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Three Essays on Civil Disturbances, Crime, and Housing Markets

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THREE ESSAYS ON CIVIL DISTURBANCES, CRIME, AND HOUSING MARKETS

by

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ABSTRACT

In these three essays, I examine the relationship between housing prices and civil disturbances. In the first essay, I examine the Ferguson Unrest in 2014 following the killing of Michael Brown. Using a hedonic model and a repeat sales model using data from ZTRAX, I find a highly significant negative affect around the events temporally and spatially. In the second essay I examine house price indices across the US during the onset of COVID and during the protests following the killing of George Floyd. I use the Zillow Home Value Index and I find cities which experienced protests experienced less growth than those which did not, and COVID requirements have a heterogeneous effect dependent on enforcement. The severity of the negative effect of the protest depends on protest size and the interaction between the COVID lockdown requirements. In the third and final essay, I continue using the Zillow Home Value Index and find the George Floyd protests had spillover effects into adjacent municipalities within the same metropolitan statistical area. Cities which experienced protests which resulted in a death experienced spillover effects with the adjacent municipalities having a statistically and economically significant reduction in housing price growth, but less severe than the city where the protest took place. Taken together the essays contribute to the literature on civil disturbances and their relationship with housing prices, the literature on crime and its relationship with housing prices, and the literature on COVID-19 restrictions and their relationship with housing prices.

To my cats, Hobbes and Orla

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The onset of the COVID-19 pandemic impacted different communities in several ways. While research regarding COVID-19 and housing prices exist, there are no studies which examine the civil disturbances which erupted following the killing of George Floyd during COVID-19. Using monthly indices from the Zillow Home Value Index (ZHVI) for individual cities across the country, I conduct a difference-in-difference analysis and find a negative and significant effect on housing prices in cities with Black Lives Matter Protests after controlling for regional COVID policies. I also find supporting evidence of an increase in housing demand during and after the lockdown periods, varying for each state. I find a positive effect on housing prices for cities in states which issued a recommended stay-at-home order, but no significant effect for cities in states with required stay-at-home orders. I interpret this finding as evidence of a positive demand shock from the stay-at-home orders, however if states enforced the policy as a requirement, the policy disrupted the market enough to no longer be a positive externality.

Recent research shows there is a significant and negative housing price effect around the epicenter of protests, and within cities which experience the protests. The effects diminish with distance and time, both of which are a function of the nature of the protest (peaceful vs violent) and the size of the protest. In Ferguson, negative price effects are significant for 3 years after the Ferguson Unrest in 2014. With the George Floyd protests, which varied significantly in costly damages and size across the nation, the effects were much more short term, only disrupting the market for approximately a month around the event. One unanswered question is how wide reaching these effects are. Many of the major George Floyd protests occurred in major metropolitan areas with multiple municipalities. Typically, the protests were confined to a couple of these individual municipalities within the metro area. How does the initiation of protests in one city affect the housing prices of the neighboring cities? Do residents move from one city to another? Or do they leave the Metro area entirely? In this paper, I find results which suggest the protests negatively affect the neighboring cities housing markets as well, indicating buyers were trending away from the metro area in its entirety.

ESSAY ONE: CIVIL DISTURBANCES AND HOUSING PRICES: THE FERGUSON EFFECT

Abstract

In the literature there are few studies examining civil disturbance effects on housing prices. I examine housing prices around Ferguson, Missouri around the time of the Ferguson Unrest from Aug 9, 2014, to Aug 14, 2015. The examination shows there is a significant price discount in housing prices post Ferguson between 2012-2016 for houses within a mile of the events, as opposed to the surrounding areas. The sale of houses in the area decrease significantly in the year after Ferguson, making any short-term analysis difficult. Using probit analysis, I identify biases in the pre and post Ferguson samples and use the repeat sales method to alleviate any sample selection issues.

Introduction

In the current literature it is not clear what effect civil disturbances have on housing prices. Previous studies on the impact of civil disturbances focus on urban development but lack emphasis on housing prices. Collins and Margo (2007) find race-related riots in the 1960s depressed the median value of Black-owned property between 1960 and 1970 and did not recover in the decade afterwards. They admit the study is not definitive, citing imperfect data, endogeneity, and a great deal of economic factors could drive the effect in such an extended period. Pyrooz et al (2016) finds no evidence of a systematic Ferguson Effect across the entire US but finds evidence for the effect in cities with a higher rate of violence before Ferguson, a higher proportion of Black residents, lower socioeconomic status, and more police per capita.

This type of event is mostly independent of idiosyncrasies across cities. The response in the form of peaceful protests and violent riots can occur in any metropolitan area and cause damage to different areas of the city. Academic research nor common knowledge fully understands the consequences of violent riots. The literature on crime struggles to differentiate between historic levels of crime and events of crime, since they tend to be correlated. The events in Ferguson, Missouri were unambiguously an event of various crimes and allows for the examination of this specific type of event. The events in Ferguson create an ideal sample to examine because the local nature, major property damage, and the events had no other concurrent effects to add ambiguity around the timeframe of interest.

I examine housing prices around Ferguson, Missouri around the time of the Ferguson Unrest, Aug 9, 2014 – Aug 14, 2015. The examination shows there is a significant price discount in housing prices post Ferguson between 2012-2016 for houses within a mile of the events, as opposed to the surrounding areas. The sale of houses in the area decrease significantly in the year after Ferguson, making any short-term analysis difficult. Using probit analysis, I identify biases in the post and pre-Ferguson samples, and use repeat sales method to alleviate any sample bias issues.

There are several channels in which riots may affect property prices. The Ferguson effect is an anecdotal theory where there is de-policing after events specific to police behavior, as officers will decrease enforcement of the law to avoid lawsuits and criticism. If this occurs, this could lead to increases in crime rates. Another channel is an incident as high profile as the events in Ferguson signal to the public a lack of fair administration of justice, which challenges the validity of the law. If the public believes this, a possible response is an increased participation in crime. There is a possibility the riots themselves affect the social bonds within a neighborhood directly, independent of the riots' effect on crime. If this is the case, the effect should capitalize into the local housing prices through a reduction in housing demand for the locales which experienced a deterioration in social bonds.

Literature Review

Tita, Petras Greenbaum (2006) asks the following question and examines Columbus Ohio to test it: How would crime incur costs to existing homeowners? Neighborhoods incur indirect costs when fleeing residents fracture the social bonds necessary for the development of local informal social control needed to deter local crime. There is a local synergy which helps to prevent crime; when residents who contribute to this synergy relocate because of an increase in crime, this damages synergy which leads to more crime, creating a dangerous cycle. Bursick & Grusmick (1993) and Sampson (2004) both reference this cycle. This manifests as price suppression of local houses, which is important for individuals as this is a major mechanism for wealth accumulation. Therefore, crime affects the desirability of ownership. Dietz & Haurin (2003) examine this aspect. The previously mentioned Tita, Petras, and Greenbaum (2006), contribute to the literature by disambiguating crime. They point out the change in local crime rates is key, as crime rates have a problem of underreporting. Underreporting varies by wealth of the neighborhood, as does crime. Property crimes dominate crime rates and are likely to be inconsistently underreported, and this becomes more severe in lower income neighborhoods. Violent crime is an effective way to measure crime because violent crime is less likely underreported. Typically, the assumption in the literature is crime homogenously impacts housing prices across a city, although impact could differ based on socio-economic composition. The cost of crime manifests itself in the actual cost of moving, as well as an implied cost of potentially sub-optimal allocations of homeowners. Duggan (1999) found victimization near one's home increases the probability of moving. Another effect which exasperates the effect of crime, is white flight. This leads to more racially homogenous neighborhoods which lowers the demand by shrinking the pool of buyers to people of the race left. Gibbons (2004) studies at the census tract level and finds vandalism, graffiti and arson are much more impactful than crime rates because the perception of neighborhood safety is important, and these are very visible signals unlike robberies. The paper looks at both changes in crime as well as levels of crime and type of crime. Skogan (1999) clarifies the underreporting claims, and finds lower income, younger, and male victims are more likely to under report, while homeowners tend to report. This raises the question of how to alleviate this relation between prices and crime reporting reliability. A solution may be to analyze neighborhoods separately based on wealth.

Odubiyi et al. (2019) attempts to predict the probability of rent price increases by using housing characteristics and finds the relationship of proximity to police stations is positive but not significant. Fondevila et al. (2021) uses a sample from Argentina to find the commission of crimes increases exponentially as the distance from the nearest police station increases until about 500-600m away. The authors find police stations have a deterrent effect on crime.

According to (Buck, Hakim, & Spiegel, Casinos, Crime, and Real Estate Values: Do They Relate?, 1991), urban economic models suggest land values diminish with distance from a central business district because of employment opportunities and amenities. If the new industry has the negative byproduct of crime, which should have a reversed effect, the net effect of (dis)amenities as a function of distance from the central city is ambiguous. Applying the model to casinos in Atlantic City they find the frequency of violent crimes, burglaries, and robberies diminish with distance and appear to have a depressing effect on property values. Both the negative effect of crime and the positive effect of proximity to the CBD decline with distance.

Beavon, Brantingham, & Brantingham (1994) explore the relationships among property crime, the accessibility of street networks and the concentration of potential targets. They hypothesize areas with the most complex road networks and the fewest common destination points should have the lowest levels of property crime. The study compares the relative amount of property crime in each street segment with the segment's relative accessibility, traffic volume and quantity of potential targets and finds crime was higher in more accessible and popular areas and lower in the less accessible and less used areas. The concentration of potential targets was highly related to accessibility and traffic flow and to overall property crime totals. The study suggests traffic barriers and road closures as effective crime prevention techniques.

Buck, Hakim, & Spiegel, Endogenous Crime Victimization, Taxes, and Property Values (1993) explores the twofold effect which results from raising taxes to finance more policing. Better security increases housing values and increases in real estate taxes yield an adverse effect on property values. They observe Atlantic City, New Jersey, and find security increase outweighs the adverse effects on average.

Ceccato & Wilhelmsson, Do crime hot spots affect housing prices? (2020) discuss what happens to housing values when properties are close to places with high concentrations of crime, often called ‘hot spots.’ They employ hedonic price modelling to estimate the impact of crime hot spots on housing sales, controlling for property, neighborhood and city characteristics in the

Stockholm metropolitan region, Sweden. The overall effect on house prices is small, but if measured by distance to a crime hot spot, the effect is non-negligible. They find moving a house 1 km closer to a crime hot spot reduces value by more than SEK 30,000. Vandalism is the type of crime which most affects prices for both multi- and single-family housing, but the effect decreases with distance from a crime hot spot. Clark & Cosgrove (1990) use a two-stage intercity hedonic model to estimate household demand for public safety to address the identification problem inherent in the hedonic model. They estimate a willingness-to-pay function for public-safety and find income be the primary determinant of willingness to pay and the influence of income outweighs the combined impact of family life-cycle considerations.

Wihlelmsson, Cecatto, & Gerell (2021) analyzes the effect of gun-related violence on housing values while controlling for the area's crime levels and locational factors. The authors create windows around the shooting (event) and estimate the causal effects of the shootings. Using a regression discontinuity, they find shootings indirectly affect the environments where shootings occur. Housing prices capitalize the indirect effect of shootings in the immediate area and the effect appears to be long-term and persistent. The capitalization effect is higher with proximity to the CBD but not significantly different in areas with a higher crime rate per capita. Cohen (1990) attempts to estimate the cost of individual crimes by examining the pain, suffering and fear endured by crime victims. They use actual victim injury rates with jury awards in personal injury accident cases to estimate pain, suffering and fear while using crime-related death rates and estimates of the value of life to value the risk of death. This approach yields crime-specific cost estimates which are consistent with the property value methods of valuing aggregate crime.

Boggess, Greenbaum, & Tita (2013) hypothesize price-based models underestimate the true costs of crime because such models rely on transactions to estimate prices. If crime inhibits sales, price indices relying on sales may provide inaccurate measures of changes in housing demand in markets characterized by low rates of home sales. Similarly, price may be a poor indicator of crime-induced shifts in demand in markets characterized by elastic supply. The study addresses this by measuring the impact of violent, property, and overall crime and changes in crime on the rate of housing transactions across Los Angeles neighborhoods between 1993 and 1997. The results indicate both higher vacancy rates and higher levels of crime in the previous year related to higher rates of housing transactions and conclude the effect of crime inhibiting sales appears primarily due to additional violent crime in neighborhoods with prominent levels of vacancies and crime. Hellman & Naroff (1979) contribute to the discourse of impact of urban crime on the total utility derived from urban living. Changes in the utility derived from living in certain sections of urban areas, or urban areas in general, caused by variations in the crime rate can affect property values and property tax revenue. They provide a framework to provide initial estimates of the magnitude of these effects.

Autor, Palmer, & Pathak (2017) use variation induced by the sudden end of rent control in Cambridge, Massachusetts in 1995 to examine variation in reported crime across neighborhoods with different rent-control levels, following a city-wide decline in criminal activity. They find rent decontrol caused overall crime to fall by 16 percent with most of the effect accruing through reduced property crime and this crime reduction accounts for 15 percent of the contemporaneous growth in the Cambridge residential property values. Thaler (1978) estimates what effect crimes have on property values and infers homeowners' preferences from these estimates. The results indicate crime has a statistically significant negative impact on property values. Lens & Meltzer (2016) test how crime affects economic activity using point-specific data on crime, commercial property sales, and assessed values from New York City. They find crime reduces commercial property values, and the magnitude of the effect depends on the type and geography of crime, where there is stronger evidence for negative violent crime effects in neighborhoods with lower incomes and higher shares of minority residents and conclude disadvantaged neighborhoods are doubly harmed by crime because they have higher crime rates, and those crimes have stronger adverse effects on economic activity.

Taylor (1995) examines impacts of past and changing crime levels on changes in relative house values and vacancy rates in Baltimore, Maryland, neighborhoods in the 1970s and finds different crimes influence several aspects of the housing market. Past and changing crime rates play roles in ecological transitions of neighborhoods. Taylor concludes the impacts of crime on neighborhood viability may be contingent on personal, historical, and locale-specific factors. Carroll & Eger III (2006) examines tax increment financing in relation to the health and welfare issues of crime and brownfields, finding the public policy of tax increment financing can effectively increase districtwide property valuation to counteract the negating effects of crime and brownfields. Albouy, Christensen, & Sarmiento-Barbieri (2020) find improving safety near parks can turn them from public "bads" to goods. They argue ignoring complements may lead to undervaluing the potential value of public goods, overestimating heterogeneity in preferences, and understating the value of public goods to minority households.

Klimova & Lee (2014) measure the impact of murders on prices and rents of homes in Sydney and find housing prices fall by 3.9 per cent for homes within 0.2 miles of the murder in the year following the murder, and weaker results in the second year after a murder. Kim & Lee (2018) study how information on local (dis)amenities transmits and manifests in housing markets. Using nationwide data on multifamily homes in South Korea, they analyze heterogeneity in the effect of a sex offender's presence on sale prices and rents of nearby homes. The price effect of the offender's move-in varies significantly by spatial context. People react more strongly and persistently to the move-in of the offender in places where social connectedness is stronger. In the study they use homeownership as a proxy for social capital. They do not find an effect on rent, attributing better acquisition of and higher sensitivity to information on local amenities among members in highly connected communities and interpret the findings as evidence of social capital and social networks importance as sources of information on local amenities.

Valentin & O'Neill (2018) find despite being the predominant spatial value driver for hotels, the effect of proximity to the city center is heterogeneous. Several spatial variables, such as access to transportation systems and neighborhood dimensions, show accessibility and environment play a lesser but decisive role in hotel market value. Vetter, Beltrao, & Massena (2014) use a hedonic residential rent model for Brazil's metropolitan areas and estimates increasing the sense of security in the home by one standard deviation would increase average home values by $757, or about $13.6 billion if applied to all 18.0 million households in the study area. Their results indicate higher income households feel more secure from crime in the home, even though theft and robbery victimization rise with household income and rent level. Higher levels of home protection measures by higher income households partially explain this result.

Valencia & Sanz (2017) extend the model of Mills (1972) to include the homicide per commune as a measure of social distance, to quantify the effect on land prices. The model shows an increase in the homicide rate of one unit reduces the prices by 1.6%. Wong, Azhari, Abdullah, & Yip (2019) find householders in crime hotspot states are willing to pay more for crime reduction compared to householders in non-hotspot states and willingness to pay has also increased since the implementation of nationwide crime reduction plans in 2010. Bartley (2000) use multiple decades of information obtained from counties across the United States to create a panel data set and isolate the effects individual crimes have on housing prices. Petras (2007) explores the effect of how homebuyers perceive crime on housing prices. Petras uses survey data to compare how the effect of satisfaction with safety from crime on house prices differs from the effect of measured crime rates on house prices and finds neither measure is a good indicator of house prices. Emmanuel & Lizam (2017) and Aliyu, Muhammad, Bukar, & Singhry (2016) reviews the literature of crimes impact on housing prices.

Tang & Le (2021) exploit the change in perceived crime risk after gun offenders move into the neighborhood and examine its impact on housing values in the vicinity. Estimates indicate houses closest to gun offenders experience a 6.0% decline in value ($7,680) after the offenders move in with the effect stemming from fear of victimization rather than actual increases in crime. Buck, Deutsch, Hakim, Spiegel, & Weinblatt (1991) find the cost of crime resulting from casinos in Atlantic City, as reflected in unrealized assessed real estate valuation, appears to be on average $5.2m per square mile. Pope & Pope (2012) exploit the dramatic, nationwide decrease in crime which occurred in the 1990s to examine the relationship between changes in crime rates and property values. Using a fixed-effects framework they find a large and statistically significant association between crime and property values. Zip codes in the top decile in terms of crime reduction saw property value increases of 7–19% during the 1990s.

Troy & Grove (2008) use a hedonic analysis of property data in Baltimore, MD, to determine whether crime rate changes how the housing market values parks. Results indicate the housing market positively values park proximity where the combined robbery and rape rates for a neighborhood are below a certain threshold rate but negatively valued above the threshold. Braakman (2012) uses street-level data on house sales and crime rates for England and Wales to look at the existence of compensating differentials for crime risk. The results suggest each additional case of anti-social behavior lowers house prices in the same street by approximately 1% and each additional case of violent crime by 2%. Drug crime or crime outside the respective street do not seem to matter.

Goncalves (2009) examine the overall impact of crime rates on average home prices in America's state capitals. Hatch (2013) measures the impact of sex offenders on housing prices and finds the arrival of a sex offender into a neighborhood decreases housing prices within 0.1 of the miles of a sex offender by approximately 6%, while all other nearby housing sales prices are unaffected. This effect is symmetrical in sign, but not magnitude. Haurin & Brasington (1996) explain variations in real constant-quality house prices in jurisdictions located in multiple MSAs. They find it important to the explanation of variations in house price variables derived from urban theory, such as distance to the CBD, and from the amenity literature, such as a community's crime rate, arts, and recreational opportunities. Larsen, Lowrey, & Coleman (2003) reports a finding of a significant effect on the selling price of a single-family house given its proximity to a sex offender's residence. For more dangerous offenders, the effect is significant for houses located up to 0.3 miles. Houses located within 0.1 mile of an offender sold for 17.4% less. Pope J. C., Do Scarlet Letters Lead to Scarlet Homes? Household Reactions to Public Information from Sex Offender Registries (2008) indicate after a sex offender moves into a neighborhood nearby housing prices fall by 2.3% and prices rebound after the exit of a sex offender. Wentland, Waller, & Brastow (2014) examine neighborhood externalities which arise from the perceived risk associated with the proximity of a registered sex offender's residence and find large negative externality effects on a property's price and liquidity, employing empirical techniques including a fixed-effects OLS model, a correction for sample selection bias and censoring using a Heckman treatment, and a three-stage least-squares model to account for simultaneity bias in the joint determination of a home's sale price and liquidity. They also find amplified effects for homes with more bedrooms (a proxy for children) and if the state designates the nearby offender as “violent.”

Skoy (2021) claims BLM had backlash from people who point to demonstrations (especially violent ones) as a potential catalyst for de-policing. According to Mazunder (2019), BLM decreased racial resentment towards Black people. This effect, however, is heterogenous with age. The literature calls the potential de-policing following protests “the Ferguson Effect.” In Skoy (2021), she finds no evidence of the Ferguson effect. In terms of police activity, she finds there is a short-term effect in reducing black fatalities, but not long term. Another tangible effect of protests is they increase voter turnout. Schwartz, Sush, Voicu (2003) states the media suggests decreasing crime caused New York real estate prices to increase, implying the inverse would be true as well. A frequent problem to these types of analysis is “omitted variables” which could be correlated with crime rate and affect housing prices independently. Specifically, dilapidated houses and school quality. This paper uses both a hedonic method and a repeat sales method to measure the effect.

David C. Pyrooz, Scott H. Decker, Scott E. Wolfe, and John A. Shjarback (2016) finds no evidence to support a systematic Ferguson Effect on overall, violent, and property crime trends in large U.S. cities. Disaggregate analysis revealed only robbery rates, declining before Ferguson, increased in the month after Ferguson. Their results offer preliminary support for the Ferguson Effect on homicide rates in a few select cities. Cities with homicide rate increases had higher rates of violence before Ferguson, a higher proportion of Black residents, lower socioeconomic status, and more police per capita.

William J. Collins and Robert A. Margo (2007) uses OLS and IV to estimate the effect of race riots from the 1960s on central-city residential property, specifically Black-owned. They find the median value of Black-owned property was depressed after the riots and did not recover later. They admit a lack of clarity on the effect but argue this is some evidence the effect is negative. Kain and Quigley (1970) use regression to conclude crime reduces prices but the effect is not significant. Rizzo (1979) uses a hedonic price model with rates of total crime, property crime, and violence and finds all these variables have a negative and significant effect on housing prices. Naroff, Hellman, and Skinner (1980) examine the city of Boston and find comparable results. Dubin and Goodman (1982) perform principal component analyses on property and violent crimes and find both types have a significant impact on reducing housing prices. Burnell (1988) analyzes suburban Chicago communities and controls for community demographic and fiscal characteristics and finds more supporting evidence of negative and significant effects from crime on housing prices. Case and Mayer (1996) use a hedonic price model in conjunction with change in price and deal with endogeneity with lagged permits and amount of vacant land. Using the rates of total crime, they find the towns with higher crime rates had house prices appreciate faster in the boom but remained similar in a bust economy. Lynch and Rasmussen (2001) examine the estimated cost of the reported crime in small, policed areas in Jacksonville, Florida. They find the cost of crime has little to no impact on house prices, but in high-crime areas discount homes by 39%. In Dekalb County, Georgia, Bowes and Ihlanfeldt (2001) find negative crime effects concentrate close to downtown, and the effects vary regionally. Munroe (2007) and Linden and Rockoff (2008) both study Mecklenburg County in North Carolina, the first study based on total crimes per census block and the second based on locations of sex offenders, they conclude both have a negative effect on prices.

J.C. Pope (2008) also looks at sex offenders, this time in Hillsborough County in Florida, and finds comparable results. Hwang and Thilll (2009) find conflicting results depending on the type of crime in Buffalo, New York. Ihlanfeldt and Mayock (2010) find the changes in crime density explain the greatest variation in changes in the price index as a 1% increase in crime reduces housing prices .1%-.3%. Ceccato and Wilhelmsson (2011) uses data from Stockholm in Sweden and find Residential burglary exhibits a greater effect on prices, and fear of crime affects prices even after controlling for signs of vandalism. Congdon-Hohman (2013) use Akron, Ohio to conclude sale prices for houses decline 10%-19% in the wake of a methamphetamine laboratory discovery. Buonanno, Montolio, and Raya-Vilchez (2012) find homes in less safe districts in Barcelona, Spain have a valuation 1.27% lower than the rest of the city. Iqbal and Ceccato (2015), Caudill, Affuso, and Yang (2015) and Ceccato and Wilhelmsson (2016) all provide additional support for crime's negative effect on housing prices.

Background

On Aug 9, 2014, Officer Darren Wilson shot and killed 18-year-old African American Michael Brown during an encounter following a report of robbery and assault at a nearby convenience store. Michael Brown and his friend Dorian Johnson were walking in the middle of the street and Darren Wilson began questioning them. During the questioning, Brown attacked Wilson, and in the struggle Wilson's gun discharged and wounded Brown. Brown fled, Wilson pursued and shot Brown when he turned to confront him. Brown was surrendering and the killing was unjustified, according to supporters. Brown had turned and charged the officer, justifying the killing according to Wilson and his supporters.

The controversy soon turned into demonstrations with bouts of violence and destruction. The day after the shooting, demonstrators looted and vandalized twelve business. A QuikTrip and Little Caesar's were each set on fire. Over the next weeks, participants vandalized, looted, or damaged various businesses. Events continued to transpire over the next year, never reaching the heights of the first few months. A notable flare up is when Darren Wilson was not indicted in November 2014, protestors burned a dozen buildings down and accompanied the arson with gunfire, looting, vandalism, and the destruction of two police patrol cars and various other cars in the vicinity.

Figure 2 shows the locations of a sample of the burned down businesses during the height of the violence of the demonstrations. Some businesses were rebuilt in latter years as new entities, some businesses were rebuilt as the same or similar entities, and others remain vacant land to this day. Figure 2 has color coded marks, red indicates buildings which were not rebuilt, yellow indicates buildings which were rebuilt into new types of buildings, and purple indicates the building was rebuilt into the same business or type of building. The reader is encouraged to open Google Streetview at these addresses and use the "See More Dates" to see how the buildings changed over time. Google does not permit published works to show Streetview.

Data

This paper uses data from the ZTRAX dataset, provided by Zillow. The ZTRAX program has been ended, and Zillow is no longer accepting applications for access. The two halves which compose the dataset are ZTrans and ZAsmt. The ZTrans dataset collects information from the given counties' local clerk of courts, and records details about various transactions including sales, deed transfers, and foreclosure, depending on the local county recording procedures. ZAsmt collects data from the property's county assessor's office, providing the most recent assessed value of all property in a county and providing details on the properties' characteristics. It includes a last sale price variable, but this is often unpopulated and slow to update. Zillow recommends not using the variable as it should be the same figure from the Clerk of Courts but populated less reliably. I merge ZTrans and ZAsmt to match the transactions with characteristics of the property: number of bedrooms, number of bathrooms, and livable square footage. Zillow created the datasets to be comprehensive in terms of time, but since it is county by county, some are behind on backfilling transactions digitally. Zillow finds sample sizes are sufficient for most areas going back to April 1996. The exact data availability depends on the local state and county reporting procedures. Since the data is based on transactions, there is no time on market or other liquidity variables.

The final data only includes observations within fifty miles of Ferguson, which is most of St. Louis County with some observations in the neighboring counties. After communicating with the clerk of courts office, they clarified although in Missouri many counties do not require disclosure of sales price, local ordinances in St. Louis City, St. Louis County, Jackson County, and St. Charles County require mandatory disclosure. The clerk's office stated despite the ordinances the disclosure rate is less than 100%, but most sellers disclose transactions in these counties. Table 1 presents the summary statistics of the entire sample. The average house in the sample sells for $196,805, with a living square footage of 1674, 3.05 bedrooms, 1.94 bathrooms, and built in 1961.

Table 1: Summary Statistics

Variable	Mean	Std Dev
Sales Price	196,804.75	165,871.82
Living Square Feet	1674.22	968.80
Bedrooms	3.05	0.77
Baths	1.94	0.83
Year Built	1961.73	25.07
N	150,332	

The statistical tests need a sufficient control group. I use subsamples to vary the control in some models. Closer homes to the event are more likely affected by the event, and the extent to which the effect travels through space is unknown. In general, there are two main ways to measure distance from the epicenter: continuously and through buckets. Using a continuous distance measure allows for more precise variation among observations but creates a more difficult interpretation and gives little information about the range of the effect. Using the bucket system gives intuitive results and allows for clear distinguishment of coefficient levels as distance increases, but the bucket size and amount is inherently arbitrary and does not allow for variation within buckets. Since the extent is unknown, I use distance sets of 3, 5, and 10 miles from Ferguson when dealing with a continuous distance variable to combat the effects of distant properties the event more likely to appreciate independent of the area of interest. When using distance sets, I use subsamples including a 2013-2016 sample to focus on the events around Ferguson, and the 10-mile sample for the same reason.

I first use a hedonic model, with different specifications for the distance variable and composition of the sample. I then use a probit to test the post and pre samples for differences in the house characteristics, as the hedonic model assumes these are the same on average. To account for any problems with the variation across samples, I then use a repeat sales framework.

Results

How strong is the effect on local house prices? To measure the effects of the event, I conduct what is effectively a difference in difference analysis through ordinary least squares regression. I use a Hedonic Pricing model which includes the house characteristics, year fixed effects, and month fixed effects. The distance variable measures the distance in miles from the intersection of Ferguson Avenue and West Florissant Avenue, which is the approximate economic center of the town of Ferguson and in proximity of specific events which occurred during the protest including a QuikTrip set on fire just north of the intersection. The intersection is roughly the geographic center of Ferguson. The dataset includes a variable for House Quality on an A-F scale, including pluses and minuses. I did not use house quality variable in the first test. I create a post-dummy variable which is equal to 1 for any transactions after August 9, the day of the shooting. The model is as follows:

$$LN(House\ Price) = \alpha + \beta PostDummy + \gamma Distance + \delta PostDummy \times Distance + \mu \boldsymbol{X} + \zeta Year + \eta Month + \varepsilon$$

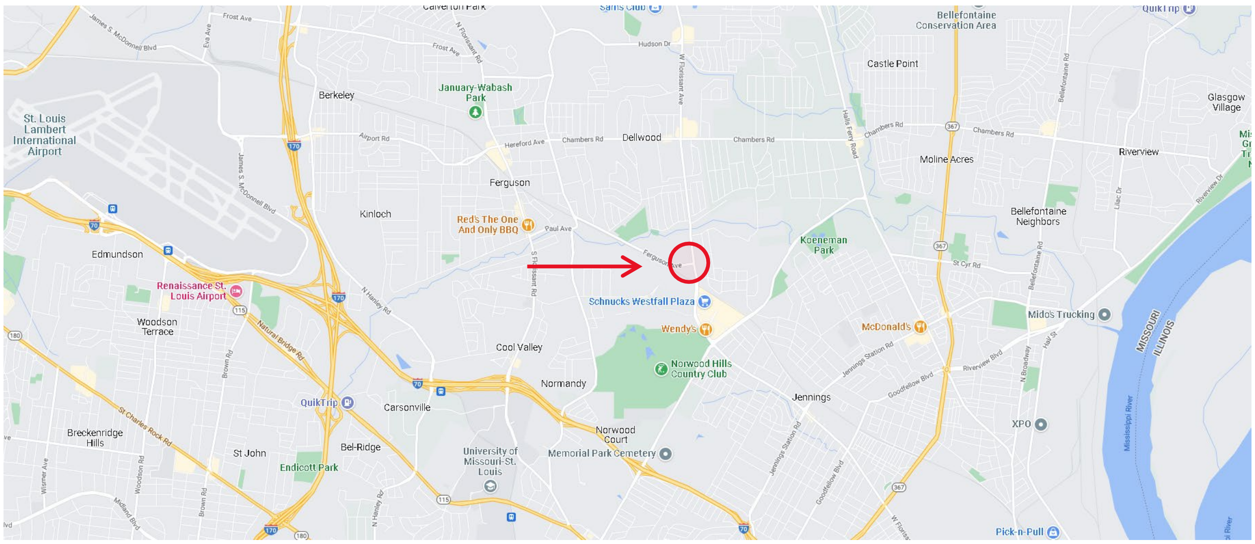

Figure 1: Epicenter

Where X represents housing characteristics including square footage, lot size, the home build year, number of rooms, and number of bathrooms. Distance is a continuous variable measuring the distance in miles from the point described before and shown in Figure 1. Given the area is already a low-income area, I expect this to be positive and significant. The post dummy is equal to 1 if the date of the transaction is after the beginning of the Ferguson riots. I expect a negative coefficient on this term, where significance will depend on the sample used. Any negative reputational effect following the events should appear here, as they are likely to be less dependent on specific location effects. The interaction term will be the variable of interest, interacting the distance from the epicenter and the post dummy. The expectation is the coefficient should be positive and significant if there is a differential in the coefficient size and significance on distance from the epicenter following the events in Ferguson. The interpretation of a positive coefficient on this interaction term would be that buyers are willing to pay a higher premium to live further away from Ferguson following the events than before the events. If there is no significant difference following the riots in Ferguson, the coefficient would be insignificant. Year fixed effects will capture the market conditions overall, whereas month fixed effects will capture any seasonality.

Table 2 presents the results of the first test. The results show there is a positive and significant effect on the interaction between the Post-Dummy and Distance in all three samples, 3, 5, and 10 miles sets. This implies a premium for houses further away from the event, or interpreted in reverse, a discount on houses closer to the event. The positive and significant result on distance shows there is already a premium to living away from Ferguson, but the economic and statistical significance of the interaction term shows this discount increased after the Ferguson events. For houses within 5-10 miles, the discount amounts to about 4.1% to 4.6% discount for every mile closer to the epicenter. The coefficients change with sample size, with the strongest coefficients occurring in the 3-mile sample where the discount per mile increase to 7%. These results indicate the effect is negative and significant but gets stronger the closer to the event area. Table 3 presents the results of the same model from Table 2, with the addition of house quality fixed effects, and the effects remain robust and similar qualitatively and quantitatively. Following the events in Ferguson, houses within the 1-mile ring sold at a discount of 28% according to the first model. The second model implies a discount of 32.9% and the third model a discount of 30.65%. Results are qualitatively identical and r-square increases.

Table 2: Continuous Distance Measure Without House Quality Controls

	Model (1) Distance < 5 miles	Model (2) Distance < 10 miles	Model (3) Distance < 3 miles
Intercept	3.770*** (14.31)	16.576*** (89.25)	4.068*** (9.52)
Post Dummy	-0.106** (-2.22)	-0.360*** (-12.50)	- 0.015*** (0.000)
Distance	0.041*** (34.31)	0.084*** (146.75)	0.038*** (11.16)
Post Dummy x Distance	0.041*** (8.32)	0.046*** (29.47)	0.070*** (4.19)
Living Sq. Ft. x 10	0.004*** (55.26)	0.004*** (63.88)	0.004*** (38.74)
Lot Size x 1000	0.003*** (6.90)	- 0.000*** (-5.05)	0.000*** (2.85)
# Rooms	0.020*** (6.66)	0.017*** (6.26)	0.016*** (3.48)
# Baths	0.106*** (26.88)	0.216*** (51.16)	0.095*** (14.81)
Year Built	0.003*** (26.92)	-0.003*** (-32.81)	0.003*** (15.28)
House Quality FE	N	N	N
Year FE	Y	Y	Y
Month FE	Y	Y	Y
N	27,227	67,818	11,670
R^2	0.60	0.77	0.59

Dependent variable is Log of Sale Price. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level.

Table 3: Continuous Distance Measure With House Quality Controls

	Model (1) Distance < 5 miles	Model (2) Distance < 10 miles	Model (3) Distance < 3 miles
Intercept	6.264*** (22.15)	14.557*** (79.07)	6.721*** (15.78)
Post Dummy	-0.160*** (-3.54)	-0.347*** (-15.54)	-0.033 (-0.44)
Distance	0.045*** (38.41)	0.073*** (138.83)	0.021*** (6.24)
Post Dummy x Distance	0.048*** (10.27)	0.047*** (32.14)	0.060*** (3.71)
House Characteristics	Y	Y	Y
House Quality FE	Y	Y	Y
Year FE	Y	Y	Y
Month FE	Y	Y	Y
N	27,227	67,677	11,669
R2	0.64	0.84	0.64

Dependent variable is Log of Sale Price. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level.

The continuous distance measure gives coefficients interpreted as relative coefficients. On average two equal houses with difference in distance to the epicenter will have the effect of the coefficient. This test does not give specific information regarding the distance the effect covers, nor does it allow for any interpretation regarding the any intensity changes with distance. The differences in the coefficients between samples implies the effect magnifies with increased proximity to the event, so to better illustrate and observe this effect, I now use distance sets around the affected area as my variables of interest. The model for the next test is as follows:

$$LN(House\ Price) = \alpha + \beta PostDummy + \gamma \boldsymbol{DistanceRing} + \delta PostDummy \times \boldsymbol{DistanceRing} + \mu \boldsymbol{X} + \zeta Year + \eta Month + \varepsilon$$

The advantage of this kind of test is that along with intuitive coefficients, it also gives insight into the spatial effect of the event without resorting to subsamples. Analysis is difficult within a small area and brief time frame in a place such as Ferguson because the level of home ownership is low in the area and therefore there is not a high volume of transactions. Further dividing this into locales presents an econometric challenge for power. The rings closer to the epicenter are closer to the destroyed amenities or amenities conditions prohibited from operating during and after the events in Ferguson. The properties within this first ring will have derived a higher proportion of their value attributed to close access to amenities, and therefore should have the largest drop in value following the riots in Ferguson. As the rings get further from the epicenter, the value derived from the amenities' proximity should decline, so the discount after the events should weaken the further out from the epicenter the ring is. Figure 2 shows the properties in their rings around the area depicted in figure 1.

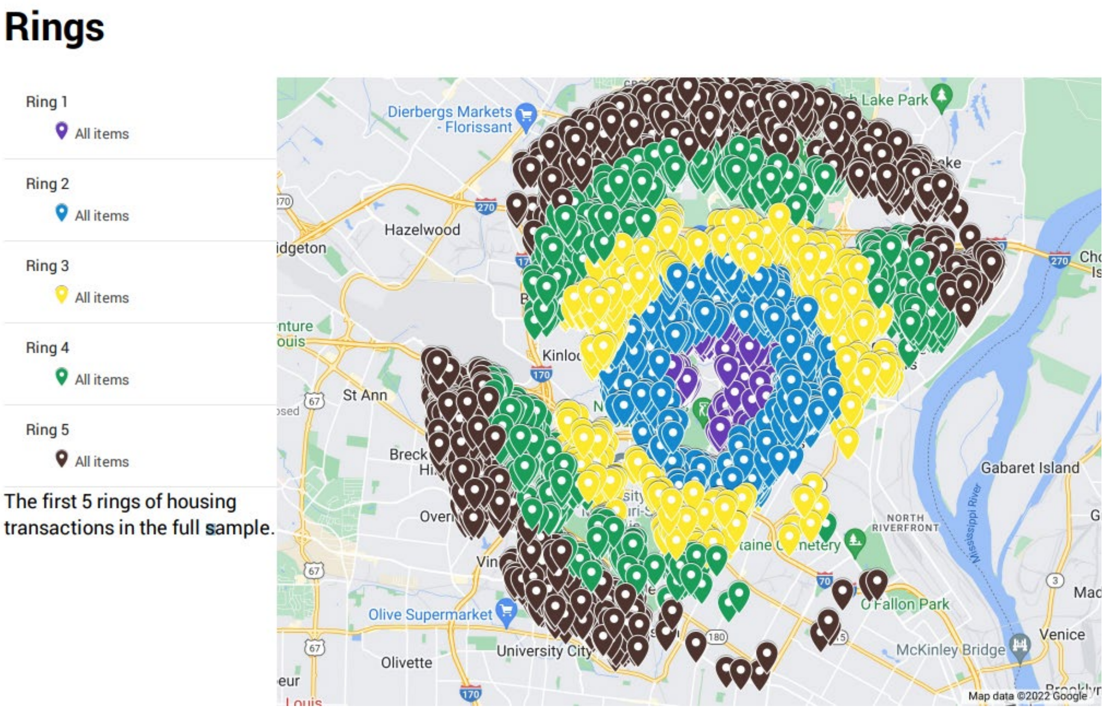

Figure 2: Sample Rings

Table 3 presents the results of this specification. The second test uses distance sets to examine the effects in 1-mile rings around the event center. The ring method will provide more intuitive results. A negative coefficient will imply a discount, and a positive coefficient will indicate a premium. I now make use of the house quality variable and add it as a fixed effect. This model shows there is a heavy discount in the area in and around Ferguson before accounting for the event itself. The interaction terms show a highly negative and significant coefficient within one mile of Ferguson at the 5% level in the sample of 2012-2016 transactions, and 1% level in the whole sample and the sample within ten miles. In the 2012-2016 sample coefficients translate to a 29% discount for homes within a mile of the event in the post environment. The 5% confidence level of the 2012-2016 is likely because of a lack of power with there being very few transactions in Ferguson following the events. In the following two samples, the effects start strongest at the 1-mile level at 37%-40%, and decline monotonically on average about 5.5%, down to about 15-18% for homes within 4-5 miles of the epicenter. R squared indicates the model explains 79-82% of variation.

Table 4: Distance Rings and House Quality Effects

	Model (1) 2012-2016	Model (2) Whole Sample	Model (3) Distance < 10 miles
Intercept	17.229*** (63.81)	16.021*** (128.51)	15.489*** (84.27)
Post Dummy	-0.007 (-0.53)	0.002 (0.14)	0.003 (0.15)
Within 1 mile	-0.608*** (-5.34)	-0.478*** (-65.91)	-0.392*** (-53.69)
Within 1-2 miles	-0.755*** (-16.22)	-0.453*** (-120.65)	-0.373*** (-93.12)
Within 2-3 miles	-0.760*** (-20.15)	-0.434*** (-120.98)	-0.355*** (-92.15)
Within 3-4 miles	-0.671*** (-19.65)	-0.361*** (-101.29)	-0.283 *** (-75.41)
Within 4-5 miles	-0.510*** (-24.30)	-0.316*** (-101.62)	-0.238*** (-70.90)
Within 1 mile x Post Dummy	-0.327** (-2.42)	-0.399*** (-10.13)	-0.366*** (-9.36)
Within 1-2 miles x Post Dummy	-0.083 (-1.46)	-0.338*** (-20.59)	-0.313*** (-18.89)
Within 2-3 miles x Post Dummy	0.078* (1.66)	-0.270*** (-17.35)	-0.249*** (-15.93)
Within 3-4 miles x Post Dummy	0.064 (1.57)	-0.218*** (-16.85)	-0.188*** (-14.16)
Within 4-5 miles x Post Dummy	-0.021 (-0.80)	-0.179*** (-20.30)	-0.149*** (-15.66)
House Characteristics	Y	Y	Y
House Quality FE	Y	Y	Y
Year FE	Y	Y	Y
Month FE	Y	Y	Y
N	14,036	130,173	67,678
R2	0.79	0.82	0.82

Dependent variable is Log of Sale Price. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level.

The statistical tests so far have examined how strong the effect is and how far the effect spreads out geographically. However, none of the tests illustrate how the effect changes temporally. How does the effect change over time? To answer this question, I interact the post event dummy with the distance ring dummies and the year effects. The interpretation of the coefficients must consider the original coefficient of the post effect and the distance ring. The model for the temporal and spatial test is as follows:

$$LN(House\ Price) = \alpha + \beta PostDummy + \gamma \boldsymbol{DistanceRing} + \delta PostDummy \times \boldsymbol{DistanceRing} + \theta \boldsymbol{DistanceRing} \times PostDummy \times Year + \mu \boldsymbol{X} + \zeta Year + \eta Month + \varepsilon$$

The expectation in this follows from the previous models, with the caveat that the interactions can be disambiguated by year. The original interaction term should capture if there is an overarching effect which carries through the post-riot period. I expect a temporally diminishing effect, where the greatest effect is closer to the event in 2014 and decreases through time. I use Google Maps and Street View to observe street level images and has an array of years to choose from, dating back to the original images taken by google in 2007. Using google maps historical pictures, I identify the arson and vandalism sustained by the businesses in the area start to fade away after two years. Many buildings are rebuilt starting from 2017 and onwards. I hypothesize the following two years after 2014 should have the strongest negative relationship, but after that, the effect will be unclear.

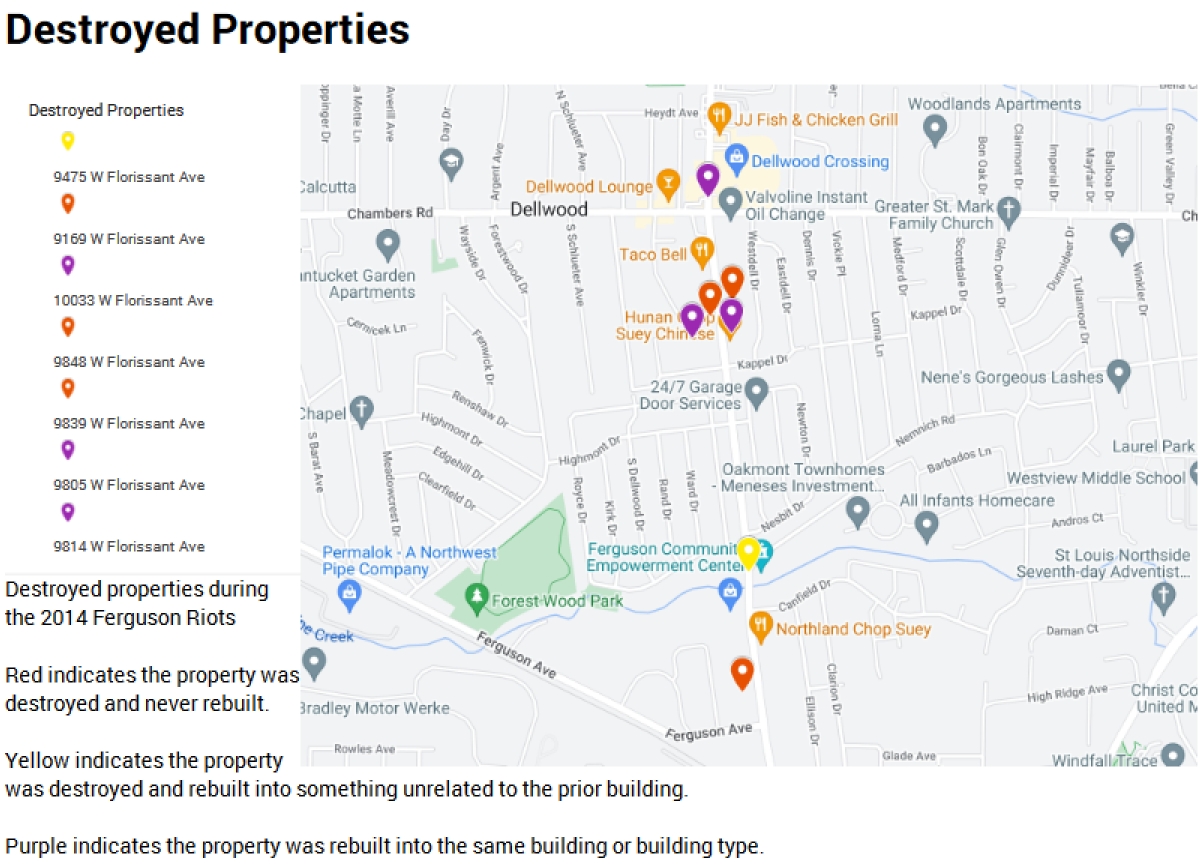

Figure 3: Arson and Vandalism in Ferguson

Table 5 examines the temporal effects of the event. Using the triple interaction terms, I identify which years the discount concentrates around Ferguson in and when the discount dissipates. The discount concentrates in 2015 and 2016, with the discounts declining with years since the event. I use the whole sample for one test, and the 10-mile sample for the other. The 2012-2016 sample did not have enough observations in each year in the first distance set to provide estimates. Figure 4 illustrates the dip in transaction volume around the event which causes this.

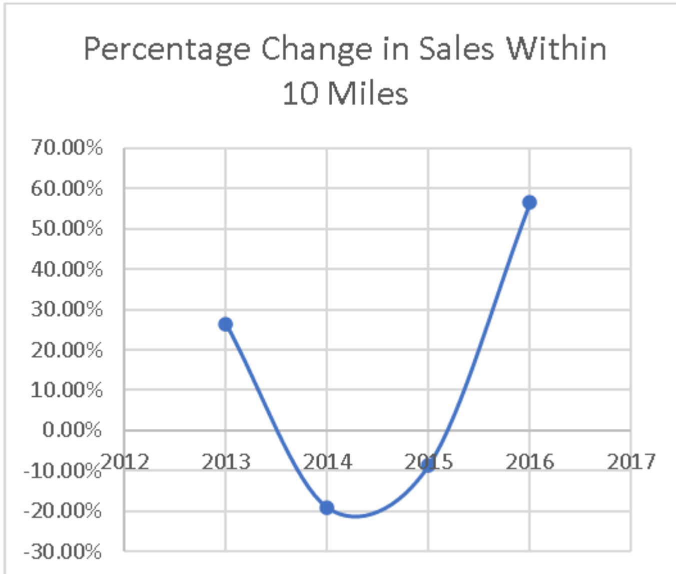

Figure 4: Home Sales in Ferguson

The discount in the first year after the event in the immediate Ferguson area is about 26%, reducing to 21% in the next year, then becoming insignificant statistically for the next two years at 8.6% and 9.5%. In 2019, there is a positive coefficient, indicating the beginning of a recovery period. The coefficient together with the original post coefficient indicates while there is a positive coefficient, the coefficient is not large enough to outweigh the discount. In other words, there is still a discount in 2019, but it is significantly less than the years before. This time frame of a significant discount for 2 years, and then 2 years of level discount before the discount begins to reduce begins is in line with some anecdotal evidence which may support the story the discount is mostly driven by destruction of local amenities. Figure 2 shows the location where a reader can observe the progression of a burned down QuikTrip at 9475 W Florissant Avenue. Google Streetview shows the QuikTrip in 2012 unharmed prior to the events, and the Streetview from late 2014 shows the building destroyed. Streetview in 2015 shows the empty lot after the demolition of the remnants in 2015, and Streetview shows the newly constructed replacement building in 2017: the Ferguson Community Empowerment Center. This example lines up reasonably well with the temporal effects shown in Table 5. There are unreported coefficients, for space concerns, but they follow a similar temporal pattern.

Table 5: Distance Rings Interacted With Years.

	Model (2) Whole Sample	Model (3) Distance < 10 miles
Intercept	15.770*** (129.42)	15.252*** (84.90)
Post Dummy	0.243*** (9.13)	0.204*** (5.04)
Within 1 mile x Post Dummy	-0.479*** (-18.23)	-0.413*** (-10.66)
Within 1-2 miles x Post Dummy	-0.417* (-1.71)	-0.343 (-1.39)
Within 2-3 miles x Post Dummy	-0.243*** (-2.89)	-0.178** (-2.08)
Within 3-4 miles x Post Dummy	-0.005 (-0.02)	0.064 (0.26)
Within 4-5 miles x Post Dummy	- 0.109** (-2.33)	-0.037 (-0.68)
Within 1 mile x Post Dummy x 2014	-0.004 (-0.37)	0.000 (0.01)
Within 1 mile x Post Dummy x 2015	-0.259*** (-50.55)	-0.248*** (-35.90)
Within 1 mils x Post Dummy x 2016	-0.213** (-2.29)	-0.213** (-2.31)
Within 1 mile x Post Dummy x 2017	-0.086 (-1.11)	-0.078 (-0.99)
Within 1 mile x Post Dummy x 2018	-0.095 (-1.24)	-0.096 (-1.28)
Within 1 mile x Post Dummy x 2019	0.192*** (3.47)	0.199*** (3.56)
House Characteristics	Y	Y
House Quality FE	Y	Y
Year FE	Y	Y
Month FE	Y	Y
N	130,173	67,678
R2	0.82	0.82

Although the hedonic model ideally controls for the housing characteristics, it is possible there are some unobserved changes to the sample in the pre-Ferguson portion to the post Ferguson sample. Table 6 shows the univariate difference between the properties transacted before and after the events in Ferguson. As expected because of the inflationary environment, price is higher in the post sample. The houses sold before Ferguson tend to be bigger, have more bathrooms, and newer than houses sold post Ferguson. Table 7 formalizes this issue and indicates there may be an underlying sample bias with a Probit analysis. The table shows there are significant differences between the pre and post samples on a house characteristic level. Bigger houses, houses with less bedrooms, and newer houses are less likely to sell post Ferguson.

Table 6: Univariate differences

Variable	Mean	Std Dev
Pre-Ferguson		
Sales Price	189,404.19	162,611.15
Living Square Feet	1693.86	994.80
Bedrooms	3.05	0.78
Baths	1.96	0.83
Year Built	1963.08	23.97
Post-Ferguson		
Sales Price	228,693.78	175,718.01
Living Square Feet	1586.40	837.20
Bedrooms	3.06	0.74
Baths	1.85	0.80
Year Built	1955.88	28.63
N Pre-Ferguson	127,606	
N Post-Ferguson	22,726	

Table 7: Probit Sample Analysis, Marginal Probabilities

	Sample (1) 2012-2016	Sample (2) Whole Sample	Sample (3) Distance < 10 miles
Living Sq. Ft. x 100	-.0033***	-0.0037***	-0.0025***
# Bedrooms	0.013*	0.0062***	0.0097***
# Baths	-0.0008	0.0536***	0.0603***
Year Built	-0.0015***	-0.0010***	-0.0008***
N	16,082	148,294	67,819

Previously in the literature there have been diverse ways to observe pricing effects in real estate markets. While the first few tests have been a purely hedonic model, the other common option is a repeat sales framework. The repeat-sales framework compares the transactions with their own previous transactions. This way, the house price changes observed are unambiguously the same characteristics of housing. Avoiding any unobserved changes in the sample. If a transaction occurs after the riots began, the variable after will be set to one. If the transaction occurs before the riots began, the dummy variable is set to zero. After this I take the difference between the previous transaction of the same property. Since the characteristics do not change, all but the year and month differences drop out of the equation. Therefore, the dummy variable will equal one if the more recent transaction occurs after the riot and the comparable transaction occurred before the riots. If both transactions occur before the riot, or both transactions occur after the riots, the dummy will subtract to zero in both cases. Here is the equation.

$$\Delta LN(House\ Price) = \alpha + \beta \Delta AfterDummy + \delta \Delta AfterDummy \times \boldsymbol{DistanceRing} - \zeta Year_{Before} + \theta Year_{After} - \eta Month_{Before} + \vartheta Month_{After} + \varepsilon$$

Table 7 shows the results of the repeat sales framework, and the results are robust under this specification. Houses sold in Ferguson before the events and then sold after the event saw a significant reduction in selling price with a coefficient of -0.616 significant at the 1% level after controlling for the year and month of each transaction. This translates to a 46% discount compared to transaction pairs which occurred — both the first and second — before Ferguson and after Ferguson. The results are all significant at the 1% level except for the interaction with houses further than five miles in the Whole Sample. They show a diminishing negative effect until the last group, farthest distance from the riots, turns positive.

Table 8: Repeat Sales Framework

	Model (2) Whole Sample	Model (3) Distance < 10 miles
Intercept	0.472*** (2.87)	0.105 (1.64)
Within 1 mile x Before/After Dummy	-0.616*** (-3.14)	-0.550*** (-2.82)
Within 1-2 miles x Before/After Dummy	-0.331*** (-9.81)	-0.270*** (-7.06)
Within 2-3 miles x Before/After Dummy	-0.226*** (-7.27)	-0.166*** (-4.73)
Within 3-4 miles x Before/After Dummy	-0.195*** (-7.74)	-0.140*** (-4.57)
Within 4-5 miles x Before/After Dummy	-0.219*** (-11.24)	-0.161*** (-6.18)
More than 5 miles x Before/After Dummy	0.002 (0.13)	0.013*** (0.55)
Year FE	Y	Y
Month FE	Y	Y
N	29,423	14,980
R2	0.23	0.25

Conclusion

There have been no robust studies to examine the effects of localized civil disturbances such as the events in Ferguson MO, following the shooting of Michael Brown by police officer Darren Wilson on Aug 9, 2014. The results point to a robust discount in the area following the events with a slow recovery which is still not complete. The effect is robust to various specifications and arrangement of the sample, including local samples and temporally local samples. The effect persists whether using a hedonic pricing model or a repeat sales framework. The results show there was a decline in demand for housing, with the destruction of amenities driving a large part of the effect. There is also a likely effect from an increased level of crime and victimization which reduces demand.

This study contributes to a couple literatures in housing prices. The first is a sparse literature on the effect of civil disturbances. The results in this paper indicate violent rioting have a large and economically significant impact on the affected area, and this effect is long lasting and long reaching. The other literature is the relationship between crime and housing prices. Common sense indicates crime would be an unambiguous negative externality, but scholars have commonly not been able to differentiate what type of crime matters. Is the discount a function of the expectation of crime or is it a function of the actions of crime? In other words, does it matter more what the crime level is in an area, or does the impact of individual crime events matter more? The two are very related and functions of each other, making it hard to distinguish the two effects. In this case, we have a dramatic uptick in crime events making an unambiguous setting of crime actions rather than the general level of crime in the neighborhood, since according to studies on the Ferguson Effect, there is little evidence the crime level changed.

What is driving the effect? The discount can manifest through multiple channels: expectation of property damaging protests in the future could increase. The Ferguson effect claims when there is this type of event, police enforcement decreases, and crime increases. If true, the discount could come from an increased risk and cost of crime. Another way a civil disturbance could discount housing prices is by destruction of local amenities, such as the burning and outing of nearby businesses. This, with the visual evidence gathered in this study, is the driving force of this discount. The businesses recoveries and destruction correlate strongly with the discount level changes temporally. The effects do not have to be completely direct, as it is possible there is an indirect cost of an increased expectation of risk for newer local businesses, disincentivizing local businesses to start up in proximity to the event location.

With the increase of these events in recent years, it is becoming increasingly important to understand the economic implications of events such as the Ferguson Unrest and understanding the exact economic channels can help prevent as much economic damage in the future. Some light implications can be drawn from the results presented here. Local government should step in and incentivize growth to facilitate a quicker recovery for the affected area, given its already disadvantaged nature. Exactly how the government could incentivize growth in the wake of an event like this is left to future research. Solutions future research should consider are ones which solve the root cause of the problem, find ways to disincentivize violence at protests, and more work to economically charge affected areas in the period after.

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ESSAY TWO: SICK AND TIRED: PROTESTS, COVID, AND HOUSING PRICES

Abstract

The onset of the COVID-19 pandemic impacted different communities in several ways. While research regarding COVID-19 and housing prices exist, there are no studies which examine the civil disturbances which erupted following the killing of George Floyd during COVID-19. Using monthly indices from the Zillow Home Value Index (ZHVI) for individual cities across the country, I conduct a difference-in-difference analysis and find a negative and significant effect on housing prices in cities with Black Lives Matter Protests after controlling for regional COVID policies. I also find supporting evidence of an increase in housing demand during and after the lockdown periods, varying for each state. I find a positive effect on housing prices for cities in states which issued a recommended stay-at-home order*,* but no significant effect for cities in states with required stay-at-home orders. I interpret this finding as evidence of a positive demand shock from the stay-at-home orders, however if states enforced the policy as a requirement, the policy disrupted the market enough to no longer be a positive externality.

Introduction

The onset of the COVID-19 pandemic impacted different communities in several ways. Early evidence suggests crime decreased during covid, concentrated in residential crimes, drug crimes, and most violent crimes (Abrams, 2021). While research regarding COVID-19 and housing prices exists (Li & Zhang, 2021) (Yang & Zhou, 2021), there are no studies in the current literature which exploit the decrease in crime to observe the effect of the Black Lives

Matter protests, following the death of George Floyd on May 25, 2020, on housing prices. COVID-19 affected the entirety of the United States, but intensity varied depending on local government responses. Black Lives Matter protests were widespread across the United States, but intensity varied based on local government responses. This contributes to the literature by adding clarity on the effects of riots while also disambiguating the effects of various COVID lockdown procedures. Since these happened concurrently, this is the first paper to sort out the effect of each.

Using monthly indices from the Zillow Home Value Index (ZHVI) for individual cities across the country, I conduct a difference-in-difference analysis to examine the effect of the Black Lives Matter protests on housing prices in the context of COVID-19. I find a negative and significant effect on housing prices in cities with Black Lives Matter Protests after controlling for regional COVID policies. I also find supporting evidence of an increase in net housing demand during and after the lockdown periods, varying for each state. Interestingly, I find a positive effect on housing prices for cities in states which issued a recommended stay-at-home order, but a significant and negative effect for cities in states with required stay-at-home orders. I interpret this finding as evidence of a positive demand shock from the stay-at-home orders, however if the states enforced the policy as a requirement, the policy disrupted the market enough to no longer be a positive externality.

There are multiple way riots may affect property prices, with varying levels of directness. The incident in Minneapolis involving George Floyd may be a signal to the public of unfair administration of justice, challenging the validity of the law. If the public believes the law is unjust, a plausible response is an increased participation in crime. In the COVID-19 era, crime decreased in most categories. Skoy (2020) finds the Black Lives Matter protests decreased fatal interactions between the Black community and police significantly. She also finds no evidence of an increased level of crime resulting from the protests. Shjarback, Pyrooz, Wolfe, & Decker (2017) and Pyrooz, Decker, Wolfe, & Shjarback (2016) investigate the Ferguson Effect, an anecdotal theory where events specific to police behavior cause de-policing ex-post, as officers will decrease enforcement of the law to avoid lawsuits and criticism. If the Ferguson Effect holds true, this could lead to increases in crime rates. The first study by Shjarback et al (2017), finds the Ferguson Effect is not systemic but may exist in specific cities. The second study by Pyrooz et al (2016) finds police officer behavior did indeed change after Ferguson but conclude this change in behavior had no significant effect on violent, property, or total crime rates. These results taken together attributing any discount to the theory suggesting the riots themselves affect the social bonds within a neighborhood directly, independent of the riots' effect on crime.

Literature Review

Abrams (2021) assembles data from 25 large U.S. cities is to estimate the impact of the onset of the COVID-19 pandemic on crime. There is a widespread immediate drop in both criminal incidents and arrests, most heavily pronounced among drug crimes, theft, residential burglaries, and most violent crimes. The decline precedes stay-at-home orders, and arrests follow a similar pattern as reports. Results suggest the orders displaced criminal activity to locations with fewer people and most of the observed changes are not due to changes in crime reporting. Pyrooz, Decker, Wolfe, & Shjarback (2016) find no evidence to support a systematic post-Ferguson change in overall, violent, and property crime trends, although there was much greater variation in crime trends in the post-Ferguson era, and select cities did experience increases in homicide.

Li & Zhang (2021) explore the spatial patterns and heterogeneous distribution of housing price change rates across different areas of the U.S. real estate market during the COVID-19 pandemic and find the changes varied across space in the U.S. They also find COVID-19 made Americans more cautious about buying property in densely populated urban downtowns that had higher levels of virus infection. Skoy (2020) uses a state-by-month fixed effects model and finds evidence that an additional protest in the preceding month leads to a decrease of .225 fatal interactions between Black people and police per 10 million Black population and finds no evidence supporting increased crime or arrests because of the BLM movement.

Wang (2021) uses individual level transaction data and a revised difference-in-differences method with nonparametric smoothing, to study the effect of COVID-19 on house prices. This study finds most cities experienced an increase in housing prices, excluding Honolulu, which is explained by the heightened sensitivity of the service industry. Yang & Zhou (2021) investigate the impacts of COVID-19 on the housing market in the Chang Jiang delta region in China. The findings suggest significantly positive impacts of COVID-19 on housing price in this region, which implies the urgent needs of housing for families to stay together during the epidemic.

Liu & Su (2021) study the impact of the COVID-19 pandemic on the location demand for housing and find that the pandemic has led to a shift in housing demand away from high density neighborhoods. D'Lima, Lopez, & Pradhan (2021) provides novel evidence on pricing effects in housing markets following government shutdown responses to COVID-19 using microlevel data on U.S. residential property transactions and find the average price of a three-bedroom property fell by approximately 1.4% in densely populated locations but increased by about 1.5% in low-density locations where shutdowns were enacted.

Cheung, Yiu, & Xiong (2021) finds there was, respectively, a 4.8% and a 5.0–7.0% year-on-year fall in house prices immediately after the pandemic outbreak after which house prices rebounded after the lockdown period, but the gradient models show that the price gradients were flattened from the epicenter to the urban peripherals. Hoesli & Malle (2021) report COVID19 mostly affected retail and hospitality properties and to a lesser extent office buildings while the residential and industrial sectors have been less affected by the crisis.

Neyman & Dalsey (2021) studies the protests following the killing of George Floyd and find in the 22 days following, there were 326 counties participating in 868 protests, attended by an estimated 757 077 protestors. Ling, Wang, & Zhou (2020) measure of the exposure of commercial real estate portfolios to the increase in the number of COVID-19 cases and find a one-standard-deviation increase in COVID-19 cases on the previous day is associated with a 0.24 to 0.93 percentage points decrease in abnormal returns over 1- to 3-day windows.

Kaklauskas, et al. (2021) presents a literature review on CIRED throughout the pandemic and to look at the responses from the real estate and construction sector. Milcheva (2022) uses the outbreak of the Covid-19 to assess the risk-return relationship in the cross-section of real estate equities in the US and in selected Asian countries and finds during the early stages of the pandemic, the sensitivity of Asian real estate companies to the market becomes negative, while it remains positive and increases in the US. higher, which is not the case in Asia.

Rosenthal, Strange, & Urrego (2022) estimates the value firms place on access to city centers and how this varied with COVID-19. Pre-COVID, across 89 U.S. urban areas, commercial rent on newly executed long-term leases declines 2.3% per mile from the city center. Collins & Margo (2007) examines census data from 1950 to 1980 to measure the 1960 race riots' impact on the value of central-city residential property, and especially on Black-owned property. Both OLS and IV estimates indicate that the riots depressed the median value of Black-owned property between 1960 and 1970, with little or no rebound in the 1970s.

Simpson, Willer, & Feinberg (2018) present a theory proposing a theory suggesting the use of violence leads the public to view a protest group as less reasonable, a perception that reduces identification with the group reducing public support for the violent group. The authors find support using a survey. Gustason (2019) concludes individuals involved in a nonviolent demonstration use the urgency of their needs and the likeliness of collective action to decide whether to initiate violence against the government. A Bayesian multilevel model of 2,405 nonviolent demonstrations from 1991 to 2017 in Africa and Latin America supports the author's hypothesis. Beyerlein, Soule, & Martin (2015) finds that law enforcement agents were less likely to show up at protests when general religious actors, actions, or organizations were present.

Background

On January 20, 2020, the United States reported its first confirmed case of COVID-19. March 19, 2022, California became the first of many states to issue a stay-at-home order. Other states would follow this practice, but on different timelines and with variation in restrictiveness.

There were only seven states in the union – Arkansas, Nebraska, Iowa, South Dakota, North Dakota, Utah, and Wyoming – which did not issue orders to residents to stay at home. As of April 10, 2022, the covid-19 cases numbered 97 million in North America. By the end of May, the United States would attribute 600,853 deaths to COVID-19. Reopening of the states would begin with some restrictions ranging from April all the way to June, with some specific restrictions lasting much longer.

In Minneapolis on May 25, 2020, during the COVID-19 pandemic and during an arrest for suspicion of using a counterfeit twenty-dollar bill, a police officer killed George Floyd. Derek Chauvin, one of the four police officers who arrived on the scene, knelt on Floyd's neck, causing cardiopulmonary arrest, and killing him according to the medical examiner. A witness recorded the event on a cellphone, and the video quickly became a viral sensation. A series of peaceful protests and violent riots would follow globally. These protests would largely take place in 2020, beginning as soon as May 26, 2020. The concurrence of the protests and COVID-19 creates a challenge to disentangle the effects. This paper aims to contribute to the literature regarding both civil disturbances and covid-19's effects on housing prices by disentangling these effects.

Data

This paper employs the use of the Zillow Home Value Index, available on Zillow's website. Numerous studies of real estate, amenity, and other externalities' value such as Holt & Borsuk (2020) or Raymond, Wang, & Immergluck (2015) have used the data. The Zillow Home Value Index is a hedonic index and according to Zillow is optimized for 3 main goals, Timeliness, Comprehensiveness, and Visibility. Prior to 2019, Zillow calculated the Zillow Housing Value Index as the median Zestimate value for a fixed set of homes in each area. Since 2019, Zillow changed the methodology, and retroactively recalculated through the mid-1990s since. After the changes, the average Zestimate within some range of home values determines the index level, which is to be interpreted as the dollar value of a typical home. Changes in the index are a weighted average of the appreciation of individual homes' Zestimate. The weight being proportional to its Zestimate in the previous month. The interpretation in geography is then as the market's total appreciation. I use the city level data in this study, although Zillow provides indices at County and State level, or even Zip Code or Neighborhood levels as well. Zillow provides data from the mid-1990s, but to achieve computational parsimony, the sample will use observations from 2018-2022.

The World Health Organization collects a global database of public health and social measures (PHSM) applied during the COVID-19 pandemic. This dataset is the result of a collaboration between the World Health Organization and London School of Hygiene and Tropical Medicine. The data set is available at the World Health Organization website. The dataset tracks the beginning and end date of distinct types of preventative measures taken by governments at the Federal, State, and Local levels. The dataset contains a comprehensive worldwide set of restrictions, ranging from Afghanistan to Zimbabwe, but I only use restrictions implemented in the United States. Hayashi, et al. (2022) or Zwieg, et al.(2021) have used the dataset and similar datasets, but there are no studies involving the public health and social measures' interaction with financial markets or real estate markets.

Count Love is an initiative started by engineers and scientists Tommy Leueng and Nathan Perkens to document protests, and their details, such as time, place, and how many people participated. Count Love compiles the dataset by crawling local newspaper and television sites daily. The dataset is available countlove.org. This dataset does not include "regular business," such as awareness events, celebrations, historical reenactments, fundraising events, town halls, or political rallies. Count estimates are determined by the most conservative estimate in the articles text. This dataset includes all protests since 2014, large and small. For this analysis, I am focusing on the George Floyd demonstrations, so the events must have occurred after the events involving George Floyd and have the tag "Civil Rights; For racial justice; For greater accountability; Police" Other miscellaneous data include Census data for city populations to control for size.

Results

Based on numerous studies such as Collins & Margo (2007), and other studies on crime events. The expectation is the protests in general will create a negative externality for the local housing index of a city. Although the protests itself in theory should not create any problems if there are no criminal events present, Gustason (2019) indicates peaceful intentioned protest can turn violent and the probability is a function of various characteristics of the locale and attributes of the peaceful protest. In general, the interpretation of the protests is not to be a peaceful demonstration, but it is also not to be a violent riot. The ideal interpretation is a peaceful protest with a heightened probability of violent acts as a function of the protests.

Table 9 presents the first stage results of an OLS regression. The regression formula is as follows:

$$\Delta ZHVI = \alpha + \beta Protest + \gamma Lockdown + \delta PostLockdown + \mu Year \times State + \varepsilon$$

Where the dependent variable is the monthly change in the Zillow Housing Value Index, protest is a dummy which is equal to 1 if there was a protest located in that city in the given month, Lockdown is a dummy equal to 1 if the given city was under any stay-at-home orders given by the state or local government. The specification also includes State by Year fixed effects to capture the local state economic climate for a given year. The results in the first column of Table 9 indicate the presence of a protest reduced the change in housing price index by -0.25% significant at the 99% level. In support of literature on housing prices and COVID-19 such as Wang (2021), I find a strong premium during lockdown and post lockdown at 0.17%, and 0.36%. The second specification in Table 9 examines the effect of larger protests, as there is an argument that a small demonstration is likely not to affect housing prices as strongly as a larger disturbance. In column 2, the model introduces the variable Big Protest, a dummy only if there was a protest in a given city in a given month and the protest was in the top quartile of protests size, at least 2550 participants. In this specification, the statistical significance drops compared to the original protest dummy, but the economic magnitude increases to -0.33% effect on housing prices in the given month. To answer the question of whether the size matters or the mere existence of a protest matters more, the third specification in the table includes both variables. The protest variable subsumes the big protest variable and retains a similar economic magnitude as specification 1, with a reduction of 0.24%.

Table 9: OLS Regression | 2018-2022 | City Level

	Model (1)	Model (2)	Model (3)
Intercept	0.0090*** (23.49)	0.0090*** (23.49)	0.0090*** (23.49)
Protest Dummy	-0.0025*** (-15.67)		-0.0024*** (-15.14)
Big Protest Dummy		-0.0033*** (-4.34)	-0.0009 (-1.18)
Lockdown	0.0017*** (37.58)	0.0017*** (37.10)	0.0017*** (37.58)
Post-Lockdown	0.0036*** (60.32)	0.0036*** (60.03)	0.0036*** (60.32)
Year x State FE	Y	Y	Y
N	803,468	803,468	803,468
R2	25.00%	24.98%	25.00%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. Mean change per month is .35%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

One shortcoming of the sample used in the first tests is it includes every city in the United States, even small remote locations, and the ordinary least squared methodology would inherently equally weight the observations. This creates the possibility of noise, and the possibility the control group, which is the cities without protests, are simply smaller cities with slower housing appreciation than more desirable locales. Table 10 presents the same specifications as Table 3, but only includes cities located within a Metropolitan Statistical Area. The census bureau defines a Metro Area as an urban area with at least 50,000 in population. For example, Winter Park is a city of 29,795 in the Orlando Metropolitan Area, which has a population of 2,673,376. In this case, the analysis would include Winter Park. The largest metropolitan area in the United States is New York-Newark-Jersey City with a population estimate of 19,768,458, and the smallest MSA in the United States is Carson City, Nevada. The results in Table 9 present a similar story to Table 9 with Protest providing a reduction in appreciation by 0.27%, and Big Protests in specification 2 exhibiting a discount of 0.41%. An interesting result to omitting the cities outside of MSAs is in specification 3, Protest no longer subsumes Big Protest fully, with a bigger protest offering an additional discount of 0.15% significant at the 10% level.

Table 10: OLS Regression | 2018-2022 | City Level | Metro Areas Only

	Model (1)	Model (2)	Model (3)
Intercept	0. 0082*** (14.86)	0. 0082*** (14.86)	0.0082*** (14.86)
Protest Dummy	-0.0027*** (-16.08)		-0.0027*** (-15.50)
Big Protest Dummy		-0. 0041*** (-5.03)	-0.0015* (-1.77)
Lockdown	0. 0017*** (33.99)	0. 0017*** (33.46)	0.0017*** (34.00)
Post-Lockdown	0. 0039*** (59.44)	0. 0039*** (59.12)	0.0039*** (59.44)
Year x State FE	Y	Y	Y
N	647,094	647,094	647,094
R2	25.76%	25.73%	25.76%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. Mean change per month is .35%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended. Cities were included only if they were part of a metro area, defined as urban areas of at least 50,000 in population.

The exclusion of cities not in an MSA does fully remediate the argument of smaller cities influencing the coefficients and significance, as some communities within MSAs can be small and remote themselves. To alleviate this issue, I run the specification for cities within a certain range of size. Table 11 presents the results of OLS for the largest 100 cities. When shortening the scope to the largest 100 cities, the coefficients strengthen drastically. The protest dummy now loads at -0.41% in specification 1, and the large protest loads at -0.76%. In specification 3, both Protest and Big Protest are significant at the 99% level. These results indicate on average, a city which experiences a protest will experience housing price depreciation, and if the city experiences a larger protest, it is likely to experience a larger decrease in home values.

Table 11 OLS Regression | 2018-2022 | City Level | Metro Areas Only | Top 100 Cities

	Model (1)	Model (2)	Model (3)
Intercept	0.0041* (1.88)	0.0047** (2.15)	0.0041* (1.87)
Protest Dummy	-0.0041*** (-7.81)		-0.0038*** (-7.00)
Big Protest Dummy		-0.0076*** (-5.30)	-0.0049*** (-3.23)
Lockdown	0.0017*** (3.43)	0.0011** (2.17)	0.0017*** (3.49)
Post-Lockdown	0.0040*** (7.48)	0.0034*** (6.34)	0.0040*** (7.53)
Year x State FE	Y	Y	Y
N	3353	3353	3353
R2	41.33%	40.81%	41.44%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. The mean change per month is .76%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended. Cities were included only if they were part of a metro area, defined as urban areas of at least 50,000 in population.

Table 12 continues analysis in the same manner of thinking, this time restricting the sample to the top 50 cities. The story remains the same, with specification 3 illustrating the main point, with a coefficient on the protest dummy resulting in a -0.31% change in ZHVI. When this is a large protest, it provides an additional change of -0.61% on top of the protest coefficient, resulting in a net effect of around -0.92%, which is economically significant compared to the average change throughout the sample of .35%. The effects are slightly less significant compared to the top 100 cities, indicating the results are not being driven by the top half of the city sizes.

Table 12: OLS Regression | 2018-2022 | City Level | Largest 50 Cities

	Model (1)	Model (2)	Model (3)
Intercept	0.0036 (1.63)	0.0042 (1.88)	0.0036 (1.60)
Protest Dummy	-0.0037*** (-5.23)		-0.0031*** (-4.22)
Big Protest Dummy		-0.0083*** (-5.80)	-0.0061*** (-3.98)
Lockdown	0.0015*** (2.76)	0. 0010** (1.94)	0.0016*** (2.96)
Post-Lockdown	0.0045*** (6.85)	0.0039*** (5.90)	0.0046*** (6.99)
Year x State FE	Y	Y	Y
N	1624	1624	1624
R2	38.62%	38.41%	38.94%

Dependent variable is the monthly percentage change in the housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. The mean change per month is .71%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended. Cities were included only if they were part of a metro area, defined as urban areas of at least 50,000 in population.

In the next set of tests, I explore the differences in the types of lockdown policies enacted by local governments and their interaction between the local housing markets in the context of the protests. Table 13 showcases the results of this split. States which issued a required stay-at-home order still experienced a premium in the housing appreciation, although at a much lower rate than states which issued a recommendation. In all the specifications, requiring lockdown resulted in an increase in housing prices by 0.02%, and recommending lockdown resulted in an increase in housing prices by 0.26%, more than 10 times higher than required and significant at the 1% level for both.

Table 13: OLS Regression | 2018-2022 | City Level

	Model (1)	Model (2)	Model (3)
Intercept	0.0090*** (23.49)	0.0090*** (23.49)	0.0090*** (23.49)
Protest Dummy	-0.0025*** (-16.14)		-0.0025*** (-15.63)
Big Protest Dummy		-0.0032*** (-4.31)	-0.0008 (-1.04)
Lockdown Required	0.0002*** (3.68)	0.0002*** (3.33)	0.0002*** (3.68)
Lockdown Recommended	0.0026*** (50.17)	0.0026*** (49.70)	0.0026*** (50.17)
Post-Lockdown	0.0037*** (62.17)	0.0037*** (61.88)	0.0037*** (62.17)
Year x State FE	Y	Y	Y
N	803,468	803,468	803,468
R2	25.15%	25.13%	25.15%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. Mean change per month is .35%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

Table 14 repeats the analysis but for the largest 100 cities and exhibits a similar story. Recommending lockdown resulted in an increase in ZHVI of about 0.35%, but requiring lockdown did not result in any significant changes in the housing index. I interpret this finding as recommending lockdown caused a higher demand for housing consumption, but if it was a required lockdown, then the lockdown disrupted the market enough to not cause any price changes due to the increased costs of doing business. Table 15 examines the top fifty cities and paints a similar picture. In this set of specifications, the lockdown required dummy resulted in consistently negative coefficient, although not statistically significant.

Table 14: OLS Regression | 2018-2022 | City Level | Largest 100 cities

	Model (1)	Model (2)	Model (3)
Intercept	0.0038* (1.74)	0.0044** (2.03)	0.0038* (1.73)
Protest Dummy	-0.0043*** (-8.35)		-0.0040*** (-7.54)
Big Protest Dummy		-0.0072*** (-5.19)	-0.0043*** (-2.94)
Lockdown Required	0.0000 (0.01)	-0.0004 (-0.67)	0.0001 (0.12)
Lockdown Recommended	0.0035*** (6.38)	0.0026*** (4.60)	0.0035*** (6.32)
Post-Lockdown	0.0043*** (8.27)	0.0037*** (6.93)	0.0044*** (8.29)
Year x State FE	Y	Y	Y
N	3,353	3,353	3,353
R2	41.70%	41.08%	41.78%

Dependent variable is the monthly percentage change in the housing price index from Zillow. T-Statistics are listed in parentheses. *, **, *** represent significance at the 10%, 5%, 1% level. The mean change per month is .76%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

Table 15: OLS Regression | 2018-2022 | City Level | Largest 50 cities

	Model (1)	Model (2)	Model (3)
Intercept	0.0032 (1.43)	0.0038* (1.78)	0.0031* (1.41)
Protest Dummy	-0.0037*** (-5.40)		-0.0031*** (-4.42)
Big Protest Dummy		-0.0076*** (-5.35)	-0.0054*** (-3.55)
Lockdown Required	-0.0007 (-0.89)	-0.0010 (-1.32)	-0.0005 (-0.65)
Lockdown Recommended	0.0035*** (5.18)	0.0029*** (4.19)	0.0034*** (5.10)
Post-Lockdown	0.0049*** (7.61)	0.0043*** (6.56)	0.0050*** (7.71)
Year x State FE	Y	Y	Y
N	1,624	1,624	1,624
R2	39.17%	38.88%	39.41%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. The mean change per month is .71%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

The next set of tests will examine the interaction between the types of lockdowns and the protests. It follows expectations if a protest happens during lockdown, it will have a stronger effect, because the gathering of people would be despite government recommendations or mandates. From there, the expectation is protests during a required lockdown would have a stronger effect than protests during a recommended lockdown period.

To begin the analysis, I run the original specification, but with the protest dummies interacted with the lockdown dummies. When examining the entire sample, protests continue to have a negative and significant coefficient. Table 15 presents the results. The protest dummy has a negative and significant coefficient of -0.09%, but the effect strengthens during lockdown. If the city was under a lockdown order, the protests effect tripled, with the interaction term with a coefficient of -0.18%. This grows worse after lockdown, as the interaction within the post lockdown period is -0.21%, significant at the 1% level. Lockdown and Post-Lockdown dummies remain positive and significant in line with previous tests. When the second specification considers large protests, there is a much larger economic effect, the lockdown period captured the focus. The big protest dummy was statistically insignificant, as is the interaction between the post lockdown period. The interaction between the lockdown period and the big protest on the other hand is -0.46%, significant at the 5% level. When both are added to model in specification 3, the existence of a protest seems to be dominated, and there is no additional effect of a larger protest, in the context of all US cities.

Table 16: OLS Regression | 2018-2022 | City Level

	Model (1)	Model (2)	Model (3)
Intercept	0.0090*** (23.49)	0.0090*** (23.49)	0.0090*** (23.49)
Protest Dummy	-0.0009*** (-15.67)		-0.0010*** (-2.75)
*Protest*Lockdown*	-0.0018*** (-4.45)		-0.0017*** (-4.12)
Big Protest Dummy		-0.0002 (-0.11)	0.0007 (0.38)
*Big Protest*Lockdown*		-0.0046** (-2.12)	-0.0029 (-1.34)
Lockdown	0.0018*** (37.70)	0.0017*** (37.12)	0.0017*** (37.58)
*Protest*Post-Lockdown*	-0.0021*** (-4.47)		-0.0021*** (-4.39)
*Big Protest*Post-Lockdown*		-0.0017 (-0.86)	0.0003 (0.17)
Post-Lockdown	0.0036*** (60.40)	0.0036*** (60.03)	0.0036*** (60.40)
Year x State FE	Y	Y	Y
N	803,468	803,468	803468
R2	25.00%	24.98%	25.00%

Dependent variable is the monthly percentage change in the housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. The mean change per month is .71%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

Examining the top 50 and top 100 cities further yields more results, different in magnitude than the tests with all of the cities in the US. Table 17 again examines the largest 100 cities, and as with previous tests, finds stronger economical results. Protests in the first specification alone do not create any statistically significant results. During lockdown they create a negative and significant effect, as well as in the post lockdown period. The interaction term for the lockdown period translates to a 0.34% reduction in the housing index for cities in which a protest occurred during lockdown. The interaction term with the post-lockdown period has a coefficient of 0.27%, a lower magnitude than the protests during lockdown. This fits the hypothesis that the effects should be stronger when done against authority. The hypothesis garners more support in the second specification. Big Protest has a coefficient of -0.58%, and the interaction with lockdown is -0.36%. The interaction between big protest and post lockdown is not significant in this specification. The hypothesis from before explains these coefficients, but a bigger protest in general is still harmful to the housing prices in the city, but more so during lockdown. The results in the last column suggest lockdowns reduced the effect of big protests, but still was a net negative effect.

Table 17: OLS Regression | 2018-2022 | City Level | Largest 100 Cities

	Model (1)	Model (2)	Model (3)
Intercept	0.0039* (1.81)	0.0047** (2.16)	0.0039* (1.79)
Protest Dummy	-0. 0014 (-1.37)		-0.0012 (-1.16)
*Protest*Lockdown*	-0.0034*** (-2.61)		-0.0031** (-2.40)
Big Protest Dummy		-0.0058*** (-7.10)	-0.0047*** (-4.19)
*Big Protest*Lockdown*		-0.0036* (-1.79)	-0.0016 (-0.75)
Lockdown	0.0021*** (3.90)	0.0011** (2.23)	0.0021*** (3.91)
*Protest*Post-Lockdown*	-0.0027** (-1.97)		-0.0028** (-2.06)
*Big Protest*Post-Lockdown*		0.0020 (1.28)	0.0041** (2.25)
Post-Lockdown	0. 0042*** (7.45)	0.0034*** (6.26)	0.0042*** (7.49)
Year x State FE	Y	Y	Y
N	3353	3353	3353
R2	41.39%	40.83%	41.52%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. The mean change per month is .76%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

When restricting the same type of analysis to the largest 50 cities in the sample, a similar story appears. The coefficients in table 17 suggest small protests before lockdown periods have no significant impact on housing price index but do have a negative effect if the protest occurred during or after a lockdown period. During the lockdown period, protests in the interaction term have a coefficient of -0.31%, but only significant at the 10% level. After lockdown, the effect reduces, as the interaction term has a coefficient of -0.22%. In specification 2, the big protests have a strong and significant negative effect at -0.63%, and during lockdown the interaction coefficient is 0.39%, indicating a strengthening of the effect during lockdown, but no changes after lockdown. The final specification in Table 17 shows results which are slightly weaker statistically, but qualitatively similar to the results in Table 17. The big protests indicator remains robust, with a coefficient of -0.50%. In this sample of the largest 50 cities, it seems there is little difference when the protests occurred, simply whether it was a larger protest or not.

Table 18: OLS Regression | 2018-2022 | City Level | Largest 50 Cities

	Model (1)	Model (2)	Model (3)
Intercept	0.0034 (1.51)	0.0042* (1.90)	0.0033 (1.48)
Protest Dummy	-0.0015 (-1.25)		-0.0013 (-1.03)
*Protest*Lockdown*	-0.0031* (-1.80)		-0.0024 (-1.42)
Big Protest Dummy		-0.0063*** (-6.35)	-0.0050*** (-3.61)
*Big Protest*Lockdown*		-0.0039* (-1.87)	-0.0029 (-1.21)
Lockdown	0.0020*** (3.32)	0.0011** (2.04)	0.0021*** (3.38)
*Protest*Post-Lockdown*	-0.0022* (-1.33)		-0.0023 (-1.33)
*Big Protest*Post-Lockdown*		0.0021 (1.28)	0.0033 (1.61)
Post-Lockdown	0.0047*** (6.88)	0.0039*** (5.77)	0.0048*** (6.97)
Year x State FE	Y	Y	Y
N	1624	1624	1624
R2	38.70%	38.48%	39.04%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. The mean change per month is .76%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

To further sort out the levels of restrictions imposed upon the cities, I separate the types of lockdowns imposed by states. The hypothesis there should be a stronger effect in the case of a lockdown extends to the separation of required lockdown orders and recommended lockdown orders. If it was a required lockdown, the protest was certainly illegal and in violation of the state orders, if it was a recommended lockdown, it is still likely the protest was not in compliance with the COVID-19 protocol, but less of an affront to the law. Therefore, I expect the coefficients to strengthen during required lockdowns and recommended lockdowns, but more so for the required lockdowns. Table 19 begins to illustrate these results.

Table 19: OLS Regression | 2018-2022 | City Level | Largest 100 Cities

	Model (1)	Model (2)	Model (3)
Intercept	0.0036* (1.65)	0.0044** (2.04)	0.0036 (1.64)
Protest Dummy	-0.0013 (-1.25)		-0.0011 (-1.03)
*Protest*Required*	-0.0041*** (-2.64)		-0.0038** (-2.34)
*Protest*Recommended*	-0.0033** (-2.50)		-0.0033** (-2.52)
Big Protest Dummy		-0.0059*** (-6.28)	-0.0050*** (-4.17)
Big Protest Required*		-0.0030 (-1.27)	-0.0003 (-0.14)
Big Protest Recommended*		-0.0022 (-0.60)	-0.0002 (-0.05)
Required	0.0005 (0.70)	-0.0004 (-0.58)	0.0005 (0.75)
Recommended	0.0038*** (6.60)	0.0026*** (4.58)	0.0038*** (6.58)
*Protest*Post-Lockdown*	-0.0030** (-2.26)		-0.0032** (-2.35)
*Big Protest*Post-Lockdown*		0.0020 (1.22)	0.0044** (2.38)
Post-Lockdown	0.0045*** (8.22)	0.0037*** (6.85)	0.0045*** (8.25)
Year x State FE	Y	Y	Y
N	3353	3353	3353
R2	41.78%	41.10%	41.87%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. The mean change per month is .76%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

The first specification in Table 19 examines the results of the protest dummy. In this test, the coefficient on protest dummy is negative but not statistically significant. Both interaction terms are significant with the interaction with required lockdowns at -0.41% and the interaction with recommended lockdowns at -0.33%. The interaction with post lockdown is also significant and negative at -0.30%. These results support the hypothesis stating situations where the protests directly oppose authority magnify the effects. The second specification tells a slightly different story. None of the interaction terms are statistically significant, but the original Big Protest dummy retains a highly negative and significant coefficient of -0.59%. This indicates a story slightly different than the hypothesis, as I expect the bigger protest to have a stronger effect in this situation, but instead it seems there is a tipping point in protest size where restrictions do not matter as much. In the last specification, the results are robust to the original hypothesis, and the interactions between protests and the lockdown types remain statistically significant and qualitative similar, as does the Big Protest dummy.

The interpretation of these results supports the original hypothesis with some caveats. The action of a smaller protest does not have a significant effect on the housing index of a city, but the smaller protests become more meaningful when done in the face of authority, but there is a point when the protest is large enough to have an effect outside of conditions of the locale. In other words, if a protest is large enough, it does not matter whether there are restrictions, the perception is similar. In this specification the effect of large protests weakens after lockdown, dropping from an effect of -0.50% to an estimated -0.06%. An initial overreaction could explain this, or a saturation of protests. Table 20 presents the results for the top fifty cities. Across the board, the effects remain the same qualitatively, although lose much of their statistical significance. There are a couple of possibilities which explain this phenomenon. First, it is possible the loss of power in terms of sample size is enough to render the coefficients insignificant. Second, it is possible the results concentrate in the 50-100 range of city sizes.

Table 20: OLS Regression | 2018-2022 | City Level | Largest 50 Cities

	Model (1)	Model (2)	Model (3)
Intercept	0. 0029 (1.30)	0. 0038* (1.72)	0.0029 (1.28)
Protest Dummy	-0.0014 (-1.21)		-0.0011 (-0.94)
*Protest*Required*	-0.0039** (-1.98)		-0.0033 (-1.60)
*Protest*Recommended*	-0.0016 (-0.90)		-0.0016 (-0.89)
Big Protest Dummy		-0.0066*** (-5.48)	-0.0054*** (-3.57)
Big Protest Required*		-0.0017 (-0.69)	0.0001 (0.03)
Big Protest Recommended*		-0.0070*** (-3.12)	-0.0070*** (-2.84)
Required	0.0001 (0.15)	-0.0009 (-1.22)	0.0002 (0.22)
Recommended	0.0036*** (4.85)	0.0029*** (4.23)	0.0036*** (4.88)
*Protest*Post-Lockdown*	-0.0026 (-1.56)		-0.0027 (-1.57)
*Big Protest*Post-Lockdown*		0.0021 (1.21)	0.0036* (1.71)
Post-Lockdown	0. 0052*** (7.60)	0.0043*** (6.44)	0.0052*** (7.62)
Year x State FE	Y	Y	Y
N	1624	1624	1624
R2	39.28%	38.93%	39.56%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **,*** represent significance at the 10%, 5%, 1% level. The mean change per month is .76%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended.

For robustness, there are unreported tests which look at county level indices instead of city level. The results are statistically significant, but economically weaker than the results looking at the cities. This is unsurprising given the increase scope of the geographic market, the events would have a smaller effect on a more aggregate market. Other definitions of big protest give comparable results qualitatively and quantitatively. Ewing, Kruse, & Wang (2007) run a similar event style study on local housing indices to measure weather events. They cite an autocorrelation issue with time series index studies which is not significant when looking at the cross section as in this study. To err on the side of caution, I perform a Fama-Macbeth style autocorrelation regression for each city in the sample and report the results in the appendix. The results again are statistically significant, if less economically significant.

Conclusion

COVID-19 created a confounding problem in housing markets, where the effects of COVID were almost certainly a positive effect on housing prices due to increasing net demand for housing consumption, however the beginning of COVID-19 in the United States coincided almost perfectly with the incident with George Floyd, and its aftermath of peaceful protests and violent riots throughout the months following. The riots and protests were widespread occurring not only in the Minneapolis area, but nationwide and even some international areas. This provides a challenge for academics attempting to conduct research on either. This paper attempts to sort out the two effects as well as their relationship with each other and other factors. To the author's knowledge, there are no current studies which differentiate between the types of lockdowns instituted by states to exploit the variation in required vs recommended lockdowns.

In this paper, I find evidence to support an increase in net housing consumption demand, as well as evidence presence of protests inhibited the growth of cities' local home value index.

Protests may increase sellers willingness to sell and decrease buyers willingness to buy, while shutdowns decreased sellers willingness to sell. Since these two effects are concurrent, the results contribute by showing there are actually two counteracting effects where the protests were decreasing buyer interest in an environment where sellers willingness to sell was reduced during lockdowns. Protesting during lockdowns magnify this effect, as sellers willingness to sell increase with the additional effect of the challenging of the law. The effects are more economically significant in bigger cities, and the effect is stronger when the protest is large. The presence of a protest inhibited monthly home value growth in a given city by 0.31-0.41%, while larger protests in the top quartile increased this effect to 0.76-0.92%. In general, the results support the hypothesis that these protests have an increased effect during lockdown periods, though larger protests seem less sensitive to whether they occur in lockdown periods or not. Regarding the distinct types of lockdowns enacted by state governments, I find there is no significant effect on housing values for cities in states which had a required lock-down during COVID-19, a novel contribution to the literature on COVID and housing prices. Cities which recommended lock-down do indeed appreciate housing value. I attribute this difference to the disruption of the market a lockdown requirement creates.

Regarding the protests' relationship with the COVID lockdown policies, I find evidence that protests which occurred during a required lockdown period had a stronger negative effect than protests which occurred in the recommended lockdown periods. Like the results in the general lockdown tests, I find the larger protests are not sensitive to this distinction and have a stronger and negative effect regardless of when it occurs. It also seems post lockdown, the effect of larger protests diminishes, either from fatigue or an initial overreaction. In all, this study contributes to both the literature of Civil Disturbances on housing prices, as well as the relationship between COVID 19 and states' policies to combat the pandemic and housing prices.

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ESSAY THREE: SPILL THE WINE: PROTEST SPILLOVER EFFECTS ON HOUSING PRICES

Abstract

Recent research shows there is a significant and negative housing price effect around the epicenter of protests, and within cities which experience the protests. The effects diminish with distance and time, both of which are a function of the nature of the protest (peaceful vs violent) and the size of the protest. In Ferguson, negative price effects are significant for 3 years after the Ferguson Unrest in 2014. With the George Floyd protests, which varied significantly in costly damages and size across the nation, the effects were much more short term, only disrupting the market for approximately a month around the event. One unanswered question is how wide reaching these effects are. Many of the major George Floyd protests occurred in major metropolitan areas with multiple municipalities. Typically, the protests were confined to a couple of these individual municipalities within the metro area. How does the initiation of protests in one city affect the housing prices of the neighboring cities? Do residents move from one city to another? Or do they leave the Metro area entirely? In this paper, I find results which suggest the protests negatively affect the neighboring cities housing markets as well, indicating buyers were trending away from the metro area in its entirety.

Introduction

During the COVID Pandemic of 2020, housing prices surged following a tightening of supply and an increase in net demand. Concurrently with this event were the George Floyd protest following the killing of George Floyd in Minneapolis on May 25, 2020. Previous research has confirmed the price increase during COVID, and previous research has confirmed the protests from the George Floyd incident negatively impacted housing prices. One question unanswered in the literature is the effect protests had on neighboring cities within the same metro area. The origin of the negative impact of prices is not clear. Are people moving their preference to other parts of the metro area? Or is the preference for the entire metro area declining? In other words: How localized is the negative externality of these protests?

To examine the spillover effects, I utilize the same dataset as essay two: a protest data set from Count Love, the ZHVI at the monthly and city level, as well as a dataset of covid restrictions from the NHS, which is granular to the city level, but for the restrictions in this analysis, only the state level is used. What is the expectation? There are two main hypotheses:

H0: Distance and urban barriers insulate neighboring municipalities enough from these events to the point there is no significant effect on the local housing markets, and buyers' expectations about the environment remain unchanged.

H1: Protests affect neighboring cities, and although the distance between the two cities and natural barriers may lessen the effect, there is an economically and statistically significant spillover of the negative externality effect on adjacent cities, more pronounced in the more violent and newsworthy protests rather than the smaller protests.

The results indicate there are spillover effects on other cities within the same metropolitan area, and these effects are weaker than the effects of the protests origin cities. After examining the protests which included a death at any point in that given city's series of protests. I find an economically and statistically significant negative effect on neighboring municipalities when there is a protest in a municipality which experienced a death at some point. What explains this result? Protests which experienced a death are among the most violent and newsworthy of all the George Floyd protests and therefore their effect should be stronger and farther reaching than those which did not experience a death.

Literature Review

The previous two essays in this dissertation provide the conclusions and basis for many of the claims in the following parts of these papers. In the first essay, I find a strong and negative house pricing effect following the violent demonstrations in Ferguson, Missouri in 2014 following the shooting of Michael Brown. The effects are both economically and statistically significant and last for 3-5 years. The conclusion of this paper is the violence caused a strong and negative effect due to the destruction of many amenities through arson, as well as some smaller indirect effects.

The second essay in this dissertation finds a negative effect in housing prices during the month of a protest during the George Floyd protests during COVID. The effects here are short term and do not last longer than the month in which they occurred. This is not surprising considering most of the protests for George Floyd were smaller and non-violent. Only a few national newsworthy cities experienced a strong violent effect and disruption. Essay two also discusses the lead up to the George Floyd experience and other COVID restriction nuances around the time.

Abrams (2021) estimates the impact of the onset of the COVID-19 pandemic on crime in the 25 largest cities in the US. There is an immediate drop in drug crimes, theft, residential burglaries, and most violent crimes. The authors suggest COVID-19 displaced criminal activity to locations with fewer people and most of the observed changes are not due to changes in crime reporting. Pyrooz, Decker, Wolfe, & Shjarback (2016) find no evidence to support a systematic post-Ferguson change in overall, violent, and property crime trends, although select cities did experience increases in homicide.

Li & Zhang (2021) find COVID-19 made Americans shift demand away from properties in densely populated urban downtowns that had higher levels of virus infection. Skoy (2020) uses a state-by-month fixed effects model and finds evidence that an additional protest in the preceding month leads to a decrease in fatal interactions between Black people and police and no evidence supporting increased crime or arrests because of the BLM movement.

Wang (2021) uses transaction data difference-in-differences method with nonparametric smoothing, to study the effect of COVID-19 on house prices. This study finds most cities experienced an increase in housing prices, but not Honolulu, which he argues is because of the heightened reliance on the service industry. Yang & Zhou (2021) investigate the impacts of COVID-19 on the housing market in China and suggest significantly positive impacts of COVID-19 on housing price in this region. Liu & Su (2021) study the impact of the COVID-19 and find a shift in housing demand away from neighborhoods with high population density. D'Lima, Lopez, & Pradhan (2021) use microlevel data on U.S. residential property transactions and find the average price of a three-bedroom property fell by 1.4% in densely populated locations but increased by 1.5% in low-density locations where governments enacted shutdowns.

Cheung, Yiu, & Xiong (2021) suggest that there was a 4.8% and a 5.0–7.0% year-on-year fall in house prices immediately after the pandemic outbreak after which house prices rebounded after the lockdown period but show that the price gradients flattened from the urban epicenter to the urban peripherals. Hoesli & Malle (2021) report that COVID-19 affected retail and hospitality properties the most, while the residential areas have been less affected by the crisis.

Neyman & Dalsey (2021) studies the protests following the killing of George Floyd and find in the 22 days following, there were 326 counties participating in 868 protests, attended by about 757 077 protestors. Kaklauskas, et al. (2021) is a literature review on CIRED throughout the pandemic and examines the responses from the real estate and construction sector. Rosenthal, Strange, & Urrego (2022) estimates the value firms place on access to city centers and how this has changed with COVID-19. Collins & Margo (2007) examines census data from 1950 to 1980 to measure the 1960 race riots' impact on the value of central-city residential property, and estimates indicate that the riots depressed the median value of Black-owned property between 1960 and 1970, with little or no rebound in the 1970s.

Simpson, Willer, & Feinberg (2018) suggest the use of violence leads the public to view a protest group as less reasonable, a perception that reduces identification with the group reducing public support for the violent group. Gustason (2019) asks a similar question: Under what conditions do nonviolent demonstrations escalate to violence? The author concludes individuals involved in a nonviolent demonstration use a measure of their needs and the likeliness of success from collective action to decide whether to initiate violence. Analysis of 2,405 nonviolent demonstrations from 1991 to 2017 in Africa and Latin America supports the author's hypothesis. Beyerlein, Soule, & Martin (2015) that law enforcement agents were less likely to show up at protests when general religious actors, actions, or organizations were present.

Data

The data used in this essay is equivalent to the data used in Essay 2, using the Zillow Home Value Index. As stated before, the Zillow Housing Value Index was calculated as the median Zestimate value for a fixed set of homes in each area. The index has recently undergone a methodology change, once in 2019, and then in early 2023. Analysis for this essay began in 2022, so the data used is that of the 2019 origin. The index measures a weighted average of the appreciation of individual homes' Zestimate proportioned by its value. I continue to use the city level data in this study. The sample period used is from 2018-2022.

The World Health Organization collects a global database of public health and social measures (PHSM) applied during the COVID-19 pandemic. This dataset is the result of a collaboration between the World Health Organization and London School of Hygiene and Tropical Medicine. I use this dataset to control for conditions in the housing market following the pandemic and pandemic lockdowns, and not as a variable of interest.

Protest data comes from Count Love. Count Love is a dataset created by Tommy Leueng and Nathan Perkens to document protests, and their details. Count Love compiles the dataset from local newspaper and television sites daily. This dataset omits what it calls "regular business," including awareness events, celebrations, historical reenactments, fundraising events, townhalls, or political rallies. Count estimates are determined by the most conservative estimate in the articles text. This dataset includes all protests since 2014, large and small. For this analysis, I am focusing on the George Floyd demonstrations, so the events must have occurred after the events involving George Floyd and have the tag "Civil Rights; For racial justice; For greater accountability; Police"

Methodology and Results

During the George Floyd protests across the United States, the level of violence and intensity varied significantly. There were hundreds of protests days across the entirety of the USA, but most were nonviolent. In examining spillover effects, it makes sense to separate the protests into the higher intensity protests rather than looking at the entirety of the United States. To do this, I separate the cities and their respective metro areas which experienced a death due to the protest. The cause of death varies from accident, to shooting. There were 18 cities which experienced a death during the George Floyd protests in 16 distinct metro areas, and a total of 26 deaths, varying from 1-3 in each metro area.

Table 21:Metro Summary Statistics

Metro	# of Cities
Austin-Round Rock-Georgetown, TX	45
Bakersfield, CA	30
Chicago-Naperville-Elgin, IL-IN-WI	307
Davenport-Moline-Rock Island, IA-IL	38
Detroit-Warren-Dearborn, MI	109
Indianapolis-Carmel-Anderson, IN	56
Kansas City, MO-KS	106
Las Vegas-Henderson-Paradise, NV	13
Louisville/Jefferson County, KY-IN	49
Minneapolis-St. Paul-Bloomington, MN-WI	143
Omaha-Council Bluffs, NE-IA	47
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	199
Portland-Vancouver-Hillsboro, OR-WA	63
San Francisco-Oakland-Berkeley, CA	87
Seattle-Tacoma-Bellevue, WA	73
St. Louis, MO-IL	205

Cities were included only if they were part of a metro area, defined as urban areas of at least 50,000 in population.

Table 21 shows the various metro areas included in the dataset and how many individual cities are within each metro. City structure varies across the United States, with some metros such as Las Vegas, are made up of very few incorporated cities whereas some metro's have small, incorporated cities scattered throughout. The largest metro by city count is the Chicago-Naperville-Elgin metro with 307 cities, and the smallest is the Las Vegas – Henderson – Paradise Metro area with 13 cities. These are the MSAs used in the rest of the analysis of the essay.

In most cases, the deaths occurred in the principal city (or one of) in the metro area. For these cases, I identify all bordering municipalities as adjacent. I did not consider Natural barriers to add consistency to the methodology. One thing to note is the municipality borders are often quite irregular and have juts and isles and occasionally will have municipalities within municipalities, (as in fully surrounded). I identify each adjacent municipality manually using Google Maps, which will highlight the borders of the municipality selected. The size of the event municipality and bordering municipalities vary significantly, adding heterogeneity in the types of marked cities, reducing some bias if the marks were entirely correlated with size or other city specific characteristics.

Table 22: City Protests Resulting In Death

City	Metro	# of Deaths
Minneapolis, MN	*Minneapolis-St. Paul-Bloomington, MN-WI*	2
Detroit, MI	*Detroit-Warren-Dearborn, MI*	1
Oakland, CA	*San Francisco-Oakland-Berkeley, CA*	1
St. Louis, MO	*St. Louis, MO-IL*	2
Omaha, NE	*Omaha-Council Bluffs, NE-IA*	1
Kansas City, MO	*Kansas City, MO-KS*	1
Chicago, IL	*Chicago-Naperville-Elgin, IL-IN-WI*	1
Indianapolis, IN	*Indianapolis-Carmel-Anderson, IN*	2
Louisville, KY	*Louisville/Jefferson County, KY-IN*	1
Davenport, IA	*Davenport-Moline-Rock Island, IA-IL*	2
Cicero, IL	*Chicago-Naperville-Elgin, IL-IN-WI*	2
Las Vegas, NV	*Las Vegas-Henderson-Paradise, NV*	1
Philadelphia, PA	*Philadelphia-Camden-Wilmington, PA-NJ-DE-MD*	2
Vallejo, CA	*San Francisco-Oakland-Berkeley, CA*	1
Bakersfield, CA	*Bakersfield, CA*	1
Seattle, WA	*Seattle-Tacoma-Bellevue, WA*	3
Austin, TX	*Austin-Round Rock-Georgetown, TX*	1
Portland, OR	*Portland-Vancouver-Hillsboro, OR-WA*	1

Cities were included only if they were part of a metro area, defined as urban areas of at least 50,000 in population.
Source: Forbes

Table 22 documents the number of deaths in each municipality and its metro area. Seattle proper has the most deaths at 3, while the Chicago metro area has 3 spread across 2 different cities, Chicago proper and Cicero. The most common occurrence for a metro area in the sample was 1 death, typically in the largest part of the metro. Omaha and Bakersfield are among the smallest metro areas included, adding some needed heterogeneity from size, although size is used as a control in robustness tests and results are qualitatively unchanged.

To understand how the protest effects can spill over to neighboring cities, there are three main components. First, I identify the adjacent municipalities. The next step is to sort out the temporal nature of overlap and adjacency. To do this, I use a dummy which is equal to 1 if a city is adjacent to a city which experienced a riot and if the current time is one in which the city is experiencing a protest. This will be the variable of interest. The logic from the previous essay follows here as well. The expectation is the larger protests should still have a larger effect, which should spill over with more ferocity than one which is smaller, even if in both these cases an act of violence occurred.

Table 23: OLS Regression | 2018-2022 | City Level | Fatal Protests

	Model (1)	Model (2)	Model(3)
Intercept	0.0038*** (54.49)	0.0018*** (2.64)	0.0008 (1.11)
Adjacent City x Protest	-0.0027*** (-3.91)	-0.0025*** (-3.77)	-0.0025*** (-3.58)
Adjacent City x Big Protest	-0.0068*** (-6.71)	-0.0056*** (-5.13)	-0.0056*** (-5.27)
Protest Dummy	-0.0039*** (-7.07)	-0.0032*** (-5.89)	-0.0032*** (-5.91)
Big Protest Dummy	-0.0065*** (-5.2of 4)	-0.0068*** (-3.77)	-0.0067*** (-4.17)
Lockdown	0.0032*** (24.11)	0.0014*** (8.17)	0.0014*** (8.17)
Post-Lockdown	0.0069*** (65.87)	0.0056*** (23.06)	0.0056*** (5.27)
Metro FE	N	N	Y
Year x State FE	N	Y	Y
N	78,124	78,124	78,124
R2	13.57%	24.10%	24.49%

Dependent variable is the monthly percentage change in housing price index from Zillow. T-Statistics are listed in parentheses. *, **, *** represent significance at the 10%, 5%, 1% level. The mean change per month is .71%. Lockdown is a dummy which indicates there was a state lockdown order active in the observed month. Post-lockdown is a dummy equal to one for all observations in a state after the final lockdown in the state was ended. Cities were included only if they were part of a metro area, defined as urban areas of at least 50,000 in population.

There are 78,124 month-city observations analyzed in Table 23. The first specification examines the events with no fixed effects. The second specification adds year and state fixed effects, and the third specification adds to that, metro area fixed effects. According to Table 23, adjacent cities to those which experienced deaths during the protests, exhibited a growth rate 0.27% points lower than other cities in the metro area. This discount stays constant even with the introduction of fixed effects, dropping slightly to -.25% in models 2 and 3. The coefficient on protest dummy remains akin to the results found in essay two, with a range of .32-.39% across the models. The results are stronger for the cities in which the main protest occurred, adding credence to the notion this is in fact a spillover effect, which I expect to have less of an impact in line with the hypothesis. R-squared is at 13.57% in the specification, rising with the addition of fixed effects to 24.10% and 24.49% for models 2 and 3. As with the main results of essay 2, the big protests within the sample, (those of which with a top quartile number of attendees of all George Floyd protests), the effect is magnified. Contrary to expectations in the first model, the spillover effect is larger than the measured effect, with a -.68% spillover effect but a -.65% protest effect. Including fixed effects reverses this relationship from a -.58% to a -.67% in line with the hypothesized expectations.

Conclusions

The George Floyd protests in early and mid-2020 were a disturbance within a disturbance, and the full extent of the effects are still not known. While in the second essay within this dissertation the results show there is a negative effect in cities with protests in the given month for the real estate market, and the first essay finds the effect of violent riots spreads far temporally and spatially, not much is known of the spillover effects in the George Floyd demonstrations. In this essay I hypothesize the neighboring cities in the George Floyd riots of cities which experienced a death in a riot should experience a negative spillover effect, which should be weaker than the original effect but follow the same pattern regarding size. I find evidence to support this theory. I find a discount following the hypothesized pattern negatively related to the size of the protest with a smaller but mirrorlike effect for the adjacent municipalities in the given month of the protest.

The results contribute to the literature on civil disturbances and their effect on housing prices as well as the literature relating to the spillover effects of different localized effects which may impact reputational capital of municipalities or a sense of safety, as theorized by many researchers in the literature. The results can be used to strengthen and add evidence to the theory presented by (Simpson, Willer, & Feinberg, 2018), where violence is more likely to cause a group to be looked upon unfavorably by the public. In this case the reaction of lowered housing prices, through a means of lowered demand due to the events nearby caused by the Black Lives Matter organization's protests.

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