

PUBLIC HIGH-SCHOOL QUALITY AND HOUSE PRICES: EVIDENCE FROM A  
NATURAL EXPERIMENT

by

EDWARD MCLEOD HEARN

(Under the Direction of Ronald S. Warren)

ABSTRACT

From its origin as a small Southern county seat, Charlotte has grown into a major urban economy. This swift economic expansion has required city authorities to respond by changing both the nature and provision of Charlotte's public goods. This dissertation examines the impacts of city-amenity provisions on house prices in Charlotte from 1997 to 2010. Chapter 1 provides a concise review of house-price capitalization of public-good amounts and qualities from different locations in the U.S. and abroad, and across a variety of empirical modelling strategies. Chapter 2 examines the fallout of a district-wide change in public-school assignment on house prices in Charlotte and Mecklenburg County, NC. The local school board shifted from a race-based assignment policy to a neighborhood assignment policy and also incorporated a school-choice lottery. This occurred after a 2002 Supreme Court decision disallowed the school board from using race as the primary assignment mechanism for local high schools. Results indicate that parents are willing to pay premia for houses in better quality high-school zones, but only if the increase in quality is substantial. Chapter 3 examines whether increases in public amenities provide arbitrage opportunities for speculative purchasing of houses in areas where these amenities were previously lacking. Findings suggest that house-price speculators make rational

decisions to purchase in neighborhoods with a high probability of experiencing gentrification.

Moreover, speculative behavior is predictable based on preferences for house and neighborhood attributes.

**INDEX WORDS:** Public High-School Quality, Public Goods, Charlotte, NC, House-Price Speculation, House-Price Capitalization, Gentrification, Charlotte-Mecklenburg Schools (CMS), Natural Experiment, Difference-in-Differences Model, Bayesian Transferability, Race-Based Busing, Urban and Regional Economics, School-Choice Lottery

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## **DEDICATION**

I dedicate this work to my family for the gifts that they have given me: to my sister, Susan, for the gifts of humor and understanding; to my father, Granville, for the gifts of inquisitiveness and patience; and to my mother, Katherine, for the gifts of knowledge and determination. I fervently hope that I have made these gifts manifest in all things I accomplish, now and always.

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## CHAPTER 1

### House-Price Capitalization of School Quality and Other Neighborhood Amenities: A Review of the Literature

#### 1.1 Introduction

If public amenities are capitalized into house prices, then homeowners will reveal their preferences for these goods through the property taxes they pay. This is Oates' (1969) main contribution, and it stems from a prediction in Tiebout (1956) regarding the mechanism by which people express their preferences for local public goods. There has been a great deal of literature on tax capitalization of public school quality into house prices. Economists have employed a number of techniques to study tax capitalization across a variety of different locations, both in the U.S. and abroad. Most of these studies employ hedonic regressions and use either Multiple Listing Service (MLS) or Tax Assessor's data on house prices and house attributes. This review examines some of the more recent studies from the U.S. and abroad. It also highlights a case study from Charlotte, NC, on which a large amount of research has been conducted. Finally, this chapter provides some ideas for future research on the role of hedonic attributes as predictors of house-price behavior and for dealing with some of the methodological pitfalls of hedonic regressions.

Assuming full knowledge of local policies and full mobility of taxpayer-consumers, individuals are predicted to move to areas that most closely coincide with their preferences for public goods. Since people do not pay for public goods outright, variations in property taxes

levied represent differences in the costs to individuals of the amount and quality of local services they prefer. According to this theory, the greater the number of locally-administered regions supplying amenities, the closer will property tax rates reflect public-goods preferences of local property owners. In most areas of the United States, property-tax-funded services include, for example, public schools, fire-fighting and police forces, and non-interstate road and travel infrastructure. Where these public amenities are of higher quality or greater amount, houses have higher property tax rates than houses in areas receiving lower-quality or no such services, holding all else constant. Moreover, potential Tiebout movements and their impacts on house prices may be predicted based on identifying areas where house prices have not yet responded to increasing neighborhood quality. Here, arbitrage opportunities for economic profit exist. Speculators' activity in improving neighborhoods can gain them short-run largesse before Tiebout movement renders neighborhood gentrification fully capitalized into local house prices.

Oates (1969) first empirically addresses Tiebout's (1956) hypothesis. He finds that house prices capitalize both property taxes and the value of the public schools these taxes finance. Because public schools represent a locally-funded public amenity, Oates (1969) focuses on educational expenditure per pupil as a proxy for public school quality. He regresses property tax rates and educational expenditures on property values, and finds a positive correlation with the latter variable and a negative correlation with the former. Oates concludes that rational taxpayers compare the cost of public amenities, in the form of local property-tax rates, with the quality and amount of these public amenities. If the tax-financed benefits in an area outweigh the tax costs, people choose to locate there.

Because of the impact of public school outcomes on students' futures, parents pay close attention to the quality of local public education. This results in paying more for houses in better

school zones within uniform tax-rate districts. The quality of students' public education affects their later-life outcomes. One of these outcomes is the probability of incarceration. Billings, Deming, and Rockoff (2012) use the end of a race-based student assignment policy in the Charlotte, NC school district to examine the impact of public school reassignment on test scores and citywide crime rates. Students rezoned to worse-performing schools face higher probabilities of committing crimes. Given the peer-effects explanation the authors posit, it makes sense that parents willingly pay more for houses in better school catchment areas. Moreover, Deming, Hastings, Kane, and Staiger (2014) find that students placed into higher-quality secondary schools by this same Charlotte school rezoning are more likely to enroll in and complete college. These outcomes provide evidence of motivations for the parents of public school students to pay higher prices for houses in high quality public-school catchment areas.

## **1.2 Studies of House-Price Capitalization: Boundary Discontinuity Designs**

A host of other empirical studies confirm the positive association between house prices and local educational quality by using boundary discontinuity designs. This methodology assumes that houses located within a small geographic band of a school-zone border are basically identical except in school quality. Any discrepancy in the quality of the different school assignments of houses on either side of a school-zone boundary is the reason for house-price differences on either side of the boundary. The key assumption is that houses in close proximity that are only separated by different school catchment areas are alike in every other way.

Black (1999) finds a positive relationship between prices and public-school quality for houses situated along school-zone boundaries in Massachusetts. Focusing on school-zone boundaries, she claims, eliminates any confounding factors like differing tax rates, neighborhood characteristics, and public education expenditures. The only difference between houses in

Black's study is the prices parents pay for houses in different school zones. These price differences exactly reflect parents' preferences for elementary-school educational quality.

Bayer, Ferreira, and McMillan (2002) find the same result for houses in the San Francisco Bay area. Like Black, they employ a boundary discontinuity design to control for confounding factors. Their model unites the hedonic regression method of estimating house prices, introduced by Rosen (1974), with a discrete choice model of whether a person decides to relocate. In addition to the educational preferences of households along school attendance zones, the authors model household preferences for neighborhood factors like demographics, income and education levels. Their findings suggest that, based on their income levels, people pay premia to sort into neighborhoods with better educational outcomes (proxied by local test scores) and a larger percentage of people of identical race. Similarly, Downes and Zabel (2002) find that parents pay more for houses in areas with higher test scores and public school expenditures. They also conclude that using districtwide school achievement measures induces a downward bias. Estimating house price impacts from district-level data downplays parents' preferences for the racial composition of the local school their children attend.

Gibbons, Machin, and Silva (2013) add more recent weight to border discontinuity methods' bearing out Tiebout sorting in the context of public education. They use different measures of test-score improvements to answer the question of what motivates parents' movements into houses zoned for schools with higher test scores. According to the authors, who test both value-added measures and demographic composition of student bodies, it does not matter whether parents prefer public schools because of better educational quality or a more favorable compositional mix of students (which they define as schools with fewer students receiving free lunches). Their results indicate that parents are indifferent as to the specific

reasons why test scores increase. Parents simply treat all test score increases the same when it comes to paying more for houses in school zones experiencing gains in student test outcomes.

Movement between localities with different amounts and qualities of public education is also evident from the supply-side. Hoxby (2000) finds that cities with more school districts enjoy more productive public schools than cities with fewer school districts. Like previous studies, Hoxby employs a boundary discontinuity design between districts, but hers is based on topographical borders. The sorting mechanism forces greater inter-district competition among public schools when families have more public-school options. When districts consolidate or state governments exert more central control over school spending, parents tend to enroll their students in private schools and voter interest in public education diminishes. Increases in the supply of new schools lead to similar effects. Neilson and Zimmerman (2014) find a positive correlation between house prices and new elementary and middle school construction in New Haven, CT. Although the authors examine panel data on just one inner-city, socio-economically disadvantaged district's construction program, they find statistically significant, positive effects of this natural experiment on elementary and middle school student reading scores and district house prices. These effects occur after the new facilities are open and functional, though, as the authors note, there are indications that local housing markets capitalize new school construction before it is complete after public announcement of new school construction plans.

### **1.3 U.S. House-Price Capitalization Results: Natural Experiment Designs**

Many studies use natural or quasi-experimental methods to examine the causal effect on house-price capitalization of school quality. These methods use policy or legal shocks as exogenous events that directly influence house prices. Because these policy or legal changes are assumed to come from outside the housing market, endogeneity concerns do not dilute the causal argument.

A prominent example of such studies is Figlio and Lucas (2004). They conclude that families reveal their preferences for better-quality public schools by sorting into higher-priced houses assigned to these schools via an independent school-grading policy in Florida in 1999. The statewide administration of a grading system for public schools, based mostly on student test scores, allow Figlio and Lucas (2004) to characterize Tiebout movements based on schools' qualities statewide. They conclude that these "school report cards" provoke a housing market response independent of test scores and other metrics which Florida's state government used to construct the reports. These differences, however, are short-lived and only occur in the year following the reports' issuance.

In contrast to the results of Figlio and Lucas (2004), Imberman and Lovenheim (2013) find that no house price effects occur after the joint release of school and teacher value-added data by the Los Angeles Unified School District (LAUSD) and the *Los Angeles Times*. The authors characterize value-added information as being free of the endogeneity concerns associated with using average school test scores as proxies for public school quality. Imberman and Lovenheim (2013) state that test score capitalization into house prices, as a measure of parental preference for educational quality, is indistinguishable from house price capitalization of school and neighborhood attributes, like demographics and median incomes. However, the authors' estimates of a model of LA county house prices before and after a public release of value-added information show no discernible effects on house prices. Chetty, Friedman, and Rockoff (2014) find, conversely, that educator influences on student test-scores are an unbiased measure of these teachers' qualities, though unbiasedness requires controlling for students' previous test outcomes. They examine value-added test scores from two and a half million elementary school students in a quasi-experimental study and conclude that high quality teachers

positively impact students' future incomes, college-attendance rates, neighborhood SES qualities, and retirement savings rates.

Untangling the effects of school quality and sorting complicates the identification of how school quality impacts house prices. Several studies use difference-in-differences estimates to attempt to address endogeneity concerns. One example is Bogart and Cromwell's (2000) study of the effects of a school rezoning on house prices in the Cleveland, Ohio suburb of Shaker Heights. The authors identify three different effects of a suburb-wide redistricting policy: a negative "neighborhood schools" effect on house prices, a negative "racial composition" effect, and a positive "transportation services" effect. Bogart and Cromwell (2000) find the first and last of these effects to be of the correct sign and statistically different than zero. Zahirovic-Herbert and Turnbull (2007) control for sorting by examining a school redistricting in a natural-experiment framework. They study how an exogenous change in school-attendance zones in the East Baton Rouge, Louisiana parish impacts the prices and times-to-sale of affected houses, and find that people pay higher prices for houses assigned to schools with better test scores. The authors also test whether the reassignment of a house into a lower-scoring elementary school adversely affects its price, and conclude that it does not.

Nonlinearity in the relationship house prices and public educational quality are also manifest in Chiodo, Hernandez-Murillo, and Owyang (2010). They study different, non-linear test score effects on the premium parents pay for houses in better school zones. The authors use a statewide school-quality index from Missouri computed from test-score data, and focus exclusively on math-score results. They use these scores, the square of these scores, and the cube of these scores to capture potential non-linear effects on house prices. Their results indicate that as the scores on the math tests in the Missouri index increase, so does the house-price premium.

Positive effects of school quality on selling prices are highest among houses zoned for schools experiencing the largest increases in math scores. By controlling for these non-linearities, Chiodo, Hernandez-Murillo, and Owyang (2010) avoid overestimating the house-price effect of an increase in test scores for houses assigned to better-performing, sub-average schools. As the authors state, in the St. Louis metropolitan area positive shocks to house-price premia are concentrated in those houses in the highest-performing school zones.

#### **1.4 International House-Price Capitalization Results**

Studies of house-price capitalization of school quality are not confined to the U.S. Cheshire and Sheppard's (2004) study the effects of primary and secondary school test scores on local property values in Reading, England in 2000. Using a simple hedonic regression model and controlling for neighborhood confounding factors, the authors use two common test-score results from nationwide English exams administered to primary school students, age 7-11, and secondary school students, age 16, to examine the house-price premia Reading parents pay. Their major conclusions are two-fold. First, the effect of increases in test scores is larger at the secondary school level than at the primary school level. Second, because of the wider variance in primary school test-score performance, coupled with the greater number of state primary schools relative to secondary schools, the house-price effect of relocating from the lowest test-score primary school to the highest test-score primary school is larger than the same move between secondary schools. The authors also confirm that access to better quality state-administered schools is conditional on parental income. This indirect constraint operates through the housing market in the form of parental competition driving up house prices in high-performing school-catchment areas, rather than from more direct constraints like school tuition rates.

Gravel, Michelangeli, and Trannoy (2006) study the effects of a host of different public goods on house prices in a suburb northwest of Paris, France. One of these goods is public education. The authors use two metrics to proxy for educational quality: the fraction of students behind grade level and the classroom student-teacher ratio. Estimates of a non-linear hedonic model reveal positive correlations between school-quality measures and property values in the Paris metropolitan area. After taking various neighborhood factors into account, the quality and amount of school inputs and the effects of students' peers on educational performance both impact local house prices. Fack and Grenet (2010) follow up the findings of Gravel, Michelangeli, and Trannoy (2006) with a boundary discontinuity model studying the extent of the house-price capitalization of public and private school quality. Since private schools in Paris are, for the most part, publically-funded, they provide a non-location-based alternative to neighborhood public-school assignment. The authors use exam scores and the peer composition of classes to evaluate the quality of both public and private middle schools from 1997 to 2004. While their results fit well into the findings from the extensive previous literature, the authors go on to state that house-price premia depend on the quality of private school alternatives. The availability of non-residential private schools tends to attenuate property value capitalization of public-school quality. The extent of this attenuation depends on private-school quality.

Evidence of house-price capitalization of school quality is also available for Australia. Davidoff and Leigh (2008) use a boundary discontinuity design to estimate the premium houses in higher test-score districts experience relative to houses in their lower-scoring counterparts immediately across the district border. The authors focus on secondary schools in the Australian Capital Territory and on houses within 600 meters of a district border. They argue that, because of the dearth of public information regarding test-score measures for public schools around

Canberra, capitalization of test score levels into house prices may be dampened. Regardless, they find that a one-standard-deviation increase in public-school test scores increases house prices in the relevant school zone by 3.5%. For families considering whether to send their children to local private schools, the authors' results indicate that a cheaper alternative would be to purchase a house in a better public school district. The more children a family has, the greater the savings with this choice.

The causal effects of school quality on house prices can be broken down further to answer more specific questions about house prices and public educational quality. Ries and Somerville (2010) indicate that public-school quality has different effects on more expensive and less expensive houses, as measured by price per square foot of living area. They use a citywide rezoning in Vancouver, British Columbia in September 2000 to examine the causal impacts of school quality on house prices at different price levels. To control for unobserved covariates that do not change over time but still affect property values, the authors employ a repeat-sales analysis. They also control for price trends occurring in reassigned neighborhoods before the 2000 public school rezonings, and show that increases in secondary-school test scores lead to higher prices of assigned houses. But the authors also find a statistically significant negative relationship between elementary-school test scores and house prices in the assigned zones. This negative effect is a byproduct of pre-2000 neighborhood price trends. More interestingly, the positive associations between house prices and secondary-school achievement scores are confined to houses in those quartiles of price per square foot above the citywide median. Apparently, school boundary changes had little or no effect on all Vancouver residences, save for houses with relatively higher price per square foot.

### 1.5 A Case Study: Charlotte, NC School Reassignment

Many house-capitalization studies focus on the housing market in Charlotte, NC. Kane, Reigg, and Staiger (2006), for example, use changes to Charlotte Mecklenburg School (CMS) zone boundaries 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. By doing so, they combine the two approaches of a natural experiment, albeit not a citywide one, and a boundary discontinuity design. Up until 2002, CMS altered school zones as needed to keep the student demographic percentages in public schools within a 15 percentage point band of Mecklenburg County's racial population percentages. The authors use data from 1994 to 2001 to explore the effects of these school reassignments on house prices. They conclude that there is a house-price premium for schools located in better school zones, but changes in neighborhood characteristics following school reassignments are what really drive this premium. Residential sorting in response to school-zone shifts is the cause of the effect of changing school quality on house prices.

Kane, Reigg, and Staiger (2006) is not the first empirical work to address the effects of school-quality and racial composition on Charlotte house prices. Jud and Watts (1981) control for both educational quality and the ratio of non-white to white students in each school using a hedonic house-price model. Like previous researchers, they study CMS because of the court-ordered desegregation of public schools by busing and the school-zone boundary redrawing that occurred every three years. Both were mandated by the 1971 Supreme Court decision *Swann vs. Charlotte-Mecklenburg Board of Education*. Jud and Watts (1981) indicate that a grade-level increase in achievement scores increased average house values from 5 to 6%. More importantly, the authors find that failing to control for school quality results in a bias that magnifies the

negative effect of a higher non-white student ratio on the prices of houses in the originally assigned zone.

In 2002, after 41 years of public school reassignment policy based on racial quotas and carried out by student busing, a court order mandated CMS drop its race-based busing policy. CMS rezoned some neighborhoods into different school zones, but left other neighborhoods' school assignments unchanged. Moreover, parents had a limited opportunity, via a school assignment lottery, concerning which public school they wanted their children to attend.

Hastings, Kane, and Staiger (2006) found that public schools scoring higher on statewide test scores induced parents with the financial wherewithal to purchase houses in better-performing school assignment zones. Conversely, lower-scoring public schools do not face the demand-side effects of Tiebout movement into better public school zones. Their assignees do not have the means to relocate. This implies that Tiebout movement, as outlined in the empirical findings of Oates (1969) and others, was at play after the 2002 citywide CMS school reassignments. This was true even though a school lottery system CMS alleviated some demand-side pressures on poor families.

## **1.6 Conflicting Results on House-Price Capitalization**

Most studies find a positive correlation between house price and school quality, but some find no evidence to support house-price capitalization of school quality. Some researchers employ different proxies for school quality (other than statewide test scores); some researchers find no statistically significant, or even negative, associations between house price and test scores; and some researchers focus on non-traditional houses. For instance, Brasington (1999) finds that not all school-quality proxies provide identical insights into house price capitalization. He examines several such measures, and finds that many of them fail to provide consistently predictable

effects on house prices, using either standard hedonic regressions or spatial autocorrelation-corrected regressions. Based on results that use thirty-seven school quality measures across six metropolitan areas, the author concludes that the only consistent school-quality proxies that have statistically significant effects on house prices are proficiency test scores. Value-added measures, teacher education levels, and graduation rates are not associated with higher house prices. However, Brasington (1999) notes that expenditure per pupil or student/teacher ratios may affect performance scores and, indirectly, house prices. Finally, teacher salaries and student attendance rates are associated with higher house prices, but the effects are sensitive to the presence of controls for spatial autocorrelation.

There are a few studies that find increases in public amenities either decrease or have no effect on property values. Brueckner (1979) assumes perfect information and a frictionless real-estate market to derive a bid-rent model of the impacts of public-goods provision on property prices, independent of sorting. He uses data from New Jersey and finds that public goods are provided in excess of what he terms their “efficient levels” (levels above local citizens’ preferences for these amenities). Clapp and Ross (2004) also find no significant relationship between house prices and towns’ (and schools’) racial composition. The authors study Connecticut towns between 1995 and 2000, and use a simultaneous equations framework to jointly model racial composition, school test-score performance, and house prices. Demographic changes in towns and schools across Connecticut do not result in consistent house price changes directly related to such movements, nor do these movements have any effect on public-school performance. Furthermore, eighth-grade math test scores, a proxy for school quality, do not provide any explanation for house price changes. The authors find that only the amount of public

money allotted to local schools, and not those schools' test-score performances, has any association with house prices in their sample.

If parents of public school children pay premia for increases in the quality of local schools, does this house price capitalization spill over into properties not usually purchased by such families? Gallagher, Kurban, and Persky (2013) provide evidence that people without school-aged children do not pay premia for residences in higher-quality school zones. The authors define "small homes" as houses with two bedrooms or less. These house owners pay the same property tax rates as families with public-school students. The authors employ a boundary discontinuity design, but single out for analysis small homes across school zone boundaries. Additional premia paid by families for large houses (i.e., houses with over two bedrooms) are not evident in small-home prices. Indirectly, the authors' results lend credence to the argument that movement into neighborhoods assigned to high-quality public schools and the resulting price competition for houses in these neighborhoods are the reasons house price premia exist. Although they do not pay house price premia, home owners who are not likely to have public school children still incur a transfer of their local property-tax dollars to families with children in public education. Kurban, Gallagher, and Persky (2012) focus on these education-based tax redistributions. Households without public-school children, a portion of which are older households whose children no longer attend public schools, pay more local public school taxes than house owners with public-school students. It is these parents and students who reap most of the benefits because the amount of property tax they pay does not cover the value of the benefits they receive. These results suggest that, over the course of a household's lifetime property-tax payments, younger and older households without children in public schools pay more into the local property-tax pool than they receive in benefits. However, during mid-life households

accrue more benefits than the amount of property tax they contribute. As a result, Gallagher, Kurban, and Persky (2012) posit that households' benefits and taxes for publically-funded education are minimally redistributive.

### **1.7 Arbitrage Opportunities for House-Price Capitalization of Neighborhood Amenities**

Much of the research on public-school qualities' impacts on house prices describes a close association between school attributes and those of the surrounding neighborhoods. The central issue is to separate the impacts of neighborhood variables and school quality on house prices. If the quality of neighborhoods, independently of their schools, has a large impact on house prices, then measurable attributes of the neighborhoods themselves are priced into property values. This establishes a link between neighborhood amenities, like high-quality public education, low crime rates, high median household incomes, and other such examples, and the prices of the houses in them. If this is true, then arbitrage opportunities exist for underpriced houses in neighborhoods that experience increases in the quantity and quality of local amenities, both public and private.

### **1.8 House-Price Speculation as Irrational Exuberance**

House-price speculation conditional on quantifiable changes in neighborhood attributes drives up house prices in areas where these prices do not yet reflect rising neighborhood quality. This speculation is rational because it reflects movements in local price determinants. However, house-price speculation, like any type of speculative purchasing, has the capacity to generate positive feedback on itself. This feedback may raise prices higher than the underlying fundamentals warrant.

Shiller (2007) outlines this possibility in the context of the U.S. housing market. He examines several price trends in house-price determinants over time. These fundamentals include real rental prices and construction costs. He compares these to the 86% increase in the U.S.

national average house price in the decade between the mid-1990s and the mid-2000s. Shiller states that the massive increase in the average U.S. house price is not explainable in terms of underlying fundamentals. Case and Shiller (2003) conclude that American house prices over the period exhibited a house-price “bubble,” or positive feedback cycle. This occurred because the rising price of houses in the U.S. market led to widespread speculation that house prices would be greater tomorrow than today. The breakneck rise in the average house price was not a byproduct of underlying factors of supply, like labor costs or the costs of building materials. Rather, it was a result of increasing speculative activity, termed “market psychology” by Shiller (2007). Market psychology takes hold when people’s expectations that house prices will continue upwards lead them to purchase houses in the present to sell at a date in the near future. The problem is that people fail to condition their house-price expectations on rational price signals from the housing market.

A more complete picture of a house-price bubble is painted by Depken, Hollans, and Swidler (DHS) (2011). They list three phases of a self-reinforcing price increase in the Las Vegas housing market over the period beginning in 1994 and ending in 2009. The first phase DHS identify is the speculative phase. During the onset of a price bubble, more houses are bought and sold for speculative purposes, or, to use the more popular term, “flipped.” Flipping quickly raises house prices. The flipping phase is followed by an end to price increases, as flipping activity subsides and speculative house-holders become unable to sell their properties at prices higher than what they originally paid. This effect spreads and eventually degenerates into the third phase: the bubble’s “burst.” Foreclosures set in due to house owners owing more on their mortgages than their houses’ present market values. The increasing number of foreclosures results in falling house prices as a market correction fully sets in. DHS (2009) examine house

prices in the context of arbitrage opportunities for flipping behavior. They use, as a definition of a house flip, two or more transactions involving the same property occurring in less than two years. Their data range from 1994 to 2007 in the Las Vegas metropolitan area, and the authors find that one-third of their sample fits their definition of a flipped house. After deducting opportunity costs from a nominal measure, DHS (2009) find a 20% return from a typical flip. The collapse of the speculative boom, however, drove economic profits to zero by 2007.

The process of demand-driven price increases in a housing market bubble is met, in some locations, with a corresponding lack of increase in the supply of housing. The failure of a housing market to clear may reflect decisions by city governments to block increases in housing supply. Fischel (2004) characterizes areas with decentralized city governments as being hostile to increases in housing stocks and to local economic development, generally. As his realm narrows and his ballot counts more in local elections, the median voter in such circumstances makes choices that reflect more closely his own narrow interests. This means that the median voter favors local policies that drive up the cost of his house at the expense of those around him. Correspondingly, the median voter votes for stringent development bylaws, via more zoning ordinances governing new owner-occupied and rental construction in his locality. Price increases based on voter-induced shortages in housing supply may then fuel the positive feedback loop of a price bubble. This leads to house-price losses for all city voters after the bubble's burst drives down prices citywide. Glaeser, Gyourko, and Saks (2005) arrive at the same conclusion. They examine house prices nationwide and find that from 1950 to 2000 both the average house price and house price variance increased. Most notably, house prices in the upper 90% of the nationwide price distribution rose from 35% higher than the median in 1970 to over 100% higher

than the median in 1990. The authors rule out other factors and favor the argument that regulatory constraints drive up house prices.

### **1.9 House-price speculation as Rational Foresight**

The speculative effects in much of Case and Shiller's (2003) and Shiller's (2007) work and in the aforementioned DHS (2009, 2011) studies are different than the consequences of people rationally speculating on house prices based on changes in fundamentals. Fundamentals include increases in the amount and quality of neighborhood attributes. When, for example, neighborhoods experience unexpected gentrification, there are rational reasons for arbitrage opportunities to exist for speculative house transactions.

The rationality of certain types of speculative house transactions has its roots in inter-urban discrepancies in house prices. House price changes within metropolitan areas can be as diverse as house price movements across these areas. Guerrieri, Hartley, and Hurst (2013) support this idea with Case-Shiller Price Index data at the zip code level in 30 U.S. cities. They find evidence for what they term "endogenous gentrification," which happens when a neighborhood experiences demographic change that alters neighborhood amenities. More succinctly, endogenous gentrification results from having wealthier neighbors. Because people tend to prefer living around others with similar incomes, formerly poorer neighborhoods that abut richer neighborhoods experience endogenous gentrification during a positive, citywide house-price shock. The influx of wealthier home buyers into poorer, bordering neighborhoods expands richer neighborhoods and subsequently increases the value and type of local amenities in these wealthier areas. Houses in poor areas bordering wealthy neighborhoods provide arbitrage opportunities because of their heightened sensitivity to house-price shocks relative to houses in other city neighborhoods.

Other neighborhood characteristics besides location may result in rising property values. O'Sullivan (2005) points to a decrease in local crime rates as a major factor contributing to neighborhood gentrification. He outlines a model in which low-income city dwellers compete for space in the inner city, and presents empirical results from Portland, Oregon that back up his theory. Areas with higher rates of crime are populated by lower-income people, the author posits, because they have lower costs of living in high-crime areas. On the other hand, decreases in crime rates attract higher-income residents to previously low-income, high-crime neighborhoods, resulting in gentrification. The effect feeds back on itself over time. Low-income earners continue to move out, crime rates continue to fall, and house prices in gentrifying neighborhoods continue to increase. As more local amenities appear in gentrifying neighborhoods, competition between providers forces amenity prices to fall. Because prices for local amenities are lower than at the onset of the high-income influx, wealthier buyers further bid up house prices in gentrifying areas.

### **1.10 Recognizing Rational Speculation as a Profit-Maximizing Opportunity**

In the midst of price increases, lending or mortgage-granting entities must identify speculation that is rationally based and separate it from speculation that simply reflects price feedback. Identifying neighborhoods in which speculators rationally expect house prices to increase minimizes the lenders' exposure to the risk of rapid house price declines. It transfers economic profits from speculators to mortgage-guaranteeing agents. In other words, lenders and mortgage-guarantors have incentives to charge different house prices or interest rates to rational flippers than to price-fueled flippers. Identifying patterns in measurable qualities of neighborhoods that rational flippers key on affords house financiers insight about who to charge what price or interest rate.

Informed speculators make up one of the three types of traders that Kyle (1985) outlines in his model of sequential auctions with insider trading. Informed traders, who have private information concerning trade liquidity initially unavailable to other types of traders, earn profits by using their monopoly power over this information to optimally time their trades. They use “noise” traders<sup>1</sup> to conceal their status as informed traders from “market makers.”<sup>2</sup> Discovering who is an inside trader and who is a noise trader, based on their patterns of trade, results in economic profits accruing to market makers, who would otherwise earn zero profits, rather than to informed traders. Identification by market makers costs informed traders their previous informational monopoly. This is why informed traders seek to camouflage themselves among noise traders. By appearing to trade on noise, insider traders are, in fact, maximizing expected profits by gradually incorporating their private knowledge about trades into prices. If many trades occur, informed traders quickly incorporate private information. Informed traders disguise private information as noise trading more effectively in thick markets: those with a high number of trades. The opposite is true in thin markets where the number of trades is low.

In her assessment of the subprime mortgage market, Kregel (2008) says that private financing entities prefer housing finance because of its perceived safety of return without undue risk of loss. This is true even after the savings and loan crisis of the 1980s, which was the result of the federal government’s removal of a law mandating that bank demand deposits pay zero interest. Financial institutions issue mortgage-backed bonds as tradable assets. Unfortunately, mortgages, as Kregel states, are not uniform. Bonds backed by mortgages differ from other company-issued debts because the intentions of the mortgage borrower are unknown. Debt issued by financial institutions and backed by the mortgages of people with unknown incentives

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<sup>1</sup> Those not endowed with private or “inside” information.

<sup>2</sup> Those who efficiently set trading prices conditional on trades with both informed and noise traders.

is necessarily risky. If financial players issuing these bonds, like investment banks, can mitigate foreclosure risk by identifying irrational house speculators, then these players' profits from mortgage-backed bond sales become more secure. Banks distinguishing uninformed speculators, who are gambling that house prices will continue upward, from informed speculators, who take out mortgages to pay for houses in areas where shifts in underlying fundamentals lead to permanent increases in average house prices, generates safer returns to stockholders.

Usurping profits from informed speculators, or flippers, in the housing market requires recognizing them as either informed or uninformed. Since flippers have an incentive to disguise the information they possess by masking their actions during upsurges in speculative house buying and selling (akin to noise trading in Kyle's (1985) work), identifying house price flippers as distinct from non-flippers can be a difficult task. However, specific attributes of houses may be used in hedonic regressions to identify which house and neighborhood attributes informed speculators value. If a metropolitan area experiences prices increasing in line with specific neighborhood amenities, such as those outlined by research like O'Sullivan (2005) and Guerrieri, Hartley, and Hurst (2013), there is an increased probability that flippers know something about gentrification prospects in this locale. Unfortunately, hedonic regressions that include neighborhood covariates along these lines are notoriously lacking in statistically significant results. Hedonic methods are also subject to model misspecification since there is little or no theoretical basis for which independent variables to include in house price regressions.

### **1.11 Hedonic Modelling Shortfalls: Frequentist Issues, Bayesian Solutions**

To deal with these methodological problems, Atkinson and Crocker (1987) couch their hedonic regressions in Bayesian, rather than frequentist, terms. They label previous hedonic studies troublesome for two reasons. First, the inclusion of additional explanatory variables affects the

bias/variance trade-off. This leads to instability in coefficient estimates across model specifications. Second, measurement errors in one or more variables infect other, collinear variables in hedonic regressions. The authors employ Bayesian methods, first brought to prominence by Leamer (1978) and Leamer and Leonard (1983), to examine and alleviate these problems. Succinctly, the method is to make a number of prior assumptions and calculate posterior estimates over a range of these priors, then use the subsequent range of posterior means to determine which hedonic covariates are robust to prior beliefs of varying magnitudes. The authors' findings indicate that uncertainty (defined as a range for the posterior mean range that includes zero) in structural variables<sup>3</sup> is negligible, but uncertainty in neighborhood variables is large. Remedies include either reducing the number of uncertain covariates in hedonic regressions or, because collinearity breeds uncertainty about which variables to include, using non-zero informative priors in a Bayesian context.

Atkinson, Crocker, and Shogren (1992) use the latter remedy to overcome collinearity issues, and investigate the amount of Bayesian transferability between hedonic property studies. They define Bayesian transferability as the existence of a common model that generates random samples from seemingly diverse groups. Because there is commonality between groups from an underlying data-generating process, the authors examine whether and to what extent previous studies' estimates describe new, unstudied locations. To do so, they use Leamer's (1982) bounded variance prior technique to establish a range of credible prior covariance beliefs. Since these beliefs are subjective, the authors vary covariance ranges after incorporating new site data and prior mean estimates from other studies into a Bayesian OLS estimation. Most hedonic variables are not robust to different prior covariance specifications, with living area, property tax

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<sup>3</sup> Independent hedonic variables related to house build, style, and interior/exterior dimensions. For example, total heated living area or the number of bathrooms a house contains.

rate, and time on the market being the only three covariates that are robust to any range of covariance priors. Another way of stating this is, unless there is a general consensus about what most researchers believe the specific prior covariance matrix in a new study should be, most posterior estimates of house attributes on selling price do not benefit from better-specified priors. Atkinson, Crocker, and Shogren (1992) conclude that including more data from a specific housing market, rather than sharper prior coefficient information from previous studies, improves the accuracy of posterior coefficient estimates. There is apparently more transferability of empirical results across different estimates of the effects of structural variables, indicating a unitary market for these amenities, than across estimated effects of neighborhood variables. Posterior estimates of neighborhood effects show little or no transferability between individual housing markets.

### **1.12 Conclusion**

The consensus from the voluminous literature concerning house prices and local amenities is that house prices consistently capitalize these amenities across different statistical techniques, locations, and time periods. School quality, for the most part proxied by test scores, is a prime example of a neighborhood amenity people are willing to pay for indirectly via higher house prices. The literature confirms that price capitalization causes houses reassigned to better school zones to increase in value. Whether this is because of school quality value-added or parents' preferences for schools' racial compositions is a thornier issue. Whatever the reason, correctly predicting where this house price appreciation will occur yields opportunities for speculative house buyers. So long as speculators enter the housing market in neighborhoods with appreciating amenities before house prices have fully capitalized these amenities, chances for short-term profit exist. Once gentrification has set in, however, economic profits disappear.

Speculative activity may be identifiable based on measurable neighborhood amenities. Because hedonic estimates of the effects of these variables are notoriously fragile and location-specific, though, Bayesian estimation methods may help to shed light on which locational attributes informed speculators associate with oncoming gentrification.

## CHAPTER 2

### **Public High-School Quality and House Prices: Evidence from a Natural Experiment in Charlotte, NC**

#### **2.1 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 public goods outright, variations in property taxes 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.

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, Ferreira, and McMillan (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 (2007) 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, houses in those neighborhoods reassigned by CMS to better schools are predicted to experience positive price shocks relative to houses in 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 prospective buyers to consume lower public school quality.

Kane, Reigg, and Staiger (2006) use pre-2002 changes in CMS 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.<sup>4</sup>

This chapter contributes to the existing literature by using this large-scale natural 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, Kane, and Staiger 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 Charlotte 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 treatments 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

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<sup>4</sup> On April 15, 2002, the Supreme Court refused to hear CMS's appeal, thereby upholding the original ruling to end busing.

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, Deming, and Rockoff 2013). 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 for each 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, Hernandez-Murillo, and Owyang (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 that negative price shocks occurred for houses CMS reassigned to lower-quality public high schools.

## **2.2 Data**

I use 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 anytime 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.<sup>5</sup> 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 footage, 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.

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<sup>5</sup> 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).

I separate these data 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<sup>6</sup>. Table 2.1 contains the average selling price and other summary statistics. It also contains data on EOC test passing-rate averages and differences.<sup>7</sup>

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.

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<sup>6</sup> I clean the data by eliminating observations with nonsensical values: negative ages, houses over 200 years old, and houses with zero bedrooms, more than six or no full bathrooms, lot sizes over ten acres, or selling prices less than \$1,000.

<sup>7</sup> EOC passing-rate data come from the North Carolina Department of Public Instruction website.

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 9<sup>th</sup> and 10<sup>th</sup> 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 attrition of the least-able students becomes an issue.

Table 2.2 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 expect 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 2.3 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 changes in aggregated sale-price averages in all neighborhoods before and after the April 15, 2002 Supreme Court decision and in both treatment and control neighborhoods.

Figure 2.1 depicts annual EOC test passing rates for houses in positively treated and control neighborhoods. Before April 15, 2002, control-neighborhood EOC scores are lower than in 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 2.2 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 initially 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 dissipates after the Supreme Court decision.

Figure 2.3 presents monthly average sale 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 average sale prices for control-group houses are above those for positively treated houses. However, after the April 15, 2002 Supreme Court decision, positively treated and

control house prices tighten, and house price averages in positively treated neighborhood begin overtaking house price averages in control neighborhoods. Eventually, almost all the monthly averages of sale prices in positively treated neighborhoods lie above their control-group counterparts.

Figure 2.4 plots average sale prices for houses in negatively treated neighborhoods and houses in control neighborhoods. The pattern in sale-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 the prices of houses reassigned to lower-quality high schools relative to non-reassigned houses.

Figure 2.5 minimizes the monthly averages, and price-trend plots for positively treated and control houses. 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 what became 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 sorting into homes that CMS reassigned into better schools. Further, the slope of the post-2002 price trend for positively treated houses 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.

Conversely, graphical evidence of price capitalization of school quality is not apparent from the plot in Figure 2.6 of the price trends for negatively treated and control houses. There is almost no change in the trend line for the prices of 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 suggests that there were 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 better catchment areas. This makes sense considering, as the next section details, these households had a chance to win a school choice lottery and gain admittance to a higher quality school within CMS without having to move and pay a price premium to do so.

## **2.3 Estimation Strategy**

### **2.3.1 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 August 1, 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 the 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 August 1, 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.<sup>8</sup> As Vanderbilt School of Law Centennial Professor James Blumstein stated, "To say anything is clear in this area is an overly optimistic statement" (Charlotte Observer April 14, 2002).

Given the legal background of the court case from 1997 to 2002, Charlotte homeowners' expectations of the Supreme Court decision would have been even less certain than the legal experts'. All of this uncertainty dissolved on 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

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<sup>8</sup> Another reason for a lack of confident predictions before the April 15, 2002 Court decision was that only four of nine justices must vote affirmatively for the case to be taken by the Supreme Court.

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 court 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 (Charlotte Observer April 16, 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, Kane, and Staiger 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 house 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 (Charlotte Observer February 28, 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 (Charlotte Observer February 28, 2002). Parents had until March 22, 2002 to appeal reassignment decisions.

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 (Charlotte Observer March 13, 2002).

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-2002S schools, regardless of previous reassignment decisions (Charlotte Observer April 11, 2002). 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 (Charlotte Observer April 16, 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 students in subsequent years. The odds of being denied entry versus gaining a seat at these preferred high schools ranged from 11:1 to 43:1 by the 2003-2004 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 (Charlotte Observer March 23, 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 high school options parents could choose for their children (Charlotte Observer October 27, 2005). The school board removed 75% of county high schools from its reassignment options list due to over-subscription (Charlotte Observer November 22, 2005). The only remaining options were high schools that were 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 (Charlotte Observer January 1, 2006). There was no longer an incentive for parents to participate

in the 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 the likelihood of a house being reassigned to a better or worse school on three alternative school-quality measures. 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 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 catchment areas with better schools.

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 receiving a 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 marginal effect of a one-standard-deviation increase in the seven-test composite EOC score was 6.1% and 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 treatment 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.

### 2.3.2 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

$$y_{ijt} = \gamma(\text{rezone}_j) + \theta(\text{post2002}_t) + \delta(\text{rezone}_j * \text{post2002}_t) + X'_{ijt}\beta + Z_t\varphi + \epsilon_{ijt} \quad (2.1)$$

where  $y_{ijt}$  is the demeaned log of the average selling price of house  $i$  in neighborhood  $j$  and year  $t$ ,  $\alpha$  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),  $X_{ijt}$  is a vector that contains house and/or neighborhood characteristics,  $Z_t$  is a vector that contains year fixed effects, and  $\epsilon_{ijt}$  is a random error term. The coefficient  $\delta$  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 examine 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

$$y_{ijt} = \pi(\Delta EOC_{j,2001}) + \theta(post2002_t) + \tau(\Delta EOC_j * post2002_t) + X'_{ijt}\beta + Z_t\varphi + \epsilon_{ijt} \quad (2.2)$$

All variables are defined as before, with the exception of the newly added variable  $\Delta EOC_j$ . This variable represents 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 in this variable because there is no difference in passing rates. Rezoned neighborhoods register non-zero values for this variable because EOC passing rates change with 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 rates 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.

## **2.4 Empirical Results**

### **2.4.1 Initial Results**

Column 1 of Table 2.4 contains the results of estimating equation (2.1) 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 the estimates of a model containing hedonic house variables and year fixed effects.

The results from estimating equation (2.1) for houses in negatively treated neighborhoods are reported in the first column of Table 2.5. 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 estimating the difference-in-differences model provides no evidence that people pay less for houses reassigned into zones with worse schools.

I then replace the rezone dummy variable with a measure of the difference between positively-treated neighborhoods' 2001 EOC passing rates. The first column of Table 2.6 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 households price in the size of the school-quality increase when purchasing (and selling) a house.

The first column of Table 2.7 provides estimates using data on houses in negatively treated neighborhoods. The model contains the interaction of 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 rezoned into worse-performing schools. Even after I account for EOC passing rate differences, the conclusion is the same as with the treatment indicator variable: no statistically significant effects on house price of negative treatment.

These results are similar to those for East Baton Rouge, LA (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 estimates for negatively treated neighborhoods, however, were imprecisely estimated, as they are here.

As striking as these results are, they may 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 it dramatically

limited school choices in the lottery (and draws occurred only 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 (Charlotte Observer October 26, 2005). 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 their parents won the yearly lottery without 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, Hastings, Kane, and Staiger 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, 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 of 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 2.4 and 2.5 contain repeat-sales sample estimates from equation (2.1). Like the full-sample estimates, the repeat-sales-sample treatment effects are not statistically significant, with or without hedonic household-level variables and year fixed effects. The second columns of Tables 2.6 and 2.7 present results that resemble those from the full-sample regressions. The price of a positively treated house rises \$3,313 for each 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-based 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 treatments defined by changes in EOC scores, on the other hand, always have positive and statistically significant effects on house prices of roughly the same magnitudes. There is little evidence, once again, of negative treatment effects. By controlling for the magnitude of the school-quality shock, it becomes clear that families relocated

based on how much school quality they gain or lose as measured by the difference in school EOC passing rates.

#### **2.4.2 Non-Linear Treatment Results**

Following Chiodo, Hernandez-Murillo, and Owyang (2010), I test for non-linear treatment effects. To do so, I use split-sample regressions by dividing the EOC-based 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 25<sup>th</sup> quantile of the distribution of EOC passing-rate differences. This bin represents houses that experienced a small positive or negative school-quality shock. The “Middle” bin includes EOC passing rate differences from the 25<sup>th</sup> to the 75<sup>th</sup> 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 75<sup>th</sup> quantile of the 2001 EOC test-score difference distribution.

As Table 2.8 shows, the top bins of the EOC-based treatment drive most of the regression results reported in 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 treatments to houses in the “Top” bin are all positively signed, statistically significant, and resemble the results of Chiodo, Hernandez-Murillo, and Owyang (2010). The prices of houses experiencing the largest increases in school quality reflect positive premia for better schools. 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 2.6. Houses treated with small- or medium-sized increases in EOC passing rates display no price effects.

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 prior to the 2006-2007 school year. The estimated 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 occurred after there was no longer the mitigating impact of a lottery to win admittance to any public school in Mecklenburg County.

Table 2.9 presents split-sample estimates of EOC-based treatment effects for houses in negatively treated neighborhoods. The evidence is much weaker than that reported in Table 2.8. The only statistically significant treatment effect has the correct sign, but it is sample-specific, and therefore is not generalizable.

### **2.4.3 Heterogeneity Test: Condominium Prices**

As a further check on the robustness of my results, I regress condominium prices on the same sets of covariates and treatments I used previously. 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, Kurban, and Persky (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, the matching procedure 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-bedroom condominiums), families with public school students may reside in some of the sample properties. Any bias in this direction, though, works in favor of finding a statistically insignificant 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 insignificantly different than zero<sup>9</sup>.

The estimating equation includes fewer structural attributes of condominiums than the model for detached 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 the inclusion of both bathrooms and bedrooms provides less variation. Condominiums vary by size, but few have more than two bathrooms or three bedrooms.

Table 2.10 provides the results of estimating the variant of equation (2.1) using data on the selling prices of condominiums in Charlotte from 1997 to 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 for their

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<sup>9</sup> 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, 2015). While I cannot identify foreclosed houses in my MLS sample, omitting foreclosures induces a bias toward finding statistically non-significant treatment estimates.

household counterparts. Condominium purchasers have different preferences than house purchasers in the data, and this may be why the estimated coefficients on characteristics like “new” and “beds” have different signs than the coefficients on these variables in the house regressions.

Results from estimating equation (2.2) are reported in Table 2.11. The estimates are nearly identical to those of Table 10. The only statistically significant effect from a change in school EOC scores occurs for negatively-treated condominiums, and it is only marginally statistically significant. There is scant consistent evidence for a negative treatment effect for either houses or condominiums.

## **2.5 Conclusion**

The results reported here indicate that houses receiving positive, EOC-measured 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 (2.1). Simply put, Charlotteans paid premia for houses reassigned to higher quality high schools, given that the quality increased substantially. 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 moving to a house in a higher performing school’s neighborhood. However,

after CMS took away 75% of the high schools parents could apply for, including all of the public high schools with the highest 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 beyond a threshold level of increases 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. Estimates from both full and repeat-sales samples of MLS data confirm that, although the results vary in magnitude, they are qualitatively consistent: the marginal effects of an increase in the 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, Zietz, and Sirmans (2008) and Wang and Liao (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 substantially better than the previously assigned school.

A comparison with average private school tuition rates sheds light on the willingness to pay 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 relatively cheaply into a neighborhood with a better-performing school 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 timeframe following the 2002 Supreme Court decision to estimate more precisely 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.

2.6 Tables and Figures

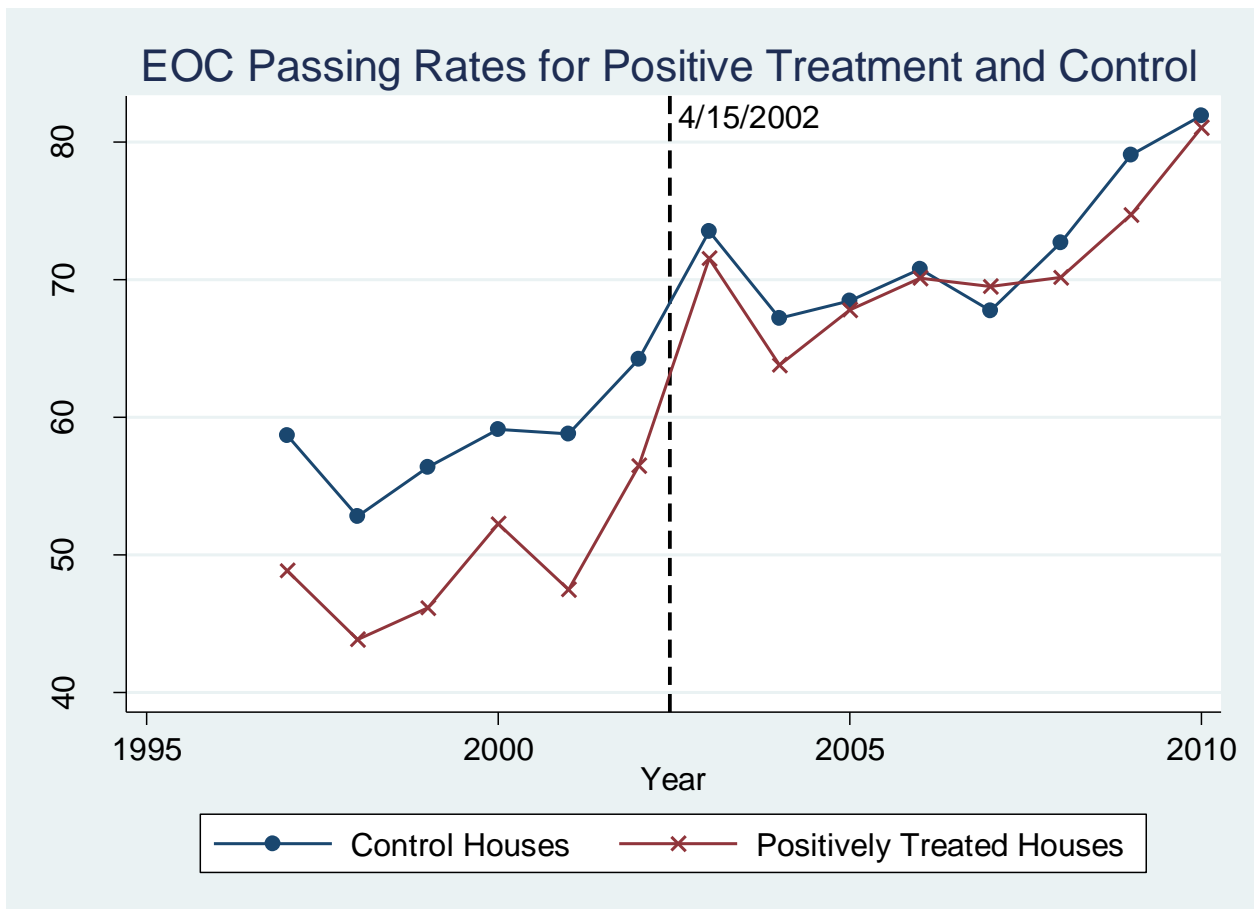
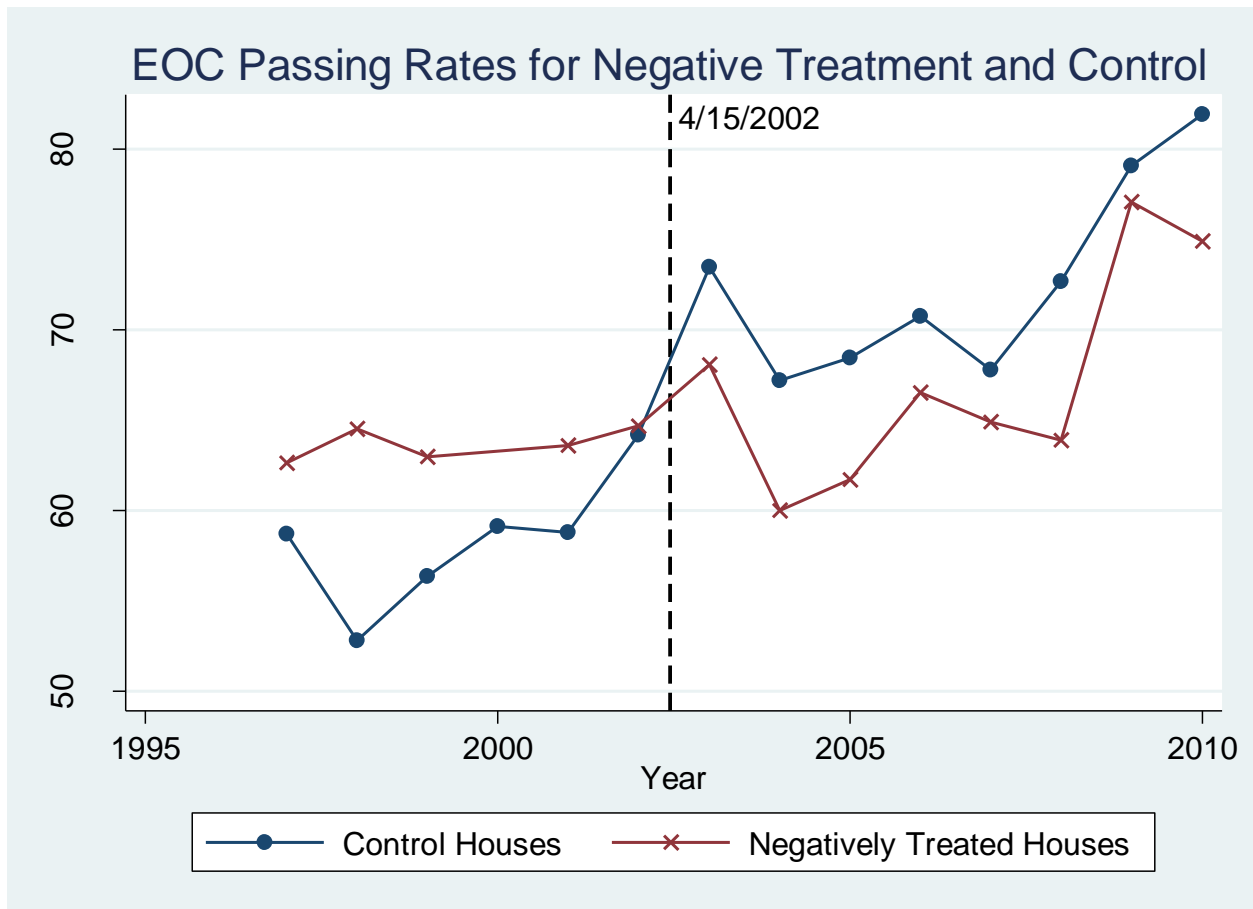
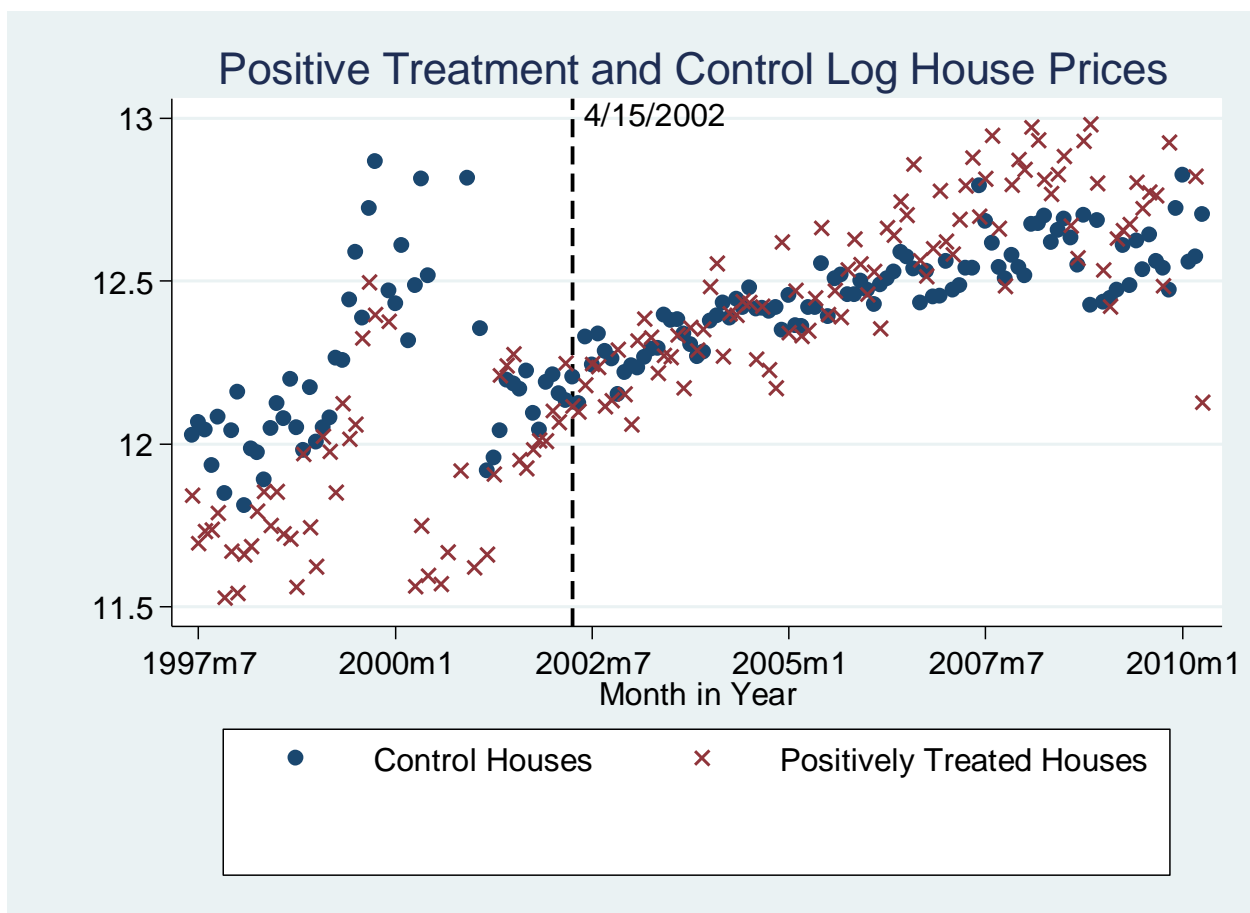


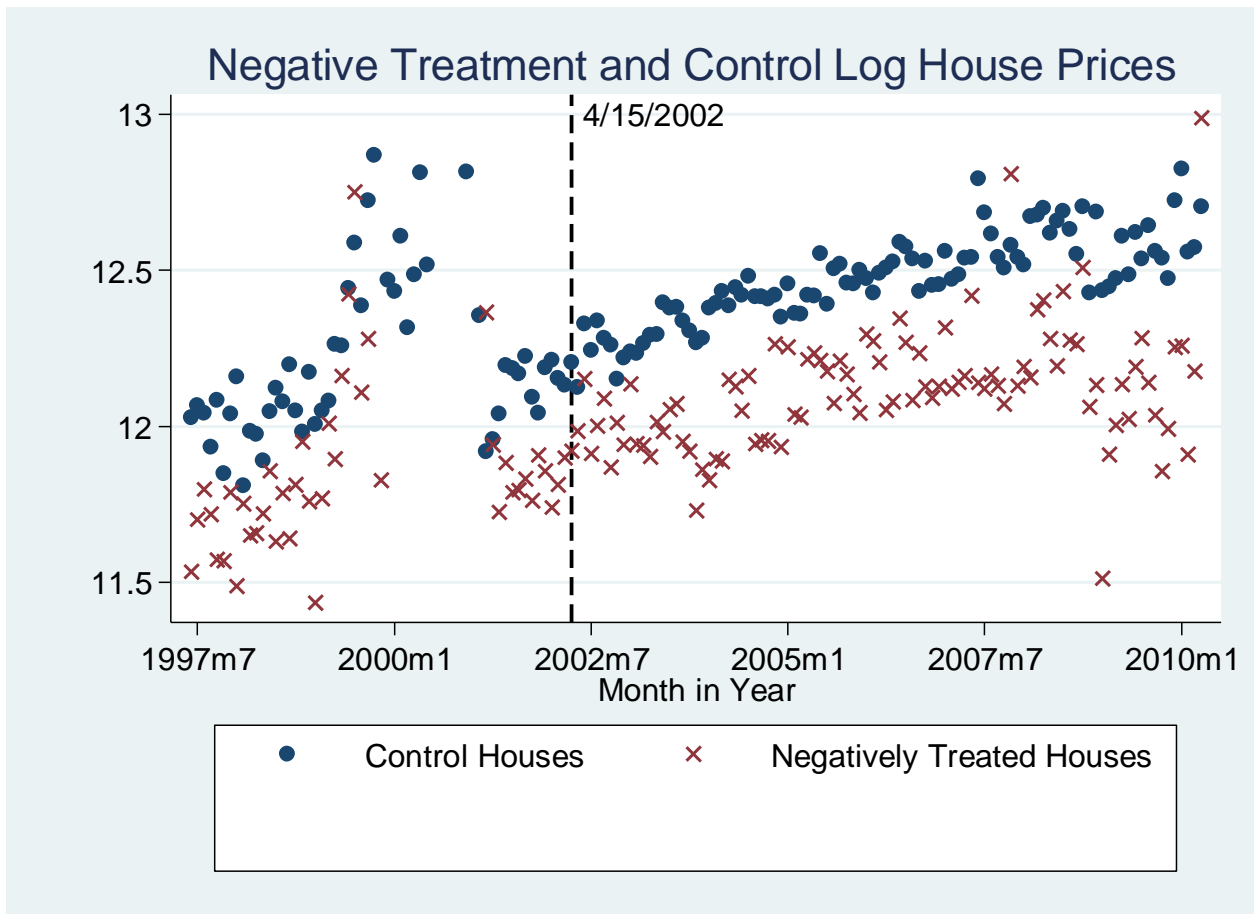
Figure 2.1: EOC Passing Rates for Positively Treated and Control Houses



**Figure 2.2: EOC Passing Rates for Negatively Treated and Control Houses**



**Figure 2.3: Log House Sale Prices of Positively Treated and Control Houses**



**Figure 2.4: Log House Sale Prices of Negatively Treated and Control Houses**

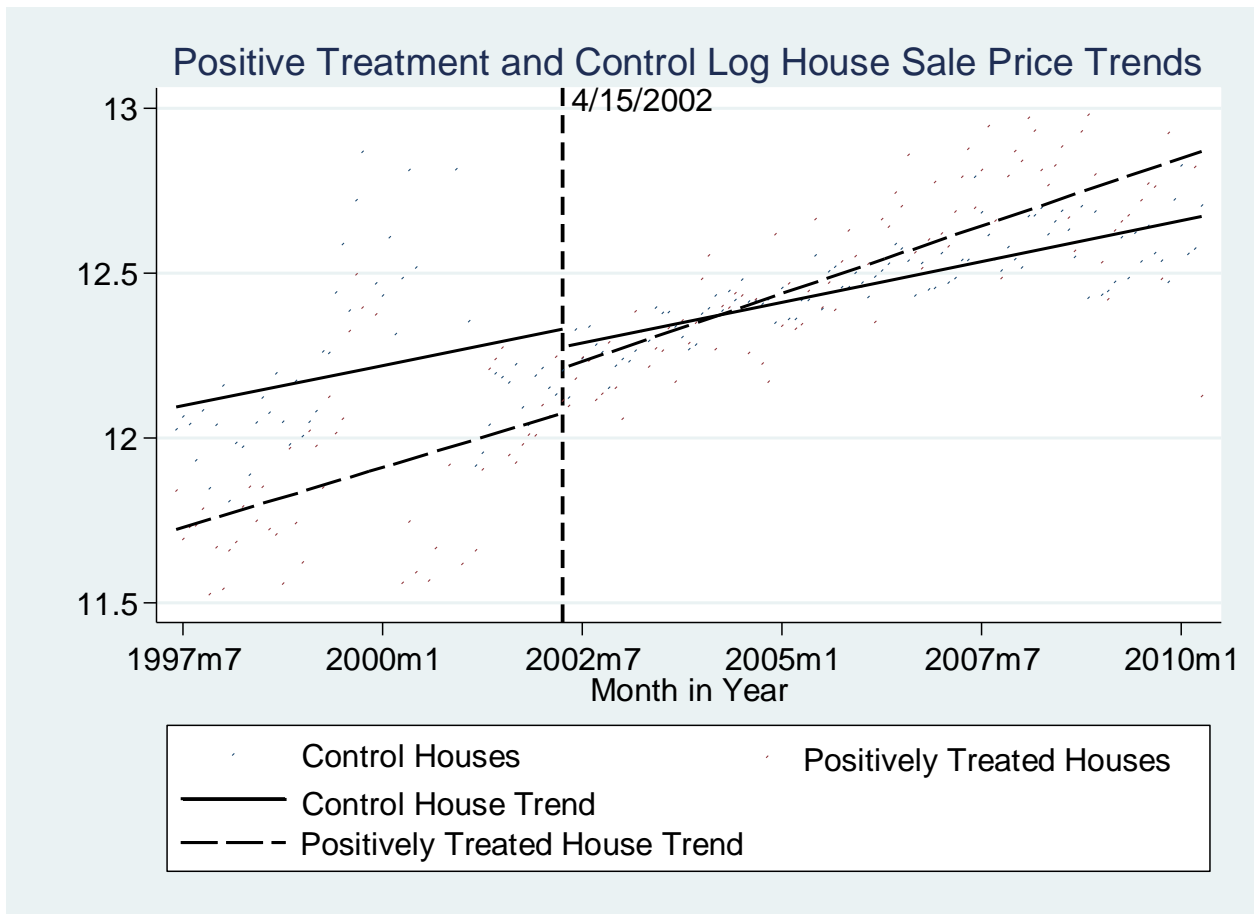
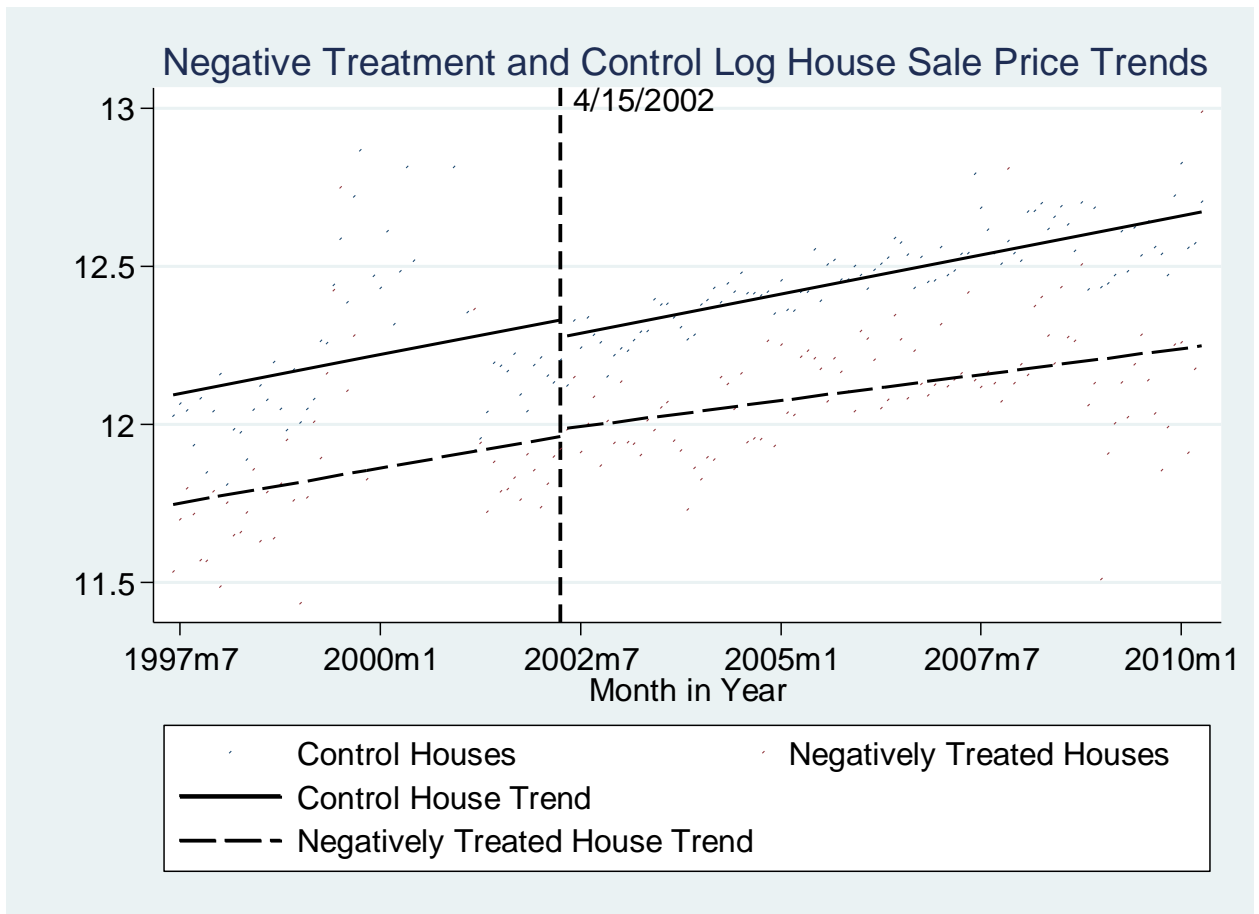


Figure 2.5: Trends in Log House Sale Prices of Positively Treated and Control Houses



**Figure 2.6: Trends in Log House Sale Prices of Negatively Treated and Control Houses**

**Table 2.1: Summary Statistics**

	<b>N</b>	<b>Mean</b>	<b>SD</b>	<b>Min</b>	<b>Max</b>
<b>House Attributes</b>					
House Price <sup>†</sup>	44,729	309,372.1	260,133.4	10,909.1	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 <sup>2</sup>	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
<b>School Attributes</b>					
% Pass EOC	43,833	66.9	12.3	26.7	95.4
$\Delta$ EOC <sub>2001</sub>	43,348	3.0	10.9	-37.7	34.5
$\Delta$ EOC <sub>2001</sub> (Positive)	10,292	17.5	11.6	3.16	34.5
$\Delta$ EOC <sub>2001</sub> (Negative)	4,530	-11.4	10.0	-37.7	-1.0

<sup>†</sup>Prices in 2010\$

**Table 2.2: Average EOC Passing Rates by Subject and School, 1996-2002**

<b>High School</b>	<b>English I</b>	<b>Algebra I</b>	<b>Biology</b>	<b>Average</b>
D.W. Butler	62.8	40.0	66.2	56.3
E.E. Wadell	50.1	29.9	54.9	43.3
East Meck.	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 Meck.	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 Meck.	71.6	35.7	64.4	57.2
West Charlotte	39.4	15.5	37.1	30.7
West Meck.	54.9	53.5	45.8	51.4
Z.B. Vance	67.5	26.7	58.7	51.0

**Table 2.3: 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

\*Prices in 2010\$

**Table 2.4: The Effect of the 2002 CMS Rezoning on Log Selling Price of Positively Treated Houses**

	Full Sample† (Std. Errors)		Repeat-Sales Sample (Robust Std. 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 <sup>2</sup>		.0002*** (3.01e-5)		.0003*** (2.96e-5)
% neigh. over 64				
dropout rate				
% on food stamp				
HH med. inc. (\$1000)				
viol. crime rate				
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

†Indicates standard errors clustered at neighborhood level. \*, \*\*, \*\*\* indicate  $p < .10, .05, .01$

**Table 2.5: The Effect of the 2002 CMS Rezoning on Log Selling Price of Negatively Treated Houses**

	Full Sample† (Std. Errors)		Repeat-Sales Sample (Robust Std. 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 <sup>2</sup>		.0002*** (3.68e-5)		.0004*** (3.62e-5)
% neigh. over 64				
dropout rate				
% on food stamp				
HH med. inc. (\$1000)				
viol. crime rate				
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

†Indicates standard errors clustered at neighborhood level. \*, \*\*, \*\*\* indicate  $p < .10, .05, .01$

**Table 2.6: The Effect of Changes in EOC Passing Rates on Log Selling Price of Positively Treated Houses**

	Full Sample† (Std. Errors)		Repeat-Sales Sample (Robust Std. Errors)	
post2002	.0292 (.0457)	-.0064 (.0138)	.0372 (.0241)	.0766* (.0433)
$\Delta$ EOC <sub>2001</sub>	.0018 (.0061)	.0014 (.0033)	.0059*** (.0018)	.0019 (.0014)
(post2002* $\Delta$ EOC <sub>2001</sub> )	.0090*** (.0031)	.0045*** (.0014)	.0079*** (.0026)	.0052*** (.0018)
acres		.2614*** (.0349)		.2819*** (.0402)
beds		.2346*** (.0131)		.2559*** (.0134)
baths		.4035*** (.0202)		.3210*** (.0166)
half-baths		.3227*** (.0240)		.3517*** (.0545)
new		.0200 (.0284)		-.0252 (.0255)
age		-.0073*** (.0024)		-.0137*** (.0022)
age <sup>2</sup>		.0002*** (2.61e-5)		.0003*** (2.94e-5)
% neigh. over 64				
dropout rate				
% on food stamp				
HH med. inc. (\$1000)				
viol. crime rate				
constant	12.41*** (.0515)	10.26*** (.0926)	12.49*** (.0170)	10.52*** (.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

†Indicates standard errors clustered at neighborhood level. \*, \*\*, \*\*\* indicate  $p < .10, .05, .01$

**Table 2.7: The Effect of Changes in EOC Passing Rates on Log Selling Price of Negatively Treated Houses**

	Full Sample† (Std. Errors)		Repeat-Sales Sample (Robust Std. Errors)	
post2002	.0384 (.0463)	.0120 (.0132)	.0493** (.0251)	.0573 (.0442)
$\Delta EOC_{2001}$	.0143*** (.0053)	.0018 (.0038)	.0086*** (.0028)	.0027 (.0044)
(post2002* $\Delta EOC_{2001}$ )	-.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 <sup>2</sup>		.0002*** (4.14e-5)		.0004 (3.66e-5)
% neigh. over 64				
dropout rate				
% on food stamp				
HH med. inc. (\$1000)				
viol. crime rate				
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

†Indicates standard errors clustered at neighborhood level. \*, \*\*, \*\*\* indicate  $p < .10, .05, .01$

**Table 2.8: The Effects of Non-Linear Changes in EOC Passing Rates on Log Selling Price: Positively Treated Houses**

	Bottom <sup>‡</sup> (Std. Errors)	Middle <sup>‡</sup> (Std. Errors)	Top <sup>‡</sup> (Std. Errors)
Full Sample <sup>†</sup>	-.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

<sup>†</sup>Indicates standard errors clustered at neighborhood level. \*, \*\*, \*\*\* indicate  $p < .10, .05, .01$ .

<sup>‡</sup>Bottom, Middle, and Top represent the bins of  $<25^{\text{th}}$ ,  $25^{\text{th}}-75^{\text{th}}$ , and  $>75^{\text{th}}$  quantiles of the differences in 2001 EOC passing rates between positively-treated neighborhoods' pre-2002 and post-2002 high school assignments.

**Table 2.9: The Effects of Non-Linear Changes in EOC Passing Rates on Log Selling Price: Negatively Treated Houses**

	Bottom <sup>‡</sup> (Std. Errors)	Middle <sup>‡</sup> (Std. Errors)	Top <sup>‡</sup> (Std. Errors)
Full Sample <sup>†</sup>	.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

<sup>†</sup>Indicates standard errors clustered at neighborhood level. \*, \*\*, \*\*\* indicate  $p < .10, .05, .01$ .

<sup>‡</sup>Bottom, Middle, and Top represent the bins of  $<25^{\text{th}}$ ,  $25^{\text{th}}-75^{\text{th}}$ , and  $>75^{\text{th}}$  quantiles of the differences in 2001 EOC passing rates between negatively-treated neighborhoods' pre-2002 and post-2002 high school assignments.

**Table 2.10: 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 <sup>2</sup>		.0002*** (4.15e-5)		.0002*** (5.30e-5)
constant	11.52*** (.0553)	11.27*** (.1986)	11.52*** (.0554)	11.21*** (.1986)
F(k, N-k)	2.65	44.20	2.15	76.02
N	161	161	141	141

\*, \*\*, \*\*\* indicate  $p < .10, .05, .01$ . Huber-White std. errors.

**Table 2.11: 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)
$\Delta\text{EOC}_{2001}$	.0039 (.0032)	.0103*** (.0022)	-.0165*** (.0057)	.0097* (.0054)
(post2002* $\Delta\text{EOC}_{2001}$ )	.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 <sup>2</sup>		.0002*** (4.17e-5)		.0002*** (4.97e-5)
constant	11.50*** (.0533)	11.21*** (.1933)	11.52*** (.0555)	11.14*** (.2244)
F(k, N-k)	4.81	44.49	7.76	67.57
N	161	161	137	137

\*, \*\*, \*\*\* indicate  $p < .10, .05, .01$ . Huber-White std. errors.

## CHAPTER 3

### **The Non-Hedonics of Property Values: Price Prediction, House Flipping and Bayesian Transferability**

#### **3.1 Introduction**

Before the 2008 recession, Robert Shiller (2007) characterized the 86% nationwide house price increase between 1996 and 2006 as inexplicable in terms of economic fundamentals. Housing market factors like real rental prices and construction costs showed no signs of such a substantial increase over the same time period. Shiller argued market psychology caused the run-up and subsequent crash in house prices. He also labelled the house price boom a positive feedback cycle, or “bubble” (Case and Shiller 2003).

Because house prices rapidly appreciated during a speculative bubble, arbitrage opportunities existed. House buyers had an incentive to purchase houses at discounted prices and quickly sell those properties, thus ensuring a swift economic profit. This behavior became more prevalent as the positive feedback loop drove house prices ever upward (Depken, Hollans, and Swidler (DHS) 2011). DHS (2011) found that, in 2004, house price flipping in Las Vegas produced a nominal return of 60% over the holding period. The authors defined flipped houses as houses that sell twice or more in two years or less. Even after adjusting for opportunity costs, a typical flip at the height of the bubble produced a return of 20% before economic profits reverted to 0 three years later (DHS 2009).

Over a longer time horizon, in contrast, abnormal returns from house price flipping were not evident from other data. House prices in Collin County, Texas displayed no differences between flipped and non-flipped houses according to a repeat-sales price index. English (2005) studied the same groups of flipped and non-flipped houses in 2001 and 2005. According to his hedonic estimates, English (2005) concluded that there was not sufficient statistical evidence to determine whether flipped and non-flipped houses sold at different prices over a longer period.

If the conclusion in English (2005) is to be believed, then what is to be said about Shiller's (2006) assessment and the empirical work of DHS (2009, 2011)? Must we also discount the empirical work of Wheaton and Nechayev (2008)? They conclude that changes in macroeconomic variables affecting house price fundamentals, like population, income, and interest rates, do not explain nationwide forecast errors between real and predicted house prices from 1998 to 2005. Or do the quality and size of housing attributes, neighborhood factors, or local zoning ordinances play larger roles in determining house prices than previously thought (Fischel 2004; Glaeser, Gyourko, and Saks 2005)? In short, is there some connection between hedonic values and house price flipping? Do discrepancies in the hedonic price estimates of otherwise uniform house attributes predict whether the house is being flipped?

Behind the questions listed lies the central issue about the data being used to investigate them. Do housing data contain information about the motivations of house transactors? Moreover, can these motivations be identified using only the hedonic attributes of houses involving different types of house transactors? If so, house characteristics may predict the type of transactors involved in a housing transaction. This is the topic I address in this chapter. I consider two types of house transactors: flippers and non-flippers. The former comprise the speculative

element seeking to exploit an arbitrage opportunity in the housing market, and the latter are traditional house buyers looking to live in the properties they acquire.

First, I use simple hedonic regressions on data from flipped and non-flipped houses to estimate the impacts of the neighborhood and structural attributes of these houses on their selling prices. This approach follows the work of Rosen (1974). I use the DHS (2009) definition of flipped houses: houses transacted twice or more in two years or less. DHS define flipped houses this way because of a provision in the federal tax code allowing the capital gains from a house sale to be excluded from taxable income if the house seller identifies the property as his primary residence for any two of the past five years. From these simple log-linear regressions, I find small but imprecisely estimated differences in the magnitudes of the coefficients on the hedonic variables affecting flipped and non-flipped house prices.

As a model validation exercise, I use a method resembling Edward Leamer's (1978) SEARCH (Seeking Extreme and Average Regression Coefficient Hypotheses) procedure to examine the effects on the extreme bounds of these hedonic coefficient estimates after introducing more observations on either flipped or non-flipped houses. In doing so, I examine the transferability of the hedonic results between information types. Do differences in flippers' and non-flippers' house attribute preferences hold up across studies and with the addition of new data? Atkinson, Crocker, and Shogren (1992) introduce this technique as a means of testing whether different, site-specific estimates can be incorporated as priors to predict accurate posterior estimates for house prices in a new market. They conclude that, while more prior information sharpens the posterior estimates, prior information cannot replace accurate data collected from the new site.

Price data on both flipped and non-flipped houses may or may not sharpen the range of posterior attribute estimates. But, because of consistent differences in the house attribute preferences of flippers and non-flippers, I conclude that there is an argument in favor of the conclusions that house flippers make rational decisions to speculate on house price appreciation in gentrifying areas. There is evidence that flippers prefer to purchase houses in high violent-crime-rate areas in an attempt to quickly reap economic profits by selling these houses as these neighborhoods begin to gentrify. Moreover, this behavior is predictable from neighborhood attribute data and is robust across a range of locational studies and prior beliefs. This information can potentially be used to identify different types of house buyers for purposes of third-degree price discrimination.

### **3.2 Motivation**

If a mortgage originator can identify flipping house buyers and non-flipping house buyers, he can price discriminate between the two. Third degree price discrimination already occurs in direct transactions between house sellers and identifiable types of buyers. Bayer, et al. (2012) conclude that house sellers of all races price discriminate against black and Hispanic house purchasers. These buyers pay a house price premium of around 3% relative to white buyers. The authors maintain ambivalence, but posit that the premium exists because of the potential inabilities of black and Hispanic house buyers to secure mortgages, the inexperience of these buyers in house price negotiations, and/or higher search costs incurred by black and Hispanic house buyers. Price discrimination may not lead to a loss of aggregate consumer surplus, in some cases, because it entails an increase in revenues for sellers. This is the result Leslie (2004) finds after examining types of price discrimination in Broadway theater ticket sales. Consumer surplus

does not decline relative to pricing tickets uniformly, but Broadway theaters' revenues increase if they charge different prices to different types of theater patrons.

It may be the case that a mortgage originator's ability to identify flipping versus non-flipping house purchasers simply transfers the temporary economic profits reaped by flipping house buyers to itself. These profits come from informed house flippers exploiting private information about the respective neighborhoods in which they purchase houses. If O'Sullivan's (2005) findings are any indication, house flippers have an incentive to purchase houses in high crime areas that they predict will gentrify in the near future. As Kyle (1985) points out in the context of traders, there are informed investors who maximize profits using private information. Flippers purchasing houses in high crime areas can be characterized this way: they know or have good reason to believe that houses in these areas are apt to experience abnormal increases in prices as crime rates fall and public and private amenities increase in quality and number when higher-income individuals relocate to these areas. If mortgage originators can distinguish house flippers from non-flippers, these originators can transfer speculative economic profits to themselves by price discriminating against house flippers. Alternatively, mortgage originators may be able to lower rates for or grant more loans to non-flipping house purchasers relative to flippers, if these originators can third-degree price discriminate. This could entail more secure profits for mortgage guarantors with no subsequent loss of consumer surplus (Kregel 2008).

The central issue for mortgage-issuing entities is to determine who is a house flipper and who is a non-flipper. Differences in preferences for house and neighborhood attributes play a role, particularly with respect to the latter. Because mortgage originators frequently require myriad personal information from prospective buyers, including a listed address and proof of sufficient income to pay the mortgage, the originator has a detailed profile of the applicant for a

house loan. Also, property transactions are a matter of public record. It stands to reason that a mortgage originator could use an online resource<sup>10</sup> or some other way of gaining access to house transaction records to determine whether the applicant owns any other properties or has purchased a number of properties in the recent past.

In addition, the guarantor also has information regarding the property the applicant wishes to purchase. Using hedonic attributes of the house and its neighborhood, an originator can parse out whether an applicant's personal information fits the profile of a person purchasing the house to flip it or to occupy it for a longer time period. Key indicators may include whether the person is purchasing a house in a high crime area and owns another house or has a job whose median income is higher than the median income of the high crime area. A mortgage originator can access all of this information via standard mortgage applications and public records. Crucially, the originator could then use this information to evaluate which applicants are house price flippers, and, if so, which areas these flippers believe are likely to experience house price increases through gentrification in the near future.

### **3.3 Methods**

To gain a sense of the differences between house-buyer hedonic preferences for flippers and non-flippers, I use simple linear regression on both of these data sub-sets. I regress the natural log of selling price on eleven covariates in common use throughout the hedonic property value literature. These variables are a mix of structural house and neighborhood attributes. Covariates unique to the property are the number of bedrooms, number of bathrooms<sup>11</sup>, house total heated

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<sup>10</sup> All property transactions in Charlotte and Mecklenburg County, NC are available via the online Mecklenburg County Real Estate Lookup System. All fifty states and the District of Columbia have similar, publically-available property transaction records.

<sup>11</sup> I define this variable as the total number of half and full bathrooms per house.

living area, house lot size, and house age. Neighborhood variables consist of the percentage of the neighborhood population over the age of 64, percentage on food stamps, high school dropout rate, median household income, and violent crime rate<sup>12</sup>. I use school average passing rates on the North Carolina state high-school examinations (EOC or “End of Course” exams) as a measure of neighborhood educational quality. A new variable, not prevalent in the literature, that I introduce is an indicator variable for neighborhoods experiencing gentrification. A gentrifying neighborhood is defined as one that goes from the bottom half of the house price distribution in 1997 to the top half of the distribution in 2010. I adopt this definition from Daniel Hartley’s (2013) work identifying the effects of gentrification in major urban areas.

A model validation exercise based off of Leamer’s (1982) SEARCH technique provides a check of my OLS results with those having a Bayesian flavor. This process incorporates a technique from Leamer and Leonard (1983) that Atkinson and Crocker (1987) and Atkinson, Crocker, and Shogren (1992) apply to hedonic property studies that involves scaling an arbitrary prior covariance matrix to glean insights about the robustnesses of coefficient posterior estimates in the face of different information types. Since there is not a specific theory about which house attributes to include in a hedonic regression and which not to include, the house covariates in property valuation models are essentially arbitrary.

The first part of the technique requires a researcher to pick “focus” and “doubtful” variables. Leamer and Leonard (1983) use a simple linear equation to outline this strategy,

$$y_t = \beta x_t + \gamma_1 z_{1t} + \gamma_2 z_{2t} + u_t \quad (3.1)$$

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<sup>12</sup> This is defined as violent crimes per 100,000 of a neighborhood’s population relative to the citywide average violent crimes per 100,000 of the population.

where  $x_t$  is a vector of variables researchers commonly use, dubbed “focus” variables. Examples include total living area, number of bedrooms, and other (mostly structural) house attributes. The variables  $z_{1t}$  and  $z_{2t}$  represent variables not commonly used by researchers. Examples of these include school test scores, median household incomes, or other less common measures of neighborhood or household quality. These represent “doubtful” variables.

Equation (3.1) contains only two doubtful variables, rendering it beneficial to estimate four regressions: those including or not including permutations of the doubtful parameters. However, simply estimating four regressions prevents a researcher from employing any prior restrictions he suspects may pertain to the sizes and/or signs of the doubtful variables. Specifically, a researcher widens his search by including whatever prior beliefs he deems necessary for the doubtful variables. He does so by defining a composite variable

$$w_t(\theta) = z_{1t} + \theta z_{2t} \quad (3.2)$$

Here,  $\theta$  represents any priors the researcher may want to impose and, as such, it is a continuous variable. The composite variable now allows prior beliefs outside of the four discrete combinations of doubtful and focus variables (Leamer and Leonard 1983; Atkinson and Crocker 1987).

Given the Bayesian flavor of Leamer’s analysis, posterior mean estimates for each of the doubtful and focus variables can be calculated by using Raiffa and Schlaifer’s (1961) equation

$$\hat{\beta} = E(\beta|y, A, H) = (A + H)^{-1}(A\bar{\beta} + Hb) \quad (3.3)$$

where  $y$  is an  $(Nx1)$  vector of dependent variable observations,  $\beta$  is a  $(Kx1)$  vector of unknown coefficients,  $\bar{\beta}$  is a  $(Kx1)$  vector of prior means,  $A$  is a  $(KxK)$  prior precision matrix,  $b$  is a  $(Kx1)$

vector of least squares sample estimates, and  $H$  represents the  $(K \times K)$  sample precision matrix known up to a scalar transform. Equation (3) follows from the normal linear model

$$y = x\beta + u \quad (3.4)$$

$$y \sim N(x\beta, \sigma^2 I_N) \quad (3.5)$$

In this case,  $x$  is an  $(N \times K)$  sample design matrix, and  $u$  is a normally-distributed, mean-zero error term with variance,  $\sigma^2$ . The researcher uses the sample design matrix to calculate

$$H = \sigma^{-2}(x'x) \quad (3.6)$$

Prior means for the focus variables may be taken from other studies or set equal to zero, and priors for the doubtful variables may be calculated by OLS after assuming diffuse priors for all variables. In other words, full statistical weight goes to the sample estimates (Atkinson and Crocker 1987). The researcher must also enter the prior precision matrix,  $A$ .

Since there are almost no situations where a researcher may assume this prior precision matrix is given, he should establish credible bounds for a range of prior precision beliefs. This is because bounds on the prior precision matrix imply bounds on the posterior mean estimates. Chamberlain and Leamer (1976) establish this fact by showing that, so long as the sample precision matrix,  $H$ , is known, posterior means are constrained to lie within an ellipsoid with its center at the midpoint of a line segment joining ellipses of the prior mean,  $\bar{\beta}$ , and the sample mean,  $b$ , with a boundary including both. The extreme values of the estimated posterior means,  $\hat{\beta}$ , lie in an ellipse of the form

$$(\hat{\beta} - c)'H(\hat{\beta} - c) \leq \frac{(b - \bar{\beta})'H(b - \bar{\beta})}{4} \quad (3.7)$$

where  $c = \frac{\bar{\beta}+b}{2}$ . Any point lying inside (3.7) represents the posterior mean estimate of a corresponding prior precision matrix.

To credibly test the robustnesses of coefficient posterior estimates, a researcher varies the bounds on his prior covariance matrix. By expanding or contracting the arbitrary covariance priors, the researcher may get a sense of which posterior estimates are robust to differing prior covariance specifications (Leamer and Leonard 1983). Leamer (1982) uses this prior covariance scaling method to test the results of McManus's (1981) study of the death penalty's effect on state murder rates. He finds that, as he expands his prior covariance matrix beliefs about executions' effects on murder rates, the data yield posterior estimate bounds that eventually switch signs. Because the posterior bounds' ranges include the origin, the author concludes that, contrary to McManus (1981), the data do not yield robust estimates of the negative effect of executions on murder rates.

Atkinson, Crocker, and Shogren (1992) use the same Bayesian technique to discover whether site-specific results of environmental benefits transfer between locations. Because a new assessment of an unstudied site involves costs, the authors examine whether coefficient estimates from a number of similar studies may be employed to mitigate the need for collecting new site-specific data. They calculate posteriors of the form

$$\hat{\beta} = (x'x + \lambda A)^{-1}[(x'x)b + \lambda \bar{\beta}_i] \quad (3.8)$$

Here,  $A$  is a properly-scaled prior precision matrix<sup>13</sup> and  $i$  denotes benefit estimates from  $i = (1, \dots, N)$  previous site studies. The information from all previous site studies is incorporated

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<sup>13</sup> Properly scaled is defined as a prior precision matrix in which all weight goes to the sample precision of focus variables and doubtful variables' precisions equal 0. This follows from the exchangeability assumptions of Lindley

in the prior coefficient matrix,  $\bar{\beta}_i$ . Equation (8) calculates benefit estimates at the new (N+1) site more efficiently than either the sample vector or prior mean vector,  $xb$  or  $\bar{\beta}_i$ , alone. Recalling Leamer (1982), the prior precision matrix is completely arbitrary and, so, it is set to  $A$  times a scalar,  $\lambda$ . This represents the Bayesian weights the researcher allocates to either the sample data from the (N+1) site or the collection of prior estimates from former studies.

By expanding and contracting the bounds on the prior precision matrix<sup>14</sup>, Atkinson, Crocker, and Shogren (1992) test the extent to which priors from previous studies affect the accuracy of hedonic property price predictions. By weakening their prior precision assumptions via  $\lambda$ , the authors evaluate whether the use of priors from different site-specific studies reduce or expand the extreme bounds of their posterior estimates. If priors reduce the bounds, the authors claim the level of Bayesian exchangeability<sup>15</sup> is strong and a unified hedonic market links past studies with the new site. If the extreme bounds of the posterior estimates grow larger after the introduction of previous study results as priors, the authors state that sample estimates from other studies have no bearing on the new site, exchangeability is weak, and more data need to be collected from the new site to improve the precision of site-specific estimates.

I employ a strategy similar to Atkinson, Crocker, and Shogren (1992). The difference is that, rather than expanding the bounds in the face of new prior mean information, I take the priors as given and evaluate extreme posterior bounds when different types of hedonic information are introduced. As Atkinson, Crocker, and Shogren (1992) conclude, more data collected from a new site improves the precision of the estimates (i.e. shrinks the bounds) to a

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and Smith (1972), the classes of prior precision matrices outlined in Chamberlain and Leamer (1976), and the methodology of Atkinson, Crocker, and Shogren (1992).

<sup>14</sup> That is, by expanding and contracting  $\lambda \in (0, \infty)$

<sup>15</sup> The ability of previous studies to represent the equivalent of a new random data set from the (N+1) site

greater extent than the introduction of more prior information. Western and Jackman (1994) urge using a similar strategy to test the robustness of Bayesian posterior estimates. Since prior beliefs are subject to the judgments of individual researchers, these authors fix their non-zero prior coefficient estimates of the impact of logged labor force size and economic concentration on union density. They check the sensitivities of their posterior estimates to their informational priors by widening the prior variances, thereby weakening prior influence on posterior coefficients. For my study, I introduce different types of information in the form of flipped and non-flipped hedonic data, separately. This allows me to see which posterior estimates' extreme bounds grow more precise as I weaken my prior precision matrix. The results identify discrepancies between house flippers' and non-flippers' attribute preferences. I can use direct empirical differences between hedonic attributes' bound results following the introduction of more data of each type to predict the housing preferences of flippers and of non-flippers.

### **3.4 Data**

The data I use for my sample estimates and extreme bound calculations come from the Charlotte, NC and surrounding-area Multiple Listing Service (MLS). The MLS is an online database containing data on house prices from a collection of postings by realtors, mortgage brokers, property appraisers, and other real-estate professionals. The Charlotte-area MLS contains price and attribute data on houses listed anytime in the past fourteen years. Because it is a rolling aggregate, the 2014 MLS would include data back to 2000.

My data pertain to houses sold from 1997 to 2010. Only houses listed on the MLS are in these data. The data contain three categories of house characteristics: historical, structural, and locational. Historical variables include the year of construction and the selling date. Structural

variables include square footage, numbers of bedrooms and bathrooms, total heated living area, lot size, and house age. Locational attributes consist of a house's neighborhood, address, and school zone. As a measure of school quality, I use EOC test passing rates for a neighborhood's assigned high school, which I collected from the North Carolina Department of Public Instruction website.

For neighborhood attributes associated with the houses in my sample, I use the Charlotte Quality of Life (QoL) study. The QoL collects information on demographics, crime rates, economic indicators, educational factors, and other measures of neighborhood quality at the level of Neighborhood Statistical Areas (NSAs). NSAs are established neighborhoods throughout the Charlotte metropolitan area. QoLs are produced semi-annually and agglomerate information from such city agencies as the Charlotte-Mecklenburg Police Department, Charlotte-Mecklenburg Schools, the Charlotte Zoning Department, and others. These reports more accurately capture neighborhood factors than are available with Census Block-level data.

Because I use Depken, Hollans, and Swidler's (2009) definition of flipping, I draw from the MLS data a repeat-sales sample for neighborhoods listed in QoLs through 2010. This leaves me with a total of 3,724 houses that were sold twice or more from 1997 to 2010. Table 3.1 presents sample summary statistics.

As Table 3.1 shows, the number of flipped houses and the number of non-flipped houses each comprise roughly half of the data. I clean the data by eliminating observations with nonsensical values, such as negative house age, zero bedrooms, or a total heated living area less than 800 square feet for detached, single-family residences. This reduces the sample size by 114 observations. The EOC passage rate variable also has a lower number of observations than

structural house attributes. This is due to the construction of a new public high school in Charlotte prior to the 2001-2002 school year. Because it was brand new at the time, this high school would have had no EOC scores before 2001.

### 3.5 Results

I first run simple OLS regressions of the natural logarithm of selling prices on house and neighborhood hedonic variables. Table 3.2 contains least-squares coefficient estimates. House flippers pay a premium for houses, but the estimated coefficient is not precisely estimated. This may indicate the short-term profit opportunities available to house flippers. Another interesting feature of Table 3.2 is the positive and statistically significant coefficient associated with a neighborhood's violent crime rate. Apparently, houses in areas with high violent crime rates sell at a premium. It may be that O'Sullivan (2005) was correct; rational house flippers target houses in neighborhoods with high but falling violent crime rates. Decreasing crime rates presage oncoming gentrification. Understanding this, flippers purchase houses in high crime rate areas and sell them to non-flippers moving in. The influx of non-flippers leads to neighborhood gentrification and the extirpation of speculative profits. Because I control for flipping, the predicted sign on the violent crime rate coefficient estimate is negative. This is because from 1997 to 2010, Charlotte experienced an average annual decrease in the number of violent crimes of 4.2%<sup>16</sup>. This non-intuitive result is possibly due to collinearity among some of the hedonic variables. Such results are why I employ Bayesian methods to estimate neighborhood posterior coefficient estimates.

OLS regression results for separate sets of flipped and non-flipped houses in the MLS data are reported in Table 3.3. Attribute coefficients for statistically significant variables are of

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<sup>16</sup> I calculate this figure using yearly violent crime totals from the North Carolina Department of Justice's online Uniform Crime Reports. These are available at: <http://www.ncdoj.gov/Crime/View-Crime-Statistics.aspx>.

the same sign in both sub-sets. However, for the number of bathrooms and dropout rates, coefficient estimates are almost double in magnitude for flipped houses relative to non-flipped houses. This evidence seems to suggest that house flippers value higher numbers of bathrooms and lower high-school dropout rates more than do non-flippers. Since a higher number of bedrooms is negatively correlated with house prices, after controlling for total heated living area, flippers may prefer buying and selling larger houses with more bathrooms rather than more bedrooms. A larger number of bedrooms may imply a duplex or shared house, while a larger number of bathrooms may be indicative of a larger, one-family house. Flippers may also be targeting houses in better school zones to buy and sell to families looking to take advantage of a higher-quality neighborhood school. Lower dropout rates are indicative of high EOC-passing rate schools, academically-qualified neighborhood students or, likely, both.

All of the structural hedonic coefficients' estimates are statistically significant for non-flipped houses. Flipped house hedonic estimates are statistically significant in the case of bathrooms, total heated living area, and age. This indicates that non-speculative house transactors value the unique attributes of a house more highly than house price flippers. Household median income is the only attribute that is not statistically significant for both flipped and non-flipped data sub-sets. Here, also, violent crime rates are positively associated with logged house prices in both sub-sets.

Since mortgage rates, interest rates, and other macroeconomic variables affect all Charlotte housing in roughly the same manner, it stands to reason that changes in such house-price fundamentals<sup>17</sup> do not have idiosyncratic effects on the prices of flipped and non-flipped houses. Charlotte is also not a "glamorous urban area" or notable vacation destination, as Shiller (2006) puts it. He argues that these areas were the most prone to speculative house price

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<sup>17</sup> See Wheaton and Nechayev (2008)

appreciation; that is, appreciation based on a price bubble rather than on changes in underlying market fundamentals.

Are the differences in OLS estimates transferable across studies or simply a random feature of the Charlotte, Mecklenburg MLS sample? To help resolve this question, I employ a variation of the SEARCH procedure. Incorporation of prior mean coefficient estimates from other studies with sample estimates clarifies which coefficient estimates are robust to different types of transactions data. This sheds light on differences in hedonic preferences between flipping and non-flipping transactors.

Table 3.4 presents the prior means I use for the focus variables. I choose to treat all structural variables as focus variables and neighborhood variables as doubtful variables. I do so based largely on the findings of Atkinson and Crocker (1987) and Atkinson, Crocker, and Shogren (1992). Both studies find that neighborhood covariates' posterior estimates are less precise than structural covariate posteriors and change radically with small changes in their priors. I take each focus variable prior as the median of nineteen hedonic studies' results. These papers all estimate standard hedonic regressions of the impact of house attributes on their logged selling prices. Doubtful variables' prior means are the OLS estimates of these variables after assuming diffuse priors for all variable coefficients. To maintain unit consistency across studies, I scale coefficient estimates according to the units in my data.

Tables 3.5 and 3.6 contain initial results of a gradual expansion of the arbitrary scalar,  $\lambda$ , assigned to represent the accuracy of my prior precision matrix and the resulting extreme bounds for structural and neighborhood coefficient estimates (Atkinson, Crocker and Shogren 1992; Leamer and Leonard 1983; Leamer 1982). I report upper-bound estimates ( $\lambda$ ), lower-bound

estimates ( $1/\lambda$ ), and the absolute value of the distance between upper and lower bounds for all coefficients.

Table 3.5 shows structural estimates from a random sub-set of my data on non-flipped houses. The results indicate that, as the prior precision lessens (i.e. the arbitrary scalar,  $\lambda$ , increases), most of the coefficient estimate ranges include the origin. Specifically, estimates of the effects of the number of bathrooms, acres, and age all eventually incorporate zero. This result renders these estimates uncertain in sign and not robust to prior precision specification. Only the number of bedrooms and total heated living area estimates are robust to the size of the arbitrary, prior precision matrix.

Table 3.6 shows neighborhood hedonic estimates from the non-flipped sub-sample on log house prices. These estimates resemble the structural hedonic estimates. Only three of the neighborhood variables' extreme-bound ranges fail to incorporate zero across the prior precision span. Neighborhood high-school dropout rates, percentage of people on food stamps, and location in a gentrifying neighborhood are the only attributes that are robust to all prior covariance specifications.

I recalculate extreme bounds after incorporating new data. I add observations from both the sub-sample of flipped houses and the random sub-set I hold out from my initial sample of non-flipped houses. I examine the effects on the coefficient estimates from the type of additional data relative to initial results in Tables 3.5 and 3.6. Tables 3.7 and 3.8 contain extreme-bound estimates of structural and neighborhood attributes, respectively, after I add random observations on non-flipped houses.

In Tables 3.7 and 3.8, coefficient bounds on number of bathrooms, house age, EOC passing rate, high-school dropout rate, household median income, and violent crime rate all

narrow in range at every prior precision relative to the initial results reported in Tables 3.5 and 3.6. However, none of these variables fails to incorporate zero at every prior precision after the addition of more observations from non-flipped houses. Additionally, gentrification incorporates zero after I add more observations from non-flipped houses. This is the only difference between my initial estimates and the estimates after I incorporate additional observations from non-flipped houses. Adding more observations of traditional house transactors mitigates the negative effect of location in a gentrifying neighborhood on average house price.

This pattern is not borne out after I add an equal number of observations from flipped houses to the initial sample of non-flipped houses. Results are shown in Tables 3.9 and 3.10. Identical conclusions apply to the number of bathrooms, EOC passing rate, high-school dropout rate, percentage of neighborhood populations over 64, and violent crime rate in this sub-sample and in my initial estimates. But there are some important differences. The coefficient bounds on the gentrification indicator, like my initial estimates, do not include zero and the violent crime rate becomes robust with the addition of observations on flipped houses. These results provide evidence that house flippers negatively value gentrified neighborhoods and positively value higher violent crime rates.

To examine the new data's impacts on extreme bound range magnitudes, I calculate the "shrinkage," or percent difference in estimated bounds' ranges after the introduction of either flipped or non-flipped sub-sets into my initial, non-flipped data. Shrinkage of the bounds indicates that the addition of more data to the initial sample narrows the bound ranges. However, the addition of more data can also expand extreme bound ranges beyond my initial estimates.

As Table 3.11 indicates, for number of bedrooms and bathrooms, total heated living area, EOC passing rate, high-school dropout rate, and violent crime rate, more data invariably shrinks

the extreme bounds. This is true whether I add observations from flipped or non-flipped houses to my initial sample. Lot size, house age, percentage of neighborhood population over 64, percentage of neighborhood population on food stamps, and location in a gentrifying neighborhood experience expansion (or negative shrinkage) of extreme bound ranges when more data from either sub-sample are introduced. However, in the case of observations from non-flipped houses, acres' and age's coefficient bounds shrink at high levels of prior precision and expand at low levels. Household median income is the only hedonic attribute that shows data-type-specific results: extreme bound ranges shrink with additional non-flipped house data and expand with additional flipped house data.

There seems to be more noise in observations from flipped houses than in observations from non-flipped houses. The magnitudes of bounds' shrinkages or expansions are larger after I add data from flipped houses for nine out of the twelve hedonic covariates. Number of bedrooms and bathrooms, total heated living area, lot size, house age, high-school dropout rate, food stamp percentage, household median income, and violent crime rate all experience larger effects after I add observations from flipped houses rather than after I add observations from non-flipped houses.<sup>18</sup>

### **3.6 Conclusion**

The noisier effects from exclusively adding observations from flipped houses provide subtle evidence for Shiller's (2007) conclusion that "market psychology" drives speculation during a house price boom. The extremity of the magnitudes in percent differences I find after adding observations from flipped houses relative to adding observations from non-flipped houses bear some of this out. Because I add an equal number of observations on flipped and non-flipped

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<sup>18</sup> A prediction error calculation using flipped and non-flipped data yields a 13% increase in the average house-price prediction error of flipped data relative to non-flipped data.

houses for identical attributes from the MLS sample, my extreme bound estimates should be close and all of the same sign. This is not the case. Adding observations from flipped houses yields noisier coefficient estimates based on the extreme bounds results. Both of these conclusions add empirical weight to Shiller's (2007) story.

However, to label house speculation as irrationally fueled by a price bubble glosses over possible discrepancies between the hedonic preferences of house flippers and non-flippers. These discrepancies are notable at the neighborhood level. My findings identify differences between preferences for house locations in gentrifying neighborhoods and neighborhood violent crime rates. Apparently, the more non-flippers in a sample, the less likely it is that house prices exhibit negative effects of location in a gentrifying area. Flippers, on the other hand, prefer houses in areas with higher violent crime rates and do not prefer houses in areas where gentrification is manifest.

There is reason to think that these differences in house attribute preferences reflect rational decisions on the part of house flippers. O'Sullivan (2005) finds that neighborhood gentrification usually begins in areas with high, but falling, violent crime rates. Higher-income inhabitants begin to occupy more space in these areas as violent crime rates, which are positively associated with lower incomes, fall. House prices subsequently increase as gentrification takes hold. This process begins on the perimeters of richer neighborhoods and expands outward due to higher income residents' desire to live closer to other high-income earners (Guerrieri, Hartley, and Hurst 2013).

Arbitrage opportunities exist for owners of houses located in high crime neighborhoods, particularly during a price boom. House flippers key on the increased probability that these neighborhoods will experience gentrification. Non-flippers do not want to purchase houses in

initially high-crime areas. But, if these areas begin to experience gentrification through the influx of high-income earners and the outflow of poorer occupants, non-flipping house buyers are more likely to purchase houses there. Flippers, knowing this, and also knowing that rapid house price increases magnify the potential economic profits from flips, purchase houses in higher crime areas at a premium in expectation that the combined forces of gentrification and the price boom ensure their offers will fall below the subsequent selling prices of purchased houses. If flippers also declare these properties to be their “primary residences,” they face no tax penalty on capital gains from selling their houses.

These preferences bear out the empirical anomalies of observations on flipped houses as opposed to observations on non-flipped houses. Flippers negatively value houses in gentrifying areas because, as gentrification sets in, the flipper’s opportunity for economic profit falls to zero, as DHS (2011) point out. Additionally, flippers’ positive valuations of high violent crime rates bear out O’Sullivan’s (2005) contention that falling crime rates lead to more gentrification.

This process can be further fueled by a boom in house prices overall. Although this boom may fuel irrational price feedback, as Shiller (2007) suggests, there is empirical justification for the view that not all of this speculative behavior is irrationally founded. Is irrational exuberance or rational price speculation along neighborhood traits responsible for forecasting errors among hedonic models in the presence of house flipping? If it is the latter, mortgage originators can use house attribute data, as well as buyer profiles, to exploit an opportunity to transfer profits from house price flippers to themselves via third-degree price discrimination. The findings in this chapter warrant further research to determine which account best explains the discrepancy in preferences among the two types of housing-market transactors. Another potential research topic is whether, and to what extent, mortgage guarantors, by engaging in third-degree price

discrimination, discourage irrational speculation during a price boom. Such discouragement may lead to a mitigation of house price increases by tempering speculation and, indirectly, deflating Case and Shiller's (2003) self-reinforcing price bubble.

## 3.7 Tables

Table 3.1: Summary Statistics

	N	Mean	SD	Min	Max
House Attributes					
House Price <sup>†</sup>	3,724	433,949.3	345,622.1	29,960	3,850,447
Flipped House Price	1,810	439,036.6	336,847.7	42,205.5	3,149,317
Non-Flipped House Price	1,914	429,138.4	353,740.2	29,960	3,850,447
Beds	3,724	3.7	0.8	1	6
Baths	3,724	2.4	0.8	1	6
Half-Baths	3,724	1.0	0.2	1	4
Total Heated Living Area	3,724	2,574.4	1,119.2	800	13,359
Acres	3,724	0.4	0.2	0	3.5
Age	3,724	35.3	22.9	1	106
Neighborhood Attributes					
% Pop.>64	3,625	11.7	6.2	2.2	32.3
Dropout Rate	3,618	2.4	2.6	0	26.3
% Pop. on Food Stamps	3,625	2.3	3.2	0	28
Med. HH Inc.	3,625	104,918	42,245.7	27,083.1	269,068.7
Violent Crime Rate <sup>‡</sup>	3,625	0.3	0.5	0	4.2
% Pass EOC	3,683	70.1	11.4	26.7	95.4

<sup>†</sup>Prices in 2010\$

<sup>‡</sup>Violent crime rate in NSA/violent crime rate in City of Charlotte. Violent crimes comprise homicides, rapes, robberies, and aggravated assaults as reported in the Uniform Crime Report (UCR).

**Table 3.2: OLS Regression Estimates of Hedonic Attributes on House Log Selling Prices**

Flipped	.0174*
	(.0103)
Beds	-.0506*
	(.0283)
Baths	.0957**
	(.0354)
Total HLA	.4307***
	(.0520)
Acres	-.1743***
	(.0407)
Age	.0079***
	(.0003)
EOC	.0043***
	(.0005)
% Pop. > 64	.0166***
	(.0012)
Dropout Rate	-.0103**
	(.0028)
% Pop. on FS	-.0400***
	(.0038)
HH Med. Inc.	.0001
	(.0002)
Viol. Cr. Rate	.1420***
	(.0274)
Gentrified	-.0808***
	(.0162)
Intercept	10.98***
	(.0892)
N	3,580
Adj. R <sup>2</sup>	.777

\*\*\*, \*\*, \* indicate  $p < .01, .05, .10$ , respectively. Eicher-White std. errors in parentheses.

**Table 3.3: OLS Regression Estimates of Hedonic Attributes on Non-Flipped and Flipped House Log Selling Prices**

	Non-Flipped Houses	Flipped Houses
Beds	-.0543*** (.0156)	-.0471 (.0505)
Baths	.0644*** (.0157)	.1226** (.0597)
Total HLA	.4542*** (.0165)	.4139*** (.0958)
Acres	-.2772*** (.0432)	-.0839 (.0612)
Age	.0073*** (.0005)	.0086*** (.0005)
EOC	.0040*** (.0006)	.0049*** (.0008)
% Pop. > 64	.0189*** (.0015)	.0139*** (.0018)
Dropout Rate	-.0081** (.0037)	-.0131*** (.0042)
% Pop. on FS	-.0366*** (.0041)	-.0431*** (.0069)
HH Med. Inc.	.0002 (.0002)	-4.90e-5 (.0003)
Viol. Cr. Rate	.1233*** (.0269)	.1644*** (.0476)
Gentrified	-.0947*** (.0209)	-.0739*** (.0248)
Intercept	11.04*** (.0605)	10.91*** (.1358)
N	1,814	1,766
Adj. R <sup>2</sup>	.792	.762

\*\*\*, \*\*, \* indicate  $p < .01, .05, .10$ , respectively. Eicher-White std. errors in parentheses.

**Table 3.4: Prior Means, Sources, Locations, and Years**

	Prior Mean	Source	Location of Prior	Years of Prior
Beds	.0150	Clapp, et. al. (2007)	Connecticut	1994-2004
Baths*	.0830	Brasington and Haurin (2006) and Lewis, et. al. (2008)	Ohio and Maine	2000 and 1997- 2005
Total HLA*	.3780	Brasington and Haurin (2006) and Graves, et. al. (1988)	Ohio and California	2000 and 1978- 1979
Acres	.0871	Johnson and Lea (1982)	New York state	1978
Age*	-.0025	Crone (2006) and Schnare and Struyk (1976)	Pennsylvania and Boston	2000 and 1970
EOC	Diffuse <sup>†</sup>	Sample OLS	Charlotte, NC	1997-2010
% Pop. > 64*	.0147	Walden (1990) and Hearn (2014)	Raleigh, NC and Charlotte, NC	1987 and 1997- 2010
Dropout Rate	Diffuse <sup>†</sup>	Sample OLS	Charlotte, NC	1997-2010
% Pop. on FS	Diffuse <sup>†</sup>	Sample OLS	Charlotte, NC	1997-2010
HH Med. Inc.	.0022	Hearn (2014)	Charlotte, NC	1997-2010
Viol. Cr. Rate	Diffuse <sup>†</sup>	Sample OLS	Charlotte, NC	1997-2010
Gentrified	Diffuse <sup>†</sup>	Sample OLS	Charlotte, NC	1997-2010

\* indicates average of two coefficient estimates for median value.

† indicates prior means computed via restricted least squares subject to diffuse priors for all variables.

**Table 3.5: Bounds of Structural Coefficient Estimates: Initial Sample of Non-Flipped**

		<b>Houses</b>					
		$\lambda = 1$	$\lambda = 2$	$\lambda = 4$	$\lambda = 16$	$\lambda = 32$	$\lambda = 64$
<b>Beds:</b>							
U		-.2128	-.1601	-.1336	-.1136	-.1103	-.1087
L		-.2128	-.3170	-.5212	-1.635	-2.87	-4.756
$ \Delta $		0	.1569	.3876	1.521	2.760	4.647
<b>Baths:</b>							
U		.0093	.0340	.0465	.0559	.0574	.0582
L		.0093	-.0400	-.1357	-.6585	-1.239	-2.115
$ \Delta $		0	.0740	.1822	.7144	1.296	2.173
<b>Total HLA:</b>							
U		.5647	.6282	.7526	1.430	2.178	3.302
L		.5647	.5325	.5164	.5042	.5022	.5011
$ \Delta $		0	.0957	.2362	.9258	1.676	2.801
<b>Acres:</b>							
U		-.2159	-.1998	-.1683	.0018	.1874	.4604
L		-.2159	-.2241	-.2282	-.2313	-.2318	-.2320
$ \Delta $		0	.0243	.0599	.2331	.4192	.6924
<b>Age:</b>							
U		.0056	.0059	.0036	.0062	.0063	.0063
L		.0056	.0049	.0061	-.0036	-.0117	-.0242
$ \Delta $		0	.0010	.0025	.0098	.0180	.0305
N		922	922	922	922	922	922

**Table 3.6: Bounds of Neighborhood Coefficient Estimates: Initial Sample of Non-Flipped**

		<b>Houses</b>					
		$\lambda = 1$	$\lambda = 2$	$\lambda = 4$	$\lambda = 16$	$\lambda = 32$	$\lambda = 64$
<b>EOC:</b>							
U		-.0013	.0019	.0036	.0048	.0050	.0051
L		-.0013	-.0077	-.0203	-.0892	-.1667	-.2867
$ \Delta $		0	.0096	.0239	.0940	.1717	.2918
<b>% Pop. Over 64:</b>							
U		.0123	.0126	.0128	.0129	.0129	.0129
L		.0123	.0116	.0103	.0033	-.0047	-.0171
$ \Delta $		0	.0010	.0025	.0096	.0176	.0300
<b>Dropout Rate:</b>							
U		-.0117	-.0071	-.0048	-.0031	-.0028	-.0027
L		-.0117	-.0207	-.0384	-.1353	-.2443	-.4131
$ \Delta $		0	.0136	.0336	.1322	.2415	.4104
<b>% on Food Stamps:</b>							
U		-.0403	-.0399	-.0398	-.0396	-.0396	-.0396
L		-.0403	-.0409	-.0423	-.0500	-.0588	-.0745
$ \Delta $		0	.0010	.0025	.0104	.0192	.0349
<b>HH Median Income:</b>							
U		-.0001	9.87e-5	.0002	.0003	.0003	.0003
L		-.0001	-.0006	-.0015	-.0063	-.0118	-.0202
$ \Delta $		0	.0007	.0017	.0066	.0121	.0205
<b>Viol. Crime Rate</b>							
U		.1504	.1800	.1947	.2059	.2077	.2087
L		.1504	.0920	-.0223	-.6459	-1.340	-2.392
$ \Delta $		0	.0880	.2170	.5818	1.548	2.601
<b>Gentrified</b>							
U		-.0020	-.0016	-.0013	-.0011	-.0011	-.0011
L		-.0020	-.0030	-.0048	-.0158	-.0298	-.0557
$ \Delta $		0	.0014	.0061	.0147	.0287	.0546
N		922	922	922	922	922	922









**Table 3.11: Percent Shrinkage in Bounds of Coefficient Estimates by House Data Type****Added to Initial Non-Flipped Sample**

	$\lambda = 2$	$\lambda = 4$	$\lambda = 16$	$\lambda = 32$	$\lambda = 64$
<b>Non-Flipped Data Added</b>					
Beds	50%	50%	48%	46%	43%
Baths	57%	57%	55%	54%	51%
Total HLA	55%	55%	53%	51%	48%
Acres	3%	2%	-1%	-5%	-12%
Age	10%	12%	5%	3%	-2%
EOC	50%	50%	49%	47%	44%
% Pop. > 64	-130%	-128%	-139%	-145%	-158%
Dropout Rate	34%	34%	32%	29%	25%
% Pop. on FS	-410%	-408%	-392%	-405%	-400%
HH Med. Inc.	14%	12%	8%	6%	0%
Viol. Cr. Rate	66%	66%	49%	64%	62%
Gentrified	-279%	-115%	-260%	-243%	-216%
<b>Flipped Data Added</b>					
Beds	53%	53%	53%	52%	52%
Baths	74%	74%	74%	73%	73%
Total HLA	69%	69%	69%	69%	68%
Acres	-16%	-16%	-17%	-19%	-21%
Age	-10%	-16%	-17%	-17%	-19%
EOC	27%	27%	27%	27%	26%
% Pop. > 64	-90%	-84%	-89%	-89%	-89%
Dropout Rate	38%	37%	37%	36%	43%
% Pop. on FS	-1230%	-1200%	-1154%	-1150%	-1081%
HH Med. Inc.	-29%	-29%	-32%	-33%	-34%
Viol. Cr. Rate	98%	98%	97%	95%	99%
Gentrified	-221%	-84%	-204%	-192%	-171%

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