
LN_AGE_HOUSE	-	-	-	-	0.029***	8.76	0.012*	1.72
STORYHGT	-	-	-	-	0.128***	2.49	-0.053*	-1.76
BASEMENT	-	-	-	-	0.113*	1.84	0.076*	2.02
ROOMS_TOT	-	-	-	-	-0.072***	-4.37	0.019*	1.90
BATHS_TOT	-	-	-	-	0.156***	4.43	0.093***	4.44
GARAGE_CAP	-	-	-	-	0.103***	3.93	0.088***	5.78
LN_POP_DENS	-0.136**	-2.35	-0.202***	-5.28	-0.013	-0.22	-0.018***	-2.43
LN_INCPRCAP	0.756***	3.21	0.662***	4.58	0.008	0.17	0.007	0.75
LN_SOUTHBND	-0.165**	-2.43	-0.090*	-1.90	-0.216***	-7.12	-0.070***	-7.06
PCT_AG	-0.003	-1.07	-0.011***	-4.36	-0.003	-2.36	0.002***	2.44
PCT_GOLF	0.002	0.31	-0.011**	-1.93	-0.004	-0.92	0.006***	3.75
PCT_PARK	-0.001	-0.22	-0.011*	-1.87	-0.003	-1.14	0.004***	2.44
PCT_RES	-0.005	-1.25	-0.019***	-7.34	-0.002	-1.55	0.003***	4.08
RHO	-0.008	-0.43	0.029***	3.22	-0.022***	-3.27	0.076***	15.89
LAMBDA	0.598***	23.96	0.474***	21.28	0.356***	26.65	0.076***	5.67
N		705		1,236		1,595		1,915
Adjusted R^2		0.40		0.46		0.31		0.45

Note: a series of likelihood ratio test comparing the general spatial model to the spatial error model, the spatial lag model, and the spatial Durbin model suggest that the general specification best.

All estimated equations resulted in high adjusted R^2, yet the number of statistically significant variables varies from one equation to another. In particular, the land use variables present differing results across years and type of transaction. The price of undeveloped land transactions in 1988 is positively impacted only by the percentage of golf area within 0.25 miles from the property, and negatively impacted by the percentage of residential land within the same distance. None of the percentages of land use variables statistically impact the price of undeveloped land within 0.50 miles from the property. In 1998, only the percentage of residential land within 0.25 miles from the property has a negative impact on the price of undeveloped land transactions, though the estimated coefficient is considerably higher than in 1988. The percentages of golf course area and park area within a distance of 0.50 miles from the undeveloped land property have a positive impact on its price, while the percentage of residential land has a negative impact, yet smaller in size compared to the impact within 0.25 miles from the property.

The percentages of agricultural land, park areas, and residential land have a negative impact on the price of developed land transactions in 1988 within 0.25 miles from the property, yet no significant impact is found within 0.50 miles from the property. These effects are reverted in 1998; all land uses have a positive impact on developed land prices with the percentage of park areas having the greatest impact.

Table 2.5 Results for the spatial hedonic models, 0.50 mile buffer

	Undeveloped				Developed			
	1988		1998		1988		1998	
Variable	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
INTERCEPT	4.65***	1.65	5.61***	2.95	10.79***	122.42	9.56***	514.08
NOTINACITY	-0.111	-1.13	0.182	1.47	0.105	0.57	-0.005	-0.19
LN_CORNYIELD	0.019	0.77	0.008	0.68	-	-	-	-
LN_SLOPE	-0.155**	-1.94	0.045	1.03	0.105***	2.77	0.027**	2.27
AREA	0.008***	3.55	-0.000	-0.11	0.002	1.06	-0.005***	-3.52
SQFT	-	-	-	-	0.179***	4.55	0.199***	8.87
LN_AGE_HOUSE	-	-	-	-	0.028***	9.06	0.016**	2.22
STORYHGT	-	-	-	-	0.134***	2.61	-0.045	-1.48
BASEMENT	-	-	-	-	0.116*	1.89	0.081**	2.16
ROOMS_TOT	-	-	-	-	-0.068***	-4.18	0.017*	1.72
BATHS_TOT	-	-	-	-	0.152***	4.33	0.093***	4.42
GARAGE_CAP	-	-	-	-	0.098***	3.75	0.087***	5.77
LN_POP_DENS	-0.150***	-3.32	-0.213***	-4.67	-0.002	-0.04	-0.012***	-2.70
LN_INCPRCAP	0.722**	1.94	0.662***	3.10	0.019	0.39	-0.009	-0.96
LN_SOUTHBND	-0.181***	-2.92	-0.056	-0.99	-0.231***	-8.75	-0.083***	-7.78
PCT_AG	0.001	0.41	-0.011***	-3.35	-0.001	-0.44	0.003***	4.17
PCT_GOLF	-0.004	-0.42	0.010	1.45	0.002	0.49	0.010***	5.24
PCT_PARK	-0.001	-0.18	-0.005	-0.83	-0.001	-0.56	0.005***	3.46
PCT_RES	-0.000	-0.03	-0.015***	-3.73	-0.000	-0.12	0.006***	6.58
RHO	-0.015	-1.15	0.036***	2.84	-0.024**	-2.40	0.074***	15.63
LAMBDA	0.597***	14.26	0.459***	15.00	0.364***	27.82	0.070***	5.26
N		705		1,236		1,595		1,915
Adjusted R^2		0.40		0.45		0.31		0.49

Note: a series of likelihood ratio test comparing the general spatial model to the spatial error model, the spatial lag model, and the spatial Durbin model suggest that the general specification best.

2.5 Discussion

Results from the seemingly unrelated regressions are used to compare marginal impacts from land use changes on undeveloped and developed land prices over time where land changes are measured as percentage changes in land uses in buffers around each property. Table 2.8 and Table 2.9 present marginal values for the four categories of land uses.[4] The impact of changes in the percentage of agricultural land is positive in both years on undeveloped and developed land values. In 1988, a 1% increase in agricultural land in the 0.25 mile radii buffer increases undeveloped land values by $186.51, while it increases developed land prices by $368.10. These effects are considerably larger in 1998. A 1% increase in agricultural land increases undeveloped land values by $2,438.47, while it increases developed land prices by $5,287.06. While the average value of land-only transactions increased in real terms by nearly 18% per year, the value of agricultural open space around land-only transactions increased 18-25% per year (0.50-0.25 mi. radius respectively).

The value of developed parcels increased substantially more slowly over the 10 year time period, only 2.4% per year when considered on a $/acre basis, and 1.9% per year when considered on a $\%/ft^2$ basis. In contrast, the value of agricultural open-space increased by 14–27% per year 0.50-0.25 mi. radius respectively). Taken

[4] We calculated the marginal implicit prices of each of the land uses assuming that the area of each property remains constant at the sample average.

Table 2.6 Results for the SUR hedonic price models using a spatially corrected covariance matrix, 0.25 mile buffer

	Undeveloped				Developed			
	1988		1998		1988		1998	
Variable	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
INTERCEPT	0.000***	0.01	0.030	1.35	0.002	0.07	0.050***	2.51
NOTINACITY	-0.297***	-1.30	0.572***	6.98	-0.091	-0.49	0.195***	4.84
LN_CORNYIELD	0.006***	0.21	0.046***	4.15	-	-	-	-
LN_SLOPE	-0.161*	-1.85	0.196***	5.32	0.084***	2.21	0.019	1.02
AREA	0.011***	4.43	0.003	0.65	0.001	0.55	-0.004**	-1.77
SQFT	-	-	-	-	0.000***	4.18	0.000**	2.37
LN_AGE_HOUSE	-	-	-	-	0.026***	8.73	0.062***	5.39
STORYHGT	-	-	-	-	0.159***	3.17	0.201***	4.08
BASEMENT	-	-	-	-	0.147**	2.43	0.371***	6.14
ROOMS_TOT	-	-	-	-	-0.079***	-4.83	0.089***	5.33
BATHS_TOT	-	-	-	-	0.156***	4.38	0.129***	3.80
GARAGE_CAP	-	-	-	-	0.141***	5.52	0.100***	4.11
LN_POP_DENS	-0.072	-1.08	0.125***	5.22	-0.029	-0.50	0.084***	7.28
LN_INCPRCAP	1.201***	21.97	0.993***	53.44	1.164***	23.98	0.803***	53.28
LN_SOUTHBND	-0.251***	-5.62	-0.153***	-5.93	-0.158***	-7.00	0.040***	2.63
PCT_AG	0.000	0.11	0.000	0.06	-0.004***	-3.82	0.010***	8.67
PCT_GOLF	0.016***	2.47	0.005	1.31	0.001	0.55	0.010***	4.11
PCT_PARK	-0.004	-0.81	0.004	1.09	-0.004*	-1.90	0.018***	6.79
PCT_RES	-0.009***	-3.88	-0.010***	-5.62	-0.003***	-3.12	0.009***	7.35
N		705		1,236		1,595		1,915
Adjusted R^2		0.97		0.97		0.98		0.98

Table 2.7 Results for the SUR hedonic price models using a spatially corrected covariance matrix, 0.50 mile buffer

	Undeveloped				Developed			
	1988		1998		1988		1998	
Variable	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat	Estimate	t-stat
INTERCEPT	0.000***	0.01	0.030	1.33	0.002	0.07	0.050***	2.52
NOTINACITY	-0.357	-1.60	0.474***	5.74	-0.164	-0.89	0.169***	4.10
LN_CORNYIELD	0.006	0.24	0.046***	4.15	-	-	-	-
LN_SLOPE	-0.165*	-1.91	0.191***	5.14	0.085**	2.22	0.0167	0.86
AREA	0.010***	4.01	0.003	0.68	0.001	0.75	-0.003	-1.57
SQFT	-	-	-	-	0.000***	3.83	0.000***	2.59
LN_AGE_HOUSE	-	-	-	-	0.024***	8.60	0.066***	5.78
STORYHGT	-	-	-	-	0.161***	3.21	0.195***	3.95
BASEMENT	-	-	-	-	0.163***	2.70	0.396***	6.55
ROOMS_TOT	-	-	-	-	-0.071***	-4.43	0.091***	5.47
BATHS_TOT	-	-	-	-	0.150***	4.23	0.130***	3.82
GARAGE_CAP	-	-	-	-	0.129***	5.10	0.105***	4.32
LN_POP_DENS	-0.158**	-2.42	0.120***	4.99	-0.028	-0.49	0.085***	7.38
LN_INCPRCAP	1.222***	19.75	0.978***	51.55	1.136***	22.61	0.811***	53.16
LN_SOUTHBND	-0.229***	-5.93	-0.158***	-5.44	-0.189***	-9.68	0.014	0.88
PCT_AG	0.003	1.10	0.001	0.70	-0.000	-0.14	0.011***	8.73
PCT_GOLF	-0.014	-1.24	0.022***	4.74	0.003	0.70	0.007***	2.46
PCT_PARK	-0.000	-0.02	0.009**	2.34	-0.000	-0.02	0.016***	6.87
PCT_RES	-0.003	-0.66	-0.008***	-3.79	0.000	0.26	0.007***	4.64
N		705		1,236		1,595		1,915
Adjusted R^2		0.97		0.97		0.98		0.98

Table 2.8 Average marginal implicit prices for changes in the percentage land uses within 0.25 mi buffer, in real dollars

	1988 Undeveloped			1998 Undeveloped		
	Mean	Lower 95%	Upper 95%	Mean	Lower 95%	Upper 95%
Agricultural land	$186.51	$116.93	$256.09	$2,438.47	$1,497.27	$3,379.68
Golf courses	$1,141.11	$715.42	$1,566.80	$2,662.75	$1,634.98	$3,690.52
Parks	-$53.00	-$72.78	-$33.23	$4,496.86	$2,761.16	$6,232.56
Residential Land	-$432.61	-$593.99	-$271.23	$3,174.19	$1,949.01	$4,399.37
	1988 Developed			1998 Developed		
	Mean	Lower 95%	Upper 95%	Mean	Lower 95%	Upper 95%
Agricultural Land	$368.10	$252.34	$483.87	$5,287.06	$4,947.34	$5,626.77
Golf Courses	$2,252.17	$1,543.89	$2,960.45	$5,773.33	$5,402.37	$6,144.29
Parks	-$104.61	-$137.51	-$71.71	$9,750.02	$9,123.54	$10,376.50
Residential Land	-$853.82	-$1,122.34	-$585.31	$6,882.24	$6,440.03	$7,324.45

together, the results for both developed and undeveloped land indicates that a large proportion of the growth in value over the time period is due to open space. For instance, while the value of developed parcels has increased only slowly, the value of the open-space around developed parcels appears to be an increasingly important component of the value of developed parcels.

Further, these effects are considerably larger when considering a greater buffer size. The process of land transformation from agricultural land into residential land progressively impacts how undeveloped and developed land owners value the agricultural land left.

Table 2.9 Average marginal implicit prices for changes in the percentage land uses within 0.50 mi buffer, in real dollars

	Undeveloped					
	1988 Undeveloped			1998 Undeveloped		
	Mean	Lower 95%	Upper 95%	Mean	Lower 95%	Upper 95%
Agricultural land	$754.43	$580.90	$927.96	$3,657.23	$1,822.42	$5,492.04
Golf courses	$152.36	$117.32	$187.41	$5,872.10	$2,926.11	$8,818.10
Parks	$706.96	$544.35	$869.57	$6,245.43	$3,112.14	$9,378.72
Residential Land	$588.14	$452.86	$723.42	$5,328.24	$2,655.10	$8,001.38
	1988 Developed			1998 Developed		
	Mean	Lower 95%	Upper 95%	Mean	Lower 95%	Upper 95%
Agricultural land	$1,752.03	$1,312.70	$2,191.36	$6,843.43	$6,393.26	$7,293.59
Golf courses	$353.83	$265.11	$442.56	$10,987.91	$10,265.12	$11,710.70
Parks	$1,641.78	$1,230.09	$2,053.46	$11,686.49	$10,917.74	$12,455.23
Residential Land	$1,365.84	$1,023.35	$1,708.33	$9,970.23	$9,314.38	$10,626.08

The same effect is observed for the percentage of golf courses surrounding a property, yet in 1988 a 1% increase in this type of land has a considerably larger impact than the percentage of agricultural land. Over time, the impact of additional land dedicated to golf course follows the same pattern as agricultural land, as well the marginal impact equalize. An increase in 1% in the percentage area dedicated to parks within 0.25 miles from both undeveloped and undeveloped land properties in 1998 has a negative impact; yet extending the area to 0.50 miles from the property results in a positive impact. This effect is inverted in 1998: both undeveloped and developed land prices are positively impacted by the percentage of land dedicated to parks. Combining these two effects, it can be observed that the marginal impact of open space lands increases as time passes and the relative abundance of these types of land are reduced at the expense of residential land. Land owners and homeowners considerably value agricultural land, golf course and parks, and their valuation increases over time.

The marginal impact of a 1% increase in the area dedicated to residential land has a negative impact of $432.61 within a 0.25 miles distance from undeveloped land properties in 1988, yet the marginal effect becomes positive considering a 0.50 mi distance. This effect is reversed in 1998; a 1% increase in the percentage area dedicated to residential land increases undeveloped land values by $3,174.19 considering a 0.25 miles distance from the property, whereas it marginally increases undeveloped land prices by $5,328.24 within 0.50 miles from the property.

2.6 Simulations

The undeveloped and developed land price equations estimated in this paper are used to predict the impact of land use changes on property values as urbanization into predominantly agricultural areas takes place. Concerns over the impact of increased urban and suburban growth on agricultural areas and open space lands have led many cities and counties in the United States to pass laws to protect farmland and the environment discouraging the expansion of suburban areas. It is arguable that these policies have unpredictable effects on land and house prices. Further, as this expansion continues, the relative abundance of land amenities decreases with respect to other types of land areas. It is therefore useful to determine whether the changing conditions in the area dedicated to a particular land use capitalizes into property values. One way to asses this impact is to simulate price changes in the hedonic price models imposing certain conditions on the land use variables. Specifically, we are interested in assessing how the actual predicted value of developed or undeveloped parcels in 1998 (with 1998 amenities) compare to the predicted value of these same parcels assuming they have 1988 amenities.

Table 2.10 Actual predicted prices in 1998 versus simulated predicted prices in 1998 within 0.25 and 0.50 miles buffer

	Undeveloped 0.25 mi buffer			Developed 0.25 mi buffer		
	Mean	Lower 95%	Upper 95%	Mean	Lower 95%	Upper 95%
Actual Price Prediction	$46,321	$44,681	$47,961	$122,266	$118,462	$126,070
Simulated Price Prediction	$54,984	$53,084	$56,884	$120,610	$117,082	$124,138
% Change	19.16	18.66	19.65	0.27	-0.30	0.84
	Undeveloped 0.50 mi buffer			Developed 0.50 mi buffer		
	Mean	Lower 95%	Upper 95%	Mean	Lower 95%	Upper 95%
Actual Price Prediction	$46,605	$44,889	$48,322	$121,899	$118,148	$125,650
Simulated Price Prediction	$49,153	$47,443	$50,863	$137,232	$133,241	$141,224
% Change	6.58	6.28	6.88	14.17	13.53	14.81

Table 2.10 present simulations of undeveloped and developed land prices in 1998 assuming that the average percentage area of the different land uses in 1998 approx-

imate the averages in 1988. These results are compared to the actual undeveloped and developed predictions from the estimated model in 1998. The average percentage of land dedicated to agriculture in this sample of transactions decreased considerably from 1988 to 1998, especially surrounding undeveloped land transactions. The average percentage area dedicated to residential land also increased, while the average percentage of area dedicated to golf course and parks were not considerably affected.

Two effects are considered here. First, residential expansion may adversely capitalize into property prices due to increased housing congestion and other resulting externalities. Second, because open space amenities are positive valued, and open space amenities are larger in 1988, one would expect houses and land in 1998 to be more valuable if they have 1988 amenities. For the 0.25 mi. buffer, undeveloped land transactions are estimated to be 19% more valuable if they have the 1988 amenities in 1998, whereas developed land transactions are estimated to be about the same value. Thus, open-space within a relatively short distance of an agricultural property tends to enhance that properties value, however, it does not appear to enhance the value of a developed property. Looking at the 0.50 mi. buffer, undeveloped land transactions would have only been about 7% more valuable with the amenities of 1988 around them, whereas developed transactions would have been 14% more valuable. These results help explain observed tendencies in the Midwest, where there is strong reliance on minimum lot size zoning. Minimum lot size zoning would appear to substantially benefit agricultural land owners because it would help preserve open space near existing farms, while at the same time allowing for development to occur. The results also imply that nearby open space does not substantially benefit developed land if that open space is close. Open space appears to have its greatest effects on developed land if it is not located in the immediate vicinity of the house.

Results from calculating the percentage difference between the simulated prices and the predicted prices indicate that in close proximity to the property, residential expansion has a greater negative impact on the price of undeveloped land transactions than developed land transactions. Assuming that the land use averages in 1998 remain at the same averages than in 1988, ceteris paribus, undeveloped land prices would have been 19.16% higher than the actual predictions, whereas developed land prices would have been only 0.27% greater considering the impact within 0.25 miles from the property. This means that the negative impact of urbanization outweighed the positive impact of increased relative area dedicated to open space lands, but the impact is greater on undeveloped land properties than in developed ones.

2.7 Conclusion

This paper addresses concerns over the impact of urban and suburban expansion into predominantly agricultural areas. We consider that the negative impact from externalities generated by increased residential areas counteracted by the positive externality generated from an increase in the relative abundance of open space lands accumulate into property price over time. Results from the estimated undeveloped and developed land prices in two different time periods are used to shed light into whether the negative impact of urbanization outweigh the positive impacts of the remaining open space land. These results are relevant policy making tools that allow us to predict how land use changes affect the value of land and house price transactions.

This paper represents a contribution to the literature because it estimates households' valuations of different types of land uses on land and house transactions considering the fact that both markets are linked by the continuity and fixity of land. Empirical results determine that households' valuation of open space lands differ between land only transactions and transactions that include a house. Further, we can predict changing preferences over time: residential movers in 1988 have different preferences compared to those in 1998.

The following concerns need to be addressed in future research. First, the land uses in this paper do not provide information about whether the parcels are potentially developable or idle. Further, it was not possible to determine whether the parcels classified as agricultural land are actually being used for agricultural production. The marginal impact of additional agricultural land most likely differs by types of agricultural operations. It has been found in the literature that certain agricultural activities have a negative impact on property values, such as swine production farms, whereas others might have positive impacts. The last concern relates to the impact of congestion externalities on property values. Most conversion of agricultural land into residential land accompanies fragmentation of large plots into smaller plots. The increase in housing congestion as measured by density of built structures per unit of land, for example, is not captured within the percentage of residential land surrounding a property. Over time the percentage increases, but at the same time the number of houses built on the parcels also increases. It is possible that the impact of residential land use on property values may differ by the capital intensity surrounding the property, an effect which is outside the scope of this study.

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Chapter 3
Neighborhood Sorting Dynamics in Real Estate: Evidence from the Virginia Sex Offender Registry

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Abstract Given the potential risk of recidivism, recent research has found that registered sex offenders impose external costs that are capitalized into the value (lower prices) and liquidity (longer time on market) of nearby residential real estate. Using more than a decade of real estate listings data and a unique data set of registered sex offenders (RSOs) in Virginia, we extend this literature by investigating how real estate markets respond to clustering of this particular group and whether there is evidence of market prices facilitating clustering phenomena. Employing a difference-in-difference approach within a hedonic empirical framework, the results show that the housing market has a nonlinear response to clusters, as it considerably discounts prices of homes located near a cluster (4+) of RSOs, providing a market incentive for further sorting/clustering. Our empirical findings and supplemental geospatial analysis are consistent with a sorting/tipping model in which individuals choose residential locations according to preferences and price incentives. Insofar as clustering of negative externalities is efficient, we further discuss the market efficiency implications in this context.

3.1 Introduction

Thomas Schelling (1971) showed that residents will tend to sort into segregated neighborhoods, even when individuals within those neighborhoods are relatively tolerant and do not have extreme preferences for such segregation as the group dy-

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namics suggest. In his model segregation is produced by individuals who chose to locate in areas where they are not surrounded by large numbers of neighbors from another group. In the presence of a critical mass of 'others' a neighborhood may 'tip' in one direction as residents move out.[1] A number of studies have investigated the dynamics of Schelling's tipping model in different contexts. In this chapter, we apply the insights of the Schelling tipping model to another group dynamic, clustering among sex offenders and non-sex offenders, a group of great concern to participants in the real estate market (which also happens to have transparent public data on where they live and when they move). In particular, using a hedonic pricing approach, we examine whether there is evidence that the price mechanism incentivizes these groups to cluster and the broader housing market implications of this result.

Following the passage of Megan's Law and related statutes at both the state and national level, convicted sex offenders must register their residences with local authorities each time they move (Virginia State Police, 2020).[2] Each state operates its own online registry, which is searchable by the general public. When a registered sex offender moves in nearby, studies have consistently shown that the neighborhood takes note, which affects the local real estate market in turn. The purpose of this chapter is to study the incremental effects that sex offenders (and sex offender clusters) have on home prices and liquidity, determining whether there is evidence of distinct price signals that facilitate tipping in the market for real estate. Using a straightforward nearest-neighbor GIS analysis, we further explore the implications of these pricing dynamics, examining the locational choices of sex offenders and homeowners (i.e. non-sex offenders), looking at whether these groups tend to separate from one another and whether sex offenders exhibit a tendency to cluster among other sex offenders over time.

Motivating our empirics analytically, we first explore household preferences that may lead to clustering dynamics. Some households may have preferences to move out in the event that one or more registered sex offenders move in nearby. On the other side of the same coin, sex offenders may be more likely than the general population to move near other sex offenders for a variety of reasons (e.g. discounted housing, less aversion to living near a fellow offender, etc.). Over time, neighborhood composition will tend to 'tip' toward segregation of these two groups, based on their preferences, market conditions, and, as we explore empirically, pricing incentives. Moreover, there may be considerable variation of individual or family-level preferences toward sex offenders within the general populace, which we test using a proxy potentially correlated with intensity of these preferences. Specifically, we use a proxy for children to explore whether pricing dynamics are different for 'family' vs. 'non-family' homes.

To summarize our results, we find evidence that the real estate market steeply discounts the price of homes near clusters (4+) of registered sex offenders by as much of 16%, on average, along with a substantial adverse effect on nearby home liquidity. This result is consistent across multiple specifications, including a three-stage-least-squares (3SLS) simultaneous equations analysis; and, when the preferences of the treated group are heightened (i.e., for more family-friendly 3+ bedroom homes), we find a pronounced effect accordingly. This result helps explain further

[1] One insight from Schelling's model is that trends and patterns of residential mobility for the group may be more extreme than the preferences of the individual within that group. We return to a related point later when we investigate effects among individuals with stronger preferences (proxied by the number of bedrooms within the home).

[2] For more information on the sex offender registry in Virginia, which is a key source of our data, see: https://sex-offender.vsp.virginia.gov/sor/.

sorting/clustering of sex offenders over time found in the literature and in our own geospatial analysis, as market prices provide an additional incentive for RSOs to cluster and concentrate this negative externality in a particular area. We conclude by discussing the market efficiency implications of these results in more detail.

3.2 Motivating Literature and Contribution

There are several streams of literature that have analyzed the primary elements of this paper (neighborhood segregation, tipping, and real estate market response to sex offenders), but one opportunity we take in this study is to combine the insights from these strands of literature to investigate market response to RSO clusters specifically and the further sorting that is incentivized by this home price response. Below, we briefly discuss a number of geography and regional sciences studies that have established that registered sex offenders tend to cluster spatially. These papers generally focus on the degree to which local or state restrictions limit locational choices and essentially force sex offenders to live in clusters in the relatively few areas that are open to them. Another strand of literature has focused on forces that lead to neighborhood segregation. These papers model the existence of stable equilibria and tipping points at which a neighborhood may change from high to low income or from one ethnic or religious group to another. Finally, economics and real estate studies have established that living near a sex offender creates negative consequences in real estate markets — lower selling price and reduced liquidity. Given that these are sizable (and growing) literatures, the review in this section is not comprehensive; but, we attempt to integrate some of these insights as we summarize closely related literature below.

When a registered sex offender (RSO) moves into a neighborhood, it presents a potential 'shock' to the neighborhood that elevates perceptions of crime risk of nearby residents, which has shown to have a measurable impact on very localized real estate markets. Given the nature of this shock, which represents a 'treatment' to nearby homes that is differentiated from the control group across both space (near vs. far) and time (post-RSO move in), the real estate and urban economics academic literature has generally estimated this effect using a difference-in-differences approach. The study closest to this paper is by Wentland et al. (2014), which also utilizes multiple listing service (MLS) data estimates the effect of nearby RSOs on real estate when there is endogenous determination of housing liquidity and price. Controlling for home and property characteristics as well as property and time fixed effects, three stage least squares results indicated that the presence of a registered sex offender within 0.1 miles resulted in a 7% decrease in selling price and an 80% increase in the time a home takes to sell. The authors also found significant but declining effects on price and liquidity of sex offenders located in larger radii up to one mile around a listed property relative to the control group (i.e., homes not presently within a mile of a sex offender). Results were robust across a number of specifications and identification strategies, including correction for sample selection and a pre-trend, placebo analysis of housing outcomes before a nearby offender moves in or after he/she moves out. Finally, they also show that the treatment varies heterogeneously based on the intensity of the preferences of potential homebuyers, where homes with more bedrooms, a proxy for households with more children, suffered larger price and liquidity effects. Perhaps most relevant for the present study, the authors also estimate (linear) effects of additional nearby sex offenders, but did not investigate potential nonlinear effects of clusters.

Wentland et al. (2014) extended a growing literature that has also found that sex offenders residing nearby had a significant impact on home prices. Linden and Rockoff (2008) analyzed housing effects in Mecklenberg County, NC, a large area which contains Charlotte. Data on the move-in date for the most recent address of each offender was matched to a data set of home sales over a ten year period. They found significant reductions in home prices across radii of less than 0.1 miles and 0.1 to 0.3 miles when an offender moves in. Results indicated that the presence of a sex offender within 0.1 miles reduces home prices by roughly 4.0% after the offender's move-in date. Living beyond one-third of a mile from an offender had no significant effect on price in their study. Another influential study in this literature, Pope (2008), analyzed the effect of proximity to a sex offender in Hillsborough County, Florida which contains Tampa. His data set included move-in and move-out dates for every house an offender has occupied since being placed on the registry. Controlling for housing characteristics, time, and neighborhood fixed effects, regression results indicated that a location within 0.1 miles of a registered sex offender reduced housing values by 2.3%. He also found that prices rebound when an offender moves out. Like the Linden and Rockoff (2008) study, the effect dissipated quickly beyond 0.1 mile, as there was no significant effect on price for homes located between 0.1 and 0.2 miles from an offender.

A somewhat older study by Larsen et al. (2003) found similar results in Montgomery County (Dayton area), Ohio. They matched data on sex offender addresses with home sales during one year, 2000. Controlling for a vector of house characteristics and neighborhood fixed effects, regression results indicate that living within 0.1 miles of a registered sex offender reduces property values by 17.4%. Significant but diminishing price effects were estimated out to a distance of 0.3 miles.

A more recent paper by Brastow et al. (2018) analyzed the effect of nearby sex offenders with a focus on housing liquidity. They use both survival analysis and a difference-in-difference framework, and their empirical results show that registered sex offenders have a large adverse impact on nearby home liquidity on average, where this effect is largely driven by 'surprises' of sex offenders moving in or out during the marketing period. However, for homes near offenders who reside nearby through the entire marketing period, the results indicate that sellers steeply discount the initial list price and may actually sell their homes more quickly.

Geography and regional science studies have employed a variety of spatial techniques to demonstrate the degree to which registered sex offenders cluster. The analysis generally focusses on local or state restrictions on the location of sex offender residences. Grubesic (2010) calculates several measures of sex offender clustering in Illinois and compares the results to residency restrictions and regional demographic characteristics. The techniques produced general correspondence in the identification of clusters. The study identifies several characteristics of the identified clusters including the existence of relatively low priced housing.

Rey et al. (2014) introduced a Markov chain framework to analyze residential movement patterns of registered sex offenders in Hamilton County, Ohio, from 2005 to 2007. The authors analyze sex offender location choices relative to spatial restrictions. Results indicate a 46 percent reduction in offenders violating spatial restriction zone policy as compared to a counterfactual case where offenders move in patterns that correspond to existing housing distributions. Strong legacy effects are also found as offenders previously in violation of restriction policies move into other restricted zones at a higher rate than offenders who were previously in compliance with the policy. Generally, registered sex offenders continue to locate in restricted zones at a higher rate than expected, despite the policy restrictions.

Finally, the economics literature of residential sorting, segregation, and tipping has its roots largely in the work of Thomas Schelling (1971). In Schelling's model, a resident's preference for neighborhood characteristics is expressed as the degree of tolerance for existing or potential residents who differ on some important margin. A resident moves when the number of different neighbors exceeds the individual's threshold. Schelling's models demonstrated that tipping and segregation can occur even if individuals have relatively high thresholds (are willing to live in integrated neighborhoods), segregation is a stable equilibrium and is likely to occur. Economists continue to develop new models of spatial segregation. In a more recent article, O'Sullivan (2009) models neighborhood preferences as bid-rent offers where vacant properties are sold to the highest bidder. Even though bid-rent curves maximize in integrated neighborhoods, inefficient segregation is the stable outcome.

Card et al. (2008) use a regression discontinuity methodology with Census tract data to test for the dynamics of neighborhood racial composition. They find evidence of white residential flows consistent with tipping behavior in most cities. Tipping points range from 5% to 20% minority portion of city population. While there is evidence of white population flows in urban areas, the authors do not find Census tract rental or housing sale price effects around the tipping point.[3] Thus, while tipping and sorting dynamics have been investigating in these other contexts (usually by race), these studies often use more aggregated data on the composition of a particular geography, rather than microdata identifying the exact membership of a household to a particular group that is sorting. The uniqueness of data from sex offender registries is that it allows researchers to analyze this type of sorting and clustering phenomenon at a very micro-level with data where we know the address, move-in, and move-out dates of each member of this particular group, which contributes to the sorting/tipping literature more broadly. Further, we are able to extend the narrower literatures on the impact of sex offenders by helping explain how price responses can facilitate clustering and further sorting, which is the primary contribution of this paper.

3.3 Neighborhood Tipping Dynamics — A Simple Model

In this section, we sketch a simple theoretical model to motivate our empirical analysis. Imagine an open city in a featureless plain. A registered sex offender randomly selects a point in the city to reside, and our model examines location choices around this point. Additionally, our model will show that such location choices can impact the value of nearby properties. We assume that each individual has a utility function:

$$U_i = U(Q(x,q),c) \tag{3.1}$$

[3] Two additional studies have modeled neighborhood dynamics and real estate outcomes. Based on a model developed in Bond and Coulson (1989), the neighborhood turnover process depends on the hedonic bid functions for housing and neighborhood quality. Coulson and Bond (1990) use FHA data from six cities from 1979 and 1980 to estimate inverse demand functions for housing quality attributes. They find significant differences among income groups in the bid for floor space, suggesting that segregation could occur as higher income groups bid for larger properties. The authors did not find differences in bid functions for racial composition.

where individual utility U is a strictly increasing function of the overall quality of housing services Q and the consumption of a composite good c. For simplicity, the price of the composite good c is assumed to be the same everywhere in the city, and the price of c is normalized to unity. Quality of housing services is determined by two things: 1) x, the intensity of the nearby sex offender(s) and 2) other housing attributes q, which include physical characteristics of the residence (e.g. square footage, number of bedrooms, etc.) and other location characteristics (e.g. school quality).[4] A greater x could result from closer proximity to the sex offender (or each additional sex offender). It may also be a consequence of the nearby sex offender being one who committed a violent offense that is perceived as a higher or more serious recidivism risk by his or her neighbors. Assuming a sex-offender-free environment is strictly preferred by everyone, we must have $Q_x < 0$. Additionally, assuming high-quality housing attributes are desirable, we have $Q_q > 0$.

Following Rosen (1974), preferences for the overall quality of housing services can be expressed by the bids for different levels of housing quality, θ, that yield the same level of utility $\bar{U}$. Assuming an individual exhausts his or her endowment y on housing and the composite good c, we must have $c = y - \theta$. Substitute this into equation 3.1 and invert, we obtain the bidding function:

$$\theta = \theta(Q(x,q), y, \bar{U}) \tag{3.2}$$

Equation 3.2 suggests that the willingness-to-pay for location depends on the level of reservation utility, the endowment y, and overall housing quality Q, which is determined by both x and q. To examine the impact of sex offenders on property prices, we implicitly differentiate θ with respect to x holding utility level constant. We obtain

$$\theta_x = \frac{U_Q}{U_c} Q_x < 0 \tag{3.3}$$

Because U_Q and U_c are strictly positive, and Q_x is strictly negative, we must have $\theta_x < 0$. Intuitively, θ_x is the marginal price an individual is willing to pay for one unit increase of sex offender intensity x. Not surprisingly, Equation 3.3 indicates that such a marginal price is negative. In other words, a nearby sex offender is a 'bad', or a neighborhood disamenity in this model. Individuals would be willing to pay to get rid of it. This also implies people must be compensated for accepting a greater x. In housing markets, such compensation takes the form of lower house prices. Therefore, Equation 3.3 generates our first testable hypothesis:

Prediction 1: An increase in sex offender intensity x reduces the willingness-to-pay for a particular location. Property prices fall as x increases.

The first prediction is rather simple and intuitive. Any exogenous shock that increases sex offender intensity would reduce property prices. Such shocks could be a sex offender moving into a sex-offender-free neighborhood. It could also be a new sex offender entering a community where some sex offenders already reside. Our model predicts that such shocks would hurt property value, which is straightforward and expected, but to this point we have only assumed homogeneous preferences. Yet, given a fall in value for these properties, the more interesting question is what happens next with regard to locational choices, particularly if individuals perceive

[4] By *other* location characteristics, we mean location characteristics other than the intensity of the nearby sex offender, x. Because x is the focus of our analysis here, we single it out from other location characteristics.

risks differently and have heterogeneous preferences over this particular type of disamenity.

To analyze location choices, we introduce heterogeneous preferences. We now assume individuals are identical except for their preference for sex offender intensity x. While everyone prefers a smaller x, the degree to which people dislike x could be different. A strong preference for a sex-offender-free environment can be represented by a smaller (more negative) value of Q_x. Intuitively, a more negative Q_x implies that each unit of increase in x reduces overall housing quality Q more significantly for individuals with a stronger distaste for, or risk aversion toward, sex offenders. Suppose there are two types of individuals, type i and j. Type i individuals are more strongly averse to living near sex offenders than type j. Thus, we must have $Q_x^i < Q_x^j$. Together with Equation 3.3, we must have

$$\frac{U_Q}{U_c} Q_x^i < \frac{v_Q}{U_c} Q_x^j \Rightarrow \theta_x^i < \theta_x^j \tag{3.4}$$

The implication of Equation 3.4 can be more easily understood via a simple illustration. $\theta_x < 0$ suggests that the bidding function θ must be downward sloping if plotted in the (x, θ) space. Equation 3.4 implies θ^i must be steeper than θ^j.

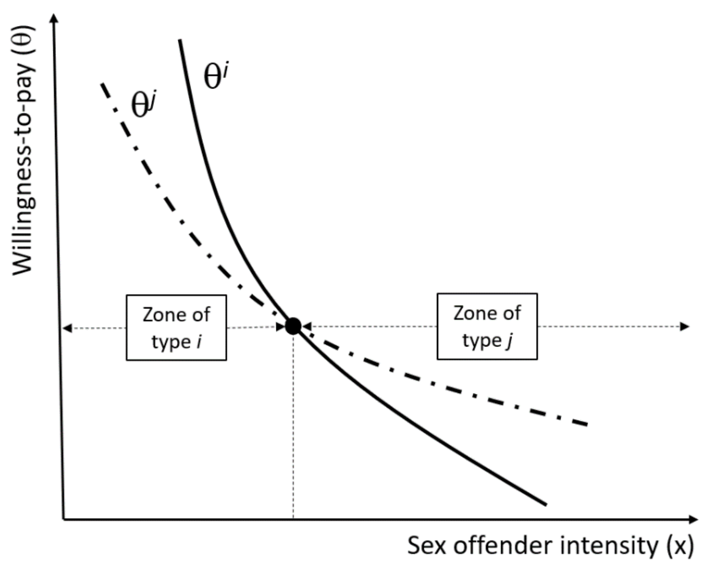

Fig. 3.1 Difference in willingness to pay between different types of individuals

This difference in willingness to pay between type i and j individuals for different levels of sex offender intensity is illustrated in Figure 3.1. The solid line, θ^i, represents the bidding function of type i individuals, who vigorously dislike or have a strong aversion toward x. The dashed line, θ^j, represents the bidding function of type j individuals, who also favor a sex-offender-free environment, but not as strongly as type i individuals. As shown in Figure 3.1, type i individuals would outbid type j for locations with lower sex offender intensity. On the other hand, type j individuals would outbid type i for locations with greater sex offender intensity. Because ownership goes to who offers the highest bid, sorting based on preference for x occurs. Specifically, type i individuals will reside at places with lower sex offender intensity, and locations with greater sex offender intensity will be occupied by type j individuals. Thus, we obtain the second testable hypothesis:

Prediction 2: individuals with a stronger preference for a sex-offender-free neighborhood will reside at locations with lower sex offender intensity.

Although the model we presented here is not a dynamic one, it is not hard to envision that an exogenous change of sex offender intensity would affect mobility. Imagine a sex offender entering a community. This, as indicated by our first prediction, will hurt nearby house values. Then, the mechanism outlined in our second prediction will lead people to rethink and modify their location choices made before such an exogenous shock. Residences who dislike sex offenders the most will have the strongest incentive to relocate and, most likely, be one of the first to do so. Furthermore, distaste for sex offenders will make a lot of potential buyers shun away from buying in the neighborhood. Increased supply and low demand may further cause house prices to drop. Other sex offenders, who perhaps have less of an aversion to their own group are attracted by the lowered prices, are more likely to move into the community. As a result, the relocation of sex offenders, then, becomes a non-random process, creating further shocks to the areas in which they move. Thus, this market process implies that price signals from declining home values associated with each additional sex offender precipitate a tipping dynamic, where a neighborhood may tip toward a critical mass of sex offenders, resulting in clustering.

3.4 Data

To explore clustering dynamics of registered sex offenders, we have obtained data from several sources. Our sample of real estate data consist of transactions from residential properties on the market between July 1999 and June 2009 and come from a central Virginia multiple listing service (MLS). The initial housing data contained 21,453 observations of both sold and unsold properties. As noted by other studies, MLS data are 'organic data' in the sense that it was initially collected and maintained for commercial purposes, entered by real estate agents, which means it can contain some incorrect or incomplete data. As a result, the data were carefully culled for incomplete, missing, or illogical outliers.[5] The final sample consists of 12,426 sold and 7,295 unsold properties.

The data collected from the MLS include typical property characteristics (square footage, bedrooms, baths, etc.), location, and market and calendar information (list date, sale date, length of listing contract). Specifically, our data includes numerous property characteristics, which we use as controls in our hedonic analysis below (e.g., square footage, age of the structure, acreage, number of bedrooms, bathrooms, length of the listing contract, indicators for whether the home is a one-story, new, vacant, whether it has a brick exterior, has hard wood floors, a pool, a fenced yard, or a walk-in closet). In addition, we control for the year and what time of year it was sold in (i.e. the season), macroeconomic controls (the unemployment rate in Virginia, Leading Economic Indicators Index, Consumer Sentiment Index), and

[5] Consistent with other studies using MLS data, we culled mobile homes and other outliers from our data set, confining our data to more 'typical' range of homes from $25,000 to $750,000. However, the findings of this study are not sensitive to dropping these observations. As an additional quality check, a sample of the MLS data was compared to county government records which contain data on price and housing characteristics and MLS data were 100% consistent with the tax data for our sub-sample.

area/neighborhood fixed effects (Census Block Groups). These control variables are commonly used within the real estate literature. Hence, we limit our discussion in this paper primarily to the variable of interest: the incremental effect of registered sexual offenders. We do include the summary statistics of our control variables (with the exception of the time and location fixed effects variables) in the summary statistics table, Table 3.4.

Table 3.1 Summary statistics for sold homes

Variable	Mean	Std. Dev.	Min	Max
Sale Price ($)	165,874.10	89,949.70	25,500.00	749,000.00
Time on Market (days)	110.55	88.79	0.00	963.00
Sold	0.61	0.48	0	1
Number of Sex Offenders $\leq$ 0.25 mile	0.08	0.38	0	9
First Sex Offender $\leq$ 0.25 mile	0.04	0.20	0	1
Second Sex Offender $\leq$ 0.25 mile	0.01	0.11	0	1
Third Sex Offender $\leq$ 0.25 mile	0.003	0.05	0	1
Four or More Sex Offenders $\leq$ 0.25 mile	0.002	0.04	0	1
Square Feet	1,924.02	782.09	417.00	8,418.00
Age (years)	26.42	28.15	0	267.00
Vacant	0.33	0.47	0	1
Bedrooms	3.20	0.78	1	8.00
Baths	2.04	0.69	1	6.00
Length of Contract (days)	186.83	102.98	0	990.00
One Story	0.39	0.49	0	1
New	0.16	0.37	0	1
Finished basement	0.27	0.44	0	1
Hardwood	0.55	0.50	0	1
Brick	0.54	0.50	0	1
Pool	0.17	0.37	0	1
Fenced Yard	0.17	0.37	0	1
Walk-in Closet	0.21	0.41	0	1
Acreage	2.04	7.67	0	248.66
Avg. Fixed Rate Mortgage at Sale Date	6.13	0.49	4.81	8.64
Virginia Unemployment Rate	3.57	0.59	2.20	7.10
Consumer Sentiment Index	86.17	10.45	55.30	112.00
Leading Economic Indicators Index	99.04	6.04	84.20	104.90
Fall	0.19	0.39	0	1
Winter	0.26	0.44	0	1
Spring	0.30	0.46	0	1
Summer	0.25	0.43	0	1

The Virginia Sex Offender and Crimes Against Minors Registry contains detailed information about registered sexual offenders and is available on a public website maintained by the Commonwealth of Virginia. However, the publicly available data on the website only relays a snapshot of sex offenders' current addresses at the time the website is accessed. While this is sufficient for homebuyers and real estate agents to check the registry when purchasing a property, from a research standpoint, this is problematic because a number of registered sex offenders move often and their history of addresses is not maintained on the public site. To overcome this issue, we have obtained a unique archived data set from the Virginia State Police which contains each of the registered sex offender's past addresses, including all move-in and move-out dates. Each observation also contains a registered sex offender's current address, along with a number of other personal characteristics (e.g., age, sex, race, description of the perpetrated crime, and whether that crime was a violent crime) that appear on the registry.

Over approximately ten years, there were 2,031 sex offenders who resided at 4,601 different addresses in the geographic area we focus on in Virginia.[6] The archived data we have obtained allows us to utilize historical real estate data effectively, essentially replicating the data that would have been accessible to homebuyers and sellers at any time during a property's marketing period, including at the time of the transaction. Analysis without such archived data is at best incomplete and is potentially biased. Unlike other studies, we did not cull the data set for transient sex offenders based on length of stay at a residence alone, although we did remove some whose obvious transient indicators did not seem useful for the purposes of our study. For example, we removed offenders if they were listed as homeless or as staying at a hotel or some other institution.

We obtain our variables of interest (i.e., whether a property listing was located near sex offenders), we use the great-circle distance formula to calculate the straight-line distance from a given house on the market to each sex offender.[7] From that, we tabulate the number of sex offenders that reside within a quarter-mile radius (i.e. $\leq$ 0.25 mile).[8] To explore the incremental impact of each offender, we then separated out this tabulation into subgroups. Dummy variables were created to correspond to each increment up to four offenders (with the fourth increment including each number of sex offenders beyond 4). Table 3.4 shows the breakout of each increment. The reference group, zero sex offenders, corresponds to the vast majority of homes that were sold in our sample (approximately 93%). In our sample, there were 584 (sold) homes that were located within a quarter-mile of a registered sex offender, 167 located near two, 38 located near three, and 23 located within a quarter-mile of four or more offenders.[9]

3.5 Methodology

3.5.1 Hedonic Diff-in-Diff Approach

We first employ a traditional OLS hedonic pricing model that accounts for heterogeneous characteristics of each property listing, its location, and time of sale. We use the following functional form:

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[6] Because our MLS covers a smaller subset of the Virginia real estate market (in central Virginia), the number of sex offenders corresponding to our area is smaller.

[7] This distance calculation (see below) approximates a straight line ("crow flies") distance, rather than a driving distance. To illustrate why this might be preferable for this study, consider an example where a sex offender lives in an adjacent property behind a home. That sex offender might actually be relatively far away in terms of driving time. Yet, the sex offender in that situation would likely be perceived just as risky as a sex offender next door (on the same street), as they may pose equal risk to, for example, children playing in the yard. Distance = 69.1 * $180/\pi$*arcos[sin(LAT_1)*sin(LAT_2)+ cos(LAT_1)*cos(LAT_2)*cos(LAT)*cos(LON_2–LON_1)].

[8] In a previous study, we explored different radii, finding an impact of registered sex offenders up to one mile. The impact, however, was a declining function in distance. For this study we confine our analysis to registered sex offenders with a quarter-mile, which is the smallest radius the produces reasonable variation in the number of sex offenders that reside within that range.

[9] These figures are higher for all categories when the data set it expanded to incorporate unsold homes in the Weibull hazard analysis below.

$$SP = \beta_0 + \sum \beta_i SO_i + \sum \beta_i X_i + \alpha_i + \varepsilon_P \quad (3.5)$$

where SP_i is a vector for property selling price,[10] X_i is a vector of property specific characteristics[11] and macroeconomic control variables,[12] α_i represents location and year fixed effects, SO_i, the variable of interest, indicates the presence of registered sex offenders who live (contemporaneously) within a quarter-mile of a home$_i$, and ε is an error term. We estimate SO_i in two forms. First, we estimate the effect the number itself has on sale price; and, second we look at the effect of each increment individually (i.e., first, second, third, and 4+ RSOs). The latter measures will help us investigate the increment at which a neighborhood might tip or the pricing dynamics that would lead to clustering.

Because location itself is a key source of differences in housing prices, hedonic analysis of the housing market must control for spatial heterogeneity. Following Pope (2008) and Wentland et al. (2014), we chose census block groups to control for time-invariant unobserved heterogeneity *across* these areas so that the explanatory variables' effects are identified from variation *within* a given area (or even in a given year, as is the case for time fixed effects).[13] This study uses block groups from the 2000 census, which on average contain between 600 to 3,000 people, usually around 1,500. Our sample of houses falls within a total of 163 census block groups in central Virginia. Like Wentland et al. (2014), we follow Davis (2004) and Heintzelman (2010) and cluster standard errors at the census block group level and use robust, heteroscedastic-consistent standard errors.

Note that like Wentland et al. (2014) and Brastow et al. (2018), this research design is a variation of a difference-in-difference (diff-in-diff) approach within a hedonic regression framework,[14] leveraging two sources of variation or 'differences' for the variable(s) of interest in comparison to the control group. That is, the control group is differentiated from the treatment by 1) presence of a sex offender in a time dimension (i.e., a listing that appears on the market 'post' or after a sex offender moves in nearby) and 2) the severity of the treatment along a distance dimension (near vs. far — a quarter mile in this study). Hence, the interpretation of the treatment effects below relative to the control group can be interpreted similarly to these other studies. Using a very similar data set to these studies (although with some differences in design choices), we refer the reader to these papers for evidence of

[10] Following Wentland et al. (2014), we use a linear form of sale price, but we also explored a number of non-linear functional forms and our results remain robust. Kuminoff et al. (2010) survey 69 hedonic studies and find no single dominant functional form, as 80% rely on linear, semi-log, or log-log functional form.

[11] We use the following property specific variables: square footage, age, acreage, number of bedrooms, bathrooms, length of the listing contract, whether the home is a one-story, new, vacant, whether it has a brick exterior, hardwood floors, a pool, fenced yard, walk-in closet.

[12] In addition to year fixed effects, we use the following macroeconomic controls: season the home sold, Consumer Sentiment Index, fixed rate mortgage interest rate at the sale date, Virginia unemployment rate, and the Leading Economic Indicator Index. The macro controls are monthly aggregates, which correspond to the month the home was sold.

[13] According to the U.S. Census, census tracts are "small, relatively permanent statistical subdivisions of a county ... designed to be homogenous with respect to population characteristics, economic status, and living conditions." Yet, census block groups are subsections of census tracts and the smallest spatial area for which the U.S. Census tabulates sample data. For more detail, see: http://www.census.gov/geo/www/cob/tr_metadata.htmlgad.

[14] See both Pope (2008) and Linden and Rockoff (2008) for a similar approach. For more detail about this approach, see their explanation for how this research design can exploit quasi-random variation in sex offender movements.

parallel trends and placebo tests that support our identification strategy, which we omit here for brevity.

3.5.2 Heterogeneous Effects by Number of Bedrooms

One test proposed by Roberts and Whited (2013), among others, is to exploit systematic variation in the treatment or among treated groups as a check on the internal validity of a difference-in-difference treatment design. If the design here is properly identified, then we should observe whether effects of nearby sex offenders vary with the intensity of household preferences (as predicted by our theoretical framework) that would show an exacerbated response. To this end, we attempt to differentiate the market response by introducing 'family' homes vs. 'non-family' treatment groups (akin to introducing an additional 'difference' or diff-in-diff-in-diff approach). We proxy for family-oriented preferences (i.e. whether the home has children or plans to have children) based on the number of bedrooms the home has. Based on evidence from the 2009 American Housing Survey, we can reasonably assume that homeowners with more children would be interested in homes with three or more bedrooms, all else equal, and would exhibit greater risk aversion to residing near sex offenders.[15] If a potential homeowner has children or plans to have children, then perceived risk associated with living more sex offenders, and thus heightened negative disamenities, should be larger for this subsection of the market.

Therefore, we build on the baseline model (Equation 3.5) by extending it in the following way:

$$SP = \beta_0 + \sum \beta_i SO_i * 3\text{Bedrooms}_i^+ + \sum \beta_i SO_i + \sum \beta_i X_i + \propto_i + \varepsilon_P \qquad (3.6)$$

where $SO_i * 3Bedrooms_i^+$ is an interaction term, multiplying the sex offender variables of interest by a dummy variable proxy for 'family homes' (which equals 1 if a listed property has 3 or more bedrooms, 0 otherwise). This additional 'difference' incorporated into the model allows the incremental impact of sex offenders to differ based on whether the home is a 'family' home or not (as proxied by bedrooms), allowing two and one bedroom homes to be among the potentially 'less treated' control.

3.5.3 Simultaneous Equation (3SLS) Model

Following a strand of papers in the hedonic real estate literature, including Brastow et al. (2018) and Wentland et al. (2014) discussed above, we also specify two equations in which sales price and liquidity are jointly determined (Krainer, 2001). An empirical complication in this literature is the fact that a home's price and liquidity, as measured by time on market, are simultaneously determined largely by identi-

[15] Our estimates hold attributes such as square footage constant. So, additional bedrooms, holding square footage constant, essentially means that homebuyers are getting more, albeit smaller bedrooms. There are a number of reasons why they might want additional bedrooms holding square feet constant, but the most straightforward explanation is additional children (or openness to having children in the future).

cal factors. We utilize an identification approach developed by Turnbull and Dombrow (2006), which identifies the system by two variables the characterize neighborhood market conditions. Turnbull and Dombrow (2006) define neighborhood competition, C, as a distance and marketing time overlap measure of competition from nearby houses for sale. An alternative measure, listing density, L, divides C by the marketing period of property i to measure competition per day from nearby listings.[16] Simultaneous equations take the form:

$$SP = \beta_0 + \beta_1 TOM + \beta_2 L + \sum \beta_i SO_i + \sum \delta_i X_i + \propto_i + \varepsilon_P \tag{3.7}$$

$$TOM = \alpha_0 + \alpha_1 SP + \alpha_2 C + \sum \beta_i SO_i + \sum \delta_i X_i + \alpha_i + \varepsilon_L \tag{3.8}$$

where TOM_i is a home's time on market (measured in days) and the other variables are defined above. We model simultaneity using a three-stage least-squares (3SLS) approach. In a 3SLS setting, Equations 3.7 and 3.8 form the system of equations between property price and time on market. In addition, 3SLS incorporates an additional step with seemingly unrelated regression (SUR) estimation to address the issue of correlations across error terms.[17] This functional form is similar to the baseline reduced form OLS equation, but allows us to estimate the effect on time on market and correcting for simultaneous equation bias.

3.5.4 Parametric (Weibull) Hazard Model – A Censoring Correction

While we model the incremental effect of sex offenders on time on market, the estimates from the 3SLS models in the previous section must necessarily be limited to homes that sold. According to Lancaster (1990, Chapter 8), a substantial amount of information is effectively thrown away with OLS (and, in our case, 3SLS) models that do not or cannot incorporate the full sample due to censoring or other issues with the data. Accordingly, we incorporate information on homes that did not sell in the estimation of a Weibull parametric hazard function using the full sample of sold and unsold homes to account for such censoring issues.

A common approach in the real estate literature is to estimate a parametric hazard model that assumes the baseline distribution follows a Weibull distribution (e.g., see Rutherford and Yavas (2012), Bian et al. (2015), or Brastow et al. (2018)). For t, (time on market) as measured in days, we estimate the following parametric Weibull hazard:

$$\lambda\,(t; x_i) = \exp\,(x_i \beta)\,\alpha t^{\alpha - 1} \tag{3.9}$$

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[16] $C(i) = \sum_j (1 - D(i,j))^2 \{\min[s(i), s(j)] - \max[l(i), l(j)]\} + 1$, $L(i) = \sum_j (1 - D(i,j))^2 \{\min[s(i), s(j)] - \max[l(i), l(j)]\} / s(i) - l(i) + 1$, where 'D(i, j)' is the distance between a given home on the market, i, and a given nearby listing, j, provided that they live within a one-mile radius, $s(i)$ is the sell date for a given home on the market i, $l(i)$ is the listing date for a given home on the market i, $s(j)$ is the sell date for a competing home j, $l(j)$ is the list date for a given competing listing j.

[17] According to Belsley (1988), 3SLS is used instead of 2SLS in estimating systems of equations because it is more efficient, particularly when there are strong interrelations among error terms. Intuitively, in hedonic regressions, unobservable quality components (not already captured by its measurable physical characteristics in the MLS listing) could drive this correlation among error terms across equations (see also Bian et al. (2015)).

where x_i are the same covariates from the prior TOM equation and incorporates list price (LP_i) instead of sale price (SP_i) so that the full sample could be used in the hazard estimation. Further, all models cluster standard errors by census block group and report heteroscedastic-consistent robust standard errors. We report the results in both the accelerated failure time form and log-relative hazard form to interpret the hazard results in regards to time on market and the probability of sale (failure), respectively. The coefficients can be interpreted as semi-elasticities with regard to right-hand side variable by exponentiating the coefficients, analogous to a semi-log interpretation in OLS.

3.5.5 Geospatial Analysis of Tipping and Clustering

If the presence of nearby sex offenders has an incremental impact on real estate, then subsequent tipping of neighborhoods toward clusters of sex offenders should be an observable geospatial phenomenon. In this section, we briefly outline how we may exploit basic GIS techniques to explore implications of the pricing dynamics that may flow from our theoretical and empirical models above.[18]

Following Clark and Evans (1954), we begin by conducting a simple Nearest Neighbor Analysis (NNA) in order to determine the pattern and distribution of a population in precise and meaningful terms. A Nearest Neighbor Analysis is often utilized as a procedure to classify the geographic distribution of a set of points according to their spatial pattern and proximity (Griffith and Arnrhein, 1991). This analysis has been applied to a diverse variety of fields including the study of spatial dispersion patterns for individuals suffering from dengue fever in Malaysia (Aziz et al., 2012). Second, we also incorporate a preliminary, broad spatial distribution analysis of registered sex offenders in order to conduct an Average Nearest Neighbor Analysis (ANNA).[19] The average nearest neighbor classification is particularly effective at looking at a large data set and understanding the hypothetical random dispersion compared to the observed positioning. The average nearest neighbor analysis determines whether a set of spatial points can be classified as distributed in a dispersed pattern, random pattern, or a clustered pattern. This is accomplished on the basis of the Average Nearest Neighbor Ratio (ANNR) value. Two distances make up the two key components of the Average Nearest Neighbor Ratio (ANNR). The ANNA tool measures what the distance to the nearest neighbor centroid is, and what it should be hypothetically if the dispersion is random. These are referred to as the observed mean distance (OMD) and the estimated mean distance (EMD) respectively. If the average distance is less than hypothetical random distribution, the ANNR is less than 1, and the distribution is considered clustered. If the average distance is greater than the hypothetical distance for random clustering, then the ANNR is greater than 1, and the distribution is considered dispersed. The point at which these two values meet can be construed as a 'tipping point' or critical period in which the points move from dispersed to clustered or clustered to dispersed.

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[18] The GIS analysis uses the full Virginia data set of sex offenders, including areas both inside and outside our MLS geographical area.

[19] Average Nearest Neighbor Ratio is given as follows: $ANN = \overline{D_o}/\overline{D_E}$ where $\overline{D_o}$ is the observed mean distance between each feature and the nearest neighbor: $\overline{D_o} = \frac{\sum_{i=1}^{n} d_i}{n}$ and $\overline{D_E}$ is the expected mean distance given a random pattern: $\overline{D_E} = 0.5/\sqrt{n/A}$. In the previous equations d_i equals the distance between feature i and its nearest feature, n corresponds to the total number of features, and A is the total study area.

The distance between points was measured as a Euclidean distance or 'as the crow flies' in meters. The precise field area was set as the minimum enclosing rectangle around the features. The null hypothesis for the ANNA is that all the points are randomly distributed. The null hypothesis is tested by Z statistic. The Z score value is a measure of statistical significance which tells us whether or not to reject the null hypothesis. Higher and lower numbers identify virtually no chance of random distribution. The ANNA also identifies a P value for the data set. This P value is the probability that null hypothesis will be falsely rejected.

For the initial analysis, we divided the registered sex offender data into 6 month increments. Each 6 month grouping identifies the registered sex offenders who lived within Virginia at that time regardless of the duration they lived at that particular address. The first grouping, designated (a), includes those registered sex offenders who were living in Virginia during the time of January 1st to June 30th. The second grouping, designated (b), includes those registered sex offenders who were living in Virginia during the time of July 1st to December 31st. Each 6 month increment made up a single layer feature class, and we utilized the average nearest neighbor analysis to all points within that layer. We applied the ANNA to 24 total data layers. The utilization of the ANNA in this fashion identified a general trend of clustering over time with a specific tipping point apparent. This preliminary analysis also identified a time in which a significant number of registered sex offenders were present within the study area.

3.6 Results -– The Effects of Clustering and the Role of the Price Mechanism

3.6.1 The Price of (Sex Offender) Clustering

The initial set of results reveals a significant relationship between the number of sex offenders and real estate outcomes, namely price and liquidity (as measured by days on market). While there are some differences in model specifications, the initial results are qualitatively consistent with Wentland et al. (2014). The first two columns of Table 3.6.1 show the general impact on sale price. Column 1 indicates that an additional sex offender located nearby (i.e. within 0.25 mile) lowers the sale price of a home by approximately $5,455. This provides preliminary evidence that registered sex offenders present a negative externality and the negative externality is an increasing function in sex offenders, which is generally consistent with the literature discussed in section two. However, the incremental impact of each sex offender may be more nuanced, which is the primary focus of this paper.

Column 2 breaks out the variable of interest into four increments (with the reference group being zero) that allow the effect to vary across increments in a nonlinear fashion. Column 2 shows that the first nearby sex offender has a large, significant effect on the sale price of a home, amounting to a discount of $8,338 or approximately 5% for the average home. The second and third nearby sex offenders do not have a statistically significant impact on price, however, implying a discontinuous or non-linear preference for living near additional sex offenders. While this does not necessarily confirm an equilibrium (or equilibrium-like behavior) at some low number of sex offenders, this evidence would be consistent with the idea that the first sex offender has a large initial effect on home prices, but an equilibrium may exist

Table 3.2 The incremental effect of sex offenders on a home's sale price and days on market – OLS and 3SLS models

Variable	OLS Model Dependent Variable: Sale Price	OLS Model Dependent Variable: Sale Price	OLS Model Dependent Variable: Sale Price	3SLS Model Dependent Variable: Sale Price	OLS Model Dependent Variable: Days on Market
Number of Sex Offenders ≤ 0.25 mile	-5,454.72***				
	(-2.81)				
First Sex Offender ≤ 0.25 mile		-8,337.78***	-14,843.68***	-7,692.50***	51.94***
		(-3.30)	(-2.68)	(-3.99)	(3.39)
Second Sex Offender ≤ 0.25 mile		-6,861.99	-2,543.79	-6,395.65*	46.81**
		(-1.19)	(-0.30)	(-1.84)	(2.01)
Third Sex Offender ≤ 0.25 mile		-10,697.24	-14,613.54	-8,181.99	61.78
		(-1.28)	(-0.89)	(-1.16)	(1.36)
Four or More Sex Offenders \leq 0.25 mile		-25,099.07**	7,830.07	-26,016.15***	163.95**
		(-2.07)	(0.89)	(-2.73)	(2.43)
Bedrooms (3+) * First Sex Offender			8,908.58		
			(1.08)		
Bedrooms (3+) * Second Sex Offender			-5,481.68		
			(-0.60)		
Bedrooms (3+) * Third Sex Offender			4,498.29		
			(0.24)		
Bedrooms (3+) * Four or More Sex Offenders			-43,765.75***		
			(-4.21)		
Property Characteristics	✓	✓	✓	✓	✓
Macroeconomic Controls	✓	✓	✓	✓	✓
Season Fixed Effects	✓	✓	✓	✓	✓
Census Block Groups	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	12,426	12,426	12,426	12,362	12,362

Notes: This table presents results of OLS and 3SLS estimations of the incremental impact of registered sex offenders on sale price and days on market (time on market) across different specifications (OLS and 3SLS); t-statistics (z-statistics) in parentheses; ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively. Coefficients on controls are omitted for brevity.

in neighborhoods with a low number of sex offenders (three or less). A low number of sex offenders may be consistent with a stable, initial discount of approximately 5% with a potential noisier discount associated with additional offenders.

If a neighborhood tips toward a critical mass of sex offenders, we would expect this to be reflected by (or precipitated by) the pricing of these homes. Indeed, we see that the equilibrium (or quasi-equilibrium) appears to break down once the fourth sex offender moves in. Table 3.6.1 shows that a cluster of four or more sex offenders coincides with a much larger, statistically significant discount on sale price. Specifically, a cluster of four or more offenders is associated with a sharp $25,099 (or 16%) drop in price of nearby homes. Given the steep discount on home prices, the cluster of sex offenders provides a stronger incentive to sort based on the theoretical framework outlined previously. All else equal, this strong price signal coupled with the likelihood that sex offenders are likely to be less averse to living near one of their own group members, we would expect additional sex offender clustering around areas with multiple sex offenders as a result of the sharp discount associated with this clustering. Thus, this provides initial evidence that pricing dynamics illustrate a mechanism through which a clustering or tipping dynamic could occur in housing markets. Whether we find evidence of additional clustering over time in this data is a hypothesis that will be explored with GIS spatial analysis.

3.6.2 The Different Price of RSO Clustering for Families

Household preferences for living near sex offenders are not likely to be uniform. Households with children may be particularly averse to living near sex offenders, which will be reflected in their bid functions for housing. After creating a simplified proxy for 'family homes' (i.e. a variable indicating whether a home has three or more bedrooms), we attempt to disentangle some of the tipping dynamics of the market by interacting this proxy with our variables of interest. The results are shown in Column 3 of Table 3.6.1. The estimates of this model show that the discount for the initial sex offender for 'non-family' homes is $14,844. Each additional sex offender does not have a statistically significant effect, implying an approximately uniform discount for living near sex offenders (regardless of the number) for 'non-family' homes.[20] The most striking result of this column is that 'family' homes have a very large discount, approximately $43,765 or 26%, associated with living near a cluster of four or more sex offenders. Given the potentially greater risk/externality that sex offender recidivism imposes for families with children, this result is consistent with the prediction of both intuition and our theoretical framework. More generally, given that we find that an intensified (nonlinear) treatment effect on a more sensitive treatment group, this also serves as additional evidence that our results are properly identified.

3.6.3 The Liquidity (or Time on Market) Effect — 3SLS

The incremental effect of sex offenders may not be capitalized solely in price, particularly as price and liquidity are jointly determined in this market. The estimated 3SLS model, shown in Columns 4 and 5 in Table 3.6.1, shows that the externality associated with additional sex offenders is reflected by both price and liquidity. The impact of the initial offender on price is slightly smaller (-$7,692), as the effect is also capitalized into liquidity by extending the time it takes to sell the home by 52 days (or 47%) on average. There is a smaller (less statistically significant) effect for the second sex offender, but no statistically significant impact for the third offender. Generally, this is consistent with the previous results like Wentland et al. (2014) and Brastow et al. (2018), suggesting a significant, moderate external liquidity effect associated with living near a few sex offenders. Moreover, there is also a large, significant effect associated with living near four or more offenders. While the price discount is approximately the same as the OLS estimate, the striking result is the massive effect on a home's liquidity. A cluster of four or more nearby offenders increases a home's time on market by 164 days, or 147% on average. While it is not surprising that homes located near a cluster of sex offenders sell for a siz-

[20] The coefficients that are not individually significant in this regression are also not jointly significant, at conventional levels of statistical significance. For all intents and purposes, the insignificant coefficients are treated as zero in this analysis. Also note that the specification here is somewhat different from Wentland et al. (2014), which has a linear interacted bedrooms specification. This analysis suggests that there may also be a nonlinear effect.

able discount and are much harder to sell in a reasonable amount of time, the sheer magnitude of the effect remains striking, possibly reflecting a much smaller pool of potential buyers willing to bid on one of these homes.

3.6.4 The Liquidity Effect and Probability of Sale – A Parametric (Weibull) Hazard Model

Using the full sample of sold and unsold properties, a parametric hazard function was estimated to account for related selection issues (which were explained in further detail in the methodology section above). The results in Table 3.6.4 are generally consistent with the three stage findings, with a couple notable exceptions. The magnitude for the effect on time on market appear to be more modest across the board as compared to the three stage results, but in this specification, the larger externality may be the reflected by the presence of the third offender, not the forth. The third offender represents a substantially longer time on market, and correspondingly, a much lower probability of sale in the hazard model specifications. The cluster of four or more still represents a large, negative externality based on the Weibull hazard model results, which we interpret as qualitatively consistent with the three stage results from the previous sub-section.

3.7 Results -– Tipping and Clustering Evidence from GIS Spatial Analysis

We conduct a basic geospatial analysis of our sample, finding initial evidence of additional clustering of registered sex offenders over time. The result of an average nearest neighbor analysis gives several values, which include the Average Nearest Neighbor Ratio (ANNR), the observed mean distance (OMD), the estimated mean distance (EMD), a z-score and a p-value. The initial Average Nearest Neighbor Analysis begins at a starting point of three registered sex offenders in the last six months of 1997. This initial analysis produces an ANNR value of 3.847748. The same analysis was completed on each six month grouping. We identified a potential tipping point within the first six months of 2003. Around the tipping point at which the observed mean distance reached the estimated mean distance, the ANNR reached 1.007778. The ANNR generally continued to decrease as seen in Figure 3.2, with the smallest ANNR is found in the 2009a data set. The Z score values identify that the RSO distribution pattern within the study area has a less than 1% chance of being the result of chance. Figure 3.3 shows the dramatic dip, as increasing clustering of registered sex offenders occurs on a state-wide level.[21]

To summarize, the GIS analysis shows a clear trend of clustering, as some neighborhoods tip toward being more densely populated by sex offenders. This offers

[21] In untabulated analysis, we created census blocks as a way to normalize the ANN analysis for populated areas within Virginia. We are able to statistically identify traits of those census blocks which contain RSOs with our sample. We compare the location of the RSOs to the census blocks in order to develop a general temporal trend and identify if any other ‘tipping point’ exists. Our analysis showed that despite the potential for the RSOs to live in any of the 285,762 census blocks, there is a spatial clustering within certain small census blocks over time. We leave more in-depth spatial analysis to future research.

Table 3.3 The incremental effect of a registered sex offender on a home's time on market and probability of sale – Weibull accelerated failure-time form and log relative-hazard form

Variable	Weibull Distribution Coefficients Dependent Variable: Time on Market	Weibull Distribution Coefficients Dependent Variable: Sold
First Sex Offender ≤ 0.25 mile	0.098**	-0.146**
	(1.98)	(-1.99)
Second Sex Offender ≤ 0.25 mile	0.012	-0.018
	(0.17)	(-0.17)
Third Sex Offender ≤ 0.25 mile	0.794***	-1.179***
	(3.30)	(-3.29)
Four or More Sex Offenders ≤ 0.25 mile	0.538**	-0.798**
	(2.50)	(-2.50)
/ln_p	0.394***	0.394***
	(28.35)	(28.35)
p	1.48	1.48
Property Characteristics	✓	✓
Macroeconomic Controls	✓	✓
Season Fixed Effects	✓	✓
Census Block Groups	✓	✓
Year Fixed Effects	✓	✓
Observations	19,701	19,701
Sold Homes	12,406	12,406

Notes: This table estimates the external effects an RSO on home liquidity using a parametric hazard that follows a Weibull distribution. Results in Column 1 report accelerated failure-time coefficients, while the results in Column 2 report log relative hazard form coefficients. Robust z-statistics in parentheses (Errors Clustered by Census Block Group) *** p¡0.01, ** p¡0.05, * p¡0.10.

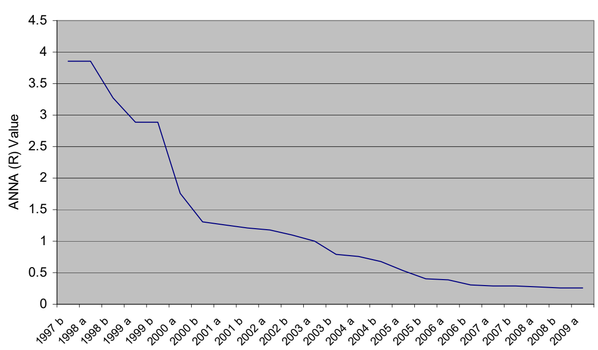

Fig. 3.2 ANNA statistic results by 6 month increment

additional, complementary evidence that the pricing dynamics are consistent with sex offender sorting.

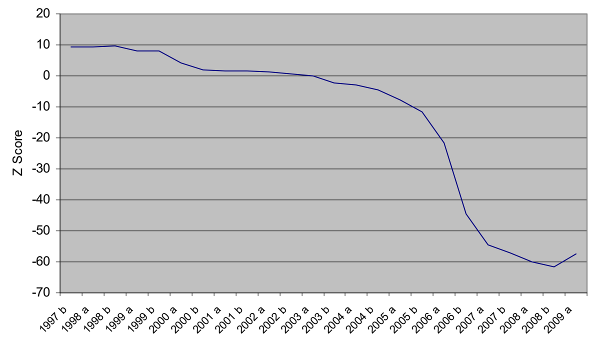

Fig. 3.3 ANNA Z score statistical results by 6 month increment

3.8 Conclusion and a Discussion of Efficiency Implications

Prior real estate research has found that registered sex offenders (RSOs) impose external costs, given potential risks of recidivism, which are capitalized into the value and liquidity of nearby residential real estate. Several studies have shown that registered sex offenders lower nearby residential home prices significantly. In this study, we find that sex offenders have a tendency to cluster in certain areas. To explain this phenomenon, we investigated whether there is evidence that market prices facilitate this process, given that RSOs could be attracted by lower prices and being less concerned with living near other sex offenders (as compared to non-offenders' concerns about living near offenders). Using a decade of multiple listing service (MLS) data in Virginia, along with corresponding sex offender data obtained with the help of the Virginia State Police, we found that the initial sex offender that moves in nearby has a significant negative effect on a home's price and liquidity. We find evidence of nonlinear effects, where each successive sex offenders that move in nearby have little significant impact until the neighborhood reaches a critical 'tipping point,' where a cluster of four or more sex offenders has much larger negative effects on the price and liquidity of nearby homes. A within-neighborhood analysis shows that living near a cluster of four or more sex offenders may reduce a home's sale price by approximately \$26,000 (or 16%) and increase the time a home spends on the market by 164 days (or 147%), on average.

Overall, the results provide evidence consistent with the hypothesis that market prices facilitate the sorting/tipping process we observe, where individuals operating within the market sort according to their preferences and incentives, resulting in segmentation of these two groups (i.e. sex offenders vs. general populace). The geospatial evidence is consistent with increasing clustering of registered sex offenders and segregation of these two groups. This research may help real estate academics and practitioners understand the locational choices/tendencies of registered sex offenders and the implications for real estate and residential mobility.

While these results are striking, it raises a broader question about whether this process is efficient. We can offer a brief discussion based on the findings here. The steep negative externality associated with living near a cluster of sex offenders at first gives one pause that a negative externality is leading to a serious issue for real estate markets. However, note that in Table I.B., the number of homes that are located nearby clusters is very small. So, while the externality effect is large for an individual listing on the market, it is not necessarily the case that the overall effect on the market is larger. The opposite could be true, depending on how many homes are affected by this externality. Intuitively, if the external impact of an RSO is spatial in nature (i.e., a decreasing function in distance), then a cluster contains overlapping externalities that are concentrated in a particular area. Conceptually, this runs parallel to the idea that it might be more efficient to concentrate pig farms, or some other spatially-specific negative externality like a paper mill, in a particular area or cluster rather than scattering them randomly next to housing developments, for example. While this may not be applicable for all externalities (nor is efficiency the only value policymakers need to take into account), this reasoning is a key motivating idea behind local zoning restrictions. However, while states and localities do have restrictions on where RSOs can live and work to induce a similar sort of clustering, the evidence from Virginia from our study suggests that the market provides an additional incentive to cluster, which is unplanned and decentralized. Thus, the extent to which real estate markets provide a price incentive to cluster negative externalities could be an extension of classic ideas in economics, whether it is Adam Smith's ([1776] 1937) invisible hand metaphor more generally, or Coase's (1960) idea more specifically, that market exchange can lead to efficient outcomes through a decentralized price mechanism (even when externalities are present, depending on the circumstances). We leave further exploration of this in real estate and other applications to future research.

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Chapter 4
The Rental Next Door: The Impact Of Rental Proximity On Home Values

Wendy Usrey

Abstract The literature on the spillover effects of rental properties mainly focuses on the impacts of multi-unit complexes and low income housing. Little has been done specifically looking at the relationship between single-family rental homes and sales price. This paper extends the literature by modeling the impact of single-family rental proximity on home sales price using a spatial approach. Through the use of GIS software, I was able to specifically measure the density of single-family rental properties for each sold home, rather than following the "blanket approach" of measuring density as the 'percent of rentals in the census tract' typical in the literature. With data collected for 2,766 homes sold between January 1, 2011 and July 1, 2012 in Fort Collins, Colorado, a hedonic price model was used to empirically test for the impacts of rental proximity on home values. I find strong evidence that proximity to single-family rental homes plays an important role in determining a home's selling price. Rentals within 1/4 mile of a sold home had a negative impact on price, while rentals between 1/4 and 1/2 mile had a positive impact on price. If rentals are considered an alternative to foreclosure or short sale, these results suggest the negative impacts of distressed sales are greater than those of rental properties on surrounding home values.

4.1 Introduction and Background Information

People generally tend to believe that rental properties will impact their neighborhood negatively as more and more homes in the subdivision turn into rental properties. Many homeowners' associations attempt to reduce the number of rental properties by regulating the placement of signs, restricting the number of rental properties in the subdivision, and in some cases, outright banning rental properties. In some cities, policy makers try to reduce the number of rental properties by passing ordinances (such as the three unrelated ordinance in Fort Collins, CO), requiring rental inspections, regulating sign placement, and charging registration and/or licensing fees. Even the mortgage industry is "anti- rental," with larger down payments required for non-owner occupied homes and "Fannie Mae and Freddie Mac refus[ing]

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to underwrite mortgages in condo projects where a majority of units are rentals" (Reagan, 2009).

The general public is quick to assume a rental property will "ruin" their neighborhood, and this is traditionally the assumption in most literature. This opposition has been well documented and takes many forms. Some of the most common arguments against rental housing include: rentals attract low-income residents and less desirable neighbors, lead to an increase in crime and traffic and place an additional financial burden on local governments and schools. Opposition also stems from the belief that rental properties are not as well-maintained as owner-occupied homes and will overall lead to a decline in property values (Ellen et al., 2007; Obrinsky and Stein, 2007; Sirmans, 1996). These arguments have been supported in some of the empirical literature. Wang et al. (1991) concluded that "an inverse relationship exists between the value of a house and the presence of rental properties in the area." Specifically, they found that the presence of rental properties within close proximity (defined as the group of 5-8 surrounding homes) reduced a home's selling price by approximately 2% (Wang et al., 1991). Similar results were found in several studies looking at the impact of subsidized and multi-family rental housing developments, though the results were mixed and varied widely between the studies (Goetz et al., 1996).

While at first glance it would appear that rental homes are "bad" for a neighborhood and property values, the evidence is mixed overall. A study analyzing the impact of federally subsidized rental housing on property values found that in most cases, property values were not reduced by federally-subsidized developments and in many instances the developments actually led to increases in property values (Ellen et al., 2007). Grether and Mieszkowski (1980)Grether and Mieszkowski found that proximity to "low-density apartment developments [were] relatively harmless" in terms of their impact on residential property values, and a document produced for the National Policy Summit on Rental Housing summarized numerous studies in which the authors determined property values were higher in areas near multi-family housing or "proximity to subsidized housing made no difference in housing values..." (Obrinsky and Stein, 2007).

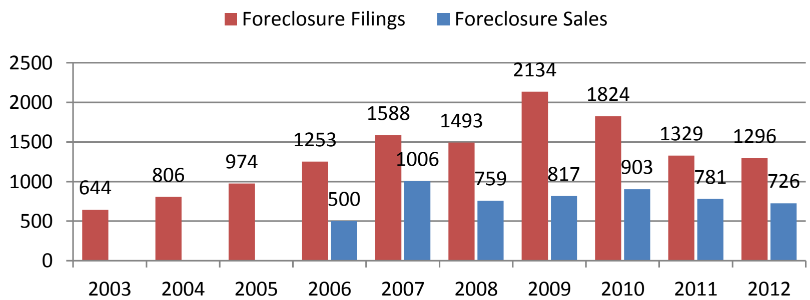
A recent addition to the debate is how economic conditions are playing a role in the desirability of rental homes. With the housing crisis plaguing the United States, home prices are down in many areas which means many homeowners can no longer sell their properties without taking a loss, or bringing money to the table. As of March 1, 2012, an estimated 11.1 million homeowners were underwater on their mortgage.[1] That's approximately 22.8% of mortgages and is at the highest level since the third quarter of 2009, when the data was first tracked (Reuters, 2012). Stricter lending requirements, along with higher rates of unemployment, means fewer people are willing or able to buy. As a result, many homeowners are faced with tough choices: sell at a loss, attempt a short sale, let the home go to foreclosure, leave the home vacant, or rent it out until conditions improve. For homeowners in these situations, rental restrictions limits their options. If they can't afford to make payments on two homes or don't have money to bring to the closing table while unable to rent out the home due to rental restrictions, their only option left is to attempt a short sale or let the home go to foreclosure.

It is commonly assumed that foreclosures and vacant homes decrease property values, and many studies have shown that this is assumption is accurate. Immergluck

[1] A borrower is considered to be underwater when their loan balance is higher than the current market value of the property.

and Smith (2005) determined that in Chicago, each foreclosure within 1/8 mile of a single-family home results in a decrease in value of approximately 1%, and Lin et al. (2009) also focused on the City of Chicago and found that a foreclosure within 0.9 kilometers of a home reduces its value by 8.7%. Mikelbank (2008) analyzed the impact of foreclosures and vacant/abandoned properties in Columbus, Ohio and determined that each foreclosure negatively impacted sold homes between 1% and 2%, while vacant or abandoned properties negatively impacted sold homes between 0.5% and 3.5% depending upon their on distance from the homes. For many locations, the debate has turned into choosing between the 'lesser of two evils.' Allow rentals, keeping homes occupied with fewer foreclosures, and accept the "problems" that come from non-owner occupied homes or ban rentals, and instead deal with the empty properties and increased foreclosures.

Fig. 4.1 Larimer County foreclosure filings and sales



Source: Colorado Division of Housing (2012).

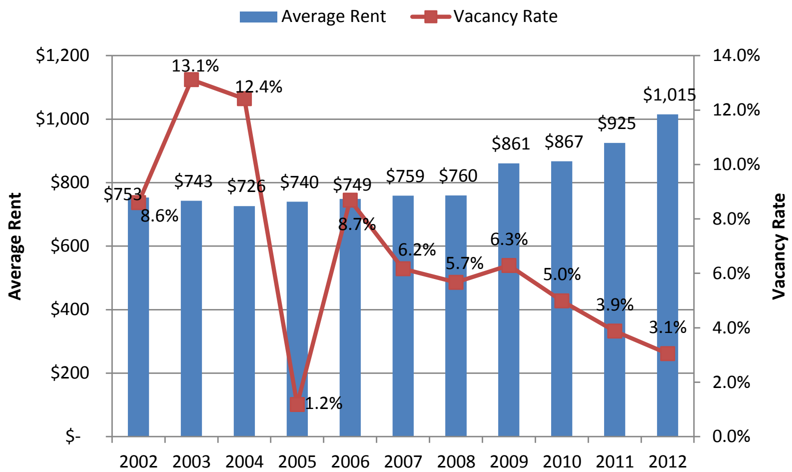
The foreclosure crisis has not only impacted property values and homeowners. Along with falling homeownership rates, the increase in foreclosures has led to a large number of households transitioning from homeowners to renters. Across the nation, rental vacancy rates have reached a ten year low, while rents continue to climb, and the rental market in Fort Collins, Colorado is no exception (Callis and Kresin, 2012). Figures 4.1 and 4.2 show the relationship between foreclosures and rental demand in Fort Collins.[2]

In 2009, foreclosure filings in Larimer County peaked, creating higher demand for rental housing. Between 2009 and 2012, as fewer rentals were available (indicated by the decreasing vacancy rates), the average rent increased by 17.9%. Rental housing policy typically focuses on balancing the need for affordable housing with the needs of homeowners' concerned with preserving the quality of their neighborhood and protecting the value of their homes. Policy discussion tends to largely focus on low-income affordable housing (i.e. subsidized housing, Section 8 housing, etc) despite the fact that such types of rental properties make up a small percentage of overall rental housing in most communities.[3] Single-family rental homes play an important role in providing quality housing to people who are unable or unwilling to purchase their own homes, and are "essential for households at middle-income and lower-income levels" (Colorado Division of Housing, 2011).

[2] Values for 2012 are estimates based on the monthly data available at the time of the analysis.

[3] For example, in Larimer County, affordable rental units (including income restricted, senior and disabled housing) make up just 10.8% of renter-occupied units (Community Strategies Institute, 2009).

Fig. 4.2 Fort Collins rent and vacancy rates



Source: Colorado Division of Housing (2011).

According to the American Housing Survey, in 2010, 34.5% of occupied housing units in Fort Collins were home to renters. The estimated median household income for renter-occupied housing was $33,803, while the estimated median household income for owner-occupied housing was $70,441. In general, housing is considered affordable when housing expenses do not exceed 30% of a family's gross income (Wallace, 1995). Based on data obtained from the 2011 American Community Survey, it was estimated that 28% of owner-occupied households and 53% of renter-occupied households in Fort Collins had housing costs that exceeded 30% of their income.

Due to the high cost of housing (relative to income), and the overall dynamics of the housing market in Fort Collins, the rental-housing debate is a particularly contentious issue. With high home prices, rock bottom vacancy rates and increasing rents, local policy makers are faced with concern from both homeowners and renters. Finding a balance between supporting home prices and providing affordable housing within the community requires that policy makers have access to as much information as possible. This study aims to provide additional insight into the specific dynamics of the housing market in Fort Collins, in terms of the relationship between single-family rental homes and home sales prices. With so much mixed evidence, and so much at stake, further investigation is required to make sound decisions about the impact of non-owner occupied homes in a subdivision.

While this issue receives a lot of public attention and policy interest, the majority of research in this area has focused on multi-family rental properties, government subsidized housing and concentration of poverty in large cities. Little has been done to look at how the presence of single family rental homes in a neighborhood for a community like Fort Collins, Colorado will impact property values. Literature in this area typically uses the percent of rental properties in the census tract as a measure of rental density. In doing so, the authors are implicitly assuming that the impact of rental composition will be identical across the tract. This paper expands upon the current literature by using a unique spatial approach where the location and concentration of rental properties is identified and explicitly considered, allowing the impact to be unique for each home. It is also different in that it looks at single family rental properties rather than focusing specifically on multi-unit complexes or subsidized housing developments. Using a hedonic pricing model, the analysis will estimate the way rental properties affect home values by looking at how property values change depending upon their proximity to rental homes.

4.2 Analytical Framework

Rosen (1974) provided the framework for the hedonic price regressions that is typically used to measure housing market impacts and externalities. These models use the characteristics of a property as independent variables to explain or predict the sales price of a house. The basic intuition in hedonic valuation is that the price of a good (in this case, a house) is a function of a bundle of attributes unique to that specific good (property). However, the prices of the individual characteristics are not directly observed; they are implicit in the sales price of the house (which is observable).

Thus the hedonic price of a house is defined as the "set of implicit prices" related to the home's specific characteristics and its observed price (Rosen, 1974). In equation form, the hedonic price of house P_i is a function of its characteristics, represented by the vector X_i.

$$P_i = f(X_i) \tag{4.1}$$

For simplicity, assume a linear function of the form:

$$P_i = \beta_0 + \beta_1 X_{1i} + \beta_2 X_{2i} + ... + \beta_k X_{ki} \tag{4.2}$$

Taking the partial derivative of price with respect to each characteristic yields the characteristic's marginal cost:

$$\delta P/\delta x_1 = \beta_1 \tag{4.3}$$

In this case, the marginal price of an additional unit of characteristic X_1 is equal to β_1. For example, if X_1 represented the number of bedrooms, this equation is telling us that for each additional bedroom, the price of the house increases by β_1, holding other things constant.

In addition to structural characteristics, (square footage, number of bedrooms, lot size, etc.), the location of a house is considered to play an important role in determining its value, so the characteristics of the neighborhood (location) must also be considered. Depending on the analysis, other factors expected to affect the price of a home are also added to the model (for example: environmental characteristics, proximity to amenities, etc.).

For this analysis, the hedonic model, $P_{ij} = f(X_{ij}, N_{ij}, R_i)$, measures the value of a home as a function of its structural characteristics, its neighborhood characteristics, and the rental density characteristics of its location. In semi-log functional form, the regression model can be written as follows:

$$ln(Price_{ij}) = \beta_0 + \beta_1 X_{ij} + B_2 N_j + \beta_3 R_i + \varepsilon_i \tag{4.4}$$

Where $Price_{ij}$ = price of house i in neighborhood j, X_{ij}=a vector of structural characteristics of house i in neighborhood j, N_j=a vector of characteristics for neighborhood j, and R_i=rental density characteristics of the location of house i. The variables that make up each of these characteristics, their definitions, and their expected relationship with the dependent variable are shown in Table 4.1.

Table 4.1 Variable descriptions

Variable	Description	Expected Sign
ln (Sales Price)	Natural log of home sale price, in real dollars	n/a
Age	Age of house at time of sale in years*	-
Stories	Number of stories (does not include basement)*	
Square Footage	square feet of total above grade living space*	+
Basement SF	Number of square feet in basement (finished or unfinished)*	+
Rooms	Number of rooms above grade (does not include bathrooms)*	
Bedrooms	Number of bedrooms*	+
Bathrooms	Number of bathrooms*	+
Fireplace	Number of fireplaces*	+
Lot	Lot size in acres*	+
Central Air	Dummy=1 if central air conditioning*	+
Loveland Schools	Dummy=1 if located inside Loveland School District**	+/-
In City Limits	Dummy=1 if located inside Fort Collins city limits**	+/-
Owner Occupied	Dummy=1 if owner occupied*	+/-
Attached Garage	Home has an attached garage*	+
carport	Home has a carport*	+
Detached Garage	Home has a detached garage*	+
Multiple Garages	Home has multiple garages and/or types*	+
Garage SF	Square feet of garage space*	+
Bank Owned	Property was bank owned/foreclosure***	-
Short Sale	Property was a short sale***	-
Q1,Q2,Q3,Q4	Quarter home was sold*	
Sold 2012	Home was sold in 2012*	+
Distance to CSU	Distance to CSU's Lory Student Center****	-
Distance to Shopping	Distance to popular shopping area****	-
Rentals 500	Number of rentals within 500 ft ****	+/-
Rentals 14	Number of rentals within 500 ft and 1/4 mile radius ****	+/-
Rentals 12	Number of rentals within 1/4 and 1/2 mile radius ****	+/-
Population	Total population in census tract****	+/-
% College	% Census tract pop. with college degree or higher****	+
Median Age	Median age in census tract in years****	+/-
Families	Husband and wife with own child****	+
Dad	Male with own child less than 18, no wife****	-
Mom	Female with own child less than 18, no husband****	-
% White	% of tract population that is white****	+
Income	Average annual income in census tract in real$****	+
Vacant	Vacant housing units in census tract****	-
% Rentals	% of rental homes in census tract****	+/-
% Students	% of tract population enrolled in 4-yr public college****	-

Sources: * Larimer County Assessor's Office; ** Author's calculations using GIS; *** RealList search option through Fort Collins multi-list service; **** All neighborhood characteristics come from the US Census Bureau.

The semi-logarithmic functional form used here was selected because it is the most commonly used functional form in hedonic studies of housing markets due to the inseparable nature of the characteristics of housing. As Coulson noted, "physical housing characteristics are for the most part, tied together in an inseparable bundle.

One cannot detach a bathroom from a house and sell it on ebay. Because of this, linearity should not be assumed in a housing hedonic function ... ." (Coulson, 2010).

4.3 Hypothesis and variable selection

The purpose of this study is to determine how proximity to rental homes affects the value of nearby homes by estimating the magnitude and direction of the rentals' impacts on nearby homes. Thus, the hypothesis being tested is that the proximity to rental homes does not affect the price of homes in the neighborhood, all else equal.

This study focuses specifically on properties with a postal delivery address of Fort Collins, Colorado. The sample was chosen so that general city characteristics such as, public service provisions, tax rates, etc. are the same for all properties in the sample, and can therefore be excluded from the model. In addition to the variables in Table 4.1, squared variables such as Age^2 and Lot^2 are included, a common practice in hedonic studies, to account for the notion of diminishing returns. These variables capture the fact that as size of the home, size of the lot or age of the home increase, the price of the house will increase, but at a decreasing rate. Numerous indicator or dummy variables are also included to indicate whether prices change as a result of the presence of different attributes. For example, the dummy variable $CentralAir$ allows us to test whether the presence of air conditioning in a home has an impact on the home's price.

The structural characteristics included in this model that are standard in most housing price regression models include: square footage, age, number of bedrooms and bathrooms, lot size, garage and/or carport spaces, presence of a fireplace, central air conditioning, number of stories, and presence of a basement. Many of these models are limited in the number of structural characteristics they can include because they lack sufficient data. My sample includes data on additional characteristics that could affect the price of a home including: basement square footage, number of total rooms above grade, age of the home at the time of sale, garage type, school district, location inside city limits, and type of occupancy at the time of sale (non-owner vs. owner occupied). Based on the literature, I have also included an indicator variable to indicate whether a sold property was a fair market sale, foreclosure or short sale (which theoretically would result in a lower sales price).

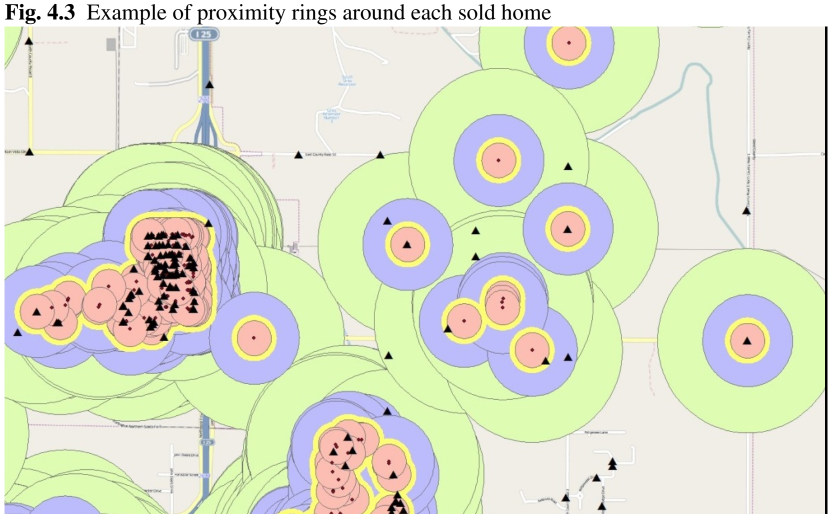
It is largely considered common knowledge among real estate professionals that the time of year a home sells has an impact on its price. The seasonality of the real estate market is supported in the literature as well (Goodman, 1993). For this reason, dummy variables were included for both the year and quarter during which the home sold.

Neighborhood characteristics that theoretically could affect a neighborhood's home values include both demographic and location variables. As already discussed, the presence of rentals in a neighborhood is a main interest of this study. In addition to the rental proximity variables discussed below, the subdivision's overall occupancy composition may also have an impact on home values. Therefore, a variable for the percent of homes occupied by renters has been included. Previous studies have found that lower home prices were associated with higher percentages of minorities, presence of single parent households and higher numbers of vacant homes (Chiodo et al., 2010). For this reason, variables for the percent of population that is white only in a neighborhood, single parent households (with both male and female heads of household) and number of vacant homes were included. Other included variables that are traditionally associated with higher property values include education levels, age of residents, and income.

A study on Fort Collins, conducted by Corona Research (2005) found that 71% of renter households with more than three unrelated persons living in a single dwelling unit were college students. While the study specifically analyzed the impact of single-family rentals on neighborhood property values, it used the percent of single-family homes in a census block group, rather than the total number of rental homes within a certain distance of the subject property, which is the focus of this paper. The authors found four characteristics with a significant impact on neighborhood home values: percent of rentals, average number of rooms in each house in the subdivision, distance from the CSU campus to the approximate center of the neighborhood, and the average age of the homes (Corona Research, 2005). Based on this information, variables for the number of college students in the census tract and distance to CSU (measured from the sold home to Lory Student Center) were included.

Typical to these types of analysis, the distance to Central Business District is included; however, the Central Business District in Fort Collins, known as "Old Town" is very close to the Colorado State University Campus (already being measured with the Distance to CSU variable). Furthermore, while Old Town is a popular area of town, it provides more of a "destination" type shopping and nightlife experience. The majority of everyday retail shopping occurs along the Harmony Road corridor which is also home to a large number of technology-oriented employers. With location to Old Town already being largely accounted for in the Distance to CSU variable, a variable for the home's distance to a major shopping center was added to the model.[4]

Fig. 4.3 Example of proximity rings around each sold home



In order to test the hypothesis that proximity to rental homes will affect a home's value rental density characteristics were constructed. These variables were created using a list of non-owner occupied homes obtained from the Larimer County Assessor's office. While it would be ideal to have information on whether a home

[4] The shopping center selected for this analysis was the Front Range Village, a lifestyle-shopping center anchored by several big box stores (including Lowe's, Super Target, and Sport Authority).

is specifically being used as a rental property, this information was not available. Therefore, the assumption was made that a home listed as non-owner occupied in the county tax records is being used as a rental property.

The homes identified as non-owner occupied were plotted, along with single-family homes sold between January 1, 2011 and July 1, 2012, using ArcGIS software. The number of rental homes within 1/2 mile of each sold home were identified, then grouped and totalled according to distance from the subject property. Rentals that were located within 500 feet of the subject make up $Rentals500$ category. Rentals that were located within a radius of 500 feet to 1/4 mile, were included in $Rentals14$. Similarly, rentals located between a 1/4 and 1/2 mile radius from the subject make up $Rentals12$. A visual representation of the construction is provided in Figure 4.3.

4.4 Data sources and summary statistics

The data for this paper came from multiple sources. Property characteristics were obtained from the Larimer County Assessor's office and the local multi-list service (MLS). Both sources provided detailed structural information on each property, as well as transaction data for single-family homes sold between January 1, 2011 and July 1, 2012. In order to ensure a consistent, quality data set, I compared the assessor's property information with the multi-list information for every property. If an inconsistency was found, I examined that specific property in both systems to check for typographical and/or data errors. In most cases, the inconsistency was due to an error in the input of data in one of the two systems and was easily corrected in the final data set. If there were inconsistencies that could not be resolved, or significant amounts of data were missing for the property, it was eliminated from the data set.

Sales that were not arms-length transactions were identified by examining both the sale price and the type of deed used in the transaction. [5] Property transfers that involved deed types that are not used for arms-length real estate sales were removed from the data set.[6] Sales for unusually low dollar amounts were also examined as they typically involved the "sale" of a home to a relative (for example, a home was sold to a family member for $100).

Data for neighborhood characteristics and demographic information was obtained from the U.S. Census Bureau and matched to the individual properties based on Census blocks and tracts (the geographic unit used to represent a "neighborhood", a common practice in the literature). GIS shapefiles for Colorado school district boundaries and Fort Collins' city limits were used to identify whether properties were located within the city limits as well as within the boundaries of Fort Collins' school district (Poudre R-1) or the city of Loveland's school district (Thompson R2-J). Table 4.2 present summary statistics for non-categorical/binary variables. Table 4.3 contains summary statistics for categorical/binary variables.

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[5] An arms-length transaction is defined as, "a transaction in which the parties are dealing from equal bargaining positions" (Reilly, 2000).

[6] For example, it is common to use Quit Claim Deeds to transfer or "gift" property to another individual (for example to a spouse or child) as they do not contain any language of warranty. There are a variety of other deeds that generally indicate a transaction was not an arms-length sale, examples include: Beneficiary Deeds, Trustee Deeds, Bargain and Sale Deeds, and Administrative Deeds

Table 4.2 Summary statistics for non-categorical/binary variables

Variable	Mean	Std. Dev.	Min.	Max.
Sales Price	272553	117376	25500	825000
Age	30.32	24.22	1	126
Square Footage	1783.56	648.85	464	7559
Basement SF	812.96	620.53	0	7279
Rooms	6.90	1.85	2	15
Bedrooms	3.33	0.87	1	7
Bathrooms	2.40	0.89	0.75	8
Fireplaces	0.67	0.49	0	2
Garage SF	498.90	268.85	0	3316
Lot Size	0.74	4.32	0	98.73
Distance to CSU	3.53	1.95	0.22	11.36
Distance to Shopping	4.04	2.07	0.33	15.26
Population	4526.46	2095.72	1214	11580
Median Age	34.29	6.22	19.50	53.10
% White	90	3	69	96
% College	9	0	37	
Income	77419	23528	19699	132402
% Students	17	13	3	90
Families	405.59	2.81	30	1218
Dad	36.34	17.48	2	102
Mom	85.74	41.133	14	160
Vacant	81.95	44.03	24	313
% Rentals	32	17	6	90
Rentals 500	139.55	134	0	802
Rentals 14	58.89	52	0	548
Rentals 12	8.51	8	0	59

The price of homes sold during this time period ranged from \$25,500 to \$825,000, with a mean sales price of \$272,553. Average square footage (above grade) was 1,783 and average basement square footage was 812. Most homes had 3 bedrooms and 2 bathrooms and garage square footage of 498 (which roughly translates into a 2 car garage). The majority of sold properties were located within city limits and within the boundaries of the Poudre R-1 School District. Five percent of the homes were foreclosures, 2.0% were short sales, and 75% were owner occupied.

The average census tract had a population of 4,526 people, 90% of which were white, and consisted of 405 families, 36 single-dad households and 85 single-mom households. On average, 15% of the census-tract population held a bachelor's degree. Annual income ranged between \$19,699 and \$132,402 with the average being \$77,419. The percent of students in each census tract ranged between 3% and 90%, with a mean of 17%. Sold homes had an average of 8 rentals within 500 feet, 59 rentals between 500 feet and 1/4 mile, and 139 rentals between 1/4 and 1/2 mile. The average percent of rentals overall in each census tract was 32%.

4.5 Empirical Results

Initially a semi-log specification was considered, however based on the relationship between sales price and rental density, the specification that best fit the data and underlying intuition was a model examining the non-linear effects of proximate rental properties on sales price. The model specification is presented below and examines

Table 4.3 Summary statistics for categorical/binary variables

Variable	Frequency	Percent
Owner Occupied	2059	75%
Bank Owned	142	5%
Short Sale	63	2%
Sold in 2012	1028	37%
Q1	655	24%
Q2	1149	42%
Q3	594	22%
Q4	361	13%
Central Air	1325	48%
No Garage	167	6%
Attached Garage	2290	83%
Carport	13	0%
Detached Garage	218	8%
Multiple Garages	71	3%
Outside City Limits	478	17%
In City Limits	2281	83%
Loveland School District	196	7%

the non-linear effects of rental proximity on home sales price by log-transforming the rental proximity variables.

This model considers the possibility that as the number of proximate rentals rises, the marginal impact on a home's price will increase, but at a decreasing rate, and allows for interpretation of the coefficients as elasticities. The rest of the model was kept in the semi-log functional form, with sales price being the only other variable that was log-transformed. The regression was corrected for heteroskedasticity using robust standard errors. Results are presented in Table 4.4.

Overall, the model was highly significant, with a p-value of less than 0.0001. The model's adjusted r-squared was 0.692, indicating that the model explains 69.2% of the variation in sales price after adjusting for degrees of freedom. The majority of the structural characteristics variables were statistically significant with the expected sign. *Square Footage*, *Basement SF*, *Rooms*, *Bathrooms* and *Fireplaces* were positive, representing a percentage increase in home price per unit increase in the characteristic. While *Multiple Garages* was the only variable for specific garage type that was significant, *Garage SF* was significant and positive. This indicates that in the real estate market for Fort Collins, CO the size of the garage is more important to buyers than the type of garage, all other things being equal.

As expected, the size of the home had a positive impact on price. Sales price increased 0.027% for every additional square foot above grade, and by 0.018% for every additional basement square foot. While at first glance this appears to be a very small number, when one puts this into the context of an example the amount appears more appropriate. Because the variables are interpreted as the change in price due to a one square foot increase, we would expect the price difference between two houses that differ only by one square foot to be very small. In other words, a house that has 1,000 square feet above grade and a house that is 1,001 square feet above grade would not be expected to differ substantially in price.

Rooms and *Bathrooms* also positively impacted home sales price by 1.9% and 5.6%, respectively, for each additional room or bathroom. Additionally, the presence of *Central Air* increased the home's price by 3.8%, all else equal. Significant variables that were predicted to have a negative impact on sales price were *Age* and

Table 4.4 Regression results with dependent variable being ln(sales price)

Variable	Coefficient	Variable	Coefficient
Age	-0.003***	Owner Occupied	0.035***
Stories	-0.048**	Bank Owned	-0.144***
Square Footage	0.0003***	Short Sale	-0.111***
Basement SF	0.0001***	Q2	0.038***
Rooms	0.018***	Q3	0.047***
Bedrooms	-0.027***	Q4	0.028*
Bathrooms	0.055***	Sold 2012	0.003
Fireplaces	0.045***	Distance to CSU	-0.065***
Lot Size	-0.007	Distance to Shopping	-0.050***
Central Air	0.036***	ln (Rentals 12)	0.056***
Attached Garage	-0.056	ln (Rentals 14)	-0.053***
Carport	0.073	ln (Rentals 500)	-0.044***
Detached Garage	-0.018	Population	-6.14E-05***
Multiple Garages	-0.178**	Median Age	0.001
Garage SF	0.000***	% White	0.698***
In City Limits	-0.025	% College	-0.008
Loveland Schools	0.090***	Families	0.000***
Dad	0.002**	Square Footage^2	-1.93E-08
Mom	0.000	Basement SF^2	-2.04E-08
Vacant	0.000***	Lot2	6.87E-05
Rentals	0.038	Distance to CSU^2	0.003
Income	8.20E-07	Distance to Shopping2	0.005***
% Students	0.331***	Constant	11.170***
Age^2	4.48E-05***		
Observations		2266	
Adjusted R^2		0.692	

Notes: Table shows the estimated coefficients from one OLS regression of listed covariates on ln (Sales Price). Standard errors are not shown for space considerations. Heteroskedasticity-adjusted robust standard errors used. *** indicates statistical significance at the 1% level and ** at the 5% level, respectively.

if the sale was a foreclosusre (*Bank Owned*) or a *Short Sale*. As expected, homes that were bank owned sold for 15% less than those that were a fair market sale, and homes that were a short sale sold for 11% less than fair market sales, all else equal. There was also evidence of seasonality in the market, with homes sold during the second and third quarters of the year selling for approximately 4% more than homes sold during the first quarter of the year.

Two of the house characteristic variables that had unexpected outcomes were number of *Stories* and *Bedrooms*, however further consideration provides some insight into the results. If one considers the fact that adding a bedroom takes away from the size of other rooms in the house, it seems appropriate that increasing the number of bedrooms would negatively impact the sales price overall (Boxall et al., 2005; Coulson, 2010). *Lot Size* and *In City Limits* were not statistically significant. Distance to CSU, as well as distance to the shopping district, were both significant. In both cases, home price decreased for each additional mile from CSU or the shopping district, falling by 6.5% as distance from CSU increased and by 5% as distance from the shopping district increased.

Neighborhood characteristics also played a role in determining the sales price of a home. As predicted in the literature, *% White*, *College*, *Families* and *Income* all positively impacted a home's sales price. The rental proximity variables were all significant at the 0.0001 level. A 1% increase in the number of rentals located within

500 feet decreased the home's price by 0.0439%. Price decreased by 0.0532% for each additional percent of rentals within a radius of 500 feet to 1/4 mile, while a 1% increase in rentals located between 1/4 mile and 1/2 mile of the home increased price by .0559%, all else equal. Putting this in the context of an example provides a clearer picture of what these results are saying. If a home has 100 rental properties within a 1/4 to 1/2 mile radius, and 10 more homes become rentals (equivalent to a 10% increase), the sales price of the property is predicted to increase by 5.59%, holding other things constant. For a home that would have sold for $250,000, this translates into an increase in sales price of nearly $14,000.

Overall, these results indicate that while rental homes that are closely located to a property detract from its selling price, as distance from the home increases, the presence of rental homes increase its price. This relationship can be explained by considering how many home buyers shop for properties. If there is a rental directly across the street that perhaps is not well maintained, it is considered an eyesore, but a rental a block away, which the buyer doesn't see from the driveway, may not matter as much to the buyer. At the same time, if rental properties are considered an alternative to a short sale or foreclosure, an increase in the number of rentals would mean a decrease in the number of distressed sales, effectively increasing prices for surrounding homes overall if the negative impacts of foreclosure are greater than the negative impacts of rentals. An interesting extension of this study would be to analyze proximity of foreclosures to the same sold homes to test for this type of scenario.

4.6 Policy Implications

In general, these findings can be used to assist policy makers, homeowner's associations and mortgage lenders in making sound decisions on rental regulation. The policy implications of these findings may be of particular interest to Fort Collins' policy makers given the three-unrelated ordinance. Passed in 2007, the three-unrelated ordinance (often referred to as U+2), limits the number of unrelated occupants in residential properties to no more than three. The ordinance was intended to reduce the number of people living in a single property, with the goal of reducing neighborhood problems and complaints, and preserving property values. A 2005 impact study conducted by Corona Research (2005) for the city predicted that "2/3 of households that would be considered in violation of the ordinance were living in single-family homes."

While the ordinance may have limited the number of people living in any one rental property, (theoretically reducing complaints and problems), it likely lead to an increase in the number of rental homes overall, as households that were made up of more than three unrelated people were forced to split up. This appears to be the case based on interviews conducted in a 2009 follow up study during which several people commented that they had "seen a marked increase in the number of rental properties where they live"(Corona Research, 2009). Another consequence of the ordinance is that it likely contributed to the decrease in vacancy rates. Basic supply and demand theory tells us that as the number of available properties falls, rents will increase. This means that in Fort Collins, the U+2 ordinance very likely exacerbated affordable housing problems.

Furthermore, if the ordinance resulted in additional rental households forming (which appears to be the case), the results of this analysis suggest that the U+2 ordinance may not have had the intended impact of preserving property values. Given these results, further research examining the full impact of the U+2 ordinance on both property values and housing affordability would be beneficial to local policy makers.

4.7 Conclusion

Single-family rental homes play a crucial role in providing quality, affordable housing to households that are unwilling or unable to purchase their own home. Literature in this area often overlooks the majority of the rental housing market as it typically focuses on low-income affordable housing or multi-unit apartment complexes. Sound housing policy requires a balance between the need for quality, affordable housing with the needs of homeowners seeking to preserve the quality of their neighborhood and protect the value of their homes.

This analysis extends the current literature by specifically measuring the impact of proximity to single-family rental homes on home sales price. In developing this model, GIS software was used to measure precise distances between rental properties and sold homes. This method resulted in rental density variables that were unique to each individual home, rather than taking the "blanket approach" of using census tract data typically found in the literature. By measuring both the number of rentals and their distance from each sold single family home, I was able to explicitly identify both the location and concentration of rental homes for each sold property, rather than implicitly assuming a uniform impact as seen in most literature. Overall, these findings suggest that rental properties do have an impact on the selling price of nearby homes.

I find strong evidence that rental density plays an important role in determining a home's overall selling price; with rentals located within 1/4 mile of a property negatively affecting price and rentals located between 1/4 and 1/2 mile positively affecting selling price. If rental homes are considered an alternative to a foreclosure or short sale for sellers that are underwater on their homes, these results suggest that while an increase in the number of rental properties may negatively affect the price of homes within close proximity, by lowering the number of properties sold in short sale or foreclosure, they have a positive effect on the price of homes in the area overall. These findings suggest areas for additional research, including an analysis of the proximity of distressed sales on sold home prices. To the extent policymakers seek to preserve property values through rental restrictions, the results of this paper suggest that these restrictions may not necessarily be having the intended effect.

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Chapter 5
Airport Noise and House Prices: Evidence from Columbus, Ohio

Kerianne Lawson

Abstract The John Glenn International Airport in Columbus, Ohio opened a new runway in 2013. This expansion made it possible for Columbus to nearly double its air traffic. The added noise from this expansion was a concern for the city, and specifically residents in the surrounding neighborhoods. I use a difference in differences approach to examine the effect of the added noise pollution. I find that after the runway expansion, house prices in noisier areas decreased compared to those in areas not as affected by aircraft noise. Depending on the area specification, decrease in home sale price ranges from approximately 2-10%.

5.1 Introduction

Living near an airport, or at least under the flight paths, can be very loud. Aircraft noise is a negative externality caused by airports that, in theory, should affect house prices. Moreover, an expansion of an airport that increases noise should be a concern as it could impact home values in noise-affected areas.

The John Glenn International Airport is located about seven miles east of downtown Columbus, Ohio. On August 22, 2013 the airport opened a new runway that was 702 feet further south than the previous runway. This move allowed for enough space between the north and south runways so that for the first time, planes could take off and/or land on both runways at once. Representatives from the airport stated in numerous news articles that the goal of the airport was to meet increasing demand and allow for more planes to get in and out of Columbus faster than ever before (Weese, 2013). The increase in air travel demand could be attributed to Columbus' population growth in recent years. It was reported in 2018 that Columbus is one of the fastest growing cities in the country, and appears to be booming compared to other northern and midwestern cities (Millsap, 2018). As Columbus Mayor Michael Coleman put it, "as the city grows, the airport needs to grow with it" (Weese, 2013).

As the demand for air travel continues to increase, many cities must decide if it is beneficial to expand their airports to accommodate more traffic. For example, the Heathrow Airport in London discussed adding a third runway in 2013 as well. The new runway would bring an additional 250,000 planes to Heathrow a year.

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Opponents to the expansion argued that the noise levels would be simply unbearable with the new runway. The airport is already a source of strife in south-east London. The concern that the affected areas would be under a "louder sky of sound" led to a debate about moving the entire airport to the outskirts of the city where the additional noise would affect fewer people (The Guardian, 2013). There are many stories like this, and as cities struggle with the decision to expand or move airports to deal with the increasing number of flights per year, economists should be concerned with the consequences of these decisions. In particular, the potentially increasing noise pollution may affect the housing market.

This paper examines the effects of an airport expansion on house sale prices in Columbus, Ohio. After the new south runway opened at John Glenn International Airport in Columbus, home values in areas under the flight paths decreased. Although, it is not clear that the decrease in home sales prices is due to the actual increase in noise from the airport, or just the expectation of new noise. Mense and Kholodilin (2014) looked at the anticipation of noise based on the release of flight path plans released two years before the scheduled opening of the Berlin-Brandenberg airport and the house prices in that area. They found a decrease in home values from the expected increase in noise.[1]

The majority of the literature uses a hedonic price model to study the relationship between airport noise and house prices. Several papers have noted that it is more important to consider flight paths rather than distance from the airport (Boes and Nüesch, 2011; Mense and Kholodilin, 2014; Tomkins et al., 1998). This is why I use a difference in differences approach to compare the homes under the flight paths to homes not under the flight paths before and after the new runway opened. I use two different methods to identify which homes are affected by airport noise. The first is using homes located in certain ZIP codes and the second using latitude and longitude coordinates.

5.2 Literature Review

City and regional airports have been regarded as amenities that benefit the surrounding area. For example, in terms of economic growth (Bilotkach, 2015; Blonigen and Cristea, 2015; Brueckner, 2003; Button and Yuan, 2013; Green, 2007; Tittle et al., 2013), and increased employment (Appold and Kasarda, 2013; Button and Lall, 1999; Sheard, 2014). However, there has been mixed evidence the nature of spillovers from nearby airports. Despite evidence that airports are associated with increased employment and economic activity, others are concerned about possible negative externalities generated from airports. In this paper, I will focus on noise pollution. Excessive noise from air traffic must be considered when we evaluate the costs and benefits of an airport's location and activities. One of the ways one can measure the social costs of airport noise is looking at prices of homes around the airport.

Studying the effects of airport noise, and other negative externalities associated with airports, on house prices is not new to the economics literature (Nelson, 1980; Suksmith and Nitivattananon, 2015). Most of the literature uses hedonic price models and studies decibel levels across cities to evaluate the effects of noise on house

[1] The airport has still not opened despite the plans being released in 2011, further showing that expectations of noise matter.

prices (Cohen and Coughlin, 2003; Dekkers and van der Straaten, 2009; Espey and Lopez, 2000; Nelson, 1979). Nearly all have found a negative relationship between home prices and noise levels. The estimates on these effects vary, because some are looking at airport openings and others at expansions.

Cohen and Coughlin (2008, 2009) look at houses around Hartsfield-Jackson Atalanta International Airport. They found that while proximity to the airport is positively related to house prices, after controlling for proximity and house characteristics, the homes in noisy areas sold for less than homes in less noisy areas. In Cohen and Coughlin (2008), they consider spatial effects and use a model that includes spatial autocorrelation and autoregressive parameters and find a strong negative relationship between noise and house prices. Cohen and Coughlin (2009) build on O'Byrne et al. (1985) that also looked at Atlanta's airport and house prices, but in the 1970s. They find similar results, but look at how the impact of noise has changed over time at the specific airport.

Nelson (1980) conducts a survey of the literature using thirteen empirical studies and finds that the noise discount in urban areas is commonly about 0.5% to 0.6% per decibel. The increase in sound from being near an airport is approximately 20 decibels, resulting in a 10-12% decrease in home values in noisy areas. These studies do not necessarily look at the opening or expansion of the airport, but just compare seemingly identical homes that are close to and far away from the airport, or lie in different noise contour zones. Despite the large number of papers that have found a negative relationship between noise and house price, it still up for debate about whether airports are an urban amenity or disamenity.

One study suggested that as aircraft technology improves, planes make far less noise than they ever have before and additional air traffic does not significantly affect noise levels or home sale prices (McMillen, 2004). McMillen (2004) looks at the expansion of the Chicago O'Hare airport and finds that homes in the areas with severe noise experience about a 10% decrease in their home values. However, the size of the area of noise concern is shrinking as planes get quieter. He estimates that in order for airport expansions to have an impact on home values, the expected increase in air traffic must exceed 60%. One could argue that John Glen International Airport's new runway meets this requirement, as the new south runway has the potential to double Columbus' air traffic and the increase in flights possible was often discussed in the local news.

This is connected to some of the more recent literature that argues that it is more about the anticipation of noise than the noise itself. Jud and Winkler (2006) study the effects on house prices after the announcement of the airport expansion in North Carolina, the Piedmont Triad International Airport, a hub for cargo and passenger air travel. They find that within a 2.5 mile band of the airport, house prices declined about 9.2%. They effect is still significantly negative, 5.7%, when the band is extended another mile and a half. This suggests that the anticipation of air noise could affect house prices, before the increased noise levels caused by the airport expansion even occur. Conversely, if residents near the airport do not perceive the expansion to be a major one, then this will not affect home prices as much. McMillen (2004) argues that the shrinking affected area because of quieter planes, and lack of reaction to relatively small expansions should be a good sign to airports who wish to grow in the coming years.

Others have argued that airports are an amenity to the nearby neighborhoods. Tomkins et al. (1998) found in a study conducted about the Manchester airport that home owners may actually experience house price premiums if they locate close to airports. They found a 4 to 12% increase in home price depending on the level of noise observed.

Some papers have suggested that using identifying homes under flight paths is a better method than distance from the airport (Boes and Nüesch, 2011; Mense and Kholodilin, 2014; Tomkins et al., 1998). The reasoning behind this is because two homes that are equivalent in distance from the airport may experience vastly different noise levels depending on the flight patterns above them. Mense and Kholodilin (2014) uses the release of the new flight patterns from the Berlin-Brandenburg airport. They find that home values decrease between a 8.3 to 12.8% because of the release of the plans for the new flight paths, not the actual increased noise itself, as the airport is yet to open. They propose that perhaps it is the anticipation of added noise that affects home values more than the actual noise after the airport's opening. Because Columbus' airport did not double its traffic overnight with the opening of the new runway but expanded for the increasing demand, then expectations of the added noise may be a driving factor of the results of this paper.

In a similar study, Almer et al. (2017) used a change in regulation in Germany that affected the Zurich airport's flight patterns for a quasi-experimental analysis of the aircraft noise on housing prices. They use online advertisements for apartments and use a difference-in-differences approach with time varying treatment effects and find the rental market experienced about a 13% decrease in the average apartment's rent for about two years after the policy change and then reached a new lower equilibrium for the rest of the observed period. Almer et al. (2017); Boes and Nüesch (2011) both use a difference-in-differences approach to estimate the effect of the increase or change in airport noise on the areas under flight paths, which should be the most affected by sound, after the policy went into effect or expansion was announced. In this paper, I will use a similar approach and use the opening date of the new runway as the beginning of the treatment period.

Unlike previous work, this paper is the first to use the difference-in-differences method to estimate the effect of opening of a new runway at an airport on house prices. While not very many cities are considering opening up new airports, new runways are an option for lots of airports that are trying to accommodate increasing demand for air travel. Airport expansions that include adding runways are happening in smaller cities like Columbus, but also major cities all over the world (Cowin, 2018). The homes potentially affected by these new runways are already near the airport, so this effect should be interpreted differently than the results in Boes and Nüesch (2011) which is considering placing an airport in an entirely new area. The effect of an opening at a new site, or a major change in flight paths like in the case of Almer et al. (2017), should be viewed differently than increasing the intensity and frequency of sound in an already noisy area. The results in this paper could be more useful in cases where airports are considering an expansion via new runway(s).

5.3 Data

The data on house sale price and characteristics comes from the Franklin County Auditor. The home sale information covers the years 2009-2017 and excludes condominiums, mobile homes and duplex dwellings. Included in the data are several variables about the location, condition, and house characteristics (Mingo, 2018). Homes that are affected by sound are identified using two methods: ZIP codes, and areas created using latitude and longitude coordinates.

The John Glenn International Airport only has runways that run east-west. So I look at homes to the east and west that are in the flight path zone, and compare them to homes to the north and south of the airport, as well as the rest of Franklin County. Just measuring distance from the airport would not be an accurate way to determine which homes are impacted by the sound.

On the Columbus airport's website, it says that they have started programs to monitor and track noise levels (Columbus Regional Airport Authority, 2018). They have devices to measure sound in the areas they believe will be most impacted by the ever increasing volume of air traffic in Columbus. They provide an interactive map that has circles where the monitors are located, and the current sound levels at any given time (Fly Columbus, 2018). As the planes move you can see the increasing sound levels denoted by the change in color and size of the circles, as shown in Figure 5.2.

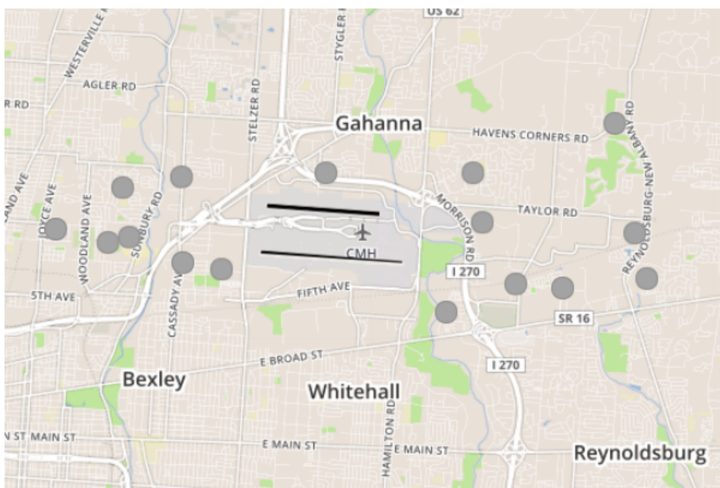

Fig. 5.1 Circles indicate sound tracking monitors around the airport

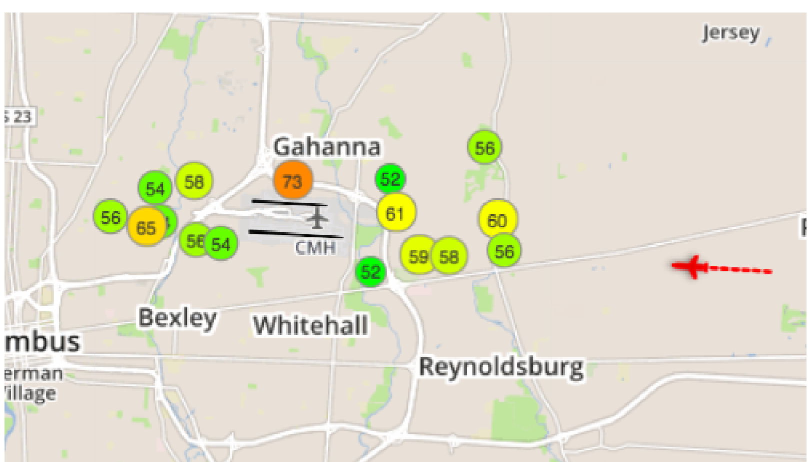

Fig. 5.2 Example of a plane approaching the airport

5.4 Empirical Methodology

This paper's difference in differences approach rests on the plausibly exogenous variation in sound due to the flight paths over the city. I identified the noisiest areas using maps from the sound tracking website provided by the Columbus airport. Then, I used the opening of the airport runway as the date to determine the post-treatment period and the ZIP code and latitude/longitude areas to determine the treated homes.

The model is as follows:

$$log(SalePrice) = \alpha + \gamma(Treated_i^{area} * Post_i) + \beta X_i + \varepsilon_i \tag{5.1}$$

where X_i is a vector of home characteristics including the year the home was built, the number of bedrooms, full baths and stories, the condition[2], the style of the home[3], and the school district in which the home is located. γ is the coefficient that can be interpreted as the percent change in home sale prices for homes in a given treatment area.

I used several different specifications to determine which homes are included in the "treated" group. That is, the homes that are under the path of the planes coming and going from the airport. The first three specifications are based on ZIP code location and the second three are based on latitude and longitude location.

The airport is located in the south central part of the 43219 ZIP code. First, I only included homes in 43219, as they would be closest to the airport. Next, I added the ZIP codes directly east and west of where the airport was located, 43211 and 43230. Next, I added two more ZIP codes that were west of 43219, but not directly west of the airport. However, as you can see in Figure 5.3, many homes that are north of the airport, and presumably not as affected by airport noise, are included in this specification.

To ensure I was truly capturing the homes most affected by airport sound, I used the latitude and longitude coordinates of each home sold. Area 1 is drawn to include all of the sound monitoring devices that the Columbus airport has placed in its immediate vicinity to measure sound. Area 1 extends about 4 miles on either side of the airport. It seems reasonable to assume that this is the area that the airport is most concerned about and is therefore tracking the noise. The next two areas are drawn to capture homes slightly further out of this area but who are almost certainly affected by the sound. Area 2 adds approximately 2 miles on either side of Area 1, and Area 3 is an additional 3 miles out. These areas are based on anecdotal evidence from the city's residents as well as the previous literature. The true shape of the area affected is more like two cones fanning out to the east and west of the airport. However, for this paper, I will just use the same height for all three areas, because I do not know how far the cone shape fans out to the north and south. It is possible that there are more homes being affected by sound that are not included in the three areas.

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[2] The condition of the home is determined by the county auditor's office. Homes can be coded as one of the following: Excellent, Very Good, Good, Fair, Average, Poor or Very Poor

[3] Some examples of the style of the home are bungalow, Cape Cod, and colonial

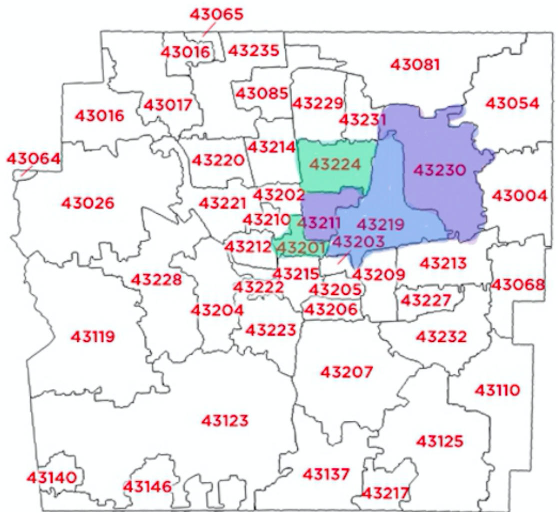

Fig. 5.3 The three ZIP code specifications: 1 ZIP is blue, 3 ZIP is blue and purple, 5 ZIP is blue, purple, and green.

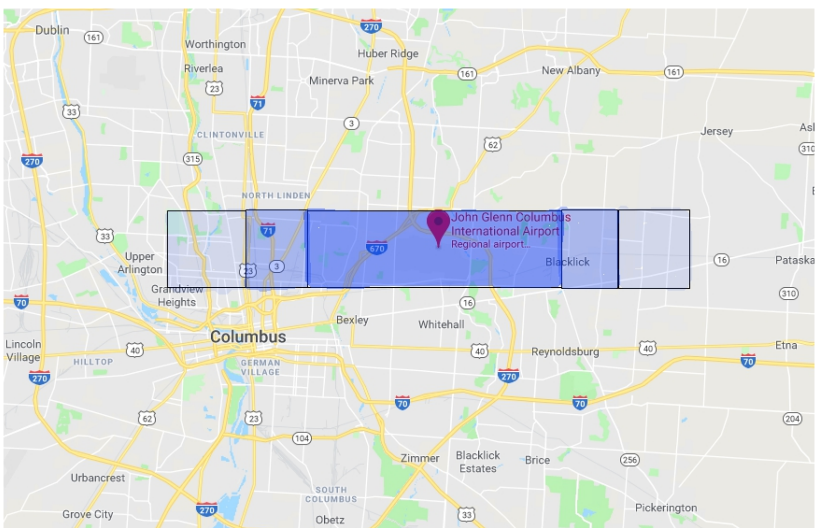

Fig. 5.4 The three area specifications, the shade of blue indicating each area. Area 1 is the darkest and Area 3 is the lightest.

5.5 Empirical results

First, a concern is that the proximity to the airport in the treated group homes is correlated with some unobservable factors that will affect house prices. One way to see if this is a valid concern is to compare home characteristics between the treated and untreated groups. Table 5.5 has the summary statistics for the three ZIP code area specifications compared to the rest of Franklin County.

There are slight differences in the characteristics and quality of the homes between the ZIP code areas and the rest of the sample. However, it does not look like the differences are not large enough to explain the difference between aver-

Table 5.1 Home characteristics comparison for the ZIP code areas

	ZIP 1 vs rest of sample		ZIP 3 vs rest of sample		ZIP 5 vs rest of sample	
Year Built	1983	1974	1977	1974	1966	1975
Stories	1.586	1.565	1.526	1.568	1.484	1.578
Sale Price	126,662	214,373	159,803	217,317	155,435	220,360
Full Baths	1.640	1.808	1.656	1.818	1.577	1.576
Bedrooms	3.157	3.292	3.220	3.296	3.152	3.308
Acres	0.178	0.255	0.239	0.256	0.209	0.260
Very Good	0.005	0.039	0.006	0.041	0.021	0.040
Good	0.076	0.176	0.153	0.176	0.179	0.174
Fair	0.083	0.030	0.052	0.028	0.059	0.027
Average	0.831	0.7513	0.783	0.750	0.736	0.755
Poor	0.003	0.003	0.004	0.003	0.005	0.003
Very Poor	0.001	0	0.001	0.001	0.001	0.001
Observations	1,873	104,731	8,313	98,291	12,360	94,244

age sale price in the two groups. I believe a lot of this difference can be explained by the school district in which the schools are located. In the rest of the sample, there are 5 school districts that rank in the top 15 in the state of Ohio (Niche.com, 2018). The treated ZIP codes are all located in the Columbus or Gahanna-Jefferson school districts (Ohio Development Services Agency, 2018). Given the poor quality of Columbus city schools and the relatively small number of private schools in the area, many central Ohio residents are willing to pay a premium for homes in certain school districts even though they are not much nicer or bigger. Because of this, I include controls for school district in the model.

Table 5.2 Diff-in-Diff results for the ZIP code areas

	Log Sale Price					
	(1)	(2)	(3)	(4)	(5)	(6)
1ZIP*Post	-0.043**			-0.0002		
	(0.016)			(0.011)		
3ZIP*Post		-0.049***			-0.035***	
		(0.007)			(0.005)	
5ZIP*Post			-0.081***			-0.041***
			(0.007)			(0.005)
Year Built				0.001***	0.001***	0.001***
				(0.000)	(0.000)	(0.000)
Bedrooms				0.008***	0.007***	0.008***
				(0.001)	(0.001)	(0.001)
Full Baths				0.138***	0.136***	0.137***
				(0.001)	(0.001)	(0.001)
Stories				0.107***	0.101***	0.101***
				(0.002)	(0.002)	(0.002)
Acres				0.032***	0.031***	0.032***
				(0.001)	(0.001)	(0.001)
Condition	No	No	No	Yes	Yes	Yes
Style	No	No	No	Yes	Yes	Yes
School District	No	No	No	Yes	Yes	Yes
Observations	106,604	106,604	106,604	93,642	93,642	93,642
R^2	0.015	0.013	0.027	0.498	0.502	0.500

Significance codes: ‘***’ 0.001, ‘**’ 0.01, ‘*’ 0.05, ‘^’ 0.1

Table 5.5 is the difference in differences results for the three ZIP code specifications. Even when controlling for style, condition, school district, and house characteristics, the house prices in the 3 ZIP and 5 ZIP areas are still negatively and significantly affected after the runway opens. However, because of the shape of the ZIP code areas, many homes north of the airport are included in these areas. And as previously discussed, planes can only take off and land over the homes to the east and west of the airport. These ZIP code areas may be considering homes largely unaffected by aircraft noise as a part of the treated group, which would be problematic for this study's analysis.

Table 5.3 Home characteristic comparison for the three areas

	Area 1 vs rest of sample		Area 2 vs rest of sample		Area 3 vs rest of sample	
Year Built	1974	1974	1982	1974	1972	1974
Stories	1.563	1.565	1.650	1.558	1.706	1.547
Sale Price	156,920	213,090	185,943	214,851	201,877	213,999
Full Baths	1.758	1.806	1.809	1.804	1.792	1.806
Bedrooms	3.279	3.290	3.255	3.292	3.257	3.295
Acres	0.236	0.255	0.213	0.257	0.190	0.262
Very Good	0.023	0.038	0.011	0.040	0.035	0.039
Good	0.156	0.175	0.099	0.180	0.154	0.177
Fair	0.056	0.030	0.064	0.028	0.053	0.028
Average	0.760	0.753	0.819	0.748	0.754	0.753
Poor	0.003	0.003	0.005	0.003	0.004	0.003
Very Poor	0.001	0.001	0.001	0.001	0.001	0.001
Observations	3,889	102,651	8,088	98,452	11,803	94,737

Table 5.5 is a comparison of the homes in the three areas to the rest of the sample. Like the three ZIP code areas, these homes sell for less than the rest of the sample despite being very similar in size and age of the home. Again, I control for school district and style of the homes in hopes of controlling for differences in the neighborhoods that can affect house prices. Table 5.5 is the difference in differences results for the three areas. Even as far out from the airport as Area 3, house prices are still affected negatively and significantly after the opening of the airport runway. In column 4, the result is negative, but loses significance once the controls are added to the regression. There are mostly industrial buildings to the east of the airport in Area 1, and those properties are not included in the data. This could contribute to the not significant result. Next, I check to see if my results are only due to the size of the areas I have created. I change three areas to different longitude ranges, but the same latitude ranges. I expand and shrink the three areas by a quarter of a mile (or 0.005 longitudinal degrees). These results are presented in Table 5.5 and are consistent with Table 5.5.

My estimates include all homes in Franklin County outside of the specified treated area as the control group. Some of these homes are nearly 25 miles away from the airport and in newer suburban areas that are very different, in many unobserved ways, from the older neighborhoods downtown that are affected by the airport sound. To attempt to include homes that are most comparable geographically and in neighborhood composition, I restrict the sample of home sales and run the same analysis with the three latitude/longitude areas. The observations included in this analysis include homes located the ZIP codes where the three areas are, and

Table 5.4 Diff-in-Diff Results for the three areas

	Log Sale Price					
	(1)	(2)	(3)	(4)	(5)	(6)
Area1*Post	-0.053*** (0.010)			-0.007 (0.008)		
Area2*Post		-0.054*** (0.008)			-0.022*** (0.006)	
Area3*Post			-0.037*** (0.006)			-0.027*** (0.005)
Year Built				0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Bedrooms				0.008*** (0.001)	0.007*** (0.001)	0.008*** (0.001)
Full Baths				0.139*** (0.001)	0.139*** (0.001)	0.139*** (0.001)
Stories				0.139*** (0.002)	0.103*** (0.004)	0.103*** (0.002)
Acres				0.032*** (0.001)	0.032*** (0.001)	0.032*** (0.001)
Condition	No	No	No	Yes	Yes	Yes
Style	No	No	No	Yes	Yes	Yes
School District	No	No	No	Yes	Yes	Yes
Observations	106,540	106,540	106,540	93,580	93,580	93,580
R^2	0.007	0.010	0.005	0.497	0.498	0.495

Significance codes: '***' 0.001, '**' 0.01, '*' 0.05, '^' 0.1

other ZIP codes close to the treated areas and the airport.[4] Table 5.5 shows the results when using a smaller area as a control group. These are homes that still should be unaffected by the airport noise, but may be a more suitable control group than using the entire county. When controlling for school district and housing characteristics, I find slightly larger negative results.

Table 5.5 Diff-in-Diff Results for the three areas with adjusted sizes

	Log Sale Price					
	(1)	(2)	(3)	(4)	(5)	(6)
Area1*Post	-0.017* (0.008)			-0.012 (0.008)		
Area2*Post		-0.025*** (0.005)			-0.019*** (0.006)	
Area3*Post			-0.026*** (0.005)			-0.028*** (0.005)
Size Change	+0.005	+0.005	+0.005	-0.005	-0.005	-0.005
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	93,580	93,580	93,580	93,580	93,580	93,580
R^2	0.497	0.497	0.495	0.496	0.499	0.495

Significance codes: '***' 0.001, '**' 0.01, '*' 0.05, '^' 0.1

[4] 43201, 43202, 43205, 43206, 43209, 43210, 43211, 43212, 43213, 43214, 43219, 43221, 43224, 43227, and 43230

Table 5.6 Diff-in-Diff Results for the three areas with restricted control group

	Log Sale Price					
	(1)	(2)	(3)	(4)	(5)	(6)
Area1*Post	-0.044** (0.014)			-0.010 (0.009)		
Area2*Post		-0.108*** (0.012)			-0.069*** (0.009)	
Area3*Post			-0.063*** (0.010)			-0.041*** (0.007)
Controls	No	No	No	Yes	Yes	Yes
Observations	32,715	32,715	32,715	32,654	32,654	32,654
R^2	0.005	0.026	0.004	0.503	0.511	0.501

Significance codes: '***' 0.001, '**' 0.01, '*' 0.05, '^' 0.1

5.6 Conclusion

John Glenn International Airport opened a new runway to accommodate increased demand. August 22, 2013 was the first time that planes could simultaneously take of and/or land on the two runways. This addition allows for Columbus' airport to possibly double its number of flights a day in the coming years. The increased sound, or at least the anticipation of the increased sound, may affect house prices to the homes under the flight paths. I used a difference in differences approach to examine the effect of the increased sound after the new runway's opening on the homes to the east and west of the airport. The results indicate that there is a negative and significant effect on home sale prices to the affected areas after the runway opens.

My results are smaller (about a third to fourth of the size) than the effects found by Mense and Kholodilin (2014), but that study was looking at the introduction of a brand new airport. Since I am only concerned with the opening of a new runway in a much smaller airport and city, it makes sense that the impact of additional sound is not as large. The Berlin-Brandenburg airport has a projected number of annual passengers that is five times the size of Columbus. In comparison to the other hedonic price models in the literature, my preliminary results are slightly smaller as well. These results could be contributed to the findings of McMillen (2004) that suggests that newer planes are quieter than ever before.

My results suggest that the Columbus airport's increasing air traffic has a negative impact on the housing market in areas that are most affected by the sound. With the different area specifications, my estimates range from about a 2% to 10% decline in house sale price after the opening of the south runway in Columbus. The project cost about $140 million dollars, and is a part of a larger effort by the airport to increase the number of planes visiting John Glenn International Airport each year. As Columbus' Mayor Coleman said in 2013, the city continues to grow in size, the airport plans to expand with it (Weese, 2013). In addition to the construction costs, increasing airport noise is a another cost of this growth, which has the potential to reduce housing prices in the loudest areas.

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Chapter 6
Homeowner evaluation of crime across space

Eliot Alexander

Abstract I analyze how a homeowner's evaluation of crime is affected by the type of crime committed and the spatial distribution of crime around a home. Nearby crime is capitalized into house prices, but it is not well understood how the degree of that capitalization varies with the crime incident's distance from the house. I employ a simple hedonic model of house prices in Baltimore City using a micro-level dataset of location-specific crimes and houses to shed light on this relationship. Because crime is potentially correlated with a myriad of spatial unobservables, as crime could be attracted to areas of greater property value, I estimate a Hausman-Taylor model with nearby tree-canopy density and distance to the nearest park as instruments for neighborhood crime. I find that, though the historical average of neighborhood-wide crime does not significantly impact house prices, nearby crimes that occur during the homeowner's search period do decrease house prices, and this effect varies with the distance of the crime to the house. There is also significant heterogeneity of the impact of different types of crime on housing prices.

6.1 Introduction

Safety from crime is an important aspect of the bundle of amenities that local governments provide in an effort to attract new citizens and retain existing ones. This goal of minimizing the impact of crime on its citizens is, however, only one goal amongst many that must be supported by a limited budget. Because crime not only varies by type but has a non-uniform spatial distribution, understanding the impact of crime on citizens by type and space will enable policymakers to better allocate scarce funds to best serve its citizens. In this way, local police departments could place a greater emphasis on preventing crimes in areas that have the greatest impact on the overall well-being of residents.

To be sure, all crime is a dis-amenity. However, until a local government can eliminate crime altogether, it can maximize the utility of its citizens by targeting crime in a way that best reduces the crimes that residents want most strongly to avoid. In order to maximize law enforcement policy in this way, we must investigate resident preference heterogeneity across crime types and over the spatial distribution

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of crime that affects them. Numerous studies have utilized hedonic analysis to study the cost of crime.

Before Rosen's (1974), Kain and Quigley (1970) estimated implicit prices of specific aspects of the bundles of residential services in St. Louis. They found strong effects for building and neighborhood characteristics but insignificant effects for crime. They explained that including a neighborhood crime index is more a statement of good intentions than any serious effort to come to grips with the conceptual and measurement problems of defining a neighborhood's perceived safety, and argued that insignificance of crime was likely due to its high correlation with the residential quality data and the relatively low variance of crime within the city. Harrison and Rubinfeld (1978) control for neighborhood crime in their estimate of marginal willingness to pay for clean air in Boston. They found that marginal pollution damages are indeed increasing with total pollution level and with consumer income. They also found that the hedonic price of clean air exhibits large variance with different specifications of the hedonic housing price model: with plausible specifications of the relationship among air pollution, housing attributes, and housing values, aggregate benefit estimates may be reduced as much as 60% below the figure based on a constant marginal valuation. The study, which included FBI data on crime rate by town, incidentally found it had a higher effect than that of pollution (in annual average pphm of nitrogen oxide). Dubin and Goodman (1982) estimated the value of education and crime in the Baltimore area using 12 neighborhood crime variables and 21 neighborhood schools variables in addition to variables for housing characteristics. Using a Box-Cox transformation, they found that a semi-log model of price provided the best fit. They compared the results between Baltimore County and Baltimore City subsets of their data. In the city, a one unit change in non-violent property loss implied a $795 fall in house price, the violent crime component implied a decrease of $3,143, and "shopping center crime" implied a price decrease of $3,721. In the county subset, crime had a much smaller impact. Of the coefficients on the three crime variables, only the one for murder was significant, and an increase by one unit lead to a $1,184 decline in house price. They suggested that crime could indeed have a smaller cost in the county but also proposed that lower variation of crime in the county was more likely the reason. In concluding, they stressed that their findings only show marginal effects and warned that dynamic effects across the city could lead to different results if education or crime policy were implemented.

In an analysis of migration, Berger and Blomquist (1992) used a hedonic model as one of many steps towards determining which factors, such as wages and quality of life, most strongly affect an individual's choice to move. Their results showed that although rate of violent crime did not affect an individual's probability of moving, it did affect the individual's decision of to which location he or she will move, given a decision to move. They found that individuals on average were willing to accept $1.07 in combined compensation via wages and housing prices in response to a one-unit increase from the mean in the violent crime rate. Bowes and Ihlanfeldt (2001) used hedonic analysis to study various effects of proximity in Atlanta to a MARTA rail transit station. They proposed that transit stations could increase crime due to increased access to new areas for criminals. They found strong, significant effects of crime density on property value as well as a strong correlation between crime density and proximity to transit stations, supporting the theory that stations lead to higher crime. For higher-income areas, they found evidence that the potential for increased "booty" slightly outweighed the supposed increased probability of arrest. The increase in crime due to greater access was stronger for transit stations nearer downtown, the area of highest criminal activity, suggesting that criminals do not travel far outside high-crime areas.

In a similar context, Troy and Grove (2008) performed a hedonic analysis to investigate the relationship between violent crime and city parks in Baltimore. They found that public parks maintain a positive market value until the crime of the surrounding neighborhoods reaches a certain threshold, at which point they maintain a negative value. They compared the results of a linear model, a semi-log model, a Box- Cox transformation, and a spatially adjusted model to test for potential spatial auto- correlation, and obtained similar results for all four models.

Bishop and Murphy (2011) expanded the hedonic model to incorporate dynamic decision-making and applied the model to a dataset of violent crime, consumer race and income, and housing characteristics across multiple counties in the Bay Area of California. Their results held that, on average, households were willing to pay $472 a year to avoid a 10% percent increase in violent crime, and by comparison, static models underestimate willingness to pay to avoid crime by 21%. They also found heterogeneity of WTP for avoiding crime across incomes and races.

Bishop and Murphy (2011) develop a new econometric method for finding the underlying marginal willingness-to-pay function that avoids the problems of endogeneity described by Brown and Rosen (1982). By specifying a parametric form of this function, they derive an estimator and apply it to violent crime data from Los Angeles and San Francisco. They found that MWTP is indeed increasing in total level of crime; with each additional increase in violent crime per 100,000 residents, MWTP increased by between 20 to 30 cents. They hold that by assuming constant WTP across all levels of crime, the researcher will find severely biased estimates, which in this case would underestimate the total value of a non-marginal shift in crime levels.

Ihlanfeldt and Mayock (2010) also argue that researchers should be concerned about the endogeneity of crime in these models. A few studies attempt to correct for this issue by using instrumental variables (Rizzo, 1979; Naroff et al., 1980; Burnell, 1988; Tita et al., 2006), but only two test the validation of their instruments. The instrument of the first, the lags of explanatory variables in Buck et al. (1993) fails the overidentification test. Only Gibbons (2004) and Ihlanfeldt and Mayock (2010) give support for their instruments as appropriate ones. As I am also concerned about the endogeneity of crime on house prices, I estimate a Hausman-Taylor model with two instrumental variables in addition to a hedonic model.

Ihlanfeldt and Mayock (2010) address two additional issues in estimating the effect of various types of crime: multicollinearity and the specification of the crime variable. First, multicollinearity is likely an issue for our study. Ihlanfeldt and Mayock (2010) address this by transforming crime type variables from levels to annual changes, which are much less correlated. Because I use house-specific measures of crime in addition to the annual, neighborhood-level crime measures that Ihlanfeldt and Mayock employ, this method will not apply to all of our crime variables, and so instead we difference each individual crime variable with the corresponding census-tract-wide average, which decreases the collinearity of the variables. Using different measures of local crime -– specifically crime density, crime rate, and crime counts — can yield different results. The results of Ihlanfeldt and Mayock suggest that crime density — crime per area — best explains local price variation, and this study will examine both crime counts and crime density.

6.2 Model

As a basis, a simple hedonic model of housing prices for household i in neighborhood n is estimated in which houses vary by such as square footage and age as well as by their provision of local amenities such as tree cover, proximity to open space, and amount of nearby crime incidents:

$$lnP_{in} = \beta_0 + \beta_C X_{Ci} + \beta_H X_{Hi} + \mu_n + \varepsilon_i \tag{6.1}$$

where X_C and X_H are vectors of local crime and housing characteristics, respectively. Neighborhood fixed-effects control for time-invariant factors that influence house prices by neighborhood. Additionally, we estimate a Hausman-Taylor model to correct for endogeneity of crime variables and to compare the effects of recent and house-specific measures of crime to neighborhood-wide measures of crime across the entire dataset. The Hausman-Taylor model uses both within neighborhood and between neighborhood variation to explain observed housing prices and is given by:

$$ln\P_{in} = \beta_0 + X_{1in}\beta_1 + X_{2in}\beta_2 + Z_{1n}\delta_1 + Z_{2n}\delta_2 + \mu_n + \varepsilon_{in} \tag{6.2}$$

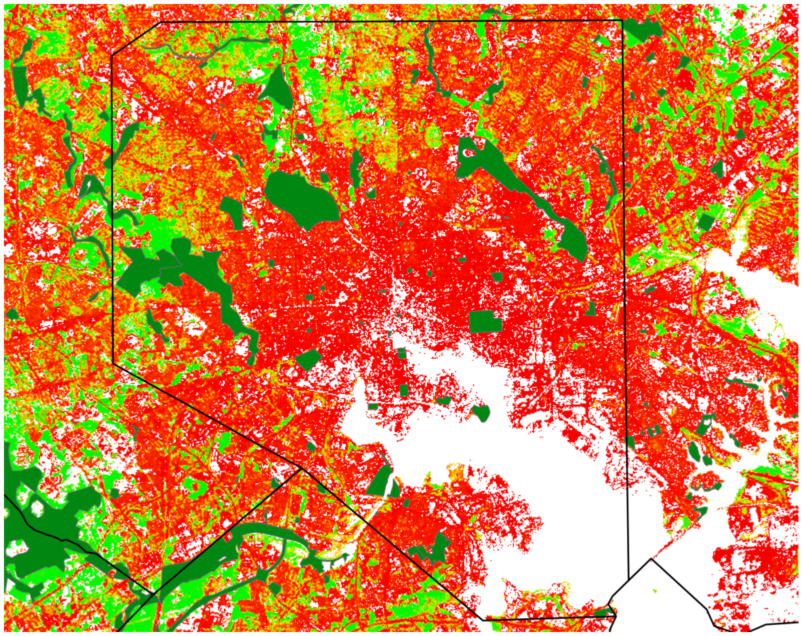
for variables X_{1in} and Z_{1n} that are assumed to be exogenous and variables X_{2in} and Z_{2n} that are possibly correlated with unobserved neighborhood effects μ_n. In the current context, Z_{1n} is the average distance to downtown of houses by neighborhood, Z_{2n} is the total number of crimes committed in the neighborhood over the four year span, X_{2in} is a vector of house characteristics that includes individual measures of local crime, and X_{1in} consists of two variables that will act as instruments for neighborhood crime: distance in feet to the nearest park, and average tree canopy cover of the house. We obtain robust standard errors that are clustered by neighborhood, as it is likely that house prices are spatially correlated. The BPD delineated these neighborhood boundaries, which presumably constitute some level of contiguous areas that are separate from one another.

A consistent estimate of neighborhood crime can be identified if the number of variables in X_{1in} is at least as large as the number of variables in Z_{2i} and if there is sufficient correlation between the instruments — distance to the nearest park and tree canopy cover -– and neighborhood crime i.e., the instruments are not weak. Further, the neighborhood averages of tree canopy, the distance to the nearest park, and the distance to downtown must be uncorrelated with the random effect μ_n.

It is reasonable to think that unobserved neighborhood-level factors that affect house prices do not significantly vary with neighborhood-level distance to parks, tree cover, or distance to downtown. The provision of parks across space in Baltimore City has been set for many years; public efforts concerning the provision of parks in Baltimore City focus on renovating existing parks rather than establishing new ones. Baltimore is a relatively old city, and most of its parks were founded in the 19th century. These historical connections are likely too old to have any remaining correlation to current neighborhood unobservables. Similarly, tree canopy cover in Baltimore City has gone largely unchanged in recent years except for small-scale efforts by private citizens that likely have a negligible effect on overall canopy cover.

Figure 6.1 shows that tree cover in Baltimore is sparse and appears to be correlated with park location. Public support of additional tree cover in Baltimore City focuses on subsidizing plantings by individuals rather than a larger-scale provision of trees in new areas of the city. Thus, it is likely that the spatial distribution of tree canopy cover in the city is based on older policies that are exogenous to current

Fig. 6.1 Baltimore City tree canopy density



neighborhood unobservables. Tree cover and distance to open space, however, are correlated with neighborhood-level crime; the correlation coefficients with crime are -0.40 and -0.2 for average neighborhood tree cover and distance to the nearest park, respectively. Though these relationships are perhaps the opposite of expected, they nevertheless entail a significant correlation. Rather than constituting a haven for criminals, perhaps parks attract residents to an extent that deters crime, as there are more potential witnesses in the surrounding area. It is possible that the same effect holds for tree cover. Tests for weak instruments and for overidentification confirm that these instruments are valid.

6.3 Data

The dataset employed is a panel of mortgage transactions in Baltimore City, Maryland between 2007 and 2010 that was matched with house-level measures of local crime, parks, and tree canopy cover. Figure 6.2 shows the distribution of the roughly 3,500 house and park locations across the city. The housing data includes detailed characteristics obtained from the Maryland Department of Planning's MDProperty View electronic GIS parcel data. The data includes US state plane coordinates for transacted houses, which were converted to latitude and longitude. We observe accurate spatial locations for both homes and local amenities and so can estimate the effect on house prices of crime as it varies spatially around the household. Table 6.1 shows summary statistics for the dataset.

The crime data was compiled by the Baltimore Police Department and contains longitude and latitude coordinates, the incident date, and the neighborhood where the crime took place. It is possible that there is a correlation between property val-

Fig. 6.2 Baltimore City housing transactions

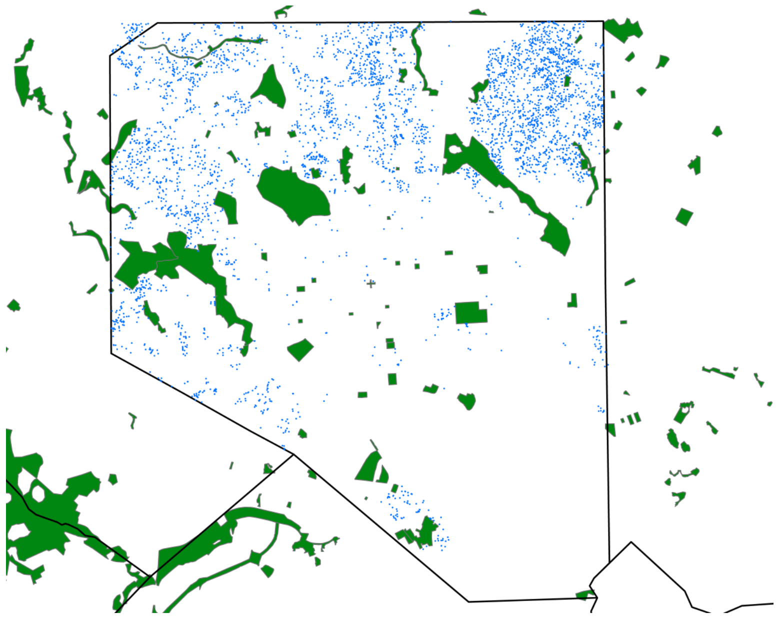


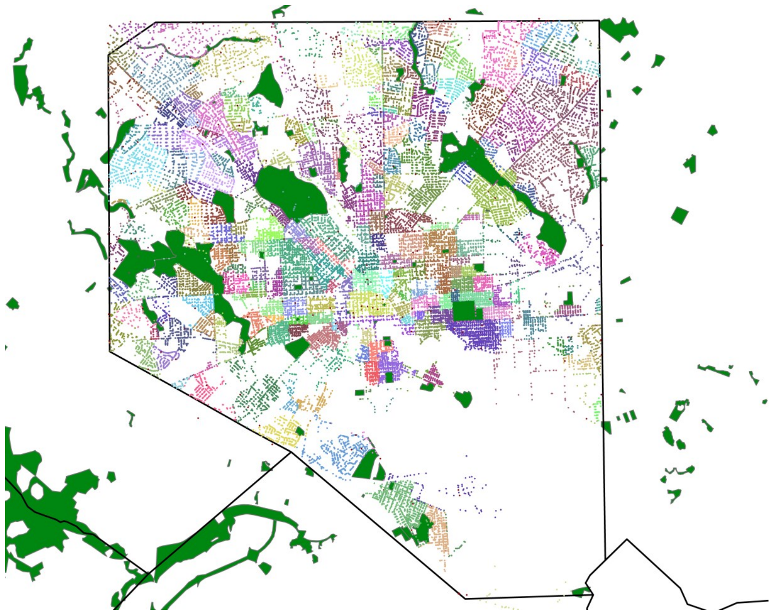
Table 6.1 Summary statistics

	N	Mean	Std. Dev.	Min.	Max.
SaleAmt	3,459	214,931	135,675	20,000	1,000,000
Crime1	3,459	18.0	13.8	0	167
Crime2	3,459	52.1	31.3	0	401
Crime3	3,459	195.9	104.6	3	1,216
CrimeSqMi1	3,459	143.4	109.5	0	1,329
CrimeSqMi2	3,459	138.3	82.9	0	1,063
CrimeSqMi3	3,459	129.9	694	2	806
MeanNeighCrime	3,459	851	913.5	27	3,689
CanopyCover	3,459	18.8	18.1	1	94.9
FeetToOS	3,459	1,288	849	0	4,191
BldgSqFt	3,459	1,672	631	528	5,926
GarageSqFt	3,459	93.4	154	0	1,152
Baths	3,459	1.6	0.75	1	5.5
Fireplace	3,459	0.3	0.46	0	1
Acreage	3,459	0.16	0.10	0.0045	1.58
HouseAge	3,459	71.4	16.7	0	100
MiToDowntown	3,459	4.9	1.1	0.54	7.6
OwnOcc	3,459	0.86	0.34	0	1

ues and crimes due to biases in reporting crimes if areas of higher crime and lower value experience underreporting of crimes; survey data shows that higher-income residents and homeowners are more likely to report crimes (Skogan, 1999). Violent crime is defined as aggravated assault, homicide, rape, arson, robbery, and shootings, while nonviolent crime is defined as auto theft, burglary, and larceny.

First, for each housing transaction, we limit certain measures of crime to crimes that occurred at a time that is likely to affect the house sale price. We define a window of time over which observed crimes will affect a house price valuation; this window spans from 180 days prior to the sale date through 30 days prior to the sale date. It is reasonable to expect that a homeowner would spend a period of roughly six months in the search of a house over which he/she observes characteristics of the prospective purchases. The last month prior to the sale date is excluded, as the final housing decision has likely been made already before this time while the relevant paperwork is carried out before the actual transaction date. Results are robust to changes in the specification of this time window.

Fig. 6.3 Baltimore City neighborhoods



A measure of crime is then calculated for each house as the total number of crime incidents by type that occurs within this time window prior to the sale date and within various exclusive radii of the house. For example, each house is assigned measures of the total number of violent and nonviolent crimes that occur within the relevant time window and occur within 0.2, 0.2 to 0.4, and 0.4 to 0.8 miles of the house. Each house is also assigned to a neighborhood, determined by that of the nearest crime to the house over the entire four-year dataset. Figure 6.3 plots each crime incident over the four-year span, color-coded by neighborhood to show the rough location of the nearly 150 neighborhoods as defined by the Baltimore Police Department. For a more long-term measure of crime across space, I also included in estimations the average number of crime incidents per neighborhood over the four-year span. In addition to a count of crime, I also calculate a measure of crime density for each house, defined as the count of crimes by type divided by the area in square miles of each concentric, exclusive band.

I must also address the potential for multicollinearity across different measures of crime, either by crime type or by distance from the house. It is not uncommon in the hedonic literature to exclude property crime and use only one measure of violent crime (Berger and Blomquist, 1992; Troy and Grove, 2008; Bishop and Murphy, 2011; Bishop and Timmins, 2011). However, when possible I include multiple types of both violent and non-violent crimes in order to reduce omitted variable bias and to compare the effects of different crime types. Table 6.2 shows the correlation coefficient between different measures of crime in the sample. Using a measure of crime counts above the census-tract mean decreases the collinearity of variables.

Table 6.2 Crime correlation

	Total crime count within				Crime count above mean within		
	0.2mi	0.4mi	0.8mi		0.2mi	0.4mi	0.8mi
0.2mi	1			0.2mi	1		
0.4mi	0.72	1		0.4mi	0.35	1	
0.8mi	0.52	0.72	1	0.8mi	0.12	0.37	1

A measure of local tree cover is also estimated using a dataset on tree canopy density from the National Land Cover Database. This includes an index of tree canopy cover as it is spatially distributed across the US. This index ranges from 0 (representing no canopy cover) to 100 (maximal canopy cover). Figure 6.1 shows the spatial distribution of tree cover for the city as a color gradient with green representing maximal cover and red representing minimal cover. The dataset is quite granulated, as it consists of multiple index values per city block. Each house is assigned the average index value of the nearest ten data points, representing roughly a city-block-wide measure of tree cover. The nearest canopy data point is also used as a robustness check. Data on parks from the United States Geological Survey was used to assign the distance to the nearest public park for each house.

6.4 Empirical results

Table 6.3 shows the results of the hedonic model of house prices in Baltimore City using three measures of crime within concentric, exclusive bands around each house. A quick check of the variance inflation factors for the various models suggests that multicollinearity is not likely an issue. The largest VIF value for the crime count and density variables is 7.94, and the largest VIF value for the crime count above the mean is 1.73. Measuring crime by count suggests that the homeowner discount of house prices for increased crime fades with the crime's distance from the house. However, these three bands cover increasingly large areas, so these measures do not account for crime density surrounding each house.

By adjusting each count for the area over which it consists, we obtain measures of crime counts per square mile. A model of these measures suggests that homeowners do not monotonically decrease their valuation of crime as it increases in distance from their house. Using crime counts and crime densities results in an equal fit of the variation in house prices. Estimation of these models while excluding counts of burglary and robbery, shown in table 3.6, generates more significant effects of crime. In this case, all three counts of crime are significant, and the effect of crime decreases as the crimes extend farther in distance from the house. Adjusting for density however, suggests that the density of crimes within 0.2, between 0.2 and 0.4, and between 0.4 and 0.8 miles around the house have similar effects on house prices. We cannot reject that these effects are equal.

Table 6.3 Hedonic crime model

Variable	Crime Count	Crime Density	Count Above Mean
Crime1	-0.0016*	-0.00021*	-0.0015*
Crime2	-0.00052	-0.00023	-0.00055
Crime3	-0.00027*	-0.00040*	-0.00013
AverageCanopy	0.0024***	0.0024***	0.0025***
FtToOS	0.000027*	0.000027*	0.000024*
OwnOcc	0.19***	0.19***	0.19***
BldgSqFt	0.00016***	0.00016***	0.00016***
GarageSqFt	0.00012***	0.00012***	0.00012***
MiToDowntown	0.033	0.033	0.067***
Fireplace	0.068***	0.068***	0.067***
Acreage	0.44***	0.44***	0.44***
Baths	0.072***	0.072***	0.07***
Age	-0.00089*	-0.00089*	-0.00094*
Constant	11.25***	11.25***	10.99***
SaleMonth FE	Yes	Yes	Yes
SaleYear FE	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes
N	3,459	3,459	3,459
R^2	0.6451	0.6451	0.6445

Notes: *, **, *** indicate statistical significance at the .10%, .05%, and .01%, respectively.

Table 6.4 Hedonic model without robberies or burglaries

Variable	Crime Count	Crime Density	Count Above Mean
Crime1	-0.0032***	-0.00040**	-0.0029**
Crime2	-0.0011*	-0.00040*	-0.00098
Crime3	-0.00043**	-0.00065**	-0.00021
AverageCanopy	0.0023***	0.0023***	0.0024***
FtToOS	0.000027**	0.000027**	0.000024*
OwnOcc	0.19***	0.19***	0.19***
BldgSqFt	0.00016***	0.00016***	0.00016***
GarageSqFt	0.00011**	0.00011*	0.00012***
MiToDowntown	0.030	0.030	0.067***
Fireplace	0.068***	0.068***	0.067***
Acreage	0.44***	0.44***	0.44***
Baths	0.071***	0.071***	0.07***
Age	-0.00089*	-0.00089*	-0.00094*
Constant	11.30***	11.30***	10.99***
SaleMonth FE	Yes	Yes	Yes
SaleYear FE	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes
N	3,459	3,459	3,459
R^2	0.6460	0.6460	0.6452

Notes: *, **, *** indicate statistical significance at the .10%, .05%, and .01%, respectively.

Table 6.5 shows a comparison of estimates between OLS, fixed-effects, random-effects, and Hausman-Taylor models. The results are fairly consistent between the models, except for estimates of the coefficient on neighborhood historical crime. Most models result in an insignificant effect except for the random-effects model, which, surprisingly, shows a positive effect of neighborhood crime on house prices.

Table 6.5 Comparison of OLS, fixed effects, random effects, and Hausman-Taylor models

	OLS	FE	RE	H-T
CrimeDensity1	-0.00043***	-0.00043***	-0.00035***	-0.00043***
CrimeDensity2	-0.00044**	-0.00040*	-0.00037*	-0.00040*
CrimeDensity3	-0.00064***	-0.0010***	-0.00055**	-0.0010***
NeighCrimes	-5.12E-6	No	0.000068*	-0.00041

Notes: *, **, *** indicate statistical significance at the .10%, .05%, and .01%, respectively. For brevity, stimates of AverageCanopy, FtToOS, MiToDowntown, BldgSqFt, GarageSqFt, Acreage, Baths, Age, Fireplace, and year effects are omitted.

Tables 6.6 and 6.7 show the results of the Hausman-Taylor model. The effects of crime count and density are very similar to those found in the hedonic model. We do not find a significant effect for total neighborhood crime, however. It is possible that the instruments used to estimate this endogenous variable are weak. Table 6.7 shows estimations that include each crime by time within certain radii. This not only provides further evidence that households value different types of crime differently, but that they treat various types of crime in different ways as they vary across space.

Table 6.6 Hausman-Taylor crime model

Spatially-Varying Exogenous	FeetToOS	0.000028***	0.000028***
	AverageCanopy	0.0023***	0.0023***
	Crime1	-0.0033***	-0.00041***
	Crime2	-0.0011**	-0.00043**
	Crime3	-0.00051**	-0.00078**
	OwnerOcc	0.19***	0.19***
Spatially-Varying Endogenous	BldgSqFt	0.00016***	0.00016888
	GarageSqFt	0.00011**	0.00011**
	Acreage	0.44***	0.44***
	Baths	0.071***	0.071***
	Fireplace	0.068***	0.068***
	Age	-0.00096**	-0.00096**
Spatially-Invariant Exogenous	MeanDistToDowntown	-0.024	0.024
Spatially-Invariant Endogenous	MeanNeighCrime	-0.00053	-0.00053
	Constant	12.18***	12.18***
	SaleMonth FE	Yes	Yes
	SaleYear FE	Yes	Yes
	N	3,459	3,459

Notes: *, **, *** indicate statistical significance at the .10%, .05%, and .01%, respectively.

Auto theft and rape are the only crime types that consistently decrease house values across multiple radii. Arson and burglary never appear to have a significant effect within any radius. Some variables such as larceny do not have a consistent pattern in the way they are valued at increasing radii.

Table 6.7 Hausman-Taylor crime by type model

Variable	Density within 0.2 miles	Density within 0.4 miles	Density within 0.8 miles
FeetToOS	0.000027**	0.000026**	0.000022**
AverageCanopy	0.0025***	0.0024***	0.0024***
Homicide	-0.0056**	8.06E-6	0.0052
Rape	-0.0091***	-0.013***	-0.022**
Robbery	0.0014***	0.0011*	0.0017
AggrAsslt	-0.00082*	-0.0012*	-0.00077
Burglary	-0.00022	-0.00004	-0.00024
Larceny	-0.00054***	-0.00041	-0.0011**
AutoTheft	-0.00082**	-0.0015***	-0.0018**
Arson	0.0021	0.00074	-0.0021
Shooting	0.0024	-0.0042*	-0.0069
MeanDistToDowntown	0.036	0.012	-0.0091
MeanNeighCrime	-0.00060	-0.00057	-0.0055
Constant	11.83***	12.0***	12.12***
SaleMonth FE	Yes	Yes	Yes
SaleYear FE	Yes	Yes	Yes
N	3,459	3,459	3,459
Hansen's J stat	0.255	0.275	0.474

Notes: *, **, *** indicate statistical significance at the .10%, .05%, and .01%, respectively.

6.5 Conclusion

This study provides evidence that there is significant heterogeneity not only in the way homeowners value different types of crime but also in the way they value the spatial distribution of crime surrounding their house. This study represents an initial look into the way homeowners value safety spatially. Spatial proximity is indeed a significant component of how homeowners evaluate local crime; homeowners have a greater willingness-to-pay for avoiding crimes that are located closer to their homes.

In an attempt to address the potential endogeneity of crime on housing prices, we find that tree canopy cover and distance to the nearest park constitute valid candidates. Multiple model estimations strongly fail to reject the hypothesis that these instruments are uncorrelated with the error term and also are correctly excluded from the price equation. However, it is possible that these are weak instruments -– that they are not jointly significant in explaining endogenous crime.

This study is a first step towards understanding how residents view the spatial component of crime. Because residents do not treat crimes of all types and in all locations equally, policy-makers and law enforcement agencies should similarly consider type and spatial heterogeneity of crimes when planning and enacting policy to reduce crime. This is not to say that certain crime types or certain areas should be ignored but that a greater focus should be placed on crime types and areas that have a greater negative impact on residents.

Residents do not appear to evaluate crimes categorized within perhaps arbitrary spatial boundaries like neighborhoods. Instead, homeowners do take into account physical distance from their home when evaluating safety from crime. For some crimes such as homicides, residents appear to only be affected by incidents that are within a small distance from their house, while for other crimes like automobile theft, larceny, and rape, homeowners are averse to incidents farther away.

Policymakers can use these results and those of further studies to implement law enforcement that accounts for the spatial distribution of houses and crime into account to not just maximize the safety of residents but the well-being of residents, measured by the willingness-to-pay to avoid crime by type and space. For crime types that appear to harm residents over large distances, such as auto theft and rape, law enforcement could focus on city-wide efforts to reduce incidents. However, for some crimes, such as homicide, law enforcement could focus on areas that have a large density of residents. In this way, in a time of tight state and city budgets, law enforcement could enact measures that are both more cost-effective and better impact residents.

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Chapter 7
The Effects of Zebra Mussel Infestations on Property Values: Evidence from Wisconsin

Martin E. Meder and Marianne Johnson

Abstract Containment strategies for invasive species often require significant changes in behavior. Success in fostering these changes requires that individuals correctly perceive the costs and benefits. Yet, while both the ecological and aesthetic impacts of some invasive species are universally negative it less clear how lakefront property owners might view zebra mussel infestations. Zebra mussels negatively affect the local ecology, crowd out native species, and deposit sharp shells on beaches. However, they are also associated with increased water clarity, a reduction in suspended pollutants, and increased numbers of popular game fish. We use a spatial lag and spatial error hedonic model to estimate the impact of zebra mussel infestations on lakefront property prices in Waupaca County, Wisconsin. We find no evidence that homes on infested lakes incur a penalty at sale; consequently, it may be difficult to generate compliance with containment strategies to effectively halt the spread of zebra mussels.

7.1 Introduction

Climate change and increased ease of transportation have accelerated the spread of invasive species with deleterious ecological consequences (Thomas and Hanson, 2007; Rahel and Olden, 2008; Zipp et al., 2019). As species such as the emerald ash borer, the spruce bark beetle, the lionfish, and Asian carp expand their range, national, state and local environmental groups have aggressively moved to implement a range of containment and mitigation strategies. Yet despite uniformly negative publicity, recent studies suggest that people form complex and multifaceted views of infestations. For example, Hansen and Naughton (2013) find that spruce bark beetle infestations are associated with an increase in property values in Alaska. Kovacs et al. (2011) document that Sudden Oak Death sometimes negatively and sometimes positively effects property values. Zhang and Boyle (2010) are unable to conclusively demonstrate that lakefront homeowners penalize properties on lakes infested with Eurasian watermilfoil, though these owners do care about the total amount of plant growth in the lake. In other cases, infestations have had an over-

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whelmingly negative impact on property values, such as that identified for the insect, the hemlock woolly adelgid (Holmes et al., 2006). These studies suggest that perceptions of the economic impact of invasions may be an empirical question. This may be especially true for zebra mussels (Dreissena polymorpha), where the ecological and scientific evidence does not convincingly tell us whether the invasive species should be perceived as “a good” or “a bad” by lakefront homeowners and recreational boaters.

Native to Eastern and Central Europe, zebra mussels were first detected in the United States in 1988 in Lake St. Clair near Detroit, Michigan. It took barely twenty years for them to spread to more than thirty states, endangering ecologies as diverse as Lake Okeechobee in Florida (Adams and Lee, 2012) and the Great Lakes in the north central United States (Timar, 2008). Primarily spread by transient recreational boating, zebra mussels are associated with significant environmental disturbances. They alter ecosystems by crowding out native species, particularly fish such as carp and sturgeon, and by increasing water clarity (Klerks et al., 2001; Strayer et al., 2004; McCabe et al., 2006). In addition to the ecological harm, zebra mussels generate millions of dollars in direct annual costs by inhibiting the functioning of intake pipes and cooling systems and by affecting the operation of other permanent submersed structures (Timar and Phaneuf, 2009).

Previous research on zebra mussels has focused modeling human-induced spread (Timar and Phaneuf, 2009), predicting contaminations (Timar, 2008), and optimal mitigation strategies (Adams and Lee, 2012). However, containment and mitigation strategies for invasive species such as zebra mussels are costly and often require significant changes in behaviors and practices (Cobourn et al., 2019; Eiswerth et al., 2011; Roberts et al., 2018). Practices such as hot washes and boat dry-outs can be effective, though all require financial and time investments from boaters and homeowners. Their incentive to conform with containment policies is complicated by the nature of the ecological changes engendered by zebra mussels. In addition to the negative implications discussed, Zebra mussels are also associated with increased water clarity, which is highly valued by lakefront property owners. For fishermen they disadvantage some unpopular species, such as carp, while encouraging more popular species such as smallmouth bass (Strayer et al., 2004).[1] For swimmers, they decrease the concentration of suspended pollutants in the water column through biosequestration, yielding cleaner water. However, the sharp shells of zebra mussels can mar beaches and encrust partially submerged structures such as piers in an unattractive layer of shells. Hence, despite consistently negative publicity, it is not clear that lakefront homeowners would necessarily place an implicitly negative value on zebra mussel infestation. Where ecological disturbances are associated with both positive and negative amenities, this makes the combined effect an empirical question. To convince individuals to modify their lake-use habits, it is therefore important to develop accurate estimates of the costs as perceived by lakefront property owners as a first step to designing response strategies.

[1] Negatively, zebra mussels impede the feeding habits of lake sturgeon, around which an entire seasonal tourism industry has developed in some areas of Wisconsin (McCabe et al., 2006).

7.2 Methodology

7.2.1 Model

We employ a hedonic model to estimate the empirical impact that zebra mussels have on lakefront property prices. Such estimates are highly useful to policy makers and for constructing cost-benefit analyses of containment and mitigation strategies. In hedonic pricing models, the implicit values consumers place on the individual features of a property can be identified by regressions of sales price on the various characteristics of the product (Rosen, 1974). These characteristics generally include the features of the structure and of the land, as well as additional location and environmental characteristics. In an estimated model, the slope coefficient on a particular characteristic represents the marginal willingness to pay. Hedonic models are commonly used to assess legislated or other land-use policy changes, as the model allows researchers to isolate the effect of the policy variable on housing prices, and hence estimate the additional value or penalty as perceived by consumers. Only a handful of studies have attempted to use hedonic price models to estimate the impact of invasive species (Halstead et al., 2003; Hansen and Naughton, 2013; Horsch and Lewis, 2009; Zhang and Boyle, 2010). While there are clearly a number of limits to hedonic pricing models in generating welfare estimates for environmental policy changes, these models can provide useful empirical evidence of the value that consumers place on an environmental amenity or dis-amenity (Hansen and Naughton, 2013; Holmes et al., 2006; Leggett and Bockstael, 2000).

One complicating factor of hedonic models is there is little theoretical basis to drive the functional form of the estimation equation. This has been extensively discussed with in the literature; the general consensus is that simpler forms perform as well or better as more complex variations, with the additional advantage of providing more intuitive relationships and explanations (Cropper et al., 1988; Leggett and Bockstael, 2000). Following the common strategy in literature, we adopt a specification where the dependent variable is the sales price of a property as determined by the market (P_i); see Equation 7.1. While hedonic models often focus on isolating the effect of a policy change or an environmental amenity or disamenity, a number of additional explanatory variables are generally included to capture the main factors that influence property values. Like Rosen (1974), we speculate that the sales price of a property is a function of the property's structural (H), land (L), geographic (G) and time (T) characteristics. Price is also influenced by presence/absence of zebra mussels (Z), as indicated in Equation 1.

$$P = f(H, L, G, T, Z) \tag{7.1}$$

Hedonic pricing models frequently suffer from the econometric problem of endogeneity. For example, zebra mussels are spread by contaminated boats or other recreational vehicles moving between bodies of water. Higher demand lakes—lakes with features that make it attractive for recreational use or lakes which are closer to population centers—are more likely to become infested. Simultaneously, these same lakes tend to evidence higher property values. Previous studies of aquatic invasive species have grappled with endogeneity in different ways. Horsch and Lewis (2009)) employ a difference-in-difference methodology to assess the impact of Eurasian watermilfoil, whereas Zhang and Boyle (2010) utilize lake fixed effects to address the same problem. However, neither of these approaches addresses the underlying spatial interdependence that is inherent in this type of hedonic model. Spatial interdependence implies that the sales prices of neighboring properties are more related than the sales prices of properties further away. Failing to account for the fact that properties near each other are more related than properties more distant generates biased results, as the error terms from the Ordinary Least Squares regression (Equation 7.2) will not be normally distributed and likely will be correlated with each other.

$$P_{it} = \beta_0 + \beta_1 H_{it} + \beta_2 L_{it} + \beta_3 G_{it} + \beta_4 Z_{it} + \beta_5 T_t + u_{it} \tag{7.2}$$

We can address the problem of endogeneity engendered by spatial interdependence through the use of a spatial lag and spatial error term. This strategy has been used effectively to examine the impact of spruce bark beetle infestations (Hansen and Naughton, 2013) and air pollution (Carriazo et al., 2013) on property values. Our spatial maximum likelihood model therefore estimates the sales price of property i in time period t, P_{it} (Equation 7.3) as a function of the same structural, land, and geographic characteristics as used in Equation 7.2

$$P_{it} = \rho \Sigma_{j \neq i} w_{jt} lnP_{jt} + \gamma_0 + \gamma_1 H_{it} + \gamma_2 L_{it} + \gamma_3 G_{it} + \gamma_4 Z_{it} + \gamma_5 T_t + u_{it} \tag{7.3}$$

but where

$$u_{it} = \lambda \Sigma_{j \neq i} w_{jt} u_{jt} + e_{it} \tag{7.4}$$

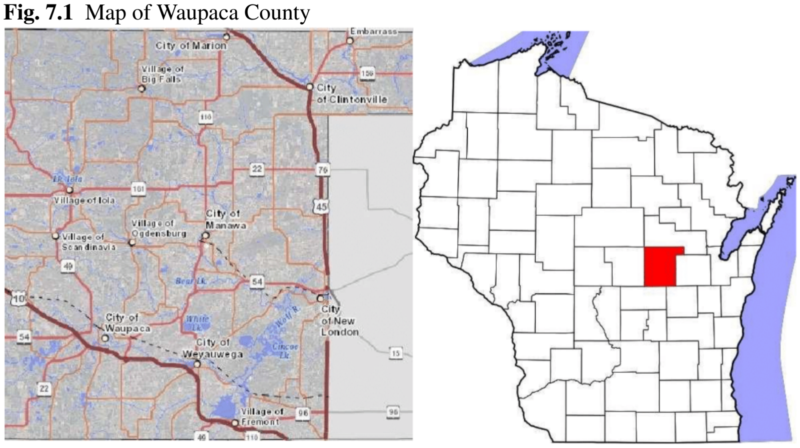
The spatial lag represented by $\Sigma_{j \neq i} w_{jt} lnP_{jt}$ is the weighted average of other properties' sales prices. Weights (w) are assigned based on the inverse distance between the properties included in our sample, as identified using GIS determined latitude and longitude measured at the center of each property. The estimated coefficient on the spatial lag term, ρ, illustrates how the sales price of one property is influenced by the sales prices of neighboring properties. Additionally, the spatial error, $\Sigma_{j \neq i} w_{jt} u_{jt}$, is the weighted average of the other observations' error terms, using the same weights (w) as the spatial lag, with e_{it} representing the error term. The estimated coefficient, λ, does not have a useful interpretation in this case (LeSage and Pace, 2009). By directly incorporating spatial location into the model, we can thus disentangle the impact of zebra mussel infestations from consumer preferences for lakes with particular location attributes.

7.2.2 Study Area and Data

Hedonic pricing models rely on the assumption that all property sales occur in the same housing market and hence prices represent the market equilibrium price, conditional on house and land characteristics. This is problematic when studying invasive species. Properties located on a single lake would constitute a single housing market but would not evidence variation in infestation status. Infestation status varies across lakes, but these lakes may not exist in the same property market. Hence, researchers face a difficult task appropriately identifying a market to study, as no agreed upon theoretical tests of market boundaries exists.[2] This leaves researchers to empirically identify reasonable markets for study.

We focus our study on the north central Wisconsin region of the United States. The region's reputation as a vacation destination as well as its proximity to the regional metropolitan areas of Madison, Milwaukee, Chicago and Minneapolis help identify it as a well-defined market (see Figure 7.2.2). Chi and Marcouiller (2012) argue that this area represents a "contiguous remote rural region" with most counties nonadjacent to a significant urban center. These counties are classified as "recreational" and are likely to include substantial numbers of second homes (Chi and Marcouiller, 2012). For our purposes, by choosing to purchase lakefront property in this area, owners have self-selected for an interest in aquatic recreational activities. Further, by indicating a premium on lake use, these buyers are likely sensitive to invasive species infestation. Unlike dis-amenities such as water pollution, which can be difficult for property owners to discern, the infestation status of lakes are well known to owners and potential buyers and is required by state of Wisconsin disclosure laws.

Fig. 7.1 Map of Waupaca County



While the expansion of zebra mussels may seem like a dynamic problem, several issues mean that it is difficult to statistically treat it as such. First, expansion is limited by the availability and suitability of ecosystems that can support them; e.g., not all lakes provide the appropriate environment for zebra mussels to thrive. The corollary is that infestations spread much faster to popular lakes that provide suitable habitats. This demonstrates itself in the data, where infestations are observed to occur in waves. A second issue is that Wisconsin Department of Natural Resource sampling practices are highly inconsistent and rely on a mix of random checks and homeowner reporting. Thus, while the true number of infestations every year is likely not zero, and therefore not entirely static, the level of dynamism—or even the extent or direction of invasion—cannot be practically observed.

This makes Waupaca County in north central Wisconsin of particular interest. Its zebra mussel invasion, which has gradually made headway in a large chain of lakes

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[2] Additional issues with hedonic pricing models have been extensively discussed (Leggett and Bockstael, 2000; Horsch and Lewis, 2009). These include the absence of a theoretical basis for the functional form of the model and difficulties with omitted variable bias (Palmquist, 1991).

in the county in the years since the establishment of the Wisconsin Integrated Property Assessment System in 2009 provides us with a natural case study. While the first zebra mussel infestation in the county was identified in Partridge Lake in 2005, all further infestations occurred between 2010 and 2014. Consequently, a large number of houses sold on infested lakes, while others with similar attributes sold in close proximity on non-infested lakes, often within the same township. While this kind of invasion has occurred in other regions of Wisconsin, such as the Spread Eagle Chain of Lakes in Florence County, Waupaca County's relative population density and affluence allows for a greater number of observations owing to a thicker market.

According to the U.S. Census, Waupaca County is home to roughly 52,000 people, its population remaining highly stable across recent decades. With 126 lakes and a land area of 747 square miles, Waupaca County is decidedly rural, averaging 70.1 persons per square mile. The population is overwhelmingly white with a median household income of $49,777, slightly below the national median. The homeownership rate is nearly 77%. The county is home to twelve incorporated cities and a variety of smaller villages and townships. Waupaca County records roughly 25,364 housing units, 14.8% of which are in multi-unit structures. However, because of the nature of our question, we focus solely on single-family lakefront properties that sold between 2009 and 2014. We exclude unimproved properties as well as properties with temporary structures. Property transactions whose real estate transfer returns indicate a transfer fee exemption were excluded from the data set to ensure that all observed transactions occurred at arm's-length. Our sample thus includes 102 properties from 16 cities, towns and villages on 43 different lakes. In our data set, 30.4% of the lakefront properties sold in Waupaca County were on lakes infested with zebra mussels, which is notably higher than the state-wide average.

Data were collected from the state's Wisconsin Integrated Property Assessment System which makes available the sales of all properties, sales price, date of sale, as well as some additional in formation. The system is designed to publicly provide information as required by state open records laws. Additional data is from the Wisconsin Department of Natural Resources lake database, county-level property assessments and the Geographic Information System (GIS). Summary statistics are reported in Table 7.1. Sales prices were adjusted to pre-recession values using the Wisconsin House Price Index of the St. Louis Federal Reserve Bank as a way to account for the recent instability in home prices in the United States. The mean sales price in 2007 dollars is nearly $385,000, with a median price of $270,457.

Structural characteristics (H) include the number of bedrooms, number of bathrooms and half baths, square footage and year in which the house was built. We also have dummy variables indicating whether the home has central heating and/or a basement. As is common in the literature, we include a measure of the distance from the center of the property to the closest highway, measured in miles, determined by GIS mapping. Land characteristics (L) include measures of acreage and lake-frontage.

The geographic characteristics (G) merit some additional discussion. We include several measures designed to capture buyer preferences for recreational lake features such as lake area, lake depth and whether the lake has been endorsed for sport fishing. Empirically, lakefront homeowners have also exhibited strong preferences for clearer lakes (Leggett and Bockstael, 2000; Zhang and Boyle, 2010), so we include Secchi depth as a measure of water clarity. However, zebra mussels are known to improve water clarity over time. Thus, we establish a baseline for water clarity by adopting the 1999 Secchi depth measure for each lake, dating Secchi depth to a period prior to the arrival of zebra mussels in this county. In this way, subsequent im-

Table 7.1 Descriptive statistics

Variables	Mean (Frequency)	Standard Deviation
Sales Price (December 2007 Dollars)	$384,734.60	$331,809.20
House Characteristics		
Bedrooms	2.74	0.99
Bathrooms	1.85	1.01
Half Baths	0.31	0.47
Existing Basement	70.59%	-
Central Heating System	95.10%	-
Square Footage	2958.53	2050.99
Year Built	1967	28.31
Land Characteristics		
Acreage	1.22	3.21
Frontage	142.45	185.23
Geographic Characteristics		
Lake Acreage (in 10s)	16.88	30.74
Lake Depth (feet)	36.12	26.42
Secchi Depth (feet)	8.87	4.18
Endorsed for Fishing	62.75%	-
Distance to Highway (miles)	0.604	0.607
Invasive Species		
Zebra Mussels	30.39%	-

provements in water clarity can be attributed to the infestation and the value placed on this change by lakefront homeowners subsumed in the estimated coefficient for zebra mussels. Anecdotally, we can see the interaction between zebra mussel infestations and lake clarity in our sample: Rainbow Lake had a baseline Secchi depth of eight feet in 1999. Infested with zebra mussels in 2011, its water clarity improved to 10.03 feet in 2013 and 10.25 feet in 2014.

While some previous hedonic studies of environmental dis-amenities have attempted to measure the extent of the "bad" (Carriazo et al., 2013; Leggett and Bockstael, 2000; Zhang and Boyle, 2010), measuring the extent of a zebra mussel infestation is highly problematic for several reasons. Although tests exist to detect the concentration of veligers, the larval form of zebra mussels, results may not provide an accurate representation of the number or concentration of adult mussels as veligers are vulnerable to cannibalism by adult mussels, lack of suitable habitat, or other environmental factors (Maclsaac et al., 1991; Strayer et al., 1996). Further, visual monitoring techniques are ineffective for a species which primarily inhabits the lake bed. These problems lead to inconsistencies in reporting and measurement by the Wisconsin Department of Natural Resources. We suggest that buyers are less sensitive to the extent of an infestation than to the existence of an infestation.[3] Considering that the visibility of zebra mussels can vary significantly between lakes and across time periods, being almost entirely unobservable over the winter months, the use of an indicator variable is most appropriate.

[3] Similar problems to those associated with zebra mussels confound Wisconsin DNR data on Eurasian watermilfoil. The visibility of milfoil is significantly affected by weather conditions and is unobservable between October and March in Wisconsin, meaning buyers during this period cannot assess an infestation. They only know if a lake is infested or not.

7.3 Analysis

To provide a benchmark for analysis, we first estimate the standard Ordinary Least Squares (OLS) model as specified in Equation 7.2, exclusive of the spatial lag and spatial error term. This is equivalent to assuming that λ and ρ are simultaneously zero.[4] These results are compared to a specification that includes only the spatial lag or the spatial error and then finally a specification that includes both the spatial lag and the spatial error. Our estimation results are presented in Table 7.2. As is common in hedonic studies, we transform the dependent variable to be the natural log of real sales price, which we measure in December 2007 dollars (lnP_{it}). We also incorporate a month time trend to ensure that differences in sales prices across time that are common in all properties will not bias estimates.

As expected, across all four regressions, square footage, the number of full bathrooms, and the year in which a house was built are of central importance to home buyers, their estimated coefficients evidencing positive and statistically significant associations with sales price. Additionally, home buyers place a premium on convenient access to the property. For each additional mile that a property is located from a main highway, home price decreases by about 12 to 15% ($p \approx 0.05$). Our estimated coefficients for these factors are highly consistent with the ranges found by other hedonic studies and are robust to specification and estimation procedure. The number of bedrooms and acreage also contribute positively to sales price in all cases, though they are not statistically significant in any of the four specifications. We retain these variables to maintain consistency with other studies in the hedonic pricing literature.[5] We find that buyers are also sensitive to the amount of lakefrontage available on the property, though the relationship is nonlinear. While the estimated coefficients on frontage are consistently negative and the estimated coefficients are quite small for the frontage-squared term, across Regressions 1 through 4, they are in fact logical. The estimates indicate that for most standard frontages, an extra foot or so has very little impact (negative, but essentially zero). However, particularly large expanses of frontage command notably higher prices.[6]

Home buyers exhibit a strong preference for deeper lakes ($p < 0.01$); lakes endorsed for fishing command an additional premium of 23% to 31% of sales price ($p < 0.05$). Neither result is surprising given the strong revealed preferences by property owners in this region for sport fishing. Our measure of baseline water clarity, 1999 Sechi depth, is positively associated with sales price, though not statistically significant in either of the regressions that use a spatial lag.

As previously discussed, zebra mussels generate both positive and negative effects on their environment, when viewed from the perspective of lakefront property owners.[7] Thus we have no intuitive guide to evaluate the estimated effect generated by a hedonic model, nor any presumption as to whether the sign should be positive

[4] The heteroskedastic robust OLS estimates, clustered by township/village yields, nearly identical results to the basic OLS model, hence we only report the most basic model.

[5] We do not find a statistically significant relationship between property sales price and number of half bathrooms, the existence of a basement or whether the property has improved central heating. We do not retain these variables in the final regressions for parsimony.

[6] The estimates indicate that after roughly 393 feet of frontage, each additional foot of frontage contributes positively to sales price. Note that one meter is equivalent to 3.28 feet.

[7] Johnson and Meder (2013) use a non-standard hedonic model and basic OLS to estimate the impact of zebra mussels in 17 counties in north central Wisconsin ($n = 1072$). They find that lakes infested with zebra mussels evidence 10% higher sales prices than comparable properties on uninfested lakes. We know of no other hedonic model estimates for zebra mussels.

Table 7.2 Hedonic regression results

Variables	OLS	Spatial Lag	Spatial Error	Mixed Spatial Lag & Spatial Error
House				
Bedrooms	0.081	0.036	0.062	0.024
	(0.058)	(0.051)	(0.055)	(0.051)
Full Bathrooms	0.183***	0.142***	0.171***	0.125**
	(0.061)	(0.053)	(0.058)	(0.052)
ln(Square Footage)	0.176*	0.183**	0.175**	0.182**
	(0.092)	(0.079)	(0.096)	(0.075)
Year Built	0.006***	0.007***	0.006***	0.007***
	(0.002)	(0.001)	(0.002)	(0.001)
Land				
Acreage	0.015	0.020	0.013	0.022
	(0.020)	(0.017)	(0.018)	(0.017)
Frontage	-0.001	-0.001**	-0.001*	-0.001*
	(0.000)	(0.000)	(0.000)	(0.000)
Frontage Squared	106e-6	1.18e-6*	1.26e-6*	1.06e-6*
	(7.03e-7)	(6.00e-7)	(6.77e-7)	(5.90e-7)
Geographic				
Lake Depth	0.011***	0.009***	0.011***	0.009
	(0.003)	(0.002)	(0.003)	(0.002)
Lake Acres	0.001	0.002	0.002	0.003*
	(0.002)	(0.002)	(0.002)	(0.001)
Secchi Depth	0.026*	0.017	0.025*	0.012
	(0.014)	(0.012)	(0.014)	(0.011)
Endorsed for Fishing	0.307**	0.250**	0.301**	0.229**
	(0.134)	(0.115)	(0.131)	(0.107)
Distance to Highway	-0.139*	-0.136*	-0.151**	-0.127**
	(0.075)	(0.064)	(0.074)	(0.060)
Zebra Mussels	0.238*	0.089	0.162	0.066
	(0.131)	(0.118)	(0.150)	(0.155)
Month Time Trend	0.006**	0.006***	0.006***	0.006***
	(0.003)	(0.002)	(0.002)	(0.002)
Spatial Lag (ρ)	-	0.506***	-	0.707***
	-	(0.129)	-	(0.144)
Spatial Error (λ)	-	-	0.357	-0.330
	-	-	(0.328)	(0.267)
Observations	102	102	102	102
R^2/Squared Correlation	0.79	0.82	0.79	0.82

Notes: Dependent variable is ln(Adjusted Sales Price). Constant included but omitted for space. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

or negative. Our standard OLS model, as reported in Regression 1 in Table 7.2, suggests that zebra mussel infestations are associated with a 26.9% increase in property values ($p = 0.07$).[8] However, this model fails to incorporate spatial effects, which

[8] This comes from using the standard transformation $100 * [e^x - 1]$ transformation for interpreting the coefficients of dummy variables in models with the dependent variable being transformed by the natural log (Wooldridge, 2010).

likely causes our estimate to be upward biased. Diagnostic tests for spatial dependence suggest the use of a mixed spatial lag and spatial error model. The need for a spatial lag is highly recommended by diagnostic tests (Lagrange Multiplier = 12.03, $p = 0.001$). Moran's I for the spatial error (= 1.54, $p = 0.124$) is suggestive but not definitive.

The spatial lag coefficient (ρ) is both positive and statistically significant in regressions 2 and 4, providing evidence of spatial spillovers between the sales price of a property and that of its neighbors. For example, a 1% increase in neighboring sales prices is associated with an increase in sales price of 0.51% (Spatial Lag regression) to 0.71% (Mixed Spatial Lag & Spatial Error regression). This demonstrates that the factors that influence the sales price of one property such as neighborhood amenities and/or infrastructure development will spillover to affect the value of neighboring properties as well. The use of the spatial lag thus captures these underlying omitted variables that contribute to the endogeneity problem inherent in hedonic studies. The spatial error performs less well in our estimated equations. In neither of the two rightmost columns is the estimated coefficient statistically significant ($p = 0.28$ and $p = 0.22$), suggesting that the Spatial Lag in column 2 is the preferred specification.

Across all specifications, zebra mussel infestations are associated with higher sales prices for lakefront homes, suggesting that the visible effects of an infestation, in including clearer water and more high-value fishing, outweigh the negative perceptions of an infestation. The spatial lag model reported in column 2 posits that homes on infested lakes will command a 9.3% premium over otherwise comparable houses on un-infested lakes (the mixed spatial lag and spatial error specification suggests 6.8%). This corresponds to a welfare gain of approximately $25,000 on a median lakefront property. Note that both estimated values are substantially smaller and less statistically significant than that generated by standard OLS, which fails to account for spatial interconnections. In neither case is the estimated coefficient statistically significant, suggesting that the true impact of an infestation cannot be differentiated from zero. This implies that even in areas where zebra mussels command significant attention in the local news, consumers do not prioritize infestation status when selecting recreational lakefront properties.

7.4 Conclusion

The absence of hedonic studies of zebra mussels is surprising given the attention devoted to invasive and potentially invasive species in the United States and Canada. One of the reasons for this absence may be the confounding effects that zebra mussels have on their environment. An infestation engenders both visible positive and negative effects on a lake. Zebra mussels improve water clarity and sports fishing while reducing suspended pollutants in the water. Zebra mussels also crowd out some fish species important to the Wisconsin fishing industry, notably sturgeon, and deposit sharp shells along beaches. It therefore becomes an empirical question, whether lakefront homeowners suffer from an infestation.

In this study, we evaluate how the spread of zebra mussels through a chain of lakes in Waupaca County, Wisconsin has influenced the sales prices of lakefront properties. In spite of their uniformly negative reputation, we find no evidence that zebra mussels are associated with decreased prices for lakefront properties. Rather, using a Maximum Likelihood estimation strategy with a spatial lag, we find that an infestation is associated with positive but not statistically significant increases in lakefront property values. Our finding of a zero or slightly positive association between zebra mussel infestations and property values has an important policy implication. Since most mitigation and control mechanisms require substantial changes in user behavior, the lack of a negative effect on property values suggests that it might be difficult to generate substantial levels of compliance. If lakefront home owners do not perceive a negative effect from zebra mussels, they are much less likely to regularly practice hot boat washes or regularly clean their boats, both of which are inconvenient and engender significant opportunity costs. These results may also indicate that mechanical or chemical control methods employed by lake associations for invasive species such as zebra mussels are unnecessary from a homeowner perspective. We caution readers that this paper is an initial attempt to assess the impact of zebra mussel infestations on lakefront property values; as such, it unavoidably has a number of limitations. The most important is the small sample size, while carefully selected and suggestive, demands further replication, both in Wisconsin and in other states.

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Chapter 8
A Case for Amenity-Driven Growth? The Value of Lake Amenities and Industrial Disamenities in the Great Lakes Region

Heather M. Stephens

Abstract In advanced economies, such as the United States, people increasingly value quality of life and are willing to pay for access to natural amenities. In the Ohio Great Lakes region, residents enjoy access to an abundance of fresh water and natural and recreational amenities. At the same time, however, they are confronted with the artifacts of the region's industrial legacy in disamenities, including factories and other industrial sites. Given the economic restructuring of many of the region's industries, there is interest in understanding how the competing effects from natural amenities and industrial disamenities affect housing demand and regional growth. To assess this, we utilize a unique dataset that includes over 300,000 housing transactions in the Greater Cleveland (Ohio) region and detailed geographically-located data on industrial disamenities, urban amenities, and natural and related built amenities. Using the hedonic pricing method, we estimate the value that residents place on these amenities and disamenities. We find a positive capitalization effect from being directly on Lake Erie. However, away from houses in close proximity to the lake, the effects are more heterogeneous. Additionally, there is evidence that proximity to manufacturing is associated with lower house values, especially near the lake. This suggests that, as the region's economy transitions, residents may positively value less industrialization near the lake and a focus on Lake Erie as a natural and recreational amenity.

8.1 Introduction

Lake Erie has long been the driving force behind economic development in the Greater Cleveland, Ohio, region; from the early settlements on the shore of the lake, to the growth of ports to transport goods from the markets of the East Coast to the Mississippi River and the West, to the growth of the automobile and other manufacturing businesses. The presence of high quality housing along the shoreline, along with the presence of beaches and marinas, also suggests that people in the region value the lake beyond its role in creating employment. In recent years, as global competition and economic restructuring have put pressure on the industrial sectors that led to the growth of this region, there is a renewed interest in exploring the role

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of the lake in attracting households who are value the lake amenities. Policymakers seek to understand how and if people value Lake Erie in order to determine whether economic development policies focused on the lake amenities make sense.

In advanced economies, such as the United States, there is evidence that people increasingly value quality of life (QOL) and are willing to pay for access to natural amenities (Rappaport, 2004; McGranahan, 2008; Partridge and Ali, 2008). However, due to the fact that proximity to Lake Erie was one of the drivers behind the industrialization of the Cleveland area, a complicating factor in this region is that there is pollution and environmental degradation near Lake Erie. In other contexts, there is evidence that housing values are negatively affected by proximity to environmental disamenities such as power plants, superfund sites, and air pollution (Smith and Huang, 1995; Hite et al., 2001; Davis, 2011).

This research thus considers the potential for QOL-oriented Lake Erie development in Greater Cleveland. We hypothesize that, if households value access to Lake Erie, they will be willing to pay a higher housing price, all else equal. We also will test whether industrialization has made it less desirable to live close to Lake Erie, offsetting any positive aesthetic or recreational benefits, and reducing housing prices. Using residential sales transactions data from the Greater Cleveland region, we use a hedonic price model to measure the capitalization effect on houses in proximity to Lake Erie and its recreational amenities as well as the proximity to industrialized sites or pollution. Our unique dataset includes over 300,000 housing transactions from 1992 to 2006 as well as detailed geographically-located data on industrial disamenities and industrial emissions, and natural and related built amenities, including parks, and Lake Erie and its beaches and marinas. To control for unobserved characteristics, we also include both time and spatial fixed effects.

This research contributes to the literature by providing insight into the value of lake amenities in a highly-industrialized, developed, and transitioning region. Much of the previous literature on lake amenities has focused on resort communities along the coasts and in the South. Thus, a priori, it was unclear whether and how much households in this region value these lake amenities. However, there is some limited evidence that at least high-income people in the Great Lakes region may value lake proximity (Stephens and Partridge, 2015). Additionally, we consider the different types of lake amenities — including lake views as well as access to boat ramps and beaches. And, we also consider the role of industrialization and abandoned facilities. We find strong evidence that, consistent with other studies which have found that access to lake views and being very close to water is valuable to households, housing values in immediate proximity to Lake Erie are higher. However, away from that, the results are more mixed, perhaps due to the fact that the lake amenities are within driving distance of all households in the region.

There is also evidence that households do notice industrialization and that it is reflected in a price decrease for houses near manufacturing facilities and especially for those located near the lake. However, the negative effect on price of being close to air pollution or Superfund sites is much lower, suggesting that it may be the visible nature of smokestacks that drives prices lower.

In what follows, we detail the previous literature. Next, we describe our empirical model and data; followed by the empirical results. The final section presents some concluding thoughts and a discussion about the implications for the region and policy.

8.2 Literature Review

Valuing non-market attributes of a region such as natural or built amenities or pollution or environmental disamenities using the hedonic pricing framework has a long history starting with Rosen (1974). By using a single housing market (such as a metropolitan area), the assumption is that wages are held constant in a region and that housing prices can be used to uncover the change in value associated with the various attributes of a house, including proximity to natural amenities and industrial and environmental disamenities, as consumers will maximize their utility by their choice of a house (Rosen, 1974; Taylor, 2003; Palmquist, 2006).

In an early application of the hedonic method to look at access to lake amenities, Brown Jr et al. (1977) found that property values decrease with distance from the lake. Feather et al. (1992) examined the relationship between water amenities and housing prices in Orange County, Florida, and saw evidence that the land value of lakefront property is greater than property away from a lake, and that the effect of being close to a lake diminishes with distance.

A number of other papers have also found a link between either access to lake amenities or proximity to a lake and housing values, including Lansford Jr and Jones (1995a,b), Nelson (2010), and Abbott and Klaiber (2013).Lansford Jr and Jones (1995a,b) also found that adjacency to the lake is the strongest effect and that the lake effect dies off quickly. White and Leefers (2007) looked at a rural housing market in Michigan and found that the only natural amenity that affected housing values in their study area was proximity to the major lake. Looking specifically at the Northeast Ohio Lake Erie lakefront, Bond et al. (2002) found some evidence of a willingness to pay a premium to live in housing that has a view of Lake Erie. However, they also qualify their results by noting that they fail to control for a number of important factors including access to recreational amenities and proximity to urban amenities and downtown Cleveland.

Among other natural amenities that may affect housing prices are open space and parklands. A number of previous studies have looked at the effect of open space on housing prices, including Smith et al. (2002) and Irwin (2002), among others. Results from Poudyal et al. (2009) showed that access to larger urban parks was associated with increased house values. In their study of urban areas in Finland, Tyrväinen and Miettinen (2000) found that proximity to forests increases housing sales prices, and there is a premium for being within view of those forests. Other studies have shown that the benefits from certain natural amenities (including national monuments and other public open space) extend a significant distance beyond their boundaries (Irwin, 2002; Schmidt and Courant, 2006).

In the environmental economics literature, the hedonic price method has been used to quantify the negative effects of pollution and proximity to hazardous waste sites and other environmental disamenities. Many studies have provided evidence that housing values are negatively affected by proximity to environmental disamenities such as power plants, Superfund and other hazardous waste sites, and air pollution (Kiel and McClain, 1995; Smith and Huang, 1993; Chattopadhyay, 1999; Hite et al., 2001; Beron et al., 2001; Ihlanfeldt and Taylor, 2004; Davis, 2011). In another study focused on Ohio, Brasington and Hite (2005) looked at proximity to what they call 'hazards' or sites that have been identified as possible brownfields or future Superfund sites. They found that proximity to such hazards is negatively correlated with housing prices. Smith and Huang (1993, 1995) looked at 25 years of hedonic analysis considering a link between housing prices and air quality which, in general, showed there is a positive relationship. However, a related study by Boyle and Kiel (2001) found that the values on air quality are generally statistically insignificant; perhaps due to the fact that air quality variables are correlated with both included and omitted variables. This study also provided evidence that studies which included multiple environmental variables tended to have higher significance, perhaps due to fewer omitted variables, although they suggested that further research is needed.

Looking at the relationship between toxic releases as measured by the U.S. Environmental Protection Agency (EPA) Toxic Release Inventory (TRI), Fonseca and Noonan (2006) saw almost no statistical relationship between proximity to the nearest toxic release and housing price. However, they suggested that future work may want to consider the amount of the releases, not just what is closest.

In other research, there is evidence that proximity to certain businesses or the presence of congestion may be associated with changes in housing prices. For example, proximity to industrial businesses is negatively correlated with housing prices (Franklin and Waddell, 2003). Perhaps it is that people are more likely to be concerned with smokestacks rather than with air quality or pollution, which they may not be able to observe (unless it is coming out of the previously mentioned smokestacks). Timmins and Murdock (2007) found that congestion was important when considering the willingness to pay for access to recreational amenities. And, hedonic methods have uncovered a link between the effects of traffic congestion and housing prices (Wilhelmsson, 2000; Davis, 2004).

The work by McConnell (1990) suggested that there may be multiple drivers of the price of houses close to natural amenities, including water. These factors include the view, the recreational opportunities, and environmental benefits, if the air quality is better close to the water, or environmental disamenities, if there is industrialization or more pollution near the water. And, these factors can interact: Stephens and Weinstein (2019) found that the positive impact from proximity to natural amenities can be negatively affected by energy development.

8.3 Empirical specification

Following Rosen (1974), a general hedonic model is:

$$P(z) = f(H, A, \varepsilon) \tag{8.1}$$

where, H includes housing characteristics, A are community or locational characteristics (including amenities and disamenities), and ε is an standard error term.

In specifying our empirical model, we attempt to address the concerns raised in the previous literature related to functional form and the potential omitted variable bias from missing housing and community characteristics, especially when using spatially-delineated variables (Rosen, 1974; Palmquist, 2006; Kuminoff et al., 2010; Coulson, 2012). Following Kuminoff et al. (2010), we use spatial fixed effects, and a somewhat flexible functional form, which they found was the most robust in the face of omitted variable bias.

Thus, to estimate our model we use equation 8.2:

$$\begin{aligned} ln(P_i) = \beta_0 + \Sigma_{h1}\beta_{h1}H_{ih1} + \Sigma_{h2}\beta_{h2}H_{ih2} + \Sigma_{h3}\beta_{h3}(H_{ih2})^2 + \Sigma_a\beta_a A_{ia} \\ + \beta_c C_i + \beta_{sc}(C_i)^2 + T_i + S_i + \varepsilon_i \end{aligned} \tag{8.2}$$

In equation 8.2, the vector of housing characteristics is split into two vectors, H_1 and H_2, due to the inclusion of quadratic terms of some of the variables. H_1 contains the total number of bathrooms, bedrooms, and dummy variables indicating if there is a garage or fireplace. H_2 consists of age, hundreds of square feet in the building, and lot acres. The housing characteristics in H_2 enter the equation both linearly and as quadratic terms. The distance to downtown Cleveland (C) is split from the rest of the original community characteristics vector and its square is also included in the estimation equation. The rest of the community characteristics, namely the natural amenities and industrial disamenities, are included in the final vector, A. T and S are the time (sales year) and spatial (block group) fixed effects, respectively. The year fixed effect will control for changes in the overall housing market, changes in environmental conditions in the overall region, and policy changes that would affect all houses in a given year. The block group fixed effect will control for other neighborhood or community characteristics that do not change over time, including local school quality and the quality of the neighborhood. In the next section, we further describe the data and the variables used in this analysis.

When estimating the models, we will test the sensitivity of our results to the inclusion of specific amenity and disamenity variables. We also correct for heteroskedasticity by using robust standard errors.

We also test whether the results could be pooled across the decades using the method outlined in Ohta and Griliches (1976), in which the standard error of the regression (SEE) of the pooled, full model was compared to that of the separate, unpooled models (results not shown). The results indicate that use of pooled regression of the data from all of the years is appropriate.

8.4 Data

This research focuses on six counties in the Greater Cleveland (Ohio) region, Ashtabula, Cuyahoga, Geauga, Lake, Lorain, and Medina Counties. A map of the study area is shown in Figure 8.1. This area includes the Cleveland Metropolitan Statistical Area (MSA) and the adjacent Ashtabula Micropolitan Area which, together, form a single regional labor market and, by assumption, a regional housing market. Thus, housing prices in this region can be used to uncover the marginal value of non-traded amenities and disamenities. The market is likely comprised of several overlapping local markets. However, since they are all part of a single greater metropolitan area, with connections by major roads and lack of physical features that would separate the areas, we expect significant overlap in these markets and believe the housing market is best modeled as a single regional market. This is consistent with the work by Irwin (2002) in looking at the housing market in Maryland.

8.4.1 Housing Transactions

We use sales data for single-family houses from the six-county region from 1992 to 2006 compiled from data provided by the Center for Urban and Regional Analysis (CURA) at The Ohio State University and purchased from Corelogic Data Services. The data are matched to parcel shapefiles using ArcGIS obtained from the six counties. Compared to relying on geocoding of addresses, this provides more accurate

Fig. 8.1 Map of the six-county region, Northeast Ohio

locations of the sales transactions, and increases the accuracy of our calculations of spatially-defined variables such as distance to lake amenities and environmental disamenities (as explained below).

Our dataset includes the sale price of the house and the following housing characteristics, which are likely to be predictors of house attractiveness: lot acres and hundreds of square feet in the building, total number of bathrooms, age of the house, whether the house has a garage, and whether the house has a fireplace (generally a sign of higher quality). The use of these characteristics is consistent with other hedonic studies and, based on the available data, provides the best set of controls for heterogeneous housing characteristics. House prices are normalized in 2006 dollars using the CPI for the Cleveland-Akron region in order to account for inflation. Since we need accurate information on these characteristics for all transactions in the sample, we eliminate transactions with missing or zero values for the relevant variables in the estimation. We also follow Klaiber (2008) in using approximately the 1st and 99th percentiles as the limits of the bounds of those transactions we retain. The final dataset includes 303,907 transactions.

8.4.2 Amenities

Since we are interested in proximity to Lake Erie, we use ArcGIS to measure the distance between each parcel in our transactions data and the closest point on the lake. We then construct a dummy variable that indicates whether or not the house is within 100 meters of the Lake Erie, to proxy for those houses that are adjacent to or have a view of the lake. Data on recreational amenities on Lake Erie, including beaches and boat ramps, were obtained from the Ohio Department of Natural Resources. We measure the distance from each parcel in our transactions data to the nearest Lake Erie beach and boat ramp.[1] We combine the distance to beach and boat ramp into a single variable, distance to nearest boat ramp or beach, due to the high collinearity between the two distance measures (0.986). We also construct dummy variables indicating whether or not a house is within 250 meters of a boat or beach, i.e. within close walking distance of these amenities. The number of parking spaces at the closest boat ramp is also included in the data and is interacted with the dummy variable that indicates if a boat ramp is within 250 meters, to get a measure of the number of parking spaces near a house. This will measure congestion related to the boat facilities.

Additional data on public parks and recreational lands were provided by Ohio Ducks Unlimited. We construct a measure of the number of park acres (in tens of acres) within one kilometer of the parcel as a proxy for other natural amenities in the neighborhood of a house.

We recognize that the recreational amenities could be endogenous. Unfortunately, detailed data on the actual construction date for each facility was not available. However, most of the public recreational infrastructure in the region was constructed before 1990, minimizing this endogeneity.

To control for proximity to urban amenities and business opportunities, we include a measure of the distance to downtown Cleveland. Downtown Cleveland contains or is near many of the urban amenities in the region including the sports stadiums for the major professional sports teams, as well as museums and restaurants. Additionally, classical urban growth theory would suggest that housing prices are (negatively) correlated with distance to downtown Cleveland, assuming that workers are generally commuting there (Muth, 1961; Alonso, 1964; Mills, 1967).

8.4.3 Disamenities

Data on businesses, by location, in the region are available by two-digit NAICS code, by year. These data are used to measure the number of manufacturing businesses, defined as businesses with NAICS code 31, 32, or 33, within one kilometer of each parcel. Since these manufacturing businesses could be heterogeneous in terms of building size and structure, we also tested several other specifications and found similar results. We hypothesize that households are more likely to notice ugly buildings or 'smokestacks,' thus the presence of a manufacturing facility in close proximity to a house will be likely to be associated with lower housing prices. To control for endogeneity, we include one-year lagged values of the numbers of businesses; for example, for a transaction in the year 2002, we use the number of nearby manufacturing businesses in 2001.

To control for other local industrial disamenities, we utilize U.S. EPA toxic release inventory (TRI) data on aggregate toxic air releases, by company, by year. We

[1] Because of available data, all distance measures here are straightline distances, not driving distances. Given the extensive road network in this highly urbanized region, straightline distances should approximate driving distances. However, we recognize that there could be some differences not controlled for here.

then use ArcGIS to measures the distance between each parcel in our transactions data to the releasing companies. To avoid counting small releases that may not be obvious to households we only consider companies whose annual air toxic releases total over 1000 pounds. We then aggregate the toxic releases (in tens of thousands of pounds) within one kilometer of a parcel and create a measure of the amount of toxic releases over 5000 pounds per year. We also tested other cut-offs, but the results were similar. For the TRI data, we also lag the values by one year.[2]

To control for proximity to abandoned hazardous industrial sites, we measure the distance to the nearest Superfund site in a particular year, using data from the U.S. EPA. Then, we created a dummy variable that indicates whether or not the parcel is located within one kilometer of a Superfund site and include that measure in our models.

Table 8.1 contains summary statistics for the data used in our analysis.

Table 8.1 Descriptive statistics, Northeast Ohio housing transactions

Variable	Mean	St. Dev.	Min	Max
Log sales price in 2006 dollars	11.51	0.585	8.876	14.28
Price in 2006 dollars	118,178	76,964	7,159	1,587,000
Number of bedrooms	3.159	0.716	1	8
Number of bathrooms	1.615	0.694	1	6
House has a garage (0/1)	0.831	0.375	0	1
House has a fireplace (0/1)	0.234	0.423	0	1
House size (hundreds of square feet)	16.75	6.650	6	45
Lot size in acres	0.438	0.847	0.0500	15
Age of house	44.61	27.55	0	130
Distance to downtown Cleveland (km)	24.37	16.92	0.862	107.2
Distance to Lake Erie (km)	8.461	8.271	0.000106	51.69
Distance to nearest beach or boat access (km)	10.13	8.888	0.0300	55.53
Acreage of parks within 1 km (tens of acres)	1.059	4.212	0	121.8
Number of manufacturing businesses within 1 km	1.680	1.121	0	3
TRI air releases within 1 km (in 10,000s of pounds)	0.856	9.572	0	634.7

Notes: Number of sales within 100 meters of Lake Erie = 2,779. Number of sales with 250 meters of a beach of boat ramp = 769. Number of sales within 1 km of a Superfund site = 348. Number of sales within 1 km of a manufacturing business = 240,362. Number of sales within 1 km of TRI releases = 27,410. Total number of Observations = 303,907. Number of block groups = 1,938.

8.5 Results and discussion

The results of our hedonic analysis are presented in Tables 8.2 and 8.3. In all models, the core housing characteristics are statistically significant and of the expected signs. The number of bathrooms, the number of bedrooms, the presence of a garage or fireplace, the building size and the lot size all are associated with an increase in the price of the house. Also, as shown by the squared terms for building size and lot acres, the residential housing price is increasing at a decreasing rate in terms of the size of the house and the size of the lot. Additionally, as expected, older houses,

[2] We recognize that the lag of one year may not fully account for the potential endogeneity of our emissions and business variables. However, due to data availability, further lags would have required us to drop many of the observations in our dataset.

Table 8.2 Northeast Ohio hedonic basic regression results

Variable	Model (1)	Model (2)	Model (3)
Number of bedrooms	0.019***	0.019***	0.019***
	(0.001)	(0.001)	(0.001)
Number of bathrooms	0.077***	0.077***	0.077***
	(0.001)	(0.001)	(0.001)
House has a garage (0/1)	0.061***	0.061***	0.061***
	(0.002)	(0.002)	(0.002)
House has a fireplace (0/1)	0.027***	0.027***	0.027***
	(0.001)	(0.001)	(0.001)
House size (hundreds of square feet)	0.023***	0.023***	0.023***
	(0.000)	(0.000)	(0.000)
House size, squared	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Lot size in acres	0.078***	0.078***	0.078***
	(0.003)	(0.003)	(0.003)
Lot size, squared	-0.005***	-0.005***	-0.005***
	(0.000)	(0.000)	(0.000)
Age of house	-0.007***	-0.007***	-0.007***
	(0.000)	(0.000)	(0.000)
Age squared	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Distance to downtown Cleveland (km)	-0.005**	-0.004*	-0.004*
	(0.002)	(0.002)	(0.002)
Distance to downtown Cleveland, squared	-0.000*	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)
Distance to Lake Erie (km)	0.002	0.006**	0.006**
	(0.001)	(0.002)	(0.002)
Lake Erie within 100 meters (0/1)	0.212***	0.213***	0.214***
	(0.009)	(0.009)	(0.009)
Distance to nearest beach or boat access (km)		-0.005**	-0.005*
		(0.002)	(0.002)
Beach within 250 meters (0/1)	-0.034**	-0.035**	-0.035**
	(0.016)	(0.016)	(0.016)
Boat access with 250 meters (0/1)	0.050**	0.048**	0.012
	(0.024)	(0.023)	(0.026)
Number of parking spots at nearby boating access			0.003***
			(0.001)
Acreage of parks within 1 km (tens of acres)	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Number of manufacturing businesses within 1 km	-0.006***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)
TRI air releases within 1 km (in 10,000s of pounds)	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)
Superfund site within 1 km (0/1)	-0.016	-0.016	-0.017
	(0.021)	(0.021)	(0.021)
Adjusted R-squared	0.815	0.815	0.815

Note: Robust standard errors in parentheses. *** p¡0.01, ** p¡0.05, * p¡0.1. Constant included but not reported for space. All regressions include year and block group fixed effects. Number of observations = 303,858 in all models.

on average, have lower prices, however, the quadratic term is positive, suggesting that the price is decreasing at a decreasing rate (as expected). As predicted by the urban bid rent model, housing prices are decreasing with the distance to downtown Cleveland.

Focusing on the lake amenities, we find strong evidence that being on or very close to Lake Erie (within 100 meters) is significantly positively capitalized into housing prices, and this is consistent across all models. Using the results from model 3, being within 100 meters of the lake is associated with a 21.4% increase in home value. Based on the average home value in our sample, this is more than a $25,000 increase in home value. It could be that this value is picking up characteristics of the lakefront homes that are not included in our analysis, given that houses on the water may be more luxurious, have Lake Erie piers, private beaches or foundations.[3] However, discussions with local leaders confirmed that similar houses do sell for more simply by being on the lake.

Table 8.3 Northeast Ohio hedonic additional regression results

Variable	Model (4)	Model (5)	Model (6)	Model (7)
Number of bedrooms	0.019***	0.019***	0.019***	0.018***
	(0.001)	(0.001)	(0.001)	(0.001)
Number of bathrooms	0.077***	0.077***	0.077***	0.077***
	(0.001)	(0.001)	(0.001)	(0.002)
House has a garage (0/1)	0.061***	0.061***	0.061***	0.050***
	(0.002)	(0.002)	(0.002)	(0.002)
House has a fireplace (0/1)	0.026***	0.026***	0.026***	0.030***
	(0.001)	(0.001)	(0.001)	(0.002)
House size (hundreds of square feet)	0.023***	0.023***	0.023***	0.025***
	(0.000)	(0.000)	(0.000)	(0.001)
House size, squared	-0.000	-0.000*	-0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Lot size in acres	0.078***	0.078***	0.078***	0.081***
	(0.003)	(0.003)	(0.003)	(0.003)
Lot size, squared	-0.005***	-0.005***	-0.005***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
Age of house	-0.007***	-0.007***	-0.007***	-0.007***
	(0.000)	(0.000)	(0.000)	(0.000)
Age squared	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Distance to downtown Cleveland (km)	-0.004*	-0.004*	-0.004*	-0.004*
	(0.002)	(0.002)	(0.002)	(0.003)
Distance to downtown Cleveland, squared	-0.000**	-0.000**	-0.000**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Distance to Lake Erie (km)	0.008***	0.008***	0.008***	0.006**
	(0.002)	(0.002)	(0.002)	(0.003)
Lake Erie within 100 meters (0/1)	0.233***	0.281***	0.233***	1.296***
	(0.009)	(0.013)	(0.009)	(0.155)
Lake Erie within 100 and 250 meters (0/1)	0.043***	0.043***	0.043***	
	(0.005)	(0.005)	(0.005)	
Distance to nearest beach or boat access (km)	-0.007***	-0.007***	-0.007***	-0.005*
	(0.002)	(0.002)	(0.002)	(0.003)
Beach within 250 meters (0/1)	-0.048***	-0.048***	-0.048***	-0.049***
	(0.016)	(0.016)	(0.016)	(0.017)
Boat access with 250 meters (0/1)	0.001	0.009	0.001	-0.053**
	(0.026)	(0.026)	(0.026)	(0.026)
Number of parking spots at nearby boating access	0.003***	0.003***	0.003***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Acreage of parks within 1 km (tens of acres)	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Number of manufacturing businesses within 1 km	-0.006***	-0.005***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
TRI air releases within 1 km (in 10,000s of pounds)	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Superfund site within 1 km (0/1)	-0.015	-0.015	-0.016	-0.020
	(0.021)	(0.021)	(0.021)	(0.021)
Number of manuf. bus. within 1 km if on the coast		-0.037***		
		(0.007)		
TRI air releases within 1 km if on the coast			-0.000	
			(0.000)	
Elevation if along the coast (DEM)				-0.002***
				(0.000)
Adjusted R-squared	0.815	0.815	0.815	0.811

Note: Robust standard errors in parentheses. *** p¡0.01, ** p¡0.05, * p¡0.1. Constant included but not reported for space. All regressions include year and block group fixed effects. Number of observations = 303,858 for models 4-5, 205,166 for Model 7.

In models 4-6 in Table 8.3, we also include a control for houses slightly farther away (100 to 250 meters) and there is a smaller (approximately 4%), yet statistically

[3] Looking at Google Maps, we saw that houses near Lake Erie seem very different than those only a short distance away.

significant, capitalization effect. This suggests that the lake effects may extend at least a few blocks. However, away from those homes immediately located on or near to Lake Erie, our results indicate there is at least a small premium on housing prices from being farther away, as demonstrated by the positive and statistically significant coefficient on the distance to Lake Erie variable in most models (even when our control for those slightly farther away is included). For example, houses prices are approximately 6-8% higher (or about $7,000 to $9,000 higher) if they are 10 kilometers away. The overall negative relationship between proximity and price may be evidence that people like living in the Greater Cleveland area because they can access Lake Erie when they want to enjoy its recreational and other benefits but that, unless they live directly on the lake (or within walking distance), they are not willing to pay to be close.

We further explore this by considering the proximity to beach and boat access. In this case, there is an offsetting, but small, negative capitalization effect from living farther from one of these access points, suggesting that people do not want to live too far away. Yet, the negative and statistically significant price effect (3-4%) from living within 250 meters of a beach, indicates that people would rather not live too close to the congestion of a beach. Interestingly, while models 1 and 2 point to positive price effects from being close (within 250 meters) to a boat access, it appears that it is boat access with parking that is the most valuable. This may be because boat owners (even those who live nearby) will drive to the boat access and want to be able to park their car after launching their boat. Initially, we had thought that the number of parking spaces could be seen as a measure of congestion, which would likely make it less desirable (all else equal) to have a house nearby. But, in addition to the convenience of parking at the boat ramp, it is also likely that there are additional amenities associated with boat ramps with lots of parking spaces that are not controlled for by our model; in other words, boat ramps with lots of parking spaces might also be nicer boat ramps.

We also find a small, but positive and statistically significant, capitalization effect from proximity to parks, consistent with other studies which have looked at a variety of open space types. This is further evidence that households in the region value proximity to natural and recreational amenities. There is some concern that industrialization may be reducing the attractiveness of portions of the region. Consistent with these hypotheses, we find evidence that proximity to manufacturing businesses is negatively related to housing prices. In models 5 and 6, there is also evidence of an additional negative impact on house prices near Lake Erie from nearby manufacturing facilities, suggesting that industrialization near the lake could drive down prices more than similar industrialization farther away from the lake. Based on these results, for the average house, prices are 4.2% lower for each manufacturing facility near houses on Lake Erie, reducing prices by about $5,000.

The industrial legacy has also left abandoned hazardous waste sites, including Superfund sites, and those manufacturing businesses and other industrial sites that create air pollution that is tracked by the TRI. However, we observe a statistically insignificant relationship between proximity to a Superfund site and home price, perhaps due to the use of our spatial fixed effects. And, the coefficient on nearby TRI air releases is very small, although it is negative and statistically significant. In model 6, we explore whether air pollution near the coast has a bigger negative impact (as we found for manufacturing). In this case, the coefficient is statistically insignificant.

8.5.1 Sensitivity analysis

Finally, on inspection of the housing along the Lake, it appears that much of the high-end private development is in areas with cliffs rather than beach access. To test for this, we considered a measure of elevation using Digital Elevation Model (DEM) data in ArcGIS for those houses within 100 meters of the lake, results are in model 7. As shown, the relationship was negative but small (a reduction in price of about 0.2%) compared to the large capitalization effect from being next to the lake. This suggests that the key effect is from being on or close to the lake, not whether there is a beach or cliff.

8.6 Conclusion

Results from this analysis suggest that households in the Greater Cleveland region value being on or close to Lake Erie, at least if they are close enough to have a view or within walking distance. Beyond that, the results are more mixed. However, away from those houses in close proximity to Lake Erie, the lake has little to no effect on housing values, perhaps because everyone in the region can benefit from the amenities being within driving distance.

Consistent with our hypothesis that industrial disamenities may play a role in repelling households, we find evidence of a negative effect on house prices from nearby manufacturing facilities, especially when houses are close to the lake. However, the effect is much smaller when we consider air pollution levels. Consistent with previous research, this may be due to the fact that households are able to see the smokestacks or industrial sites but may not necessarily notice pollution otherwise.

Overall, our findings suggest that, even in a highly industrialized region, natural and related built amenities are valued by households and increase their quality of life. As the region transitions from its historic reliance on heavy industry and manufacturing, it appears that investments in environmental quality and increased recreational facilities, especially boat access, would be valued by residents. Additionally, as there is a large capitalization effect from being directly next to the lake, perhaps due to the view, efforts to clean up past industrial sites could pay off.

While these results are based on the Greater Cleveland region, they should provide insight into the value of natural amenities and industrial disamenities in other cities and regions located on coastlines or with other natural amenities who have an industrial legacy. It appears that even in the face of that legacy, policies to enhance quality of life enhancing amenities are worth pursuing.

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Chapter 9
The Impact of Controlled Choice School Assignment Policy on Housing Prices: Evidence from Rockford, Illinois

Tammy Batson

Abstract In 1996, in response to a court order, the Rockford School District implemented a Controlled Choice school assignment policy to desegregate their school district. This paper investigates the impact of the Controlled Choice policy on the housing prices within the city of Rockford. The extent to which the loss of certainty in the school assignment process impacted housing prices within three policy zones is examined using a hedonic model and data from 1993 to 2000. Once the quality of the previously assured neighborhood school is added the results show that formerly high quality schools lost an average of 9.3 percent of their value whereas homes previously assigned to poor quality schools saw increases in values.

9.1 Introduction

For many people who are deciding to buy a home, the quality and proximity of local schools is an important consideration. The theoretical framework supporting this observation comes from the Tiebout (1956) model in which homeowners incorporate the quality of local public services into their decision when purchasing a residential property. In the example of Rockford, traditional neighborhood school assignment is replaced by a Controlled Choice assignment policy following a court order after a desegregation lawsuit. Under this new policy, parents submitted a list of preferred schools, which could include their neighborhood school. However, there is no guarantee that they would receive their first choice. The inability of school districts to provide everyone with their first choice left many homeowners uncertain of school assignment and, therefore, unable to incorporate the quality of this local public good into the value of their property. Many economists argue that changes to school choice programs can be an informative source of changes in housing markets (Bogart and Cromwell, 1997; Brennan and Buchanan, 1980; Hoxby, 2000; Oates, 1969, 1985). Using data from Rockford, Illinois housing transactions from 1993–2000, this paper investigates the connection between Controlled Choice and the changes in the housing market.

Schools are an important component of the bundle of public services in a locality, and because of this, the relationship between public schools and housing prices

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has attracted considerable interest from researchers. The hedonic methodology used by Rosen (1974) gathers cross-sectional data on neighborhood characteristics to examine how taxes or public services are capitalized into the value of the house (Black, 1999; Hayes et al., 1996; Reback, 2005). In Reback's (2005) work, a school choice program that allowed for inter-district choice in Minnesota is reviewed. Reback found houses appreciated significantly when students are given a choice to attend a "preferred school," but values declined in districts that accepted these transferred students. Some researchers have focused on housing data in which properties are sold more than once over an observed time period to observe the link between schools and housing prices (Bogart and Cromwell, 2000; Downes and Zabel, 2002; Figlio and Lucas, 2004). Figlio and Lucas found that, in the Florida School System, once school quality measures are made available to the public, this information has a significant effect on housing prices. Housing prices are about 6% higher on average in areas where the local elementary schools received an "A" rating than prices of houses where local schools received a "B" rating. What is attractive about the repeat sales approach is that it allows analysts to ignore the fixed elements that determine home prices, limits missing variable bias, and allows analysts to focus on the policy change.

Results of this chapter show that home buyers under Controlled Choice have evaluated this policy differently based on their proximity to a school, its historical quality, and the zone assigned to residents by the district. Since giving all students the school of their choice may conflict with racial guidelines, the certainty of school assignment is difficult for many residents to accurately evaluate. Therefore, the capitalization effects on these houses decrease due to the uncertainty in school assignment. Using repeat sales, homeowners for whom the assignment policy changed during ownership saw prices fall an average of 5.59%. In addition, when home prices are reviewed introducing a spatial dummy variable indicating the zone each home is assigned to, the policy impact ranges from a 1.49% to 9.19% decrease, depending on the zone. Finally, these interactions are included along with a new variable introduced to represent former school quality based on local scores relative to the state average. This variable for school quality becomes very important; not only is it significant at 1%, but all three policy-created zone variables appear to be significant factors in falling home prices. Houses located near formerly well-performing schools lost, on average, 9.3% after the policy change. This is an indication that properties formerly included a premium to neighborhood schools of an average of 2.9%, if the school performed at least one standard deviation above the district average. In contrast, some houses that located next to schools with lower school quality had discounts capitalized into current values. Formerly poor-performing schools under neighborhood assignment combined with the zones result in some positive impacts to houses located close to schools performing two standard deviations lower than the district average. This chapter is devoted to the examination of a school district ordered to desegregate its schools and how the element of uncertainty, as well as the limiting of selection based on residency, caused capitalization effects. These results emphasize that under Controlled Choice the removal of certainty in school assignment make potential home buyers unable to evaluate school quality.

9.2 Background

Rockford, Illinois is located about 70 miles west of Chicago in Winnebago County. Rockford is the second-largest city in the state of Illinois with approximately 150,115 residents according to the 2000 census. The Rock River acts as a natural divider of the city: the central city's older housing and neighborhoods are located west of the river, while a significant amount of recent growth has occurred on the east side of the river. This geographical division has also led to some controversy between the Westside and the Eastside residents. This controversy is often compounded by the single school district of approximately 27,000 students that serves the entire central city.

Rockford School District #205 (RSD) is one of many districts that have dealt with the challenge of desegregating their public schools after Brown v. Board of Education of Topeka (347 US 483) in 1954. The United States, for years, attempted to integrate students with a form of school choice referred to as "freedom of choice." This, however, failed to attract African American students to white schools or white students to African American schools. In 1976, a federal judge "ordered the Department of Health, Education and Welfare (HEW) to begin enforcement proceedings against certain Northern and Western school districts for violations of school desegregation requirements" (Times, 1976, p. 13) to increase efforts to desegregate schools. RSD was included in the list of school districts required to take action to desegregate the district. Along with many areas in the country, a number of initiatives were introduced in Rockford to bring about desegregation including changes in school boundaries, magnet schools, and school bussing. The original results show that in Rockford, by 1980, the number of elementary students attending racially identifiable schools had declined. Following these improvements, and with similar results occurring all across the country, many school districts included in the 1976 ruling were released from oversight. By 1981, RSD was dismissed from its desegregation lawsuits.

By the late 1980s and early 1990s, with forced busing on the decline across the country, the schools within the RSD began to look more like the neighborhoods that surrounded the schools and therefore, showed signs of residential segregation. No longer under the watchful oversight of the federal government, and in an attempt to ease budget problems, the RSD subsequently made plans to close 10 schools located on the west side of town which served primarily minority students. Local residents, who had followed an ongoing Eastside/Westside of the river debate for years, were already exasperated when the district decided to close a large Westside high school in 1988. The decision one year later to close 10 elementary schools and turn the aforementioned high school into a super elementary school, known as Together Toward a Brighter Tomorrow, attracted considerable controversy. This move by the RSD seemed to show a disregard for the earlier warnings they had received from HEW and resulted in some Rockford parents creating a coalition known as People Who Care.

In 1989, in response to the proposed closings under Together Toward a Brighter Tomorrow, the People Who Care sued the district over the proposal on behalf of African American and Hispanic students. The court found the school district guilty of systematic discrimination against African American and Hispanic students, and the closings were stopped (Taylor and Alves, 1999). United States Magistrate Judge P. Michael Mahoney directed the district to make a number of improvements. First, the school district was directed to close the achievement gap between white and minority students by 50%. Second, it was directed to meet strict racial quotas in courses across the curriculum.[1] The Court Remedial Order gave the district full discretion in how to achieve these directives. In late 1996, the school board approved a system known as Controlled Choice to facilitate the desegregation process for the district.

Controlled Choice is a system that attempts to provide open choice while still maintaining ethnic and racial integration. The system replaces neighborhood attendance districts with zones and allows families to choose schools according to their zone. Alves and Willie (1990), the creators of the system, state that the policy allows for parental choice so long as that choice does not upset the racial and ethnic balance at the school in question. Only a few cities have adopted Controlled Choice, with most programs patterned after a pilot program implementation in Cambridge, Massachusetts. The Cambridge plan developed into the following system: New families would visit a central registration area where they would choose four schools and rank them in order of preference. The district would then review the lists and every effort would be made to assign students to one of their top choices while still ensuring all schools reflected the district's racial and ethnic composition without exceeding capacity (Tan, 1990). Though not every student would be able to attend their first choice, the policy succeeded in striking a balance between providing students' families with some choice while ensuring school improvement overall (Yanofsky and Young, 1992). At the beginning of the 1997/98 school year, the RSD had been divided into three zones: Northeast, Southeast, and West.

Students are no longer assigned to neighborhood schools, but instead, parents are asked to submit a list of preferred schools based on the new attendance zones. Families in the West Zone, where the minority population is concentrated, are allowed to choose schools from any of the three zones. Families in the Southeast and Northeast Zones are asked to choose from two zones, their own zone or the West Zone. The Racial Fairness Guidelines used for assignment are determined according to the court's remedial order (CRO). These guidelines defined racial balance as being when the minority enrollment is plus or minus 15% of the minority student population of the district. Minority students in this court order are defined as African American and Hispanic, with all other groups considered part of the majority in terms of student assignment.

Under the former neighborhood school assignment, parents knew exactly what school their children would attend based on where they purchased their house. After Controlled Choice is implemented, it creates uncertainty even though the majority of students received their first-choice school. Research conducted by Taylor and Alves (2000) found that in 1999, only 24% of all Kindergarten students did not receive their first choice, while 8% of students did not receive one of their top three choices. Prior to Controlled Choice, parents had certainty over the school their child would attend once they purchased a home. After Controlled Choice, there was considerable uncertainty, in addition to many many students now faced increased travel time to and from school as well as to after-school activities because many students are assigned to schools located a significant distance from their residence. Parents who were unhappy with the assignment for their children sorted themselves out of the district as predicted by Tiebout (1956). This should change the demand to live in different neighborhoods in Rockford. Families living in the West Zone have a wider range of choice, while families living in the Northeast and Southeast Zones are lim-

[1] These guidelines defined racial balance as being when the minority enrollment is plus or minus 15% of the minority student population of the district.

ited to choosing among schools located within their zones or the West Zone.[2] This suggests there will be a higher preference for families residing in the West Zone because residents are offered a larger choice of schools. This should create a premium for residing in the West Zone implying that, everything else being equal, the value of homes in the West Zone would increase. Instead of parents finding the West Zone more attractive because of its higher quality, they demand housing in the West Zone due to its larger choice set compared to the other zones. As a result, the remaining zones would theoretically see little-to-no premium in housing price after the implementation of Controlled Choice or a decline due to the increased uncertainty and consequent reduced demand.

9.3 Methodology

The hedonic method is often difficult to estimate, based on constraints of the data sample. Therefore, the repeat sales regression is a special case of the hedonic method where one can create a hedonic price function to estimate implicit prices of characteristics of residential property when data are limited. Therefore, by focusing the analysis on repeat sales, these unique comparables will allow for comparing the value of a property to itself over time. This allows for the estimation of what a consumer is willing to pay for a good whose attributes have changed during the sample and should help track the changing value given to this public good. This analysis follows Bogart and Cromwell (2000), who, using several techniques including repeat sales, estimated the changes in prices due to new school assignment for some residents after redistricting of the neighborhood schools. Properties are assigned dichotomous variables based on whether or not they are considered part of the treatment group sold after November 1996, following the policy implementation.

The typical hedonic estimation regresses the value of a property against a set of variables controlling for characteristics, location, and other factors that may affect price:

$$ln(P_{it}) = P[Z_{it}, L_{it}, D_{it}] \tag{9.1}$$

Here, P is the residential property of i value at time t, and Z_{it} is a vector of physical attributes of property i at time t, such as the number of rooms, the lot size, and whether or not the unit is attached. The location of the property, L_{it}, describes the location of property i value at time t, with D_{it} describing the dummy variable for the policy i at time t. This function is based on the basic assumption that the price of a composite good is a linear function of the prices of its characteristics. As Palmquist (1984) showed, the repeat sales technique allows for a considerably simplified model specification since many of the characteristics do not change over time and are omitted for estimation. Repeat sales measures, initially proposed by Bailey et al. (1963), provide an alternative estimation method based on price changes of houses sold more than once. This model is frequently used to create price indices, a technique that has been refined by Palmquist (1984) and Case and Shiller (1989). In particular, if the restricted hedonic model in Equation 9.1 is differenced with respect

[2] The rationale for this requirement is that the students from West Zone schools are mainly from minority groups while the student bodies at Northeast and Southeast Zone schools are primarily from non-minority groups.

to consecutive sales of houses that have sold more than once in the sample period; it follows that

$$lnP_{t+1}[Z_{it}, L_{it}, D_{it}] - lnP_t[Z_{it}, L_{it}, D_{it}] = lnP[\Delta D_{PC}] \tag{9.2}$$

What remains is a subset of variables observed to be those that change between sales. The assumption in which relevant elements of Z and L are constant between sales is supported since the time between sales is sufficiently short. With the creation of dummy variables to indicate D_{PC}=1 if a policy change occurred between sales and D_{PC}=0 for those houses otherwise as the same policy is in effect for both sales. This clearly exogenous policy impact makes the use of repeat sales a very attractive option. Equation 9.2 is then estimated as

$$\begin{aligned} lnP_{t+1} - lnP_t &= b_2 D_{PC} + e \\ &= b_1 + b_2 D_{PC} \text{ if } D_{PC} = 1 \\ &= b_1 \text{ if } D_{PC} = 0 \end{aligned} \tag{9.3}$$

The estimated coefficients are then transformed using the technique created by Halvorsen et al. (1980) and Kennedy et al. (1981) to interpret dummy variables that are regressed against the natural log of the dependant variable as shown in Equation 9.4:

$$b_i = 100(exp(b_i - V(b_i)/2) - 1) \tag{9.4}$$

Next, a matrix of dummy variables, denoted as D_{ZonePC}, is created to indicate the Controlled Choice policy zone assigned to each household. It is assumed that these zones are created arbitrarily as they are bounded by the Rock River and Broadway, a road that bisects the north and south ends of Rockford. The study tests to determine whether the impact to the district is experienced uniformly across the different zones by estimating equation (2). Since this policy ties residential locations (L_i) to school choice zones, with some zones having more choices than others, this policy effect needs to be interacted with the different zones. Estimation of a model that fails to account for the potential interactions among the independent variables may not provide an accurate estimation of the true relationship between the dependent and independent variables. Friedrich (1982) argues it is a low-risk strategy to determine whether the product term is significant. If it is significant, it is kept in the model; otherwise, an analyst can drop the product term out of the model. The inclusion of an interaction term is nonadditive; therefore, the effect of one set of dummy variables on the dependent variable varies according to the value of a second set of dummy variables.

The second set of regressions can then be run as shown in Equation 9.5:

$$lnP_{t+1} - lnP_t = f(b^*) = b_1 + b_2 d_{PC} + b_3 D_{ZonePC} + e \tag{9.5}$$

Finally, a crude measure of former school quality of the neighborhood school previously assigned to each home is added. This may help to capture the response by homeowners who had previously paid a premium or discount to be located near a high or low quality school.

$$lnP_{t+1} - lnP_t = f(b^*) = b_1 + b_2 d_{PC} + b_3 D_{ZonePC} + b_4 SQ + e \tag{9.6}$$

One possible critique of the repeat sales method is its failure to control for temporal market trends. Toward this end, the above equations estimates using a fixed effects methodology as a check of robustness. Omitted variable bias occurs when a study lacks crucial information primarily due to data set constraints (Leventhal and Brooks-Gunn, 2000). Thus, the fixed effects model allows one to control for variables that are unobserved and are more permanent or the same across houses. In this case, the fixed effects model is used as a test for consistency. The fixed effects model measures differences in intercepts for each group and allows a researcher to include time dummies to control specifically for annual market fluctuations, while the repeat sales approach does not.

$$lnP_{it} = b_{it} + b_{it}(PC * Zone) + \gamma_2 D_y 2_i + ... + \gamma_n Dyn_i + e_{it} \quad (9.7)$$

This new indicator $Dy2_i$=1 for sales occurring within the second year, and 0 if otherwise, and $Dy3_i$= ..., etc. for all years in the sample. These binary regressors for each year help account for bias that may show up in the estimated coefficients within the data.[3]

9.4 Data

The Winnebago Township House Sales is used to determine the number of houses sold in Rockford and for what amounts. The core housing price data from Winnebago Township House Sales for the years 1993 to 2000 include the following: the sale price every time a property is sold, households' assessed values, tax codes, neighborhood codes, addresses, and an assortment of other house characteristics. The data, which consist of a little over 18,000 records, yield a mean house price of $93,315, a standard deviation of $55,525, and a median house price of $82,227. These house prices are adjusted to 2000 values using the consumer price index not seasonally adjusted (NSA) calculated by the United States Bureau of Labor Statistics. The policy variable splits the data among the houses that sold before the policy change in November 1996 and those sold after.

Table 9.1 Descriptive statistics for houses in data set, before and after Controlled Choice

	Houses sold before Nov. 1996	Houses sold after Nov. 1996
Mean Price	$95,947.50	$90,655.10
Median Price	$83,951.42	$80,594.09
Standard Deviation	$56,344.60	$48,215.95

Notes: The Controlled Choice policy begins in Nov. 1996. Data is Winnebago County housing data from 1993-2000.

Table 9.1 shows summary statistics for housing sale prices before and after Controlled Choice. The county database also includes parcel identifiers, making it possible to match sales of the same house over time. The data refocused contain only repeat sales occurring in the RSD and only houses that sold twice within a minimum of 12 months between sales, and for greater than the NSA-Adjusted price of

[3] The summary statistics for the characteristics are not shown due to the use of the repeat sales model.

$20,000. The resulting repeat sales data are 2,540 paired observations that contain 2,362 individual properties.[4]

Table 9.2 Dummy variable definitions and observations

Variable	No. of obs.
Repeat sales data sample size	2,540
Houses sold twice with former neighborhood assignment, prior to change in assignment policy-neighborhood assignment policy (NAP)	455
Houses sold twice once prior to change in assignment policy and once after-neighborhood assignment policy changed to controlled choice assignment policy (NAPCCAP)	1,741
Houses sold twice after change in assignment policy-controlled choice assignment policy (CCAP)	344

This repeat sales data set is divided into the school assignment policy that is in place when the home is originally purchased (first sale) and sold (second sale). Table 9.2 shows the size of each group. Of the 2,540 pairs, 18% are bought and sold under the neighborhood school assignment policy, 69% are bought under neighborhood assignment and sold under Controlled Choice, and the remaining 13% bought and sold under Controlled Choice.

Table 9.3 Summary statistics for each zone

Zone	Obs	Mean	Std. Dev.	Min	Max
West Zone (W)	599	71,480	37,922	20,230	477,173
Southeast Zone (S)	624	84,452	33,157	20,531	264,885
Northeast Zone (N)	1,317	125,627	64,426	21,502	581,480

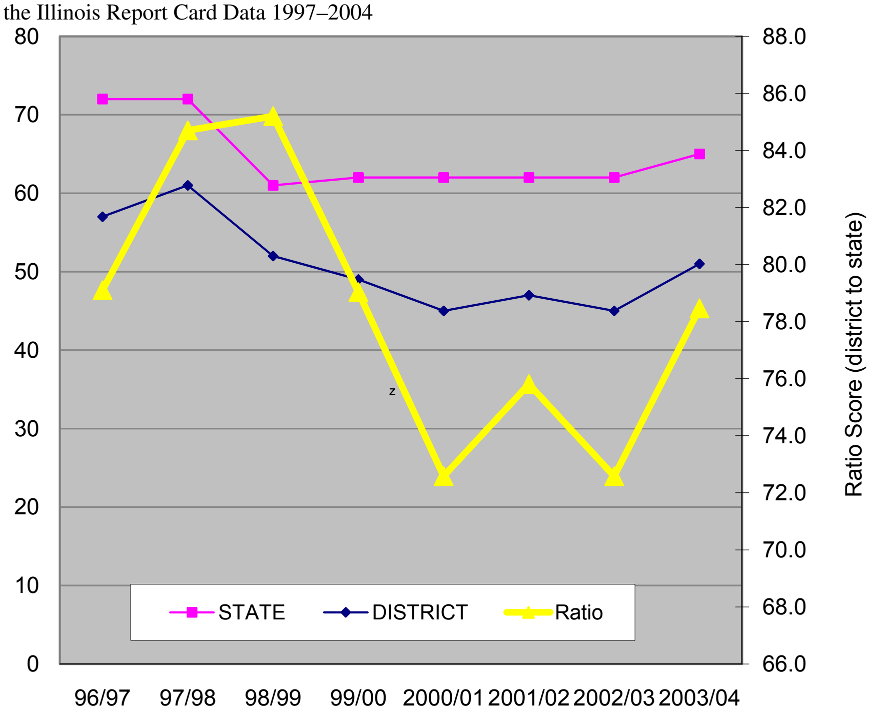
Note: Winnebago County Housing Data in 2000 dollars.

Next, each home is assigned to its Controlled Choice zone according to its location with the use of ArcGIS® (an integrated collection of Geographical Information System software). Table9.3 indicates the amount of observations available for each zone as well as some differences in the mean value of housing in each zone. Analysis of repeat sales data allows one to better control for housing characteristics.

Finally, the former school attendance boundaries are attached to the repeat sales observations and merged with the Illinois Report Card data. Figure 9.1 presents the RSD mean meet and exceed ratio during the Controlled Choice policy and the years following. The 3rd grade reading scores in 2000/2001 reflect the first class using the Controlled Choice assignment criteria as students apply for choice as they enter kindergarten, the 6th grade, and the 9th grade. During the first 2 years, grades improve. This may indicate that the desegregation policy has had a positive effect on those students who chose to change out of their neighborhood schools. However, despite the public support for school choice policies, it is possible that the decreases experienced are a result due to the sort and may prevent these policies from working as intended.

[4] Properties sold in less than 12 months and for less than $20,000 are omitted because repeat sales measures are estimated on the premise that house characteristics (that is, quality) have not changed over time. Given the potential for renovation or other reasons for homes selling below this threshold, these two restrictions are put in place. This is similar to Figlio and Lucas (2004) who eliminate all sales of less than $10,000.

Fig. 9.1 School quality measure of meet and exceed variable for third grade reading scores from the Illinois Report Card Data 1997–2004

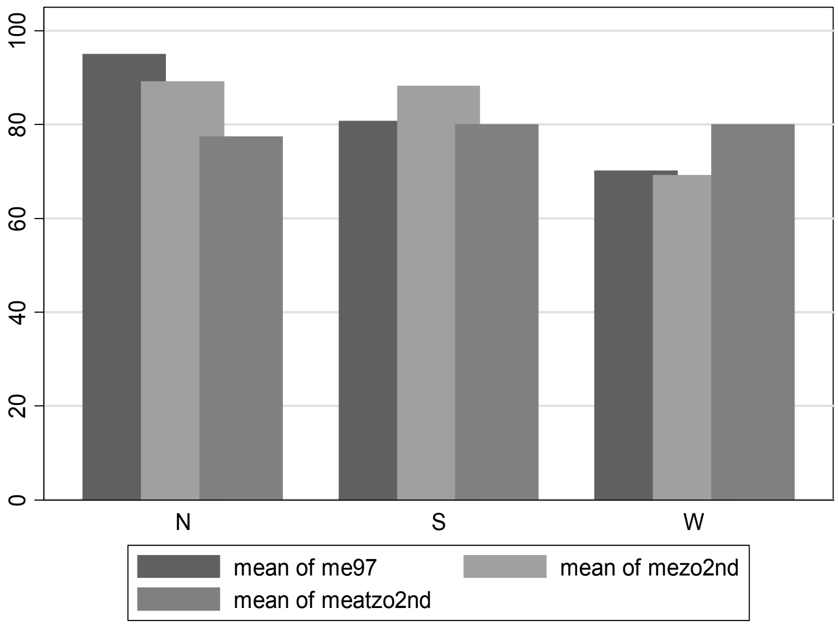


Note: The ratio score in yellow are the mean results of the district level variable used for meet and exceed 1996–1997 (*me*97), meet and exceed of the zone at the time of the second sale (*mezo*2*nd*), and meet and exceed of the attendance zone at the time of the second sale (*meatzo*2*nd*).

Assessing the impact of a school choice policy on the quality of education is not the main intention of this research. However, what is tested is how houses previously assigned to high quality schools are impacted under Controlled Choice, based on the expectation that houses formerly attached to high quality schools will see the largest price drop. The Illinois Report Card data, which is available for each elementary school in the district before and after the policy, indicate a students' abilities to meet and exceed established subject guidelines. In Figure 9.2, these variables are summarized by zone and illustrate the decreasing measures of school quality experienced in the Northeast Zone (N) and increases in the West Zone (W).

Once the new policy has been implemented, parents will factor the new quality of education into the utility of their current housing decision, giving rise to the following question: "Does the community feel school quality has decreased due to this new assignment policy?" Theoretically, the new policy should cause school quality to increase since it is implemented to improve the education for both African American and Hispanic children. If this coefficient is negative, it means that parents consider school quality an important factor contributing to housing values. Therefore, given changes in school quality, with repeat sales data the within-district policy effects are calculated. After merging data from the RSD assignment policy and the housing data, it is possible to determine the within-district differences and changes in housing values based on former school quality.

Fig. 9.2 School quality changes by zone



Note: Variables are from Illinois report card data 1997, 1998, 1999, and 2000.

9.5 Empirical results

In order to determine whether changing the school assignment policy has an impact on the capitalized value of housing, the repeat sales data are used to estimate the coefficients of the policy dummy (D_{PC}). The first regression, shown in Table 9.4, shows that having a change in policy leads to a significant decrease in home values at -5.59%. The intercept is also revealing; it represents the coefficient for houses that did not experience a change in policy, and it showed a 3.46% increase in price.

Table 9.4 Regression results for repeat sales change in housing price in Rockford

Variable	Difference ln(sale price)	Difference ln(sale price)
Sale and policy change (NAPCCAP)	-0.055 (-5.59) [0.005]**	-0.060 (-6.15) [0.007]**
Both sold after change (CCAP)		-0.012 (-1.67) [0.0096]
Constant	0.036 (3.46) [0.004]**	0.041 (3.85) [0.0063]**
Observations	2540	2540
R^2	0.0351	0.0357

Note: Robust standard errors in brackets []. Interpretation of log in ().
* denotes statistical significancce at 5% and ** at 1%.

This suggests that the policy change in the district led to a decrease in housing values. Given that choice removes the consideration of school quality from the location decision as outlined previously, this result is not surprising. The constant term in the first regression contains both types of assignment policy; therefore, the second regression (both are in Table 9.4) distinguishes between houses that are bought and sold under the neighborhood assignment policy and houses that are bought and sold under Controlled Choice. These results show that the marginal impact of switching from neighborhood assignment to Controlled Choice decreases homes' values about 6.15% compared to not experiencing a change. In addition, when Controlled Choice policy is in place, houses appeared to decrease in value 1.67%, relative to the 3.85% increase estimate for homes under neighborhood assignment. This decrease, however, is not significant, with a p-value of 0.202, indicating the decrease may have occurred without Controlled Choice. Note that the R^2 values of the first and second regression are small; that is often seen in repeat sales analysis (i.e., 0.033 and 0.035, respectively). This, however, does not represent a threat to the validity of these results as low R^2 values are common in behavioral science research (Chau and Hu, 2002; Cohen, 1988). Overall, the first and second regressions indicate a nonpreference of the homebuyers for the new policy.

Table 9.5 Subset results by policy change and zone

Variable	Difference ln(sale price)	Difference ln(sale price)	Difference ln(sale price)
Northwest Zone	-0.049	-0.050	-0.028
	(-5.40)	(-5.21)	(-3.49)
	[0.013]**	[0.007]**	[0.015]
West Zone	0.029	0.031	0.041
	(2.07)	(2.63)	(2.58)
	[0.017]	[0.010]**	[0.031]
Constant	0.060	-0.001	0.038
	(5.55)	(-0.39)	(3.20
	[0.012]**	[0.006]	[0.013]**
Subsample	NAP	NAPCCAP	CCAP
Observations	455	1741	344
R^2	0.08	0.06	0.04

Note: Robust standard errors in brackets []. Interpretation of log in (). * denotes statistical significancce at 5% and ** at 1%.

Because Controlled Choice creates three zones, the policy implication on housing is likely not unilateral, but differential by zone. An additional set of dummies are created based on the zones designated by the choice program in the district. As mentioned previously, parents in the West Zone are able to pick any school in the district, whereas parents in the Northeast and Southeast Zones are limited to their own zone and the West Zone. Additionally, households that exist in the Southeast Zone are more likely to be assigned to their neighborhood school since most of the preferred schools are in the Northeast Zone. Table 9.5 contains in the first column a regression where the data used are a subset of the repeat sales data where the homes sold had the neighborhood assignment policy (NAP). The general trend for Southeast Zone housing prices is positive at 5.55%, with no significant change in the West Zone. The Northeast Zone appears to already experience a decrease of 5.21%

in housing values. This may reflect anticipation of the change in attendance policy since the district had been dealing with a desegregation lawsuit since 1990.

The second column in Table 9.5 examines the houses sold both before and after a change in policy (NAPCCAP). This indicates that the change in policy has little impact on the houses in the Southeast Zone (constant), and the result is not significant. However, the Northeast Zone and West Zone both have significant results at 1% (p-value of 0.00 for the Northeast Zone and 0.001 for the West Zone). The houses decrease in the Northeast Zone at 5.2% and increase in the West Zone at 2.43%.

Finally, the third column includes houses that are purchased after the Controlled Choice assignment policy (CCAP) is in place. The only significant result is a general trend showing an increase of 3.2% within the Southeast Zone. In contrast, the drop in the Northeast Zone values of 3.49%, or the increase in the West Zone of 2.58%, are results that are just under a significance level with p-values of 0.090 and 0.053, respectively. This is an indication that, under this policy, zones are no longer a factor. In essence, only the Southeast Zone saw any premium from having this policy in place. This result coincides with the increased demand for housing in the Southeast Zone that appears to be a result of the growth of the Hispanic population.

Table 9.6 Regression results from interactions of zone and change in policy

Variable	Difference ln of sale price	Difference ln of sale price
Northeast Zone-(NAPCCAP)	-0.093 (-9.19) [0.007]**	-0.083 (-8.27) [0.007]**
Northeast Zone-(CCAP)	-0.032 (-3.63) [0.008]**	-0.021 (-2.60) [0.008]*
West Zone-(NAPCCAP)	-0.042 (-4.5) [0.008]**	-0.048 (-5.04) [0.008]**
West Zone-(CCAP)	-0.004 (-1.12) [0.014]	-0.006 (-1.36) [0.014]
Southeast Zone-(NAPCCAP)	-0.011 (-1.49) [0.010]	-0.026 (-3.05) [0.010]**
e Southeast Zone-(CCAP)	0.038 (4.51) [0.028]	0.02 (3.34) [0.028]
Meet and exceed 1997 (Me97)		-0.0011 (-0.12) [0.0002]**
Constant	0.041 (3.87) [0.006]**	0.131 (12.98) [0.018]**
Observations	2540	2540
R^2	0.0855	0.1041

Note: Robust standard errors in brackets []. Interpretation of log in (). * denotes statistical significancce at 5% and ** at 1%.

In Table 9.6, interaction dummies are used to determine the zone impact when interacted with the policy. If the interactions exist and are not included in the estimation, this would introduce a specification error in the form of omitted variable bias.

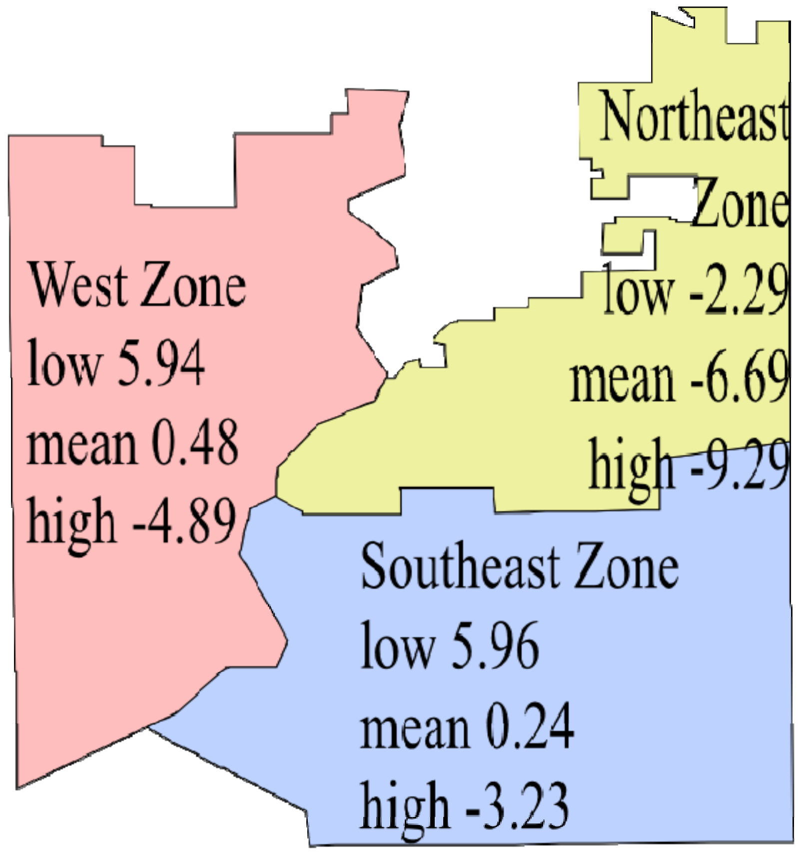
The first column demonstrates that having a change in the assignment policy results in a drop in the real value of a house by 5.59%. However, in the regression, this drop becomes larger, at 9.19%, for houses in the Northeast Zone. The West Zone has a positive result associated with Controlled Choice, leading to a 2.94% increase for housing this zone. This expanded choice and lower uncertainty should put upward pressure on the housing prices in that zone. The results of regression three show that, on average, all houses in the Southeast Zone, which have no change in policy, show a 5.55% increase in value. This indicates that the omission of these interactions does lead to a bias in previous estimates in which the original dummy variable indicating a change in school policy is dropped. Interestingly, here the Northeast Zone dummy loses significance while the West Zone shows the positive impacts of this new policy. This indicates that overall Controlled Choice had positive impacts for the West Zone. The Northeast Zone's drop in housing values is mainly due to this change from neighborhood schools to Controlled Choice and depends less on being in this zone.

In the second column in Table 9.6, a measure is added to indicate former school quality. Here a variable constructed from elementary school level data are included to describe the percentage of students who either meet or exceeded state standards reported by the Illinois State Report Card data. The addition of this variable allows for marked improvement in the R-squared to 0.1041. Figure 9.3 shows that inclusion of this variable for school quality influences the zone effects. This coefficient indicates that a 1% change in this school quality measure leads to a 0.001% change in housing value. This result may not seem like much of an impact; however, if one evaluates the coefficient at one standard deviation away from the mean of 85%, there is an impact to home buyers depending on former school quality.[5] This variable for school quality becomes very important; not only is it significant at 1% but all three policy zone variables appear to be significant factors in falling home prices due to a change from neighborhood school assignment. Houses located near formerly high performing schools lost 9.3% after the policy change. This is an indication that properties formerly included a premium for neighborhood schools of an average of 2.9% if the school performed at least one standard deviation above the district average. In contrast, some houses reside next to schools with lower school quality and had discounts capitalized into current values. Thus, formerly poor performing schools under neighborhood assignment, combined with some average decrease in all zones, result in positive impacts to homes located close to schools, those performing two standard deviation slower than the district average saw a 5.9% increase in both the Southeast and the West Zone.

In repeat sales analysis, all of the differences are computed (i.e., before and after the implementation of Controlled Choice) and a single regression is calculated. Instead of thinking of each year's observation in terms of how much it differed from the prior sale for the same house, the focus is placed on the use of a fixed effect model, that is, how much each observation differed from the average sales price in each year. If one assumes fixed effects, time-independent effects are accounted for each house and possibly correlated with the independent variables. By obtaining multiple observations about each house and looking at the effect of price, the pernicious effect of omitted variable bias is eliminated, which is the intuition behind fixed effect regression. Table 9.5 shows that the changes occurring within the Northeast

[5] To compare changing school testing methods over time, I normalize this data with the state level. The 85% means that before this change in policy, 85% of those who meet and exceed 3rd grade reading scores in the RSD compare to those at the state

Fig. 9.3 Graphical depiction of empirical results by zone



Note: Zone results base on former school quality and data from Table 9.6. All results in the figure are significant at the 1% level.

Zone and the West Zone in the third column hold true when tested against the fixed effect model, which controls for the time trend occurring in the data, shown here in Table 9.7.

Coefficients from regression 8 show the fixed effects model to yield the same coefficient from the repeat sales coefficient (regression 5) in which the Northeast Zone drop of 0.028 are similar to impact under the fixed effects coefficient for Controlled Choice of 0.029. In addition, the West Zone coefficient of a 0.041 increase, seen in regression 5, compares to 0.040, in regression 8. When factored in with repeat sales results, any unobserved bias does not appear to affect the coefficients associated with this change.

9.6 Conclusion

Education policies involving some form of school choice are of great interest and concern to parents, taxpayers, educators, and policymakers alike. In order to determine the impact of the capitalization of public goods into housing prices, this study

Table 9.7 Fixed Effect Regression Results-Interactions with D_{pczone}

Variable	ln of Sales Price
Northeast Zone-(CCAP)	-0.029 (.006)**
West Zone-(CCAP)	0.040 (.008)**
Dy2= 1994	0.022 (.007)**
Dy3= 1995	0.032 (.007)**
Dy4= 1996	0.026 (.007)**
Dy5= 1997	0.012 (.007)
Dy6= 1998	-0.004 (.007)
Dy7= 1999	-0.003 (.007)
Dy8= 2000	0.013 (.0097)**
Constant	11.382 (.009)**
Observations	4857
Number of pid	2362
R^2-within, between, overall	0.05, 0.17, 0.10

Note: Robust standard errors in (). * denotes statistical significancce at 5% and ** at 1%.

adopted an approach of using repeat sales. This control for heterogeneity in property characteristics allowed for the introduction of choice program dummies that are significant to show reduced values of houses that experience a change in policy of 5.59%. In Rockford, the policy aligned all schools to the racial composition of the district while making improvements to the gap between the schools and the percentage of students meeting the state's reading goals. However, house prices drop approximately 9.3% when located near high quality schools following the removal of the guaranteed attendance, unlike houses near former poor-performing schools that have marginal positive effects due to the school choice policy.[6] These results indicate removal of neighborhood assignment removes any premium or discount that previously had been capitalized into housing values. However, the next chapter will examine the idea that this policy impact to housing is caused when residents reevaluate the utility received from schooling. This sorting can lead to increased segregation as homogenous patterns emerge as residents vote with their feet avoiding the RSD Controlled Choice guidelines.

Acknowledgements This paper is drawn from Chapter 2 of my dissertation. I would like to thank my primary advisor, Jeremy Groves, who challenged me. My committee, Virginia Wilcok-Gök and Susan Porter-Hudak, were always been there to listen and give advice. And finally, to my husband, Levi, thank you for being patient and giving me words of encouragement when I needed them most.

[6] Variable based on Illinois Report Card data on 3rd grade students' achievement scores during the policy.

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Chapter 10
Public High-School Quality and House Prices: Evidence from a Natural Experiment

Edward Hearn

Abstract Are changes in local public-school quality capitalized into house prices? In 2002, the Charlotte-Mecklenburg School district CMS rezoned 50% of Charlotte houses to different public schools after the court-ordered end of race-based busing. This chapter contributes to the literature on public-school quality and housing prices by exploiting this natural experiment from Charlotte, NC. Using a difference-in-differences model to identify the causal effect of reassignment into a better or worse public high school on the sale prices of affected houses, my results indicate that people willingly pay between \$3,300 and \$4,700 per percentage point increase in passing rates on the North Carolina Department of Public Instruction's End-of-Course exams. This effect is limited to houses in neighborhoods experiencing large increases in EOC passing rates after the 2002 reassignments. The positive effects of school quality on selling prices increase non-linearly among houses experiencing the largest increases in EOC passing rates. There is no solid evidence that moderate or small increases in EOC passing rates that occurred after the 2002 citywide reassignment are capitalized into house prices in the affected school zones. There is also no evidence to suggest negative price shocks exist for houses CMS reassigns to low-quality public high schools.

10.1 Introduction

Are changes in local public-school quality capitalized into house prices? This is the central question in Oates (1969) regarding the mechanism by which people's preferences for public goods are expressed. Individuals are predicted to move into areas that reflect most closely their desires for public goods. Since people do not pay for these public goods outright, variations in property taxes levied represent differences in the costs to individuals of the amount and quality of local public services they prefer. In most areas of the United States, property-tax-funded services include public schools. Where public schools are of higher quality, houses zoned for these schools are predicted to have higher prices relative to houses zoned for lower-quality schools, holding all else constant.

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Oates (1969), in the spirit of Tiebout (1956), finds that house prices capitalize both property taxes and the value of the public services these taxes finance. Black (1999) finds a positive relationship between house prices and public-school quality for houses along school-zone boundaries in Massachusetts. Bayer et al. (2007) find the same result for houses located along school-zone boundaries in the San Francisco Bay Area. Downes and Zabel (2002) and Figlio and Lucas (2004) conclude that families reveal their preferences for better-quality public schools by sorting into higher-priced houses assigned to these schools. Moreover, Hoxby (2000) finds that cities with more school districts enjoy more productive public schools than cities with fewer school districts. Parental sorting forces greater inter-district competition among public schools when families have more public-school options.

Untangling the effects of school quality and sorting complicates the identification of how school quality impacts house prices. Zahirovic-Herbert and Turnbull (2009) control for sorting by examining how an exogenous change in school-attendance zones in East Baton Rouge Parish, Louisiana affects the price and time-to-sale of affected houses. They find evidence that people pay higher prices for houses assigned to schools with better test scores. However, the East Baton Rouge rezoning pales in comparison to the massive reassignment the Charlotte-Mecklenburg School (CMS) district implemented in 2002. In Charlotte, those neighborhoods reassigned by CMS to better schools are predicted to experience positive price shocks relative to neighborhoods retaining their pre-2002 assignments. People express their preferences for public schools of higher quality through the prices they pay for properties assigned to better performing schools, holding property-tax rates constant. The converse should be true of neighborhoods CMS reassigned into worse schools. Homes in these adversely affected neighborhoods should experience negative price shocks, reflecting the implicit compensation required by residents to consume lower public school quality.

Kane et al. (2006) use the CMS changes in school zones as a natural experiment to investigate how differences in school quality affect house prices, independent of sorting. They examine houses located along school-assignment borders and houses that changed school assignments in Charlotte. The primary difficulty with their identification strategy is that they have a limited time frame in which to study the full effects of the court-ordered rezoning. They used data through 2001, which was prior to the citywide redistricting CMS instituted in 2002. Because it appealed the school-busing reversal all the way to the Supreme Court, CMS did not implement its school reassignment plan until after the citywide rezoning that occurred in April 2002.[1]

This chapter contributes to the existing literature by exploiting this natural experiment from Charlotte, NC. I use this experiment to test whether people pay more for houses assigned to better public schools. In 2002, CMS rezoned 50% of Charlotte houses to different public schools (Hastings et al., 2006) after the court-ordered end of race-based busing. CMS rezoned some neighborhoods into different school zones, but left other neighborhoods' school assignments unchanged. Thus, the district-wide rezoning provides natural treatment and control neighborhoods for an investigation of whether house prices reflect public-school quality.

Because the 2002 school reassignments were plausibly exogenous, they allow for a sharper identification of citywide causal effects than does previous work. Most studies of house-price capitalization use either boundary discontinuity designs or hedonic pricing models. My study has a cleaner identification strategy because Char-

[1] On April 15, 2002, the Supreme Court refused to hear CMS's appeal, thereby upholding the original ruling to end busing.

lotte has only one school district, unlike other large cities that have multiple school districts within city limits. Mecklenburg County also has uniform property tax rates. Since CMS is the sole school district in Charlotte, citywide changes in school attendance zones prior to the 2002-2003 academic year affected all Charlotte neighborhoods similarly. Most other studies do not have citywide levels of treatment within one school district. Rather, these studies typically focus on houses located near a school boundary (Black and Machin, 2011). I examine changes in the prices of all houses in the city of Charlotte and Mecklenburg County, instead of just those houses within narrow geographical bands around school-zone boundaries. In doing so, I am able to capture citywide effects of school quality on house prices in a large urban area.

I use a difference-in-differences model to identify the causal effect of reassignment into a better or worse public school on the sale prices of affected houses. I focus exclusively on changes in the quality of high schools, whereas most studies have examined changes in elementary-school quality. High schools typically have larger catchment areas than elementary and middle schools. School catchment size is important because it permits the inclusion of the many different neighborhoods that CMS assigned to a single high school, and facilitates estimation of the effects of school reassignments on house prices across the citywide price distribution. This variation helps isolate the causal effect of school reassignment on the median house price in a variety of neighborhoods. Also, high-school assignment is arguably more important in terms of student outcomes than assignment to elementary or middle school. High-school quality affects if and where a student attends college, whether a student drops out of formal schooling, and even if a student turns to criminal activity (Billings et al., 2014). Parents (and students) pay close attention to the quality of the high school to which their children (or they) are assigned. My results indicate that people willingly pay between $3,300 and $4,700 per percentage point increase in passing rates on the North Carolina Department of Public Instruction's End-of-Course (EOC) exams. This effect, however, is limited to houses in neighborhoods experiencing large increases in EOC passing rates after the 2002 CMS reassignments.

Because of this limitation, I also test for non-linear effects of changes in the EOC passing rate. My results are similar to those of Chiodo et al. (2010), who conclude that the premium parents pay for houses in better-quality public school zones increases as school quality increases. The positive effects of school quality on selling prices increase non-linearly among houses experiencing the largest increases in EOC passing rates. There is no solid evidence that moderate or small increases in EOC passing rates that occurred after the 2002 citywide reassignment are capitalized into house prices in the affected school zones. There is also no evidence to suggest negative price shocks exist for houses CMS reassigns to low-quality public high schools.

10.2 Data

I used data on house prices from the Charlotte, NC and surrounding-area Multiple Listing Service (MLS). The MLS is an online database containing house-price postings from Realtors, mortgage brokers, property appraisers, and other real-estate specialists. The Charlotte MLS contains data on houses that have been listed any-time within the past fourteen years. Because it is a rolling aggregate, the 2014 MLS would include data back to 2000.

The data consist of observations of houses sold in Charlotte and Mecklenburg County between 1997 and 2010. Only houses listed in the MLS are in these data, however, over 90% of all houses for sale appear in the MLS.[2] The data include both quantitative and qualitative house attributes that fall into three categories: historical, structural, and locational. Historical variables include the year of construction and the selling date. Structural variables include the architectural style, square foot age, type of residence, and so forth. Locational attributes consist of the house's neighborhood, address, zoning code, assigned public schools, and tax location. The data also include the list price and selling price of the house.

I separate these data into neighborhoods containing houses with sell dates before and after April 15, 2002. On this date, implementation of citywide school reassignments took effect for the next school year after CMS lost its appeal to maintain forced busing. I use selling price, rather than list price, because selling price represents the price that the buyer and seller willingly agreed upon. It is, therefore, more indicative of the market price for a house. I follow the recent literature by not limiting the sample to houses with three or more bedrooms. The resulting sample consists of 46,957 houses in 969 neighborhoods. After cleaning the data, I end up with a sample consisting of 44,729 houses in 838 neighborhoods.[3] Table 10.1 contains average selling price and other summary statistics for these houses from the MLS data. It also contains data on EOC test passing-rate averages and differences.[4]

Table 10.1 Summary statistics

Variable	N	Mean	Std. Dev.	Min	Max
		House Attributes			
House Price in 2010 $	44,729	309,372	260,133	10,909	6,645,636
Beds	44,729	3.6	0.7	1	8
Baths	44,729	2.3	0.7	1	6
Half-Baths	44,729	1.0	0.2	1	5
Total Heated Living Area	44,729	2,358.3	982.2	106	13,359
Acres	44,729	0.3	0.3	0	9.41
New	44,729	0.2	0.4	0	1
Age	44,729	23.1	17.7	1	149
Age^2	44,729	845.4	1,363.6	1	22,201
Rezone	44,729	0.4	0.5	0	1
Post-2002	44,729	0.8	0.4	0	1
		School Attributes			
% Pass EOC	43,833	66.9	12.3	26.7	95.4
ΔEOC_{2001}	43,348	3.0	10.9	-37.7	34.5
ΔEOC_{2001} (Positive)	10,292	17.5	11.6	3.16	34.5
ΔEOC_{2001} (Negative)	4,530	-11.4	10.0	-37.7	-1.0

[2] Houses may not appear in the MLS for a number of reasons: a house may be undergoing foreclosure proceedings and the seller does not want it advertised; a house may be purchased within a family; or, the seller may simply not want to price compete with similar properties listed on the MLS server. In any case, there appears to be no systematic bias in the prices of houses listed in the MLS (Higgins, 2013).

[3] I clean the data by eliminating observations with nonsensical values: negative ages, houses over 200 years old, zero bedrooms, more than 6 or no full bathrooms, lot sizes over 10 acres, and house selling prices less than $1,000

[4] EOC passing-rate data come from the North Carolina Department of Public Instruction.

From this sample, I identify neighborhoods that experienced public high-school reassignments and those which experienced no such school reassignments. The former comprise the "treatment" group of neighborhoods and the latter the "control" group. I consider separately two types of treatments: one where a neighborhood experienced a negative shock to high-school quality as a result of high-school reassignment, and one where a neighborhood received a positive shock to high-school quality. A positive school-quality shock occurred when CMS reassigned a neighborhood in 2002 to a better-performing high school. I refer to this group of neighborhoods as having been positively treated. Correspondingly, reassignment induced a negative shock to neighborhood school quality when CMS rezoned the neighborhood into a lower-performing high school than it previously enjoyed. I refer to this group of neighborhoods as having been negatively treated. Control neighborhoods retained their pre-2002 school assignments.

As a measure of school quality, I use EOC test passing rates for every high school in the CMS district. North Carolina state law requires students in grades 9 through 12 to take EOC tests at school-year's end. These tests cover subject material from ten specific courses in the fields of mathematics, science, English, and social studies. I use scores from the Algebra I, English I, and Biology EOC tests for two reasons: the North Carolina Department of Public Instruction mandates that all students in these courses take EOC tests, and high-school students take English I, Algebra I, and/or Biology courses during their 9th and 10th grades. Since the legal age to drop out of high school in North Carolina is 16 years old (with a parent's permission), every student, regardless of ability, takes at least one of these courses. This means that every student in CMS takes these three EOC tests. Taking high-school averages across years in the three subject areas yields estimates of annual school quality based on a population of high-school students before attrition. Since every student must take these EOC tests before they can drop out, Algebra I, English I, and Biology test scores contain information regarding student performance before self-selection of the least-able students becomes an issue.

Table 10.2 Average EOC passing rates by subject and school, 1996-2002

High School	English I	Algebra I	Biology	Average
D.W. Butler	62.8	40.0	66.2	56.3
E.E. Wadell	50.1	29.9	54.9	43.3
East Mecklenburg.	62.4	41.6	62.8	55.6
Garinger	51.0	23.9	33.8	36.2
Independence	70.4	35.7	60.2	55.4
Myers Park	70.7	30.4	68.8	56.6
North Mecklenburg	78.1	33.4	64.0	58.5
Olympic	49.8	25.4	48.7	41.3
Providence	81.3	59.7	80.5	73.8
South Mecklenburg	71.6	35.7	64.4	57.2
West Charlotte	39.4	15.5	37.1	30.7
West Mecklenburg	54.9	53.5	45.8	51.4
Z.B. Vance	67.5	26.7	58.7	51.0

Table 10.2 aggregates EOC passing-rate averages across subjects by year leading up to Charlotte's citywide rezoning. Using EOC test scores from 1996-1997 to 2009-2010 provides multiple years of data before and after April 15, 2002. From these averages, I determine which neighborhoods experienced positive school-quality treatments and which neighborhoods experienced negative treatments. I ex-

pect average house prices in negatively treated neighborhoods to fall relative to the prices of similar houses in their control-neighborhood counterparts, and average house prices in positively treated neighborhoods to rise relative to the prices of control-neighborhood houses.

Table 10.3 Differences in mean prices: Positively and negatively treated and control houses

	Positive Treatment (N)	Control (N)	Differences in Groups
Pre-2002	257,112.7 (1,554)	298,796 (4,980)	-41,683.3
Post-2002	338,364.1 (9,223)	323,118 (23,592)	15,246.1
Differences in Years	81,251.4	24,322	56,929.4
	Negative Treatment (N)	**Control (N)**	**Differences in Groups**
Pre-2002	225,253.3 (707)	298,796 (4,980)	-73,542.7
Post-2002	232,903.3 (3,823)	323,118 (23,592)	-90,214.7
Differences in Years	7,650	24,322	-16,672

Table 10.3 shows that reassignment of a neighborhood into a better public high school from a worse one increases that neighborhood's average house price by $56,929. A reassignment from a better public high school to a worse one, on the other hand, has a negative effect on average price of $16,672. These results represent aggregated selling-price averages in all neighborhoods before and after the April 15, 2002 Supreme Court decision and in both treatment and control neighborhoods.

Figure 10.1 depicts annual EOC test passing rates for houses in positively treated and control neighborhoods. Before April 15, 2002, control neighborhood EOC scores lie below those neighborhoods experiencing no rezoning by about ten percentage points. After April 15, 2002, this gap narrows substantially.

The graph illustrates that houses in positively-treated neighborhoods experience significant improvements in school EOC passing rates, and this effect lasts over the entire sample period after the rezoning. Figure 10.2 provides similar graphical evidence that houses in negatively treated neighborhoods suffer in school quality from the CMS rezoning in the entire post-2002 period. The EOC passing rates in negatively treated neighborhoods actually lie above the control group annual averages, but fall below the control-group averages after 2002. The negative shock to school quality for negatively treated neighborhoods never recovers after the Supreme Court decision.

Figure 10.3 presents monthly average selling prices of houses in both positively treated and control neighborhoods from June 1997 until April 2010. During the era of race-based busing (1997-2002), treatment and control trends are roughly parallel. Even in 2000, where a lack of data makes monthly averages for both treatment and control houses fluctuate significantly, monthly control averages lie above positively treated averages. However, after the April 15, 2002 Supreme Court decision, positive treatment and control monthly averages tighten, and treatment averages begin overtaking control averages. Eventually, almost all the monthly averages of house prices in positively treated neighborhood lie above their control group counterparts.

Fig. 10.1 EOC passing rates for positively treated and control houses

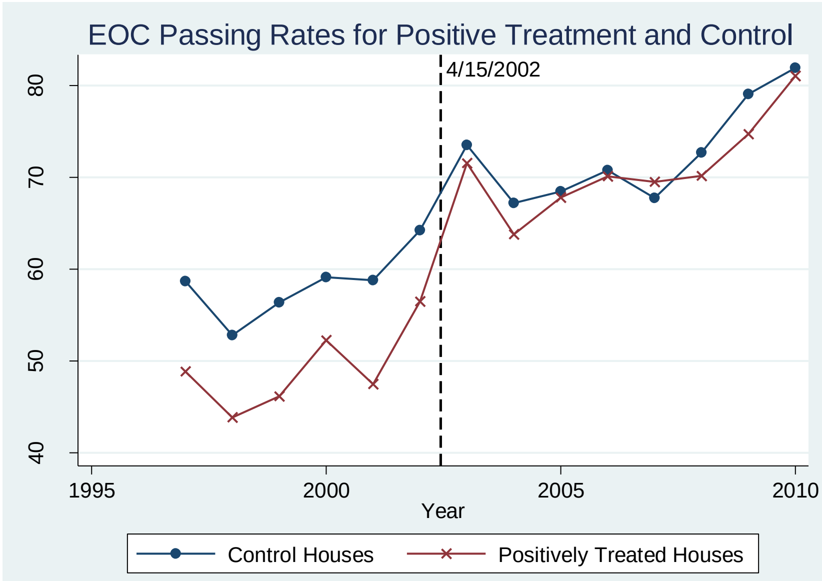


Fig. 10.2 EOC passing rates for negatively treated and control houses

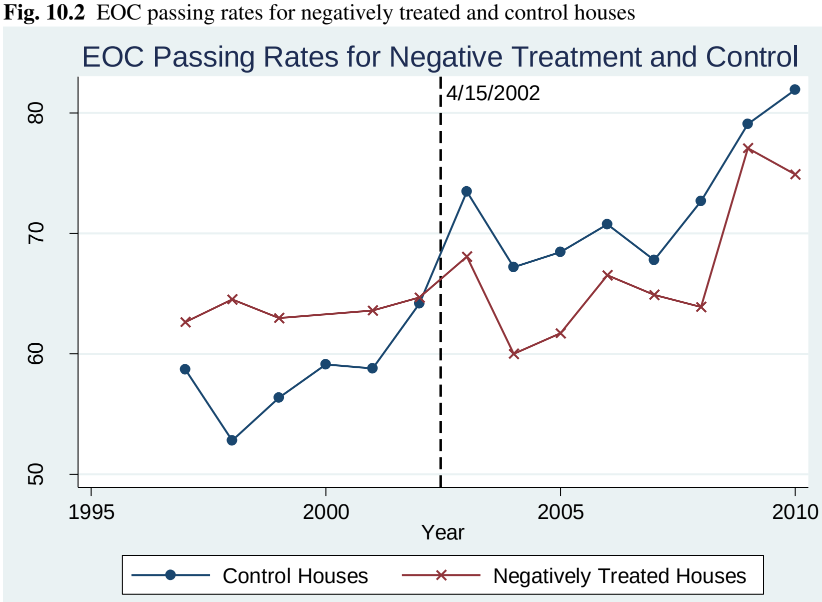


Figure 10.4 plots monthly average selling prices for houses in negatively treated and control neighborhoods. The pattern in monthly house price averages observed prior to 2002 remains unchanged after 2002. Negatively treated houses never achieve price parity with control-group houses. There is no visual evidence in the scatter plot to suggest that the Supreme Court decision on April 15, 2002 had any impact on houses reassigned to lower-quality high schools relative to non-reassigned houses.

Fig. 10.3 Log house sale prices of positively treated and control houses

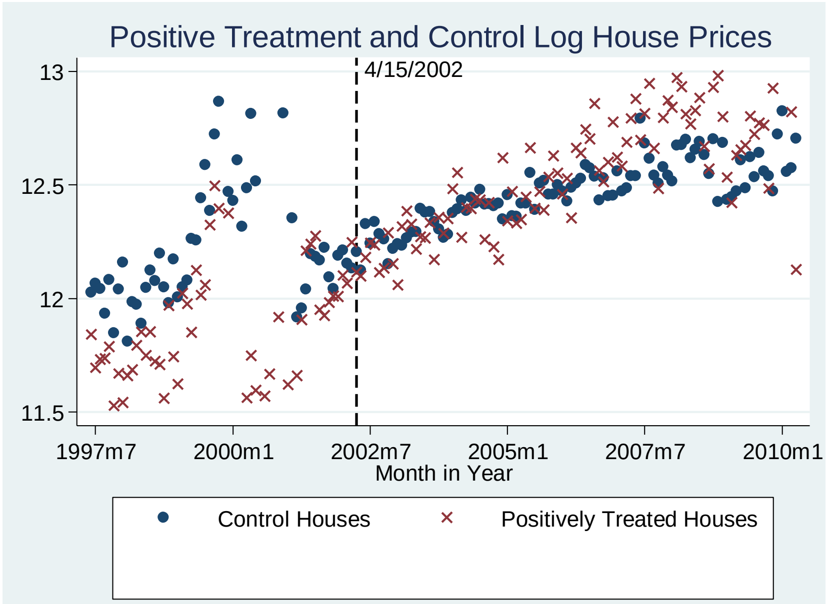


Fig. 10.4 Log house sale prices of negatively treated and control houses

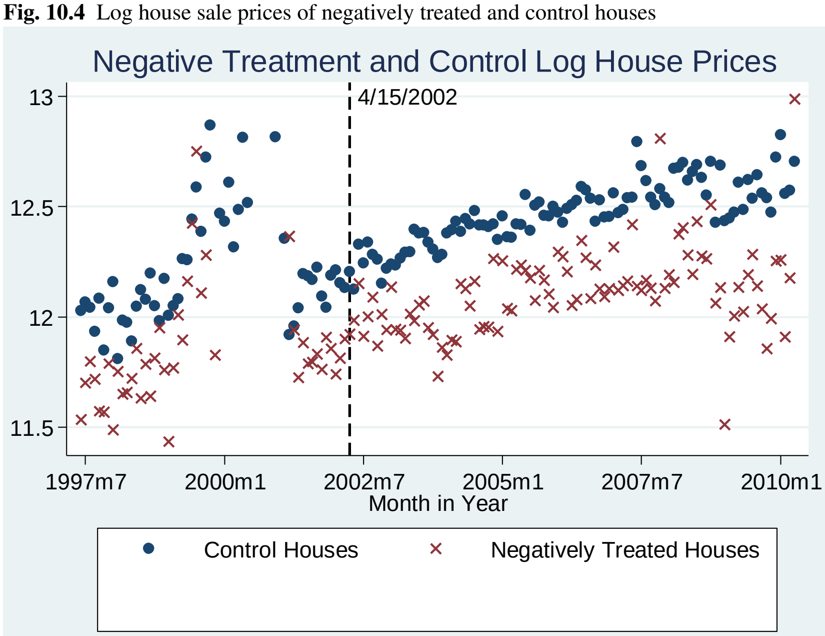
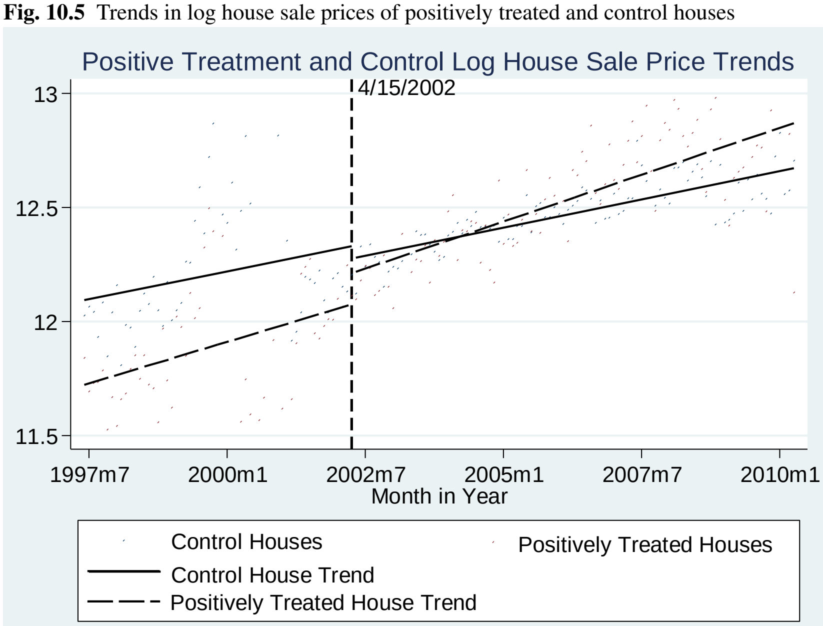


Figure 10.5 minimizes the monthly averages and plots positively treated and control house price trends. During the era of race-based busing (1997-2002), treatment and control trends are roughly parallel, so there is no strong visual evidence that Charlotteans moved into houses in higher quality school zones prior to the Supreme Court decision. There is a small drop in the control neighborhood house price trend from the pre-2002 period to the post-2002 period. This magnifies the upward jump

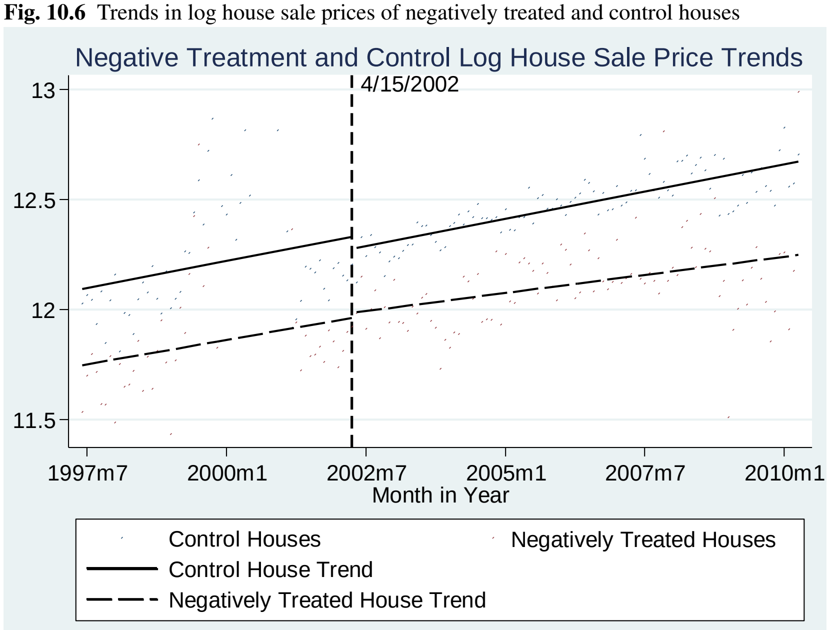
in trend prices for the positively treated neighborhoods between the two periods. By plotting a simple linear fit, in the pre-2002 and post-2002 periods for both positively treated and control houses, the graphical evidence of a positive effect suggests a small but distinct degree of household sorting into homes that CMS reassigned into better schools. Further, the slope of the post-2002 positively treated house trend steepens. The effects of school rezoning in the wake of the Supreme Court case may not have all occurred immediately after April 15, 2002. These effects could have been spread out over several subsequent years.

Fig. 10.5 Trends in log house sale prices of positively treated and control houses



Conversely, graphical evidence of house price capitalization of school quality is not apparent from the plot in Figure 10.6 of the price trends for negatively treated and control houses. There is almost no change in the trend line for negatively treated houses before and after April 2002. The only difference between the two fitted lines is that the slope of the trend line for negatively treated houses becomes slightly less positive after April 15. This graph predicts no significant effects from the CMS school reassignment on house prices in negatively treated neighborhoods. It appears that Charlotteans experiencing a negative school-quality shock did not move to get into a better area. This makes sense considering, as the next section details, these Charlotteans had a chance to win a school choice lottery and gain admittance to a higher quality school within CMS with out having to pay a house price premium.

Fig. 10.6 Trends in log house sale prices of negatively treated and control houses



10.3 Estimation Strategy

10.3.1 Exogeneity of CMS school-zone reassignments

There are two justifications for interpreting the CMS school reassignments as an exogenous change in the Charlotte housing market: the nature of the legal battle to end busing and the structure of the CMS school-choice plan. The legal battle began in 1997 when a parent sued CMS over its race-based busing policy. In 1999, CMS administrators proposed a new school-choice plan which the elected school board rejected in January 2000. From February until June 2000, CMS proposed, debated, and was in the process of instituting a second school-choice plan that was identical to the plan the school board subsequently implemented in 2002. However, in November 2000 the fourth circuit court of appeals in Richmond (VA) reversed its previous ruling and reinstated forced busing. CMS then scrapped its reassignment plan and went back to race-based busing (Charlotte Observer, 2001). The first attempt to legally overturn race-based busing had failed.

In early 2001, a group of parents sued CMS over its race-based student-assignment policy in an attempt to keep legal battle alive. As the case languished in the fourth circuit court, CMS initiated a third reassignment plan that was approved in July 2001, pending the fourth circuit court's decision. On July 15, 2001 the appellate court reversed again and ruled that CMS must dismantle race-based busing (Charlotte Observer, 2001). CMS immediately appealed this decision to the Supreme Court. The school board put implementation of the school-choice reassignment plan on hold until the Supreme Court ruled on whether to hear the appeal.

The lead-up to the Supreme Court's decision took nearly a year. Uncertainty over whether the Court would hear the case abounded. Predictions were mixed: some legal experts said the Supreme Court would not take the case, while others noted that the Court had not heard a school desegregation case in many years and, so, would agree to hear it.

The plaintiffs' case was comprised of two issues: the first challenged the constitutionality of race-based school assignment policies and the second addressed the issue of whether CMS should have to pay the suing parents' legal fees. The dual nature of the case rendered it difficult, if not impossible, to say with any confidence whether the Supreme Court was going to take the case or defer to the fourth circuit court's ruling to overturn race-based busing.[5] As Vanderbilt School of Law Centennial Professor James Blumstein stated, "To say anything is clear in this area is an overly optimistic statement" (Smith, 2002).

Given the legal background of the court case from 1997-2002, Charlotte homeowners' expectations of the Supreme Court decision would have been even less certain than the legal experts'. All of this uncertainty dissolved after April 15, 2002. The Supreme Court ruled on that day not to hear CMS's appeal. This ruling effectively upheld the fourth circuit court's 2001 decision which forced CMS to dismantle race-based busing. The Supreme Court's decision essentially codified the new school-choice plan, which had already been put in place prior to the final decision, and resolved any lingering doubts Charlotteans might have had about whether there would be school reassignments for the 2002-2003 school year. According to the *Charlotte Observer*, parents were simply relieved that the uncertainty had been resolved (Smith and Helms, 2002).

Although most parents were likely familiar with earlier proposals and reassignment plans before April 15, 2002, what parents knew about the 2002 plan's structure clouded their expectations concerning new school assignments. The school board used a lottery system, in conjunction with a parental-choice plan, to rezone students (Hastings et al., 2006). Parents gave CMS their preferences for the top three schools they wanted their student(s) to attend. Concurrently, CMS assigned Charlotte homes into "neighborhood" schools at all levels of education. CMS rezoned 35% of all houses into different high schools and guaranteed students a seat in their neighborhood school, along with transportation to and from neighborhood schools. There was no longer a racial basis for school assignments based on county demographics in which, as was the case under forced busing, CMS could rezone any houses to any public school at any time for the ensuing school year.

The board assigned school seats by lottery if the number of parental requests outnumbered available seats in a high school (assuming that the school the parents chose was not their neighborhood school). Because of the lottery, parents had a chance to enroll their child at a high-performing public high school even if their neighborhood was not assigned to that school. This was true for everyone in Charlotte-Mecklenburg schools, including students just entering CMS (Rothacker and Smith, 2002).

Furthermore, the lottery only applied if a school was oversubscribed. If parents did not "win the lottery" for their first-choice school, they also had a chance to enroll their child in their second- or third-choice school by the same lottery assignment mechanism. Students who did not receive their first-choice assignments joined "waiting pools," while CMS drew their second or third choices from citywide lotteries. CMS conducted the first round of school-choice lotteries and mailed reassignment letters to homes on February 28, 2002 (Rothacker and Smith, 2002). Parents had until March 22, 2002 to appeal reassignment decisions.

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[5] Another reason for a lack of confident predictions before the April 15 Court decision was that only four of nine justices must vote affirmatively for the case to be taken by the Supreme Court.

Many parents complained about the school assignments before this deadline. This forced CMS to revamp its plan, which it did twice after parents received initial assignments but before the Supreme Court decision. The school board voted to give higher priority to students in waiting pools whose first choice was their pre-2002 school, but who had been reassigned to another public school. This alteration occurred on March 8, 2002 after the initial round of rezoning decisions (Helms, 2002b).

The lotteries concluded on March 22, 2002. Subsequently, just four days prior to the Supreme Court's refusal to hear its appeal, CMS altered the plan again by allowing high-school juniors to remain in their pre-2002 public schools, regardless of previous reassignment decisions (Helms, 2002a). After April 15, 2002, the school board made no further changes to the school-choice reassignment plan.

Charlotteans knew about the school choice plan but did not know whether it would be implemented until after the Supreme Court decision. Because of this uncertainty and given prior school reassignment failures, parents would have had little, if any, incentive to move to another house based on predictions about CMS neighborhood reassignments. Even if parents did not get their first-choice school straightaway, there was a possibility that they would "win the lottery" or benefit from a last-minute reassignment plan alteration and their student would be assigned to their first-choice school. After CMS implemented its reassignment plan, 74% of parents got their first-choice school for the 2002-2003 school year (Smith and Helms, 2002).

The choice plan's provision that parents could enter the assignment lottery for any high school in Mecklenburg County became a problem, however, because high-quality public high schools quickly became over-subscribed. While most parents got their first-choice schools in the 2002-2003 school year, many high schools with high EOC passing rates had to turn away "choice-in" students in subsequent years. The odds of being denied entry versus gaining a seat at these high schools ranged from 11:1 to 43:1 by the 2003 school year and became longer in subsequent years. Concurrently, CMS schools with low EOC passing rates suffered diminishing enrollments and increased poverty levels as students who won the lottery for reassignment into better schools disenrolled (Helms, 2003).

By 2005, CMS began considering alterations to the school choice lottery to deal with the dual problems of over-crowding in high-performing high schools and shrinking student populations in low-performing high schools. The school board announced its solution in Fall 2005 for the 2006-2007 school year. CMS elected to keep the lottery in place so parents still had the option to transfer to non-over-subscribed schools. But, CMS dramatically scaled back the options of which high schools parents could choose for their children (Helms, 2005b). The school board removed 75% of county high schools from its reassignment options list due to over-subscription (Helms, 2005a). The only remaining options were those high schools under-subscribed prior to Fall 2006. Because these high schools also had the lowest EOC passing rates citywide, most parents sent their kids to neighborhood schools (Helms, 2006). There was no longer an incentive for parents to participate in the choice lottery after the 2005-2006 school year. The only way for parents to secure a seat at a high EOC-passing-rate high school was to move into that school's catchment area.

To examine the possibility of bias arising from the selection of neighborhoods for school reassignments based on pre-2002 school quality, I estimated Probit regressions of three alternative school-quality measures on the likelihood of a house being reassigned to a better or worse school. First, I found that the marginal effect of a one-standard-deviation increase in the three-subject EOC composite test passing rate diminished the probability of a house being rezoned into a better school zone after 2002 by 4.5%. As an alternative measure of school quality, I also used a composite EOC test-performance measure for the other seven subject areas the NC Department of Public Instruction covers with such tests. The estimated effect was small and not statistically significant. The last school-quality measure I used was average SAT score. The marginal effect of a one-standard-deviation increase in this score resulted in an increase in the probability of receiving a positive treatment by 4.4%. The size and precision of these effects before CMS reassigned neighborhoods were not large enough to indicate a systematic bias by the school board of reassigning neighborhoods assigned previously to lower-quality schools into better school catchment areas.

I repeated this analysis for houses in neighborhoods that were reassigned to lower-quality schools. The marginal effect of a one-standard-deviation increase in the EOC passing rate led to a decrease of 7.1% in the probability of negative treatment. The marginal effect of a one-standard-deviation increase in SAT score decreased the likelihood of reassignment to a worse-performing school by 3.3%. The seven-test composite EOC's marginal effect was 6.1% and also statistically significant. The magnitudes of these effects, while larger than for positively rezoned neighborhoods, are still not large enough to suggest a systematic bias toward reassigning neighborhoods initially assigned to high-quality schools to lower-performing schools in the lead up to the Supreme Court decision.

The direction of the bias indicates that, if endogeneity was present in the selection of neighborhoods receiving positive treatment, then removing the bias would increase the effect of school reassignments on house prices after April 15, 2002. That is, if CMS was more likely to pick worse performing students for reassignment into better performing schools, that would diminish those schools' quality. This reduction in the quality of better high schools from endogenous selection would also reduce the importance of positive sorting on house prices in newly reassigned neighborhoods.

The converse is true of negatively treated neighborhoods. If CMS was more likely to pick higher performing students for reassignment into worse performing schools (before 2002), the quality of schools in negatively treated neighborhoods would have increased. This increase in school quality would have mitigated any negative sorting effects on house prices in those neighborhoods. Endogeneity in school reassignments biases the results towards finding no effects. This fact, and the small magnitudes of the Probit results, suggest that endogenous assignment of CMS houses to positive and negative treatments is not of great concern.

10.3.2 Model Specification

I use a difference-in-differences model to identify the causal effect of a change in school quality on house price. My initial specification is

$$\begin{aligned} y_{ijt} = \alpha + \gamma(rezone_j) + \theta(post2002_t \\ +\delta(rezone_j * post2002_t) + X'_{ijt}\beta + Z_t\zeta + \varepsilon_{ijt} \end{aligned} \quad (10.1)$$

where y_{ijt} is the log of average selling price of house i in neighborhood j and year t, α is the intercept term, $rezone_j$ is a dummy variable equal to one if a house lies in a neighborhood that CMS rezoned, $post2002_t$ is a dummy variable equal to one if a house sells after the end of school year 2001-2002 (with April 15, 2002 designated as the threshold date), X_{ijt} contains house and/or neighborhood characteristics, Z_t contains year fixed effects, and ε_{ijt} is a random error term. The coefficient δ on the interaction of the two dummy variables measures the causal effect of a neighborhood high-school rezoning on house prices. I identify which houses received a positive treatment and which houses received a negative treatment based on their respective neighborhoods' school reassignments. Since the school reassignment occurred at the neighborhood level, I do not include fixed effects for neighborhoods because these would be perfectly collinear with the $rezone_j$ variable.

To further refine the degree to which changes in EOC test passing rates influence house prices, I interact the treatment dummy variable with the change in an EOC test-score measure:

$$\begin{aligned} y_{ijt} = \alpha + \pi(\Delta EOC_{j,2001}) + \theta(post2002_t) \\ + \tau(\Delta EOC_{j,2001} * post2002_t) + X'_{ijt}\beta + Z_t\zeta + \varepsilon_{ijt} \end{aligned} \tag{10.2}$$

All variables are defined as before, with the exception of the newly added variable $\Delta EOC_{j,2001}$. This variablerepresents the difference in 2001 between neighborhood j's pre-2002 public high-school passing rate on the three-subject EOC composite and neighborhood j's post-2002 public high-school passing rate on the same EOC test composite. Neighborhoods not reassigned in 2002 have zero change because there is no difference in passing rates. Rezoned neighborhoods register non-zero differences for this variable because their EOC passing rates change with their new school assignments. Parents knew the public high school into which CMS rezoned their students. In order to assess the school quality shock their children would experience, parents would have had to assess the difference between the 2001 EOC passing-rate differences because students take EOC exams in early June. Therefore, the pertinent comparison of EOC passing rates between pre-2002 and post-2002 public high school assignments would have been the 2001 difference in EOC passing rates. I interact the difference in the 2001 EOC passing rates with the treatment variable to estimate the effect of an increase in the size of the school-quality shock on house prices. If CMS did not reassign a neighborhood, then houses in that neighborhood experienced no school-quality shock. These houses remain in the control group as they would under the dichotomous specification of the treatment using the rezone dummy variable.

10.4 Empirical Results

10.4.0.1 Initial Results

Column 1 of Table 10.4 contains the results of estimating Equation 10.1 using the data for positively treated houses. There are no statistically significant treatment effects on the prices of houses in neighborhoods that CMS transferred to schools of higher quality. This holds for the results from estimating a model containing just the causal variables and a model containing hedonic house variables and year fixed effects.

Table 10.4 The effect of the 2002 CMS rezoning on log selling price of positively treated houses

	Full Sample† (Std. Errors)		Repeat-Sales Sample (Robust Standard Errors)	
post2002	.0534 (.0484)	.0016 (.0148)	.0518** (.0259)	.0792* (.0454)
rezone	-.1044 (.0967)	-.0363 (.0432)	-.0901*** (.0332)	-.0741*** (.0234)
post2002*rezone	.0785 (.0884)	.0411 (.0374)	.0760 (.0515)	.0378 (.0340)
acres		.2559*** (.0365)		.2740*** (.0387)
beds		.2288*** (.0131)		.2473*** (.0133)
baths		.4114*** (.0249)		.3287*** (.0166)
half-baths		.3309*** (.0264)		.3445*** (.0567)
new		.0298 (.0287)		-.0108 (.0263)
age		-.0082*** (.0027)		-.0163*** (.0023)
age^2		.0002*** (3.01e-5)		.0003*** (2.96e-5)
constant	12.44*** (.0546)	10.27*** (.1125)	12.53*** (.0184)	10.58*** (.0935)
Year FE	N	Y	N	Y
F(k, N-k)	6.53	119.26	6.42	112.08
N	39,349	39,349	2,250	2,250

Notes: † indicates standard errors clustered at neighborhood level. *, **, *** indicate p< .10, .05, .01, respectively.

Results from estimating Equation 10.1 for houses in negatively treated neighborhoods are reported in the first column of Table 10.5. Here, again, there are no statistically significant effects on average house price of having been rezoned into a lower quality public high school. This is true whether or not I control for hedonic variables and year fixed-effects. I conclude that the results from the difference-in-differences model provides no evidence that people pay less for houses reassigned into worse school zones.

I replace the rezone dummy with a measure of the difference between positively-treated neighborhoods' pre-reassignment and post-reassignment high schools' 2001 EOC passing rates to estimate Equation 10.2. The first column of Table 10.6 contains the results. Estimates of these treatments in both the basic and hedonic regressions indicate positive and statistically significant house-price effects after accounting for the magnitude of the difference in EOC passing rates. Using the hedonic results, the marginal effect of an increase in the EOC passing rate in a positively treated neighborhood increases the average house price by about $4,711 per one-percentage point increase in EOC passing rate. This effect is large and statistically significant at the 1% level, and provides evidence that people price in the size of the school-quality increase when purchasing (and selling) a house.

Table 10.5 The effect of the 2002 CMS rezoning on log selling price of negatively treated houses

	Full Sample† (Std. Errors)		Repeat-Sales Sample (Robust Standard Errors)	
post2002	.0534 (.0484)	.0192 (.0137)	.0518** (.0259)	.0595 (.0444)
rezone	-.2247 (.0805)	-.0591 (.0399)	-.1548*** (.0482)	-.0434*** (.0344)
post2002*rezone	-.0642 (.0695)	-.0344 (.0372)	.0040 (.0711)	-.0161 (.0499)
acres		.3094*** (.0402)		.3535*** (.0463)
beds		.2310*** (.0155)		.2493*** (.0151)
baths		.3833*** (.0217)		.3132*** (.0174)
half-baths		.2800*** (.0245)		.2736*** (.0540)
new		-.0004 (.0308)		-.0314 (.0296)
age		-.0128*** (.0027)		-.0230*** (.0026)
age^2		.0002*** (3.68e-5)		.0004*** (3.62e-5)
constant	12.44*** (.0547)	10.48*** (.1050)	12.53*** (.0184)	10.73*** (.1000)
Year FE	N	Y	N	Y
F(k, N-k)	5.55	106.58	8.00	95.39
N	33,102	33,102	1,880	1,880

Notes: † indicates standard errors clustered at neighborhood level. *, **, *** indicate p< .10, .05, .01, respectively.

The first column of Table 10.7 provides estimates using data for negatively treated neighborhoods in a model where I interact the treatment dummy variable with the difference in EOC passing rates. These estimates contrast with the evidence associated with the previous results for positively treated houses. There is no evidence to suggest that people pay less for houses CMS rezones into worse schools. Even after I account for EOC passing rate differences, the conclusion is the same as with the treatment indicator variable: no statistically significant results of negative treatment.

These results are similar to results for East Baton Rouge (Zahirovic-Herbert and Turnbull, 2009). These authors found that a one-standard-deviation increase in test performance scores resulted in a 2.5% increase in average house price. Their results for negatively treated neighborhoods, however, were inconclusive, as they are here.

As striking as these results are, they may, in fact, represent a lower bound on Tiebout movement in the wake of the 2002 CMS school reassignments. The lotteries predating April 15, 2002 were the largest, but CMS kept the lottery system in place until October 2005, when they dramatically limited lottery system school choices (draws occurred once per year). At the beginning of the 2006-2007 school year, CMS radically scaled back lottery assignments. In so doing, CMS effectively gutted its choice lottery in favor of assigning students solely to high schools closest to their residence (Helms, 2005c). For this reason, some parents may have applied to the lotteries each year instead of purchasing a new (higher-priced) home in a better quality school zone. CMS transferred students into their school of choice if they

Table 10.6 The effect of changes in EOC passing rates on log selling price of positively treated houses

	Full Sample† (Std. Errors)		Repeat-Sales Sample (Robust Standard Errors)	
post2002	.0292	-.0064	.0372	.0766*
	(.0457)	(.0138)	(.0241)	(.0433)
ΔEOC_{2001}	.0018	.0014	.0059***	.0019
	(.0061)	(.0033)	(.0018)	(.0014)
(post2002*	.0090***	.0045***	.0079***	.0052***
ΔEOC_{2001})	(.0031)	(.0014)	(.0026)	(.0018)
acres		.2614***		.2819***
		(.0349)		(.0402)
beds		.2346***		.2559***
		(.0131)		(.0134)
baths		.4035***		.3210***
		(.0202)		(.0166)
half-baths		.3227***		.3517***
		(.0240)		(.0545)
new		.0200		-.0252
		(.0284)		(.0255)
age		-.0073***		-.0137***
		(.0024)		(.0022)
age^2		.0002***		.0003***
		(2.61e-5)		(2.94e-5)
constant	12.41***	10.26***	12.49***	10.52***
	(.0515)	(.0926)	(.0170)	(.0904)
Year FE	N	Y	N	Y
F(k, N-k)	10.57	132.42	24.54	113.45
N	38,802	38,802	2,226	2,226

Notes: † indicates standard errors clustered at neighborhood level. *, **, *** indicate $p < .10, .05, .01$, respectively.

won the yearly lottery without their parents having to relocate. Lotteries represented an alternative mechanism for parents and students to fulfill their preferences for higher quality education without having to pay a house price premium for it. In this way, Tiebout sorting may have been attenuated by the presence of the annual school choice lottery. Household sorting may also have been deferred until after the 2005-2006 school year, when CMS effectively scrapped its lottery system in favor of neighborhood school assignments.

This idea gains credence from recent work on the educational outcomes for students who “won the lottery” for their first-choice school. On average, students who were selected for reassignment from low-quality schools to high-quality schools through the lottery showed increases in post-secondary achievements. In contrast, students already in high-quality school zones who won lotteries for their first-choice schools exhibited declines in the quality of post-secondary outcomes (Deming et al., 2014). Since lottery draws occurred annually from 2002-2005, parents in low-performing school zones had a yearly incentive to apply for their first-choice school through the lottery system rather than moving between school zones.

As a robustness exercise, I repeat the analysis above using an alternative sample of MLS data. I do so to try and capture neighborhood fixed effects without the problem of multicollinearity arising between fixing effects at the neighborhood level and my difference-in-difference variables. I match addresses before and after April 15, 2002 to form a repeat-sales sample. This significantly cuts down on sample size,

Table 10.7 The effect of changes in EOC passing rates on log selling price of negatively treated houses

	Full Sample† (Std. Errors)		Repeat-Sales Sample (Robust Standard Errors)	
post2002	.0384 (.0463)	.0120 (.0132)	.0493* (.0251)	.0573* (.0442)
ΔEOC_{2001}	.0143*** (.0053)	.0018 (.0038)	.0086*** (.0028)	.0027 (.0044)
(post2002* ΔEOC_{2001})	-.0034*** (.0042)	-.0019 (.0022)	-.0020*** (.0047)	.0003 (.0039)
acres		.3144*** (.0408)		.3552*** (.0464)
beds		.2341*** (.0155)		.2492*** (.0150)
baths		.3848*** (.0218)		.3133*** (.0175)
half-baths		.2802*** (.0245)		.2755*** (.0540)
new		.0011 (.0304)		-.0324 (.0295)
age		-.0128*** (.0028)		-.0235*** (.0026)
age^2		.0002*** (4.14e-5)		.0004 (3.66e-5)
constant	12.43*** (.0524)	10.46*** (.1049)	12.53*** (.0177)	10.73*** (.1000)
Year FE	N	Y	N	Y
F(k, N-k)	6.09	108.17	6.11	95.35
N	33,040	33,040	1,880	1,880

Notes: † indicates standard errors clustered at neighborhood level. *, **, *** indicate $p < .10, .05, .01$, respectively.

but allows me to control for unobserved fixed effects at the neighborhood level. Using identical houses before and after the end of race-based busing also allows a more exact causal interpretation to my estimates, since individual houses do not change much over time. I also control for household attributes and year fixed effects.

The second columns of Tables 10.4 and 10.5 contain repeat-sales sample estimates from Equation 10.1. Like the full-sample estimates, the repeat-sales sample treatment effects are not statistically significant either with or without hedonic household-level variables and year fixed effects. The second columns of Tables 10.6 and 10.7 present results that resemble those from the full-sample regressions. A positively treated house experiences a marginal effect of $3,313 per percentage point increase in the EOC passing rate of its assigned public high school. This premium is smaller than, but roughly similar to, the estimate from the full sample. Estimated premiums for negatively-treated houses are, like the full sample, not statistically significant.

The estimated effects of both the dummy-variable and EOC-interacted treatments are robust in both the full and repeat-sales MLS samples. Because school quality is so closely intertwined with neighborhood characteristics, controlling for the latter isolates the effects of the treatment from the post-busing school reassignments. There is no consistent evidence that the rezonings affected citywide house prices. Positive EOC-interacted treatments, on the other hand, are always positively signed, statistically significant, and of roughly the same magnitudes. There is scant evidence of negative EOC-interacted treatment effects. By controlling for the magnitude of the school-quality shock, it becomes clear that families relocate based on how much school quality they gain or lose as measured by the differences in school EOC passing rates.

10.4.1 Non-Linear Treatment Results

Following Chiodo et al. (2010), I test for non-linear treatment effects. To do so, I use split-sample regressions by dividing the EOC-interacted treatments in my full and repeat-sales samples into three bins based on quantiles of the differences in 2001 EOC passing rates. The "Bottom" bin represents differences up to the 25th quantile of the distribution of EOC passing-rate differences. This bin represents houses that experienced a small positive or negative school-quality shock. I bin "Middle" as EOC passing rate differences from the 25th to the 75th quantiles. There are surprisingly few passing-rate differences that fall into the middle of the distribution, and this is why I aggregate the middle two quartiles in filling the middle bin. Finally, the "Top" bin contains EOC passing-rate differences above the 75th quantile of the 2001 EOC test-score difference distribution.

Table 10.8 The effects of non-linear changes in EOC passing rates on log selling price, positively treated houses

	Bottom‡ (Std. Errors)	Middle‡ (Std. Errors)	Top‡ (Std. Errors)
Full Sample†	-.0036 (.0047)	-.0061* (.0036)	.0048*** (.0012)
N	33,008	29,650	33,161
Repeat-Sales Sample	-.0117** (.0053)	.0017 (.0047)	.0053*** (.0019)
N	1,931	1,738	1,878

Notes: † Indicates standard errors clustered at neighborhood level. *, **, *** indicate p< .10, .05, .01, respectively. ‡ Bottom, Middle, and Top represent the bins of <25th, 25th-75th, and >75th quantiles of the differences in 2001 EOC passing rates between positively-treated neighborhoods' pre-2002 and post-2002 high school assignments.

As Table 10.8 shows, the top bins of the EOC-interacted treatment drive most of the regression results from the previous sections. There is weak evidence from both the full and repeat-sales samples that, in the middle and bottom bins, there is a negative effect on house prices from positive treatment. This evidence, however, is inconsistent across samples. The results for "Top" treatments are all positively signed, statistically significant, and resemble the results of Chiodo et al. (2010). Houses experiencing the largest increases in school quality pay premia for better schools. Houses experiencing small-or medium-sized increases in EOC passing rates display no evidence of price effects. This translates into price premia for houses for which high school reassignment results in EOC passing-rate increases of 25 percentage-points or more. These price premia are essentially equal to those in the EOC-interacted results from Table 10.6.

Table 10.9 The effects of non-linear changes in EOC passing rates on log selling price, negatively treated houses

	Bottom‡ (Std. Errors)	Middle‡ (Std. Errors)	Top‡ (Std. Errors)
Full Sample†	.0134 (.0138)	-.0019 (.0038)	-.0036** (.0017)
N	29,725	30,333	29,270
Repeat-Sales Sample	-.0033 (.0211)	-.0012 (.0054)	.0014 (.0050)
N	1,716	1,791	1,694

Notes: † Indicates standard errors clustered at neighborhood level. *, **, *** indicate p< .10, .05, .01, respectively. ‡ Bottom, Middle, and Top represent the bins of <25th, 25th-75th, and >75th quantiles of the differences in 2001 EOC passing rates between positively-treated neighborhoods' pre-2002 and post-2002 high school assignments.

Because CMS took away all of its top quality high schools as lottery choices after 2005, the only way to get into a high EOC-passing-rate school was to move houses prior to the 2006-2007 school year. The marginal effects may reflect this movement. For the full sample, there is a price premium of $4,734 for a one-point increase in EOC passing rates and for the repeat-sales sample there is a price premium of $2,694. Most of the effect of CMS's neighborhood reassignment policy probably occurs after there is not the mitigating impact of a lottery to win admittance to any public school in Mecklenburg County.

Table 10.9 presents split-sample results of EOC-interacted treatment estimates for houses in negatively treated neighborhoods. The evidence is much weaker than in Table 10.8. The only statistically significant treatment effect has the correct sign, but is sample-specific, and, so, is not generalizable.

10.5 Heterogeneity Test: Condominium Prices

As a further check on the robustness of my results, I regress the same sets of covariates and treatments I used previously on condominium prices. Since families with school-aged children are more likely to own detached houses rather than condominiums, sorting along school quality lines should not affect condominium prices to the same extent. Gallagher et al. (2013) proposed a similar test. They posit that the prices of small houses do not capitalize differences in public school quality because people in these houses generally do not have school-aged children. If small houses are less likely to contain public school students, then condominiums are similarly unlikely to contain families with public school children.

I control for neighborhood and fixed effects by matching condominium sales by address before and after the end of race-based busing on April 15, 2002. This limits my sample size because condominiums' neighborhoods are unlisted in the MLS data prior to 2003. However, matching controls for confounding neighborhood and locational attributes. Since I cannot narrow my sample to condominiums least likely to have school-aged kids (i.e., one-room condominiums), families with public school students may reside in some of my sample properties. Any bias in this direction, though, works to the benefit of a statistically non-significant result. Because families would sort into condominiums located in better public school zones for what would probably be a much lower price premium than for detached houses in these zones, treatment estimates will tend toward being significantly different than zero.[6]

My results include less structural attributes of condominiums than of houses. The excluded variables do not translate from house attributes to condominium attributes. For instance, lot size is solely a detached-housing attribute. I also include total heated living area instead of the number of bathrooms, since inclusion of both bathrooms and bedrooms provides less variation. Condominiums vary by size, but few have more than two bathrooms or three bedrooms.

Table 10.10 The effect of the 2002 CMS rezoning on log selling price of condominiums

	Positively Treated	Positively Treated	Negatively Treated	Negatively Treated
post2002	.1899** (.0814)	.1652*** (.0523)	.1899** (.0815)	.1658*** (.0527)
rezone	-.0031 (.1052)	.1897** (.0918)	-.1758 (.2759)	-.3228*** (.0634)
post2002*rezone	.0477 (.1716)	.0497 (.1480)	-.0564 (.5093)	-.0640 (.1722)
beds		-.1697*** (.0589)		-.0868 (.0579)
total heated living area		.8037*** (.0613)		.7411*** (.0530)
new		-.1513* (.0798)		-.1454* (.0763)
age		-.0180*** (.0046)		-.0226*** (.0047)
age^2		.0002*** (4.15e-5)		.0002*** (5.30e-5)
constant	11.52*** (.0553)	11.27*** (.1986)		11.21*** (.1986)
F(k, N-k)	2.65	44.20	2.15	76.02
N	161	161	141	141

Note: *, **, *** indicate p<.10, .05, .01, respectively. Huber-White standard errors.

Table 10.10 provides the results of estimating equation (1) using data on the selling prices of condominiums in Charlotte from 1997-2010. For both positively and negatively treated units, there are no statistically significant effects of school-zone changes on selling prices. This is true for both basic (columns 1 and 3) and hedonic (columns 2 and 4) regressions. Also, some of the signs of estimated hedonic coefficients are different than their household counterparts. Condominium purchasers have different preferences than house purchasers in the data, and this may be why the coefficients on characteristics like "new" and "beds" have different signs than the coefficients on these variables in the house regressions.

[6] This bias works identically for foreclosed houses. To the extent that banks price these properties to sell quickly, parents can move into a foreclosed house in a higher-quality school zone at a lower price than a non-foreclosed house (Saccacio, n.d.). While I cannot identify foreclosed houses in my MLS sample, omitting foreclosures induces a bias toward finding statistically non-significant treatment estimates.

Table 10.11 The effect of changes in EOC passing rates on log selling price of condominiums

	Positively Treated	Positively Treated	Negatively Treated	Negatively Treated
post2002	.1813** (.0795)	.1555*** (.0511)	.1791** (.0824)	.1627*** (.0538)
ΔEOC_{2001}	.0039 (.0032)	.0103*** (.0022)	-.0165*** (.0057)	.0097* (.0054)
(post2002* ΔEOC_{2001})	.0040 (.0050)	.0039 (.0036)	-.0188 (.0119)	-.0163 (.0095)
beds		-.1457** (.0654)		-.0305 (.0657)
total heated living area		.8122*** (.0640)		.6740*** (.0582)
new		-.1626* (.0671)		-.1234 (.0788)
age		-.0178*** (.0044)		-.0228*** (.0050)
age^2		.0002*** (4.17e-5)		.0002*** (4.97e-5)
constant	11.50*** (.0533)	11.21*** (.1933)		11.14*** (.2244)
F(k, N-k)	4.81	44.49	7.76	67.57
N	161	161	137	137

Note: *, **, *** indicate p<.10, .05, .01, respectively. Huber-White standard errors.

Results from estimating Equation 10.2 are reported in Table 10.11. The estimates are nearly identical to those of Table 10.10. The only statistically significant effect from a change in school EOC score occurs for negatively-treated condominiums, and it is only marginally statistically significant. Recall that the results from negatively treated houses indicated only one statistically significant result, by way of the neighborhood-attribute sample. There is scant consistent evidence for a negative treatment effect for either houses or condominiums.

10.6 Conclusion

The results reported here indicate that houses receiving positive, EOC-interacted treatments in assigned-school quality experienced increases in prices. This effect is non-linear, however, and is precisely estimated only for those houses in neighborhoods which experience a twenty-five percentage-point or higher jump in their school EOC passing rates. Negative treatment effects are never consistently statistically significant, and neither are positive or negative treatment estimates from equation (1). Simply put, Charlotteans paid premia for houses reassigned to higher quality high schools, given the quality increased enough. On the other hand, there is no convincing empirical evidence that Charlotteans receive price discounts for houses reassigned to lower quality high schools after April 2002.

There would have been no incentive to sell a house in a negatively treated neighborhood, especially during the first three years of the school choice plan. A school choice lottery provided Charlotteans a method by which they could potentially get their students into high-quality high schools without incurring a house price premium from moving to a house in a higher EOC-passing school's neighborhood. However, after CMS took away 75% of the high school parents with public high-school kids could apply for, including all of the public high schools with the best EOC passing rates in the city, the effects of house-price capitalization are obvious in those high-performing schools' zones. Furthermore, my results suggest that most of the house-price premia occurs after a threshold level of increase in EOC-passing rates. The marginal effects on house prices in the "Top" bin of EOC passing rate differences are essentially the same as the marginal effects on house prices in the EOC-treatment regressions.

Robustness checks confirm these conclusions. Using full and repeat-sales samples of MLS data confirms that, although the results vary in magnitude, they are qualitatively consistent: the marginal effects of an EOC passing rate increases house prices in positively treated neighborhoods. This result is non-linear in both samples I analyze. My findings are broadly consistent with Zietz et al. (2008) and Liao and Wang (2012) who find evidence, using quantile regressions, that houses from different quantiles of the price distribution value identical private amenities differently. The rezoning of a house into a higher-performing school zone has no effect on house price unless the newly assigned school is sufficiently better than the previously assigned school.

A comparison with average private school tuition rates sheds light on the willingness to pay house-price premia for better quality public schools. In Charlotte, there are eighteen denominational and non-denominational private high schools. The average annual tuition for the twelfth grade in all of the private high schools was $13,180 in 2012. The maximum private high-school tuition is $21,200 and the minimum is $6,307 (Colwell and Thurmond, 2012). Parents with the ability to move houses would do so rather than pay private tuition every year. Parents with students newly entering or having recently entered high school would benefit more from a move to a significantly better school zone rather than from sending their children to private school. However, this may not be the case for parents with students in their last year or two of high school. In this case, it may be more cost-effective to enroll their students in private schools, depending on how much of an additional post-threshold increase in EOC-passing rates parents seek.

Possible extensions of this study would involve using the MLS data in several different ways. Future work could examine school-reassignment effects on the prices of small houses (those with fewer than three bedrooms). These properties should exhibit smaller premia for public-school quality. I could also incorporate additional measures of high-school quality, such as graduation rates or SAT scores. Unlike the three-subject EOC-composite measure, however, these alternative school-quality metrics are subject to selection biases because they reflect only those students who have elected to not drop out of high school or who take the SAT. A natural extension of this paper would be to investigate the yearly effects of the rezoning variable in an event-study framework. This type of analysis could examine the annual shifts in house prices over the time frame following the 2002 Supreme Court decision to more exactly estimate how changes in CMS policy impacted area house prices. The application of MLS data to examine the effects of the CMS post-busing rezoning on Charlotte house prices would enable empirical investigations of these questions and many others.

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Chapter 11
Hospitals and Housing Prices in Small Towns in Wisconsin

Russell Kashian, Michael Kashian and Logan O'Brien

Abstract Research has determined that hospitals often have significant impacts on real estate housing prices. The objective of this study is to refine and add to the growing literature on the proximity-based effect that hospitals have on surrounding home values due to their semi-obnoxious characteristics. By limiting this inquiry to the population of all home sales in Wisconsin Cities and Villages with a population of under 10,000 persons and hospital with an emergency room, other desirable (and undesirable) externalities are limited. The results suggest that, when controlling for the individual housing qualities, proximity to the hospital is desirable.

11.1 Introduction

Hospitals often have significant impacts on real estate housing prices. The development of a hospital can have positive effects, whether it be increasing the rate of economic development, or access to healthcare, especially urgent health care at an emergency room. Facilities that provide desirable services like these are often referred to as 'yes-in-my-backyard (YIMBY) facilities' and generally yield increased home values based on proximity to a YIMBY facility. Living too close to a hospital can also produce negative externalities for residents, through hospital-related traffic as well as noise pollution from ambulances, emergency helicopters, etc. Other facilities that cause negative externalities are referred to as 'not-in-my-backyard (NIMBY) facilities' and usually correlate with lowered property values based on proximity to them (Peng and Chiang, 2015). Facilities, like hospitals for example, that provide the positive resources as well as the undesirable externalities are known as 'semi-obnoxious facilities.' Other examples of semi-obnoxious facilities include large sports stadiums, airports, and police/fire stations, among many others. Ideally for homebuyers, living near an ER/Hospital brings valuable accessibility, but living

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too close brings undesirable externalities. Therefore there is a zone in which property values are maximized, based on an ideal distance from the nearest hospital.

The objective of this study is to refine and add to the growing literature on the proximity-based effect that hospitals have on surrounding home values due to their semi-obnoxious characteristics. Drawing from a database of over 3,000 homes within a two mile radius of each hospital, we were able to determine the effect on house values through proximity to the main entrance of the hospital. This study expands the body of housing research by assessing the impact of hospitals on local real estate without the clutter of other external variables, and utilizes spline variables for distance from the hospital to allow for non-linearities. Surprisingly, the results show that the home sales price is maximized nearest to the hospital.

The remainder of this paper continues as follows: related literature review, explanation of the models and methodologies, summary of the data, interpretation of the empirical results, and conclusions.

11.2 Literature Review Methods

Hedonic analysis describes commodities as consisting of differentiated characteristics, which together determine total value. Hedonic analysis has roots in the early 20th century in non-housing studies, however it has since expanded into use for real estate, more specifically, the analysis of housing. The concept for the empirical hedonic analysis developed around the idea that goods, previously thought of as homogenous, are actually comprised of a multitude of differentiated components. Through this concept researchers use hedonic analysis to estimate the contribution derived from the components of a unit and the value offered by the components to the overall value of the unit (Rosen, 1974). Importantly, hedonic analysis captures the implicit and explicit contributions of the characteristics that comprise the commodity. To do this, a majority of hedonic models explain total value (e.g., sale price) as either a semi-log or a linear combination of the attributes comprising the commodity.

Each characteristic of a commodity provides a unique level of utility to buyers. As a commodity, any home then provides a total utility based on the values the differentiated characteristics take on (Harding et al., 2003). The market for single-family housing units does not contain a supply of homogeneous homes, but rather it contains the demand for and supply of differentiated attributes determined on the purchasing side of the market (Epple, 1987).

11.2.1 Housing Characteristics

Outside of proximity to semi-obnoxious facilities, the value of individual housing characteristics is important in determining the overall value of properties (Harding et al., 2003). In an analysis of roughly 125 studies, the number of bedrooms, bathrooms, and square feet generally yielded positive coefficients, the home age commonly had a negative coefficient, and none of the studies found a negative coefficient for the lot size or presence of a garage (Sirmans et al., 2006). Comparing hedonic research results directly to one another is often difficult, because several methods for measuring and incorporating housing attributes exist in a hedonic analysis, including binary dummy variables, complex dummy variables, and utilizing a count to indicate more than one unit.

11.2.2 Community Amenities

Location can be a benefit or a detriment to a home's value. Determining how location causes the impact on the value of a house can be difficult. In turning the location into a variable, the impact of the location can become influenced by other variables (Malpezzi, 2002). This study was limited to exurban and smaller suburban communities throughout Wisconsin that contain an ER/Hospital, because larger, more condensed urban communities are affected by other attractions and facilities that subsequently have their own effect on home values.

11.3 Data and Empirical Model

The data was limited to single-family home sales within two miles of a hospital, located within towns or cities with a minimum of 674 residents and a maximum of 39,114 residents, for the years from 2013 to 2019. This approach yielded a sample of 3,013 home sales with complete information on all variables.

Following Peng and Chiang (2015), the distance from the nearest hospital, measured in miles, was split into splines with the provided distances in a fixed range (less than .25 miles, or between .25 and .5 miles), such that coefficients could be interpreted as slopes within the range. Peng and Chiang (2015) present a single set of spline variables but, given that the ranges are necessarily arbitrary, we instead considered splines with only two ranges (less than .5 miles to at least .5 miles), three (less than .33 miles, and .33 to .66 miles), four (less than .25 miles), five (less than .2 miles) or 10 ranges (less than .1 miles). This experimentation allows the identification of appropriate ranges. Splines for ranges above one mile were considered and rejected since only 157 home sales, approximately 5% of the entire sample, were in that range.

As is standard in hedonic models, we regress the natural logarithm of housing prices against a series of independent variables, such that coefficients () can be interpreted with a 1 unit change in an independent variable yielding a proportional change in the dependent variable. The specific formulation is:

$$\ln(\text{price}) = \alpha + \beta(X) + \eta \text{ (year, month) } + \varepsilon \qquad (11.1)$$

where price is the sales price, β a vector of coefficients, X a series of independent variables including the hospital distance spline variables, η is a vector of coefficients for *year* and *month* dummy variables utilized to control for any fixed effects of either, and ε is the error term.

For this study, we were concerned that hospitals might be located near the center of these small cities and towns such that measured distance from the hospital might actually measure the distance from the city or town center. To control for this possibility, a variable is included for the distance from the town or city post office to the hospital, using the assumption that the post office is in fact located at the center.

Table 11.1 Descriptive statistics

Variable	Mean	Std. Dev.
Price	$124,260	72,090
Hospital Distance	.449 miles	.294
Post office distance to hospital	.771 miles	.514
Square feet of house	1,761	698.5
Full baths in house	1.626	.682
Half baths in house	.327	.484
Bedrooms in house	3.146	.774
Attached garage (proportion)	.541	.498
Population in town/city	7,721	8345

Note: n=3,013.

Descriptions for the variables are provided in Table 11.3. Note that square feet and population are entered in quadratic form in the regression to allow for declining marginal effects.

11.4 Regression Results

Regression results for either 3, 4 or 5 splines are presented in Table 11.4. Results for 2 or 10 splines are not provided because, for the prior, both coefficients were insignificant, while for the latter only 3 of 10 coefficients were significant, and these did not yield any clear pattern, meaning the significant coefficients were not contiguous with each other.

Overall explanatory power is reasonable for each regression, with R^2 statistics above .5 in each. Other results are also as expected, with larger homes yielding higher prices with a declining marginal effect, and with full and half baths and attached garages yielding significantly higher sales prices. The distance between the hospital and the post office provides a positive, significant coefficient, which might be due to hospitals located further from the city center being newer and in newer developments. In unreported results, the year fixed effects suggest prices tended to rise over time, while the month effects show peak selling prices between June and September.

Turning to the splines themselves, none are significant with either 3 or 5 splines, but three are significant in the regression with 4 splines, which is also the regression with the highest R^2 statistic. Those results suggest that price declines for houses further away from the hospital, with the largest effect for houses within .25 miles of the hospital, and with the effect dissipating thereafter and becoming insignificant for houses more than .75 miles from the hospital.

11.5 Discussion

The analysis here sought to understand the relationship between the distance to the nearest hospital in rural and exurban locations in Wisconsin and the home selling price. The results for control variables were quite reasonable and consistent with prior research. The distance result is surprising, in that we found home price drop-

Table 11.2 Selling price regression

Variable	3 Splines	4 Splines	5 Splines
Hospital distance range 1 of 3	0.015		
	(0.120)		
Hospital distance range 2 of 3	0.007		
	(0.060)		
Hospital distance range 3 of 3	0.026		
	(.033)		
Hospital distance range 1 of 4		-0.511**	
		(0.205)	
Hospital distance range 2 of 4		-0.220**	
		(0.094)	
Hospital distance range 3 of 4		-0.135**	
		(0.062)	
Hospital distance range 4 of 4		-0.034	
		(0.037)	
Hospital distance range 1 of 5			-0.242
			(0.279)
Hospital distance range 2 of 5			-0.122
			(0.129)
Hospital distance range 3 of 5			-0.073
			(0.085)
Hospital distance range 4 of 5			-0.096
			(0.064)
Hospital distance range 5 of 5			0.015
			(0.039)
Post office	0.171***	0.174***	0.175***
	(0.019)	(0.019)	(0.019)
Square feet	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Square feet2	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)
Full baths	0.230***	0.229***	0.230***
	(0.016)	(0.016)	(0.016)
Half baths	0.129***	0.129***	0.128***
	(0.017)	(0.017)	(0.017)
Bedrooms	-0.008	-0.008	-0.008
	(0.013)	(0.013)	(0.013)
Attached garage	0.233***	0.232***	0.229***
	(0.016)	(0.016)	(0.017)
Population	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Population2	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Year and Month Fixed Effects	✓	✓	✓
Observations	3,013	3,013	3,013
R^2	0.538	0.540	0.539

ping at a decreasing rate as the distance increases from the hospital, with the largest effect being on the closest spline (0 to .25 miles).

It was expected that close proximity to a hospital would reduce selling price due to the high levels of noise and traffic associated with hospitals. Further, each hospital in the sample had an emergency room, so noise and traffic are expected. The results suggest instead that proximity to the hospital is desirable. There are two plausible and complementary explanations for this finding. First, hospital employees may value locations within short walking distance to the hospital. Second, given the hospitals are in rural and exurban location, they may be small and generate relatively infrequent noise from ambulances or helicopters. Nonetheless, it is still possible that home locations in very close proximity to these hospitals (e.g., across the street) are associated with reduced home prices, but there are an insufficient number of observations for those locations in this data.[1]

A limitation of this study is that it does not include urban locations. Parallel tests of home real estate markets and hospital location in medium and large urban areas might yield very different results.

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[1] The closest the analysis comes to testing very close location is the regression with 10 splines. The coefficient on location within one-tenth of a mile of the hospital is negative (-.697) and insignificant.