THREE ESSAYS ON THE IMPACT OF DEMOGRAPHIC AND ENVIRONMENTAL CHANGES ON HOME SALES

by

MIA GOODNATURE

AMANDA ROSS, COMMITTEE CHAIR
DANIEL J. HENDERSON
ERIK B. JOHNSON
ALECIA CASSIDY
LAWRENCE POWELL

A DISSERTATION

Submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy
in the Department of Economics, Finance, and Legal Studies
in the Graduate School of
The University of Alabama

TUSCALOOSA, ALABAMA

2020

ABSTRACT

Gentrification occurs when low-income areas transition into higher-income neighborhoods. Chapter 1 examines one possible driver of gentrification: the influx of same-sex couples into a community. Anecdotal evidence suggests that there is a relationship between same-sex couples and gentrification, but this could be because these couples sort into neighborhoods that are more likely to gentrify. To address the endogeneity problem, we employ an instrumental variables strategy using voting results for the state-level equivalent of the Defense of Marriage Act in Ohio as an instrument for the change in the number of same-sex couples. We find that areas with a higher change in the number of same-sex couples are more likely to experience gentrification. In addition, using semi-parametric techniques, we find there is a tipping point after which gentrification is more likely to occur. Overall, our results suggest that same-sex couples can initiate gentrification, but there is a threshold that has to be met for neighborhood change to be more likely to occur. These findings are important for policy makers because understanding the drivers of gentrification is crucial to designing effective policy to revitalize urban neighborhoods and address any problems attributed to gentrification.

Chapter 2 identifies same-sex couple households who purchase homes together and evaluates the concentration of their residential location. We draw upon a novel data set of real estate transactions from Miami-Dade County, Florida; Franklin County, Ohio; and King County, Washington. We are able to separately identify male same-sex couple homebuyers and female same-sex couple homebuyers at the property level by predicting the homebuyers' sex based on homebuyers' full names. To show that the method suggested in this paper to identify members of the LGBTQ+ community is identifying same-sex couple homebuyers, we compare distributions from the Decennial Census and look at summary statistics of houses

purchased by same-sex couples. Based on these comparisons, this method is accurately identifying same-sex couple homebuyers.

As hurricane destruction has become more frequent and more dramatic, it is important to understand how communities respond to this damage. Chapter 3 explores how the selling price of houses responds to spillover effects of living near houses with hurricane-induced damages and repairs. These spillover effects are investigated in Punta Gorda, Florida, which was hit by Hurricane Charley, a Category 4 hurricane, in August 2004. Results indicate that house prices temporarily increase after the hurricane. Nearby damaged houses have no statistically significant effect. Nearby houses that were repaired to a larger square footage have a positive spillover effect while all other repaired houses, like those that do not increase their square footage, have a negative spillover effect on housing prices.

DEDICATION

This is dedicated to my grandmother Dr. Gaby Berryer. She set the bar high for me through her actions and encouragement. A wonderful example of a powerful, strong, sassy female - I hope I can follow in your footsteps.

ACKNOWLEDGMENTS

I would like to take this opportunity to thank all the people who played a part in this phase of my academic journey. However, I also greatly appreciate everyone who allowed and encouraged me to reach this point in my career.

First of all, I would like thank my advisor, Dr. Amanda Ross, for all the input and advice throughout this process. I also appreciate the laughter that we have shared over the years. I would like to thank Dr. Daniel J. Henderson, Dr. Erik B. Johnson, Dr. Alecia Cassidy, Dr. Lawrence Powell, and Dr. Amir B. Ferreira Neto for taking an interest in my research. Thank you for all the valuable help over the last several years.

My cohort, Andre Harrison, Jay Herndon, Jiahong Gao, Hasan Isomitdinov, Olivia Lu, Tina Zhang, and Virginia Zhong, has been instrumental in my academic success through their advice and friendship. I appreciate all our encounters, whether it be discussion about assignments, exams, research, or just a friendly chat. I would also like to extend gratitude to Kalee Burns, Dr. Laura Connolly, Rushaine Goulbourne, Dr. Matthew Hampton, Natalie Millar, Dr. Shamar Stewart, Dr. Alice Sheehan, and Dr. Charlotte Souto for all the emotional support over the years. You have provided me with tremendous advice throughout this journey. Your willingness to stop whatever you were doing to help me in my times of need were greatly appreciated.

It is important for me to extend gratitude to the administrative staff in the Department of Economics, Finance, and Legal Studies. I would like to thank RJ Thurman for helping me with the job market process. To Debra Wheatley, thank you for just about everything. A special thank you goes out to Angela Marcum as well.

I also acknowledge the support of my family. My father, Bryce Goodnature, supported me in so many ways on this journey. My mother, Dr. Yvanne Berryer, has been another powerful female role model for me to look up to. Nothing would be written without the help of my sister Mel Nicnick. Thank you Marie Goodnature and Michael Goodnature for shaping me into the person I am today. My aunts Kris Berryer and Marie Frederick reminded me to enjoy the process. I would not be where I am today without the support of my friends Kaitlin Bouwkamp, Miranda Frederick, Christina Healy, Lauren Serig, Dr. Suzy Thornton, and Spencer Wilkins.

Finally, I would like to thank Adam Baumgartner. This process brought me to a lot of lows and a lot of highs and Adam made each experience better. Thank you for being here for me every step of the way.

Thank you.

CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1

DO SAME-SEX COUPLES INDUCE GENTRIFICATION? (WITH AMANDA ROSS AND DANIEL J. HENDERSON)

1.1 Introduction

Although a standard definition does not exist, gentrification is generally associated with increases in household incomes, education levels, and property values, as well as decreases in poverty rates (Kennedy and Leonard, 2001). When gentrification occurs, high income, more educated households move into an area and replace poor, often minority residents (Guerrieri et al., 2013; Meltzer and Ghorbani, 2017). The existing housing stock and local amenities tend to improve in gentrifying neighborhoods (Christafore and Leguizamon, 2019). While some argue that gentrification is desirable because decaying neighborhoods become vibrant areas again, others argue that it harms current residents by displacing poor households when property values rise. While researchers and policy makers are interested in the consequences of gentrification, understanding what causes areas to gentrify is equally important. Although there are many theories about what causes neighborhood change, isolating a specific mechanism is problematic due to omitted variable bias.

In this paper, we examine one possible driver of gentrification – the presence of same-sex couples – and draw upon an instrumental variables strategy to isolate this relationship. Anecdotal evidence suggests that same-sex couples may induce neighborhood change.[1] For

[1] There are many individuals in the LGBTQ+ community who could contribute to neighborhood change. Unfortunately, data limitations prevent researchers from capturing all individuals in this community. In this paper, we focus on the effect of co-habitating same-sex couples on gentrification, not the entire LGBTQ+ population.

example, the Castro district in San Francisco had a large gay male and lesbian population and experienced rising property values and condominium conversions. As gentrification continued to become more prevalent, it is important to understand what drives this neighborhood change.[2]

While there are numerous case studies suggesting that there is a relationship between same-sex couples and gentrification, it may be argued that these couples are not directly causing gentrification, but that they self-select into neighborhoods that would have gentrified anyway. For example, same-sex couples are less likely to have children and thus are less likely to spend their income on child-related amenities, such as higher-quality public schools (Black et al., 2002). Therefore, same-sex couples may spend more of their money on adult-related amenities. If this is true, then the areas that same-sex couples self-select into may be more likely to gentrify due to the amenities in the area. If unobservable neighborhood amenities are present and cannot be controlled for properly, then results could be misinterpreted and researchers may conclude that these couples are causing gentrification. Since it is difficult to separate out these effects in a standard OLS framework, as well as to gather data on every amenity that could affect neighborhood change, the standard OLS methods may produce biased estimates.

To isolate the relationship between same-sex couples and gentrification, we draw upon an instrumental variables (IV) strategy. We use precinct-level voting data for the state-level equivalent of the Defense of Marriage Act (DOMA) in Ohio as an instrument for the number of additional same-sex couples in an area. This ballot initiative allowed Ohio residents to vote on the legalization of same-sex marriage in the state. We believe that voting outcomes on this initiative will be correlated with changes in the number of same-sex couples in an area because voting for legalizing same-sex marriage indicates that an area is supportive of these couples and their lifestyle. Tolerance towards same-sex couples is important for these couples, as they frequently face discrimination and safety concerns (Herek, 2009; Christafore

[2] McKinnish et al. (2010) also examine what types of people cause areas to gentrify.

and Leguizamon, 2012; Christafore et al., 2013). Therfore, we believe that same-sex couples are likely to be attracted to areas that are more tolerant of their lifestyle. Our first stage regressions support using the percent of a census tract that voted against having marriage legally defined as solely between a man and a woman in 2004 as an instrument for the change in the number of same-sex couples between 2000 and 2010. Furthermore, we argue that voting on this law will not be correlated with our outcome variable that measures gentrification after including our control variables.

Using our instrumental variables approach, we find that areas that experience a larger change in the number of same-sex couples are more likely to gentrify. Our results suggest that when there is an additional same-sex couple per 1,000 households between 2000 and 2010,[3] there is an average increase in median income of about $1,997.86 and an average increase in median house price of about $4,520.08 for urban census tracts in the bottom income and house price quartile, respectively. Our results are consistent across various definitions of gentrification, including the percent change in income for tracts in the bottom income quartile and the probability that a tract gentrifies, where gentrification is defined as going from the bottom income quartile to the top two income quartiles. These findings are robust across various specifications, such as restricting the sample to only urban areas. We also look at different measures of same-sex couples, such as using the change in the so-called Gay Index, developed by Florida and Gates (2015), and our results are similar. Furthermore, we consider the effect of opposite-sex couples on neighborhood change and find either statistically insignificant effects or effects that are significantly smaller than those found for same-sex couples. Overall, we find consistent evidence that same-sex couples are contributing to increases in income and house prices for neighborhoods in the lowest quartile of the income and house price distribution.

While same-sex couples may induce gentrification, it seems plausible that this relationship

[3]In all of the tracts considered in this analysis, there are an average of 1,147 households in each tract in 2000. In urban tracts that are in the bottom income quartile in 2000, the average number of households is 1,103. These numbers correspond to 1,031 and 970 in 2010.

is non-linear. For example, it seems unlikely that one couple will drive an entire neighborhood to become a significantly higher income area or to have notably higher property values. It is more likely that there is a threshold of same-sex couples needed to move into and invest in the area before significant neighborhood change occurs. To allow for the possibility of a non-linear relationship, we use semi-parametric techniques to examine how changes in the number of same-sex couples affect the likelihood that a neighborhood gentrifies. Using a semi-parametric instrumental variables methodology, we find that there is a tipping point of approximately 20 additional same-sex couples per 1,000 households, after which a neighborhood is more likely to gentrify. This is consistent with previous research that finds that a tipping point in neighborhood segregation exists after which neighborhoods are more likely to transition (Card et al., 2008).

Our results and analysis make several contributions to the literature. First, we add to a growing literature that is working to understand the mechanisms driving gentrification, such as artists (Florida and Mellander, 2010; Schuetz, 2014), amenities (Couture and Handbury, 2017), distance to the Central Business District (CBD) (Kolko, 2007; Edlund et al., 2015), the desire to live next to richer neighborhoods (Guerrieri et al., 2013), and changes in the housing market (Wyly and Hammel, 1999; Bostic and Martin, 2003; Boustan et al., 2019). Policy makers are interested in gentrification, as there are concerns about displacement of low-income families when low-income neighborhoods transition into higher-income areas and there are simultaneous interests in revitalizing declining urban neighborhoods.[4] Therefore, understanding what drives gentrification is important to designing effective policy.

Second, while other papers have explored the possibility that same-sex couples affect gentrification, we improve upon this research by using an instrumental variables strategy to improve identification. Christafore and Leguizamon (2017) examine the relationship between

[4]Several recent papers look at the issue of displacement of low-income residents. In general, this research has found that the low-income population that is generally associated with displacement is a mobile population in general, and there is little difference in the probability that a household moves out of a gentrifying tract compared to the population in low-income tracts that do not gentrify (Brummet and Reed, 2019; Dragan et al., 2019).

same-sex couples and gentrification.[5] While they utilize an extensive number of controls in their analysis, there are still likely to be other uncontrolled characteristics of neighborhoods that affect gentrification. We improve upon the literature by including additional control variables and by incorporating an IV approach. We are the first paper, to our knowledge, to use an instrumental variable at a micro-level to examine the relationship between same-sex couples and gentrification.[6]

Finally, we are the first, to our knowledge, to utilize semi-parametric methods to examine potential non-linearities in the relationship between same-sex couples and gentrification. These techniques allow us to gain insights into possible tipping points when examining neighborhood change. Previous papers have examined various tipping points that result in neighborhood change such as when neighborhoods transition from white to black (Schelling, 1971; Miyao, 1978; Becker and Murphy, 2000; Card et al., 2008; Zhang, 2011), high-income to low-income (Bond and Coulson, 1989; Malone, 2020), and owner-occupied houses to renter-occupied properties (Coulson and Wommer, 2019). We examine neighborhood change by looking at the number of same-sex couple households that move into the neighborhood before there is a large change in income or house prices.

The remainder of the paper will proceed as follows. The next section discusses the theories and literature regarding how same-sex couples may induce gentrification. Section 1.3 describes our methodology and the data is discussed in Section 1.4. Parametric results are presented in Section 1.5 while Section 1.6 discusses the parametric effect of opposite-sex couples. The semi-parametric model and results are presented in Section 1.7. Section 1.8 concludes and discusses the policy implications of our research.

[5]Some papers used same-sex couples as a proxy for tolerance to see if tolerance affects gentrification (Fu, 2008; Florida and Mellander, 2010; Berggren and Elinder, 2012; Leguizamon and Leguizamon, 2017).

[6]Fu (2008) investigates the effect that the percentage of same-sex unmarried partners in the city has on individual house prices in that city. As an instrument he uses a dummy variable for if a city has passed a law that prevents discrimination based on sexual orientation in public employment. We use the percent that voted on a gay-related law in individual census tracts as our instrumental variable. As gentrification typically occurs in specific neighborhoods, using a smaller level of geography will be more appropriate for a gentrification analysis.

1.2 Conceptual Model

Gentrification began receiving significant attention from policy makers and the media in the 1980s, when low-income, predominately black neighborhoods such as Harlem in New York City transitioned into higher-income neighborhoods (Schaffer and Smith, 1986). Interest in gentrification has increased even more since then, as more and more low-income neighborhoods are said to have gentrified. One reason this is such a concern is because the public is concerned with potential displacement of low-income residents from gentrifying neighborhoods.[7]

Existing research has examined various issues related to gentrification – many examining the consequences (Ellen and O'Regan, 2011; Dastrup and Ellen, 2016; Ding and Hwang, 2016; Meltzer, 2016; Autor et al., 2017). Much of the initial literature on the causes of gentrification assumed that proximity to the CBD resulted in higher-income families moving back into the downtown areas to reduce their commute to work (Helms, 2003; Rosenthal, 2008). Kolko (2007) and Guerrieri et al. (2013) examined the importance of location, specifically the possibility that tracts directly adjacent to higher-income tracts are more likely to gentrify. More recently, several papers have considered the effects of artists and Bohemians on gentrification, generally finding a positive relationship (Florida and Mellander, 2010; Schuetz, 2014).

In more recent literature, same-sex couples have been explored as a possible driver of neighborhood change (Collins, 2004; Fu, 2008; Florida and Mellander, 2010; Berggren and Elinder, 2012; Christafore and Leguizamon, 2017; Leguizamon and Leguizamon, 2017). One reason that people believe same-sex couples may induce gentrification is that these couples are less likely to have children than straight couples,[8] which affects location preferences and spending behaviors (Black et al., 2002, 2007b). Therefore, couples without children may

[7] For the effect of gentrification on displacement, see Vigdor (2002), Freeman and Braconi (2004), Freeman (2005), Newman and Wyly (2006), McKinnish et al. (2010), Doan and Higgins (2011), Ellen and O'Regan (2011), Ding et al. (2016), Brummet and Reed (2019), Dragan et al. (2019).

[8] While 62.0% of heterosexual couples have children, only 9.7% of gay male couples and 22.1% of lesbian couples have children (Black et al., 2007b).

spend their income on activities like eating out and home improvements, as they do not have the expenses or commitments associated with children.

A priori, there are reasons to think same-sex couples may sort into expensive neighborhoods as well as reasons to expect them to sort into rundown neighborhoods. On one hand, same-sex couples may choose to live in high-amenity urban areas if they spend their income on high-quality, adult-related amenities (Black et al., 2007a). These amenities will be capitalized into housing prices, causing these neighborhoods to have higher property values. Therefore, same-sex couples may choose to live in areas that are more likely to gentrify if these amenities are driving gentrification. Alternatively, discrimination might force same-sex couples to locate in areas that are less desirable, such as decaying urban areas with high crime rates (Black et al., 2002, 2007b).[9] This discrimination might also lead to same-sex couples clustering together for safety reasons (Black et al., 2007b). These clusters create a safe, more welcoming environment for same-sex couples' lifestyles. Once there, same-sex couples may invest in the area and their homes, increasing property values and inducing gentrification.

To see which of these effects is present in our data, consider Figure 1.1 Panel A, which maps the number of same-sex couples in 2000 that are located in census tracts in the Columbus Metropolitan Statistical Area (MSA) in Ohio. As urban tracts will be smaller and more clustered due to the higher population density, it appears that these same-sex couples are choosing to live in urban areas. Figure 1.1 Panels B and C map income and house price in tracts in 2000, respectively. We can see from these figures that in the urban areas, same-sex couples tend to live in tracts with lower incomes and lower house prices. Therefore, it seems plausible that same-sex couples are living in decaying urban areas, possibly due to discrimination that prevents them from living in nicer areas.[10]

[9] Gay men, lesbians, and bisexuals are likely to deal with discrimination and prejudice as 49.2% have experienced verbal abuse in their adult lives and 13.1% have experienced physical violence (Herek, 2009). In addition, 34.6% of gay men, lesbians, and bisexuals think that people who live nearby think less of a person who is gay (Herek, 2009).

[10] A similar pattern of locational choice appears when looking at the correlation coefficient between the number of same-sex couples and a variety of other variables. The correlation between same-sex couples and

Although the areas that gays live in might be rundown initially, Collins (2004) proposes that the clustering of gay men and lesbians could induce gentrification, specifically based on observations from the Soho District in London. Gay men and lesbians might be attracted to a rundown area where there is some signal that the area is tolerant of their lifestyle, such as the presence of a gay bar. Then, additional gay male or lesbian run establishments emerge in the neighborhood, like nightclubs, hotels, and gyms. Next, there is an increase in gay males and lesbians in the area, both living there and frequenting the area. In addition, same-sex couples, especially those without children, are likely to have a higher disposable income, which allows them to renovate the housing stock and spend money at various establishments.[11] This allows the area to be sufficiently built up and invested in, causing the quality of housing and amenities in the neighborhood to assimilate into the mainstream and be considered to have gentrified.

1.3 Methodology

We use the following initial equation to examine the relationship between same-sex couples and gentrification:

$$y_i = \beta_0 + \beta_1(\Delta \textit{same-sex couples}_i) + \beta_2 X_i + \gamma_m + \epsilon_i \tag{1.1}$$

where i indexes individual census tracts in the Akron, Cincinnati, Cleveland, Columbus, Dayton, Toledo, and Youngstown MSAs in Ohio in 2000; y_i measures gentrification; X_i are additional control variables; and γ_m are MSA-fixed effects.

For our measure of gentrification, because there is no official definition, we employ dif-

income is negative, as is the correlation between same-sex couples and house price. This signifies that same-sex couples are living in low income, low house price areas. As the correlation between same-sex couples and crime is positive, it appears that same-sex couples tend to live in poor, decaying, and high-crime areas. These correlation coefficients are available from the authors upon request.

[11] Same-sex couples are more likely to have both partners working than heterosexual couples. In addition, more same-sex couples than heterosexual couples have a bachelor's degree or higher. Therefore, same-sex couples, specifically gay male couples, tend to have a higher household income than heterosexual couples (Black et al., 2007b).

ferent measures used in the literature (Kolko, 2007; Florida and Mellander, 2010; Guerrieri et al., 2013; Christafore and Leguizamon, 2017, 2019; Meltzer and Ghorbani, 2017). Initially, we use the percent change in median household income between 2000 and 2014 for tracts in the bottom income quartile.[12] We also use a dummy variable for if the tract gentrified, where we define a tract as gentrified if it starts in the bottom income quartile in 2000 and moves to the top two quartiles in 2014. We focus on tracts in the bottom quartile because growth in income for the tracts at the top of the distribution is not considered gentrification. We also run these regressions with the additional requirement that the tract is in an urban area, as gentrification typically applies to urban neighborhoods. When we use the percentage change as the outcome variable, we use an OLS model, and when we use the probability a tract gentrified, we use a linear probability model (LPM). We also consider the same measures using median house prices instead of income as our measure of gentrification.[13]

Our measure of same-sex couples is the change in the number of same-sex unmarried partner households per 1,000 households between 2000 and 2010. However, we also consider other measures of the change in same-sex couples to test the robustness of our results. As an additional measure, we use the change in the Gay Index between 2000 and 2010. The Gay Index, developed by Florida and Gates (2015),[14] is a measure of the over- or under-representation of same-sex couples in the tract and is equal to:

$$\frac{\frac{\textit{same-sex unmarried partner households in tract}}{\textit{total unmarried partner households in Ohio}}}{\frac{\textit{population in tract}}{\textit{total population in Ohio}}} \tag{1.2}$$

When the change in the Gay Index is positive, there is an increase in the concentration of

[12]Following Christafore and Leguizamon (2017) and McKinnish et al. (2010), income and house price quartiles are calculated by MSA.

[13]We also looked at the percent change in income for all tracts, the percent change in income for tracts in the bottom two income quartiles, the probability of moving out of the bottom income quartile, and the probability of moving out of the bottom two income quartiles. These measures are repeated using house price. In addition, we explore the percent change in the percent with a bachelor's degree or higher, the percent change for tracts in the bottom education quartile, and the percent change for tracts in the bottom two education quartiles. Our results are consistent when we use these other measures. These additional results are available from the authors upon request.

[14]The Gay Index has also been used in Walther and Poston Jr. (2004), Florida and Mellander (2010), Madden and Ruther (2015), and Poston and Chang (2016).

same-sex couples that are living in the tract, which implies that gays are clustering more in that tract. We also explore results using the change in the number of gay male unmarried partner households per 1,000 households between 2000 and 2010 and the change in the number of lesbian unmarried partner households per 1,000 households between 2000 and 2010.[15]

We include a variety of control variables in X_i to reduce the possibility that characteristics associated with same-sex couples are drivers of gentrification. As described above, it could be that same-sex couples are correlated with gentrification if they select into areas that have higher-quality amenities, or it could be that same-sex couples drive gentrification if they form communities in less desirable areas that they are forced into due to discrimination. To address these concerns, we include a variety of controls including measures of school quality, crime rate, air pollution, temperature, precipitation, median house value, distance to the CBD, family size, the amount of opposite-sex unmarried couples, the amount of married couples, median age of people in the tract, the amount of children living in the tract, education level, median income, measures that capture diversity,[16] and the amount of Bohemians[17] living in the tract. In addition, we include the number of same-sex couples in 2000 per 1,000 households to control for the initial level of same-sex couples in the tract. We also include several additional variables that can affect gentrification, such as being within a mile of a tract that is in the top income quartile, the percentage of houses that are more than 20 years old, the vacancy rate, the percent of houses that are owner-occupied, the percent of the population in the labor force, and the population density.

[15]Gay men and lesbians have been found to behave differently, likely because lesbian couples are more likely to have children than gay male couples. We expand on this discussion in detail later.

[16]To measure diversity, we include the percent of the population that is only white, only black, and Hispanic and a measure of ethnic heterogeneity from Brasington (2007). The measure of ethnic heterogeneity is based on the Leik (1966) index of racial heterogeneity in Census block groups. A value of zero represents that the tract is racially homogeneous and a value of one is racially heterogeneous.

[17]The Bohemian Index used is an over- or under-representation of the number of Bohemians in the tract. Bohemians are considered to be artists, designers, actors, producers, directors, dancers, choreographers, musicians, singers, writers, authors, and photographers. This index is similar to the Gay Index defined above and is calculated as the fraction of Bohemians who live in the tract divided by the fraction of the total population that live in the tract.

However, despite including all of these control variables, there may still be something we have not been able to properly control for in an OLS specification. If unobservable amenities have not been capitalized into house prices, then failing to control for amenities could cause OLS to be biased. For example, same-sex couples are less likely to have children than heterosexual couples (Black et al., 2002; Christafore and Leguizamon, 2017). As a result, these households will demand more amenities that singles enjoy, like restaurants and stores, and less amenities that families value, like quality schools and other goods and services associated with children. Therefore, the direction of the bias is ambiguous, as some of the local amenities are positively correlated with same-sex couples, like adult bars and nice restaurants, while others are negatively correlated with same-sex couples, like parks for children. If these amenities are positively correlated with gentrification, then the amenities that same-sex couples are positively correlated with will cause OLS to have a positive bias and the amenities that same-sex couples are negatively correlated with will cause OLS to have a negative bias. Hence, the sign of the bias is ambiguous a priori.

To address the concerns regarding endogeneity and to isolate a mechanism driving the relationship between gays and gentrification, we adopt an instrumental variables approach. As our instrumental variable, we use Ohio's state-level version of the Defense of Marriage Act (DOMA), a federal law passed on September 21, 1996, which defined marriage solely as the union between a man and a woman and allowed states to pass a law refusing to recognize same-sex marriages. Although DOMA was ruled unconstitutional in 2013, following its passage in 1996, many states passed laws that banned same-sex marriages or clarified the language used in DOMA (McVeigh and Diaz, 2009).[18] In 2004, Ohio voted on State Issue 1 which would "preserve in Ohio law the universal, historical institution of marriage as the union of one man and one woman, and to protect marriage against those who would alter

[18]The following thirty states passed laws banning same-sex marriages between 1998 and 2008: Alabama, Alaska, Arizona, Arkansas, California, Colorado, Florida, Georgia, Hawaii, Idaho, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Missouri, Montana, Nebraska, Nevada, North Dakota, Ohio, Oklahoma, Oregon, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, and Wisconsin. Arizona failed to pass legislation that banned same-sex marriage in 2006 but succeeded in 2008. Therefore, all of these states ultimately succeeded at passing laws banning same-sex marriage (McVeigh and Diaz, 2009).

and undermine it" (Ohio Secretary of State[19]). Therefore, as an instrumental variable, we use the percent of the census tract that voted no on 2004's State Issue 1, which we will refer to as DOMA from this point forward for simplicity. The voting outcome for each precinct serves as a change that either attracts or repels same-sex couples from the area. Precincts with a high percent that voted no to defining marriage as only between a man and a woman are areas that can be perceived as being more gay friendly, which could attract same-sex couples to these precincts so that the change in same-sex couples would be positive. The first stage regression is as follows:

$$\Delta\, \textit{same-sex couples}_i = \delta_0 + \delta_1(\textit{vote no}_i) + \delta_2 X_i + \gamma_m + u_i \tag{1.3}$$

where all of the variables are defined as before and $\textit{vote no}_i$ is the percent of tract i that voted no on DOMA in 2004.

For voting on a gay-related law to be an appropriate instrumental variable for the change in same-sex couples in the area, the voting outcome has to satisfy two conditions. First, the precinct voting outcome must be correlated with the amount of same-sex couples in the area. It has been found that gay men and lesbians are more likely to live in or be "out" on social media in areas that have more support for pro-gay laws (Klawitter and Flatt, 1998; Stephens-Davidowitz, 2013). Therefore, this condition is likely satisfied and our first stage results support this argument.

The second condition is that the voting outcome is not correlated with the error term. The outcome variables we use are either the change in income or house price. We do not believe that the voting outcome on a gay-marriage law will affect these outcome variables except through the number of same-sex couples given the controls that we have included in our model to control for how tolerant an area is likely to be. Specifically, we include controls for Bohemians (Florida and Mellander, 2010) and education and diversity (Jenkins

[19]See https://www.sos.state.oh.us/elections/election-results-and-data/2004-elections-results/state-issue-1-november-2-2004/state-issue-1-argument-in-support-of/.

and Baker, 2009; Qian, 2013; Clark, 2015) as these have been found to be correlated with tolerance. If these variables sufficiently control for tolerance, the exclusion restriction will be satisfied and our estimates will be unbiased.

All of our control variables take on their 2000 value and are used to control for common attributes that could affect gentrification. Since we measure gentrification occurring over fourteen years from 2000 to 2014, the control variables are included to see how they affect gentrification fourteen years later. The voting on DOMA, which is used as an IV, occurs in 2004. The variable of interest, the change in same-sex couples, is measured between 2000 and 2010. Therefore, we measure how many additional same-sex couples are in a tract six years after voting has occurred. Then, we investigate how these additional same-sex couples in 2010 affect gentrification between 2000 and 2014.

1.4 Data

Data for 2000 and 2014 median income and median house price come from the 2000 Decennial Census[20] and the 2012-2016 five-year American Community Survey,[21] respectively. Measuring the number of same-sex couples is problematic given the nature of the questions in the Decennial Census. Starting in the 1990 Census, the "Relationship to Householder" question had "unmarried partner" as a response option, allowing people to identify themselves as living as an unmarried cohabitating couple. Therefore, cohabitating, unmarried same-sex couples are identified when Person 1 is the same sex as Person 2 and chooses that Person 2 is their "unmarried partner." This change in the form allowed the Census to gather data on gay males and lesbians that lived with their partner. Therefore, all measures of same-sex unmarried partnered households come from the 2000 and 2010 Census, but additional information is needed to correct these counts due to opposite-sex couples miscoding their gender

[20]This data is in 2000 nominal dollars and is converted to 2012 real dollars.

[21]The 2016 data on income and house price is broken down by 2010 tracts. This is converted to 2000 tracts by using a conversion file from the Census. The 2010 data is in 2016 nominal dollars and is converted to 2012 real dollars.

and being recorded as same-sex couples.[22] This additional information comes from the 2000 5% Integrated Public Use Microdata Series, the 2010 state-level count of the sex-corrected number of same-sex unmarried partnered households, and the 2010 Census mail-in participation rates. See Black et al. (2007a) and Gates and Cooke (2012) for a discussion on how to deal with this miscoding in 2000 and 2010, respectively.

For our instrumental variable, we use the percent that voted no on DOMA in Ohio which stated that marriage could only be between a man and a woman. The 2004 voting results on this issue are available from the Ohio Secretary of State at the precinct level.

The measure of ethnic heterogeneity, the percent of 12th grade students who passed all five sections of the Ohio proficiency test in 2000-2001, total expenditure per pupil for the 2000-2001 school year, the crime rate, and the total air pollution come from Brasington (2007). The average maximum temperature and total precipitation are from the U.S. Department of Agriculture. The number of Bohemians is from the 2000 5% Integrated Public Use Microdata Series. A dummy variable equal to 1 if the census tract is within one mile of another tract is created using median income from the 2000 Decennial Census and distances available from the National Bureau of Economic Research. All remaining variables come from the 2000 Decennial Census. Table 1.1 presents summary statistics for all variables.

1.5 Parametric Results for Same-Sex Coupled Households

The results from the OLS regressions, where gentrification is defined as the percent change in income or house prices for tracts in the bottom quartile, are presented in Table 1.2 Panel A. The first two columns correspond to the percent change in income when focusing on tracts that are in the bottom income quartile, while the last two columns correspond to the

[22]Given that sexual preference is not asked by the Census, there is no way to determine how many single gay or non-cohabitating same-sex couples live in an area. For that reason, we are only able to consider same-sex cohabitating couples. See Klawitter and Flatt (1998), Carpenter (2008), Carpenter and Gates (2008) for a discussion of the implications of this data limitation. There are some miscoding issues with this data from the Census, as there is a concern that opposite-sex couples miscode one of their sexes so that the Census counts them as a same-sex couple. We follow Black et al. (2007a), Christafore and Leguizamon (2012), Gates and Cooke (2012), and Christafore and Leguizamon (2013) to address these issues.

percent change in house price when focusing on tracts that are in the bottom house price quartile. In columns (1) and (3), all tracts in the bottom income and house price quartile, respectively, are used. In columns (2) and (4), the sample is restricted from the previous column to only include tracts that are 100% urban.

The rows represent the different independent variables used to measure the change in the number of same-sex couples. The first row uses the change in the number of same-sex unmarried partner households per 1,000 households between 2000 and 2010. The second row uses the change in the Gay Index. The third and fourth rows use the change in gay male unmarried partner households and the change in lesbian unmarried partner households per 1,000 households between 2000 and 2010, respectively.

Table 1.2 Panel B follows the same format as Table 1.2 Panel A but uses a LPM to measure the probability that a tract gentrifies as the outcome variable. The first two columns consider a tract as gentrified if it goes from the bottom quartile of income in 2000 to the top two quartiles of income in 2014. The last two columns use house price instead of income to measure the probability of going from the bottom house price quartile in 2000 to the top two house price quartiles in 2014. The rest of the rows and columns follow the same setup as Table 1.2 Panel A.

The results from Table 1.2 suggest that additional same-sex couples have a positive effect on gentrification, and this effect is statistically significant. Specifically looking at urban tracts that are in the bottom income quartile, when there is an additional same-sex couple per 1,000 households between 2000 and 2010, the percent change in income is expected to increase by 1.9 percentage points. This corresponds to an average increase in income of about $587.32.[23] For urban tracts in the bottom house price quartile, when there is an additional same-sex couple per 1,000 households, the percent change in house price is expected to increase by 2.0 percentage points, corresponding to an average increase in house price of about $1,494.17.[24]

[23]The average median income in urban tracts in the bottom income quartile is $30,637.26 and 1.917% of this is $587.32.

[24]The average median house price in urban tracts in the bottom house price quartile is $75,121.81 and 1.989% of this is $1,494.17.

Looking at urban tracts in the bottom income or house price quartile, when there is an additional same-sex couple per 1,000 households, the tract is 0.6 percentage points more likely to gentrify, regardless of whether gentrification is measured with income or house price.

We also see in Table 1.2 that areas with higher changes in the number of gay male couples are more likely to experience gentrification. As we can see in the final row of each panel in Table 1.2, the effect of lesbian couples is not always statistically significant. The literature has documented differences in behavior between lesbians and gay males, which may explain why lesbians do not affect gentrification as much as gay males. The previous literature suggests that lesbian couples tend to have similar household incomes as heterosexual couples and behave similarly to heterosexual couples, as they are more likely to have children (Black et al., 2002, 2007b). Lesbians are also less likely to cluster together (Fu, 2008), implying that the theory that clustering leads to more amenities and improvements in the housing stock might not apply to lesbian couples. Therefore, it is not surprising that gay male couples, who tend to behave differently than straight couples and tend to cluster together, might be driving the results.

As discussed above, there are reasons to believe that the OLS regressions may be biased since there may still be unobservable variables in the error term that affect our outcome of interest. To address the endogeneity concern, an instrumental variable approach is used. Specifically, the percentage of the tract that voted against DOMA is used as an instrument for the change in the number of same-sex couples. The results from the first stage regression are presented in Table 1.3, which shows the coefficient of the voting outcome on the change in same-sex couples. The layout of columns in Table 1.3 is identical to the columns in Table 1.2. However, the rows in Table 1.2 represent different independent variables of interest, while the rows in Table 1.3 represent different dependent variables used in different regressions. The first stage is the same regardless of which outcome gentrification variable is used since all regressions focus on tracts in the bottom income or house price quartile.

Voting on DOMA is positive and statistically significant in all the specifications. Except for the effect of voting on the change in lesbian couples in low-income tracts, the F-statistic is above the standard threshold of 10, suggesting that we have an appropriate IV. Areas that had a higher percent of the tract vote no on DOMA are areas that are more likely to be accepting of same-sex couples' lifestyles. As expected, these areas attract same-sex couples since the coefficient on the voting variable is positive and statistically significant.

Results using the instrumental variables approach are presented in Table 1.4. These tables are identical to Table 1.2, respectively, but use the percent of the tract that voted no on DOMA as an IV for the change in the number of same-sex couples. Table 1.4 Panel A uses the percent change as a measure of gentrification and Table 1.4 Panel B uses the probability that a tract gentrifies. In all specifications, a positive change in the number of same-sex couples has a positive and statistically significant effect on gentrification. Given that the first stage regressions for lesbian couples do not meet the popular F-statistic threshold, we do not put much weight on those results.

Specifically looking at urban tracts that are in the bottom income quartile, when there is an additional same-sex couple per 1,000 households between 2000 and 2010, the percent change in income is expected to increase by 6.5 percentage points. This corresponds to an average increase in income of about $1,997.86. For urban tracts in the bottom house price quartile, when there is an additional same-sex couple, the percent change in house price is expected to increase by 6.0 percentage points, corresponding to an average increase in house price of about $4,520.08. Looking at urban tracts in the bottom income or house price quartile, when there is an additional same-sex couple per 1,000 households, the tract is 2.6 or 2.8 percentage points more likely to gentrify when using income or house price to measure gentrification, respectively. Overall, the results from the two-stage least squares (2SLS) regressions support the argument that tracts with more same-sex couples are more likely to experience gentrification. The coefficients from the IV regressions are larger than the OLS regressions, suggesting that the downward bias is stronger in the OLS specification.

1.6 Parametric Results for Opposite-Sex Couples

One argument for why same-sex couples induce gentrification is that they are less likely to have children and therefore have more disposable income to invest in their home and spend outside the home on various amenities. However, if it is the lack of children that is responsible for same-sex couples driving gentrification, then the effect of same-sex unmarried households should be the same as the effect of opposite-sex couple households without children. To confirm that there is something different about same-sex couples and opposite-sex couples without children, we examine the effect of opposite-sex couples on gentrification.

First, instead of using the change in same-sex unmarried partner households per 1,000 households as our key independent variable, we use the change in opposite-sex unmarried partner households per 1,000 households, the change in opposite-sex married households with own children under the age of 18 per 1,000 households, and the change in opposite-sex married households without own children under the age of 18 per 1,000 households. The results without using the instrumental variable and the first stage regression results from using these can be seen in Table 1.5 and Table 1.6, respectively.

The layout of Table 1.5 is similar to Table 1.2 but uses the change in various types of opposite-sex households as the independent variable of interest. Table 1.5 shows that the magnitude of the effect of the change in opposite-sex couples (with or without children, married or unmarried) is significantly lower than the magnitude on the effect of the change in same-sex unmarried partner households. In addition, the effect of the change in opposite-sex couples is not always statistically significant. This result suggests that the effect of same-sex couples on gentrification is not driven by the lack of children and thus a higher disposable income. Therefore, we argue that same-sex couples drive gentrification through the formation of communities - as they feel comfortable where they live, these couples choose to invest in their neighborhood which drives gentrification. The formation of communities is further investigated in the next section through a semi-parametric model.

The first stage regression results are available in Table 1.6, which is set up similar to

Table 1.3 but the dependent variable in these first stage regressions is the various measures for the change in opposite-sex households. The percent of the tract voting against DOMA does not have a positive, statistically significant effect on various changes in opposite-sex couples (with or without children, married or unmarried). In addition, the F-statistic is not above the threshold of 10. This result provides additional support for our instrument, as these regressions suggest that there is something about the voting outcome that is affecting same-sex couples differently than opposite-sex couples.[25]

1.7 Semi-Parametric Results

In addition to the parametric regressions discussed above, we also use semi-parametric methods. If gentrification is occurring due to same-sex couples clustering, then there might be a tipping point in the number of same-sex couples after which gentrification occurs. A semi-parametric model allows us to see if this type of relationship exists. Our semi-parametric model is as follows:

$$\Delta\, \textit{same-sex couples}_i = m(\textit{vote no}_i) + \delta_2 X_i + \gamma_m + u_i \tag{1.4}$$

$$y_i = g(\Delta\, \textit{same-sex couples}_i) + \beta_2 X_i + \gamma_m + \varphi(\hat{u}_i) + \varepsilon_i \tag{1.5}$$

where all of the variables are defined as before and $g(\cdot)$ and $m(\cdot)$ are unknown smooth functions. Here, the percent that voted no on DOMA enters non-parametrically in the first stage while all other variables enter in parametrically. The change in same-sex couples enters non-parametrically in the second stage while all other variables enter in parametrically. The method employed by Ozabaci et al. (2014), which uses a combination of B-splines and kernel smoothing, is used here to estimate both the parametric parameter vectors and the unknown functions.[26]

[25]Since the first stage results suggest we have a weak instrument, the second stage results are not shown. These results are available from the authors upon request.

[26]The method used here is detailed in Ozabaci et al. (2014) and Chu et al. (2017).

The marginal effect from the semi-parametric regressions can be seen in Figure 1.2, which displays the semi-parametric gradients in the solid line and the parametric coefficient in the dotted line. The corresponding 95% confidence intervals are also displayed. When measuring gentrification by the percent change in income or house price, the semi-parametric gradients are within the 95% confidence intervals of the parametric coefficient. This indicates that the results from the semi-parametric model are statistically similar to the results of the parametric model, implying that the parametric model is correctly specified.

The second row in Figure 1.2 shows the marginal effect when using the probability that a tract gentrifies. Defining gentrification by income or house price generates similar results: when the change in same-sex couples per 1,000 households between 2000 and 2010 is under 15, the semi-parametric model shows that additional same-sex couples have very little effect on the probability of gentrification. However, when the change in same-sex couples per 1,000 households is over 15, and especially over 20, the semi-parametric marginal effect significantly diverges from the parametric results and each additional same-sex couple per 1,000 households has a larger marginal effect. When the change in same-sex couples is high, the semi-parametric results estimate a higher marginal effect than the marginal effect estimated from the parametric results.

These semi-parametric results suggest that as the change in the number of same-sex couples per 1,000 households gets sufficiently larger, the chances of gentrifying increases drastically. It appears that there is a tipping point in the number of gay couples that are needed to induce gentrification. In the model developed by Collins (2004), decaying areas become gentrified as the number of gay men and lesbians increase and they demand more amenities and services in the area. Therefore, the semi-parametric results show that it could be the clustering of same-sex couples that induces gentrification. However, prior to this threshold, the amount of additional same-sex couples may not be sufficient to drive neighborhood change.

1.8 Conclusion

Gentrification is becoming an increasingly important issue for policy makers and researchers. We examine one of the possible causes of gentrification: the influx of same-sex couples into an area. However, there are likely to be endogeneity issues present in an OLS regression due to omitted variable bias. To address these concerns, we focus on Ohio, where there was a statewide vote on a law similar to the federal Defense of Marriage Act (DOMA), and use an instrumental variables approach. Our first stage results support using the percentage of a tract that voted against this law as an instrument for the change in the number of same-sex couples. The results from our instrumental variables regression suggests that more same-sex couples induce gentrification.

In addition, we consider the non-linear effects of a change in the number of same-sex couples. Specifically, we use a semi-parametric model to look at the possible non-linear relationship between the number of same-sex couples and the likelihood of gentrification. The semi-parametric results suggest that there is a tipping point in the number of same-sex couples required for an area to gentrify. Once same-sex couples are sufficiently clustered, they begin to demand amenities and invest in the housing, which may be what ultimately drives gentrification (Collins, 2004). However, given we only have one state and one law change, we are limited in terms of a sample size to examine this effect. More data from other areas will help shed additional light on how many gay couples need to cluster in an area to induce gentrification.

The results presented in this paper explore the effect of cohabitating same-sex couples due to data limitations. Couples that do not live together are not included in this analysis, nor are single gay men and lesbians. Therefore, our results are specific to same-sex couples that live together and may not be generalizable to the entire LGBTQ+ population. Future research should work to find appropriate data and measures of single gay men and lesbians and non-cohabiting same-sex couples to analyze their effect on gentrification.

These results have important implications for policy makers, as they have important insights to neighborhood change. Moss (1997) emphasizes the need for public officials to incorporate pro-gay actions in the steps towards urban redevelopment. Creating a welcoming and more tolerant environment for individuals with this type of lifestyle increases neighborhood incomes and house prices. Therefore, local policy makers who are looking to encourage neighborhood redevelopment should pursue policies that remove discriminatory practices against same-sex couples.

Table 1.1: Summary Statistics for Tracts in the Bottom Income or House Price Quartile

Variable	N	Mean	St. Deviation	Min	Max
% Δ in median income	632	-21.82	31.95	-81.40	287.86
% Δ in median house price	632	-16.87	42.40	-98.55	397.70
P(gentrify – income, Q1 to Q3-Q4)	632	0.01	0.12	0	1
P(gentrify – house price, Q1 to Q3-Q4)	632	0.03	017	0	1
change in same-sex couples	632	3.10	4.48	-10.58	33.30
change in Gay Index	632	0.16	1.22	-4.26	9.03
change in gay male couples	632	1.23	3.03	-6.17	23.14
change in lesbian couples	632	1.87	2.63	-7.00	14.36
same-sex couples	632	3.46	3.92	0	36.76
% with bachelor's degree or higher	632	11.19	10.97	0	78.96
% white	632	48.29	33.72	0.26	99.37
% black	632	45.40	35.54	0.11	98.72
% Hispanic	632	4.36	7.77	0	57.42
ethnic heterogeneity	632	0.13	0.04	0.02	0.34
Bohemian Index	632	0.93	0.56	0.18	2.35
% passed all 5 exams	632	42.40	5.85	33.70	59.82
total school expenditure	632	8461	794	6030	9472
crime rate	632	74.59	17.40	9.43	103.49
total air pollution	632	3241	4517	0	39501
precipitation in Dec, Jan, Feb	632	196.62	15.89	158.86	231.65
precipitation in June, July, Aug	632	295.49	14.89	258.18	324.97
temperature in Dec, Jan, Feb	632	3.01	1.11	1.38	5.00
temperature in June, July, Aug	632	27.88	0.99	26.65	29.31
median house value in 2000	632	87543	43090	18266	599984
distance to CBD	632	5.97	6.58	0	36.99
average family size	632	3.12	0.26	2.19	4.26
median income	632	34041	10145	9126	60877
opposite-sex unmarried partners	632	66.02	21.67	1.99	147.32
median age	632	32.52	5.97	15.60	68.60
number of children in households	632	770.75	280.18	21.93	1994.44
married households	632	280.59	112.57	21.63	854.44
one mile from top quartile	632	0.09	0.28	0	1
% house age over 20	632	92.17	11.10	14.55	100
% vacant	632	12.33	6.77	0	44.28
% owner-occupied	632	45.66	19.75	0.81	92.95
% labor force	632	57.94	9.90	10.20	84.30
population density	632	5884	3855	99	21323
Akron	632	0.08	0.27	0	1
Cincinnati	632	0.18	0.38	0	1
Cleveland	632	0.31	0.46	0	1
Columbus	632	0.18	0.39	0	1
Dayton	632	0.11	0.32	0	1
Toledo	632	0.09	0.28	0	1
Youngstown	632	0.06	0.23	0	1
% vote no on DOMA	632	45.89	8.76	7.83	86.21

Table 1.2: OLS and LPM Coefficients Estimating the Impact of Same-Sex Couples on Gentrification

Panel A: Percentage Change				
	Income		House Price	
	(1) Tracts in Q1 (N=507)	(2) Urban Tracts in Q1 (N=492)	(3) Tracts in Q1 (N=509)	(4) Urban Tracts in Q1 (N=488)
Change in Same-Sex Couples	1.906*** (0.308)	1.917*** (0.315)	1.989*** (0.418)	1.989*** (0.419)
Change in Gay Index	7.122*** (1.080)	7.171*** (1.103)	8.913*** (1.540)	9.184*** (1.543)
Change in Gay Male Couples	2.895*** (0.437)	2.923*** (0.446)	2.916*** (0.636)	2.937*** (0.635)
Change in Lesbian Couples	1.473*** (0.552)	1.430** (0.565)	1.994*** (0.704)	1.929*** (0.704)
Panel B: Probability of Gentrification				
	Income		House Price	
	(1) Tracts in Q1 (N=507)	(2) Urban Tracts in Q1 (N=492)	(3) Tracts in Q1 (N=509)	(4) Urban Tracts in Q1 (N=488)
Change in Same-Sex Couples	0.006*** (0.001)	0.006*** (0.001)	0.005*** (0.002)	0.006*** (0.002)
Change in Gay Index	0.026*** (0.004)	0.027*** (0.005)	0.025*** (0.007)	0.027*** (0.006)
Change in Gay Male Couples	0.006*** (0.002)	0.006*** (0.002)	0.010*** (0.003)	0.010*** (0.003)
Change in Lesbian Couples	0.007*** (0.002)	0.007*** (0.002)	0.003 (0.003)	0.003 (0.003)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.3: First Stage Regression Coefficients Estimating the Impact of the Percent that Voted No on DOMA on the Change in Same-Sex Couples

	Income		House Price	
	(1) Tracts in Q1 (N=507)	(2) Urban Tracts in Q1 (N=492)	(3) Tracts in Q1 (N=509)	(4) Urban Tracts in Q1 (N=488)
Change in Same-Sex Couples	0.203*** (0.045) 20.80	0.198*** (0.046) 18.68	0.273*** (0.049) 30.70	0.265*** (0.051) 27.12
Change in Gay Index	0.060*** (0.013) 22.63	0.059*** (0.013) 20.51	0.068*** (0.013) 26.04	0.066*** (0.014) 23.05
Change in Gay Male Couples	0.139*** (0.031) 19.70	0.135*** (0.032) 17.43	0.166*** (0.033) 25.90	0.161*** (0.034) 22.78
Change in Lesbian Couples	0.065** (0.026) 6.163	0.063** (0.027) 5.627	0.107*** (0.030) 12.53	0.104*** (0.031) 10.98

Notes: Standard errors in parentheses and F-statistics below. *** p<0.01, ** p<0.05, * p<0.1

Table 1.4: 2SLS Coefficients Estimating the Impact of Same-Sex Couples on Gentrification

	Panel A: Percentage Change			
	Income		House Price	
	(1) Tracts in Q1 (N=507)	(2) Urban Tracts in Q1 (N=492)	(3) Tracts in Q1 (N=509)	(4) Urban Tracts in Q1 (N=488)
Change in Same-Sex Couples	5.946*** (1.747)	6.521*** (1.923)	6.024*** (1.852)	6.017*** (1.932)
Change in Gay Index	20.152*** (5.767)	21.947*** (6.271)	24.233*** (7.405)	24.252*** (7.703)
Change in Gay Male Couples	8.715*** (2.557)	9.593*** (2.834)	9.914*** (3.126)	9.891*** (3.258)
Change in Lesbian Couples	18.714** (8.511)	20.362** (9.521)	15.355*** (5.813)	15.366** (6.141)
	Panel B: Probability of Gentrification			
	Income		House Price	
	(1) Tracts in Q1 (N=507)	(2) Urban Tracts in Q1 (N=492)	(3) Tracts in Q1 (N=509)	(4) Urban Tracts in Q1 (N=488)
Change in Same-Sex Couples	0.025*** (0.008)	0.026*** (0.008)	0.029*** (0.009)	0.028*** (0.008)
Change in Gay Index	0.084*** (0.024)	0.087*** (0.026)	0.115*** (0.035)	0.111*** (0.034)
Change in Gay Male Couples	0.036*** (0.012)	0.038*** (0.013)	0.047*** (0.014)	0.045*** (0.014)
Change in Lesbian Couples	0.078** (0.035)	0.081** (0.038)	0.073*** (0.027)	0.071** (0.028)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: OLS and LPM Coefficients Estimating the Impact of Couples on Gentrification

	Panel A: Percentage Change			
	Income		House Price	
	(1) Tracts in Q1 (N=507)	(2) Urban Tracts in Q1 (N=492)	(3) Tracts in Q1 (N=509)	(4) Urban Tracts in Q1 (N=488)
Change in Same-Sex	1.906***	1.917***	1.989***	1.989***
Unmarried Partner Couples	(0.308)	(0.315)	(0.418)	(0.419)
Change in Opposite-Sex	0.221**	0.219**	-0.011	-0.076
Unmarried Partner Couples	(0.090)	(0.093)	(0.123)	(0.124)
Change in Opposite-Sex Married	0.252***	0.255***	0.378***	0.370***
Couples with Children	(0.068)	(0.070)	(0.080)	(0.083)
Change in Opposite-Sex Married	0.447***	0.458***	0.287***	0.273***
Couples without Children	(0.052)	(0.054)	(0.078)	(0.081)
	Panel B: Probability of Gentrification			
	Income		House Price	
	(1) Tracts in Q1 (N=507)	(2) Urban Tracts in Q1 (N=492)	(3) Tracts in Q1 (N=509)	(4) Urban Tracts in Q1 (N=488)
Change in Same-Sex	0.006***	0.006***	0.005***	0.006***
Unmarried Partner Couples	(0.001)	(0.001)	(0.002)	(0.002)
Change in Opposite-Sex	0.001*	0.001*	0.000	-0.000
Unmarried Partner Couples	(0.000)	(0.000)	(0.001)	(0.001)
Change in Opposite-Sex Married	-0.000	-0.000	0.000	-0.000
Couples with Children	(0.000)	(0.000)	(0.000)	(0.000)
Change in Opposite-Sex Married	0.000**	0.001**	0.000	0.000
Couples without Children	(0.000)	(0.000)	(0.000)	(0.000)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

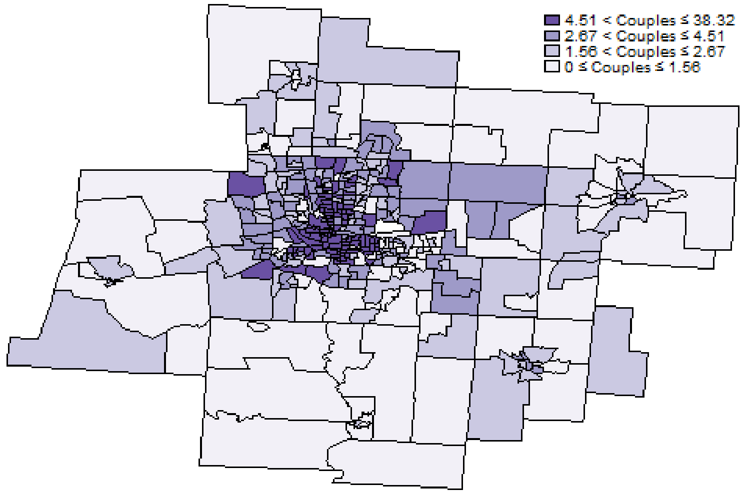
Table 1.6: First Stage Regression Coefficients Estimating the Impact of the Percent that Voted No on DOMA on the Change in Couples

	Income		House Price	
	(1) Tracts in Q1 (N=507)	(2) Urban Tracts in Q1 (N=492)	(3) Tracts in Q1 (N=509)	(4) Urban Tracts in Q1 (N=488)
Change in Same-Sex	0.203***	0.198***	0.273***	0.265***
Unmarried Partner	(0.045)	(0.046)	(0.049)	(0.051)
Couples	20.80	18.68	30.70	27.12
Change in Opposite-Sex	0.055	0.064	0.176	0.163
Unmarried Partner	(0.161)	(0.164)	(0.177)	(0.182)
Couples	0.117	0.152	0.992	0.802
Change in Opposite-Sex	-0.583***	-0.530**	0.304	0.369
Married Couples with	(0.211)	(0.215)	(0.265)	(0.266)
Children	7.615	6.092	1.313	1.918
Change in Opposite-Sex	-0.432*	-0.321	-0.240	-0.154
Married Couples without	(0.258)	(0.262)	(0.277)	(0.274)
Children	2.801	1.507	0.753	0.316

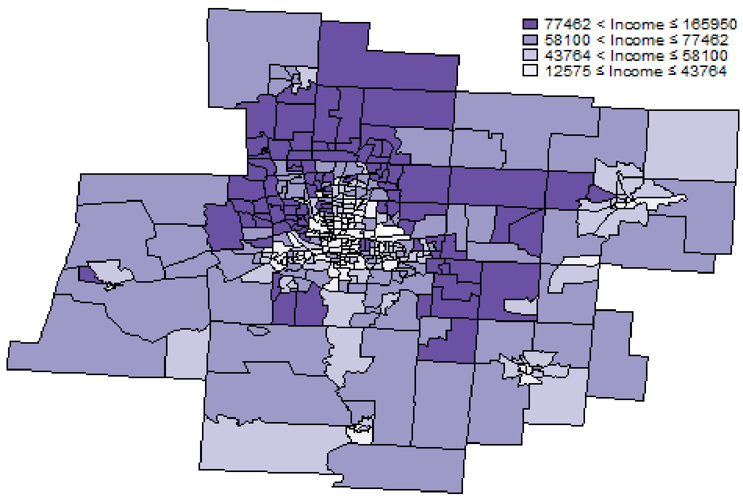
Notes: Standard errors in parentheses and F-statistics below. *** p<0.01, ** p<0.05, * p<0.1

Figure 1.1: Maps of Columbus MSA in 2000

Panel A: Same-Sex Couples in 2000 in Columbus MSA



Panel B: Income in 2000 in Columbus MSA



Panel C: House Price in 2000 in Columbus MSA

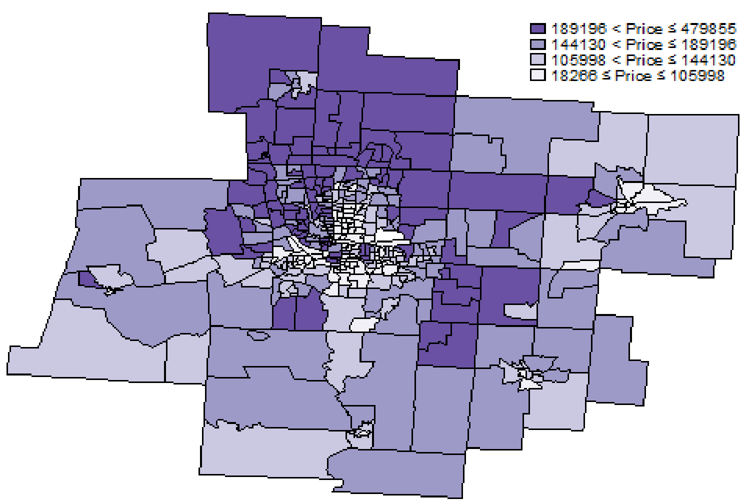
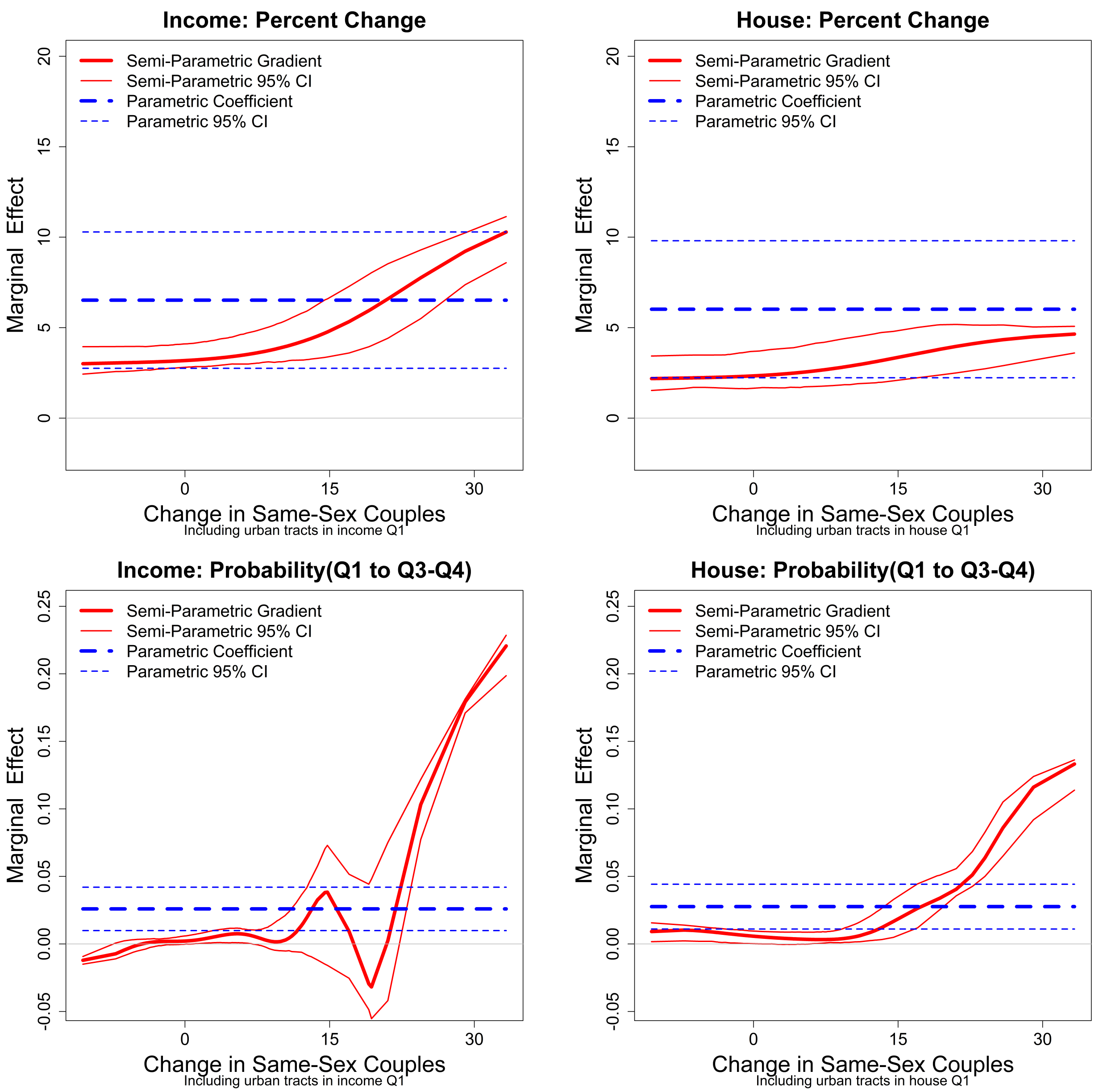


Figure 1.2: Gradients and Marginal Effect from the Semi-Parametric and Parametric Models Estimating the Effect of the Change in Same-Sex Couples on Gentrification



CHAPTER 2

IDENTIFICATION AND CONCENTRATION OF SAME-SEX HOMEBUYERS: USING HOUSE SALE DATA (WITH AMIR B. FERREIRA NETO AND ADAM NOWAK)

2.1 Introduction

Proper identification of the LGBTQ+ community can be extremely useful for current and future research. Reports associating gay community to economic growth (Florida and Gates, 2001; Florida, 2002; Berggren and Elinder, 2012; Alm et al., 2014; Christafore and Leguizamon, 2017) have made researchers and policy makers pay more attention to this community and their locational behavior. Literature on if gays are displaced in gentrifying neighborhoods (Doan and Higgins, 2011), if gays are discriminated in the housing market (Leppel, 2007b; Christafore and Leguizamon, 2012), and where gays are located (Black et al., 2002; Collins, 2004; Spring, 2013) need to know specifically where members of the LGBTQ+ community are living in order to make strong claims about these phenomena, but often lack the necessary data.

This paper proposes a new method to identify members of the LGBTQ+ community by identifying same-sex couple homebuyers at an individual level.[1] We draw upon a novel data set, using individual real estate transaction records from three counties across the United States – Miami-Dade County, Florida; Franklin County, Ohio; and King County, Washington. These detailed transaction records for individual properties provide geolocation

[1]This paper identifies same-sex couple homebuyers as a proxy for identification of all members of the LGBTQ+ community. Most literature identifies same-sex couple households using data from the Decennial Census. Both our method and the Census method are unable to identify all members of the LGBTQ+ community.

information as well as the name of buyers. Following a similar approach to the one described in Nowak and Sayago-Gomez (2018), we determine the sex of homebuyers using their names which allows for the identification of male same-sex couple homebuyers and female same-sex couple homebuyers. The method proposed in this paper will overcome some of the problems associated with using the Decennial Census to identify measures of the LGBTQ+ community.

In this paper, we identify same-sex couples that have purchased a home together, which we refer to as same-sex couple homebuyers. Although this paper identifies a subset of all members of the LGBTQ+ community, we aim to show that this method is accurately identifying members of the LGBTQ+ community who are coupled homebuyers. In recent literature, the Decennial Census is commonly used to identify members of the LGBTQ+ community by identifying same-sex unmarried partner households, which we refer to as same-sex couple households. Researchers who use the Census identification method typically use the percent of households that are same-sex couple households. Since same-sex couple homebuyers will be a subset of same-sex couple households and same-sex couple households will be a subset of all members of the LGBTQ+ community, this paper suggests using the percent of homebuyers that are same-sex couple homebuyers in each census tract.[2] However, first we will discuss the method to identify the number of same-sex couple homebuyers and later we will discuss how to identify the percent of homebuyers that are same-sex couple homebuyers.

The identification of same-sex couple homebuyers is accomplished by determining the sex of homebuyers based on their name for houses purchased by two homebuyers. We separately identify male same-sex couple homebuyers and female same-sex couple homebuyers in order to find the total number same-sex couple homebuyers. To ensure that the house purchases identified here are purchased by members of the LGBTQ+ community, we have four different counts of same-sex couple homebuyers. The first count of same-sex couple homebuyers counts

[2]The percent is explored to determine if the method proposed in this paper is properly identifying same-sex couple homebuyers. The number is aggregated at the Census tract level in this paper, although any geographic area can be used to suit future research ideas. In addition, depending on the future research question, the number and location of same-sex couple homebuyers may be appropriate.

all transactions purchased by two homebuyers whose first name belong to the same sex. The second count removes houses purchased by two homebuyers with the same last name to remove the possibility that houses are purchased by a father and son. The third count takes the first count and removes houses that are flipped to remove houses that are purchased by two people of the same sex with the sole purpose of renovating and selling. The fourth count combines the second and third requirements. Based on summary statistics and a comparison of distribution to the Census, the first count seems to be appropriate.

This paper proposes three improvements over the Census method of identifying same-sex couple households, which is typically used to proxy for members of the LGBTQ+ community. First, we are able to identify same-sex couple homebuyers on a more granular level, specifically we identify these homebuyers at the property level. The smallest unit of geography that the Census releases information on same-sex couple households is the Census tract, which can be very large geographically, and makes it difficult to do an analysis on a neighborhood level.[3] Secondly, our method of identification does not rely on self-identification since we use the homebuyers' name. The Census relies on individuals recording their sex correctly and concerns that mistakes occur when filling out the Census form require additional calculations to be made to the count of same-sex couple households from the Census.[4] Thirdly, our method can identify the number of same-sex couple homebuyers in any particular month of any year. Using Census data to identify same-sex couple households is limited to data in 1990, 2000, and 2010, which makes it difficult to know how many same-sex households there are in non-Census years.

A formal review on the literature on gay men and lesbians and where they live can be found in Badgett (2001), Black et al. (2002), Gates and Ost (2004), Anacker and Morrow-Jones (2005), Black et al. (2007b), Florida and Mellander (2010), Spring (2013), Ghaziani (2014), Ghaziani (2015), Whittemore and Smart (2016), Christafore and Leguizamon (2017),

[3]A Census tract has about 4,000 inhabitants on average, with a minimum of 1,200 inhabitants and a maximum of 8,000.

[4]The steps necessary to deal with miscoding are discussed in Section 2.2.

and Morales (2018). As for the strand focusing on housing and real estate, regarding home-ownership of same-sex households, Leppel (2007b) and Jepsen and Jepsen (2009) find that same-sex couple households are more likely to own rather than rent compared to opposite-sex unmarried partner households but less likely to own than opposite-sex married partner households.[5] Jepsen and Jepsen (2009) also find that the average value of houses owned by male same-sex couple households are similar to the value of homes owned by opposite-sex married unmarried partner, but is higher than that owned by female same-sex couple households. In addition, Leppel (2007a) finds that there is no statistical difference in location of ownership for same-sex couple households, while opposite-sex couples avoid the city center.

The rest of the paper will proceed as follows. The next section details previous methods used to find same-sex households and our improvement over these methods while Section 2.3 proposes our new method to count same-sex couples through the use of homebuyer information. Section 2.4 discusses how the method proposed in this paper is accurately identifying same-sex couple homebuyers. Section 2.5 concludes and discusses future research.

2.2 Previous Identification Method of Same-Sex Households

The main issue on the study of the LGBTQ+ community lies on the identification of such individuals. No dataset explicitly reports the sexual orientation, with exception of specific surveys, such as the General Social Survey (GSS) (Black et al., 2003; Blandford, 2003); the National Health and Social Life Survey (NHSLS) (Black et al., 2000); the Behavioral Risk Factor Surveillance System (BRFSS) (Carpenter, 2004); Third National Health and Nutrition Examination Surveys (NHANES III) (Carpenter, 2007); California's 2003 Lesbian, Gay, Bisexual, and Transgender Tobacco Survey (Carpenter and Gates, 2008); and California's Health Interview Survey (CHIS) (Carpenter and Gates, 2008). These surveys are specific to geographic locations at one specific time and may not be generalizable to other geographic

[5] Jepsen and Jepsen (2009) and Leppel (2007b) use data from the 2000 Decennial Census to analyze housing choices of same-sex couple households, but the data they use is not publically available from the Census.

locations or other time periods. Therefore, these surveys cannot be used to identify members of the LGBTQ+ community in different years and different locations than where the survey originally took place. In addition, surveys like the CHIS rely on individual's interpretation of what it means to be gay, lesbian, and bisexual (Carpenter and Gates, 2008) which could result in inaccurate data collected.

Whittemore and Smart (2016) and Smart and Whittemore (2017) study the spatial distribution of same-sex couple households, but use a novel approach. The authors use the advertisement for rent and sale of property in the gay-oriented newspaper Dallas Voice. This approach allows the authors to have a more granular level data, but is still bounded by a pre-determined geographic area over a specific time interval. In addition, since the authors only observe the advertisement for the property, they are unable to distinguish who is purchasing and actually moving into these properties.

In the United States,[6] most researchers rely on the Decennial Census as this survey covers the entire United States (Gates and Ost, 2004; Anacker and Morrow-Jones, 2005; Hayslett and Kane, 2011; Christafore and Leguizamon, 2017).[7] The 1990 Decennial Census added "unmarried partner" as a response option to describe how the head of the house is related to other household members. Figure 2.1 shows a sample of the response options. Same-sex couple households are identified when the head of the house says they are living with their unmarried partner who is the same sex as the head of the house. Therefore, the 1990, 2000, and 2010 Decennial Census allows identification of gay couples that are living together, but does not identify the entire LGBTQ+ population.

Unfortunately there are issues with the Census method to identify same-sex couple households. The possibility that respondents do not understand what an "unmarried partner" is might lead to reporting the wrong relationship to the head of the house. In addition, respondents' might dislike of concept of disclosing their sexual orientation to the government which

[6]Examples of similar research in other countries are Collins (2004) and Collins and Drinkwater (2017) in the UK and Goldie (2018) in Australia.

[7]Other sources on gay individuals exist, but these sources require special permission to gain access to these datasets.

would mean respondents purposely misreport their relationship to the head of the house. There are also concerns that opposite-sex couples accidentally misreport their partner's sex so that the opposite-sex couple are recorded as a same-sex couple in the Census. Different methods are used in 1990, 2000, and 2010 Decennial Census to correct for this last issue but only the methods in 2000 and 2010 are discussed below.[8]

The 2000 Census had to determine how to recode households who said they were a same-sex married household because same-sex marriages were not allowed in the United States at this time. While the 1990 Census changed the sex to record these households as opposite-sex married households, the 2000 Census changed the relationship to the householder so that these households are recorded as same-sex unmarried partners. Since the amount that identified their opposite-sex partner as the wrong sex in 2000 is nontrivial, there is a concern that an opposite-sex couple accidentally checked the wrong sex and identified their opposite-sex partner as the same sex. This would mean that the number of people living with same-sex partner recorded by the Census is actually made up of a mixture of same-sex partners and opposite-sex partners.

Although this reduces the sample size of same-sex couples in the 2000 Census, Black et al. (2007a) recommend not including same-sex couples with an allocated marital status. Unfortunately, the 2000 Census data does not make it clear how many same-sex unmarried partners had their marital status allocated. Therefore, to correct the recorded count of same-sex couple households in the 2000 Census, the following method can be used: find the proportion of same-sex unmarried partners that did not have their marital status allocated in the 2000 5% Integrated Public Use Microdata Series (IPUMS) data and apply those proportions to the number of same-sex unmarried partner households in the 2000 Decennial Census. This requires finding the proportion of unallocated same-sex unmarried partners

[8] In 1990, same-sex marriages were not allowed so a decision on how to edit responses that said that the head of household lived with their same-sex spouse. The short form of the 1990 Census asked for each person's marital status. The decision on how to edit same-sex couples who responded that they are living with their husband/wife was decided by their marital status (Black et al., 2007a). If the same-sex couple were "currently married" then the Census edited the sex of the husband/wife to make the couple an opposite-sex married couple.

in each Public Use Microdata Area (PUMA) and multiplying this PUMA-level proportion by the count of same-sex couple households from the Census. In order to apply PUMA-level proportions to the Census tract-level counts, a geographic conversion must be used to determine which tracts are in which PUMA. This method to correct the Census recorded count of same-sex households is used by Christafore and Leguizamon (2012), Christafore et al. (2013), and Leguizamon and Leguizamon (2017).

For the 2010 Decennial Census a new method was developed to deal with the possibility that opposite-sex couples miscode their sex and are incorrectly recorded as same-sex couples. The Census edited respondent's sex based on their first name to obtain a "preferred" count of same-sex couples (Census, 2011). The Census changed the sex of respondents based on if there was a 95 percent chance that the name was specific to one sex (O'Connell and Feliz, 2011). For example, the Census would only change the sex of a recorded female named Michael if the name Michael had a 95 percent chance that the name is male. After editing sex based on the name of respondents, the count of same-sex couple households is referred to as the "preferred" count of same-sex couple households in the 2010 Census.

Unfortunately, the "preferred" count of same-sex couple households for the 2010 Census was only released at the state level meaning that sub-state level "preferred" counts was not made available. Gates and Cooke (2012) describe a method to obtain the "preferred" counts of same-sex couple households on sub-state level. The method used to obtain the 2010 tract-level counts of same-sex couple households, which is described below, includes obtaining data from the 2010 "preferred" count of same-sex couple households, raw data from the 2010 Census, and the 2010 Census mail-in participation rates.[9] The method described in Gates and Cooke (2012) to obtain tract-level counts of same-sex couples finds the following

[9]The 2010 state-level "preferred" count of same-sex couples can be obtained from Gates and Cooke (2012) and the mail-in participation rates are available in an interactive map at https://www.census.gov/censusexplorer/2010ratemap.html but can be obtained in a downloadable file at https://www.census.gov/data/datasets/2010/dec/2010-participation-rates.html.

for each tract i[10]

$$error_i = (0.003 * \frac{mail\ in\ participation\ rate_i}{100}) + (0.01 * (1 - \frac{mail\ in\ participation\ rate_i}{100})) \tag{2.1}$$

$$\begin{aligned} &preferred\ \#\ of\ same\text{-}sex\ male\ couples_i = \\ &\quad same\text{-}sex\ male\ unmarried\ partners_i - (error_i * \\ &\quad (opposite\text{-}sex\ married\ couples\ with\ male\ householder_i + \\ &\quad opposite\text{-}sex\ unmarried\ couples\ with\ male\ householder_i)) \end{aligned} \tag{2.2}$$

$$\begin{aligned} &preferred\ \#\ of\ same\text{-}sex\ female\ couples_i = \\ &\quad same\text{-}sex\ female\ unmarried\ partners_i - (error_i * \\ &\quad (opposite\text{-}sex\ married\ couples\ with\ female\ householder_i + \\ &\quad opposite\text{-}sex\ unmarried\ couples\ with\ female\ householder_i)) \end{aligned} \tag{2.3}$$

$$\begin{aligned} &proportion\ of\ preferred\ \#\ of\ same\text{-}sex\ couples_i = \\ &\frac{preferred\ \#\ of\ same\ sex\ male\ couples_i + preferred\ \#\ of\ same\ sex\ femalecouples_i}{\sum_{i=1} preferred\ \#\ of\ same\ sex\ male\ couples_i + preferred\ \#\ of\ same\ sex\ femalecouples_i} \end{aligned} \tag{2.4}$$

$$\begin{aligned} &tract\text{-}level\ preferred\ \#\ of\ same\text{-}sex\ couples_i = \\ &\quad (state\text{-}level\ preferred\ \#\ of\ same\text{-}sex\ couples * \\ &\quad proportion\ of\ preferred\ \#\ of\ same\text{-}sex\ couples_i) \end{aligned} \tag{2.5}$$

where all non-preferred counts of couples are obtained from the 2010 Decennial Census. The $tract\text{-}level\ preferred\ \#\ of\ same\text{-}sex\ couples_i$ is what is used as the corrected "preferred" count of same-sex couple households in each 2010 census tract.

Counting same-sex couple homebuyers overcomes several limitations from previous methods used to identify gay couples. Previously used surveys are specific to geographical areas at a specific time, while Census data can only be considered in ten-year intervals and at

[10] This method can be used to find any other sub-state level, like county, city, or block group. However, this paper describes how to obtain tract-level counts of same-sex couple households.

specific, predefined geographic areas. Using the name of homebuyers can be used in any geographical area and the amount of same-sex couple households living in areas can be determined in small time intervals, like months. Using homebuyer information overcomes the problems of correcting the Census data as corrections to the Census data can be lengthy and time-consuming. Although surveys, the Census, and homebuyer information can be subject to fear of disclosing sexual orientation, it is likely not as much of a problem for homebuyers.[11]

2.3 Proposed Identification Method of Same-Sex Couple Households

In this paper we use different data and propose a new approach to identify same-sex couple homebuyers. We use datasets of residential transactions from three counties: Miami-Dade County, Florida; Franklin County, Ohio; and King County, Washington. The Miami-Dade County dataset is available for purchase from the Miami-Dade County Public Access Bulletin Board, while the Franklin County and King County datasets are publicly available from the Franklin County Auditor's Office and from the King County Assessor's Office, respectively. These datasets provide transaction data for single-family homes and contains information on the year of transaction, characteristics of the property, transaction value, address, and the name of buyers and sellers. We follow a similar procedure to the one described in Nowak and Sayago-Gomez (2018) and Humphreys et al. (2019) to predict the sex of the home buyers. If the buyers are predicted to be both male or both female, we argue that this household is occupied by a male same-sex couple homebuyer or female same-sex couple homebuyer, respectively.

The buyer name field includes the names for one or more individuals but does not include

[11] The repercussions for withholding information on the Census or in a survey is minimal compared to the repercussions of not correctly putting names on the deed to a house. In addition, Figure A.1 in the Appendix shows how many male same-sex homebuyers there were in King County, Washington between 1990 and 2015. At first, the amount of male same-sex homebuyers increases until about the mid-2000s. This increase could be a combination of more gay couples buying houses together and more couples feeling comfortable with putting both of their names on the deed when buying the house. However, after the mid-2000s, the amount of male same-sex couple homebuyers decreases, which would likely not be observed if previous increases were solely due to increased comfortability with disclosing sexual orientation.

the sex or relationship of the individuals. To determine information about sex based on the name of homebuyers, we use data from the Social Security Administration (SSA). This data is freely available at the SSA website[12] and can be accessed via the *gender* package in *R*. The SSA data is used to determine the likelihood of names being either first or last names and the likelihood of first names being male or female names, based on the frequencies of male and female first names in the SSA database. Below are the steps taken to determine the number of same-sex homebuyers:

1. Remove non-residential transactions from the dataset of sold parcels

2. Identify transactions with exactly two homebuyers based on the presence of only a single conjunction "and" in the buyer name field (AND). Hence, we do not count transactions with more than two homebuyers

3. For each homebuyer token[13] we create three counts: male first name (MALE), female first name (FEMALE), and last name (LAST). These counts are based on a cutoff in which the probability that a name meets the criteria must be at least 95%. For illustration purposes:

   (a) Token = "dobbs james deborah". TOTAL = 3, MALE =1, FEMALE = 1, LAST = 1

   (b) Token = "jennings jerry steven piatt". TOTAL = 4, MALE =2, FEMALE = 0, LAST = 2

   (c) Token = "norman john norman janie". TOTAL = 3, MALE =1, FEMALE = 1, LAST = 0[14]

[12] See https://www.ssa.gov/oact/babynames/limits.html.

[13] According to Humphreys et al. (2019), tokens are units of text represented by exchangeable collections of words. Using names as tokens for classification into different groups is usual in the literature (Nowak and Smith, 2017; Nowak and Sayago-Gomez, 2018; Humphreys et al., 2019).

[14] In this example, TOTAL=3 because we count the number of unique names in each token. In addition, LAST=0 because "norman" has a higher probability of being a first name than a last name.

4. Using these counts and the "and" identifier we create our measures of same-sex homebuyers by using the following filter:

   (a) Same-sex male homebuyer: AND = YES, MALE = 2, FEMALE = 0

   (b) Same-sex female homebuyer: AND = YES, MALE = 0, FEMALE = 2

   This count of same-sex couple homebuyers that results from this is referred to as Measure 1.

5. This first measure of same-sex homebuyers does not take the last name into account, which can possibly result in overcounting same-sex homebuyers if same-sex family members purchase homes together like a father and son. Thus, we create a second measure, referred to as Measure 2, in which there are two different last names, using the following filter:

   (a) Same-sex male homebuyer with different last names: AND = YES, MALE = 2, FEMALE = 0, LAST = 2

   (b) Same-sex female homebuyer with different last names: AND = YES, MALE = 0, FEMALE = 2, LAST = 2

6. Finally, we impose an additional restriction. We do not count possible flipped houses that could have been bought by same-sex business partners instead of same-sex homebuyers. We follow Depken II et al. (2009) by counting those in which the transaction date is less than 730 days (2 years) from the next sale as flipped houses. That is:

   (a) Same-sex male homebuyer: AND = YES, MALE = 2, FEMALE = 0, FLIP = NO

   (b) Same-sex female homebuyer: AND = YES, MALE = 0, FEMALE = 2, FLIP = NO

   (c) Same-sex male homebuyer with different last names: AND = YES, MALE = 2, FEMALE = 0, LAST = 2, FLIP = NO

(d) Same-sex female homebuyer with different last names: AND = YES, MALE = 0, FEMALE = 2, LAST = 2, FLIP = NO

Removing flipped houses from Measure 1 results in Measure 3, which consists of (6a) and (6b). Measure 4 is when the restrictions imposed in Measure 2 and Measure 3 are implemented together, which consists of (6c) and (6d).

In total, there were 2,056,198 residential transactions in Miami-Dade County; 679,443 in Franklin County; and 898,067 in King County. From Measure 1, that is, no last name restriction, we identified 12,947 same-sex homebuyers in Miami-Dade County; 7,797 same-sex homebuyers in Franklin County; and 42,270 in King County. Adding the last name restriction, the amount of identified same-sex homebuyers in Measure 2 are 3,397; 789; and 6,469, respectively.

2.4 Summary Statistics and Distribution of Same-Sex Couple Households

To determine if our proposed method is successful at identifying same-sex couple homebuyers, we will explore the names identified in Section 2.3 Step 3, summary statistics of houses purchased by same-sex couple homebuyers, and the distribution and correlation of same-sex couple homebuyers and same-sex couple households.

The sex of homebuyers is determined by the name of homebuyers. Figure 2.2, Figure 2.3, and Figure 2.4 present a wordcloud of the 100 most frequent (first and last) names identified in Miami-Dade County, Franklin County, and King County, respectively. Panel A, Panel C, Panel E, and Panel G show the first and last names of males using Measure 1, Measure 2, Measure 3, and Measure 4 respectively, while Panel B, Panel D, Panel F, and Panel H show the first and last names of females using Measure 1, Measure 2, Measure 3, and Measure 4, respectively. As the names identified as male appear to be common male names and names identified as female appear to be common female names, using names of homebuyers is an appropriate method to identify the sex of homebuyers. Table A.1, Table A.2, and

Table A.3 in the Appendix shows the top 20 names in Figure 2.2, Figure 2.3, and Figure 2.4, respectively.

According to the current real-estate literature on same-sex households, we should expect that these same-sex homebuyers would live in older and/or smaller properties which could be associated to those closer to city centers, where it is harder to develop (Gates and Ost, 2004). Thus, we compare the average characteristics for houses purchased by same-sex homebuyers and for houses purchased by all other homebuyers. Table 2.1 displays the average number of rooms (bedrooms and bathrooms), year the house was built, price, and house's square footage for same-sex couple homebuyers and also for other homebuyers. The Welch Two Sample T-test of means is reported. The statistics are presented in Table 2.1

In order to compare our count of same-sex couple homebuyers to the count of same-sex couple households from the Census, we create two separate counts of same-sex couple homebuyers. We determine the number of same-sex couple homebuyers living in each census tract in 2000 and separately in 2010. For each of these, we consider only the last transaction for each parcel before the year 2000 or 2010, respectively. Therefore, even though we are able to have a panel-data of repeated sales, we restrict our data to a cross-section of parcels with information on the last sale for each parcel. We use shapefiles from the Census to determine which census tract the house parcel belongs to.[15] Since researchers use the percent of households that are same-sex couple households, we will use the percent of homebuyers that are same-sex couple homebuyers which is accomplished by dividing the number of same-sex couple homebuyers by the number of homebuyers and multiplying by 100.

The distribution of the percent of same-sex couple homebuyers from all four measures can be seen in Figure 2.5. Panel A depicts the distribution for 2000 and Panel B depicts the distribution for 2010. Figure 2.6 shows the distribution of the percent of same-sex couple homebuyers from Measure 1 with the unedited and edited percent of same-sex couple households from the Census where Panel A is for 2000 and Panel B is for 2010. Based on

[15] The shapefiles used were obtained from the Census for 2000 and 2010 at https://www2.census.gov/geo/pvs/tiger2010st/.

these figures, Measure 1 same-sex couple homebuyers match the Census same-sex couple households best as the distributions take on the same shape. In particular, the shape of the distributions match better in 2010 than in 2000, which could be because more overall data is available in 2010 than in 2000. Therefore, this method for identifying same-sex couples might be a more appropriate method if more current data is needed.

Although the shape of the distributions of Measure 1 same-sex couple homebuyers and same-sex couple households are similar, the distribution of same-sex couple homebuyers is shifted to the left, which can be seen in Figure 2.6. This shift could be due to different reasons. For instance, homebuyers are a subset of householders. In addition, this could be due to the method of the editing the 2000 and 2010 count of same-sex couple households. For example, the 2000 method assumes even distribution between PUMA and census tracts during the conversion.

In addition, we explored the correlation matrix between the percent of same-sex couple homebuyers from our four different measures and the percent of Census same-sex couple households (both the edited and unedited). The correlation matrices can be found in Table 2.2 for 2000 and Table 2.3 for 2010. The correlation matrices show that for Measure 1 and Measure 2 there is about a 50% correlation between these measures and the same-sex couple households. Measure 3 and 4 do not work as well, which could stem from issues associated with identifying flipped houses. Ideally these correlations would be higher, however, since we compare same-sex couple homebuyers and same-sex couple households, which can include different groups of people, we show that our measures follow the same tendency as those calculated by the Census. Overall, the method proposed in this paper to identify same-sex couple homebuyers appears to be correctly identifying same-sex couple homebuyers.

2.5 Conclusion

This paper proposes a new method for the identification of same-sex households. Using the real estate transaction records from the Miami-Dade County, Franklin County, and King

County, we identify same-sex couple homebuyers by predicting the sex of homebuyers based on their first and last names. After exploring the names identified as male names and female names, it appears that our method to identify sex based on names accurately predicts the sex of homebuyers. The density of same-sex couple homebuyers appears to match the density of same-sex couple households from the Census, implying that the method proposed in this paper can be used for future research.

The method proposed in this paper to identify same-sex couple homebuyers as a proxy for members of the LGBTQ+ community is an improvement over previous methods. Surveys exist but pose issues as they are limited to smaller geographic areas. The Census is commonly used to identify same-sex unmarried partner households, but require additional data and calculations in order to use the Census data. The Census is limited to pre-defined geographic areas in pre-specified time periods. Homebuyer information include addresses and date of sale so the number of same-sex couple homebuyers can be determined for any geographic area and any time period.

Our analysis does not come without limitations. Similar to other studies we rely on same-sex couples that are living together and are unable to identify all individuals in the LGBTQ+ community. Additionally, we use the name of homebuyers for our identification method. Therefore, we can misidentify names that are commonly used for both male and female individuals. Unfortunately, the 2010 Decennial Census encounters the same problem.

Table 2.1: Comparison of Means of Same-Sex Couple Homebuyers and Other Homebuyers

	Measure 1			Measure 2		
	Same-Sex Couple Homebuyers	Other Homebuyers	Difference	Same-Sex Couple Homebuyers	Other Homebuyers	Difference
Miami-Dade County, Florida						
# of Rooms	5.03	8.70	-3.67***	4.91	8.68	-3.78***
Year Built	1942.28	1918.45	23.83***	1940.14	1918.57	-21.56***
Price ($1000)	152.51	437.76	-285.25***	161.31	436.28	-274.97***
Area (Sq. Ft.)	9543.09	21925.21	-12382.12***	9226.09	21862.10	-12636.01***
Franklin County, Ohio						
# of Rooms	5.9	5.72	0.18***	5.62	5.72	-0.10
Year Built	1747.38	1781.13	-33.75***	1614.29	1780.92	166.63***
Price ($1000)	73.06	186.92	-113.86***	53.21	185.74	-132.53***
Area (Sq. Ft.)	1467.1	1460.3	6.80	1347.62	1460.51	-112.89***
King County, Washington						
# of Rooms	4.66	4.49	0.17***	4.57	4.50	0.08***
Year Built	1675.12	1542.38	132.74***	1677.11	1547.69	129.42***
Price ($1000)	330.81	316.48	14.33***	322.47	317.12	5.35*
Area (Sq. Ft.)	1875.77	1783.66	92.11***	1788.36	1787.99	0.37
	Measure 3			Measure 4		
Miami-Dade County, Florida						
# of Rooms	5.03	8.70	-3.67***	4.91	8.68	-3.78***
Year Built	1942.28	1918.45	23.83***	1940.14	1918.57	-21.56***
Price ($1000)	152.51	437.76	-285.25***	161.31	436.28	-274.97***
Area (Sq. Ft.)	9543.09	21925.21	-12382.12***	9226.09	21862.10	-12636.01***
Franklin County, Ohio						
# of Rooms	5.9	5.72	0.18***	5.62	5.72	-0.10
Year Built	1747.38	1781.13	-33.75***	1614.29	1780.92	166.63***
Price ($1000)	73.06	186.92	-113.86***	53.21	185.74	-132.53***
Area (Sq. Ft.)	1467.1	1460.3	6.80	1347.62	1460.51	-112.89***
King County, Washington						
# of Rooms	4.66	4.49	0.17***	4.57	4.50	0.08***
Year Built	1675.12	1542.38	132.74***	1677.11	1547.69	129.42***
Price ($1000)	330.81	316.48	14.33***	322.47	317.12	5.35*
Area (Sq. Ft.)	1875.77	1783.66	92.11***	1788.36	1787.99	0.37

Welch Two Sample t-test. *p<0.10, **p<0.05. ***p<0.01

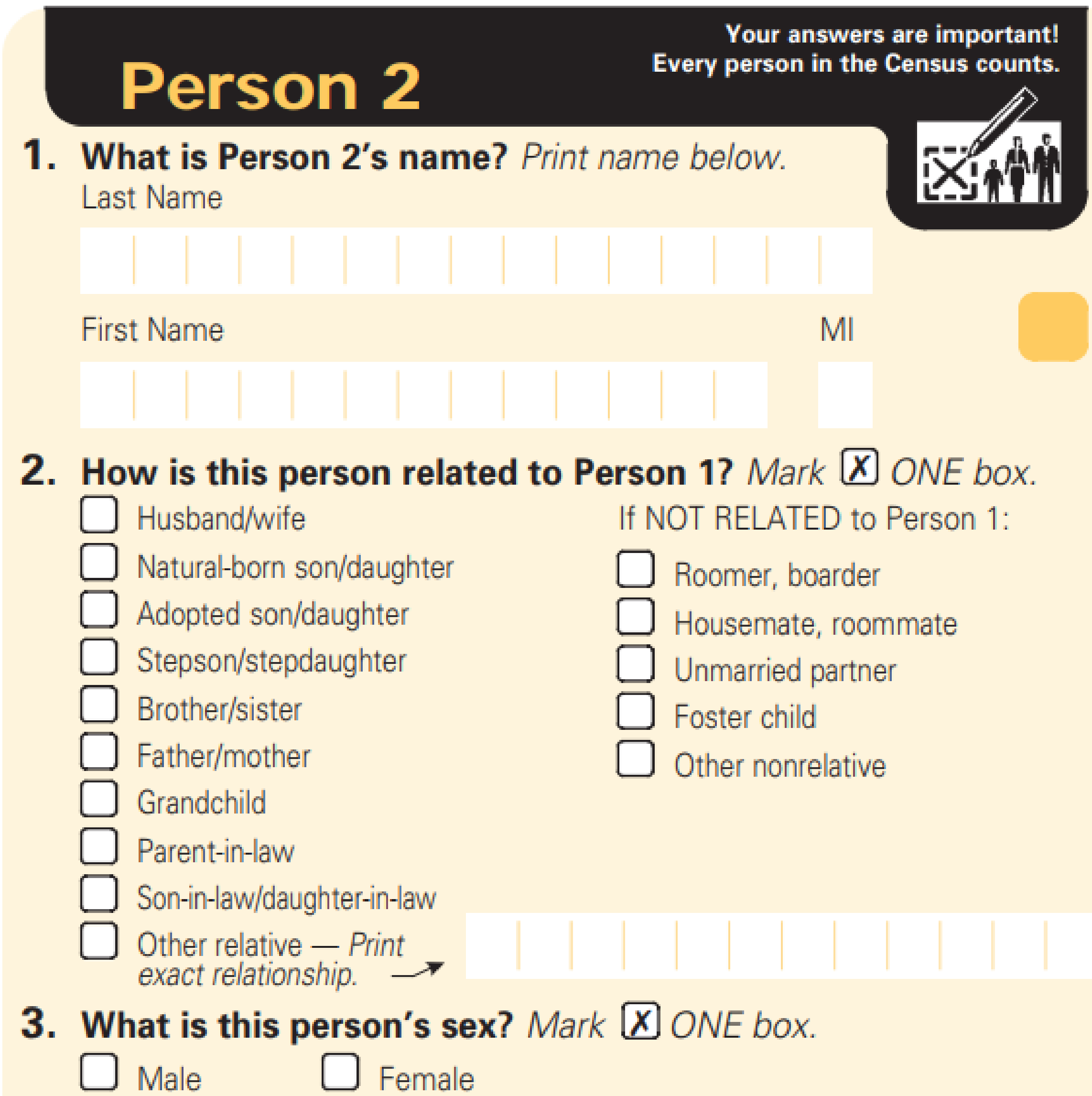
Table 2.2: Correlation Matrix for Percent of Same-Sex Couples in 2000

	Measure 1 Percent of Same-Sex Couple Homebuyers	Measure 2 Percent of Same-Sex Couple Homebuyers	Measure 3 Percent of Same-Sex Couple Homebuyers	Measure 4 Percent of Same-Sex Couple Homebuyers	Percent of Edited Same-Sex Couple Households	Percent of Unedited Same-Sex Couple Households
Measure 1 Percent of Same-Sex Couple Homebuyers	1.0000					
Measure 2 Percent of Same-Sex Couple Homebuyers	0.6076	1.0000				
Measure 3 Percent of Same-Sex Couple Homebuyers	0.2034	-0.0114	1.0000			
Measure 4 Percent of Same-Sex Couple Homebuyers	0.0479	0.2004	0.3102	1.0000		
Percent of Edited Same-Sex Couple Households	0.5554	0.4146	-0.0534	-0.0075	1.0000	
Percent of Unedited Same-Sex Couple Households	0.5092	0.3800	-0.0239	0.0180	0.9707	1.0000

Table 2.3: Correlation Matrix for Percent of Same-Sex Couples in 2010

	Measure 1 Percent of Same-Sex Couple Homebuyers	Measure 2 Percent of Same-Sex Couple Homebuyers	Measure 3 Percent of Same-Sex Couple Homebuyers	Measure 4 Percent of Same-Sex Couple Homebuyers	Percent of Edited Same-Sex Couple Households	Percent of Unedited Same-Sex Couple Households
Measure 1 Percent of Same-Sex Couple Homebuyers	1.0000					
Measure 2 Percent of Same-Sex Couple Homebuyers	0.6319	1.0000				
Measure 3 Percent of Same-Sex Couple Homebuyers	0.2503	0.0414	1.0000			
Measure 4 Percent of Same-Sex Couple Homebuyers	0.0973	0.2912	0.3442	1.0000		
Percent of Edited Same-Sex Couple Households	0.4785	0.3446	-0.0344	0.0061	1.0000	
Percent of Unedited Same-Sex Couple Households	0.4639	0.3336	-0.0309	0.0075	0.9970	1.0000

Figure 2.1: Census Questionnaire Identifying Same-Sex Couple Households



Person 2

Your answers are important! Every person in the Census counts.

1. What is Person 2's name? *Print name below.*

Last Name

First Name

MI

2. How is this person related to Person 1? *Mark* ☒ *ONE box.*

- ☐ Husband/wife
- ☐ Natural-born son/daughter
- ☐ Adopted son/daughter
- ☐ Stepson/stepdaughter
- ☐ Brother/sister
- ☐ Father/mother
- ☐ Grandchild
- ☐ Parent-in-law
- ☐ Son-in-law/daughter-in-law
- ☐ Other relative — *Print exact relationship.*

If NOT RELATED to Person 1:

- ☐ Roomer, boarder
- ☐ Housemate, roommate
- ☐ Unmarried partner
- ☐ Foster child
- ☐ Other nonrelative

3. What is this person's sex? *Mark* ☒ *ONE box.*

☐ Male ☐ Female

In the 1990, 2000, and 2010 Decennial Census, same-sex couple households could be identified when Person 2 is the same sex as Person 1 and is considered to be the "unmarried partner" of Person 1.

Figure 2.2: Wordcloud of First and Last Names in Miami-Dade County, Florida

Male First and Last Names | Female First and Last Names

Panel A: Male Measure 1 | Panel B: Female Measure 1

Panel C: Male Measure 2 | Panel D: Female Measure 2

Panel E: Male Measure 3 | Panel F: Female Measure 3

Panel G: Male Measure 4 | Panel H: Female Measure 4

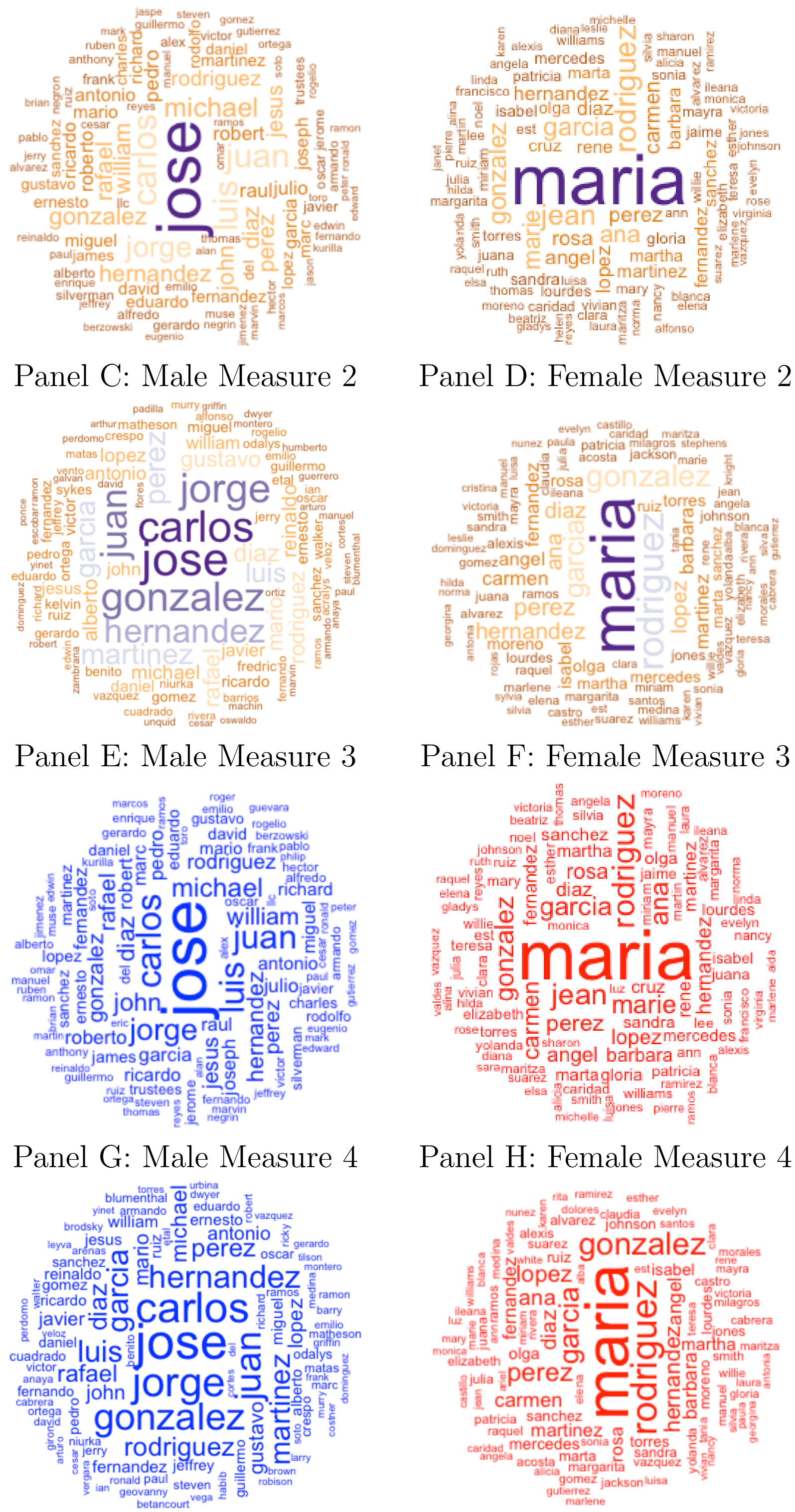
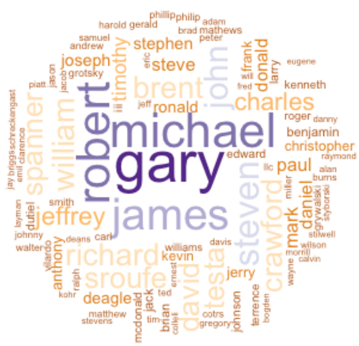


Figure 2.3: Wordcloud of First and Last Names in Franklin County, Ohio

Male First and Last Names | Female First and Last Names

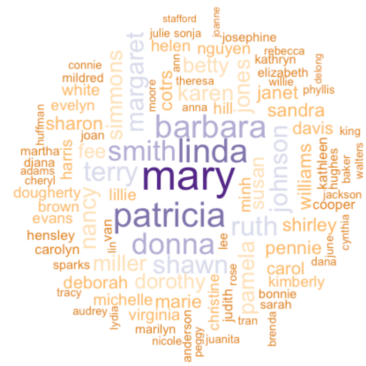
Panel A: Male Measure 1



Panel B: Female Measure 1

Panel C: Male Measure 2

Panel D: Female Measure 2



Panel E: Male Measure 3

Panel F: Female Measure 3

Panel G: Male Measure 4

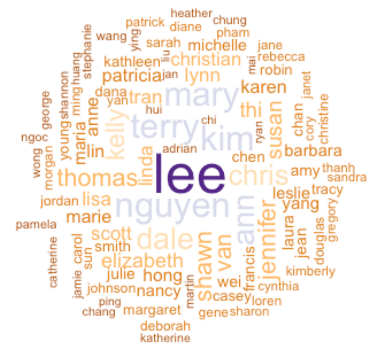
Panel H: Female Measure 4

Figure 2.4: Wordcloud of First and Last Names in King County, Washington

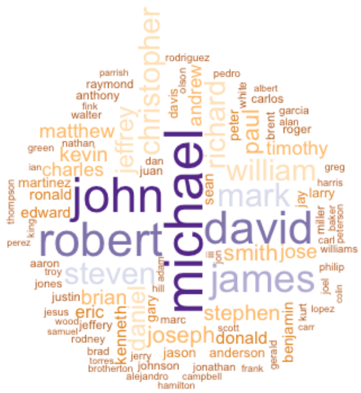
Male First and Last Names | Female First and Last Names

Panel A: Male Measure 1

Panel B: Female Measure 1



Panel C: Male Measure 2



Panel D: Female Measure 2

Panel E: Male Measure 3

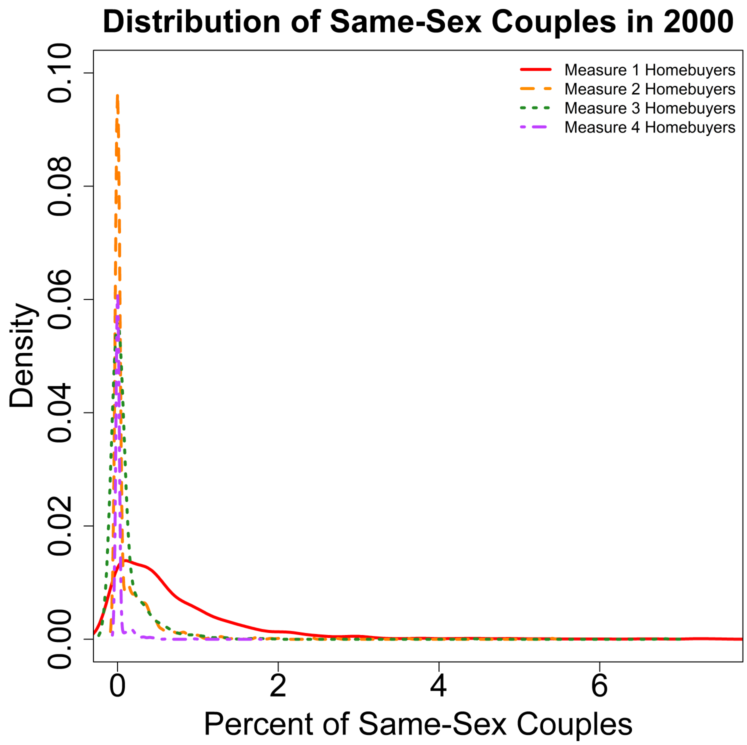
Panel F: Female Measure 3

Panel G: Male Measure 4

Panel H: Female Measure 4

Figure 2.5: Distribution of Same-Sex Couple Homebuyers

Panel A: Distribution of Same-Sex Couple Homebuyers in 2000



Panel B: Distribution of Same-Sex Couple Homebuyers in 2010

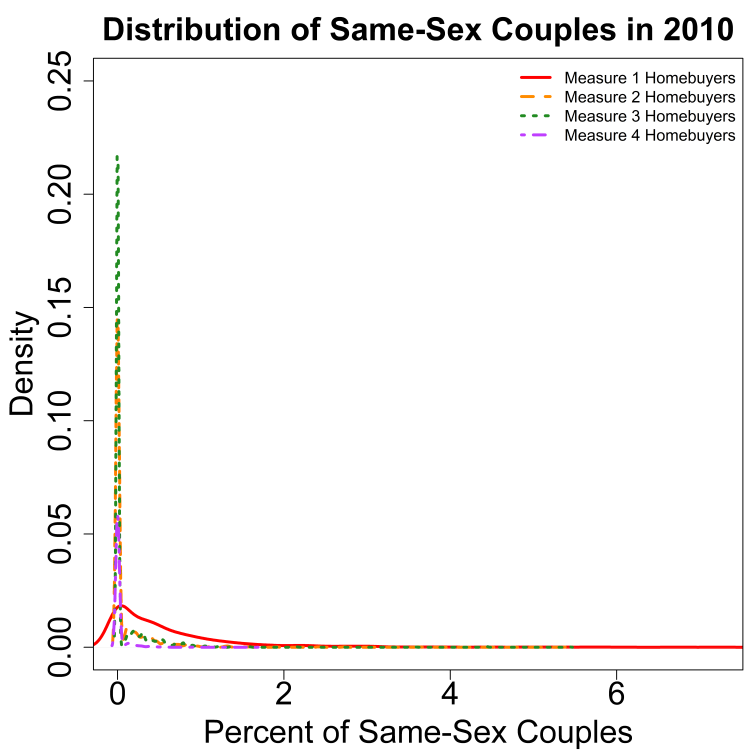
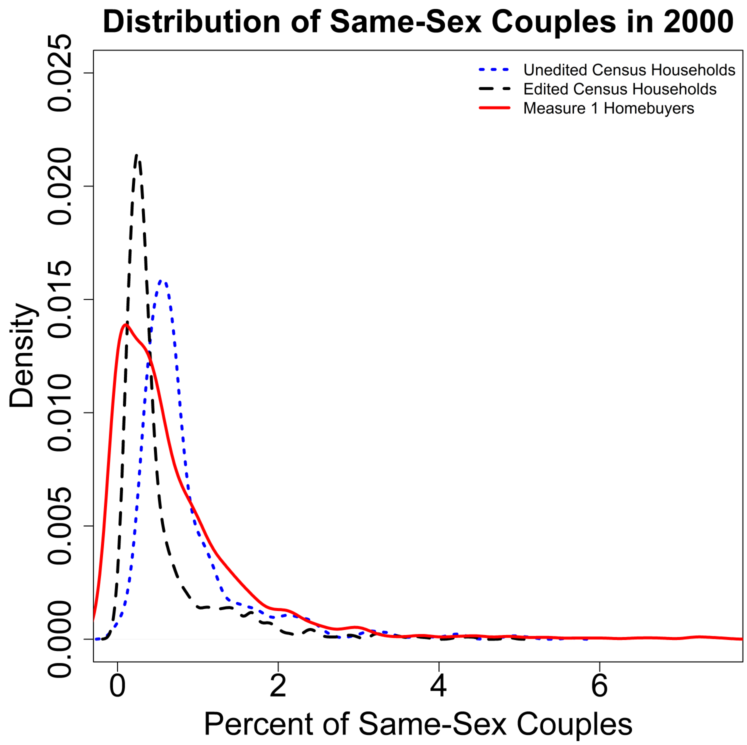
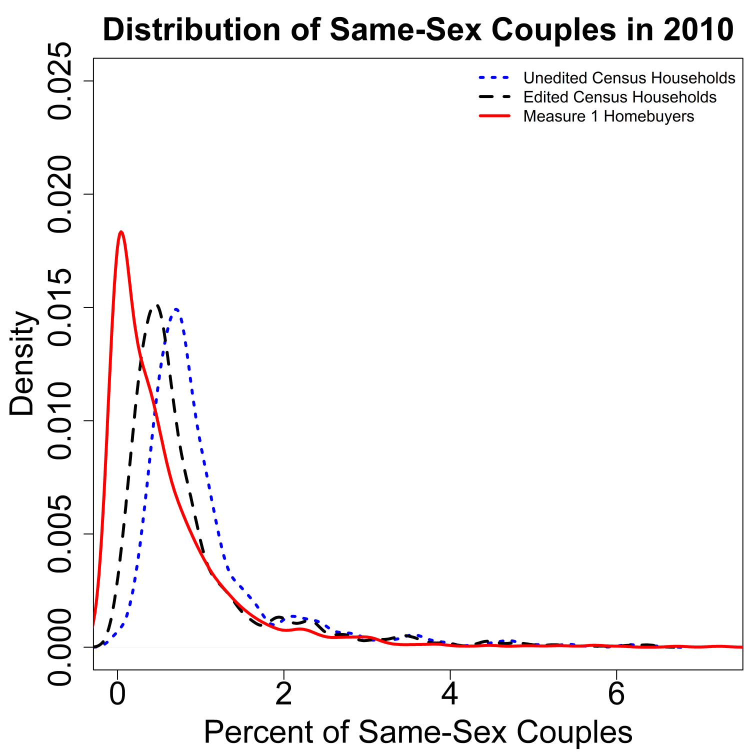


Figure 2.6: Distribution of Same-Sex Couple Households and Homebuyers

Panel A: Distribution of Same-Sex Couple Households and Homebuyers in 2000



Panel B: Distribution of Same-Sex Couple Households and Homebuyers in 2010



CHAPTER 3

HOUSING SPILLOVER EFFECTS FROM HURRICANE DAMAGES AND REPAIRS: A LOOK AT HURRICANE CHARLEY

3.1 Introduction

The destruction caused by natural disasters, and hurricanes in particular, can be intense and long-lasting. As the population density increases in coastal areas, these populations have a greater chance of experiencing hurricanes and other natural disasters (Hallstrom and Smith, 2005; Kousky, 2010; Boustan et al., 2017). Since 2000, economic losses from natural disasters have exceeded $600 billion (Carson et al., 2013). Hurricanes, like the ones experienced in Florida, affect migration patterns, house prices, the number of home sales, housing tenure, poverty rates, the demographics and income of residents, property abandonment, taxable sales, and employment (Lenze, 1997; Smith et al., 2006; Belasen and Polachek, 2008, 2009; Zhang and Peacock, 2009; Belasen and Dai, 2014; Peacock et al., 2014; Boustan et al., 2017; Davlasheridze and Fan, 2017; Lee., 2018; Liao and Panassié, 2019).

This paper uses data on houses in Punta Gorda, Florida following Hurricane Charley in 2004 to measure the spillover effects of damaged and repaired houses on the selling price of nearby houses. Following previous spillover papers (Schuetz et al., 2008; Lin et al., 2009), the number of damaged houses and the number of repaired houses are added to a hedonic price model. This allows the characteristics of the selling house to be controlled for, while the number of damaged and repaired houses are added to explore the spillover effect they exert on the selling house.

To the best of the author's knowledge, this paper is the first to investigate the spillover effects of hurricane-induced damages and repairs on house prices. Exploring spillover effects related to natural disasters has found that houses are more likely to rebuild if neighboring house rebuild (Spader, 2015; Hornbeck and Keniston, 2017; Fu and Gregory, 2019) and that property abandonment induces abandonment of neighboring properties (Zhang, 2012). However, this literature does not explore the spillover effect on house prices.[1] In addition, previous papers that look at the non-spillover effect of hurricanes on house prices (Hallstrom and Smith, 2005; Carbone et al., 2006; Davlasheridze and Fan, 2017; Liao and Panassié, 2019) typically use aggregate house prices for counties or census blocks (Smith et al., 2006; Boustan et al., 2017), use tax appraised home values (Zhang and Peacock, 2009; Peacock et al., 2014), or exposure to damage. This paper focuses on the effect of a hurricane on individual house sales, which might help determine more accurate market effects. In addition, this paper is able to identify which individual houses received damage from Hurricane Charley.

Hurricanes that destroy housing can be extremely detrimental as the time it takes to rebuild houses in a neighborhood is at least two years (Belasen and Polachek, 2008; Zhang and Peacock, 2009; Peacock et al., 2014) but typically takes four or five years (Spader, 2015; Smart and Prohaska, 2017). Intense hurricane damage requires labor, material, permits, funding, and time to repair, and waiting on insurance settlements or government funding can delay repairs (Liao and Panassié, 2019). Therefore, it is possible that houses remain damaged or destroyed for several years after a hurricane. A damaged house might negatively affect the selling price of neighboring houses, similar to the negative spillover effects associated with foreclosed or vacant houses found in the previous literature (Schuetz et al., 2008; Harding et al., 2009; Lin et al., 2009; Zhang and Peacock, 2009; Campbell et al., 2011; Zhang, 2012; Sadayuki et al., 2019). In addition, damaged houses might increase the perceived risk associated with living in locations prone to hurricanes, which could reduce house prices.

[1] The literature on spillover effects on house prices is vast, investigating the spillover effects of removing rent control (Autor et al., 2014), sex offenders moving into neighborhoods (Lin et al., 2008; Pope, 2008), urban renewal programs (Rossi-Hansberg et al., 2010), shale gas development (Meuhlenbachs et al., 2015), foreclosures (Schuetz et al., 2008; Harding et al., 2009; Lin et al., 2009; Campbell et al., 2011; Ellen et al., 2013; Lacoe and Ellen, 2015), and vacant houses (Sadayuki et al., 2019). However, the author is not aware of any literature that investigates the spillover effects on house prices due to natural disasters.

On the other hand, rebuilding after a hurricane poses an interesting opportunity to improve the community. As housing is a durable good, homes may last for several decades (Glaeser and Gyourko, 2005) and not get the necessary updates (Siodla, 2015). Hurricane damage might incentivize, or even force, residents into improving their house. In the United States, once the repairs cost 50% of the value of the pre-damage house, the law requires that the building be brought up to the most current building code (Peacock et al., 2018).[2]

Repairing a damaged house could increase the value of that house for several reasons. First, the house might be built to a more stringent building code or with strong building materials such as fortified windows with the hope that the house will perform better in future hurricanes (Rathfon et al., 2013). Having additional mitigation features has been found to increase the value of the house (Dumm et al., 2011; Gatzlaff et al., 2018; Awondo et al., 2019). Second, as the majority of the damage in Punta Gorda was roof related, houses had to build new roofs. This new roof could increase the value of the house. Third, home remodels or additions might occur in the repair process (Lenze, 1997). Although remodels and additions can be costly, they are likely to increase the value of the house. Hornbeck and Keniston (2017) found land values increased after the 1872 Boston Fire as positive spillovers were generated by new, better buildings. Taken together, the value of these repaired houses could increase resulting in a positive spillover effect on the value of nearby houses.

Estimates from a hedonic regression find that Hurricane Charley acts as a temporary positive shock to the housing market, increasing the housing prices immediately after the hurricane but then the prices slowly return to pre-hurricane levels. Damaged houses have no statistically significant effect. Repaired houses that increased their square footage have a positive spillover effect. However, all other repaired houses have a negative spillover effect.

[2]The Uniform Building Code in 1927, Southern Standard Building code in 1946, and the Basic Building Code of the Building Officials Conference of America in 1950 required that a house be repaired to the current building code if the repairs cost 50% of the value of the house before the house was damaged. This required the entire house to be upgraded, not just the damaged portions. If the cost of repairs were between 25 and 50% of the house value then only the damaged portions of the house needed to be upgraded. With the International Building Code in 2000, only the damaged portions required upgrading. Starting in 2015, the International Existing Building Code relaxed these standards to allow reconstruction to bring the house back to pre-damage levels and not to current code. See https://www.structuremag.org/?p=11023.

The spillover effects of damaged and repaired houses found in this paper are for nearby houses – specifically houses that are less than 0.1 mile away.

The next section give some background information on Hurricane Charley. The Section 3.3 discusses the data and Section 3.4 provides the methodology. Results are shown in Section 3.5 and robustness checks are presented in Section 3.6. Section 3.7 concludes.

3.2 Timeline of Hurricane Charley in Florida

Hurricanes disproportionately hit the state of Florida relative to other states in the United States.[3] Table 3.1 lists the hurricanes whose eye traveled across the state of Florida since 1970, while Figure 3.1 depicts the paths of these hurricanes. Although the state is often affected by hurricanes, Hurricane Charley was the second strongest since the 1970s (Rathfon et al., 2013) and its recent occurrence allows the after effects to be analyzed due to rich housing data available. Since this hurricane was primarily a wind event, roof damage was the most frequent source of damage and acts as a measure of damage for this study.

Charley became a tropical depression on August 9, 2004 and developed into a hurricane on August 11 as the storm approached Jamaica as a Category 1 hurricane.[4] Charley continued to strengthen as it passed Cuba and headed towards Florida. The forecasts estimated that Charley would strengthen to a Category 2 or 3 hurricane and make landfall on the west coast of Florida near Tampa. On August 13 at 2:00 pm, special advisories were issued to alert people that Charley had strengthened into a Category 4 hurricane. The tract shifted east from the north-moving forecast, causing Charley to make landfall further south than expected (Pasch et al., 2004).

As the storm moved towards land, the eye shrank, causing extreme winds to be confined to a very small area and, around 3:45 pm on August 13, Charley made landfall on the

[3] See https://www.cnn.com/2017/09/11/us/hurricanes-landfall-by-state-trnd/index.html.

[4] The category of the hurricane is determined by Saffir-Simpson hurricane scale, which is based on wind speeds. A Category 1 hurricane has wind speeds between 74 and 95 mph, a Category 2 hurricane has wind speeds between 96 and 110 mph, a Category 3 has wind speeds between 111 and 129 mph, a Category 4 hurricane has wind speeds between 130-156 mph, and a Category 5 has wind speeds over 157 mph.

southwest coast of Florida near Cayo Costa, which is located just north of Captiva.[5] One hour later, Charley's eye passed over Punta Gorda in Charlotte County with maximum sustained winds of 150 mph. Charley continued to move across Florida, losing strength, as it passed Orlando. Then Charley's eye exited the state around Daytona Beach on August 13 at 11:30 pm (Pasch et al., 2004). Figure 3.2 Panel A shows the path of Hurricane Charley through Charlotte County.

3.3 Data

The analysis focuses on Hurricane Charley in the western portion of Punta Gorda in Charlotte County. Figure 3.2 Panel B highlights the part of Punta Gorda used in this analysis.[6] To provide ample recovery time from Hurricane Charley that hit in August 2004, house sales that occur from 2001 to the end of 2009 will be included.

Data on individual house sale transactions is available for download from the Charlotte County Property Appraiser.[7] This data includes the date of the transaction, the folio number,[8] the selling price, an indication for if the sale was for a single-family house, and an indication for if the lot was vacant at the time of sale. The house prices are adjusted to 1982-1984 dollars using the consumer price index from the Federal Reserve Economic Database. This study focuses on non-vacant single-family houses that sold for between the real prices of $43,266 and $545,590.[9] Houses that sold in August 2004 are excluded as the

[5] Cayo Costa is an island located southwest from Punta Gorda and there are minimal building structures on the island which makes it an inappropriate location to investigate housing spillover effects. Punta Gorda was the first area with a large number of housing structures that Charley's eye passed over.

[6] Punta Gorda was the first developed area affected by Hurricane Charley. The small portion of Punta Gorda is analyzed for a variety of reasons. This area analyzed is composed mainly of houses, as compared to businesses or non-developed land. This portion of Punta Gorda is also used in the study of damaged houses by Rathfon et al. (2013), which is where the data on hurricane damage is obtained.

[7] See https://www.ccappraiser.com/Downloads.aspx.

[8] The sale transaction data includes the folio number but no address. The addresses are obtained by merging the folio number in the sales data with a shapefile from the Charlotte County government portal website that has the folio and address. The shapefile can be downloaded from https://www.charlottecountyfl.gov/services/gis/Pages/Shapefile-Gallery.aspx.

[9] Initially, house sales were excluded for houses that sold for less than a nominal price of $2,000. After converting the sale price into real 1982-1984 dollars, the bottom and top 1% are also excluded from this analysis. This removed houses that sold for less than $43,266 and houses that sold for more than $545,590.

transaction data is only available monthly and it is impossible to determine if these sales occurred before or after Charley.

Data on damages and repairs are obtained from Rathfon et al. (2013), who used remote sensing imagery/satellite images to assess roof damage.[10] The images are from February 2004, March 2004, August 2004, January 2005, January 2006, January 2007, and January 2008. Rathfon et al. (2013) categorized the status of individual house parcels in Punta Gorda in each of these images as one of the following: undamaged, damaged, under construction, repaired, rebuilt,[11] demolished, or non-existent. These classifications were based on the amount of roof damage sustained by each house shown in each aerial image. Although hurricanes can cause other damage besides roof damage like window damage or landscaping damage, roof damage was the most frequent source of damage, so roof damage is an appropriate way to measure how much damage was inflicted upon houses in Punta Gorda. If the parcel was non-existent at the time of the hurricane, as evidenced by the August 2004 aerial photo, then the effect of that parcel is not included, even if a house existed and was demolished prior to the hurricane or was built after the hurricane.

Building permits from Rathfon et al. (2013) are used to supplement the house status obtained from the aerial photos. The aerial photos give incomplete information about the exact time when houses transition from damaged to repaired or rebuilt. For example, if a house was damaged in January 2005 and rebuilt in January 2006, it is impossible to know when the house changed status if only using the aerial photos. For that reason, the building permits taken out to repair damaged houses are analyzed.[12] The building permits allow rough estimations of what month damaged houses transition from damaged to repaired or rebuilt, but they do not give exact dates. To estimate when houses are repaired, the house is

[10] There are some houses that are missing data from the Rathfon et al. (2013) data. The damage status of these missing houses were explored by hand using aerial imagery purchased from Apollo Mapping and the repair status of these missing houses were determined from building permits obtained from Rathfon et al. (2013).

[11] The difference between rebuilt and repaired is that a rebuilt house was completely demolished and rebuilt, whereas repaired houses were not demolished.

[12] In particular, the building permits analyzed include permits for roof, screen enclosure, demolition, single family residence, single family additions/alterations, non-category, and concrete.

classified as under construction for two months after the permit was filed and then classified as repaired for all remaining months.[13]

The tax appraisal data for all single-family houses in Charlotte County come from the Charlotte County Property Appraisers, which can be acquired directly from the Property Appraisers, but separately from the house sale data. The tax appraisal data includes the folio number, address of the property, the square footage of housing, year the house was built, an indicator for if the house has a homestead exception, an indicator for if there is a pool, and an indicator for if the house is waterfront. These variables take on the value in the tax appraisal data from the year that the house sold in.[14]

To calculate the distances between houses in Punta Gorda, the *distCosine* function in *R* is used, which calculates distance "as the crow flies" or "great-circle-distance" method.[15]

There are 1,722 houses that sold in Punta Gorda between the start of 2001 and the end of 2009. Of these 1,722 houses, 621 did not receive any damage from Hurricane Charley and 1,651 houses were undamaged or repaired at the time of sale. In the study area, there were 6,192 parcels and 4,966 parcels had a house on it. Of these 4,966 houses, 1,692 were undamaged and the remaining 3,274 houses experienced various amounts of damage.

[13]Using building permits required analyzing each houses' building permits individually. One example of simple situation would have a damaged house take out a roof permit and the house would be classified as "under construction" for the month that permit was filed and the month after, then the house would be considered "repaired" for all remaining months. Another example of a simple situation would have a damage house take out a demolition permit, in which case the house would be categorized as "demolished" for all remaining months. If this demolished house took out a permit to build a new house then the demolished house be categorized as "under construction" for four months after the permit was filed, then the house becomes classified as "rebuilt" for all remaining months. Issues arise when multiple building permits are taken out, like two roof permits taken out within a four month time period, or when no roof permit is filed for. The author's best judgement is used in cases like this.

[14]For example, if the house sold in 2002, then the information about the square footage, year the house was built, if the house has a homestead exception, if there is a pool, and if the house is waterfront comes from the 2002 tax appraisal dataset.

[15]The distance between each house that sold and each house in this analysis is obtained. The latitude and longitude for each pair of houses is obtained and the formula used to determine the distance is $d = 6378137 arccos(sin(lat_1)sin(lat_2)+cos(lat_1)cos(lat_2)cos(|long_1-long_2|))$ where 6,378,137 meters is the radius of the earth, lat_i is the latitude of house i, and $long_i$ is the longitude of house i.

3.4 Methodology

The following hedonic price model is estimated to measure the spillover effect from hurricane damages and repairs:

$$\begin{aligned} \ln(p_{itl}) = \beta_0 + \beta_1 hurricane + \beta_2 X_{it} + \gamma_l + \tau_t + \sum_{d=0}^{0.9} \delta^{[d,d+0.1]} (damaged)^{[d,d+0.1]} \\ + \sum_{d=0}^{0.9} \theta^{[d,d+0.1]} (repaired)^{[d,d+0.1]} + \epsilon_i \end{aligned} \tag{3.1}$$

where p_{itl} is the real price for which house i sold at time t in census tract l; X_{it} includes common hedonic price model variables like the square footage, square footage squared, age of house in years, age squared, an indicator for if the house has a homestead exception, an indicator for if the house has a pool, an indicator for if the house is located on the water, and an indicator for if the house was ever damaged; γ_l consists of tract fixed effects; and τ_t consists of annual fixed effects. The dummy variable *hurricane* is equal to one if house i sold after the hurricane in August 2004. The number of houses that are damaged between d and $d + 0.1$ miles from house i at the time of sale is captured by $(damaged)^{[d,d+0.1]}$, while $(repaired)^{[d,d+0.1]}$ is the number of repaired houses between d and $d + 0.1$ miles from house i at the time of sale. Here, $[d, d + 0.1]$ are 0.1 mile intervals going up to one mile, which is consistent with the spillover literature that finds results in small distance intervals (Lin et al., 2008; Pope, 2008; Schuetz et al., 2008; Harding et al., 2009; Lin et al., 2009; Rossi-Hansberg et al., 2010). Summary statistics are presented in Table 3.2.

An example of the method used to identify spillover effects can be seen in Figure 3.3. Each house that sold between January 2001 and January 2009 is identified, like the single folio highlighted in Figure 3.3 Panel A. The number of damaged and repaired houses that are within a 0.1 mile radius from house i are counted and is captured by $(damaged)^{[0,0.1]}$ and $(repaired)^{[0,0.1]}$, respectively. Then, the number of damaged and repaired houses are counted that are between 0.1 and 0.2 miles from the sold house, captured by $(damaged)^{[0.1,0.2]}$ and

$(repaired)^{[0.1,0.2]}$, respectively. This process continues as damaged and repaired houses are counted in 0.1 mile intervals all the way to a maximum of 1.0 mile. The different distance circles can be seen in Figure 3.3 Panel B, which only illustrates distances up to 0.3 miles away from the sold house.

In a separate model, the effect of repaired houses is split into two categories: houses that repaired to a larger square footage based on tax appraisal data and all other repaired houses. Houses that increase their square footage are likely to have an increase in their house value, which could translate into positive spillovers for neighboring properties. Therefore, these spillover effects might be different than the spillover effects associated with all other repairs. The following model is used to see if the houses that repair to a larger size affect the price of nearby houses:

$$\ln(p_{itl}) = \beta_0 + \beta_1 hurricane + \beta_2 X_{it} + \gamma_l + \tau_t + \sum_{d=0}^{0.9} \delta^{[d,d+0.1]}(damaged)^{[d,d+0.1]} + \sum_{d=0}^{0.9} \mu^{[d,d+0.1]}(repaired\,larger)^{[d,d+0.1]} + \sum_{d=0}^{0.9} \sigma^{[d,d+0.1]}(repaired\,other)^{[d,d+0.1]} + \epsilon_i \tag{3.2}$$

where all variables are defined as before. Here, $(repaired\,larger)^{[d,d+0.1]}$ counts the number of houses that repaired to a larger square footage than the square footage in 2004, based on the tax appraisal square footage in 2004 and the year the house was repaired, that are d and $d + 0.1$ miles from house i. Houses that are between d and $d + 1$ miles from house i that repaired to the same square footage or a smaller square footage are counted in $(repaired\,other)^{[d,d+0.1]}$.[16]

In the regressions for both Equation 3.1 and Equation 3.2, house i that sold must not be damaged at the time of sale, meaning house i either had no roof damage or was damaged and repaired or rebuilt before selling.

[16]The square footage of neighboring houses is obtained from the tax appraisal data. If the square footage increased between 2004 and the year the house was repaired then the house is classified as being larger. Some houses have missing tax data so the square footage cannot be determined. Houses that repaired to the same square footage, smaller square footage, or to an unknown square footage are classified as being repaired to other sizes.

The model specifications outlined in Equation 3.1 and Equation 3.2 assume that the effect of each additional damaged and repaired house is linear and constant. In reality, this might not be a valid assumption. For example, if there is only one damaged house within a 0.1 mile radius from selling house i then there might not be any spillover effect. When there are several damaged houses within a 0.1 mile radius from house i then there might be a negative spillover effect, due to any of the reasons outlined above. If every house, expect for house i, is damaged within a 0.1 mile radius from house i, then these damaged houses might have a positive spillover effect as they send a signal that house i is extremely well built and that house i will soon be surrounded by new housing. In order to drop the assumption of linearity, a threshold or semi-parametric model can be explored. These models are left as future work.

Although Equation 3.1 and Equation 3.2 attempt to control for all potential variables that could affect the selling price of house i, a potential bias may exist. Fronstin and Holtmann (1994) found that houses that received substantial damage from 1992's Hurricane Andrew in Miami, Florida tended to be newer. Due to eroding building codes, these newer houses were constructed quickly, cheaply, and poorly. If similar building practices occurred in Punta Gorda, then it is possible that the houses that received damage from Hurricane Charley were worth less prior to the hurricane due to less mitigation features. This would result in a positive bias as negative spillover effects might be found when the reality is that house prices were already lower to begin with in areas with substantial damage.

To deal with this bias, Equation 3.1 can be reestimated by the following Equation 3.3:

$$\begin{aligned}\ln(p_{itl}) = \beta_0 + \beta_1 hurricane + \beta_2 X_{it} + \gamma_l + \tau_t + \sum_{d=0}^{0.9} \delta^{[d,d+0.1]}(damaged)^{[d,d+0.1]} \\ + \sum_{d=0}^{0.9} \theta^{[d,d+0.1]}(repaired)^{[d,d+0.1]} + \sum_{d=0}^{0.9} \rho^{[d,d+0.1]}(damaged\,ever)^{[d,d+0.1]} + \epsilon_i\end{aligned} \tag{3.3}$$

and Equation 3.2 can be reestimated by the following Equation 3.4:

$$
\begin{aligned}
\ln(p_{itl}) = {} & \beta_0 + \beta_1 hurricane + \beta_2 X_{it} + \gamma_l + \tau_t + \sum_{d=0}^{0.9} \delta^{[d,d+0.1]} (damaged)^{[d,d+0.1]} \\
& + \sum_{d=0}^{0.9} \mu^{[d,d+0.1]} (repaired\, larger)^{[d,d+0.1]} + \sum_{d=0}^{0.9} \sigma^{[d,d+0.1]} (repaired\, other^{[d,d+0.1]}) \\
& + \sum_{d=0}^{0.9} \rho^{[d,d+0.1]} (damaged\, ever)^{[d,d+0.1]} + \epsilon_i
\end{aligned} \quad (3.4)
$$

where everything is defined as before and $(damaged\, ever)^{[d,d+0.1]}$ is the number of houses that were damaged between d and $d + 0.1$ miles from house i. Note that $(damaged)^{[d,d+0.1]}$ is the number of houses that are damaged between d and $d + 0.1$ miles from house i at the time of sale, while $(damaged\, ever)^{[d,d+0.1]}$ counts the number of houses that were damaged, regardless of when the sale took place and if these damaged houses were repaired after they were damaged. Therefore, $(damaged\, ever)^{[d,d+0.1]}$ will be non-zero even before the hurricane. This will capture how many houses were damaged to rule out the possibility that areas with damaged houses had lower priced housing prior to the hurricane. Equation 3.3 and Equation 3.4 are not estimated here and are left as future work.

3.5 Results

Table 3.3 shows the results from Equation 3.1, the model when the spillover effect of damaged and repaired houses are analyzed. Column (1) and Column (2) only consider the effect of damaged and repaired houses. They do not include the effect of houses that are demolished or under construction. Column (3) and Column (4) consider houses that are under construction as damaged. Column (1) and Column (3) include houses that are not damaged at the time of sale, meaning they could have been damaged and repaired prior to the sale. Column (2) and Column (4) only include houses that were never damaged by Hurricane Charley.

Living next to a damaged house might result in a negative externality. Damaged houses can act as a reminder of the potential damage possible from hurricanes, which could affect risk perceptions of future buyers. Fear of future hurricanes increase risk perceptions after a hurricane which can lower the value of housing in hurricane-prone areas (Bernknopf et al., 1990; Bin and Polasky, 2004; Hallstrom and Smith, 2005; Kousky, 2010; Bin and E., 2013; Atreya and Ferreira, 2015). Damaged houses might decrease the aesthetics of the neighborhood and encourage vandalism and crime, which is often associated with foreclosed houses (Schuetz et al., 2008; Harding et al., 2009; Lin et al., 2009; Campbell et al., 2011; Ellen et al., 2013; Lacoe and Ellen, 2015). The results in Table 3.3 indicates that damaged houses have no statistically significant effect on neighboring selling prices.[17]

Repairs that occur for a damaged house could have an ambiguous spillover effect. The repairs could result in new roof, an increase in the square footage, or improved mitigation features. These improvements are likely to have a positive spillover effect. On the other hand, pressure to rebuild quickly could result in poor construction practices in the repair process. This means that houses are repaired poorly, which could decrease the value of that house and induce a negative spillover effect. In addition, if risk perceptions are high then it is possible that all houses that received damaged will suffer from reduced prices, even houses that are no longer damaged, as future residents might desire neighborhoods that experienced less damage. Results for repaired houses that are less than 0.1 miles away are negative and statistically significant in Table 3.3. In addition, repaired houses that are between 0.1 and 0.2 miles away have a positive and statistically spillover significant effect.

The variable *hurricane* and the annual fixed effects capture time and indicate a positive, but not permanent, shock to housing prices. Previous literature typically finds a temporary negative shock to houses prices after a natural disaster due to increased risk perceptions

[17]Rathfon et al. (2013) find that there were more permits in Punta Gorda to install hurricane shutters after Charley. The installation of mitigation features, like hurricane shutters or a stricter building code, has been found to increase house prices, potentially due to risk mitigation benefits or a decrease in insurance premiums (Dumm et al., 2011; Gatzlaff et al., 2018; Awondo et al., 2019). This could potentially offset the increase in risk perceptions, meaning that the negative effect of damaged houses is likely not due to increased risk perceptions. Similar to foreclosed houses, some homeowners of damaged houses might collect their insurance settlement and immediately sell their damaged property in a quick sale below market value (Zhang, 2012), driving down the price of neighboring properties.

(Bernknopf et al., 1990; Bin and Polasky, 2004; Hallstrom and Smith, 2005; Kousky, 2010; Dumm et al., 2011; Bin and E., 2013; Atreya and Ferreira, 2015; McCoy and Walsh, 2018), but few researchers have found positive shocks after a natural disaster (Liao and Panassié, 2019).

Table 3.4 shows the results for Equation 3.2, which splits the effect of repaired houses into the effect of houses that repaired larger and all other repaired houses. Therefore, Table 3.4 shows the results of our model when the spillover effect of damaged houses, houses that repaired larger, and all other repaired houses are analyzed. The format of Table 3.4 is identical to Table 3.3.

The effect of damaged houses in Table 3.4 is the same as the effect in Table 3.3 as damaged houses have no statistically significant effect on neighboring selling prices. Damaged houses that repair to a larger square footage tend to have a positive and statistically significant spillover effect when they are less than 0.1 miles away. Results for all other repaired houses that are less than 0.1 miles away are negative and statistically significant, while these repaired house that are between 0.1 and 0.2 miles away have a positive and statistically spillover significant effect.

3.6 Robustness Checks

The sign and significance on variables that results from the model specification outlined in Equation 3.2 are robust to different specifications, like including time as a monthly time tread instead of annual dummies, including Census block fixed effects instead of tract fixed effects, and capturing the spillover effects by including the percent of houses that are damaged and repaired at different distances instead of the number of these houses. In these different specifications, the results from Equation 3.1 tend to be robust or have no statistical significance and these results will not be presented here.

Time is currently included by incorporating annual dummy fixed effects. However, time can also be incorporated as a monthly time trend and a post-hurricane monthly time trend.

Incorporating time with monthly time trends creates Equation 3.5 which is the equivalent to Equation 3.2:

$$\begin{aligned}\ln(p_{itl}) &= \beta_0 + \beta_1 month + \beta_2 hurricane + \beta_3 monthafter + \beta_4 X_{it} + \gamma_l \\ &+ \sum_{d=0}^{0.9} \delta^{[d,d+0.1]}(damaged)^{[d,d+0.1]} + \sum_{d=0}^{0.9} \mu^{[d,d+0.1]}(repaired\,larger)^{[d,d+0.1]} \\ &+ \sum_{d=0}^{0.9} \sigma^{[d,d+0.1]}(repaired\,other)^{[d,d+0.1]} + \epsilon_i\end{aligned} \tag{3.5}$$

where all variables, except for $month$ and $monthafter$, are defined the same way as Equation 3.2. A monthly time trend is included in $month$, which equals 1 if the house sold in January 2001 and equals 108 if the house sold in December 2009. A monthly time trend after the hurricane is included in $monthafter$, which is equal to 1 if the house sold in September 2004, 64 if the house sold in December 2009, and 0 if the house sold prior to the hurricane in August 2004.

The results from Equation 3.5 can be seen in Table 3.5. House prices are increasing before the hurricane hit Punta Gorda in 2004, which can be seen by the positive coefficient on $month$. Since the coefficient on $hurricane$ is positive, house prices increased immediately after the hurricane. After the hurricane, the house prices were slowly reduced since the combined effect of the coefficients on $month$ and $monthafter$ is negative indicating that there is a positive and temporary shock to house prices after the hurricane. The effect of repaired houses, both houses that repaired larger and all other houses that repaired, is the same as the results from Equation 3.2, which are shown in Table 3.4. However, in this model specification, the effect of damaged houses that are 0.1 miles away have a negative and statistically significant effect.

This paper focuses on the effect of hurricanes on houses, however hurricanes can also affect businesses and downed trees can block roads, making traveling more difficult. The part of Punta Gorda that is studied here is mostly comprised of houses, so any business closures will affect all houses equally. Unfortunately, the model is unable to control for issues with road access. However, to deal with potential biases from failing to control for business and road access, Census block fixed effects can be controlled for instead of Census tract fixed effects. There are three Census tracts in the dataset but there are 61 Census blocks. Including block fixed effects would result in a model identical to the one presented in Equation 3.2 but γ_l includes dummies for each block instead of a dummy for each tract.

The results for this model specification can be seen in Table 3.6. The annual fixed effects indicate there is a temporary positive shock to house prices after the hurricane. The effect of repaired houses is robust for the two separate categories of repaired houses. However, in this model specification, the effect of damaged houses that are 0.1 miles away have a negative and statistically significant effect, which is similar to the result found in Equation 3.5 whose results are show in Table 3.5.

As an additional model specification, Equation 3.2 is adapted so that $(damaged)^{[d,d+0.1]}$ is the percent of houses that are damaged between d and $d + 0.1$ miles from house i, $(repaired\ larger)^{[d,d+0.1]}$ is the percent of houses that repaired to a larger square footage that are d and $d + 0.1$ miles from house i, and $(repaired\ other)^{[d,d+0.1]}$ is the percent of houses that are repaired but do not increase their square footage that are d and $d+1$ miles from house i.

The results for this model specification can be seen in Table 3.7. Combining the effect of the annual time dummies and the dummy hurricane results in a temporary positive shock to house prices immediately after the hurricane. Houses that repaired to a larger square footage have a positive spillover effect while all other repairs have a negative spillover effect for houses that are within 0.1 miles from the sold house.

3.7 Conclusion

Documentation of disaster damage can be difficult to measure, but is important for studying how areas recover after a disaster. In the aftermath of a natural disaster, damaged houses pose the potential for negative spillovers on nearby homes. Aerial photos (Bevington et al., 2011; Rathfon et al., 2013), on-site observations or surveys (Xiao and Van Zandt, 2012; Spader, 2015), and building permits (Stevenson et al., 2010; Rathfon et al., 2013) are useful, but a standard procedure needs to be established to measure and document damage caused by hurricanes and the recovery process. Knowing exactly what buildings received damage and when the damage was repaired can be vital to understanding how neighborhoods recover. Having different definitions for recovery can have drastically different results (Cheng et al., 2015; Horney et al., 2017), which makes it difficult to design effective policies. In addition, housing recovery can be dependent on business recovery, and vice-versa (Lenze, 1997; Belasen and Polachek, 2008, 2009; Strobl, 2011; Xiao and Van Zandt, 2012; Belasen and Dai, 2014) so it is important develop methods to also document business damage and recovery.

Peacock et al. (2018) find that reestablishing permanent housing after natural disasters is extremely important for the community to return to a state of normalcy and this paper explores the effect of hurricanes on house prices in Punta Gorda after Hurricane Charley, a Category 4 hurricane. Specifically, this paper looks into the spillover effect of hurricane-induced damages and repairs on the selling price of nearby houses.

This paper finds that Hurricane Charley acts as a temporary positive shock to the housing market, which increases the housing prices immediately after the hurricane before prices slowly return to pre-hurricane levels. Damaged houses have no statistically significant spillover effect. Houses that repaired to a larger square footage have a positive spillover effect. However, all other repaired houses have a negative spillover effect.

As future work, additional observations will be added to this analysis to confirm results. An extensive analysis of building permits can shed light on the underlying mechanisms behind the results found in this paper.

Table 3.1: List of Hurricanes that hit Florida between 1970 and 2018

Hurricane	Date	Year	Category
Agnes	June 19	1972	1
Eloise	September 23	1975	3
David	September 3	1979	2
Kate	November 21	1985	2
Floyd	October 12	1987	1
Andrew	August 24	1992	5
Erin	August 2	1995	1
Opal	October 4	1995	3
Earl	September 3	1998	2
Georges	September 20	1998	2
Irene	October 15	1999	1
Charley	August 13	2004	4
Frances	September 5	2004	2
Ivan	September 16	2004	3
Jeanne	September 26	2004	3
Katrina	August 25	2005	1
Rita	September 20	2005	1
Wilma	October 24	2005	3
Hermine	September 2	2016	1
Irma	September 10	2017	4
Michael	October 10	2018	4

This table includes hurricanes whose eye passed over the state of Florida, with the exception of Hurricane Ivan whose eye did not touch the state of Florida. Therefore this list is an incomplete list of hurricanes that might have affected various parts of Florida.

Table 3.2: Summary Statistics

Variable	N	Mean	St. Dev.	Min	Max
sale price in 1982-1984 dollars	1651	205776.6	85335.31	43266.63	545590.4
=1 if ever damaged	1651	0.62	0.48	0	1
square footage	1651	2015.19	485.61	994	4823
=1 if has Homestead Exception	1651	0.40	0.49	0	1
=1 if has pool	1651	0.43	0.50	0	1
=1 if on waterfront	1651	0.83	0.37	0	1
house age	1651	21.04	10.97	1	50
hurricane	1651	0.42	0.49	0	1
=1 if in Census tract 101	1651	0.81	0.39	0	1
=1 if in Census tract 102	1651	0.19	0.39	0	1
=1 if in Census tract 103	1651	0.001	0.03	0	1
$damaged^{[0.0,0.1]}$	1651	0.29	1.63	0	19
$damaged^{[0.1,0.2]}$	1651	0.82	4.54	0	60
$damaged^{[0.2,0.3]}$	1651	1.22	6.40	0	81
$damaged^{[0.3,0.4]}$	1651	1.68	9.54	0	128
$damaged^{[0.4,0.5]}$	1651	1.87	10.54	0	164
$damaged^{[0.5,0.6]}$	1651	2.15	12.08	0	186
$damaged^{[0.6,0.7]}$	1651	2.39	13.17	0	214
$damaged^{[0.7,0.8]}$	1651	2.48	13.54	0	170
$damaged^{[0.8,0.9]}$	1651	2.36	12.55	0	159
$damaged^{[0.9,1.0]}$	1651	2.29	11.79	0	155
$repaired\,larger^{[0.0,0.1]}$	1651	1.39	2.26	0	12
$repaired\,larger^{[0.1,0.2]}$	1651	3.70	5.25	0	22
$repaired\,larger^{[0.2,0.3]}$	1651	5.22	7.41	0	31
$repaired\,larger^{[0.3,0.4]}$	1651	6.34	8.92	0	39
$repaired\,larger^{[0.4,0.5]}$	1651	7.24	10.36	0	48
$repaired\,larger^{[0.5,0.6]}$	1651	8.21	11.76	0	55
$repaired\,larger^{[0.6,0.7]}$	1651	8.98	12.99	0	49
$repaired\,larger^{[0.7,0.8]}$	1651	9.63	14.27	0	60
$repaired\,larger^{[0.8,0.9]}$	1651	9.73	14.18	0	58
$repaired\,larger^{[0.9,1.0]}$	1651	9.72	13.71	0	53
$repaired\,other^{[0.0,0.1]}$	1651	7.40	10.34	0	42
$repaired\,other^{[0.1,0.2]}$	1651	20.02	27.78	0	110
$repaired\,other^{[0.2,0.3]}$	1651	30.36	42.09	0	167
$repaired\,other^{[0.3,0.4]}$	1651	38.40	53.11	0	197
$repaired\,other^{[0.4,0.5]}$	1651	44.49	61.06	0	226
$repaired\,other^{[0.5,0.6]}$	1651	49.67	68.27	0	250
$repaired\,other^{[0.6,0.7]}$	1651	53.75	74.54	0	282
$repaired\,other^{[0.7,0.8]}$	1651	55.37	76.29	0	286
$repaired\,other^{[0.8,0.9]}$	1651	55.74	75.84	0	276
$repaired\,other^{[0.9,1.0]}$	1651	54.55	73.15	0	280
=1 if sold in 2001	1651	0.13	0.34	0	1
=1 if sold in 2002	1651	0.18	0.39	0	1
=1 if sold in 2003	1651	0.16	0.37	0	1
=1 if sold in 2004	1651	0.11	0.32	0	1
=1 if sold in 2005	1651	0.07	0.26	0	1
=1 if sold in 2006	1651	0.06	0.24	0	1
=1 if sold in 2007	1651	0.08	0.26	0	1
=1 if sold in 2008	1651	0.10	0.30	0	1
=1 if sold in 2009	1651	0.10	0.29	0	1

Note that $repaired^{[d,d+0.1]} = repaired\,larger^{[d,d+0.1]} + repaired\,other^{[d,d+0.1]}$.

Table 3.3: Results from Equation 3.1 Estimating the Spillover Effects of Damaged and Repaired Houses on Selling Prices

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
$damaged^{[0.0,0.1]}$	-0.006	-0.024	-0.01	-0.021*
	(0.013)	(0.018)	(0.009)	(0.012)
$repaired^{[0.0,0.1]}$	-0.005***	-0.008**	-0.005***	-0.007**
	(0.002)	(0.003)	(0.002)	(0.003)
$damaged^{[0.1,0.2]}$	0.005	0.009	0.006	0.012**
	(0.005)	(0.007)	(0.004)	(0.005)
$repaired^{[0.1,0.2]}$	0.003***	0.003**	0.003***	0.003*
	(0.001)	(0.002)	(0.001)	(0.002)
$damaged^{[0.2,0.3]}$	-0.005	-0.005	-0.002	-0.003
	(0.006)	(0.008)	(0.003)	(0.005)
$repaired^{[0.2,0.3]}$	-0.000	-0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.3,0.4]}$	0.000	-0.005	0.001	-0.001
	(0.004)	(0.006)	(0.003)	(0.004)
$repaired^{[0.3,0.4]}$	0.000	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.4,0.5]}$	0.006	0.011	-0.000	0.001
	(0.004)	(0.007)	(0.003)	(0.005)
$repaired^{[0.4,0.5]}$	-0.000	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.5,0.6]}$	-0.003	-0.002	0.000	0.003
	(0.003)	(0.005)	(0.002)	(0.003)
$repaired^{[0.5,0.6]}$	0.000	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.6,0.7]}$	-0.001	-0.003	0.002	-0.001
	(0.003)	(0.005)	(0.002)	(0.003)
$repaired^{[0.6,0.7]}$	-0.001	0.000	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.7,0.8]}$	0.000	-0.001	-0.003*	-0.005*
	(0.003)	(0.004)	(0.002)	(0.002)
$repaired^{[0.7,0.8]}$	0.000	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.8,0.9]}$	-0.002	-0.002	-0.002	-0.000
	(0.003)	(0.004)	(0.002)	(0.003)
$repaired^{[0.8,0.9]}$	-0.001	-0.001	-0.001*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.9,1.0]}$	0.004	0.003	0.003**	0.002
	(0.003)	(0.003)	(0.001)	(0.002)
$repaired^{[0.9,1.0]}$	0.000	0.000	0.001	0.001

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
	(0.000)	(0.001)	(0.000)	(0.001)
=1 if ever damaged	-0.029**	-	-0.028**	-
	(0.014)		(0.014)	
square footage	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
square footage squared	-0.000***	-0.000**	-0.000***	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if has Homestead Exception	0.045***	0.062**	0.044***	0.057**
	(0.015)	(0.026)	(0.015)	(0.026)
=1 if has pool	0.127***	0.159***	0.128***	0.163***
	(0.024)	(0.037)	(0.023)	(0.037)
=1 if on waterfront	0.084***	0.054*	0.082***	0.048*
	(0.016)	(0.029)	(0.016)	(0.029)
house age	-0.019***	-0.021***	-0.019***	-0.021***
	(0.002)	(0.004)	(0.002)	(0.004)
house age squared	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if sold in 2002	0.115***	0.112***	0.114***	0.112***
	(0.021)	(0.038)	(0.021)	(0.038)
=1 if sold in 2003	0.218***	0.217***	0.218***	0.220***
	(0.024)	(0.043)	(0.024)	(0.043)
=1 if sold in 2004	0.206***	0.166***	0.205***	0.167***
	(0.033)	(0.052)	(0.033)	(0.052)
=1 if sold in 2005	0.625***	0.514***	0.486***	0.473***
	(0.143)	(0.183)	(0.107)	(0.137)
=1 if sold in 2006	0.531***	0.384*	0.386***	0.347**
	(0.159)	(0.205)	(0.119)	(0.155)
=1 if sold in 2007	0.332**	0.186	0.184	0.147
	(0.160)	(0.208)	(0.120)	(0.158)
=1 if sold in 2008	0.120	-0.025	-0.028	-0.060
	(0.159)	(0.206)	(0.119)	(0.155)
=1 if sold in 2009	0.010	-0.162	-0.138	-0.197
	(0.160)	(0.208)	(0.120)	(0.157)
hurricane	-0.097	0.090	0.043	0.117
	(0.158)	(0.207)	(0.115)	(0.115)
=1 if in Census tract 101	0.602***	-	0.602***	-
	(0.172)		(0.172)	
=1 if in Census tract 102	0.636***	-	0.638***	-
	(0.173)		(0.172)	
Constant	10.465***	11.072***	10.471***	11.121***
	(0.189)	(0.149)	(0.189)	(0.149)
Observations	1,651	621	1,651	621

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
R-Squared	0.670	0.676	0.671	0.681

Notes: Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.4: Results from Equation 3.2 Estimating the Spillover Effects of Damaged Houses, Houses that Repaired Larger, and All Other Repaired Houses on Selling Prices

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
$damaged^{[0.0,0.1]}$	-0.007	-0.025	-0.011	-0.021*
	(0.013)	(0.018)	(0.009)	(0.012)
$repaired\ larger^{[0.0,0.1]}$	0.011**	0.022***	0.011**	0.022***
	(0.005)	(0.008)	(0.005)	(0.008)
$repaired\ other^{[0.0,0.1]}$	-0.006***	-0.015***	-0.006***	-0.014***
	(0.002)	(0.004)	(0.002)	(0.004)
$damaged^{[0.1,0.2]}$	0.006	0.013**	0.007*	0.013***
	(0.005)	(0.007)	(0.004)	(0.005)
$repaired\ larger^{[0.1,0.2]}$	0.001	0.001	0.002	0.002
	(0.003)	(0.005)	(0.003)	(0.005)
$repaired\ other^{[0.1,0.2]}$	0.003***	0.004**	0.003***	0.004**
	(0.001)	(0.002)	(0.001)	(0.002)
$damaged^{[0.2,0.3]}$	-0.006	-0.010	-0.003	-0.004
	(0.006)	(0.008)	(0.003)	(0.005)
$repaired\ larger^{[0.2,0.3]}$	0.002	-0.002	0.002	-0.002
	(0.002)	(0.004)	(0.002)	(0.004)
$repaired\ other^{[0.2,0.3]}$	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.3,0.4]}$	-0.000	-0.007	0.002	-0.001
	(0.004)	(0.005)	(0.003)	(0.004)
$repaired\ larger^{[0.3,0.4]}$	-0.001	-0.007*	-0.001	-0.006
	(0.002)	(0.004)	(0.002)	(0.004)
$repaired\ other^{[0.3,0.4]}$	0.000	0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.4,0.5]}$	0.007	0.014**	-0.001	0.001
	(0.004)	(0.007)	(0.003)	(0.005)
$repaired\ larger^{[0.4,0.5]}$	0.004*	0.011***	0.003	0.010***
	(0.002)	(0.004)	(0.002)	(0.004)
$repaired\ other^{[0.4,0.5]}$	-0.001	-0.002	-0.001*	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.5,0.6]}$	-0.004	-0.001	0.000	0.003
	(0.003)	(0.005)	(0.002)	(0.003)
$repaired\ larger^{[0.5,0.6]}$	0.002	0.000	0.002	0.001
	(0.002)	(0.003)	(0.002)	(0.003)
$repaired\ other^{[0.5,0.6]}$	-0.000	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.6,0.7]}$	-0.001	-0.005	0.002	-0.001
	(0.003)	(0.005)	(0.002)	(0.003)
$repaired\ larger^{[0.6,0.7]}$	0.002	0.000	0.001	0.000

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.6,0.7]}$	-0.001	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.7,0.8]}$	0.001	0.001	-0.003*	-0.004*
	(0.003)	(0.004)	(0.002)	(0.002)
repaired larger$^{[0.7,0.8]}$	0.001	0.007**	0.000	0.006**
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.7,0.8]}$	0.000	-0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.8,0.9]}$	-0.002	-0.003	-0.002	-0.000
	(0.003)	(0.004)	(0.002)	(0.003)
repaired larger$^{[0.8,0.9]}$	0.001	-0.000	0.001	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.8,0.9]}$	-0.001	-0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.9,1.0]}$	0.004	0.003	0.003**	0.002
	(0.003)	(0.003)	(0.001)	(0.002)
repaired larger$^{[0.9,1.0]}$	0.004**	0.007**	0.004**	0.007**
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.9,1.0]}$	-0.000	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
=1 if ever damaged	-0.030**	-	-0.030**	-
	(0.014)		(0.014)	
square footage	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
square footage squared	-0.000***	-0.000**	-0.000***	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if has Homestead Exception	0.044***	0.058**	0.043***	0.053**
	(0.015)	(0.026)	(0.015)	(0.026)
=1 if has pool	0.120***	0.152***	0.121***	0.155***
	(0.024)	(0.036)	(0.024)	(0.036)
=1 if on waterfront	0.083***	0.056**	0.080***	0.048*
	(0.016)	(0.028)	(0.016)	(0.029)
house age	-0.019***	-0.023***	-0.019***	-0.022***
	(0.002)	(0.004)	(0.002)	(0.004)
house age squared	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if sold in 2002	0.115***	0.115***	0.115***	0.115***
	(0.021)	(0.038)	(0.021)	(0.037)
=1 if sold in 2003	0.219***	0.222***	0.219***	0.225***
	(0.024)	(0.042)	(0.024)	(0.042)
=1 if sold in 2004	0.213***	0.177***	0.212***	0.177***

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
	(0.033)	(0.052)	(0.033)	(0.051)
=1 if sold in 2005	0.642***	0.514***	0.499***	0.508***
	(0.143)	(0.179)	(0.107)	(0.135)
=1 if sold in 2006	0.514***	0.351*	0.368***	0.355**
	(0.158)	(0.201)	(0.119)	(0.153)
=1 if sold in 2007	0.309*	0.124	0.161	0.128
	(0.160)	(0.204)	(0.120)	(0.156)
=1 if sold in 2008	0.094	-0.063	-0.053	-0.057
	(0.159)	(0.202)	(0.119)	(0.153)
=1 if sold in 2009	-0.020	-0.211	-0.167	-0.205
	(0.160)	(0.204)	(0.120)	(0.155)
hurricane	-0.112	0.137	0.026	0.108
	(0.158)	(0.207)	(0.115)	(0.147)
=1 if in Census tract 101	0.607***	-	0.607***	-
	(0.171)		(0.171)	
=1 if in Census tract 102	0.649***	-	0.652***	-
	(0.172)		(0.171)	
Constant	10.480***	11.135***	10.487***	11.178***
	(0.189)	(0.149)	(0.188)	(0.150)
Observations	1,651	621	1,651	621
R-Squared	0.675	0.698	0.676	0.700

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Results from Equation 3.5 Estimating the Spillover Effects of Damaged Houses, Houses that Repaired Larger, and All Other Repaired Houses on Selling Prices with a Monthly Time Trend

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
$damaged^{[0.0,0.1]}$	-0.024**	-0.037**	-0.026***	-0.038***
	(0.012)	(0.017)	(0.008)	(0.011)
$repaired\ larger^{[0.0,0.1]}$	0.013***	0.025***	0.013***	0.026***
	(0.005)	(0.008)	(0.005)	(0.008)
$repaired\ other^{[0.0,0.1]}$	-0.006***	-0.016***	-0.006***	-0.014***
	(0.002)	(0.004)	(0.002)	(0.004)
$damaged^{[0.1,0.2]}$	0.005	0.013**	0.007*	0.014***
	(0.005)	(0.007)	(0.004)	(0.005)
$repaired\ larger^{[0.1,0.2]}$	-0.000	-0.001	0.001	0.001
	(0.003)	(0.005)	(0.003)	(0.005)
$repaired\ other^{[0.1,0.2]}$	0.002**	0.004**	0.002**	0.004**
	(0.001)	(0.002)	(0.001)	(0.002)
$damaged^{[0.2,0.3]}$	-0.008	-0.013	-0.002	-0.004
	(0.006)	(0.008)	(0.003)	(0.005)
$repaired\ larger^{[0.2,0.3]}$	0.001	-0.001	0.001	-0.001
	(0.002)	(0.004)	(0.002)	(0.004)
$repaired\ other^{[0.2,0.3]}$	-0.000	-0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.3,0.4]}$	-0.001	-0.009*	0.003	0.002
	(0.004)	(0.005)	(0.003)	(0.004)
$repaired\ larger^{[0.3,0.4]}$	-0.001	-0.007*	-0.001	-0.006
	(0.002)	(0.004)	(0.002)	(0.004)
$repaired\ other^{[0.3,0.4]}$	0.000	0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.4,0.5]}$	0.008*	0.016**	-0.002	-0.003
	(0.004)	(0.007)	(0.003)	(0.005)
$repaired\ larger^{[0.4,0.5]}$	0.004**	0.012***	0.004*	0.010***
	(0.002)	(0.004)	(0.002)	(0.003)
$repaired\ other^{[0.4,0.5]}$	-0.001	-0.002	-0.001*	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.5,0.6]}$	-0.002	0.000	0.002	0.007**
	(0.003)	(0.005)	(0.002)	(0.003)
$repaired\ larger^{[0.5,0.6]}$	0.000	-0.001	0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.003)
$repaired\ other^{[0.5,0.6]}$	0.000	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.6,0.7]}$	-0.002	-0.005	0.002	-0.002
	(0.003)	(0.005)	(0.002)	(0.003)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
repaired larger$^{[0.6,0.7]}$	0.002	-0.000	0.001	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.6,0.7]}$	-0.001	-0.000	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.7,0.8]}$	0.001	0.001	-0.004**	-0.005**
	(0.003)	(0.004)	(0.002)	(0.002)
repaired larger$^{[0.7,0.8]}$	0.000	0.006**	-0.000	0.005*
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.7,0.8]}$	0.000	-0.001	0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.8,0.9]}$	-0.003	-0.004	-0.001	0.000
	(0.003)	(0.003)	(0.002)	(0.003)
repaired larger$^{[0.8,0.9]}$	0.001	0.000	0.001	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.8,0.9]}$	-0.001	-0.001	-0.001*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.9,1.0]}$	0.002	0.002	0.003**	0.002
	(0.003)	(0.003)	(0.001)	(0.002)
repaired larger$^{[0.9,1.0]}$	0.004**	0.007**	0.004**	0.006**
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.9,1.0]}$	-0.000	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
=1 if ever damaged	-0.028**	-	-0.027*	-
	(0.014)		(0.014)	
square footage	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
square footage squared	-0.000***	-0.000***	-0.000***	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if has Homestead Exception	0.043***	0.063***	0.043***	0.055**
	(0.015)	(0.024)	(0.015)	(0.024)
=1 if has pool	0.085***	0.103***	0.087***	0.106***
	(0.019)	(0.031)	(0.019)	(0.031)
=1 if on waterfront	0.081***	0.052*	0.079***	0.047
	(0.016)	(0.028)	(0.016)	(0.029)
house age	-0.018***	-0.022***	-0.018***	-0.021***
	(0.002)	(0.004)	(0.002)	(0.004)
house age squared	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
month	0.008***	0.007***	0.008***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
hurricane	0.470***	0.613***	0.437***	0.521***
	(0.052)	(0.088)	(0.053)	(0.088)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
monthafter	-0.023***	-0.023***	-0.023***	-0.023***
	(0.001)	(0.002)	(0.001)	(0.002)
=1 if in Census tract 101	0.626***	-	0.627***	-
	(0.171)		(0.171)	
=1 if in Census tract 102	0.669***	-	0.673***	-
	(0.172)		(0.171)	
Constant	10.372***	11.042***	10.383***	11.087***
	(0.189)	(0.150)	(0.189)	(0.151)
Observations	1,651	621	1,651	621
R-Squared	0.674	0.695	0.675	0.694

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Results from Equation 3.2 Estimating the Spillover Effects of Damaged Houses, Houses that Repaired Larger, and All Other Repaired Houses on Selling Prices with Block Fixed Effects

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
$damaged^{[0.0,0.1]}$	-0.013	-0.025	-0.014*	-0.021*
	(0.012)	(0.018)	(0.008)	(0.012)
$repaired\ larger^{[0.0,0.1]}$	0.010**	0.022***	0.011**	0.022***
	(0.004)	(0.008)	(0.004)	(0.008)
$repaired\ other^{[0.0,0.1]}$	-0.003*	-0.015***	-0.003*	-0.014***
	(0.002)	(0.004)	(0.002)	(0.004)
$damaged^{[0.1,0.2]}$	0.005	0.013**	0.007*	0.013***
	(0.005)	(0.007)	(0.003)	(0.005)
$repaired\ larger^{[0.1,0.2]}$	-0.002	0.001	-0.002	0.002
	(0.003)	(0.005)	(0.003)	(0.005)
$repaired\ other^{[0.1,0.2]}$	0.003***	0.004**	0.003***	0.004**
	(0.001)	(0.002)	(0.001)	(0.002)
$damaged^{[0.2,0.3]}$	-0.002	-0.010	-0.002	-0.004
	(0.005)	(0.008)	(0.003)	(0.005)
$repaired\ larger^{[0.2,0.3]}$	-0.000	-0.002	-0.000	-0.002
	(0.002)	(0.004)	(0.002)	(0.004)
$repaired\ other^{[0.2,0.3]}$	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.3,0.4]}$	-0.001	-0.007	0.001	-0.001
	(0.004)	(0.005)	(0.002)	(0.004)
$repaired\ larger^{[0.3,0.4]}$	-0.002	-0.007*	-0.002	-0.006
	(0.002)	(0.004)	(0.002)	(0.004)
$repaired\ other^{[0.3,0.4]}$	0.000	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.4,0.5]}$	0.006	0.014**	-0.001	0.001
	(0.004)	(0.007)	(0.002)	(0.005)
$repaired\ larger^{[0.4,0.5]}$	0.003	0.011***	0.002	0.010***
	(0.002)	(0.004)	(0.002)	(0.004)
$repaired\ other^{[0.4,0.5]}$	-0.001	-0.002	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.5,0.6]}$	-0.003	-0.001	0.001	0.003
	(0.003)	(0.005)	(0.002)	(0.003)
$repaired\ larger^{[0.5,0.6]}$	0.000	0.000	0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.003)
$repaired\ other^{[0.5,0.6]}$	-0.000	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$damaged^{[0.6,0.7]}$	-0.001	-0.005	0.002	-0.001
	(0.003)	(0.005)	(0.002)	(0.003)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
repaired larger$^{[0.6,0.7]}$	0.000	0.000	0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.6,0.7]}$	-0.000	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.7,0.8]}$	0.000	0.001	-0.003*	-0.004*
	(0.003)	(0.004)	(0.002)	(0.002)
repaired larger$^{[0.7,0.8]}$	0.002	0.007**	0.002	0.006**
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.7,0.8]}$	0.001	-0.001	0.001*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.8,0.9]}$	-0.003	-0.003	-0.002	-0.000
	(0.003)	(0.004)	(0.002)	(0.003)
repaired larger$^{[0.8,0.9]}$	-0.000	-0.000	-0.001	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.8,0.9]}$	-0.001*	-0.001	-0.001**	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
damaged$^{[0.9,1.0]}$	0.005**	0.003	0.004***	0.002
	(0.002)	(0.003)	(0.001)	(0.002)
repaired larger$^{[0.9,1.0]}$	0.004**	0.007**	0.004**	0.007**
	(0.002)	(0.003)	(0.002)	(0.003)
repaired other$^{[0.9,1.0]}$	-0.000	-0.001	-0.000	-0.000
	(0.000)	(0.001)	(0.000)	(0.001)
=1 if ever damaged	-0.055	-	-0.052	-
	(0.047)		(0.047)	
square footage	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
square footage squared	-0.000***	-0.000**	-0.000***	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if has Homestead Exception	0.050***	0.058**	0.050***	0.053**
	(0.014)	(0.026)	(0.014)	(0.026)
=1 if has pool	0.103***	0.152***	0.105***	0.155***
	(0.022)	(0.036)	(0.022)	(0.036)
=1 if on waterfront	0.033**	0.056**	0.031*	0.048*
	(0.016)	(0.028)	(0.016)	(0.029)
house age	-0.021***	-0.023***	-0.021***	-0.022***
	(0.002)	(0.004)	(0.002)	(0.004)
house age squared	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if sold in 2002	0.118***	0.115***	0.118***	0.115***
	(0.020)	(0.038)	(0.020)	(0.037)
=1 if sold in 2003	0.219***	0.222***	0.220***	0.225***
	(0.023)	(0.042)	(0.023)	(0.042)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
=1 if sold in 2004	0.238***	0.177***	0.237***	0.177***
	(0.031)	(0.052)	(0.031)	(0.051)
=1 if sold in 2005	0.643***	0.514***	0.502***	0.508***
	(0.134)	(0.179)	(0.099)	(0.135)
=1 if sold in 2006	0.511***	0.351*	0.363***	0.355**
	(0.148)	(0.201)	(0.111)	(0.153)
=1 if sold in 2007	0.324*	0.124	0.174	0.128
	(0.149)	(0.204)	(0.112)	(0.156)
=1 if sold in 2008	0.118	-0.063	-0.031	-0.057
	(0.149)	(0.202)	(0.111)	(0.153)
=1 if sold in 2009	-0.005	-0.211	-0.154	-0.205
	(0.149)	(0.204)	(0.112)	(0.155)
hurricane	-0.0.096	0.137	0.045	0.108
	(0.150)	(0.207)	(0.108)	(0.147)
Constant	11.279***	11.135***	11.290***	11.178***
	(0.086)	(0.149)	(0.086)	(0.150)
Observations 1,651	621	1,651	621	
R-Squared	0.675	0.698	0.676	0.700

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
As there are 61 blocks, the results for the 61 block dummy fixed effects have been suppressed.

Table 3.7: Results from Equation 3.2 Estimating the Spillover Effects of the Percent of Damaged Houses, Houses that Repaired Larger, and All Other Repaired Houses on Selling Prices

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
$damaged^{[0.0,0.1]}$	-0.002	-0.005	0.003	-0.001
	(0.005)	(0.007)	(0.003)	(0.005)
$repaired\ larger^{[0.0,0.1]}$	0.005***	0.010***	0.005***	0.009***
	(0.002)	(0.003)	(0.002)	(0.003)
$repaired\ other^{[0.0,0.1]}$	-0.002*	-0.004**	-0.002*	-0.004**
	(0.001)	(0.002)	(0.001)	(0.002)
$damaged^{[0.1,0.2]}$	-0.001	-0.001	-0.001	0.003
	(0.006)	(0.007)	(0.005)	(0.007)
$repaired\ larger^{[0.1,0.2]}$	-0.001	-0.000	-0.001	0.000
	(0.003)	(0.004)	(0.003)	(0.004)
$repaired\ other^{[0.1,0.2]}$	0.002	0.001	0.002	0.001
	(0.001)	(0.002)	(0.001)	(0.002)
$damaged^{[0.2,0.3]}$	-0.011	-0.011	-0.002	0.002
	(0.007)	(0.010)	(0.005)	(0.007)
$repaired\ larger^{[0.2,0.3]}$	0.004	0.001	0.004	-0.001
	(0.003)	(0.005)	(0.003)	(0.005)
$repaired\ other^{[0.2,0.3]}$	0.001	-0.001	0.001	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
$damaged^{[0.3,0.4]}$	-0.016**	-0.021**	-0.015**	-0.019**
	(0.008)	(0.010)	(0.006)	(0.008)
$repaired\ larger^{[0.3,0.4]}$	-0.006	-0.015**	-0.006	-0.017**
	(0.004)	(0.007)	(0.005)	(0.008)
$repaired\ other^{[0.3,0.4]}$	-0.002	0.002	-0.002	0.002
	(0.002)	(0.003)	(0.002)	(0.003)
$damaged^{[0.4,0.5]}$	0.007	0.004	-0.002	-0.016*
	(0.009)	(0.012)	(0.006)	(0.008)
$repaired\ larger^{[0.4,0.5]}$	0.006	0.011	0.005	0.007
	(0.004)	(0.007)	(0.005)	(0.007)
$repaired\ other^{[0.4,0.5]}$	-0.004**	-0.007**	-0.004**	-0.008***
	(0.002)	(0.003)	(0.002)	(0.003)
$damaged^{[0.5,0.6]}$	0.005	0.022*	0.018***	0.039***
	(0.009)	(0.012)	(0.006)	(0.009)
$repaired\ larger^{[0.5,0.6]}$	-0.003	0.004	-0.003	-0.002
	(0.005)	(0.008)	(0.005)	(0.008)
$repaired\ other^{[0.5,0.6]}$	0.000	0.005	0.001	0.006*
	(0.002)	(0.003)	(0.002)	(0.003)
$damaged^{[0.6,0.7]}$	-0.004	-0.017	-0.006	-0.025***
	(0.008)	(0.011)	(0.006)	(0.008)

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
repaired larger$^{[0.6,0.7]}$	-0.001	-0.004	-0.002	-0.009
	(0.005)	(0.007)	(0.005)	(0.008)
repaired other$^{[0.6,0.7]}$	-0.001	-0.002	-0.001	-0.002
	(0.002)	(0.003)	(0.002)	(0.003)
damaged$^{[0.7,0.8]}$	0.007	0.007	-0.002	-0.002
	(0.009)	(0.011)	(0.008)	(0.011)
repaired larger$^{[0.7,0.8]}$	-0.008*	0.002	-0.009*	-0.002
	(0.005)	(0.008)	(0.005)	(0.009)
repaired other$^{[0.7,0.8]}$	0.001	-0.003	0.001	-0.003
	(0.002)	(0.003)	(0.002)	(0.003)
damaged$^{[0.8,0.9]}$	-0.011	-0.022*	-0.005	-0.007
	(0.009)	(0.012)	(0.007)	(0.009)
repaired larger$^{[0.8,0.9]}$	-0.003	-0.009	-0.004	-0.012
	(0.005)	(0.008)	(0.005)	(0.008)
repaired other$^{[0.8,0.9]}$	-0.002	-0.004	-0.002	-0.004
	(0.002)	(0.004)	(0.002)	(0.004)
damaged$^{[0.9,1.0]}$	0.012	0.030***	0.008*	0.015**
	(0.008)	(0.011)	(0.005)	(0.006)
repaired larger$^{[0.9,1.0]}$	0.007	0.008	0.007	0.005
	(0.005)	(0.008)	(0.005)	(0.008)
repaired other$^{[0.9,1.0]}$	0.001	0.005*	0.001	0.006**
	(0.002)	(0.003)	(0.002)	(0.003)
=1 if ever damaged	-0.026*	-	-0.024*	-
	(0.014)		(0.014)	
square footage	0.001***	0.001***	0.001***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
square footage squared	-0.000***	-0.000	-0.000***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if has Homestead Exception	0.046***	0.065**	0.046***	0.065**
	(0.015)	(0.026)	(0.015)	(0.026)
=1 if has pool	0.122***	0.148***	0.124***	0.152***
	(0.023)	(0.035)	(0.023)	(0.035)
=1 if on waterfront	0.087***	0.050*	0.084***	0.043
	(0.016)	(0.027)	(0.016)	(0.027)
house age	-0.020***	-0.023***	-0.020***	-0.023***
	(0.002)	(0.004)	(0.002)	(0.004)
house age squared	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
=1 if sold in 2002	0.114***	0.115***	0.115***	0.115***
	(0.021)	(0.037)	(0.021)	(0.037)
=1 if sold in 2003	0.218***	0.217***	0.218***	0.217***
	(0.024)	(0.041)	(0.024)	(0.041)

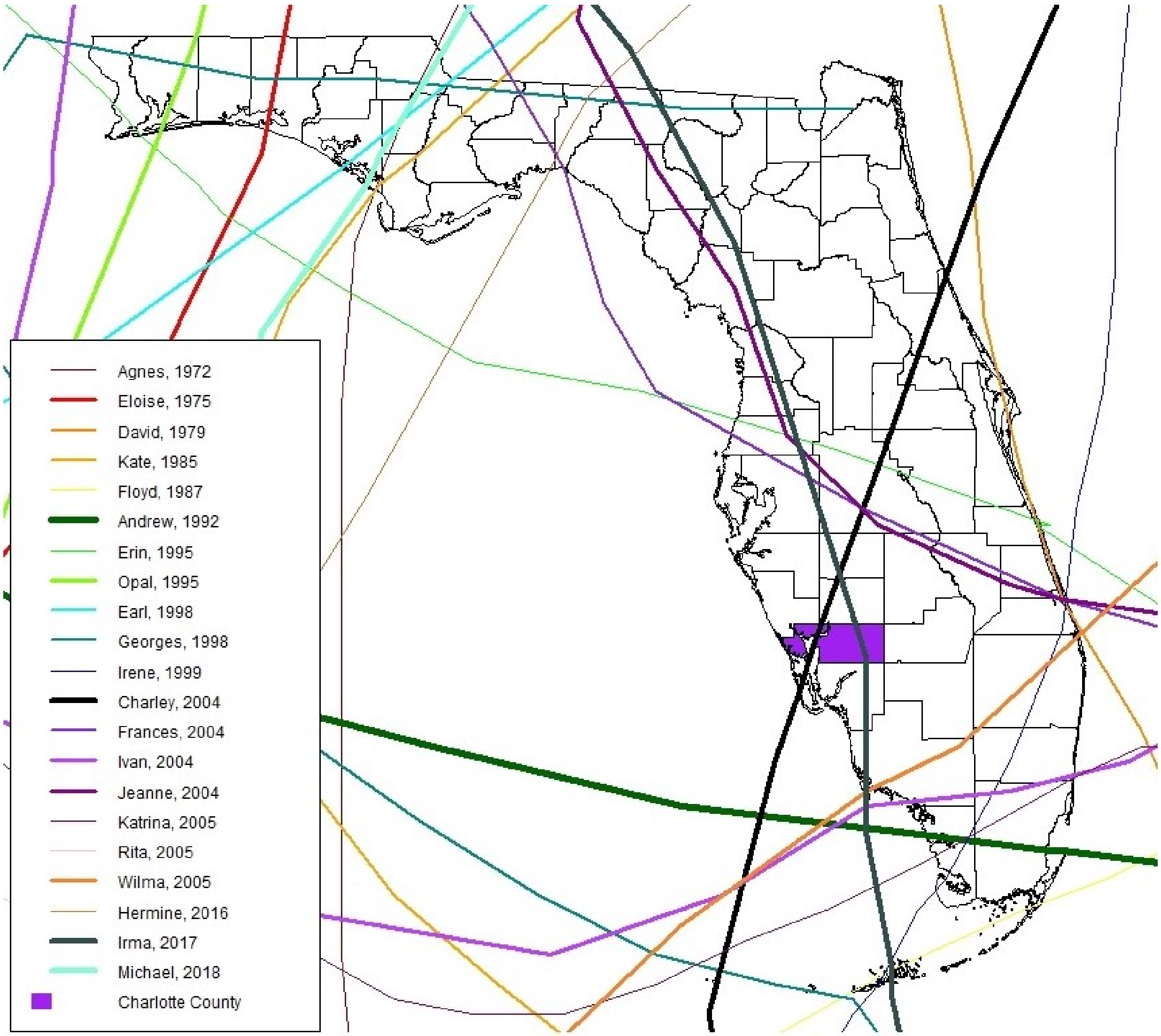
Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Previous table continued

	Baseline Regression		Under Construction counted as Damaged	
	Not Damaged	Never Damaged	Not Damaged	Never Damaged
	ln(price)	ln(price)	ln(price)	ln(price)
	(1)	(2)	(3)	(4)
=1 if sold in 2004	0.209***	0.174***	0.208***	0.170***
	(0.033)	(0.051)	(0.033)	(0.051)
=1 if sold in 2005	0.251	0.209	0.440***	0.250
	(0.158)	(0.206)	(0.105)	(0.154)
=1 if sold in 2006	0.129	0.0.085	0.333***	0.150
	(0.171)	(0.225)	(0.120)	(0.175)
=1 if sold in 2007	-0.081	-0.164	0.124	-0.104
	(0.172)	(0.228)	(0.120)	(0.177)
=1 if sold in 2008	-0.291	-0.332	-0.084	-0.262
	(0.171)	(0.226)	(0.120)	(0.177)
=1 if sold in 2009	-0.410**	-0.492**	-0.203*	-0.424**
	(0.172)	(0.228)	(0.120)	(0.178)
hurricane	-0507**	0.515*	0.333*	0.724**
	(0.240)	(0.305)	(0.192)	(0.298)
=1 if in Census	0.616***	-	0.616***	-
tract 101	(0.170)		(0.170)	
=1 if in Census	0.651***	-	0.652***	-
tract 102	(0.171)		(0.171)	
Constant	10.521***	11.264***	10.525***	11.300***
	(0.188)	(0.147)	(0.187)	(0.146)
Observations	1,651	621	1,651	621
R-Squared	0.679	0.706	0.679	0.711

Notes: Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

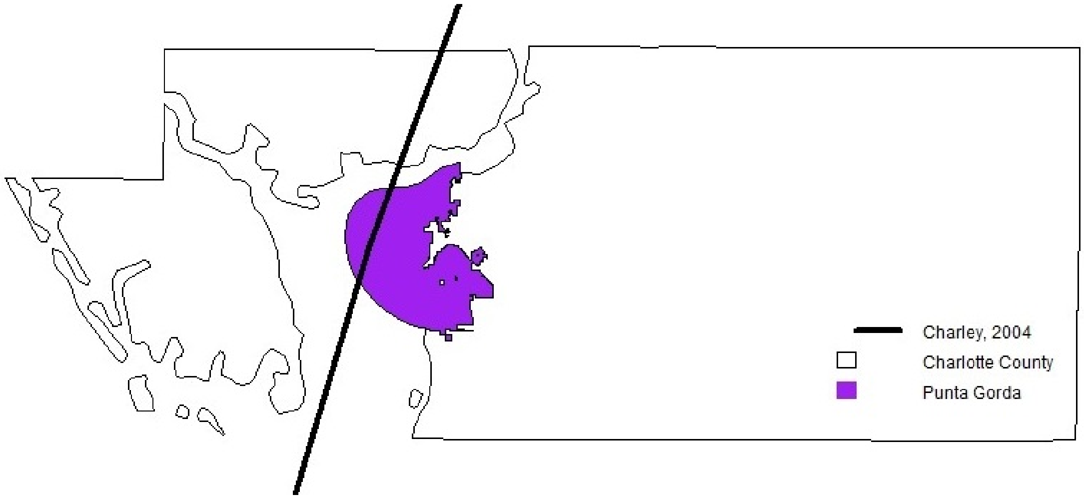
Figure 3.1: Eye Path of Hurricanes that hit Florida between 1970 and 2018



Similar to Table 3.1, this figure includes hurricanes whose eye passed over the state of Florida, with the exception of Hurricane Ivan whose eye did not touch the state of Florida. Therefore this list is an incomplete list of hurricanes that might have affected various parts of Florida.

Figure 3.2: Hurricane Charley's Path through Charlotte County and Punta Gorda

Panel A: Hurricane Charley's Path through Charlotte County



Panel B: Hurricane Charley's Path through Punta Gorda

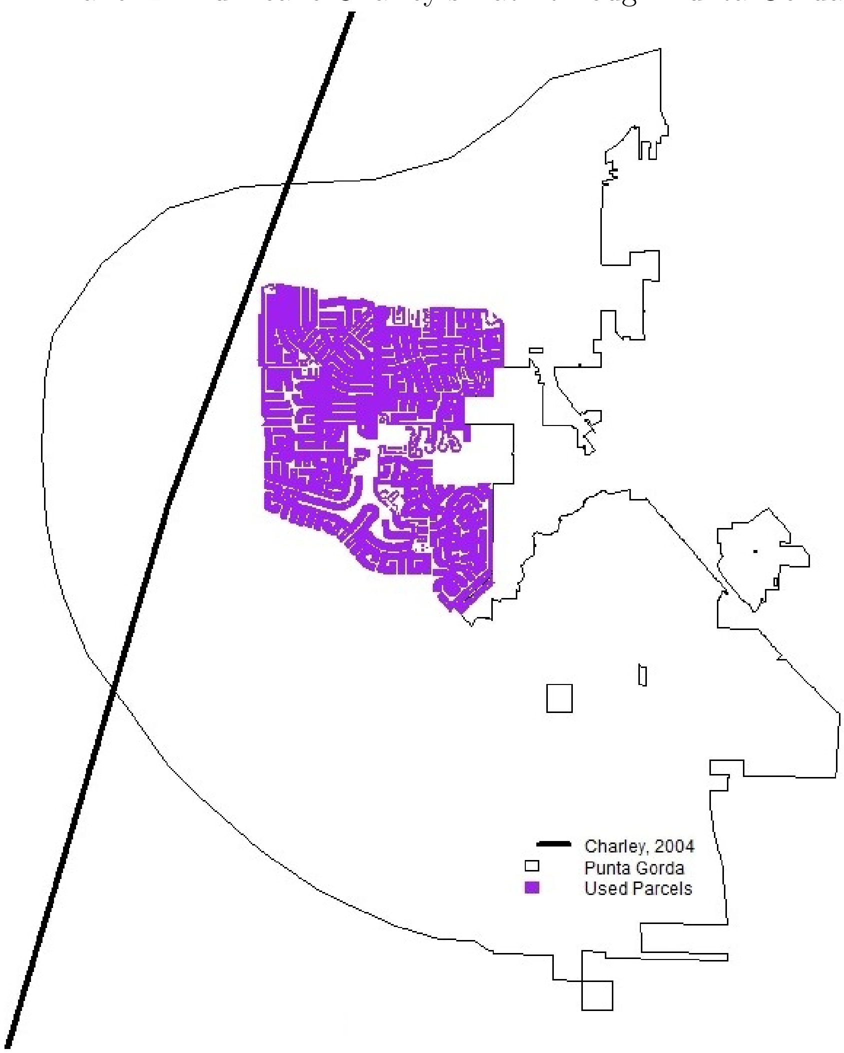
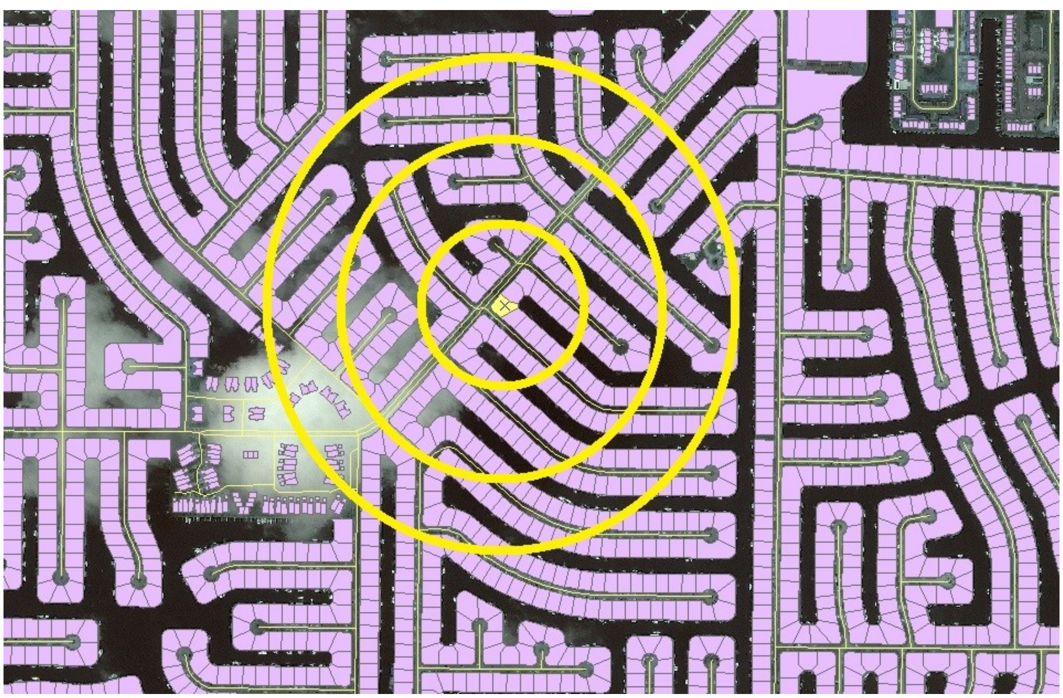


Figure 3.3: Example of Model Specification

Panel A: House i that Sold

Panel B: Distances from House i



REFERENCES

Alm, J., Leguizamon, J.S., Leguizamon, S., 2014. Revisiting the Income Tax Effects of Legalizing Same-Sex Marriages. Journal of Policy Analysis and Management 33, 263–289.

Anacker, K.B., Morrow-Jones, H.A., 2005. Neighborhood Factors Associated with Same Sex Households in U.S. Cities. Urban Geography 26, 385–409.

Atreya, A., Ferreira, S., 2015. Seeing is Believing? Evidence from Property Prices in Inundated Areas. Risk Analysis 35, 828–848.

Autor, D.H., Palmer, C.J., Pathak, P.A., 2014. Housing Market Spillovers: Evidence from the End of Rent Control in Cambridge, Massachusetts. Journal of Political Economy 122.

Autor, D.H., Palmer, C.J., Pathak, P.A., 2017. Gentrification and the Amenity Value of Crime Reductions: Evidence from Rent Deregulation. NBER Working Paper No. 23914 .

Awondo, S., Hollans, H., Powell, L., Wade, C., 2019. Estimating Effects of Wind Loss Mitigation on Home Value. Technical Report. SSRN.

Badgett, M.V.L., 2001. Money, Myths and Change: The Economic Lives of Lesbians and Gay Men. University of Chicago Press, Chicago, IL.

Becker, G.S., Murphy, K.M., 2000. Social Economics: Market Behavior in a Social Environment. Harvard University Press .

Belasen, A.R., Dai, C., 2014. When oceans attack: assessing the impact of hurricanes on localized taxable sales. Annals of Regional Science 52, 325–342.

Belasen, A.R., Polachek, S.W., 2008. How Hurricanes Affect Wages and Employment in Local Labor Markets. American Economic Review: Papers Proceedings 98, 49–53.

Belasen, A.R., Polachek, S.W., 2009. How Disasters Affect Local Labor Markets. Journal of Human Resources 44, 251–276.

Berggren, N., Elinder, M., 2012. Is tolerance good or bad for growth? Public Choice 150, 283–308.

Bernknopf, R.L., Brookshire, D.S., Thayer, M.A., 1990. Earthquake and Volcano Hazard Notices: An Economic Evaluation of Changes in Risk Perceptions. Journal of Environmental Economics and Management 18, 35–49.

Bevington, J.S., Hill, A.A., Davidson, R.A., Chang, S.E., Vicini, A., Adams, B.J., Eguchi, R.T., 2011. Measuring, Monitoring and Evaluating Post-Disaster Recovery: A Key Element in Understanding Community Resilience. Technical Report.

Bin, O., E., L.C., 2013. Changes in implicit flood risk premiums: Empirical evidence from the housing market. Journal of Environmental Economics and Management 65, 361–376.

Bin, O., Polasky, S., 2004. Effects of Flood Hazards on Property Values: Evidence Before and After Hurricane Floyd. Land Economics 80, 490–550.

Black, D., Gates, G., Sanders, S., Taylor, L., 2000. Demographics of the Gay and Lesbian Population in the United States: Evidence from Available Systematic Data Sources. Demography 47, 139–154.

Black, D., Gates, G., Sanders, S., Taylor, L., 2002. Why Do Gay Men Live in San Francisco? Journal of Urban Economics 51, 54–76.

Black, D., Gates, G., Sanders, S., Taylor, L., 2007a. The Measurement of Same-Sex Unmarried Partner Couples in the 2000 U.S. Census. Working paper. California Center for Population Research, University of California, Los Angles. .

Black, D., Sanders, S.G., Taylor, L.J., 2007b. The Economics of Lesbian and Gay Families. Journal of Economic Perspectives 21, 53–70.

Black, D.A., Makar, H.R., Sanders, S.G., Taylor, L.J., 2003. The Earnings Effects of Sexual Orientation. Industrial and Labor Relations Review 56, 449–469.

Blandford, J.M., 2003. The Nexus of Sexual Orientation and Gender in the Determination of Earnings. Industrial and Labor Relations Review 56, 662–642.

Bond, E.W., Coulson, N.E., 1989. Externalities, Filtering, and Neighborhood Change. Journal of Urban Economics 26, 231–249.

Bostic, R.W., Martin, R.W., 2003. Black Home-owners as a Gentrifying Force? Neighbourhood Dynamics in the Context of Minority Home-ownership. Urban Studies 40, 2427–2449.

Boustan, L.P., Kahn, M.E., Rhode, P.W., Yanguas, M.L., 2017. The Effect of Natural Disasters on Economic Activity in US Counties: A Century of Data. NBER Working Paper 23410 .

Boustan, L.P., Margo, R.A., Miller, M.M., Reeves, J.M., Steil, J.P., 2019. Does Condominium Development Lead to Gentrification? NBER Working Paper No. 26170 .

Brasington, D.M., 2007. Private schools and willingness to pay for public schooling. Education Finance and Policy 2, 152–174.

Brummet, Q., Reed, D., 2019. Gentrification and the location and well-being of original neighborhood residents. Federal Reserve Bank of Philadelphia Working Paper .

Campbell, J.Y., Giglio, S., Pathak, P., 2011. Forced Sales and House Prices. American Economic Review 101, 2108–2131.

Carbone, J.C., Hallstrom, D.G., Smith, V.K., 2006. Can Natural Experiments Measure Behavioral Responses to Environmental Risks? Environment Resource Economics. Environment & Resource Economics 33, 273–297.

Card, D., Mas, A., Rothstein, J., 2008. Tipping and the dynamics of segregation. The Quarterly Journal of Economics 123, 177–218.

Carpenter, C., 2004. New Evidence on Gay and Lesbian Household Incomes. Contemporary Economic Policy 22, 78–94.

Carpenter, C., 2007. Revisiting the Income Penalty for Behaviorally Gay Men: Evidence from NHANES III. Labour Economics 24, 25–34.

Carpenter, C., Gates, G.J., 2008. Gay and Lesbian Partnership: Evidence from California. Demography 45, 573–590.

Carpenter, C.S., 2008. Sexual orientation, work, and income in Canada. Canadian Journal of Economics 41, 1239–1261.

Carson, J.M., McCullough, K.A., Pooser, D.M., 2013. Deciding Whether to Invest in Mitigation Measures: Evidence From Florida. Journal of Risk and Insurance 80, 309–327.

Census, 2011. Census Bureau Releases Estimates of Same-Sex Married Couples. Technical Report. Washington, DC: Bureau of the Census.

Cheng, S., Ganapati, E., Ganapati, S., 2015. Measuring disaster recovery: bouncing back to reaching the counterfactual state? Disasters 39, 427–446.

Christafore, D., Leguizamon, J.S., 2013. Revisiting Evidence of Labor Market Discrimination against Homosexuals and the Effects of Anti-Discriminatory Laws. Review of Regional Studies 42, 213–238.

Christafore, D., Leguizamon, J.S., Leguizamon, S., 2013. Are black neighborhoods less welcoming to homosexuals than white neighborhoods? Regional Science and Urban Economics 43, 579–589.

Christafore, D., Leguizamon, S., 2012. The influence of gay and lesbian coupled households on house prices in conservative and liberal neighborhoods. Journal of Urban Economics 71, 258–267.

Christafore, D., Leguizamon, S., 2017. Is "Gaytrification" a Real Phenomenon? Urban Affairs Review 54, 1–23.

Christafore, D., Leguizamon, S., 2019. Neighborhood inequality spillover effects of gentrification. Papers in Regional Science 98, 1469–1484.

Chu, C., Henderson, D.J., Wang, L., 2017. The Robust Relationship Between US Food Aid and Civil Conflict. Journal of Applied Econometrics 32, 1027–1032.

Clark, T.N., 2015. Gays and Urban Development: How are they Linked? The City as an Entertainment Machine. Research in Urban Policy 9, 221–234.

Collins, A., 2004. Sexual Dissidence, Enterprise and Assimilation: Bedfellows in Urban Regeneration. Urban Studies 41, 1789–1806.

Collins, A., Drinkwater, S., 2017. Fifty Shades of Gay: Social and Technological Change, Urban Deconcentration and Niche Enterprise. Urban Studies 54, 765–785.

Coulson, N.E., Wommer, G., 2019. Tenure tipping. Regional Science and Urban Economics 77, 172–181.

Couture, V., Handbury, J., 2017. Urban Revival in America, 2000 to 2010. NBER Working Paper No. 24084 .

Dastrup, S., Ellen, I.G., 2016. Linking Residents to Opportunity: Gentrification and Public Housing. Cityscape 18, 87–107.

Davlasheridze, M., Fan, Q., 2017. Household Adjustment to Hurricane Katrina. Review of Regional Studies 47, 93–112.

Depken II, C.A., Hollans, H., Swidler, S., 2009. An empirical analysis of residential property flipping. Journal of Real Estate Finance and Economics 39, 248–263.

Ding, L., Hwang, J., 2016. The Consequences of Gentrification: A Focus on Residents' Financial Health in Philadelphia. Cityscape 18, 27–56.

Ding, L., Hwang, J., Divringi, E., 2016. Gentrification and Residential Mobility in Philadelphia. Regional Science and Urban Economics 61, 38–51.

Doan, P.L., Higgins, H., 2011. The Demise of Queer Space? Resurgent Gentrification and the Assimilation of LGBT Neighborhoods. Journal of Planning Education and Research 31, 6–25.

Dragan, K., Ellen, I., Glied, S.A., 2019. Does Gentrification Displace Poor Children? New Evidence from New York City Medicaid Data. NBER Working Paper No. 25809 .

Dumm, R.E., Sirmans, G.S., Smersh, G., 2011. The Capitalization of Building Codes in House Prices. Journal of Real Estate Finance and Economics 42, 30–50.

Edlund, L., Machado, C., Sviatschi, M.M., 2015. Bright Minds, Big Rent: Gentrification and the Rising Returns to Skill. NBER Working Paper No. 21729 .

Ellen, I.G., Laco, J., Sharygin, C.A., 2013. Do foreclosures cause crime? Journal of Urban Economics 74, 59–70.

Ellen, I.G., O'Regan, K.M., 2011. How Low Income Neighborhoods Change: Entry, Exit, and Enhancement. Regional Science and Urban Economics 41, 89–97.

Florida, R., 2002. The Rise of the Creative Class. Basic Books, New York, NY.

Florida, R., Gates, G., 2001. Technology and Tolerance: The Importance of Diversity to High- Technology Growth. Emerald Group Publishing Limited.

Florida, R., Gates, G., 2015. Technology and Tolerance: The Importance of Diversity to High-Technology Growth. The City as an Entertainment Machine 9, 199–219.

Florida, R., Mellander, C., 2010. There goes the metro: how and why bohemians, artists and gays affect regional housing values. Journal of Economic Geography 10, 167–188.

Freeman, L., 2005. Displacement or Succession? Residential Mobility in Gentrifying Neighborhoods. Urban Affairs Review 40, 463–491.

Freeman, L., Braconi, F., 2004. Gentrification and Displacement New York City in the 1990s. Journal of the American Planning Association 70, 39–52.

Fronstin, P., Holtmann, A.G., 1994. The Determinants of Residential Property Damage Caused by Hurricane Andrew. Southern Economic Journal 61, 387–397.

Fu, C., Gregory, J., 2019. Estimation of an Equilibrium Model with Externalities: Post-Disaster Neighborhood Rebuilding. Econometrica 87, 387–421.

Fu, S., 2008. Sexual orientation and neighborhood quality: Do same-sex couples make better communities? Technical Report.

Gates, G., Cooke, A.M., 2012. United States Census Snapshot: 2010. Technical Report.

Gates, G.J., Ost, J., 2004. The Gay & Lesbian Atlas. The Urban Institute Press, Washington, DC.

Gatzlaff, D., McCullough, K., Medders, L., Nyce, C.M., 2018. The Impact of Hurricane Mitigation Features and Inspection Information on House Prices. Journal of Real Estate Finance and Economics 57, 566–591.

Ghaziani, A., 2014. There Goes the Gayborhood? Princeton University Press, Princeton, NJ.

Ghaziani, A., 2015. 'Gay Enclaves Face Prospect of Being Passé': How Assimilation Affects the Spatial Expressions of Sexuality in the United States. International Journal of Urban and Regional Research 39, 756–771.

Glaeser, E.L., Gyourko, J., 2005. Urban Decline and Durable Housing. Technical Report.

Goldie, X., 2018. Together, but Separate: Neighborhood-Scale Patterns and Correlates of Spatial Segregation Between Male and Female Same-Sex Couples in Melbourne and Sydney. Urban Geography 39, 1391–1417.

Guerrieri, V., Hartley, D., Hurst, E., 2013. Endogenous gentrification and housing price dynamics. Journal of Pubic Economics 100, 45–60.

Hallstrom, D.G., Smith, V.K., 2005. Market responses to hurricanes. Journal of Environmental Economics and Management 50, 541–561.

Harding, J.P., Rosenblatt, E., Yao, V.W., 2009. The contagion effect of foreclosed properties. Journal of Urban Economics 66, 164–178.

Hayslett, K.L., Kane, M.D., 2011. "Out" in Columbus: A Geospatial Analysis of the Neighborhood-Level Distribution of Gay and Lesbian Households. City & Community 10, 131–156.

Helms, A., 2003. Understanding Gentrification: An Empirical Analysis of the Determinants of Urban Housing Renovation. Journal of Urban Economics 54, 474–498.

Herek, G.M., 2009. Hate Crime and Stigma-Related Experiences Among Sexual Minority Adults in the United States. Journal of Interpersonal Violence 24, 54–74.

Hornbeck, R., Keniston, D., 2017. Creative Destruction: Barriers to Urban Growth and the Great Boston Fire of 1872. American Economic Review 107, 1365–1398.

Horney, J., Dwyer, C., Aminto, M., Berke, P., Smith, G., 2017. Developing indicators to measure post-disaster community recovery in the United States. Disasters 41, 124–149.

Humphreys, B.R., Nowak, A., Zhou, Y., 2019. Superstition and real estate prices: transaction level evidence from the US housing market. Applied Economics 51, 2818–2841.

Jenkins, M., L.E.G., Baker, D.N., 2009. The attitudes of black and white college students toward gays and lesbians. Journal of Black Studies 39, 589–613.

Jepsen, C., Jepsen, L.K., 2009. Does Home Ownership Vary by Sexual Orientation? Regional Science and Urban Economics 39, 307–315.

Kennedy, M., Leonard, P., 2001. Dealing with neighborhood change: A primer on gentrification and policy choices. Discussion Paper, Brookings Institution Center on Urban and Metropolitan Policy and PolicyLink 2, 23–43.

Klawitter, M.M., Flatt, V., 1998. The Effects of State and Local Antidiscrimination Policies on Earnings for Gays and Lesbians. Journal of Policy Analysis and Management 17, 658–686.

Kolko, J., 2007. The Determinants of Gentrification.

Kousky, C., 2010. Learning from Extreme Events: Risk Perceptions after the Flood. Land Economics 86, 395–422.

Lacoe, J., Ellen, I.G., 2015. Mortgage Foreclosures and the Changing Mix of Crime in Micro-neighborhoods. Journal of Research in Crime and Delinquency 52, 717–746.

Lee., D., 2018. The Impact of Natural Disasters on Neighborhood Poverty Rate: A Neighborhood Change Perspective. Journal of Planning Education and Research , 1–13.

Leguizamon, J.S., Leguizamon, S., 2017. Disentangling the effects of tolerance on housing values: how levels of human capital and race alter this link within the metropolitan area. Annals of Regional Science 59, 1–22.

Leik, R.F., 1966. A Measure of Ordinal Concensus. Pacific Sociological Review 9, 85–90.

Lenze, D.G., 1997. Dynamic and Spatial Impact of Hurricane Andrew on Florida's Taxable Sales: An Intervention Analysis. Review of Regional Studies 27, 163–183.

Leppel, K., 2007a. Home-Ownership Among Opposite- and Same-Sex Couples in the US. Feminist Economics 13, 1–30.

Leppel, K., 2007b. Married and Unmarried, Opposite- and Same-Sex Couples: A Decomposition of Homeownership Differences. Journal of Housing Research 16, 61–82.

Liao, Y., Panassié, Y., 2019. How hurricanes sweep up housing markets: Evidence from Florida.

Lin, Z., Rosenblatt, E., Yao, V.W., 2008. Estimates of the Impact of Crime Risk on Property Values from Megan's Laws. American Economic Review 98, 1103–1127.

Lin, Z., Rosenblatt, E., Yao, V.W., 2009. Spillover Effects of Foreclosures on Neighborhood Property Values. Journal of Real Estate, Finance, and Economics 38, 387–407.

Madden, J.F., Ruther, M., 2015. Gayborhoods: Economic Development and the Concentration of Same-Sex Couples in Neighborhoods Within Large American Cities. New York: Springer.

Malone, T., 2020. There Goes the Neighborhood: Does Tipping Exist Amongst Income Groups? Journal of Housing Economics 48, 1–15.

McCoy, S.J., Walsh, R.P., 2018. Wildfire risk, salience housing demand. Journal of Environmental Economics and Management 91, 203–228.

McKinnish, T., Walsh, R., White, T.K., 2010. Who Gentrifies Low-Income Neighborhoods? Journal of Urban Economics 67, 180–193.

McVeigh, R., Diaz, M.E.D., 2009. Voting to ban same-sex marriage: Interests, values, and communities. American Sociological Review 74, 891–915.

Meltzer, R., 2016. Gentrification and Small Business: Threat or Opportunity? Cityscape 18, 57–85.

Meltzer, R., Ghorbani, P., 2017. Does Gentrification Increase Employment Opportunities in Low-Income Neighborhoods? Regional Science and Urban Economics 66, 52–73.

Meuhlenbachs, L., Spiller, E., Timmins, C., 2015. The Housing Market Impacts of Shale Gas Development. American Economic Review 105, 3633–3659.

Miyao, T., 1978. Dynamic Instability of a Mixed City in the Presence of Neighborhood Externalities. American Economic Review 68, 454–463.

Morales, D.X., 2018. Residential Segregation of Same-Sex Partnered Households in the US: New Evidence from US Census 2010. International Journal of Sociology and Social Policy 31, 973–981.

Moss, M.L., 1997. Reinventing the central city as a place to live and work. Housing Policy Debate 8, 471–490.

Newman, K., Wyly, E.K., 2006. The Right to Stay Put, Revisted: Gentrification and Resistance to Displacement in New York City. Urban Studies 43, 23–57.

Nowak, A., Sayago-Gomez, J., 2018. Homeowner Preferences After September 11th, a Microdata Approach. Regional Science and Urban Economics 70, 330–351.

Nowak, A., Smith, P., 2017. Textual Analysis in Real Estate. ournal of Applied Econometrics 32, 896–918.

Ozabaci, D., Henderson, D.J., Su, L., 2014. Additive nonparametric regression in the presence of endogenous regressors. Journal of Business & Economic Statistics 32, 555–575.

O'Connell, M., Feliz, S., 2011. Same-sex couple household statistics from the 2010 census. Technical Report.

Pasch, R.J., Brown, D.P., Blake, E.S., 2004. Tropical Cyclone Report: Hurricane Charley. Technical Report.

Peacock, W.G., Dash, N., Zhang, Y., Van Zandt, S., 2018. Post-Disaster Sheltering, Temporary Housing and Permanent Housing Recovery. Springer International Publishing AG.

Peacock, W.G., Van Zandt, S., Zhang, Y., Highfield, W.E., 2014. Inequities in Long-Term Housing Recovery After Disasters. Journal of the American Planning Association 80, 356–371.

Pope, J.C., 2008. Fear of crime and housing prices: Household reactions to sex offender registries. Journal of Urban Economics 64, 601–614.

Poston, D.L., Chang, Y., 2016. Patterns of Gay Male and Lesbian Partnering in the Metropolitan Areas of the United States in 2010. Frontiers in Sociology 1, 1–11.

Qian, H., 2013. Diversity Versus Tolerance: The Social Drivers of Innovation and Entrepreneurship in US Cities. Urban Studies 50, 2718–2735.

Rathfon, D., Davidson, R., Bevington, J., Vincini, A., Hill, A., 2013. Quantitative assessment of post-disaster housing recovery: a case study of Punta Gorda, Florida, after Hurricane Charley. Disasters 37, 333–355.

Rosenthal, S.S., 2008. Old Homes, Externalities, and Poor Neighborhoods: A Model of Urban Decline and Renewal. Journal of Urban Economics 63, 816–840.

Rossi-Hansberg, E., Sarte, P.D., Owens III, R., 2010. Housing Externalities. Journal of Political Economy 118, 485–535.

Sadayuki, T., Kanayama, Y., Arimura, T.H., 2019. Evaluating the externality of vacant houses in japan: The case of Toshima municipality, Tokyo. Research Institute for Environmental Economics and Management Discussion Paper Series N. 1901 .

Schaffer, R., Smith, N., 1986. The Gentrification of Harlem? Annals of the Association of American Geographers 76, 347–365.

Schelling, T.C., 1971. Dynamic Models of Segregation. Journal of Mathematical Sociology 1, 143–186.

Schuetz, J., 2014. Do art galleries stimulate redevelopment? Journal of Urban Economics 83, 59–72.

Schuetz, J., Been, W., Ellen, I.G., 2008. Neighborhood effects of concentrated mortgage foreclosures. Journal of Housing Economics 17, 306–319.

Siodla, J., 2015. Razing San Francisco: The 1906 disaster as a natural experiment in urban redevelopment. Journal of Urban Economics 89, 48–61.

Smart, K., Prohaska, A., 2017. Disaster Capitalism, Housing Vulnerability, and the Tuscaloosa Tornado: A Critical Analysis of Rebuilding Efforts. International Journal of Mass Emergencies and Disasters 35, 1–19.

Smart, M.J., Whittemore, A.H., 2017. There Goes the Gaybourhood? Dispersion and Clustering in a Gay and Lesbian Real Estate Market in Dallas TX, 1986–2012. Urban Studies 54, 600–615.

Smith, V., Carbone, J., Pope, J., Hallstrom, D., Darden, M.E., 2006. Adjusting to natural disasters. Journal of Risk Uncertainty 33, 37–54.

Spader, J., 2015. Following Hurricanes Katrina and Rita. Cityscape: A Journal of Policy Development and Research 17, 199–221.

Spring, A.L., 2013. Declining Segregation of Same-Sex Partners: Evidence from Census 2000 and 2010. Population Research and Policy Review 32, 687–716.

Stephens-Davidowitz, S., 2013. How Many American Men Are Gay? New York Times.

Stevenson, J.R., Emrich, C.T., Mitchell, J.T., Cutter, S.L., 2010. Using Building Permits to Monitor Disaster Recovery: A Spatio-Temporal Case Study of Coastal Mississippi Following Hurricane Katrina. Cartography and Geographic Information Science 37, 57–68.

Strobl, E., 2011. The Economic Growth Impact of Hurricanes: Evidence from U.S. Coastal Counties. Review of Economics and Statistics 93, 575–589.

Vigdor, J.L., 2002. Does Gentrification Harm the Poor? Brookings-Wharton Papers on Urban Affairs 2002, 133–182.

Walther, C.S., Poston Jr., D.L., 2004. Patterns of gay and lesbian partnering in the larger metropolitan areas of the United States. Journal of Sex Research 41, 201–214.

Whittemore, A.H., Smart, M.J., 2016. Mapping Gay and Lesbian Neighborhoods Using Home Advertisements: Change and Continuity in the Dallas-Fort Worth Metropolitan Statistical Area Over Three Decades. Environment and Planning A 48, 192–210.

Wyly, E.K., Hammel, D.J., 1999. Islands of Decay in Seas of Renewal: Housing Policy and the Resurgence of Gentrification. Housing Policy Debate 10, 711–771.

Xiao, Y., Van Zandt, S., 2012. Building Community Resiliency: Spatial Links between Housing and Business Post-disaster Return. Urban Studies 49, 2523–2542.

Zhang, J., 2011. Tipping and Residential Segregation: A Unified Schelling Model. Journal of Regional Science 51, 167–193.

Zhang, Y., 2012. Will natural disasters accelerate neighborhood decline? A discrete-time hazard analysis of residential property vacancy and abandonment before and after Hurricane Andrew in Miami-Dade County (1991-2000). Environment and Planning B: Planning and Design 39, 1084–1104.

Zhang, Y., Peacock, W.G., 2009. Planning for Housing Recover? Lessons Learned From Hurricane Andrew. Journal of the American Planning Association 76, 5–24.

APPENDIX A

TABLES AND FIGURES FOR CHAPTER 2

Table A.1: Top 20 First and Last Names in Figure 2.2 Miami-Dade County, Florida

Male First and Last Names				Female First and Last Names			
Measure 1	Frequency	Measure 2	Frequency	Measure 1	Frequency	Measure 2	Frequency
jose	396	jose	71	maria	1290	maria	378
carlos	197	carlos	65	rodriguez	457	rodriguez	206
luis	195	gonzalez	60	jean	428	gonzalez	160
juan	190	jorge	60	garcia	358	garcia	147
jorge	159	juan	54	gonzalez	357	perez	116
michael	142	hernandez	48	ana	333	hernandez	106
gonzalez	121	perez	41	marie	331	ana	102
william	118	martinez	40	carmen	275	diaz	102
hernandez	116	garcia	39	perez	271	lopez	97
rafael	116	luis	37	rosa	264	carmen	83
diaz	116	diaz	33	angel	247	martinez	80
john	116	rafael	30	hernandez	240	barbara	78
perez	113	gustavo	29	lopez	233	fernandez	76
jesus	106	mario	28	diaz	233	angel	76
rodriguez	100	rodriguez	28	martinez	198	isabel	71
pedro	92	alberto	25	barbara	198	olga	64
raul	88	reinaldo	25	fernandez	195	rosa	64
robert	87	michael	24	rene	192	torres	61
julio	84	lopez	23	sanchez	189	moreno	61
roberto	83	antonio	22	cruz	186	martha	57

Table A.2: Top 20 First and Last Names in Figure 2.3 Franklin County, Ohio

Male First and Last Names				Female First and Last Names			
Measure 1	Frequency	Measure 2	Frequency	Measure 1	Frequency	Measure 2	Frequency
gary	165	james	23	terry	515	mary	30
michael	136	john	22	mary	381	patricia	24
robert	130	robert	21	dale	329	linda	23
james	122	timothy	18	lee	217	barbara	22
steven	95	david	17	francis	192	donna	20
john	86	michael	14	barbara	166	smith	19
william	79	william	13	patricia	160	terry	18
brent	78	richard	13	linda	156	ruth	18
richard	78	johnson	11	gene	153	shawn	17
sroufe	75	jeffrey	11	willie	152	johnson	16
crawford	74	jerry	10	shawn	127	margaret	16
david	74	ronald	8	leslie	126	nancy	15
spanner	72	charles	8	susan	126	simmons	14
testa	72	mark	8	betty	124	pamela	14
jeffrey	61	paul	7	kelly	111	jones	14
charles	59	steven	7	nancy	107	karen	14
timothy	43	deans	6	thomas	105	miller	14
mark	39	johnny	6	margaret	103	dorothy	13
paul	38	stilwell	6	karen	101	betty	13
steve	37	barta	5	carol	98	fee	12

Table A.3: Top 20 First and Last Names in Figure 2.4 King County, Washington

Male First and Last Names				Female First and Last Names			
Measure 1	Frequency	Measure 2	Frequency	Measure 1	Frequency	Measure 2	Frequency
michael	606	michael	121	lee	1889	nguyen	477
john	513	john	108	nguyen	1118	tran	243
david	458	robert	105	kim	1107	thi	215
james	445	david	100	terry	1078	van	171
robert	394	james	84	mary	1068	lee	153
william	279	mark	67	ann	954	kim	144
mark	257	steven	63	dale	931	pham	132
paul	223	william	60	chris	859	chen	126
richard	220	christopher	60	kelly	760	mary	122
daniel	210	richard	57	shawn	704	jennifer	113
christopher	206	jeffrey	47	thomas	704	ann	109
steven	204	daniel	46	jennifer	695	smith	109
joseph	199	paul	44 van	667	elizabeth	101	
jeffrey	190	joseph	42	susan	596	wei	94
matthew	157	smith	41	thi	574	hoang	94
brian	150	stephen	38	elizabeth	538	wong	89
kevin	141	brian	36	lynn	498	shawn	88
charles	134	kevin	36	tran	495	hong	87
eric	130	jose	35	lisa	493	huang	85
andrew	128	matthew	34	linda	492	thanh	82

Figure A.1: Distribution of Same-Sex Couple Homebuyer Transactions per Year in King County, Washington