Three Essays on the Property Value Impact of Neighborhood Disamenities

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This dissertation employs hedonic analysis to examine market demand for three neighborhood disamenities. The first chapter investigates the property value impact of a No Child Left Behind “failing” school designation. The second chapter studies the property value impact of a local homicide. The third chapter examines the property value impact of proximity to a mosque post 9/11. Each disamenity has a negative and significant effect on property values with the magnitude of impact varying across household type.
THREE ESSAYS ON THE PROPERTY VALUE IMPACT OF NEIGHBORHOOD DISAMENITIES

By

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DISSERTATION

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1. Property Values and Neighborhood Amenities

For the majority of Americans, the purchase of a home represents a household’s single largest investment and a significant portion of total wealth. As such, housing demand is an important area of study. One of the key tools in studying housing demand is hedonic analysis, which is used to estimate the implicit price of individual housing attributes and neighborhood amenities. The three essays presented here employ hedonic analysis to examine market demand for a series of neighborhood disamenities.

The first essay examines the property value impact of a No Child Left Behind (NCLB) “failing” school designation. Under NCLB, schools receiving Title 1 funding that fail to meet adequate academic performance targets for two consecutive years are deemed “failing” and sanctioned. Details on these sanctions and “failing” designations are widely reported in local newspapers providing parents and prospective home buyers with additional information, beyond publically available test score data, about academic quality. Employing a highly parameterized model which controls for a series of fixed effects, I examine whether this additional information on school quality is capitalized into the housing market, and find that a NCLB “failing” school designation results in a 6.9% decrease in property values holding constant test scores, student characteristics, and neighborhood and property attributes.

To explore how demand for non-“failing” schools differs across household types, I introduce a two-stage estimator, which recovers neighborhood specific estimates of the impact of a “failing” designation. These neighborhood specific estimates are then used in a
second stage regression to examine the relationship between the magnitude of impact of a “failing” designation and pre-period household income. Applying this two-stage estimator, I find pre-period household income to be negatively correlated with the magnitude of impact of a “failing” designation. In other words, low income families unable to afford private school tuition and without the necessary resources to sufficiently investigate a “failing” designation’s true information content are most affected. This finding expands upon the single, uniform estimates of the market-wide impact of school-level categorical rankings currently available in the extant literature.

The second essay investigates the property value impact of a local homicide. Local instances of crime, especially when violent or aggressive in nature, can engender a climate of fear and anxiety among nearby residents, negatively impacting market demand for neighborhood housing. Employing a difference-in-difference identification strategy, I exploit a unique dataset containing dates and locations of Mecklenburg County, North Carolina homicides to examine variation in property values, pre and post homicide, within 500 feet and 500-1,000 feet of the crime, a small homogenous area characterized by similar neighborhood and housing attributes. Controlling for housing characteristics and time varying neighborhood-level heterogeneity, I find that, on average, homes sold within 500 feet decrease in value by 13.4% relative to homes 500-1,000 feet of the crime in the year following the homicide.

I investigate how the impact of a homicide differs across high and low crime areas, and find that in areas characterized by low levels of pre-period crime, homes located within 500 feet of a homicide decline in value by 25.5% post homicide. Alternatively, in areas characterized by high levels of pre-period crime, homes located within 500 feet of a
homicide see a statistically insignificant 9.4% decline. These results are intuitively appealing, given that a local homicide likely has a differential impact on perceived neighborhood safety in low and high crime neighborhoods. In low crime neighborhoods a local homicide represents a jarring divergence from the status quo, leading to a large change in perceived neighborhood safety. Alternatively in high crime neighborhoods already mired in some degree of violence, a local homicide fails to provide as striking a signal, leading to only a small change in perceived neighborhood safety. In addition, households that sort into low crime neighborhoods are more likely to have a higher willingness to pay for perceived neighborhood safety than households that sort into high crime neighborhoods.

The third essay examines the impact of a post September 11th, 2001 increase in anti-Islamic sentiment on property values, specifically analyzing whether, after the terrorist attacks, homes within close proximity to a mosque dropped in value. Using a difference-in-difference methodology, I find that post terrorist attacks, homes sold within 1,000 feet of a mosque declined in value by 17.1% relative to homes located within 1,000-2,000 feet of a mosque within the same neighborhood and sold during the same year.

I explore how the impact of post 9/11 proximity to a mosque varied across household type, and find a negative correlation between the magnitude of impact and pre-period neighborhood income. Households at the top of the property value distribution were largely unaffected by post-attack proximity to a mosque. To the extent income is correlated with education, this muted property value impact among higher income households may be a function of a broader cultural understanding and tolerance attained through higher education.
Together these three essays build upon our understanding of consumer demand for neighborhood disamenities. Furthermore, by examining how the impact of disamenities varies across income levels, these essays offer evidence of differing consumer demand across household types.
2. The Impact of a No Child Left Behind “Failing” School Designation on Local Property Values

2.1 Introduction

In January 2002 President George W. Bush signed No Child Left Behind (NCLB) into law, enacting legislation aimed at increasing K-12 academic standards and raising school accountability through measurable goals. Under NCLB, on a yearly basis schools are required to satisfy a series of academic performance targets in order to achieve Adequate Yearly Progress (AYP). Schools receiving Title 1 funding that fail to meet AYP for two consecutive years are deemed “failing” and sanctioned.

For a Title 1 school that fails to meet AYP for two consecutive years, details on sanctions and its NCLB “failing” designation are widely reported in local newspapers, providing parents and prospective home buyers with additional information, beyond test score data, about the school’s academic quality. Several studies have shown that publicly available information on school quality, generally in the form of average test score data, has a significant impact on property values. I examine whether the additional information contained in a NCLB “failing” school designation is further capitalized into the housing

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1 The term “failing school” is used in NCLB literature to denote Title 1 schools that fail to meet AYP for two consecutive years.
2 These sanctions increase in severity for every subsequent year a “failing” school does not meet AYP. Initial sanctions include offering students alternative attendance opportunities and the development of a corrective action plan. After five consecutive years of failing to meet AYP, Title 1 schools are temporarily shut down for a “restructuring and planning” year.
market. Specifically, using data from Mecklenburg County, North Carolina, I investigate whether a NCLB “failing” school designation has an additional impact on property values, controlling for school-level math and reading proficiency.

Employing a highly parameterized model which controls for a series of fixed effects, I find that a NCLB “failing” school designation results in a 6.9% decrease in property values holding constant other measures of school quality, student characteristics, and neighborhood and property attributes. To explore how demand for non-“failing” schools differs across household types, I introduce a two-stage estimator, which recovers neighborhood specific estimates of the impact of a “failing” designation. These neighborhood specific estimates are then used in a second stage regression to examine the relationship between the magnitude of impact of a “failing” designation and pre-period household income. Applying this two-stage estimator, I find pre-period household income to be negatively correlated with the magnitude of impact of a “failing” designation. In other words, low income families unable to afford private school tuition and without the necessary resources to sufficiently investigate a “failing” designation’s true information content are most impacted. This finding expands upon the single, uniform estimates of the market-wide impact of school-level categorical rankings currently available in the extant literature.

2.2 Literature Review

This section examines recent empirical work on the impact of school quality on property values. While much of the previous work has focused on the impact of various test scores on property values, three recent papers by Kane, Staiger, Samms, Hill, and Weimer (2003), Zahirovic-Herbert and Turnbull (2008), and Figlio and Lucas (2004) have examined the impact of school-level categorical ratings on housing values.
Black (1999) estimates the impact of school quality on property values by adopting a border methodology and examining home sales on either side of, but adjacent to, elementary school zoning boundaries. She finds that a one school-level standard deviation increase in elementary school test scores increases home prices by 2.2%. Kane, Staiger, and Riegg (2006) employ a similar methodology in their study of the relationship between school characteristics and housing prices in Mecklenburg County from 1994 through 2001. They estimate a one school-level standard deviation increase in elementary school test scores leads to a 2% increase in property values. Border methodologies have also been used by Leech and Campos (2003), Kane, Staiger and Samms (2003), Gibbons and Machin (2003), and Brasington and Haurin (2006).

Compared to work on the capitalization of test scores, very little has been written on demand for school-level categorical rankings. Kane, Staiger, Samms, Hill, and Weimer (2003) investigate, among other school-level characteristics, the impact of pre-NCLB state mandated categorical ratings on property values in Mecklenburg County. With the exception of a negative and statistically significant composite measure detailing the number of years a school is labeled “low performing,” the authors find categorical ratings had no statistically significant effect on property values. Zahirovic-Herbert and Turnbull (2008) investigate the impact of a similar rating system in Louisiana and find that moving up a categorical ranking increases house values by approximately 5%. Alternatively, moving down a ranking increases a house’s time on the market, but has no statistically significant effect on its selling price. Figlio and Lucas (2004) examine the impact of school-level grades of A through F on property values. Precursors to NCLB’s system of school accountability, Florida’s grades were based upon a school’s test performance on nationally normed assessments. Using data
from before and after the introduction of these grades, Figlio and Lucas find moving up a grade increment raises property values by roughly 10%.

All three of these studies offer valuable insights into consumer demand for school-level categorical rankings, but, by only investigating market-wide effects, they limit themselves to single, uniform estimates of the property value impact of changes to a school’s categorical ranking. As such, they fail to fully explore how consumer demand varies across neighborhood specific household types. In addition to estimating the overall market-wide impact of a “failing” school designation, my paper recovers neighborhood specific impacts, which allow me to investigate how the impact of a “failing” school designation varies across neighborhood specific submarkets characterized by differing household types.

2.3 Charlotte Mecklenburg Schools

This section provides a brief background on school choice at Charlotte Mecklenburg Schools (CMS) and examines the wide array of elementary schools, as characterized by differing levels of student academic achievement, receiving a NCLB “failing” designation.

2.3.1 School Choice in Mecklenburg County

Starting in 2002 and continuing throughout my period of analysis, CMS operated under a district-wide school choice plan. The school choice plan allowed parents to submit their top three choices of school programs for each of their children. Key for my analysis, admittance to a student’s home school was guaranteed, but access to “choice” schools was limited due to significant oversubscription. Approximately 65% of district schools were oversubscribed in fall 2004 (Hastings and Weinstein, 2007). Given oversubscription to a “choice” school, entry was granted on the basis of a lottery.
In summer 2004, the Charlotte Mecklenburg School (CMS) system began the first phase of NCLB sanctions for Title 1\(^3\) schools failing to meet AYP for two consecutive years. Parents of students slated for fall enrollment in a “failing” school were notified of the school’s NCLB status and offered alternative attendance opportunities\(^4\). Again, guaranteed admittance among alternative choices was limited due to oversubscription in a significant fraction of district schools. Over 84% of students slated for fall 2004 enrollment in a “failing” school ultimately attended, unable to find feasible alternative attendance opportunities (Hastings and Weinstein, 2007). Because, even with significant oversubscription, a certain degree of school choice exists for parents confronted with a NCLB “failing” school designation, my estimates of the property value impact of a NCLB “failing” school designation will represent a lower bound.

2.3.2 “Failing” School Designations in Mecklenburg County

Under NCLB, to achieve adequate yearly progress, a school has to satisfy academic performance targets across a wide array of student sub-groups. If even one of these sub-groups is unable to meet their prescribed performance targets, the entire school fails to achieve adequate yearly progress regardless of the academic success of the larger student body. If a Title 1 school fails to achieve adequate yearly progress for two consecutive years it is deemed “failing”. As a result, a “failing” designation is largely orthogonal to more traditional measures of academic quality, namely school-wide average test scores.

Table 2.1 details 2003-2004 adequate yearly progress reports for four Title 1 schools and offers evidence of the disconnect between school-wide average test scores and “failing”

\(^3\) At CMS, a school receives Title 1 funding if 75% or more of its students qualify for federal lunch subsidies.
\(^4\) NCLB “failing” designations are assigned each year in late July.
designations. Although Hidden Valley Elementary enjoyed the highest school-wide math and reading proficiency (89.2% math proficiency and 75.2% reading proficiency) across the four Title 1 Schools listed in Table 2.1, it failed to meet adequate yearly progress for the “Limited English Proficient” student subgroup in reading, and received a “failing” designation. Alternatively, Westerly Hills Elementary had school-wide math proficiency of 76.4% and school-wide reading proficiency of 66.9%, but met all adequate yearly progress requirements across sub-groups and remained non-“failing.”

The disconnect between “failing” designations and average test scores is further evidenced in Figure 2.1, which depicts the miscellany of school types receiving a NCLB “failing” designation during 2004, its first year of implementation. As shown, average student proficiency in math and reading, which is calculated based upon end-of-year test scores, varies from a low of 65% to a high of 85% among “failing” schools. Because of the disconnect between a “failing” designation and test scores, a wide array of household types with differing valuations of school quality experienced a “failing” designation including those households sorting into neighborhoods characterized by schools with 85% student proficiency.

Even households aware that “failing” designations were to be released in summer 2004 could do little, in terms of an anticipatory move, to ensure a “failing” school would be

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5 A student sub-group must have at least 40 members to be subject to AYP requirements. If a particular student sub-group fails to meet their proficiency goal there are two provisionary calculations under which they may still be able to meet adequate yearly progress, Safe Harbor and Confidence Interval. Under Safe Harbor, a student sub-group can meet its proficiency target if the sub-group has reduced same subject area non-proficiency by at least 10% from the previous year and the sub-group “shows progress” on the Other Academic Indicator. Under Confidence Interval, a 95% confidence interval is calculated based upon the percentage of students scoring proficient. If the confidence interval contains the proficiency target, the student sub-group provisionally meets adequate yearly progress.
avoided. Previous year test scores offered little predictive power in determining future NCLB “failing” school designations, precluding pre-announcement household sorting upon a school’s anticipated NCLB designation. This lack of predictive power is demonstrated in a probit regression of 2004-2005 NCLB “failing” school designations on 2002-2003 and 2003-2004 grade-level test scores. All previous year test score measures included in the regression are statistically insignificant, unsuccessful in explaining across school variation in future “failing” designations.

2.4 Theoretical Framework

According to the consensus model of bidding and sorting (Ross and Yinger, 1999) households sort into neighborhoods based upon the slope of their bid functions with respect to location specific amenities. The slope of the bid function for an amenity indicates a household’s willingness to pay for an additional unit of amenity quality. Households with steeper bid functions are willing to pay more for an increment in amenity quality and win the bidding competition to locate in neighborhoods with high quality amenities.

Prior to No Child Left Behind and the advent of “failing” school designations, household perception of school quality was largely informed by word of mouth and publically available information on test scores. During this period, households that placed a high value on education, corresponding to a steep bid function for school quality, won the bidding competition for housing in neighborhoods linked to high-quality schools (as defined by positive word of mouth and high test scores). Figure 2.2 illustrates three household types bid functions for perceived school quality. In this figure, household type H1 places relatively

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6 Apart from moving to non-Title 1 school attendance zones, although this was potentially cost prohibitive.
little value on an increment in school quality and locates in neighborhoods linked to low-quality schools, H2 places more value on a school quality increment than H1 and locates in neighborhoods linked to mediocre schools, and H3 places the most value on a school quality increment and locates in neighborhoods linked to high-quality schools.

With the introduction of NCLB “failing” school designations, households are provided with an additional indicator of school quality, which, conditional on test scores and word of mouth assessment, reduces a household’s bid by $\frac{\Delta P_i}{\Delta F}$, where $i$ indexes household type and $\Delta F$ denotes moving from non-“failing” to “failing” status. Because of the disconnect between a “failing” designation and other, more traditional measures of school quality, H1, H2, and H3 household types all experience a “failing” designation.

Figure 2.3 illustrates the impact of a NCLB “failing” designation on each household type’s bid function. As illustrated, the magnitude of $\frac{\Delta P_i}{\Delta F}$ is likely to vary across household types depending on their valuation of education, private school access, and understanding of the information content (or lack thereof) contained in a “failing” designation. For example, while high-income college educated households often put significant value on the quality of their children’s education, they are also more likely to send their children to private school, moderating the impact of a public school “failing” designation. College educated households are also potentially more likely to research “failing” designations and discover their tenuous connection to overall school quality.

Given $\frac{\Delta P_i}{\Delta F}$, the property value impact of a “failing” school designation can be calculated as follows:

$$\frac{\Delta V_i}{\Delta F} = (\Delta P_i/\Delta F) * \frac{H}{r}$$  \hspace{1cm} (2.1)
where $V$ is the value of a house, $H$ is units of housing services, and $r$ is a discount rate.

While a standard property value hedonic identifies a NCLB “failing” school designation’s average housing market impact, $\frac{1}{n} \sum_{i}^{n} \Delta V_i / \Delta F$, it fails to provide information on the varying impact across neighborhood specific household types.

I introduce a two-stage estimator (described in detail in Section 2.7.3b) which recovers these neighborhood specific impacts for use as the dependent variable in a second stage regression examining the relationship between the impact of a “failing” designation and pre-period household income. This two-stage estimator improves upon the single, uniform estimates of the property value impact of school-level categorical rankings presented in Kane, Staiger, Samms, Hill, and Weimer (2003), Figlio and Lucas (2004), and Zahirovic-Herbert and Turnbull (2008).

2.5 Data

My analysis draws on three datasets: arm’s length sales of single family homes in Mecklenburg County from June 2002 through May 2006; disaggregated tax assessor data containing the locations and housing characteristics of all parcels in Mecklenburg County; and student characteristics, yearly test scores, Title 1 status, and NCLB “failing” designations for elementary schools in the Charlotte Mecklenburg School District.

I combine the sales and tax assessor data to create a dataset containing the locations, characteristics, and sale dates and prices, normalized to August 2006 dollars, of all properties sold within Mecklenburg County from June 2002 to May 2006. The data include comprehensive building characteristics including the year built, the number of bedrooms and baths, two measures of build quality, and the type of build materials used. Within the data,
homes are separated into 1,004 neighborhoods characterized by similarly valued, homogenous properties. These neighborhoods are extremely small - on average only 0.47 square miles in area – even smaller than census tracts or block groups; there were 144 census tracts and 373 block groups in Mecklenburg County in 2000. Sales outside the range of $5,000 to $1 million dollars (representing the 1st and 99th percentile of the price distribution) are dropped. The resulting sample consists of 84,405 home sales.

After merging the sales and tax assessor data I link each real estate parcel to its assigned or home elementary school, for which a student is automatically guaranteed admission. This is accomplished through Geographic Information System (GIS), which I use to overlay a map of elementary school boundaries, provided by Charlotte Mecklenburg Schools (CMS), onto a map of all Mecklenburg County real estate parcels. I then join the layers. With each real estate parcel attached to an assigned elementary school, I append student characteristics, test scores, Title 1 status, and NCLB “failing” school designation. Figure 2.4 displays a map of CMS elementary schools that received a “failing” designation for the 2004-2005 school year.

Table 2.2 provides summary statistics of various characteristics of the properties sold within Mecklenburg County from June 2002 through May 2006. The first column provides information on all sales within Mecklenburg County, and the second provides information on sales of homes linked to “failing” schools. We see that homes linked to “failing” schools have fewer stories, are of lower build quality, roughly 12 years older, and sell for approximately $96,000 less than homes in our larger sample of sales across Mecklenburg County.

7 Because North Carolina has county-level school districts, every parcel in my sample of real estate sales faces the same school tax rate and policy regime.
2.6 Empirical Methodology

To reduce the probability that unobserved factors are driving the relationship between a “failing” school designation and property values, I adopt an empirical methodology originally proposed by Figlio and Lucas (2004)\(^8\) and estimate a highly parameterized model, which controls for a series of fixed effects:

\[
\log(P_{ijst}) = \alpha + B_1 X_{ijst} + B_2 Z_{st} + B_3 Failing_{st} + \gamma_{jt} + \delta_m + \varepsilon_{ijst} \tag{2.2}
\]

where \(P_{ijst}\) is the sales price of home \(i\) in neighborhood \(j\) assigned to home elementary school \(s\) at time \(t\). \(X_{ijst}\) is a vector of housing characteristics, and \(Z_{st}\) is a vector of elementary school attributes, which includes the following student characteristics, each measured as a percentage of the school’s total student population: Black, Hispanic, Limited English Proficiency (LEP), Economically Disadvantaged, and Special Education. \(Z_{st}\) also includes percentage proficient on 4\(^{th}\) grade reading and math end-of-year assessments\(^9\) and the school’s Title 1 status. \(Failing_{st}\) denotes a NCLB “failing” school designation, \(\gamma_{jt}\) is a non-parametric neighborhood-by-calendar year time trend, and \(\delta_m\) is a month-of-year indicator.

Neighborhood-by-calendar year fixed effects coupled with mid-year changes in a school’s “failing” status allow me to identify the impact of a “failing” designation via discontinuity regression. Changes in a school’s “failing” status are associated with within neighborhood changes in housing prices immediately before and immediately after a late July change in “failing” status.

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\(^8\) Figlio and Lucas use repeat sales while I rely on a rich set of housing characteristics to control for time invariant parcel-level heterogeneity.

\(^9\) Proficiency levels are associated with housing sales based upon the release date of test results.
In this specification, the vector of housing characteristics captures parcel-level heterogeneity, and the neighborhood-by-calendar year fixed effects allow for non-parametric neighborhood specific time trends which capture time varying heterogeneity at the neighborhood-level such as the introduction of a local park or new sidewalks and streetlamps. Student characteristics and Title 1 status capture time varying school-level heterogeneity such as changing student demographics and the level of poverty in an elementary school’s catchment area. Percentage proficient on 4th grade reading and math end-of-year assessments controls for an alternate measure of overall academic quality. Finally, month-of-year indicators control for the seasonal nature of Mecklenburg County’s housing market.

As in Figlio and Lucas (2004), given this extensive set of controls, an omitted variable would need to change systematically within a neighborhood mid-way through a year, while at the same time co-varying with changes in an elementary school’s NCLB “failing” school designation for it to bias my analysis.

2.7 Estimation Results

2.7.1 Impact of Test Scores on Property Values

To assess the impact of test scores on property values and compare my results with previous findings, I estimate equation (2.2) without the NCLB “failing” school designation and find that a one point increase in average fourth grade math and reading proficiency increases property values by 0.19%, a result significant at the 1% level. This equates to a one school-level standard deviation increase in fourth grade reading proficiency increasing property values by approximately 1.9%, an estimate similar in magnitude to the test score effects found by Black (1999) and Kane, Staiger, and Riegg (2004).
2.7.2 Impact of a NCLB “Failing” School Designation on Property Values

Next, I re-estimate equation (2.2) with the “failing” indicator and find that a NCLB “failing” school designation decreases property values by 6.9%, a result statistically significant at the 1% level (Table 2.3, column 1). Because the “failing” indicator is binary and the dependent variable is logged, percentage change is calculated by exponentiating $\hat{B}_3$, the coefficient estimate attached to $\text{Failing}_{st}$, and subtracting one$^{10}$. By including neighborhood-by-calendar year fixed effects I am identifying the impact of a NCLB “failing” designation from two sources of variation: schools transitioning in and out of a NCLB “failing” designation, and for those neighborhoods extending over one or more attendance zone boundaries, inter-school differences in “failing” designations.

To generate a more complete understanding of the overall impact of a NCLB “failing” school designation, I estimate a model with separate neighborhood and sale year fixed effects; $\gamma_j$ and $\lambda_t$ respectively. This specification allows me include the effect of a second successive year of NCLB sanctions for neighborhoods contained within a single attendance zone$^{11}$. Given my sample includes two years of pre-period data and two years of post-period data, neighborhood fixed effects can be interpreted as neighborhood specific event windows spanning 24 months prior and 24 months post “failing” designation. To

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$^{10}$ Because $\text{Failing}_{st}$ is a binary variable its correlation coefficient measures $\Delta \log(P_{js,t})/\Delta \text{Failing}_{st}$. This can be re-expressed as follows: $\hat{B} = \log(P_{js,t}) - \log(P_{js,t-1}) / [\text{Failing}_{st}(1) - \text{Failing}_{st-1}(0)] = \log(P_{js,t}) - \log(P_{js,t-1})$. Exponentiating, subtracting one from each side, and multiplying by 100 yields: $[\exp(\hat{B}) - 1] * 100 = \log(P_{js,t} - P_{js,t-1}) * 100/P_{js,t-1}$, or the percentage change in sales price from a “failing” designation.

$^{11}$ With neighborhood-by-year fixed effects, identifying variation for neighborhoods contained within a single attendance zone arises from schools transitioning in and out of a NCLB “failing” designation. As a result, information from a second successive year of “failing” is lost.
control for time varying heterogeneity within these neighborhood specific event windows, I add a series of interaction terms, $Neigh_j \ast e^{-1/t}$, which allow for non-linear time trends:

$$\log(p_{ijst}) = \alpha + B_1 X_{ijst} + B_2 Z_{st} + B_3 Failing_{st} + \gamma_j + \sum_j \sum_{t} \theta_{jt} Neigh_j \ast e^{-1/t} + \lambda_t + \delta_m + \varepsilon_{ijst} \quad (2.3)$$

Estimating equation (2.3), I find that a NCLB “failing” school designation decreases property values by 4.7%, a result statistically significant at the 5% level (Table 2.3, column 2). The lower estimated impact of a NCLB “failing” designation when including the effect of a second successive year of sanctions may reflect the lack of an information shock associated with a second year of “failing”. Alternatively, it may be a function of increased bias from unobserved time varying neighborhood-level heterogeneity, which I am unable to fully capture with parameterized neighborhood-level time trends.

2.7.2a Analysis Limited to Title 1 Neighborhoods

By estimating a single set of parameters across neighborhoods associated with Title 1 and non-Title 1 schools, I am implicitly assuming poolability across the two subgroups. As an alternative specification, I limit my analysis sample to neighborhoods contained within or extending over an attendance zone associated with a Title 1 school, and re-estimate equation (2.2). Analyzing this sub-sample, I find that a “failing” school designation decreases property values by an estimated 5.6%, a result statistically significant at the 1% level and qualitatively similar in magnitude to the full sample results12 (Table 2.3, column 3).

12 Post estimation, I test the equality of the “failing” coefficient estimates from the full sample and Title 1 neighborhood regressions and am unable to reject the null hypothesis that the estimates are equal at the 5% level.
2.7.2b Time since NCLB Announcement

To examine how the impact of a NCLB “failing” school designation varies with time since announcement, I interact the “failing” indicator with a cubic of time since initial announcement:

\[
\log(P_{ijst}) = \alpha + B_1X_{ijst} + B_2Z_{st} + B_3Failing_{st} \times Time + B_4Failing_{st} \times Time^2 + B_5Failing_{st} \times Time^3 + \gamma_j + \sum_j \sum_t \theta_{jt} Neigh_j \times e^{-t/t} + \lambda_t + \delta_m + \epsilon_{ijst} \tag{2.4}
\]

It is important to note that this analysis requires me to again extend the time horizon of the discontinuity regression by replacing neighborhood-by-calendar year fixed effects with separate neighborhood and year fixed effects.

The coefficient estimates attached to all three terms of the cubic - \(B_3, B_4,\) and \(B_5\) – are statistically significant and their combined effect, describing the varying impact of a “failing” designation with time since initial announcement, is graphed in Figure 2.5. As illustrated, the estimated impact of a “failing” designation is smaller in magnitude than estimates from equation (2.2). Again, I attribute this decrease in magnitude to bias from unobserved time varying neighborhood-level heterogeneity, which I am unable to fully capture with parameterized neighborhood-level time trends. Even so, noting the direction of likely bias, the dynamic analysis offers useful insight into how the impact of a “failing” designation varies with time since initial announcement.

Interestingly, the negative impact of a “failing” school designation increases in magnitude in the months following announcement, plateauing at approximately -4.8% in month six, and then slowly decreasing in magnitude after month ten. At month 19, the estimated impact again plateaus at approximately -1.0% through month 22. Because my data
only extends to May 31st, 2006, and, for the majority of Mecklenburg County schools, “failing” designations were first received in late July 2004, I am unable to test for post twenty-two month impacts.\footnote{To ensure my results are not a construct of the functional form imposed upon the time trend, I re-run my dynamic analysis replacing $failing \times time$, $failing \times time^2$, and $failing \times time^3$ with a series of $failing \times month\ post\ announcement$ indicators. This alternative specification allows for a non-parametric time trend and yields qualitatively similar results to equation (2.4).}

The increasing magnitude of a “failing” school designation in the first six months post NCLB announcement appears to indicate a relatively slow diffusion of information regarding this additional indicator of school quality. Two potential explanations arise for the decreasing impact of a “failing” designation beginning in month 11. The initial shock of a “failing” designation coupled with a commensurate overreaction may wane, and as time passes, either through lack of any reinforcing information, or increased investigation, more and more families may come to realize that a “failing” designation is largely disconnected from measures of overall school quality.

2.7.2c Varying Impact at Different Points along the Property Value Distribution

To investigate whether a NCLB “failing” school designation has an equal impact across heterogeneous properties, I run a simultaneous quantile regression to estimate the impact of a NCLB “failing” school designation at different points on the property value distribution, with results illustrated in Figure 2.6. The impact of a “failing” designation appears relatively uniform across the property value distribution yielding an estimated 6%-7% property value decline.
2.7.3 Differential Impact across Homes Stratified by Size and Neighborhood Income

As discussed in the theoretical framework (Section 3), the impact of a NCLB “failing” school designation varies across income-taste classes, with certain household types willing to pay more for improved school quality, and others willing to pay less. I examine the differential impact of a NCLB “failing” school designation across two variables which characterize household type, family-size and household income.

2.7.3a Homes Stratified by Size

Since larger houses are more likely to be occupied by families with children, I next investigate the differential impact of a NCLB “failing” school designation on houses based upon number of bedrooms. Dividing my sales data into two sub-samples, homes with 0-2 bedrooms and homes with 3+ bedrooms, I re-run equation (2.2). I find that homes with 3+ bedrooms, which are significantly more likely to be occupied by families with children, decline in value by 7.5% as a result of a NCLB “failing” school designation, a result statistically significant at the 1% level (Table 2.4, column 2). Alternatively, homes with 0-2 bedrooms, which are less likely to be occupied by families with children, see a statistically insignificant 5.0% decline (Table 2.4, column 1). This lack of statistical significance is likely a function of a relatively small sample size; only 6,061 0-2 bedroom homes were sold during my period of analysis.

The differential impact of a “failing” designation across number of bedrooms suggests that my results are primarily being driven by the buying and selling decisions of families with children.

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14 Figlio and Lucas (2004) perform a similar analysis in an earlier draft of their paper.
2.7.3b Homes Stratified by Neighborhood Income

To explore the relationship between neighborhood income and the hedonic price of a NCLB “failing” school designation, I re-estimate equation (2.2) allowing the coefficient attached to the failing indicator to vary across Census block groups. This yields 48 block group specific “failing” school coefficient estimates\(^{15}\), which I then regress on a cubic of 1999 block group level median household income\(^{16}\).

Because of a lack of pre-period sorting (discussed in Section 2.5) this two-stage regression allows me to estimate the hedonic price of a NCLB “failing” school designation across a miscellany of income-taste classes residing in neighborhood, or block group specific sub-markets. It is important to note, that given the possibility of post-announcement resorting, these neighborhood, or sub-group specific estimates potentially underestimate the true property value impact of a “failing” designation for pre-period occupants. To clarify, assume a representative neighborhood, Park Place, which experiences a “failing” announcement. Prior to the “failing” announcement, neighborhood homes were sold to households from the income-taste class with the highest willingness-to-pay for Park Place’s unique combination of housing attributes and neighborhood amenities. In what follows, these pre-announcement occupants will be referred to as income-taste class A.

For sales post-announcement, two possibilities arise: households from income-taste class A continue to win the bidding competition for homes in Park Place, or because of a large \(\Delta P/\Delta F\) relative to competing households, lose the bidding competition resulting in

\(^{15}\)To avoid small sample bias, I limit my second stage analysis to block group estimates derived from at least 30 sales post-NCLB “failing” announcement. This results in 48 observations in the second stage regression.

\(^{16}\)I arrive at a qualitatively similar result when statistically insignificant (p-value>0.10) first stage coefficient estimates are set to zero in the second stage regression.
Park Place being re-populated by households in a new income-taste class (subsequently referred to as income-taste class B). It is important to note that prior to the “failing” announcement, households in income-taste class B valued Park Place’s unique combination of housing attributes and neighborhood amenities less than households in income-class A; evidenced by having failed to win the pre-“failing” announcement bidding competition.

In Figure 2.7, detailing pre and post-“failing” announcement bids for an illustrative Park Place house, households from income-taste class A continue to win the bidding competition post-“failing” announcement. If this dynamic holds for all post-announcement transactions, the Park Place “failing” coefficient measures the average capitalization of a “failing” designation for pre-announcement occupants (income-taste class A). Figure 2.8 details pre and post-“failing” announcement bids given re-sorting. Post-announcement, households from income-taste class B win the bidding competition for housing. If Park Place is re-populated by households from income-taste class B, the Park Place “failing” coefficient measures a combination of two effects: the difference in income-taste class A and income-taste class B’s valuations of Park Place’s unique combination of housing attributes and neighborhood amenities, and the average capitalization of a “failing” designation for post-announcement occupants (income-taste class B). As evidenced in Figure 2.8, to the extent this occurs the Park Place “failing” coefficient will underestimate the impact of a “failing” designation on pre-announcement occupants.

Figure 2.9 graphs predicted values of the magnitude of impact of a NCLB “failing” designation based upon second stage regression coefficient estimates\textsuperscript{17}. As illustrated, the

\textsuperscript{17} It is important to note that given potential omitted variable bias in the second stage regression, any estimated relationship between neighborhood income and the hedonic price of NCLB “failing” school designation should be interpreted as correlational and not causal.
impact of a NCLB “failing” school designation is relatively constant at approximately -6 to -8% for homes located in block groups with median household incomes between $22,000 to $38,000, but then begins to lose magnitude, eventually falling to zero for homes located in block groups with median household incomes of $42,00018.

As discussed in the theoretical framework, while high-income college educated households often put significant value on the quality of their children’s education, they are also more likely to send their children to private school, moderating the impact of a public school “failing” designation19. High-income college educated households are also potentially more likely to research “failing” designations and discover their tenuous connection to overall school quality.

2.7.4 Differential Impact across “Failing” Student Sub-Groups

Under NCLB, to achieve adequate yearly progress and avoid a “failing” designation, a Title 1 school has to satisfy academic performance targets across a wide array of student sub-groups. If even one of these sub-groups is unable to meet their prescribed performance targets, the entire school fails to achieve adequate yearly progress. After two consecutive years of failing to achieve adequate yearly progress, a Title 1 school is deemed “failing.”

I investigate whether the impact of a “failing” designation varies by the type and size of student sub-group(s) unable to meet prescribed performance targets. If parents sufficiently investigate “failing” designations, a “failure” resulting from a relatively large student sub-group like “White” or “Black” unable to meet prescribed performance targets

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18 A kernel-weighted local polynomial regression estimating the regression function underlying the relationship between the magnitude of impact of a “failing” designation and 1999 block group level median household income yields qualitatively similar results. The local polynomial regression uses an Epanechnikov kernel function and rule of thumb bandwidth selection.

19 During the 2004-2005 CMS school year 12.6% of students enrolled in private school.
should have a larger impact on property values than a failure resulting from a relatively small
student sub-group like “Special Education” or “Limited English Proficiency” unable to meet
prescribed performance targets.

Re-running equation (2.2) with an additional interaction term, $failing \times size\ of\ non-
performing\ student\ sub-group(s)$, I find that the relative size of the non-performing student sub-
group(s), as measured by its percentage of the total student population, has no statistically
significant effect on the magnitude of impact of a “failing” designation. Next, I test for a
differential impact across student sub-group type by re-running equation (2.2) with $failing \times$
type of non-performing student sub-group indicators. Post estimation, I test the equality of the
coefficient estimates attached to each interaction term and fail to reject the null hypothesis
that the estimates are equal.

The lack of sufficient investigation into the varied determinants of a “failing”
designation is further evidenced by deconstructing the failing indicator into first year failing,
second year failing $\times$ met AYP requirements, and second year failing $\times$ did not meet AYP requirements and re-running equation (2.2). A school has to meet AYP requirements for two successive
years following a “failing” designation to return to non-“failing” status. As a result, schools
receiving a “failing” designation in year one, are still considered “failing” in year two, even if
they meet all AYP requirements (second year failing $\times$ met AYP requirements). If parents
investigate a school’s AYP student sub-group reports they become aware of this distinction,
muting the property value impact of a second year of “failing” for improving schools. I test
the equality of the coefficient estimates attached to second year failing $\times$ met AYP requirements
and second year failing $\times$ did not meet AYP requirements, and fail to reject the null hypothesis that

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20 To ensure sufficient cell sizes, a single indicator variable is used for “Limited English Proficient” and
“Students with Disabilities.”
the estimates are equal. Again, this equality suggests parents fail to sufficiently investigate the varied determinants behind a “failing” designation.

Parental failure to sufficiently investigate the determinants of a “failing” designation may be a function of ease of access to necessary information. A school’s “failing” designation is reported in local newspapers and mandated district mailings, while information on the “failing” designation’s key determinants, namely the size and type of student sub-groups failing to meet prescribed performance targets, is available only after navigating through a succession of five links on the North Carolina Public School’s website.

2.8 Sensitivity Tests

2.8.1 Testing for Serial Correlation

Bertrand et al. (2004) demonstrate that given positive serial correlation in the residuals, even clustered standard errors can significantly understate true standard errors leading to faulty inference. Alternatively, negative serial correlation in the residuals can lead standard error estimates to overstate true standard errors.

Testing for serial correlation in my residuals\textsuperscript{21}, I find a statistically insignificant first order autocorrelation coefficient, and a small, negative (-0.037) second order autocorrelation coefficient, statistically significant at the 5% level. This suggests my reported standard errors are overly conservative, likely overstating true standard errors.

\textsuperscript{21} As in Figlio and Lucas (2004), residuals have been aggregated to the school attendance zone-month level.
2.8.2 Testing for Sample Selection Bias

To ensure my results are not a product of sample selection where systematically different types of homes are sold within a neighborhood prior to and post “failing” announcement, I limit my sample to homes sold in 2004, the first year schools received a “failing” designation, and test for within neighborhood differences in housing characteristics prior to and post “failing” announcement:

\[
\text{Housing Characteristic}_{ijst} = \alpha + B_1 \text{Failing}_{st} + \gamma_{jt} + \delta_m + \varepsilon_{ijst} \tag{2.5}
\]

where \(\text{Housing Characteristic}_{ijst} \in X_{ijst}\). I run equation (2.5) for the following dependent variables: beds, baths, square footage, age, and build quality. In each instance, the coefficient attached to failing is statistically insignificant indicating no substantive difference in within neighborhood housing characteristics for homes sold prior to and post “failing” announcement. Taken together, these regression results suggest my findings are not a product of sample selection bias.

2.8.3 Testing for Omitted Variable Bias

2.8.3a Additional Test Score Measures

To alleviate the concern that the absence of additional test score measures, although weakly correlated with a NCLB “failing” school designation, are biasing estimates I re-estimate equation (2.2) adding third and fifth grade math and reading proficiency scores. I again find a negative and statistically significant relationship between the NCLB “failing” school designation and property values, with a coefficient estimate of -0.066 (-6.4%)

22 These additional test score measures are highly correlated with average 4th grade math and reading proficiency.
attached to the “failing” school indicator, a result which is statistically significant at the 1% level.

2.8.3b Time Invariant School-level Heterogeneity

To remove any time invariant school-level heterogeneity, potentially correlated with a NCLB “failing” school designation, I re-run equation (2.2) adding elementary school fixed effects. While I still find a negative point estimate, -0.034 (-3.3%) attached to the “failing” school indicator, the coefficient estimate does not meet traditional levels of statistical significance (p-value of 0.155). I attribute this to a loss of variation\textsuperscript{23}. Furthermore, even though the coefficient estimate attached to the “failing” indicator does not meet traditional levels of statistical significance, it remains roughly 1.5 times larger than its standard error, 0.023, indicating an effect remains, albeit measured imprecisely. It should also be noted, that given evidence of negative serial correlation in the residuals, my estimate of the standard error is overly conservative and most likely overstates its true value.

Given the multitude of fixed effects already included in my primary regression, the dampened impact of a “failing” designation resulting from the addition of school fixed effects is to be expected and should be interpreted as a highly conservative lower bound.

2.8.4 Border Methodology

To test the robustness of my results to alternative specifications I limit my sample to neighborhoods extending over one or more attendance zone boundaries and re-run equation (2.2). Analysis of this sub-sample is akin to the border methodology identification strategy used by Black (1999) with neighborhood-by-year fixed effects acting as time varying

\textsuperscript{23} As documented in Gujarati’s “Basic Econometrics” (2003).
boundary dummies. These boundary dummies control for unobserved time varying characteristics shared by homes on either side of, but adjacent to, elementary attendance zone boundaries.

The success of a border methodology identification strategy in accurately estimating the property value impact of school quality hinges on the assumption that neighborhoods change continuously over space while school quality changes discontinuously at attendance zone boundaries. To the extent this assumption holds, a border methodology offers an attractive option for addressing omitted variable bias.

Results from the border methodology identification strategy are presented in Table 2.3, column 3. As shown, the coefficient estimate attached to the “failing” indicator is equal to -0.076 (-7.3%) and statistically significant at the 1% level. This qualitatively similar result demonstrates the robustness of my results to alternative specifications.

When I re-run this analysis with school fixed effects, the coefficient estimate attached to the “failing” indicator drops in magnitude to -0.042 (-4.1%), but remains statistically significant at the 5% level. Again, given the array of fixed effects and time-varying school-level controls already included in equation (2.2), this muted impact is largely a function of loss of variation and should be interpreted as a lower bound.

2.8.5 Testing for Differential Pre-period Trends

To ensure the decrease in value ascribed to a NCLB “failing” school designation is valid, and not a function of differential trends in pre-period property value growth, I run a falsification test using erroneous announcement dates for a school’s NCLB “failing” school designation set two years prior to actual events. Re-running equation (2.2) with these false
dates leads to statistically insignificant results (Table 2.5, column 2). This suggests my results are not a function of differential trends in pre-period property value growth.

2.9 Conclusion

Given that several studies have shown publicly available information on school quality, most often in the form of test scores, has a significant impact on property values, I examine whether the additional information contained in a NCLB “failing” school designation is further capitalized into the housing market.

Employing a highly parameterized model which controls for a series of fixed effects, I find that a NCLB “failing” school designation results in a 6.9% decrease in property values holding constant other measures of school quality, student characteristics, and neighborhood and property attributes. This decline is statistically significant at the one percent level and robust to a number of specifications. Given average housing prices in “failing” school attendance zones, a NCLB “failing” school designation results in a $7,597 decrease in the average property value. In many ways this property value decrease is a product of informational inefficiency. Parents are reacting to the seeming severity of a “failing” designation, without understanding its disconnect from overall school quality. A program aimed at eliciting a better understanding of the true nature of a “failing” designation would aid in eliminating this market inefficiency.
3. The Localized Impact of a Homicide on Property Values

3.1 Introduction

Crime is a major societal problem with annual costs estimated in excess of 1.7 trillion dollars (Anderson, 1999). Besides its direct and immediate cost to victims, crime necessitates spending on law enforcement, the criminal justice system, private security and incarceration, all costly. In addition, although often overlooked when calculating its economic damage, crime can represent a serious neighborhood disamenity which is capitalized into the housing market reducing property values.

Local instances of crime, especially when violent or aggressive in nature, can engender a climate of fear among nearby residents reducing perceived neighborhood safety and heightening imagined susceptibility to future assault. In fact, crime can represent such a significant disamenity, that area residents are driven to relocate. Dugan (1999) finds that an attack or victimization near one’s residence increases the probability of relocation. In their study of urban flight, Cullen and Levitt (1999) find that high crime cities experience an increase in residential exit relative to their low crime counterparts. They estimate a 10% increase in crime results in a 1% decline in city population.

Beginning with Ridker and Henning (1967), a number of studies have attempted to document the impact of crime on property values. Thaler (1978) was first to empirically establish an inverse relationship between crime and property values. Rizzo (1979) was first to instrument for crime in an effort to control for its endogeneity. Since then, instrumental

In this paper, I examine the property value impact of a local homicide, but, because of the difficulty and often poor defensibility\textsuperscript{24} of instrumental variable approaches I confront the potential endogeneity of the crime variable with an alternative methodology as proposed by Linden and Rockoff (2008). Employing a difference-in-difference identification strategy, I exploit a unique dataset containing dates and locations of Mecklenburg County, North Carolina homicides to examine variation of property values, pre and post murder, within 500 feet and 500-1,000 feet of a homicide, a small homogenous area characterized by similar neighborhood and housing attributes. In this way, I control for un-observed time varying neighborhood level heterogeneity, which might otherwise bias my estimates.

I estimate that homes sold within 500 feet of a homicide fall in value by 13.4\% in the year following the crime. This sharp localized decline in home prices post homicide is evident in Figure 3.1, which depicts the results of a kernel-weighted local polynomial regression of home sales pre and post homicide for homes located within 500 feet and 500-1,000 feet of a homicide. Sale prices for homes located within 500 feet of a homicide dropped sharply post homicide, while sale prices of homes located 500-1,000 feet of a homicide maintained their pre-homicide trajectory.

My paper is organized as follows: in the next section, I provide a theoretical framework for my analysis; I then describe the data used and discuss my empirical

\textsuperscript{24} With a just identified model, there is no test for an instrument’s exogeneity and one can often tell a story, which calls the author’s exclusion restriction(s) into serious question.
methodology; this is followed by a results section, a series of falsification tests, and a brief conclusion.

3.2 Theoretical Framework

This section presents the conceptual framework supporting my hypothesis that a local homicide lowers residential property values via a decrease in perceived neighborhood safety.

Following Ross and Yinger (1999), I make the following assumptions:

- Household utility depends on housing services, H, a vector of neighborhood amenities, S, and a numeraire good, Z.
- Households are perfectly mobile and all households are homeowners.
- Each household is grouped into a particular income-taste class, and because of perfect mobility, households in the same income-taste class share the same level of utility, \( U^0 \).

Households maximize their utility function, \( U = U(Z, H, S) \), with respect to the following budget constraint:

\[
Y = Z + PH + \tau V = Z + PH \left(1 + \frac{\tau}{r}\right) = Z + PH \left(1 + \tau^*\right) \tag{3.1}
\]

where \( Y \) is household income, \( P \) is the price per unit of housing services \( H \), \( \tau \) is the effective tax, \( V \) is home value, \( r \) is the discount rate, and \( \tau^* \) is equal to \( \tau/r \).

3.2.1 Household Bidding

Solving equation (3.1) for \( P \) leads to the following household maximization problem, indicating a household’s maximum willingness to bid for a unit of household services:
\[\text{Max}_{(Z,H)} \quad P = \frac{Y-Z}{H(1+\tau^*)} \quad \text{subject to } U\{Z,H,S\} = U^0 \quad (3.2)\]

Setting up the Lagrange function and taking derivatives with respect to \(Z\) and \(H\) yields the following first order conditions:

\[
\frac{\partial L}{\partial H} = \frac{Y-Z}{H^2(1+\tau^*)} + \lambda U_H = 0 \quad (3.3)
\]

\[
\frac{\partial L}{\partial Z} = \frac{1}{H(1+\tau^*)} + \lambda U_Z = 0 \quad (3.4)
\]

Solving equation (3.3) for \(\lambda\), and substituting the solution into equation (3.4) results in the following maximization condition:

\[U_H/U_Z = P(1+\tau^*) \quad (3.5)\]

To maximize utility, households choose \(H\) and \(Z\) such that \(U_H/U_Z = P(1+\tau^*)\).

Since \(P_Z\) is equal to one, this simplifies to choosing \(H\) and \(Z\) such that the marginal benefit of \(H\) (in dollar terms) is equal to its marginal cost.

Let \(S_j\) equal perceived neighborhood safety, which decreases in the event of a local homicide, \(LH\). Prior to a local homicide (\(LH=0\)), a household chooses \(H_1\) units of housing services, \(Z_1\) units of a numeraire good, and achieves utility level \(U^0\) as illustrated in Figure 3.2. Now assume \(\Delta U/\Delta LH = \Delta U/\Delta S_j * \Delta S_j/\Delta LH < 0\), or household utility decreases in the event of a local homicide as a result of a decrease in perceived neighborhood safety.

After a local homicide (\(LH=1\)), \(U(Z_1,H_1,S|LH = 1) < (Z_1,H_1,S|LH = 0) = U^0\). In order for households to achieve \(U^0\), ensuring locational equilibrium, \(H\) and/or \(Z\) must increase as
represented by a rightward shift in the $U^0$ indifference curve as illustrated in Figure 3.3. Since $P_z$ is fixed at one, households can only reach this higher indifference curve by decreasing $P$, their bid per unit of housing services.

Given $\Delta P / \Delta LH$, the property value impact of a local homicide can be calculated as follows:

$$\Delta V / \Delta LH = (\Delta P / \Delta LH) * H / r$$ (3.6)

It is important to note that $\Delta V / \Delta LH$ will vary across income-taste classes based upon each group’s income and preferences. A standard property value hedonic will identify the average housing market impact of a local homicide, $\frac{1}{n} \sum_i^n \Delta V_i / \Delta LH$, where $i$ indexes individual households.

3.2.2 Household Sorting

Assume a homicide occurs in a representative neighborhood, Park Place. Prior to the local homicide ($LH = 0$), homes were sold to households from the income-taste class with the highest willingness-to-pay for Park Place’s unique combination of housing attributes and neighborhood amenities. In what follows, these pre-homicide occupants will be referred to as income-taste class A.

For sales post-homicide, two possibilities arise: households from income-taste class A continue to win the bidding competition for homes in Park Place, or because of a large $\Delta P / \Delta LH$ relative to competing households, lose the bidding competition resulting in Park Place being re-populated by households from a new income-taste class (subsequently referred to as income-taste class B). It is important to note that prior to the homicide,
households in income-taste class B valued Park Place’s unique combination of housing attributes and neighborhood amenities less than households in income-class A; evidenced by having failed to win the pre-homicide bidding competition.

In Figure 3.4, detailing pre and post-homicide bids for an illustrative Park Place house, households from income-taste class A continue to win the bidding competition post-homicide. If this dynamic holds for all post-homicide transactions, $\frac{1}{n} \sum_{i}^{n} \Delta V_i / \Delta LH$ measures the average capitalization of a local homicide for pre-homicide occupants (income-taste class A). Figure 3.5 details pre and post-homicide bids given re-sorting. Post-homicide, households from income-taste class B win the bidding competition for housing. If Park Place is re-populated by households from income-taste class B, $\frac{1}{n} \sum_{i}^{n} \Delta V_i / \Delta LH$ measures a combination of two effects: the difference in income-taste class A and income-taste class B’s valuations of Park Place’s unique combination of housing attributes and neighborhood amenities, and the average capitalization of a local homicide for the new, post-homicide occupants (income-taste class B). As evidenced in Figure 3.5, to the extent this occurs $\frac{1}{n} \sum_{i}^{n} \Delta V_i / \Delta LH$ will underestimate the impact of a homicide on pre-homicide occupants.

3.3 Data

Focusing on Mecklenburg County, North Carolina, my analysis draws on two datasets: county tax assessor data and the dates and locations of homicides in Mecklenburg County from 2001 through 2004 collected from the Charlotte Observer.

The tax assessor data provides information on the locations, characteristics, and sale dates and prices, normalized to December 2004 dollars, of all properties sold within
Mecklenburg County from 2000 to 2004. The data includes comprehensive building characteristics including the year built, the number of bedrooms and baths, two measures of build quality, and the type of build materials used. Within the data, homes are separated into 1,004 neighborhoods characterized by similarly valued, homogenous properties. These neighborhoods are extremely small, on average only 0.47 square miles; much smaller than census block groups. In 2000 there were 373 census block groups in Mecklenburg County and 1,004 tax assessor defined neighborhoods.

The dates and locations of homicides were collected from the *Charlotte Observer*, the major daily newspaper for Mecklenburg County. Whenever a death in the county was investigated as a homicide, the *Charlotte Observer* would run a short piece in the Metro section of the paper providing summary details on the case. These included the date of the incident and its exact or approximate location. The *Charlotte Observer* reported a total of 203 unique homicide date/locations from 2001 to 2004. Out of the 203 unique date/locations, the newspaper provided sufficiently detailed location information on 173 date/locations.

As a result, I am only able to successfully identify the dates and locations for approximately 85% of the reported homicides in Mecklenburg County from 2001 to 2004. Although this is not ideal, to the extent that real estate sales are driven by out of neighborhood moves, and potential buyers get their news from the same information sources I investigate, we are similarly hampered. In other words, the majority of potential buyers are making their decisions based upon the same information set I draw from. Figure
3.6 illustrates the 173 successfully identified locations of reported homicides in Mecklenburg County from 2001 to 2004. 

To construct my analysis sample, I combine my two datasets by matching reported homicides to property sales. For each property sale, I identify all homicides occurring within 1,000 feet of the property and within a 2 year window (1 year prior and 1 year post) of the sale date. The resulting sample has 840 sales matched to a homicide and 86,777 unmatched sales. Sales outside the range of $5,000 to $1 million dollars (representing the 1st and 99th percentile of the price distribution) are dropped.

Table 3.1 provides summary statistics of various characteristics of the properties sold within Mecklenburg County from 2000 to 2004. The first column provides information on all sales within the County, and the second provides information on sales of homes within 1,000 feet of a homicide occurring within a 2 year window of the sale date (from now on simply referred to as matched home sales within 1,000 feet of a homicide). We see that matched homes within 1,000 feet of a homicide are substantively smaller, have a lower build quality rating and sell for approximately $94,000 less than homes in our larger sample of sales across Mecklenburg County. These differences serve to demonstrate the importance of properly controlling for the endogeneity of the location of a homicide when assessing its impact on property values.

---

25 When information as to the location of a homicide was provided at the block level I linked it to the middle of the block, in this way minimizing any potential spatial mismeasurement. For example, if the Charlotte Observer reported that a homicide had occurred on the 500 block of Genesee Street, which runs from 500 to 599, I assigned the homicide to 550 Genesee Street. Using this simple method of imputation, I calculate an average maximum potential measurement error of approximately 141 feet for the 173 successfully identified homicides.
3.4 Empirical Methodology

As mentioned, a major concern in estimating the relationship between crime and property values is properly controlling for the endogeneity of the crime variable. In cross-sectional data, geographic variation in the crime rate is most likely correlated with unobservables which also impact property values. In time series data, changes in the crime rate are potentially linked to changes in neighborhood composition and other time varying neighborhood characteristics.

In an effort to control for this endogeneity, a number of studies have adopted instrumental variable identification strategies. These studies include Rizzo (1979), Naroff et al (1980), Burnell (1988), Buck et al (1993), Gibbons (2004), Tita et al (2006), Pope et al (2009), and Mayock et al (2010). Out of these eight studies, half fail to test for the validity of their instruments, and among the four that do, one offers an overidentification test which rejects the exogeneity of the proposed instruments (Mayock and Ihlanfeldt, 2010). This is in no way a condemnation of previous work on the subject, but simply an indication of how difficult it is to identify instruments which are both strongly correlated with crime and plausibly excludable from the primary home price hedonic regression.

Because of the difficulty and often poor defensibility of instrumental variable approaches I confront the potential endogeneity of the crime variable with an empirical methodology originally proposed by Linden and Rockoff (2008) in their paper estimating the impact of registered sex offenders on local property values. Employing a difference-in-difference identification strategy, I exploit a unique dataset containing dates and locations of Mecklenburg County homicides to examine variation in property values, pre and post murder, within 500 feet and 500-1,000 feet of a homicide - a small homogenous area
characterized by similar neighborhood and housing attributes. In this way, I control for unobserved time varying neighborhood level heterogeneity, which might otherwise bias my estimates.

Like Linden and Rockoff’s analysis, the efficacy of my proposed estimation strategy centers on the relative similarity of matched homes sold within 500 feet and 500-1,000 feet of a homicide. Figure 7 depicts homes within 500 feet and 500-1,000 feet of a homicide to indicate just how closely these two cohorts are grouped. I test the cross-sectional difference between these two groups of homes with the following regression:

\[
\log(P_{ijt}) = \alpha_{jt} + BD_{ijt}^{500} + \varepsilon_{ijt}
\] (3.7)

where \(P_{ijt}\) is the sales price of home \(i\) in “neighborhood” \(j\) at time \(t\), \(\alpha_{jt}\) is a non-parametric neighborhood time trend, and \(BD_{ijt}^{500}\) is an indicator variable for matched homes sold within 500 feet of a homicide.

By limiting my sample to matched home sales within 1,000 feet of the murder and working with only pre-period data, equation (3.7) allows me to examine whether home prices significantly differed with distance to the future site of the murder. I also test for the similarity of other neighborhood amenities as well as structural attributes by including each measure as the dependent variable in equation (3.7) and performing the same analysis.

If any cross-sectional differences exist, I can control for them with the following difference-in-difference model, again proposed by Linden and Rockoff, which I use to estimate the impact of a murder on property values:

\[
\log(P_{ijt}) = \alpha_{jt} + B_{1}X_{t} + (\omega_{0}D_{ijt}^{1000} + \pi_{0}D_{ijt}^{500}) + (\omega_{1}D_{ijt}^{1000} + \pi_{1}D_{ijt}^{500}) \times Post_{it} + \varepsilon_{ijt}
\] (3.8)
where $\alpha_{jt}$ is a non-parametric neighborhood-time trend, $X_i$ is a vector of housing characteristics specific to home $i$, $D_{ijt}^{1000}$ is an indicator variable for matched homes sold within 1,000 feet of a homicide, and $Post_{jt}$ is an indicator for time periods post homicide$^{26}$.

3.5 Estimation Results

3.5.1 Differences in Pre-Period Characteristics

3.5.1a Homes Sold within 500 feet of a Homicide relative to All County Sales

To investigate the average pre-period price difference between homes sold within 500 feet of a homicide and homes sold within the same neighborhood and in the same year, I estimate equation (3.7) using all Mecklenburg County home sales (Table 3.3, column 1). The coefficient estimate attached to the indicator variable $D_{ijt}^{500}$ is not statistically significant indicating that homes sold close to the future site of a homicide were, on average, no more or less expensive than unmatched homes sold within the same neighborhood during the same year.

$^{26}$ There is a degree of imprecision in identifying the exact location of many of the homicides in my dataset; I calculate an average maximum potential measurement error of approximately 141 feet for the 173 successfully identified homicides. Fortunately, given my difference-in-difference strategy such imprecision in identifying the exact location of a homicide should downward bias any estimated impact on property values.

Take my previous example of a homicide reported on the 500 block of Genesee Street. I assign the homicide an imputed location of 550 Genesee Street and construct my indicator variables for homes sold within 500 feet and 500-1000 feet of 550 Genesee Street. If the homicide actually occurred at 595 Genesee, many of the homes assigned to the within 500-1000 feet group will actually be situated much closer to the site of the homicide, potentially within 500 feet.

If we assume a homicide leads to a 10% drop in post period property values for homes within 500 feet, the homes mistakenly assigned to the 500-1000 feet group will see a significant drop in value. As a result, the difference-in-difference estimator, which calculates the differential impact in post period prices between homes located within 500 feet and homes located within 500-1000 feet, will return a downward biased estimate.
3.5.1b Homes Sold within 500 feet a Homicide relative to Homes Sold within 500 to 1,000 Feet of a Homicide

Next, I limit my sample to pre-period sales of homes within 1,000 feet of a future homicide and re-estimate equation (3.7). The resulting coefficient estimates provide pre-period cross-sectional differences between matched homes sold within 500 feet and 500-1,000 feet of a homicide.

The coefficient estimates presented in Table 3.2 present differences in log sales price, age, square footage, number of bedrooms, and numbers of bathrooms – none of which are statistically significant. These results demonstrate the relative pre-period homogeneity of homes sold within 500 feet and 500-1,000 feet of a future homicide.

3.5.2 The Impact of a Homicide

To investigate the property value impact of a homicide, I begin by estimating a simple pre-post comparison using equation (3.8), but excluding an indicator variable for homes sold within 1,000 feet of a homicide (Table 3.3, column 2). I find that homes sold within 500 feet of a homicide declined in price by 10.2%\(^{27}\) post homicide relative to homes sold within the same neighborhood in the same year, a result statistically significant at the 5% level.

Next, I re-estimate equation (3.8) with an indicator variable for homes sold within 1,000 feet of a homicide. Again, controlling for household and time varying neighborhood-

\(^{27}\) Because \(D_i^{500} \times Post_t\) is a binary measure, its correlation coefficient measures \(\Delta \log (P_{ij,t}) / \Delta (D_i^{500} \times Post_t)\). This can be re-expressed as follows: 

\[
\hat{B} = \frac{[\log (P_{ij,t}) - \log (P_{ij,t-1})]}{[1 - 0]} = \log (P_{ij,t}) - \log (P_{ij,t-1}).
\]

Exponentiating, subtracting one from each side, and multiplying by 100 yields:

\[
[\exp (\hat{B}) - 1] \times 100 = \frac{P_{ij,t} - P_{ij,t-1}}{P_{ij,t-1}} \times 100, \text{ or the percentage change in sales price due to proximity to a homicide.}
\]
level heterogeneity, I find that homes located within 500 feet of a homicide declined in value by 13.4% post homicide relative to homes located between 500-1,000 feet of the homicide, a result statistically significant at the 5% level (Table 3.3, column 3).

3.5.2a Varying Impact at Different Points along the Property Value Distribution

To investigate whether a homicide has an equal impact across heterogeneous properties, I run a simultaneous quantile regression, which estimates the impact of a homicide at different points along Mecklenburg County’s property value distribution, with results illustrated in Figure 3.8. Because of computational constraints, this regression includes neighborhood and year fixed effects instead of neighborhood-by-year fixed effects.

Interestingly, the property value impact of a homicide appears relatively uniform across homes with only slightly larger coefficient estimates at the upper end of the property value distribution. This differential, albeit slight, is intuitively appealing. More expensive homes are likely to be located in affluent, low crime neighborhoods where a local homicide represents a jarring divergence from the status quo, leading to a large change in perceived neighborhood safety. Alternatively less expensive homes are likely to be located in poor, high crime neighborhoods already mired in some degree of violence, where a local homicide fails to provide as striking a signal, leading to only a small change in perceived neighborhood safety. I more fully investigate this dynamic in section 3.5.3 (Differential Impact across High and Low Crime Areas).

3.5.2b Attenuation with Distance from a Homicide

To examine how the impact of a homicide attenuates with geographic distance from the crime, I interact $D_i^{500} * Post_t$ with a fourth order polynomial of distance and graph the
results in Figure 3.9\textsuperscript{28}. As illustrated, the impact of a homicide appears relatively uniform for homes located within 500 feet of the crime with the exception of a -22.3% estimated impact at 200 feet. I attribute this anomaly to a paucity of relevant post-period sales at this distance and interpret the deviation from an otherwise uniform trend as noise resulting from small sample bias.

Discounting this aberration, these results suggest that for homes located within 500 feet of a crime, distance fails to play a substantive role in ameliorating the fear, anxiety and decrease in perceived neighborhood safety associated with a nearby homicide. As a neighborhood resident living within 500 feet of recent homicide, there is no consolation living three, as opposed to two, houses down from site of the crime. Within such a short distance residents feel equally vulnerable.

3.5.2c Attenuation with Time since a Homicide

To investigate how the impact of a homicide attenuates with time, I interact $D_t^{500}$ with a second order polynomial of days since the crime and graph the resulting coefficient estimates in Figure 3.10\textsuperscript{29}.

As illustrated, the impact of a homicide is greatest immediately after the crime, but relatively short lived. Thirty days post-homicide homes within 500 feet are devalued by an estimated 18.8% relative to homes 500-1,000 feet of the crime. Sixty days post, property values have already began to recover among homes within 500 feet devalued by 13.4% relative to homes within 500-1,000 feet of the crime. As more time passes, police investigations are concluded and the homicide fades from the public consciousness.

\textsuperscript{28} Higher order polynomials fail to improve upon the model's explanatory power.

\textsuperscript{29} Again, higher order polynomials were tested, but they failed to improve upon the model's explanatory power.
Property values for homes within 500 feet continue to recover. One hundred twenty days after the crime, homes within 500 feet are devalued by 6.1% relative to homes within 500-1,000 feet and by six months, homes within 500 feet are devalued by only 4.6% relative to homes 500-1,000 feet of the crime\textsuperscript{30}.

### 3.5.3 Differential Impact across High and Low Crime Areas

To investigate whether the impact of a homicide differs across high and low crime areas, I subdivide my sales data into two samples, one characterized by relatively high pre-period crime\textsuperscript{31} and one characterized by relatively low pre-period crime, and re-run equation (3.8). I find that in areas characterized by low levels of pre-period crime, homes located within 500 feet of a homicide saw a 25.5% decline in property values post homicide relative to homes between 500-1,000 feet of a homicide, a drop in value which is statistically significant at the 1% level (Table 3.4, column 1). Alternatively, in areas characterized by high levels of pre-period crime, homes located within 500 feet of a homicide saw a statistically insignificant 9.4% decline in property values post homicide relative to homes between 500 and 1,000 feet of a homicide (Table 3.4, column 2).

Again, such a result is intuitively appealing, suggesting a local homicide has a differential impact on perceived neighborhood safety in low and high crime neighborhoods.

\textsuperscript{30} Interestingly, after regaining much of their value six months post homicide; homes within 500 feet begin to again lose value relative to homes 500-1,000 feet of the crime. This may be a function of additional, lesser crimes being committed in the same area post homicide, potentially spurred on by the original crime, and leading to a second wave of sales as perceived neighborhood safety among residents is further eroded. Unfortunately, given data constraints, the basis of this second order effect cannot be further investigated. Furthermore, it is important to note that since sales are assigned to homicides up to one year post, this second order effect is included when calculating the overall property value impact of a homicide. But, an alternative specification where sales are only associated with homicides up to six months post, capturing just the first order effect, yields a qualitatively similar result, $\pi_1(D_{i500} \times \text{Post}_i) = -0.128^\ast$.

\textsuperscript{31} The level of pre-period crime is calculated from year 2000 census tract crime rates.
In addition, further amplifying the divergent effect, households that sort into low crime neighborhoods are more likely to have a higher willingness to pay for perceived neighborhood safety than households that sort into high crime neighborhoods.

To better understand how the impact of a homicide varies across high and low crime areas, I subdivide my sales data by the level of pre-period property and violent crime and re-run equation (3.8). In areas characterized by low levels of pre-period property crime, homes located within 500 feet of a homicide decline in value by 27.4% post homicide relative to homes between 500-1,000 feet of a homicide, an impact significant at the 1% level (Table 3.4, column 3). Alternatively, in areas characterized by high levels of pre-period property crime, homes located within 500 feet of a homicide decline in value by a statistically insignificant 9.3% post homicide relative to homes between 500-1,000 feet of a homicide (Table 3.4, column 4).

Interestingly, point estimates for areas characterized by differing levels of pre-period violent crime, are relatively similar (Table 3.4, columns 4-5). To the extent lesser forms of violent crime, even when successfully prosecuted, are not as visible as destruction of property, vandalism, break-ins, and thefts, the differential impact of a local homicide on perceived neighborhood safety will be larger for areas characterized by low vs. high property crime rather than areas characterized by low vs. high violent crime.

3.6 Sensitivity Tests

3.6.1 Homicide Area by Year Fixed Effects

Confining my sample to homes sold within 2,000 feet of a homicide, I re-estimate equation (3.8), but replace neighborhood-by-year fixed effects with homicide area by year
fixed effects. In this way, I control for variation specific to areas surrounding each homicide. This specification also allows me to test an assumption implicit to my larger estimation strategy, namely that the relationship between housing characteristics and prices for homes located farther afield, outside the proximate area of a homicide, are helpful in estimating the relationship between housing characteristics and prices for homes located within close proximity of a homicide. Running this specification yields results consistent with my previous estimates suggesting that using additional data from sales outside a 2,000 foot radius of a homicide does not bias my estimates.

3.6.2 Test of Residual Means

As a further refinement and additional test of the applicability of using data from this larger cohort of homes, I investigate whether homes located within 1,000 feet of a homicide are characterized by lower quality un-observables by performing a t-test on the equality of residual means from a hedonic price regression for homes located inside and outside a 1,000 foot radius of a homicide. I am unable to reject the null hypothesis of equal residual means across the two groups. Again, this suggests that using additional sales data does not bias my results.

3.6.3 Falsification Tests

Although my analysis shows little evidence of preexisting differences in homes located within 500 feet and 500-1,000 feet of a homicide, hypothetically the decrease in value ascribed to a homicide could be a function of differential trends in property value growth leading to a spurious result.
I check for this possibility by running a series of falsification tests using erroneous homicide dates set one and two years prior to dates of actual events. Re-running my difference-in-difference specification with these false dates leads to statistically insignificant results (Table 3.5, column 2). In other words, I find no evidence of a spurious effect.

3.7 Conclusion

Employing a difference-in-difference methodology, I exploit a unique dataset containing dates and locations of reported homicides in Mecklenburg County, North Carolina to examine variation in property values, pre and post murder, within 500 feet and 500-1000 feet of a homicide, a small homogenous area characterized by similar neighborhood and housing attributes. I estimate that homes sold within 500 feet of a homicide fall by roughly 13.4% in the year following the crime. Given average housing prices in Mecklenburg County, this represents a $15,290 average property value decline.

A homicide’s short-lived, but significant property value effect provides evidence of the myopic and highly reactive nature of the real estate market. Buyers and sellers appear easily swayed in their perception of the underlying safety of a neighborhood immediately following a homicide, but within six months these fears seem to have largely evaporated as evidenced by a lack of long-term capitalization.
4. Anti-Islamic Sentiment and its Impact on Residential Property Values

4.1 Introduction

On September 11th, 2001, America suffered one of the worst terrorist attacks in the nation’s history. Four passenger-filled commercial airliners were hijacked by members of Al Qaeda and in an act of Jihad, intentionally crashed killing all onboard. Two airliners hit the Twin Towers, a third hit the Pentagon, and a fourth was brought down in rural Pennsylvania during a passenger insurrection while on its way to the White House. These unanticipated attacks claimed almost 3,000 lives and hundreds of billions of dollars of economic damage.

Following the attacks, anti-Islamic sentiment significantly increased within the United States. The FBI reported a 17-fold increase in crimes directed at Muslims nationwide during 2001. In addition, Arab and Muslim groups reported over 2,000 incidents of September 11th related backlash. An ABC poll conducted in January 2002 reported that 14% of respondents believed mainstream Islam supported violence towards non-members.

By September 2003, this number had increased to 34%, indicating over 1/3 of the US population believed Islam promoted violence against the West. A June 2003 Pew Research Study released similar findings: 49% of Americans believed a significant portion of Muslims held anti-American views. Even three years later, misperception and fear persisted.
In March 2006, an ABC poll reported 33% of Americans believed mainstream Islam advocated violence towards non-members.

In this paper, I examine the impact of the post September 11th, 2001 increase in anti-Islamic sentiment on property values. Specifically, I analyze whether, after the terrorist attacks, homes within close proximity to a mosque dropped in value. Using a difference-in-difference methodology, I find that after the terrorist attacks, homes located within 1,000 feet of a mosque decreased in value by over 17% relative to homes located between 1,000-2,000 feet of a mosque. This decline in value is statistically significant at the one percent level and robust to a number of specifications.

4.2 Literature Review

This section examines recent empirical work on the economic effects of terrorism, the majority of which has focused on terrorism’s impact on broader economic measures such as GDP growth and stock market returns.

Abadie and Gardeazabal (2003) estimate the economic effects of terrorism on the Basque region of Spain using a synthetic control, or weighted combination of other Spanish regions with little to no exposure to terrorism. The authors compare the economic evolution of this synthetic control to that of the Basque region, attributing any differences in GDP per capita between the two regions to the negative effects of terrorism. They estimate that the Basque region lost 10% of real GDP due to terrorism in the 1980s and 1990s. In an even broader analysis, Abadie and Gardeazabal (2007) examine the relationship between the risk of terrorism and net foreign direct investment in 110 countries. They find that a one standard deviation increase in the risk of terrorism decreases net foreign direct investment by approximately 5% of GDP, equivalent to 16 billion 2003 US dollars.
Eldor and Melnick (2004) and Eckstein and Tsiddon (2003) investigate the impact of terrorism on the Israeli economy. Focusing on the period 1990-2003, during which Israel experienced 639 terrorist attacks resulting in 1,212 deaths and over 5,700 injuries, Eldor and Melnick (2004) estimate a 30% decline in the value of the Tel Aviv Stock Market 100 Index as a result of terrorism. Eckstein and Tsiddon (2003) use quarterly data to examine the impact of terror on GNP and find that, in the absence of terrorist attacks from the third quarter of 2003 to the fourth quarter of 2004, the Israeli economy would have grown by an additional 4%.

In the study most closely linked to my own, Gautier et al (2009) examine the effects of terrorism at a more localized level. Employing a difference-in-difference identification strategy, the authors compare house prices in Amsterdam neighborhoods with more than 25% Moroccan and Turkish inhabitants with house prices in other Amsterdam neighborhoods before and after the murder of Theo van Gogh.

The murder of filmmaker van Gogh was religiously motivated, committed by a 26-year old Dutchman of Moroccan origin who had recently converted to radical Islam. A wave of national outrage and increased racial tension followed the murder. A survey conducted shortly after the crime found that 86% of respondents believed the murder had negatively impacted the relationship between Muslims and non-Muslims. The authors investigate whether this change in public opinion had an impact on house prices and find that relative house prices in neighborhoods characterized by more than 25% Moroccan and Turkish inhabitants decreased by approximately 2.4% in the year following the Theo van Gogh murder.
4.3 Theoretical Framework

This section presents the conceptual framework supporting my hypothesis that post-terrorist attacks, residential properties within close proximity to a mosque decreased in value relative to residential properties located farther afield.

Following Ross and Yinger (1999), I make the following assumptions:

- Household utility depends on housing services, $H$, a vector of neighborhood amenities, $S$, and a numeraire good, $Z$.
- Households are perfectly mobile and all households are homeowners.
- Each household is grouped into a particular income-taste class, and because of perfect mobility, households in the same income-taste class share the same level of utility, $U^0$.

Households maximize their utility function, $U = U\{Z, H, S\}$, with respect to the following budget constraint:

$$Y = Z + PH + \tau V = Z + PH \left(1 + \frac{\tau}{r}\right) = Z + PH(1 + \tau^*) \hspace{1cm} (4.1)$$

where $Y$ is household income, $P$ is the price per unit of housing services $H$, $\tau$ is the effective tax, $V$ is home value, $r$ is the discount rate, and $\tau^*$ is equal to $\tau/r$.

4.3.1 Household Bidding

Solving equation (4.1) for $P$ leads to the following household maximization problem, indicating a household’s maximum willingness to bid for a unit of household services:

$$\text{Max}_{(Z,H)} \quad P = \frac{Y-Z}{H(1+\tau^*)} \quad \text{subject to } U\{Z, H, S\} = U^0 \hspace{1cm} (4.2)$$
Setting up the Lagrange function and taking derivatives with respect to Z and H yields the following first order conditions:

\[
\frac{\partial L}{\partial H} = \frac{Y - Z}{H^2(1 + \tau^*)} + \lambda U_H = 0 \tag{4.3}
\]

\[
\frac{\partial L}{\partial Z} = \frac{1}{H(1 + \tau^*)} + \lambda U_Z = 0 \tag{4.4}
\]

Solving equation (4.3) for \(\lambda\), and substituting the solution into equation (4.4) results in the following maximization condition:

\[
U_H/U_Z = P(1 + \tau^*) \tag{4.5}
\]

To maximize utility, households choose H and Z such that \(U_H/U_Z = P(1 + \tau^*)\). Since \(P_Z\) is equal to one, this simplifies to choosing H and Z such that the marginal benefit of H (in dollar terms) is equal to its marginal cost.

Let \(S_j\) indicate close proximity to a mosque, \(AI_1\) equal the level of pre-9/11 anti-Islamic sentiment, and \(AI_2\) equal the level of post-9/11 anti-Islamic sentiment, where \(AI_2 > AI_1\). Prior to the terrorist attacks, given close proximity to a mosque (\(S_j = 1\)) a household chooses \(H_1\) units of housing services, \(Z_1\) units of a numeraire good, and achieves utility level \(U^0\) as illustrated in Figure 4.1. Now assume \(\Delta U/\Delta AI|S_j = 1 < 0\), or, given close proximity to a mosque, household utility decreases with anti-Islamic sentiment. Post terrorist attacks, \(U(Z_1, H_1, S|S_j = 1, AI_2) < U(Z_1, H_1, S|S_j = 1, AI_1) = U^0\). In order for households in close proximity to a mosque to achieve \(U^0\), ensuring locational equilibrium, H and/or Z must increase as represented by a rightward shift in the \(U^0\) indifference curve as
illustrated in Figure 4.2. Since $P_z$ is fixed at one, households can only reach this higher indifference curve by decreasing $P$, their bid per unit of housing services.

Given $\Delta P/\Delta AI$, the property value impact of a post-September 11th increase in anti-Islamic sentiment can be calculated as follows:

$$\frac{\Delta V}{\Delta AI} = (\Delta P/\Delta AI) \cdot H/r$$ (4.6)

It is important to note that $\Delta V/\Delta AI$ will vary across income-taste classes based upon each group’s income and preferences. A standard property value hedonic will identify the average housing market impact of a post-September 11th increase in anti-Islamic sentiment, $\frac{1}{n} \sum_i^n \Delta V_i/\Delta AI$, where $i$ indexes individual households.

4.3.2 Household Sorting

Assume a representative neighborhood, Park Place, in close proximity to a mosque. Prior to the terrorist attacks of September 11th, neighborhood homes were sold to households from the income-taste class with the highest willingness-to-pay for Park Place’s unique combination of housing attributes and neighborhood amenities. In what follows, these pre-attack occupants will be referred to as income-taste class A.

For sales post-attacks, two possibilities arise: households from income-taste class A continue to win the bidding competition for homes in Park Place, or because of a large $\Delta P/\Delta AI$ relative to competing households, lose the bidding competition resulting in Park Place being re-populated by households in a new income-taste class (subsequently referred to as income-taste class B). It is important to note that prior to the terrorist attacks, households in income-taste class B valued Park Place’s unique combination of housing
attributes and neighborhood amenities less than households in income-class A; evidenced by having failed to win the pre-attack bidding competition.

In Figure 4.3, detailing pre and post-announcement bids for an illustrative Park Place house, households from income-taste class A continue to win the bidding competition post-attacks. If this dynamic holds for all post-attack transactions, \( \frac{1}{n} \sum_{i=1}^{n} \Delta V_i / \Delta AL \) measures the average capitalization of a post-September 11\(^{th}\) increase in anti-Islamic sentiment for pre-attack occupants (income-taste class A). Figure 4.4 details pre and post-attack bids given re-sorting. Post-attack, households from income-taste class B win the bidding competition for housing. If Park Place is re-populated by households from income-taste class B, \( \frac{1}{n} \sum_{i=1}^{n} \Delta V_i / \Delta AL \) measures a combination of two effects: the difference in income-taste class A and income-taste class B’s valuations of Park Place’s unique combination of housing attributes and neighborhood amenities, and the average capitalization of a post-September 11\(^{th}\) increase in anti-Islamic sentiment for post-attack occupants (income-taste class B). As evidenced in Figure 4.4, to the extent this occurs \( \frac{1}{n} \sum_{i=1}^{n} \Delta V_i / \Delta AL \) will underestimate the impact of a post-September 11\(^{th}\) increase in anti-Islamic sentiment on pre-attack occupants.

4.4 Data

Focusing on Baltimore, Maryland, my analysis draws on two datasets: the first, a compendium of disaggregated tax assessor data, and the second, the location of all Baltimore mosques continuously operating from 1998 to 2007.

The tax assessor data provides information on the locations, characteristics, and sale dates and prices, normalized to December 2008 dollars, of all residential properties sold within Baltimore from 1998 to 2007. The data includes comprehensive building
characteristics including the year built, the square footage and number of stories, a measure of build quality, and the type of build materials used. Sales outside the range of $5,000 to $1 million dollars are dropped and the resulting sample consists of 146,050 observations.

The locations of mosques continuously operating from 1998-2007 were collected by taking the intersection, or set of all mosques as defined by name and location, present in both a 1998 and a current year compendium of religious institutions in the United States.

To construct my analysis sample, I combine my two datasets by matching mosques to property sales. For each property sale, I identify all mosques within 2,000 feet of the property. The resulting sample has 1,054 sales matched to a mosque and 144,996 unmatched sales.

Table 4.1 provides summary statistics of various characteristics of the properties sold within Baltimore from 1998 to 2007. The first column provides information on all sales within Baltimore, and the second provides information on sales of homes within 2,000 feet of a mosque. We see that homes within 2,000 feet of a mosque have fewer stories and sell for approximately $19,000 less than homes in our larger sample of sales across greater Baltimore. Interestingly, build quality and age are relatively similar across the two cohorts.

4.5 Empirical Methodology

This section presents the empirical framework and develops the motivation behind my difference-in-difference specification.

Let \( P_{it}^{1} \) equal the period \( t \) sales price of home \( i \) located within 1,000 feet of a mosque, post terrorist attacks. The sales price for the same home in the absence of the
terrorist attacks is defined as $P_{it}^0$. Given these two prices, the impact of anti-Islamic sentiment engendered by the terrorist attacks for home $i$ can be computed as $P_{it}^1 - P_{it}^0$.

The average impact across affected homes can be calculated as $E(P_{it}^1 - P_{it}^0 | \text{PROX} = 1)$, where PROX=1 denotes homes located within 1,000 feet of a mosque. Unfortunately, the counterfactual $P_{it}^0$ is not observed precluding direct estimation of $E(P_{it}^0 | \text{PROX} = 1)$.

To estimate this counterfactual mean, I define two alternative comparison groups: all remaining homes in greater Baltimore and homes located within 1,000-2,000 feet of a mosque, as depicted in Figure 4.5. Let PROX=1 denote homes located within 1,000 feet of a mosque and PROX=0 the respective comparison group.

Define $t = 0$ as the period prior to the terrorist attacks and $t = 1$ as the period after. By assuming a common trend, later formally tested, in the rate of property value growth between these two cohorts of homes in the absence of the terrorist attacks, the missing counterfactual can be replaced by:

$$E(P_{it}^0 | \text{PROX} = 1, t = 1) = E(P_{it}^0 | \text{PROX} = 1, t = 0) + m_t$$ (4.7)

where $m_t$ is the aggregate rate of property value growth among homes in the comparison group,

$$m_t = E(P_{it}^0 | \text{PROX} = 0, t = 1) - E(P_{it}^0 | \text{PROX} = 0, t = 0)$$ (4.8)

Given this framework, $E(P_{it}^1 - P_{it}^0 | \text{PROX} = 1)$ can be computed with a difference-in-difference estimator. Controlling for individual-level structural characteristics and
neighborhood\textsuperscript{32} by year fixed effects, my initial specification compares post 9/11 changes in sales price between homes located within 1,000 feet of a mosque and all other homes within greater Baltimore:

$$\log(P_{ijt}) = \alpha_{jt} + B_1 X_i + \pi_0 D_{i,1000}^{1,000} + \pi_1 D_{i,000}^{1,000} * Post_t + \epsilon_{ijt}$$ (4.9)

where $P_{ijt}$ is the sales price of home $i$ in census tract $j$ at time $t$. $\alpha_{jt}$ is a neighborhood-time trend, $X_i$ is a vector of housing characteristics, $D_{i,1000}^{1,000}$ is an indicator variable for homes sold within 1,000 feet of a mosque, and $Post_t$ is an indicator variable for time periods post 9/11.

The coefficient $\pi_1$ measures the differential impact, or Post 9/11 percentage change in housing prices, for homes located within 1,000 feet of a mosque relative to all other homes within greater Baltimore. The consistency of this coefficient estimate hinges on the assumption of a common trend in sales prices shared by homes located within 1,000 feet of a mosque and all other homes in greater Baltimore. Given the highly localized nature of real estate markets such an assumption may be inappropriate. Therefore, I construct a second, more refined comparison group consisting of homes located within 1,000-2,000 feet of a mosque:

$$\log(P_{ijt}) = \alpha_{jt} + B_1 X_i + (\omega_0 D_{i,000}^{2,000} + \pi_0 D_{i,1000}^{1,000}) + (\omega_1 D_{i,2,000}^{2,000} + \pi_1 D_{i,1,000}^{1,000}) * Post_t + \epsilon_{ijt}$$ (4.10)

where $D_{i,000}^{2,000}$ is an indicator variable for homes located within 2,000 feet of a mosque.

Again, the coefficient $\pi_1$ measures the post 9/11 percentage in housing prices for homes

\textsuperscript{32} Neighborhoods are defined by census tract boundaries.
located within 1,000 feet of a mosque, but now relative to homes located between 1,000-2,000 feet of a mosque.

Using only pre-period data and limiting my sample to homes sold within 2,000 feet of a mosque, I test my assumption of a common pre-period trend, conditional on building characteristics, in property value growth with the following specification:

\[
\log(P_{ijt}) = \alpha_{jt} + B_1 X_i + \pi_0 D_{i,1,000} + \varepsilon_{ijt}
\]  \hspace{1cm} (4.11)

A statistically significant coefficient estimate, \(\hat{\pi}_0\), attached to \(D_{i,1,000}\) would indicate differential pre-period trends in property value growth for homes located within 1,000 feet of a mosque and homes located between 1,000-2,000 feet of a mosque.

4.6 Estimation Results

4.6.1 The Common Trends Assumption

Limiting my sample to homes sold within 2,000 feet of a mosque, I estimate equation (4.11) with pre-period data and find no statistically significant differential trend in property value growth between homes located within 1,000 feet of a mosque and homes located between 1,000-2,000 feet of a mosque (Table 4.2, column 1). Running a similar specification, I find no statistically significant pre-period difference in building age or square footage for homes sold within 1,000 feet of a mosque and homes sold within 1,000-2,000 feet of a mosque (Table 4.2, columns 2-3). Taken together, these results suggest homes sold within 1,000-2,000 feet of a mosque can be confidently used as an appropriate comparison group to homes sold within 1,000 feet of a mosque.
4.6.2 Impact of Post September 11th, 2001 Increase in Anti-Islamic Sentiment

Using all other greater Baltimore homes sales as my initial comparison group, I estimate equation (4.9) with year fixed effects and find an 10.1% post terrorist attack decline in property values for homes located within 1,000 feet of a mosque, statistically significant at the 5% level (Table 4.3, column 1). Next, I estimate equation (4.9) with neighborhood by year fixed effects and find a 13.2% decline, statistically significant at the 1% level (Table 4.3, column 2). This indicates that post terrorist attacks, homes sold within 1,000 feet of a mosque declined in value by 13.2% relative to other residential properties located within the same neighborhood and sold during the same year.

Moving to my second comparison group, homes located within 1,000-2,000 feet of a mosque, I estimate equation (4.10) with year fixed effects and find a 17.7% post terrorist attack decline in property values for homes located within 1,000 feet of a mosque, statistically significant at the 1% level (Table 4.3, column 3). Re-estimating equation (4.10) with neighborhood by year fixed effects; I find a 17.1% decline, statistically significant at the 1% level (Table 4.3, column 4). This indicates that post terrorist attacks, homes sold within 1,000 feet of a mosque declined in value by 17.1% relative to homes located within 1,000-2,000 feet of a mosque within the same neighborhood and sold during the same year.

4.6.2a Varying Impact with Time since Terrorist Attacks

To examine how the impact of the post September 11th, 2001 increase in anti-Islamic sentiment varied with time since the attacks, I add two additional interaction terms to

\[^{33}\text{Because } D_{t,1000} \ast Post_t \text{ is a binary measure, its correlation coefficient measures } \Delta \log(P_{jst})/\Delta (D_{t,1000} \ast Post_t). \text{ This can be re-expressed as follows: } \hat{B} = [\log(P_{jst}) - \log(P_{jst-1})]/[1 - 0] = \log(P_{jst}) - \log(P_{jst-1}). \text{ Exponentiating, subtracting one from each side, and multiplying by 100 yields: } [\exp(\hat{B}) - 1] \ast 100 = [P_{jst} - P_{jst-1} \ast 100/P_{jst-1}], \text{ or the percentage change in sales price due to post September 11th, 2001 proximity to a mosque.}\]
equation (4.10), $D_{t}^{1,000} * Post_{t} * Time \text{ Since Attacks}$ and $D_{t}^{1,000} * Post_{t} * Time \text{ Since Attacks}^{2}$ and graph the resulting coefficient estimates in Figure 4.6.34.

As illustrated, three months after the attacks homes located within 1,000 feet of a mosque had decreased in value by 4% relative to homes in the same neighborhood located within 1,000-2,000 feet of a mosque. Nine months after, homes within 1,000 feet of a mosque were devalued by 10.1%. Over the next two years, homes located with 1,000 feet of a mosque continued to decline in value relative to homes in the same neighborhood located within 1,000-2,000 feet of a mosque. By fall 2004, the impact of a post September 11th, 2001 increase in anti-Islamic reached its peak with homes located within 1,000 feet of a mosque devalued by 22.9% relative to homes in the same neighborhood located within 1,000-2,000 feet of a mosque. Taken together, this three year sustained decline in home values suggests less of an immediate abreaction to the events of September 11th, 2001, and more of a slow fomenting prejudice towards Arab Americans and Islam, coinciding with the lead-up to and first year of the Iraq war, a period defined, at least among certain cohorts, by increasing American nationalism and anti-Arab sentiment.

From fall 2004 through spring 2007, the property value impact of anti-Islamic sentiment attenuated and by September 2007, six years after the attacks, homes located within 1,000 feet of a mosque were devalued by less than 1% relative to homes in the same neighborhood located between 1,000-2,000 feet of a mosque. This attenuation can be attributed to two potential factors: a nationwide decrease in anti-Islamic sentiment brought

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34 In support of the dynamic analysis, a less restrictive analysis sample is used to increase the number of post period sales attached to homes within 1,000 feet of a mosque. This sample includes observations missing one or more of the following: a measure of building quality, number of stories, age, and square footage. To control for missing data, the dynamic specification includes a series of indicator variables for each of the aforementioned building characteristics.
on by the increasing length of time since the attacks and a rising dissatisfaction with the Iraq war dampening nationalistic anti-Arab sentiment, and a new sorting equilibrium in neighborhoods surrounding mosques, populated by fewer household types prejudiced towards Islam.

4.6.2b Varying Impact at Different Points along the Property Value Distribution

To investigate whether a post September 11th, 2001 increase in anti-Islamic sentiment has had an equal impact on heterogeneous properties, I run a simultaneous quantile regression estimating the impact of proximity to a mosque for homes at different points along Baltimore’s property value distribution and graph the resulting coefficient estimates in Figure 4.7.

Interestingly, as property values increase, the magnitude of the estimated impact of anti-Islamic sentiment declines. At the 20th percentile of the property value distribution, proximity to a mosque post terrorist attacks is estimated to reduce property values by over 30%. At the 40th percentile of the property value distribution, proximity to a mosque post terrorist attacks only reduces property values by an estimated 8.6%. And for homes at the 60th and 80th percentiles of the property value distribution, the estimated effect of proximity to a mosque post terrorist attacks is statistically insignificant. This declining impact potentially suggests a more moderated reaction among wealthier households, which is investigated below.

35 Because of computational constraints, this regression includes neighborhood and year fixed effects instead of neighborhood-by-year fixed effects.
4.6.3 Differential Impact across Homes Stratified by Neighborhood Income

As discussed in the theoretical framework (Section 4.3), the impact of a post September 11th, 2001 increase in anti-Islamic sentiment varies across income-taste classes, with certain household types willing to pay more to avoid proximity to a mosque, and others willing to pay less. In order to better understand this heterogeneous effect, I investigate the differential impact of proximity to a mosque, post terrorist attacks, across pre-period neighborhood-level median household income by re-estimating equation (4.10) with two additional interaction terms: \( D_{i1}^{1,000} \times Post_t \times Neighborhood\ Income \) and \( D_{i1}^{1,000} \times Post_t \times Neighborhood\ Income^2 \). The coefficient estimates attached to the interaction terms are statistically significant at the 1% level, and results are graphed in Figure 4.8.

For homes located in neighborhoods characterized by a median household income of less than $30,000, post September 11th, 2001 proximity to a mosque resulted in an approximately 20% property value decline. As illustrated in Figure 4.8, starting at $30,000, the impact of anti-Islamic sentiment begins to decrease as median household income climbs. For homes located in neighborhoods with a median household income of $50,000, post September 11th, 2001 proximity to a mosque resulted in only a 12% property value decline. At the top of the income distribution for neighborhoods surrounding mosques, proximity to a mosque post terrorist attacks had a negligible property value impact.

This declining impact coheres with results from the simultaneous quantile regression and suggests that households in wealthier neighborhoods were less likely to perceive proximity to a mosque as a neighborhood disamenity post September 11th, 2001. To the extent income is correlated with education, the muted property value impact among higher
income households may be a function of a broader cultural understanding and tolerance attained through higher education.\textsuperscript{36}

4.7 Sensitivity Tests

4.7.1 Two Alternative Measures of Proximity to a Mosque

As a robustness check, I re-estimate equation (4.10) using two alternative measures of proximity, homes located within 500 feet of a mosque and homes located within 750 feet of a mosque. Using 500 feet as my measure of proximity, I find that post September 11th, 2001 homes sold within 500 of a mosque decline in value by 16.4% relative to homes sold between 500 and 2,000 feet of a mosque, a result statistically significant at the 5% level (Table 4.4, column 1). Using 750 feet as my measure of proximity, I find that homes sold within 750 feet of a mosque decline in value by 23.1% relative to homes sold between 750 and 2,000 feet of a mosque, a result statistically significant at the 1% level (Table 4.4, column 2). I attribute the larger magnitude of this second estimate to a cleaner division of affected and non-affected properties. When examining homes located within 500 feet of a mosque relative to homes located within 500 to 2,000 feet of a mosque, my control group contains a

\textsuperscript{36}To more directly test this hypothesis, I examine the differential impact of proximity to a mosque, post-terrorist attacks, across neighborhood-level educational attainment by re-estimating equation (4.10) with two additional interaction terms: $D_{1,000}^t \times \text{Post}_t \times \text{Neighborhood Education}$ and $D_{1,000}^t \times \text{Post}_t \times \text{Neighborhood Education}^2$. The coefficient estimates attached to the added interaction terms are statistically significant at the 5% level and indicate that for homes located in neighborhoods with less than 40 percent of the over 25 population college educated, post September 11th, 2001 proximity to a mosque resulted in an approximately 21% property value decline. Alternatively, for homes located in neighborhoods with 50 percent of the over 25 population college educated, post September 11th, 2001 proximity to a mosque resulted in only a 13% property value decline. For homes located in neighborhoods with more than 60 percent of the over 25 population college educated, post September 11th, 2001 proximity to a mosque had a negligible property value impact. Because neighborhood-level educational attainment is positively correlated with income, these results cannot be completely deconvoluted from the effect of wealth.
cohort of homes still significantly impacted by proximity to a mosque, which mutes any
differential estimated impact.

4.7.2 Proximity to a Jewish Temple

Given that among properties situated near places of worship, only those in close
proximity to a mosque are likely to be detrimentally impacted by an increase in anti-Islamic
sentiment, I run a falsification test by re-estimating equation (4.10) with a placebo treatment
group, homes located within 1,000 feet of a Jewish temple. If I find that areas surrounding
Jewish temples were similarly negatively affected, it would call into question my proposed
mechanism of impact. Alternatively, if I find no negative effect, it would suggest there is
something specific to areas surrounding mosques that led to their negative property value
growth post 9/11. Running the falsification test, I find that post attack proximity to a
Jewish temple has no statistically significant effect on property values (Table 4.5).

4.7.3 False Event Date

Next, to test the validity of my results, I run a falsification test using an erroneous
attack date set two years prior to September 11, 2001. Confining my sample to pre-period
data, I re-estimate equation (4.10) with this false event date, and find no statistically
significant result (Table 4.6). This formal test of the parallel trends assumption, which
underlies my difference-in-difference specification, indicates no evidence of a spurious
effect. Unfortunately, I am unable to run a similar test with an erroneous attack date set one
year prior to September 11, 2001 because of a paucity of September 2000–September 2001
sales data for homes located within 1,000 feet of a mosque.
4.8 Conclusion

In this paper I examine the impact of the post September 11th, 2001 increase in anti-Islamic sentiment on property values, specifically investigating whether after the terrorist attacks homes within close proximity to a mosque dropped in value. Using a difference-in-difference methodology, I find that post terrorist attacks, homes located within 1,000 feet of a mosque decreased in value by over 17% relative to homes located between 1,000-2,000 feet of a mosque. This decline in value is statistically significant at the one percent level and robust to a number of specifications.

Recent events involving a proposed mosque near the site of the former World Trade Center have reignited a national discussion about anti-Islamic sentiment and residential proximity to local mosques. Across the country, news reports have abounded with residents voicing dissatisfaction about their relative proximity to nearby mosques. Although too soon to formally test, it’s likely this new wave of anti-Islamic sentiment has again detrimentally impacted property values for homes in close proximity to a mosque.
Figures and Tables

Figure 2.1: Title 1 Elementary Schools and 2004 NCLB “Failing” Designations Sorted by Average Student-Level Proficiency.

![Bar Chart](image)

Figure 2.2: Bid Functions for Three Income-taste Classes.

![Graph](image)
Figure 2.3: The Impact of a NCLB “Failing” Designation on Bids.
Figure 2.4: Map of 2004-2005 Title 1 and “Failing” Elementary Schools in Mecklenburg County.
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Figure 2.8: Pre and Post-“Failing” Announcement Bids: Re-Sorting.
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![Graph showing the percentage change in median household income compared to the impact of a NCLB Failing School Designation.](image)

Figure 3.1: Price Trends Before and After Homicide.

![Graph showing housing prices over time relative to homicide.](image)
Figure 3.2: Indifference Curve and Budget Constraint in the Absence of a Local Homicide.

Figure 3.3: Indifference Curve and Budget Constraint in the Event of a Local Homicide.
Figure 3.4: Pre and Post-Homicide Bids: No Re-Sorting.

Figure 3.5: Pre and Post-Homicide Bids: Re-Sorting.
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Figure 3.9: The Impact of a Homicide with Distance from the Crime.
Figure 3.10: Impact of a Homicide with Time since the Crime.

![Graph showing the impact of a homicide with time since the crime.](image)

Figure 4.1: Indifference Curve and Budget Constraint Prior to the Terrorist Attacks on September 11th, 2001.

![Indifference curve and budget constraint graph](image)

\[ U^0 = U(Z, H, S|S_j = 1, A1_1) \]
Figure 4.2: Indifference Curve and Budget Constraint after the September 11th, 2001 Terrorist Attacks.

\[ U^0 = U(Z, H, S | S_f = 1, AI_2) \]
\[ U^0 = U(Z, H, S | S_f = 1, AI_1) \]

Figure 4.3: Pre and Post-Attack Bids: No Re-Sorting.

Bid

Impact on Pre-Attack Occupants

Pre-Attack Post-Attack

Income-Taste Class A

Income-Taste Class B
Figure 4.4: Pre and Post-Attack Bids: Re-Sorting.
Figure 4.5: Homes Located within 1,000 Feet and 1,000-2,000 Feet of a Mosque.
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Figure 4.7: Coefficient Estimates from Simultaneous Quantile Regression.
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![Graph showing the percentage change in median household income with proximity to a mosque, with a coefficient estimate line reaching up to 20% and then declining to 10%.](image-url)
Table 2.1: 2003-2004 adequate yearly progress reports for four Title 1 schools.

### Hidden Valley Elementary - Did Not Meet AYP, "Failing"

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<tr>
<th>Reading Grades 3 through 8</th>
<th>All Students</th>
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### Nations Ford Elementary - Met AYP, "Non-Failing"

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<td>*</td>
<td>69.0%</td>
<td>*</td>
<td>*</td>
</tr>
<tr>
<td>Met AYP Proficiency Goal?</td>
<td>Met</td>
<td>Insuf Data</td>
<td>Insuf Data</td>
<td>Met</td>
<td>Insuf Data</td>
<td>Insuf Data</td>
<td>Insuf Data</td>
<td>Met</td>
<td>Insuf Data</td>
<td>Insuf Data</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Mathematics Grades 3 through 8</th>
<th>All Students</th>
<th>American Indian</th>
<th>Asian</th>
<th>Black</th>
<th>Hispanic</th>
<th>Multi-Racial</th>
<th>White</th>
<th>Economically Disadvantaged</th>
<th>Limited English Proficient</th>
<th>Students With Disabilities</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Students Tested</td>
<td>189</td>
<td>&lt;5</td>
<td>&lt;5</td>
<td>126</td>
<td>38</td>
<td>&lt;5</td>
<td>16</td>
<td>162</td>
<td>29</td>
<td>24</td>
</tr>
<tr>
<td>Target Goal Percent Proficient</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
</tr>
<tr>
<td>Percent Proficient</td>
<td>87.7%</td>
<td>*</td>
<td>*</td>
<td>85.5%</td>
<td>*</td>
<td>*</td>
<td>*</td>
<td>86.0%</td>
<td>*</td>
<td>*</td>
</tr>
<tr>
<td>Met AYP Proficiency Goal?</td>
<td>Met</td>
<td>Insuf Data</td>
<td>Insuf Data</td>
<td>Met</td>
<td>Insuf Data</td>
<td>Insuf Data</td>
<td>Insuf Data</td>
<td>Met</td>
<td>Insuf Data</td>
<td>Insuf Data</td>
</tr>
</tbody>
</table>
Table 2.1: 2003-2004 adequate yearly progress reports for four Title 1 schools (continued).

### Oakdale Elementary - Met AYP, "Non-Failing"

<table>
<thead>
<tr>
<th></th>
<th>All Students</th>
<th>American Indian</th>
<th>Asian</th>
<th>Black</th>
<th>Hispanic</th>
<th>Multi-Racial</th>
<th>White</th>
<th>Economically Disadvantaged</th>
<th>Limited English Proficient</th>
<th>Students With Disabilities</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Reading Grades 3 through 8</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Students Tested</td>
<td>215</td>
<td>6</td>
<td>6</td>
<td>145</td>
<td>10</td>
<td>&lt;5</td>
<td>46</td>
<td>155</td>
<td>8</td>
<td>27</td>
</tr>
<tr>
<td>Target Goal Percent Proficient</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
</tr>
<tr>
<td>Percent Proficient</td>
<td>72.8%</td>
<td>*</td>
<td>*</td>
<td>69.8%</td>
<td>*</td>
<td>*</td>
<td>84.4%</td>
<td>68.4%</td>
<td>*</td>
<td>*</td>
</tr>
<tr>
<td>Met AYP Proficiency Goal?</td>
<td><strong>Met</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Met</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Met</strong></td>
<td><strong>Met w/ CI</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>All Students</th>
<th>American Indian</th>
<th>Asian</th>
<th>Black</th>
<th>Hispanic</th>
<th>Multi-Racial</th>
<th>White</th>
<th>Economically Disadvantaged</th>
<th>Limited English Proficient</th>
<th>Students With Disabilities</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Mathematics Grades 3 through 8</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Students Tested</td>
<td>215</td>
<td>6</td>
<td>6</td>
<td>145</td>
<td>10</td>
<td>&lt;5</td>
<td>46</td>
<td>155</td>
<td>8</td>
<td>27</td>
</tr>
<tr>
<td>Target Goal Percent Proficient</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
</tr>
<tr>
<td>Percent Proficient</td>
<td>88.0%</td>
<td>*</td>
<td>*</td>
<td>85.3%</td>
<td>*</td>
<td>*</td>
<td>&gt;95%</td>
<td>86.5%</td>
<td>*</td>
<td>*</td>
</tr>
<tr>
<td>Met AYP Proficiency Goal?</td>
<td><strong>Met</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Met</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Met</strong></td>
<td><strong>Met</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
</tr>
</tbody>
</table>

### Westerly Hills - Met AYP, "Non-Failing"

<table>
<thead>
<tr>
<th></th>
<th>All Students</th>
<th>American Indian</th>
<th>Asian</th>
<th>Black</th>
<th>Hispanic</th>
<th>Multi-Racial</th>
<th>White</th>
<th>Economically Disadvantaged</th>
<th>Limited English Proficient</th>
<th>Students With Disabilities</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Reading Grades 3 through 8</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Students Tested</td>
<td>182</td>
<td>5</td>
<td>13</td>
<td>138</td>
<td>12</td>
<td>&lt;5</td>
<td>11</td>
<td>159</td>
<td>22</td>
<td>27</td>
</tr>
<tr>
<td>Target Goal Percent Proficient</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
<td>68.9%</td>
</tr>
<tr>
<td>Percent Proficient</td>
<td>66.9%</td>
<td>*</td>
<td>*</td>
<td>70.6%</td>
<td>*</td>
<td>*</td>
<td>*</td>
<td>63.2%</td>
<td>*</td>
<td>*</td>
</tr>
<tr>
<td>Met AYP Proficiency Goal?</td>
<td><strong>Met w/ CI</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Met</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Met</strong></td>
<td><strong>Met w/ CI</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>All Students</th>
<th>American Indian</th>
<th>Asian</th>
<th>Black</th>
<th>Hispanic</th>
<th>Multi-Racial</th>
<th>White</th>
<th>Economically Disadvantaged</th>
<th>Limited English Proficient</th>
<th>Students With Disabilities</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Mathematics Grades 3 through 8</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Students Tested</td>
<td>215</td>
<td>6</td>
<td>6</td>
<td>145</td>
<td>10</td>
<td>&lt;5</td>
<td>46</td>
<td>155</td>
<td>8</td>
<td>27</td>
</tr>
<tr>
<td>Target Goal Percent Proficient</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
<td>74.6%</td>
</tr>
<tr>
<td>Percent Proficient</td>
<td>76.4%</td>
<td>*</td>
<td>*</td>
<td>79.8%</td>
<td>*</td>
<td>*</td>
<td>*</td>
<td>73.5%</td>
<td>*</td>
<td>*</td>
</tr>
<tr>
<td>Met AYP Proficiency Goal?</td>
<td><strong>Met</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Met</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Met</strong></td>
<td><strong>Met w/ CI</strong></td>
<td><strong>Insuf Data</strong></td>
<td><strong>Insuf Data</strong></td>
</tr>
</tbody>
</table>
Table 2.2: Characteristics of Homes Sold in Mecklenburg County, June 2002-May 2006.

<table>
<thead>
<tr>
<th></th>
<th>Entire Sample</th>
<th>Parcels in “Failing” School Catchment Zones</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Mean</strong></td>
<td></td>
<td><strong>Mean</strong></td>
</tr>
<tr>
<td><strong>(Standard Deviation)</strong></td>
<td></td>
<td><strong>(Standard Deviation)</strong></td>
</tr>
<tr>
<td>Sale Price ($100,000)</td>
<td>2.060 (1.444)</td>
<td>1.101 (0.866)</td>
</tr>
<tr>
<td>Square Footage (1,000 Square Feet)</td>
<td>2.113 (0.916)</td>
<td>1.330 (0.487)</td>
</tr>
<tr>
<td>Quality Rating (1-6)</td>
<td>3.273 (1.212)</td>
<td>2.709 (0.963)</td>
</tr>
<tr>
<td>Age (years)</td>
<td>12.021 (11.981)</td>
<td>24.942 (10.603)</td>
</tr>
<tr>
<td>Bedrooms</td>
<td>3.261 (0.774)</td>
<td>2.645 (0.873)</td>
</tr>
<tr>
<td>Bathrooms</td>
<td>2.047 (0.640)</td>
<td>1.444 (0.573)</td>
</tr>
<tr>
<td>Air-Conditioned**</td>
<td>91.9%</td>
<td>62.2%</td>
</tr>
<tr>
<td>Percentage**</td>
<td></td>
<td>Percentage**</td>
</tr>
<tr>
<td>Story Height</td>
<td></td>
<td><strong>Percentage</strong></td>
</tr>
<tr>
<td>1</td>
<td>35.5%</td>
<td>78.5%</td>
</tr>
<tr>
<td>1.5</td>
<td>6.3%</td>
<td>4.0%</td>
</tr>
<tr>
<td>2</td>
<td>54.5%</td>
<td>14.2%</td>
</tr>
<tr>
<td>3 or more</td>
<td>3.8%</td>
<td>3.3%</td>
</tr>
<tr>
<td>Quality Tier</td>
<td></td>
<td><strong>Percentage</strong></td>
</tr>
<tr>
<td>Below Average</td>
<td>0.7%</td>
<td>2.7%</td>
</tr>
<tr>
<td>Average</td>
<td>76.5%</td>
<td>90.0%</td>
</tr>
<tr>
<td>Good</td>
<td>16.6%</td>
<td>5.3%</td>
</tr>
<tr>
<td>Very Good</td>
<td>4.5%</td>
<td>2.0%</td>
</tr>
<tr>
<td>Excellent</td>
<td>1.1%</td>
<td>0.1%</td>
</tr>
<tr>
<td>Custom</td>
<td>0.6%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Sample Size</td>
<td><strong>84,405</strong></td>
<td><strong>3,573</strong></td>
</tr>
</tbody>
</table>
Table 2.3: Impact of a “Failing” School Designation.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percentage Proficient: 4th Grade Math and Reading</td>
<td>0.002***</td>
<td>0.001***</td>
<td>0.002***</td>
<td>0.001**</td>
<td>0.002***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>&quot;Failing&quot; Designation</td>
<td>-0.071***</td>
<td>-0.048**</td>
<td>-0.058***</td>
<td>-0.076***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.023)</td>
<td>(0.020)</td>
<td>(0.023)</td>
<td></td>
</tr>
<tr>
<td>&quot;Failing&quot; Designation x Time Since Initial Announcement</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-0.180***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.058)</td>
</tr>
<tr>
<td>&quot;Failing&quot; Designation x Time Since Initial Announcement^2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.194***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.063)</td>
</tr>
<tr>
<td>&quot;Failing&quot; Designation x Time Since Initial Announcement^3</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-0.053***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.017)</td>
</tr>
<tr>
<td>Housing Characteristics</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Neighborhood and Year Fixed Effects</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Neighborhood-by-Year Fixed Effects</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Sample Restricted to Title 1 Neighborhoods</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Sample Restricted to Dual Attendance Zone Neighborhoods</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Standard Errors Clustered by School</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sample Size</td>
<td>83,569</td>
<td>83,569</td>
<td>19,201</td>
<td>28,375</td>
<td>83,569</td>
</tr>
<tr>
<td>R2</td>
<td>0.732</td>
<td>0.721</td>
<td>0.611</td>
<td>0.700</td>
<td>0.715</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1
Table 2.4: Impact of a “Failing” School Designation: Houses Stratified by Number of Bedrooms.

<table>
<thead>
<tr>
<th></th>
<th>Log of Sales Price</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>0-2 Bedrooms</td>
<td>3+ Bedrooms</td>
</tr>
<tr>
<td>&quot;Failing&quot; Designation</td>
<td>-0.051</td>
<td>-0.078***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.029)</td>
<td></td>
</tr>
<tr>
<td>Percentage Proficient: 4th Grade Math and Reading</td>
<td>0.004**</td>
<td>0.001***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.000)</td>
<td></td>
</tr>
<tr>
<td>Housing Characteristics</td>
<td>x</td>
<td>x</td>
<td></td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td>x</td>
<td>x</td>
<td></td>
</tr>
<tr>
<td>Standard Errors Clustered by . .</td>
<td>School</td>
<td>School</td>
<td></td>
</tr>
<tr>
<td>Sample Size</td>
<td>6,061</td>
<td>77,508</td>
<td></td>
</tr>
<tr>
<td>R2</td>
<td>0.733</td>
<td>0.715</td>
<td></td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Falsification Test.

<table>
<thead>
<tr>
<th></th>
<th>Baseline Estimates</th>
<th>&quot;Failing&quot; Announcement Set Two Years Prior</th>
</tr>
</thead>
<tbody>
<tr>
<td>&quot;Failing&quot; Designation</td>
<td>-0.071***</td>
<td>0.016</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>Percentage Proficient: 4th Grade Math and Reading</td>
<td>0.002***</td>
<td>0.002***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Housing Characteristics</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Standard Errors Clustered by . .</td>
<td>School</td>
<td>School</td>
</tr>
<tr>
<td>Sample Size</td>
<td>83,569</td>
<td>28,906</td>
</tr>
<tr>
<td>R2</td>
<td>0.732</td>
<td>0.702</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1
Table 3.1: Characteristics of Homes Sold in Mecklenburg County, 2000-2004.

<table>
<thead>
<tr>
<th></th>
<th>Entire Sample</th>
<th>Within 1000 Ft</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean (Standard Deviation)</td>
<td>Mean (Standard Deviation)</td>
</tr>
<tr>
<td>Sale Price ($100,000)</td>
<td>2.084 (1.370)</td>
<td>1.141 (0.911)</td>
</tr>
<tr>
<td>Square Footage (1,000 Square Feet)</td>
<td>2.074 (0.886)</td>
<td>1.372 (0.659)</td>
</tr>
<tr>
<td>Quality Rating (1-6)</td>
<td>3.257 (1.214)</td>
<td>2.458 (0.861)</td>
</tr>
<tr>
<td>Age</td>
<td>11.569 (11.941)</td>
<td>22.349 (11.579)</td>
</tr>
<tr>
<td>Bedrooms</td>
<td>3.314 (0.654)</td>
<td>2.826 (0.696)</td>
</tr>
<tr>
<td>Bathrooms</td>
<td>2.025 (0.617)</td>
<td>1.467 (0.624)</td>
</tr>
<tr>
<td>Air-Conditioned</td>
<td>92.6%</td>
<td>60.1%</td>
</tr>
<tr>
<td>Sold in Year Built</td>
<td>24.5%</td>
<td>7.9%</td>
</tr>
<tr>
<td>Story Height</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>37.8%</td>
<td>74.8%</td>
</tr>
<tr>
<td>1.5</td>
<td>6.5%</td>
<td>3.5%</td>
</tr>
<tr>
<td>2</td>
<td>51.2%</td>
<td>17.7%</td>
</tr>
<tr>
<td>3 or more</td>
<td>4.5%</td>
<td>4.0%</td>
</tr>
<tr>
<td>Quality Tier</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Below Average</td>
<td>0.7%</td>
<td>4.5%</td>
</tr>
<tr>
<td>Average</td>
<td>75.1%</td>
<td>89.8%</td>
</tr>
<tr>
<td>Good</td>
<td>18.2%</td>
<td>3.3%</td>
</tr>
<tr>
<td>Very Good</td>
<td>4.6%</td>
<td>2.4%</td>
</tr>
<tr>
<td>Excellent</td>
<td>1.1%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Custom</td>
<td>0.4%</td>
<td>0.0%</td>
</tr>
<tr>
<td>Sample Size</td>
<td><strong>87,617</strong></td>
<td><strong>840</strong></td>
</tr>
</tbody>
</table>
### Table 3.2: Pre-Homicide Differences in Characteristics of Homes Sold Close to a Homicide.

<table>
<thead>
<tr>
<th>Pre-Homicide Differences in Sales:</th>
<th>Log of Sales Price</th>
<th>Age</th>
<th>Square Footage</th>
<th>Bedrooms</th>
<th>Bathrooms</th>
</tr>
</thead>
<tbody>
<tr>
<td>Within 500 Ft of Homicide</td>
<td>0.066</td>
<td>-0.274</td>
<td>-69.914</td>
<td>-0.026</td>
<td>-0.034</td>
</tr>
<tr>
<td>Constant</td>
<td>(0.050)</td>
<td>(0.930)</td>
<td>(45.884)</td>
<td>(0.079)</td>
<td>(0.048)</td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sample Size</td>
<td>438</td>
<td>438</td>
<td>438</td>
<td>438</td>
<td>438</td>
</tr>
<tr>
<td>R2</td>
<td>0.737</td>
<td>0.824</td>
<td>0.855</td>
<td>0.633</td>
<td>0.835</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1

### Table 3.3: Impact of a Homicide on Property Value.

<table>
<thead>
<tr>
<th></th>
<th>Log of Sales Price (Pre-Homicide)</th>
<th>Log of Sales Price, Pre- and Post-Homicide</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Within 500 Ft of Homicide</td>
<td>-0.009 (0.041)</td>
<td>0.031 (0.046)</td>
</tr>
<tr>
<td>Within 500 Ft x Post-Homicide</td>
<td></td>
<td>-0.108** (0.053)</td>
</tr>
<tr>
<td>Within 500 Ft x Post x Days Since Homicide</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Within 1000 Ft of Homicide</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Within 1000 Ft x Post-Homicide</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Housing Characteristics

<table>
<thead>
<tr>
<th></th>
<th>x</th>
<th>x</th>
<th>x</th>
</tr>
</thead>
<tbody>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sample Size</td>
<td>86,564</td>
<td>86,935</td>
<td>86,935</td>
</tr>
<tr>
<td>R2</td>
<td>0.734</td>
<td>0.782</td>
<td>0.782</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1
Table 3.4– Impact of a Homicide on Property Value across High and Low Crime Areas

<table>
<thead>
<tr>
<th></th>
<th>Level of Pre-Period Total Crime</th>
<th>Level of Pre-Period Property Crime</th>
<th>Level of Pre-Period Violent Crime</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Low</td>
<td>High</td>
<td>Low</td>
</tr>
<tr>
<td></td>
<td>High</td>
<td></td>
<td>High</td>
</tr>
<tr>
<td></td>
<td>High</td>
<td></td>
<td>High</td>
</tr>
<tr>
<td>Within 500 Ft of Homicide</td>
<td>0.087 (0.054)</td>
<td>0.069 (0.067)</td>
<td>0.107* (0.064)</td>
</tr>
<tr>
<td></td>
<td>0.060 (0.062)</td>
<td>0.068 (0.070)</td>
<td>0.073 (0.059)</td>
</tr>
<tr>
<td>Within 500 Ft x Post-Homicide</td>
<td>-0.294*** (0.089)</td>
<td>-0.099 (0.110)</td>
<td>-0.316*** (0.102)</td>
</tr>
<tr>
<td></td>
<td>-0.098 (0.102)</td>
<td>-0.182* (0.099)</td>
<td>-0.167* (0.089)</td>
</tr>
<tr>
<td>Within 1000 Ft of Homicide</td>
<td>-0.000 (0.025)</td>
<td>-0.063 (0.044)</td>
<td>0.009 (0.027)</td>
</tr>
<tr>
<td></td>
<td>-0.064 (0.041)</td>
<td>0.028 (0.025)</td>
<td>-0.077* (0.041)</td>
</tr>
<tr>
<td>Within 1000 Ft x Post-Homicide</td>
<td>0.015 (0.043)</td>
<td>0.042 (0.065)</td>
<td>0.018 (0.050)</td>
</tr>
<tr>
<td></td>
<td>0.039 (0.062)</td>
<td>-0.045 (0.057)</td>
<td>0.075 (0.054)</td>
</tr>
<tr>
<td>Housing Characteristics</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Standard Errors Clustered by . .</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Sample Size</td>
<td>35,483</td>
<td>35,061</td>
<td>34,905</td>
</tr>
<tr>
<td></td>
<td>35,639</td>
<td>34,237</td>
<td>36,307</td>
</tr>
<tr>
<td>R2</td>
<td>0.795</td>
<td>0.803</td>
<td>0.795</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1
Table 3.5: Falsification Tests.

<table>
<thead>
<tr>
<th></th>
<th>Baseline Estimates</th>
<th>One-Year Prior Homicide Dates</th>
<th>Two-Year Prior Homicide Dates</th>
</tr>
</thead>
<tbody>
<tr>
<td>Within 500 Ft of Homicide</td>
<td>0.077 (0.048)</td>
<td>0.071 (0.049)</td>
<td>0.021 (0.031)</td>
</tr>
<tr>
<td>Within 500 Ft x Post-Homicide</td>
<td>-0.144** (0.057)</td>
<td>-0.015 (0.069)</td>
<td>0.040 (0.048)</td>
</tr>
<tr>
<td>Within 1000 Ft of Homicide</td>
<td>-0.054** (0.025)</td>
<td>-0.046 (0.036)</td>
<td>-0.035* (0.020)</td>
</tr>
<tr>
<td>Within 1000 Ft x Post-Homicide</td>
<td>0.037 (0.044)</td>
<td>-0.001 (0.039)</td>
<td>0.006 (0.033)</td>
</tr>
<tr>
<td>Housing Characteristics</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Standard Errors Clustered by . .</td>
<td>Neighborhood</td>
<td>Neighborhood</td>
<td>Neighborhood</td>
</tr>
<tr>
<td>Sample Size</td>
<td>86,564</td>
<td>83,995</td>
<td>81,994</td>
</tr>
<tr>
<td>R2</td>
<td>0.734</td>
<td>0.812</td>
<td>0.852</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1

<table>
<thead>
<tr>
<th></th>
<th>Entire Sample</th>
<th>Within 2,000 Ft</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Mean</td>
</tr>
<tr>
<td></td>
<td>(Standard Deviation)</td>
<td>(Standard Deviation)</td>
</tr>
<tr>
<td>Sale Price ($100,000)</td>
<td>2.265</td>
<td>2.079</td>
</tr>
<tr>
<td>Square Footage (1,000 Square Feet)</td>
<td>1.508</td>
<td>1.636</td>
</tr>
<tr>
<td>Age</td>
<td>48.876</td>
<td>51.238</td>
</tr>
<tr>
<td></td>
<td>(23.618)</td>
<td>(22.122)</td>
</tr>
<tr>
<td>Story Height</td>
<td></td>
<td>Percentage</td>
</tr>
<tr>
<td>1</td>
<td>16.0%</td>
<td>23.0%</td>
</tr>
<tr>
<td>1.5</td>
<td>11.4%</td>
<td>8.0%</td>
</tr>
<tr>
<td>2</td>
<td>64.5%</td>
<td>49.7%</td>
</tr>
<tr>
<td>2.5 or More</td>
<td>8.1%</td>
<td>19.4%</td>
</tr>
<tr>
<td>Quality Tier</td>
<td></td>
<td>Percentage</td>
</tr>
<tr>
<td>1</td>
<td>0.8%</td>
<td>0.7%</td>
</tr>
<tr>
<td>2</td>
<td>52.5%</td>
<td>50.1%</td>
</tr>
<tr>
<td>3</td>
<td>37.1%</td>
<td>43.5%</td>
</tr>
<tr>
<td>4</td>
<td>8.2%</td>
<td>5.0%</td>
</tr>
<tr>
<td>5</td>
<td>1.2%</td>
<td>0.5%</td>
</tr>
<tr>
<td>6</td>
<td>0.2%</td>
<td>0.2%</td>
</tr>
<tr>
<td>Sample Size</td>
<td>146,050</td>
<td>1,054</td>
</tr>
</tbody>
</table>

Table 4.2: Pre-Terrorist Attack Differences in Characteristics of Homes Sold Close to a Mosque

<table>
<thead>
<tr>
<th>Pre-Terrorist Attack Differences:</th>
<th>Log of Sales Price</th>
<th>Age</th>
<th>Square Footage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Within 1,000 Ft of Mosque</td>
<td>0.052</td>
<td>-3.506</td>
<td>-147.178</td>
</tr>
<tr>
<td>Constant</td>
<td>(0.069)</td>
<td>(3.523)</td>
<td>(126.113)</td>
</tr>
<tr>
<td>11.758***</td>
<td>(0.018)</td>
<td>(0.910)</td>
<td>(32.579)</td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Sample Size</td>
<td>360</td>
<td>360</td>
<td>360</td>
</tr>
<tr>
<td>R2</td>
<td>0.656</td>
<td>0.401</td>
<td>0.396</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1
Standard Errors Clustered by Neighborhood-Year
Table 4.3: Impact of Post September 11th, 2001 Increase in Anti-Islamic Sentiment.

<table>
<thead>
<tr>
<th>Log of Sales Price, Pre- and Post-Terrorist Attacks</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Within 1,000 Ft of Mosque</td>
<td>-0.080***</td>
<td>0.086**</td>
<td>0.146***</td>
<td>0.121**</td>
<td>0.107</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.035)</td>
<td>(0.055)</td>
<td>(0.052)</td>
<td>(0.066)</td>
</tr>
<tr>
<td>Within 1,000 Ft x Post-Terrorist Attacks</td>
<td>-0.107**</td>
<td>-0.141***</td>
<td>-0.195***</td>
<td>-0.188***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.054)</td>
<td>(0.072)</td>
<td>(0.070)</td>
<td></td>
</tr>
<tr>
<td>Within 1,000 Ft x Post x Time Since Terrorist</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-0.173***</td>
</tr>
<tr>
<td>Attacks</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.062)</td>
</tr>
<tr>
<td>Within 1,000 Ft x Post x Time Since Terrorist</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Attacks^2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.029***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.011)</td>
</tr>
<tr>
<td>Within 2,000 Ft of Mosque</td>
<td></td>
<td></td>
<td>-0.227***</td>
<td>-0.052</td>
<td>-0.066</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.060)</td>
<td>(0.054)</td>
<td>(0.052)</td>
</tr>
<tr>
<td>Within 2,000 Ft x Post-Terrorist Attacks</td>
<td>0.089</td>
<td>0.069</td>
<td>0.061</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.068)</td>
<td>(0.059)</td>
<td>(0.059)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Housing Characteristics</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Sale Year Fixed Effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>x</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td></td>
</tr>
<tr>
<td>Sample Size</td>
<td>146,050</td>
<td>146,050</td>
<td>146,050</td>
<td>146,050</td>
<td>151,169</td>
</tr>
<tr>
<td>R2</td>
<td>0.665</td>
<td>0.751</td>
<td>0.665</td>
<td>0.751</td>
<td>0.738</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1
Standard Errors Clustered by Neighborhood-Year
Table 4.4: Impact at Varying Proximities to a Mosque.

<table>
<thead>
<tr>
<th>Distance from a Mosque</th>
<th>Log of Sales Price, Pre- and Post-Terrorist Attacks</th>
</tr>
</thead>
<tbody>
<tr>
<td>Within 500 Ft of a Mosque</td>
<td>0.131**</td>
</tr>
<tr>
<td>(0.063)</td>
<td></td>
</tr>
<tr>
<td>Within 500 Ft x Post-Terrorist Attacks</td>
<td><strong>-0.179</strong></td>
</tr>
<tr>
<td>(0.085)</td>
<td></td>
</tr>
<tr>
<td>Within 750 Ft of a Mosque</td>
<td>0.147**</td>
</tr>
<tr>
<td>(0.067)</td>
<td></td>
</tr>
<tr>
<td>Within 750 Ft x Post-Terrorist Attacks</td>
<td><strong>-0.263</strong>*</td>
</tr>
<tr>
<td>(0.091)</td>
<td></td>
</tr>
<tr>
<td>Within 2,000 Ft of Mosque</td>
<td>-0.035</td>
</tr>
<tr>
<td>(0.049)</td>
<td></td>
</tr>
<tr>
<td>(0.052)</td>
<td></td>
</tr>
<tr>
<td>Within 2,000 Ft x Post-Terrorist Attacks</td>
<td>0.043</td>
</tr>
<tr>
<td>(0.054)</td>
<td></td>
</tr>
<tr>
<td>(0.057)</td>
<td></td>
</tr>
<tr>
<td>Housing Characteristics</td>
<td>x</td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td>x</td>
</tr>
<tr>
<td>Sample Size</td>
<td>146,050</td>
</tr>
<tr>
<td>R2</td>
<td>0.751</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1
Standard Errors Clustered by Neighborhood-Year

Table 4.5: Distance from a Jewish Temple.

<table>
<thead>
<tr>
<th>Distance from a Mosque</th>
<th>Distance from a Temple</th>
</tr>
</thead>
<tbody>
<tr>
<td>Within 1,000 Ft</td>
<td>0.121**</td>
</tr>
<tr>
<td>(0.052)</td>
<td>(0.025)</td>
</tr>
<tr>
<td>Within 1,000 Ft x Post-Terrorist Attacks</td>
<td><strong>-0.188</strong>*</td>
</tr>
<tr>
<td>(0.070)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>Within 2,000 Ft</td>
<td>-0.052</td>
</tr>
<tr>
<td>(0.054)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Within 2,000 Ft x Post-Terrorist Attacks</td>
<td>0.069</td>
</tr>
<tr>
<td>(0.059)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Housing Characteristics</td>
<td>x</td>
</tr>
<tr>
<td>Neighborhood-Year Fixed Effects</td>
<td>x</td>
</tr>
<tr>
<td>Sample Size</td>
<td>146,050</td>
</tr>
<tr>
<td>R2</td>
<td>0.751</td>
</tr>
</tbody>
</table>

Note: *** p<0.01, ** p<0.05, * p<0.1
Standard Errors Clustered by Neighborhood-Year
Table 4.6: Falsification Test.

<table>
<thead>
<tr>
<th></th>
<th>Baseline Estimates</th>
<th>Terrorist Attacks Dated Two-Years Prior</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Within 1,000 Ft of Mosque</strong></td>
<td>0.121** (0.052)</td>
<td>0.132 (0.082)</td>
</tr>
<tr>
<td><strong>Within 1,000 Ft x Post-Terrorist Attacks</strong></td>
<td><strong>-0.188</strong>* (0.070)**</td>
<td><strong>-0.016</strong> (0.103)</td>
</tr>
<tr>
<td><strong>Within 2,000 Ft of Mosque</strong></td>
<td>-0.052 (0.054)</td>
<td>-0.031 (0.095)</td>
</tr>
<tr>
<td><strong>Within 2,000 Ft x Post-Terrorist Attacks</strong></td>
<td>0.069 (0.059)</td>
<td>-0.013 (0.103)</td>
</tr>
</tbody>
</table>

Housing Characteristics
Neighborhood-Year Fixed Effects

Sample Size 146,050 54,615
R2 0.751 0.716

Note: *** p<0.01, ** p<0.05, * p<0.1
Standard Errors Clustered by Neighborhood-Year
Bibliography


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