ENVIRONMENTAL REGULATION, POLLUTION, AND PUBLIC HEALTH

by

Xiaoxi Zhao

B.A. in Economics and Biomedical English, Peking University, China, 2009

M.A. in Economics, Georgia State University, 2012

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This dissertation was presented

by

Xiaoxi Zhao

It was defended on

March 31st, 2017

and approved by

Randall Walsh, Department of Economics

Werner Troesken, Department of Economics

Arie Beresteanu, Department of Economics

Jeremy Weber, Graduate School of Public and International Affairs

Dissertation Director: Randall Walsh, Department of Economics

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Xiaoxi Zhao, PhD

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In the first chapter, I investigate the effects of the Residential Lead Based Paint Hazard Reduction Act. Enacted in 1996, the lead hazard disclosure policy requires sellers and landlords to disclose known lead-based paint hazards to potential buyers or renters. Employing a difference-in-differences approach, I find evidence that the law prompted some families with children to reallocate toward homes without significant lead risks, increased lead mitigation in rental properties, and reduced blood-lead levels among children in rental properties. However, because white families appear to be more responsive to information disclosure than other groups, the information disclosure law might exacerbate racial disparities in lead exposure.

In the second chapter, I estimate the spatio-temporal dynamics between wildfire and infant birthweight. Exposure to wildfire smoke is determined using the latitude and longitude coordinates of each infant's home address and a fine-scaled, spatial dataset of wildfire smoke plumes re-constructed in GIS from satellite images of the landscape. Using a difference-in-differences estimation strategy, model estimates show that wildfire smoke leads to a 4% to 6% reduction in birthweight. These effects are most pronounced among mothers exposed during their second and third trimesters of pregnancy and attenuate with respect to distance to a fire. We find no statistically significant relationships between proximity to wildfire and the birthweights of infants located outside the path of wildfire smoke.

In the third chapter, I examine the relationship between hurricanes, the salience of flood risk, and residential property investment. Utilizing a difference-in-differences estimation strategy, I find a significant increase in the probability a homeowner invests in a damaged building located in a statutorily designated flood risk area. However, I find no change in the rate of property investment in damaged homes located outside of these areas. Results suggest that a recent storm may elevate households' perceptions of flood risk; however, we show that the primary mechanism driving these changes is a household's exposure to storm damage. We find no evidence of saliency effects in regions less proximate to storm damage. These findings cast doubt on the potential for an information-based regulation to align risk-perceptions with risk-actualities.

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1.0 INTRODUCTION

My dissertation consists of three chapters. "Information Disclosure, Housing Markets and Public Health" contributes to two bodies of literature, one on socioeconomic disparities in lead exposure, as well as emerging literature on information-based environmental regulations, which are often claimed to be more efficient than traditional command-and-control approaches. This paper examines the economic and health effects of the Residential Lead Based Paint Hazard Reduction Act (Title X). An information-based approach to environmental regulation, Title X was enacted in 1996 and requires home sellers and landlords to disclose to potential buyers and renters any hazards related to lead-based paint. The Act applies only to houses built before 1978, however, when the use of lead-based paint was outlawed. Using houses built after 1978 as a control and a difference-in-differences strategy to identify the impact of Title X on various market-related and health outcomes allowed me to reveal several interesting findings: Title X prompted some families with children to move to homes without significant lead risks, increased lead mitigation, and reduced blood-lead levels among children in rental properties. However, because white families appear to be more responsive to information disclosure than other groups, Title X appears to exacerbate racial disparities in lead exposure. The results as a whole suggest that regulation by information can be an effective approach to managing environmental risk, but at the cost of potential distributional effects across racial and socioeconomic groups.

In a second paper co-authored with Shawn McCoy, we estimate the effects of wildfires on infant health. Previous attempts to identify the effects of wildfires and other forms of air pollution on infant health rely on relatively coarse measures of exposure and cannot precisely distinguish between children who were exposed to the contaminant and those who were not. To address this concern, we identify the latitude and longitude of each infants home address and use satellite imagery to locate the trail of wildfire smoke plumes in relation to each infants residence. Using a difference-in-differences estimation strategy, we find that children living within the smoke plume experienced a 4-6% reduction in birthweight, while those near wildfires but just outside the smoke plume experienced no such reduction. These results are important on two levels: First, it is well established that birthweight plays a key role in determining long-term economic outcomes. Our results suggest that birthweight reduction induced by wildfires ultimately translates into a 0.54-0.72 percent decrease in full-time earnings. Second, because children just outside the smoke plume do not experience any detectable adverse effects, our results suggest that maternal stress is not an important pathway to low birthweight in this setting. This result contrasts with findings by Dunkel Schetter (2011), Torche (2011), and Simeonova (2011), who find that maternal stress caused by natural disasters can reduce birthweight. One of the key features of this paper is that we used big data a confidential database detailing the vital statistics and natality records for every infant born in the state of Colorado. As explain below, I will be using this dataset in future projects as well.

In addition to public health implications of natural disasters, I am also deeply curious about how natural disasters affect risk assessments and decision-making. In "City Under Water," my coauthor and I investigate how Hurricane Sandy affected homeowners decisions to invest in their residences. Combining a micro-level data set on household investments, which details the complete history of alterations made to residential structures, with a spatial data set of every property damaged by the storm, we estimate changes in householders perceptions of risk. To do this, we model relative changes in investment between properties in statutorily-designated flood risk areas and properties immediately outside of these areas, restricting attention to structures that were not damaged by the storm. Model results suggest that a recent storm may elevate householder perceptions of flood risk; however, we show that the primary mechanism driving these changes is a householder's exposure to storm damage. We find no evidence of saliency effects in regions further from storm damage. These findings are important because they cast doubt on the potential for information-based regulation to link risk-perceptions with risk-actualities.

2.0 INFORMATION DISCLOSURE, HOUSING MARKETS, AND PUBLIC HEALTH

Since the late 1980s, information disclosure has been widely used to regulate environmental risks, with much of the popularity due to its low cost when compared with command-and-control regulations.[1] In this paper, I examine the effectiveness of information disclosure as a policy tool by studying the impacts of the Residential Lead-Based Paint Hazard Reduction Act (Title X). Introduced in 1996, Title X requires sellers and landlords to disclose known lead-based paint hazards to potential buyers or renters. Because lead is a neurotoxin that impairs cognitive and behavioral development, and because the most significant source of lead exposure is lead paint in old houses (EPA, 2012, Lanphear et al., 1998, Binns et al., 2007), Title X may have significant effects on housing markets and public health. Despite this potential significance only one study, Bae (2012), provides econometric evidence of Title X's effects.[2] One particularly important question that has yet to be addressed is how Title X, and information disclosure regulation more generally, might reinforce socioeconomic disparities. In the case of lead, it is widely recognized that African American children face the greatest risks (Markowitz and Rosner, 2013). If disadvantaged socioeconomic groups are less able to exploit information disclosure than others, such laws might exacerbate differences.

In this paper, I offer a detailed and comprehensive econometric evaluation of Title X.

[1] One example of information disclosure regulations is the Toxics Release Inventory (TRI) which was established in 1986 requiring facilities whose toxic emission exceeds certain threshold to disclose annual emission of toxic chemicals. Another example, Natural Hazards Disclosure Act was enacted in 1998 in California which requires real estate seller and brokers to disclose "if the property being sold lies within one or more state or locally mapped hazard areas."

[2] In this paper, Bae (2012) finds that information disclosure increases the probability of home buyers lead testing and decreases the probability of having peeling paint in old homes. Bae also finds that the policy reduces the instances of households with young children occupying old homes. Finally, Bae shows that information disclosure does not result in a substantial switch from old houses to new houses for any socioeconomic group.

The analysis is based on a difference-in-differences estimation strategy that exploits the fact that Title X only applies to homes built before 1978, when the residential use of lead-based paint was banned. Homes built before 1978 are assigned as treated properties, while homes built after 1978 are assigned as control properties. In this way, for instance, a comparison of sale prices between treated and control properties sold before and after 1996 - when Title X was enacted - offers a causal estimate of information disclosure on sale prices. This strategy allows me to assess the effects of Title X on various market-related and health outcomes, while controlling for contemporaneous shifts in labor and housing markets, the age of the housing stock, and preferences over the age of homes. Not surprisingly, I find that homeowners with children are more likely to move away from lead risky properties, since children are the group with the greatest risk of lead poisoning. Following information disclosure, homeowners are shown to be not only more aware of lead-based paint, but also lead hazards from pipes and solders. Education and income play important roles in how homeowners respond to information disclosure, with high-income and educated families more responsive to disclosure.

I also explore the possibility that Title X had differential effects on owner and rental markets, as the two markets are fairly distinct (Glaeser and Gyourko, 2007). Not surprisingly, I find different responses among renters, where there are no measurable effects on the demographic composition of properties. In addition, results suggest that landlords mitigate lead hazards following the law, and that disclosure significantly reduces blood lead levels (BLLs) among the children of renters. I show suggestive evidence that the decrease in BLLs can be attributed to a reduction in homes' lead dust levels.

One key aspect of the lead poisoning problem is the high incidence rate among low-income, minority families (CDC, 2013, Jacobs et al., 2003, Gaitens et al., 2009, Lanphear et al., 1996, Lanphear et al., 1998, Lanphear et al., 2002). Elevated BLLs have been shown to lower IQ and educational attainment, as well as cause behavioral problems and increased criminal activity. Therefore, understanding the racial and socioeconomic disparities in BLLs can help to explain other gaps across groups.

Previous research has shown that minority children are more likely to be exposed to pollutants. One reason is that their mothers are less likely to move away from harmful sources of pollution when new information about toxic releases around their homes becomes available (Currie, 2011, Currie and Walker, 2011). This may result from the fact that they are "less aware of them, less able to move, or perhaps more concerned about other problems in their lives" (Currie, 2011). For similar reasons, Title X may not benefit minority children. My research shows that, although minority children are the group most at risk, white homeowners with children, especially those with higher education, are much more likely to move away from lead risky properties.

2.1 BACKGROUND

Lead has long been recognized as a harmful environmental pollutant. Many neurodevelopmental studies have shown that developing fetuses, infants, and children are at the greatest risk of lead exposure.[3] In 1991, US Secretary of Health and Human Services called lead the "number one environmental threat to the health of children in the United States" (Goldman, 1997, EPA, 2012). Figure (1) shows the percentage of children under six that have BLLs above 5 g/dL over years, the reference level at which the Centers for Disease Control and Prevention (CDC) recommends public health actions be initiated (CDC, 2013).[4] According to a 2013 estimate from CDC (2013), approximately half a million US children ages one to five have BLLs above 5 g/dL.

A long list of epidemiological literature provides evidence on the link between lead exposure and both cognitive and non-cognitive outcomes. Because lead exposure is often associated with socio-environmentally disadvantaged families, disentangling confounding factors is a major challenge to identifying a causal relationship between exposure and relevant outcomes. Even so, after controlling for confounding effects, an overwhelming majority of studies continue to find associations of lead exposure with intellectual ability, even at a rela-

[3] see Bellinger (2011) for a detail neurological literature review on lead hazard

[4] In 1991, CDC defined BLLs $\geq 10\ \mu g/dL$ as the "level of concern" for children aged 15 years. However, in May 2012, CDC accepted the recommendations of its Advisory Committee on Childhood Lead Poisoning Prevention (ACCLPP) that the term "level of concern" be replaced with an upper reference interval value defined as the 97.5th percentile of BLLs in US children aged 15 years from two consecutive cycles of the NHANES. Recent studies suggest that lead has detrimental effects even at a very low level.

tively low level of exposure (Bellinger et al., 1992, Canfield et al., 2003, Dietrich et al., 1993, Grandjean et al., 1991, Lanphear et al., 2000, Lanphear et al., 2005, McMichael et al., 1994, Stiles and Bellinger, 1993,Needleman et al., 1990). In particular, Ferrie, Rolf and Troesken (2012) exploits variation in the use of lead water pipes across time and place to estimate a negative relationship between exposure to leaded water at an early age and later intelligence test scores among WWII army enlistees.

Significant associations have also been reported in academic achievement (Bellinger et al., 1992, Bellinger et al., 1994, Chandramouli et al., 2009, Fergusson and Horwood, 1993, Fergusson et al., 1993, Fulton et al., 1987, Needleman et al., 1990, Yule et al., 1981). Notably, exploiting a unique individual-level longitudinal dataset that links preschool BLLs and students' test scores, Aizer et al. (2016) find that a 5 $\mu g/dL$ increase in child lead levels reduces test scores by 30-60 percent of a standard deviation. Furthermore, the gap in BLLs relates to the gap in test scores of children across racial groups.

Recently, increased attention has been directed to examining behavioral problems in children exposed to lead. Children exposed to lead in early childhood are more likely to behave in an impulsive, aggressive, antisocial, and delinquent manner (Bellinger et al., 1994, Byers and Lord, 1943, Dietrich et al., 2001, Feldman and White, 1992, Needleman et al., 1996, Mendelsohn et al., 1998). These childhood behavioral problems often lead to violent or criminal behavior later in life (Reyes, 2015). Indeed, recent economic research has shown the link between childhood lead exposure and increased criminal activity later in life (Feigenbaum and Muller, 2016, Grönqvist et al., 2014, Masters et al., 1998, Mielke and Zahran, 2012, Nevin, 2007, Nilsson, 2009).

Today, the most significant source of lead exposure comes from lead-based paint (EPA, 2012, Lanphear et al., 1998, Binns et al., 2007), as over 4 million households with children contain high levels of lead (Jacobs et al., 2002). Lead-based paint first came into use in the early 17th century and quickly became popular due to its durability and resistance to moisture (Warren, 1999). Although researchers began documenting lead poisoning in children as early as the 1900s, the US government did not curb residential lead paint use until 1978 (Gibson, 2005). In 1978, the US Consumer Product Safety Commission banned paint containing more than 0.06 percent lead (by weight of dried product) for residential use (16 Code of Federal Regulations CFR 1303).[5] Notably, the ban only applied to the new paint, and did not affect the existing housing stock. In addition, lead remains in a house permanently unless the lead-based paint coat is completely removed. As a result, 64 million housing units in the US, approximately 75% of the total housing stock, contain lead-based paint as of 1996, and there are about 1.7 million children who have BLLs above the old safe limits (10 $\mu g/dL$), mostly due to exposure to lead-based paint (EPA and HUD, 1996).

In this context, the US Environmental Protection Agency (EPA) and the Department of Housing and Urban Development (HUD) jointly issued a regulation in March 1996, under Title X: The Residential Lead-Based Paint Hazard Reduction Act of 1992, Section 1018. Title X requires sellers and landlords of homes built before 1978 to disclose known lead-based paint and to provide available reports to buyers or renters of the property prior to purchase or rental. Title X also requires that the potential buyer or tenant be given the lead information pamphlet, "Protect Your Family From Lead In Your Home", which contains low-cost tips on identifying and controlling lead-based paint hazards.[6] Title X also allows home buyers to get a 10-day period to conduct a lead-based paint inspection or risk assessment. Sellers, landlords, and real estate agents are responsible for compliance. Title X took effect on December 6, 1996.

According to a CDC estimate (2013), 2.1 percent of children who lived in homes built after 1978 have BLLs of more than 5 $\mu g/dL$, compared with 18.4 percent who lived in older houses. This suggests that the high incidence rate of lead poisoning in minority children may result from high residency rates in poorly maintained, older homes that still contain lead-based paint. However, complete avoidance of the old homes as residential choices is not an optimal response to Title X: maintaining paint surfaces in good condition is sufficient to eliminate lead paint hazard. Moreover, the high-cost permanent removal of lead paint is not only inefficient but also often ineffective, as the process might leave lead paint dust or soil tainted with lead, both of which are even more difficult to remove.

According to estimates in the Presidents Task Force (2000), the cost of interim controls of lead-based paint is about $1,200 per housing unit. For full abatement of lead-based paint,

[5] In 2009 that limit further dropped to 0.009 percent.

[6] The pamphlet is available in both English and Spanish.

the estimated cost is $10,800 per housing unit. The abatement varies due to variation in abatement requirements, regional differences in costs, housing stock condition, and local regulations. Korfmacher (2003) estimates the national average cost of making housing lead-safe as $7,000 per unit.

It has been well documented in the public health literature that minority children are more likely to suffer from lead poisoning than white children. Figure (2) shows the racial disparities in lead poisoning using National Health and Nutrition Examination Surveys. Between 1991 and 1994, African American children showed an 11.2 percent incidence rate of lead poisoning (defined as BLLs $\geq 10\mu g/dL$), while white children reported a 2.3 percent incidence rate (CDC, 1997). An assessment of Mexican-American children showed that approximately five percent of children of all ages still have BLLs above $10\mu g/dL$ at the same time (Pirkle et al., 1994). According to the 1999-2002 NHANES, African American children showed an 18.5 percent incidence rate of lead poisoning (defined as BLLs $\geq 5\mu g/dL$), while white children reported a 7.1 percent incidence rate (CDC, 2013). Lanphear et al (2002) concludes that differences in housing conditions and exposures to lead-contaminated dust contribute strongly to the racial disparity in children's BLLs. These disparities are consistent with a large literature on environmental inequalities. In the next section, I will provide a simple model to discuss when and how information disclosure may change housing prices and demographic compositions of occupants in lead risky houses.

2.2 A SIMPLE MODEL OF PRICE, WILLINGNESS TO PAY, AND SORTING

To better understand the link between housing prices, and residents occupancy choices, I formulate a simple theoretical model of preference-based sorting in response to information disclosure. Using this framework, my model can provide insights regarding the relationship of dynamics between price change, and sorting behaviors of heterogeneous buyers.

I consider an economy comprised of a continuum of individuals of measure one who choose to live in one of the two types of houses $j \in \{f, r\}$. I define f as a lead free house and r as a lead risky house. A lead risky house has a positive probability to contain lead. Houses are identical within types. Prior to information disclosure, these two types of houses are substitutes. Denote the price premium of a type f house over a type r house as $p = \bar{p}_f - \bar{p}_r$. I fixed the price of a type r house at $\bar{p}_r$, such that the price premium p adjusts endogenously to clear both housing markets. Suppose there are two types of individuals in this economy, $i \in \{c, n\}$, with each type of population N_i. Note that $N_n + N_c = 1$. I define c as a type of individuals who care about whether their house has lead and type n as those who do not. Each individual has an exogenously determined willingness to pay for the price premium, WTP_i whose distribution can be described by $F_i(\cdot)$, a strictly increasing cumulative distribution function (CDF).

I assume that the economy contains a unit measure of housing supply, with $q_f + q_r = 1$. Since any individual with $WTP_i \leq p^*$ prefers a type r house, in equilibrium the price premium adjusts endogenously to satisfy the equilibrium condition:

$$N_n F_n(p^*) + N_c F_c(p^*) = q_r. \tag{2.1}$$

That is, p adjusts such that the share of individuals satisfy $WTP_i \leq p^*$ exactly equals to the share of the housing supply of type r.

To conceptualize the effects of information disclosure, I assume that WTP_c shifts to the right by γ as a result of information disclosure; at the same time, the distribution of WTP_n does not change. This assumption indicates that individuals who consider lead as a nuisance increase their willingness to pay for a type f house once they realize a type r house may contain lead; while those who are indifferent to the presence of lead in their house do not change their willingness to pay following disclosure.

After disclosure, the price premium, p, adjusts to satisfy the equilibrium condition:

$$N_n F_n(p^*) + N_c \tilde{F}_c(p^*) = q_r, \tag{2.2}$$

where $\tilde{F}_c(\cdot)$ is a shift of the $F_c(\cdot)$ to the right γ units. As such, the new market clearing condition post disclosure is equivalent to

$$N_n F_n(p^*) + N_c F_c(p^* - \gamma) = q_r, \tag{2.3}$$

With this framework in place, I make two observations regarding how equilibrium changes following information disclosure.

Observation 1: Lead free houses become more expensive following information disclosure, and the changes in price are smaller than the changes in type c individuals' willingness to pay for a lead free house. Observation 1 follows directly from the following. First, I take a partial derivative with respect to γ in equation (2.3), I obtain

$$\frac{\partial p^*}{\partial \gamma} = \frac{N_c F_c'(p^* - \gamma)}{N_n F_n'(p^*) + N_c F_c'(p^* - \gamma)} \tag{2.4}$$

All terms in equation (2.4) are non-negative; as such, $dp^*/d\gamma$ is greater than zero. This suggests that lead free houses become more expensive, when there is a positive shift of γ following disclosure. In addition, $dp^*/d\gamma$ is also no larger than one. That being said, one unit change in willingness to pay for a lead free house leads to less than one unit change in price. This suggests that price effect is an underestimate of changes in willingness to pay.

Observation 2: Information disclosure leads to resorting. The share of type r houses being occupied by type c individuals is

$$S_r^c = \frac{N_c F_c(p^* - \gamma)}{N_c F_c(p^* - \gamma) + N_n F_n(p^*)} = \frac{F_c(p^* - \gamma)}{F_c(p^* - \gamma) + \frac{N_n}{N_c} F_n(p^*)}. \tag{2.5}$$

This share depends on the CDF of WTP_i and the ratios of the population of types of individuals. Keeping everything else constant, a higher N_n/N_c leads to a lower share of type c individuals living in type r houses.

Now, I take a look at how disclosure affects resorting. Take the derivative of S_r^c w.r.t γ, we obtain

$$\frac{\partial S_r^c}{\partial \gamma} = -\frac{N_n F_n'(p^*)}{N_c F_c(p^* - \gamma) + N_n F_n(p^*)} = -\frac{N_n F_n'(p^*)}{q_r} \times \frac{\partial p^*}{\partial \gamma}. \tag{2.6}$$

Since $\partial S_r^c/\partial \gamma$ is non-negative, S_r^c decreases with γ: the share of type r houses being occupied by type c individuals decreases after disclosure. By the same token, the share of type r houses being occupied by type n individuals increases as a result of disclosure. The magnitude of resorting effect is determined by the population at margin, $N_n F_n'(p^*)$. Notably, when N_n is large, the effect of disclosure on sorting is large, with a small price effect.

To summarize the theoretical results, the predictions of my model allow me to interpret price and sorting responses following information disclosure. My model predicts a price change and changes in compositions of occupants. Notably, the change in price is smaller than the change in willingness to pay, in line with the empirical evidence by Kuminoff and Pope (2014). In addition, when the share of individuals who are indifferent to lead is large, a small effect on price and a large effect on sorting are expected.

2.3 DATA

2.3.1 American Housing Survey

The first data set in this study comes from the American Housing Survey National Sample (AHS) 1993 to 2005. The AHS is used to estimate the market and risk mitigation responses to Title X. Designed to be representative of the housing stock in the US, the AHS is a panel of database that tracks roughly 55,000 randomly selected houses every other year. The survey collects information about self-reported house prices (if owner occupied); rents (if renter occupied); dwelling characteristics (e.g., metropolitan area of the house, the type of house, number of bedrooms, number of bathrooms, year built); occupant characteristics (e.g. age , race, education, income); when the current occupants moved in; whether there is peeling paint in the house; and whether the occupants tested for lead-based paint when purchasing the properties. The AHS does *not* collect information about whether a house contains lead-based paint.

Table (1) presents summary statistics of the AHS data for this analysis. The data is subdivided into owner and rental markets. Column (1) in Table (1) shows the summary statistics of owner occupied housing units. Columns (2) - (4) present summary statistics of the sample that this paper focuses on. Column (3) shows characteristics of houses built before 1978 - houses at risk of lead - which forms the treatment group in this paper; Column (4) shows characteristics of houses that are free of lead risk. Comparison between Columns (3) and (4) suggests that houses built between 1975 and 1978 and those built between 1979 and

1982 are statistically identical in all characteristics.[7] Columns (5) - (8) present summary statistics in renter occupied housing units. Similarly, a comparison between Columns (7) and (8) suggests that houses built before and after 1978 are similar in terms of housing characteristics. For the remainder of this paper, I'll refer to observations in Columns (2) and (6) as the "study sample."

Table (1) also provides evidence for disparities between owners and renters in the characteristics of their housing units. Owner occupied units are more likely to have more bedrooms, bathrooms, and square footage. In addition, homeowners are more likely to be white, with higher education and longer tenure in their properties.

To explore the effects of Title X information disclosure, I constructed a "new occupant sample" from the survey information. The AHS samples the same houses every other year regardless of whether a house's occupants have changed. The new occupant sample is constructed by taking the entire AHS sample and selecting only the houses that have changed occupants between surveys. Whether the occupant has changed is determined to have occurred if the move in date was after the previous survey date and the survey respondent was not reported as part of the same household as in the previous survey. For owner markets, the new occupant sample is the sale data: each observation is a unique housing transaction, together with the new homeowner's characteristics. Similarly, for rental markets, each observation in the new occupant sample contains information about new rental rate, and new tenant information. Using the new occupant sample, I can analyze not only how price responds to Title X information disclosure, but also who moved into the lead risky and non-risky houses.

2.3.2 National Health and Nutrition Examination Survey

The second data set in this study is from the National Health and Nutrition Examination Survey (NHANES). The NHANES is a nationally representative cross-sectional household survey, which uses a complex, stratified, multi-stage probability sampling design to track the health of civilian US population. The NHANES conducts a nationally representative sample

[7] I cannot reject the null hypothesis that characteristics of pre- and post-1978 houses are identical at the 5% level.

of about 5,000 people each year and collects information on demographics, socioeconomic status, housing characteristics, and health.

I examine four waves of the NHANES data for children aged under six years old at the time they first moved into current homes (1999-2000, 2001-2002, 2003-2004, 2005-2006). The NHANES - which has been the primary source of information about the national distribution of children's BLLs - provides BLL estimates for population subgroups by age, sex, race/ethnicity, etc. The NHANES also contains information about respondents' housing characteristics, including year built; year moved in and lead dust level in the house. One point worth mentioning is that, the NHANES and the AHS cannot be linked. In this paper, I analyze these two datasets separately.

The NHANES has collected separate single-surface floor dust lead loading samples from the room where children spent most of their time while awake. Lead poisoning is often caused by lead dust from lead-based paint, and even very small amounts can be devastating health hazards. I use floor dust lead loadings as a measure of lead risk mitigation effort.[8] The appropriate sample weights for combined NHANES 1999-2006 were constructed using National Center for Health Statistics guidelines (NCHS, 2013). Table (2) presents the weighted and unweighted summary of statistics from the NHANES.

Column (1) in Table (2) shows the summary statistics of children living in owner occupied properties. Columns (2) - (4) present summary statistics of the sample that form the analysis of this paper. The NHANES does not report specific year built information. Instead the surveys collect a range of years built such as whether the house was built between 1960 and 1978 or between 1979 and 1989. Columns (3) and (4) display the characteristics of children living in lead risky and non-risky houses, respectively. Lead dust levels and average BLLs are lower in post-1978 homes.[9] Comparisons between Columns (1) and (5) suggest that rental units are more likely to have lead-based paint hazards than owner occupied units. In addition, children living in renter occupied houses tend to have higher BLLs than those in owner occupied houses.

[8]Details of the NHANES protocol, survey and analytical procedures, and handling of samples are available at: http://www.cdc.gov/nchs/nhanes/nhanes_questionnaires.html

[9]Lead dust in households results not only from deteriorating lead-based paint, but also other sources outside houses, such as industrial pollution and soil contaminated by exteriors of lead-based paint and past use of gasoline.

2.4 ESTIMATION APPROACH

2.4.1 Impacts on Sale and Rental Prices

Housing prices serve as a useful way to track people's willingness to pay to avoid disamenities. I investigate the impact of the Title X's information disclosure policy on housing markets using hedonic models of residential sale and rental prices. Contemporaneous shifts in local and macroeconomic housing and labor markets complicate the identification of information disclosure on housing and rental prices. A further complication is that older houses and newer houses are systematically different in many ways that are difficult to account for using data.

To overcome these empirical challenges, I employ a difference-in-differences estimation strategy. I assign homes to the treatment group (labeled Risky in the model) based on their years built, with houses built between 1975 and 1978 being treated. Houses built between 1979 and 1982 are assigned to the control group.[10] I then compare sale and rental prices in the treatment group to those in the control group before and after Title X disclosure policy was enacted (labeled "Disclosed" in the model). To control for confounding factors mentioned above, I use data with years sold a few years before and after 1996 when the information disclosure rule was enacted.

The hedonic model for price effects takes the form:

$$\begin{aligned} y_{it} = \beta_1 Risky_i + \beta_2 Disclosed_{it} + \beta_3 Risky_i \times Disclosed_{it} \\ + \alpha X_{it} + \delta_i + T_i + (\delta_i \times T_i) + \epsilon_{it}, \end{aligned} \tag{2.7}$$

with y_{it} is the log form of price: sale price or rent. $Risky_i$ is an indicator set equal to one if a house is built before 1978, zero otherwise.[11] $Disclosed_{it}$ is an indicator set equal to one if a family moved into the property after 1996; if a family moved into a pre-1978 property after 1996, they should have been informed about the potential lead hazard in the property. X_{it} includes a set of housing characteristics including the number of bedrooms, number of bathrooms, lot size, lot size squared, square footage, square footage squared,

[10] For houses built before 1975, the AHS documents decade built instead of detailed year built. The AHS also only indicates whether a house is built between 1975 and 1978 without further detailed year built.

[11] I do not include observations from Maryland, since Maryland banned lead-based paint prior to 1978.

number of porches, number of garages, stories of the property, and house age at the time the occupants moved in. T_i controls for the sale year (or move in year) fixed effect. δ_i is a metropolitan statistical area (MSA) fixed effect. To account for heterogeneous housing market shifts across MSAs, I also control for the sale year (or move in year) by MSA fixed effect for owner markets: $\delta_i \times T_i$. Controlling for these fixed effects allows me to compare the prices of old and new houses within a small spatial and temporal range. Standard errors are clustered at the MSA and treatment group levels. The coefficient of interaction between $Risky_i$ and $Disclosed_{it}$ is the coefficient of interest which captures the casual effect of the information disclosure on prices.

It is worth noting that I only select houses that have changed hands between surveys for rental properties. The first reason I dropped returning respondents is that typically landlords can only modestly increase the rent, if at all. Therefore, using multiple years of rents from the same renters does not add information about rental prices. The second reason is that the disclosure rule has more stringent requirements for new leases compared to lease renewals.[12] It is reasonable to believe that, compared to signing a new lease, renters living in pre-1978 houses are less likely to receive the information treatment when they renew a lease. As such, the effect on rental prices captured in equation (2.7) is the effect on new lease rents.

Unlike sale prices, rents are reported in each survey. To take advantage of rich information on rental prices, I also use all the rental properties across all years and estimate the hedonic model with house fixed effects to account for any unobserved heterogeneity among individual houses.[13] An additional hedonic model for price effects in the rental market takes the form:

$$y_{it} = \beta_1 Disclosed_{it} + \beta_3 Risky_i \times Disclosed_{it} + \alpha X_{it} + \tau_i + T_i + \nu_{it}, \tag{2.8}$$

where τ_i represents house fixed effects, and all other variables are defined the same as in equation (2.7).

[12] The disclosure rule does not require repeated disclosure during the renewal of existing leases in which the lessor has previously disclosed all required information and no new information has come into the possession of the lessor.

[13] The EPA and the HUD interpret renewal to occur at the point when the parties agree to a significant written change in the terms of the lease, such as a rental rate adjustment. The disclosure requirements apply to any new information obtained subsequent to the original disclosure. For this reason, I also examine the hedonic model selecting houses that have changed rents between surveys. This approach is similar to the model presented in equation (2.7), but with an additional sample. The regression results are statistically very similar and are available upon request.

2.4.2 Impacts on Demographic Composition of Lead Risky Houses

It is important to understand whether and how homeowners and renters change their residential choices as a result of information disclosure. Following Bae (2012), I adopt the random bidding model (RBM) introduced by Ellickson (1981). The RBM assumes that houses choose their occupant's based on their bidding prices; the regression model places occupants characteristics on the left-hand side of the equation as a dependent variable, while housing characteristics on the right-hand side of the equation act as explanatory variables. A linear probability model is adopted, and the econometric framework takes exactly the same form as equation (2.7).

Variables on the right hand side are defined in the same way as in equation (2.7). y_{it} is a dummy variable that describes certain family characteristics, including the educational attainment of family head, race of family head, whether there are kids under six years old living in the house, and combinations of these characteristics. I use a linear probability model to estimate the causal effect of information disclosure on demographic composition: how information disclosure changes the probability that a lead risky house is occupied by a certain type of family.

In my main specification, I use the new occupant sample to understand how information disclosure affects sorting behaviors by comparing changes in the characteristics of new occupants of pre- and post-1978 houses, before and after the disclosure. However, the new occupant sample only contains occupants' information as of the year they moved in. If families are planning to have kids in the near future, they may take lead-based paint hazard into consideration when buying or renting a house. For the aforementioned reasons - and also to control for potential unobserved heterogeneity from individual house - I also use the full study sample to examine the effects of information disclosure on demographic compositions, controlling for house fixed effects following equation (2.8).

2.4.3 Impacts on Risk Mitigation Behaviors

One of the goals of information disclosure is to make homeowners and landlords better maintain their houses rather than changing their buying choices. Here, I look at whether information disclosure has an effect on buyer's lead inspection behavior. A paint inspection will tell you the lead content of every different type of painted surface in a home. According to the EPA, this is most appropriate when you are buying a home, and to help you determine how to maintain your home for lead safety. In addition, I also examine whether there are any *spillover effects* of information disclosure. I explore whether disclosure on lead-based paint also raises awareness of lead hazard in general by looking at lead pipe and lead solder inspections. Since lead hazard inspections are usually conducted when purchasing a house, I use the new occupant sample in the analysis. The estimation strategy is identical to equation (2.7), except using different dependent variables. y_{it} is equal to one if the homeowner conducted a lead inspection when he or she bought the house, and zero otherwise.

In addition, I also explore whether information disclosure has changed the probability that peeling paint is present in a house. Lead paint is still present in millions of homes, sometimes under layers of newer paint. If the paint is well maintained, the lead paint is usually not a problem. However, deteriorating paint is a hazard that needs immediate attention. To estimate the effects of information disclosure on the existence of peeling paint, I use the new occupant sample to explore the effects when the houses change hands. It is also important to understand how people maintain paint once they move into a property. Therefore, I also use the study sample controlling for house fixed effects. As a dependent variable, y_{it} is equal to one if there is peeling paint in the house.[14]

Lead-contaminated dust is one of the most common causes of lead poisoning, and is a more direct measure of lead hazard than the presence of peeling paint. Using the lead dust measures in the NHANES, I use the changes in lead dust levels in houses as another measure of risk mitigation behavior. Different from the AHS, the NHANES has a relatively coarse measure for a house's year built. Therefore I use children who live in houses built between 1960 and 1977, and houses built between 1978 and 1989 as my study sample. In addition, I restrict my attention to children under six years old when they moved to the current residence. The econometric analysis takes the same form as equation (2.7), with y_{it} representing lead dust levels in a house. $Risky_i$ is defined similarly. $Disclosed_{it}$ is equal to

[14] A peeling paint condition is determined by the existence of an area of peeling paint larger than 8 by 11 inches, as measured by the AHS.

one if the child moved the house after 1996 and zero otherwise.

2.4.4 Impacts on Health

The last empirical question is to examine whether market and mitigation responses can translate into health effects. For health effects, I use the NHANES information about children's BLLs, and characteristics of the houses they occupied. The empirical approach adopted in this part is identical to equation (2.7), except the dependent variable is the BLL of the child. The econometric model takes the form as following:

$$y_{it} = \beta_1 Risky_i + \beta_2 Disclosed_{it} + \beta_3 Risky_i \times Disclosed_{it} + \alpha X_{it} + T_t + \epsilon_{it}, \tag{2.9}$$

where y_{it} is the BLL of a child who moved in their current residence when he/she was under six years old. X_{it} include age fixed effects, gender, race, parental education and family income. T_t controls for year fixed effects, controlling for the time trend which shows a steady decrease in BLLs as a result of environmental regulation and public health efforts.

2.5 RESULTS

In this section, I show the effects of information disclosure on sale and rental markets separately. There are many reasons to expect differential effects between the two groups. One of them is the different capitalization patterns between the owner and rental markets (Glaeser and Gyourko, 2007). Although the high turnover rates and low financial costs of moving should allow rental prices to adjust quickly, rental market rigidity may not be able to capture the rental change. For example, tenants in rent-controlled apartments tend to move less, and typically landlords can only modestly (if at all) adjust prices unless there is turnover. There are also disparities between rental and owner units in term of geography and unit characteristics.

In addition to the owner and rental market having a different structure, they also attract two different sets of individuals. Owners may be more attentive to lead-based paint given their financial stake in the property as well as the fact that they are more likely to have kids and have a longer anticipated tenure in one location than renters. These disparities in characteristics between owners and renters and their housing units may lead to different responses from information disclosure.

2.5.1 Owner Market

2.5.1.1 Visual Evidence In order for my difference-in-differences estimates to represent the causal effects of information disclosure on housing outcomes (price or the probability that a house is occupied by a certain family type), I need assume the average change in housing outcomes of pre-1978 houses would have been proportional to the average change in housing outcomes of post-1978 houses, in the absence of information disclosure. I assess the validity of this assumption by comparing the prior trends in housing outcomes of pre-1978 houses leading up to information disclosure with the prior trends in post-1978 houses prior to disclosure. I also need information disclosure to not coincide with any unobserved shocks which differentially affected each group. Since I am using nationally representative data, unless the unobserved shocks occurred nationally or in a majority of states, this issue should not be a concern.

I limit my analysis to houses with year built between 1975 and 1982, and year that occupants move in between 1992 and 2001. Then I regress log-price and an indicator which equals one if a house is occupied by a certain type of family on MSA fixed effects, year trend, and structural control variables. I then fit the group-specific local polynomials on the residuals of these regressions. This approach allows me to illustrate the temporal variation in the data that is explained by the variables of interest to my analysis controlling for differences in outcome variables due to housing characteristics.

Trend analysis for owner and rental markets are presented in Figures (3) and (4), respectively. These plots provide an opportunity to judge the validity of the difference-in-differences assumption of similar trends in advance of information disclosure. Panel (1) in Figure (3) presents the residual plot for log sale prices. The trend in risky houses is only slightly - if at all - different from the trend in non-risky houses. This may be because some home buyers responded to Title X when it was passed in 1992. However, there is no clear evidence of price changes in owner market. Panels (2) - (4) in Figure (3) present the residual plots for the probability that lead risky houses are occupied by certain types of families.[15] The figures support the validity of the design as there is little evidence of differential trends between risky and non-risky properties prior to information disclosure. Furthermore, they suggest that there is a decrease in the probability of lead risky houses being occupied by homeowners with children, white children and children with educated parents. Figure (4) presents the trend analysis for rental markets. The parallel pre-trend assumption is satisfied, however, there are no clear price or sorting patterns.

2.5.1.2 Sale Price If market participants are unaware of lead hazards prior to disclosure, then pre-1996 housing market clears without fully accounting for lead risks. Information disclosure of lead-based paint will lower some consumers' willingness to pay for houses with lead paint risk relative to lead free houses. As such, the relative prices of lead risky houses may decrease after information disclosure as my theoretical model predicted. However, if the changes in willingness to pay are small or the percentage of individuals who consider lead as a nuisance is small, I may not be able to find measurable price effects.

To examine the effects of information disclosure on housing prices, I limit transactions to arm length sales of owner occupied, residential single family residences. I dropped houses without year built, sale year, or sale price from the sample. Finally, I drop observations with zero rooms (such as efficiencies and lofts), because these properties are not covered by the information disclosure rule. The seven surveys from 1993 to 2005 were merged, using only those newly purchased houses, which composes an unbalanced sales-panel during the period from 1993 to 2005.

Table (3) reports estimates for the causal effect of information disclosure on sale prices. This table shows the coefficients and the standard errors associated with $Risky_i$, $Disclosed_{it}$,

[15] In order for the difference-in-differences to work, pre-trends need to be satisfied for any and all outcome variables. In consideration of space, I presented only the most important sorting patterns. Evidence on other sorting patterns is available upon request.

and their interaction for five separate regressions, each varying by sample restrictions and each based on equation (2.7). Other variables in regressions include: number of bedrooms; number of bathrooms; lot size and its square; square footage and its square; sale year interacted with MSA fixed effects; and region dummies. Coefficients from other housing characteristics are as expected and not reported due to space limitation.

Focusing on the years sold immediately before and after disclosure helps to measure shifts in the demand for non-risky housing along an inelastic short-run housing supply curve, since longer run estimates will reflect shifts in both supply and demand. The coefficient of the interaction term is 3%, but it is insignificant.[16] Although still insignificant, when I increased the year sold symmetrically, restricting my attention to houses sold between 1993 and 2000, the point estimation of the interaction term becomes negative. The effects still stay insignificant, and the magnitudes are similar when I increase the time window of year sold from 1999 to 2001, while keeping the year built window the same.[17] To summarize, I do not find measurable sale effects following information disclosure. Bae (2016) finds a similar result using a different model.[18]

As suggested in my theoretical model, trading between heterogeneous buyers and sellers will drive a wedge between price effects and buyers willingness to pay for a lead free house. As such, the price effect will be an underestimate of changes in willingness to pay. As Kuminoff and Pope (2014) points out, the bias can be serious. In addition, when the share of individuals who do not care about lead risk is large, the small effect on sale price should be expected. As such, if heterogeneous households sort themselves into post-1978 houses, then these capitalization effects do not reflect buyers' willingness to pay. In the next section, I show evidence that such sorting did happen.

[16]The positive coefficient may be the result of measurement error, since the sale year information is based on the retrospective memory of residents. Together with the fact that the gap between making an offer on and closing on a house usually takes a few weeks - sometimes even months - it is even more likely that "pre-" and "post-" were mistakenly assigned.

[17]Due to data limitations, I keep the year built of the houses the same across regressions. As mentioned earlier, the AHS does not provide any detailed year built information until 1980.

[18]In this paper, Bae (2016) examines the same question using the AHS, but approaches the question using a repeated sale model. She focused on the pre-1978 houses only, and compared prices of repeated sales of the same house. The assumption that the house characteristics are constant over time , which allows the model to attribute the changes between two time points (pre-policy and post-policy) to the policy. Based on this analytic frame, this study estimated the changes in sale prices because of the policy. She found that the policy did not lower prices of old homes.

2.5.1.3 Demographic Composition

The goal of Title X is to protect children from lead paint poisoning. Therefore, I now look at whether disclosure has reduced the probability of a lead risky house being occupied by a family with children under six years old. Among homeowners, parents are most likely to prefer properties without lead, since children are most vulnerable to lead hazard. Table (4) shows how disclosure changed the incidence of lead risky houses being occupied by families with children. Columns (1) - (3) use the new occupant sample to confirm that parents are more likely to move out from lead risky properties to avoid potential lead hazard. Column (3) shows that disclosure decreases the probability of children occupying lead risky houses by 0.045, or about 15%.[19] Results are robust to different sample restrictions.

As mentioned earlier, the new occupant sample includes families without children, but who plan to have children in the near future. Although such families are categorized as not having children in the new occupant sample, they may take lead risks into consideration when purchasing a house families with children. To address this issue - and the unobserved heterogeneity of each house - I analyze the data using the "study sample" controlling for house fixed effects. Using the study sample, I am able to track the same families over survey years, allowing me to see if they had children after moving into the house. Column (4) and (5) in Table (4) show that coefficients are quantitatively similar when using either the new occupant or study sample.

Thus far, I have focused on the *average* effect of Title X on housing markets. One concern with information based regulation is that it can heterogeneously impact different groups, and actually end up *hurting* minority and lower socioeconomic status (SES) groups. This focus obscures a tremendous amount of heterogeneity across racial and SES groups. I test for heterogeneous effects by grouping households based on race, education, and the presence of children. Table (5) presents the effects of disclosure on the probability that a certain type of family occupies a lead risky home, with results for 16 different demographic groups. Each regression uses the study sample, houses built between 1975 and 1982, and sold between 1992 and 2001. Results using the new occupant sample are quantitatively similar and are

[19] The percentage change is calculated by dividing the coefficient of interaction term, 0.0453, by 0.302, which is the incidence of kids under six living in the lead risky houses before 1996.

reported in the Appendix.

Columns (1a) - (1d) in Table (5) show the effects across racial groups. Although white families tend to move out from lead risky properties and minority families tend to move in, the estimates are not statistically significant. Hispanic families are actually *more* likely to move into lead risky houses after disclosure.

Column (2a) shows the probability that lead risky houses are occupied by white families with children decreased by 0.04 or 16%. Together with (1a), the coefficient indicates that white families without kids are more likely to move into lead risky properties. In contrast, minority families with children are more likely to move into lead risky houses, especially black families, which indicates that the probability that minority children is exposed to lead hazard has increased. This may result from the fact that even after disclosure minority parents are still unaware of lead hazard, or that they have higher willingness to pay for those lead risky houses compared with other parents who are looking for family-friendly houses.

Although on average there is no evidence of sorting patterns across families with different education backgrounds (Columns (3a) and (3b)), it seems that parents with no more than a high school degree are more likely to move into lead risky houses following disclosure. The coefficients for educated minority parents are insignificant and not presented in this table. The insignificance may result from that fact that educated minority parents do not respond to information disclosure or from a small sample size. As a quick robustness check, I also examine how information disclosure affects families with each member older than 60. Since lead is a major threat to children and has minimal effects on the elderly, no effect should be found. The result in Column (4d) confirms this.

I now look at how disclosure affects different income groups. Figure (5) shows that the highest reduction in the share of families with children living in lead risky houses occurs amongst households in the third quintile of income distribution. Prior to disclosure, the share of lead risky houses occupied by families with children was between 39% and 47% across the quintiles. Following disclosure, the point estimation of the share of families with children living in the lead risky houses decreased for all but the fourth quintile income group. However, only for the third quintile group, the reduction is significant. It suggests that the middle-income families are those at the margin: they both have resources to move and may have to move. As suggested by Currie (2011) and Currie et al. (2011), poor families may be less able to move to a newer and safer homes to avoid lead hazard. Considering the fact that children of wealthier families have lower BLLs, rich families may have already acted toward lead-based paint hazard: richer families have more resources to acquire information; at the same time, richer families are more likely to be educated and people with high educational attainment might have more information searching capabilities. In addition, they have more resources to abate the lead hazards in their houses. In a research project that focused on awareness of lead poisoning, Rajaram (2007) finds that there is a higher level of residents' awareness on residential lead paint among higher-income groups compared to lower-income groups. Prior empirical studies have also confirmed the fact that better educated and higher income consumers tend to have more extensive seeking behaviors and thus possess more knowledge about products. In contrast, families without children tend to move into lead risky houses, as presented in Figure (6).

2.5.1.4 Risk Mitigation Behaviors Complete avoidance of the pre-1978 homes is not an optimal response to information disclosure, since the effects of lead hazard can be negligible so long as occupants maintain their home's paint in good condition. In addition, permanent abatement is inefficient and sometimes it may be ineffective.[20]

As of 1996, roughly 75% of houses built before 1978 are likely to contain some lead-based paint. The first step to avoiding lead hazard is home inspection. Table (6) shows the effects of information disclosure on buyers' lead inspection. Results show that disclosure increased the probability that homeowners tested for lead-based paint when purchasing a house by about 35%. In addition, I also find that families with high income and education respond to disclosure more in terms of lead inspection as presented in Table (8). This is also in line with the fact that families with high income and education have more resources to acquire information and to take actions to avoid lead hazards. Interestingly, information on lead-based paint also has increased homeowners' awareness of lead hazard elsewhere. Columns (4) and (5) in Table (6) indicate this positive spillover: more homeowners tested for lead

[20] Improper work may leave behind paint dust on lead-tainted soil. The cost of improper removal of lead-based paint can be as high as $186,481 for a single house as estimated by Jacobs et al. (2003), where they present a case study and calculated the cost of decontamination after uncontrolled use of power sanders.

pipes and solders when purchasing properties post disclosure.

In order to induce occupants to manage existing lead-based paint risks, Title X mandates the provision of the informative pamphlet that contains low-cost maintenance tips to identify and minimize lead hazard. Although lead is a threat to children's health, lead hazard will be minimal if households maintain their paint carefully. Table (7) presents whether information disclosure changed the presence of peeling paint in owner occupied properties. Columns (1) - (3) show the effects on the presence of peeling paint when a transaction occurred. The absence of any effects is expected; many brokers suggest painting a house before sale as one of the best and easiest things to increase the sale price. Columns (4) and (5) show the effects using the study sample to understand how homeowners maintain paint after moving into new homes. In general, results from Table (7) suggest that information disclosure does not affect risk mitigation behavior in terms of the presence of peeling paint in the house. Peeling paint in the AHS is determined by the existence of an area larger than 8 by 11 inches. This is a crude measure, and effects may be occurring at a finer level.

Lead dust is most often the result of old, peeling, or chipped lead paint. Between 1999 and 2003, the NHANES collected dust samples from the homes of children under six years old to be tested for the presence of lead. This provides an opportunity to examine risk mitigation behavior at a finer level. Table (9) reports the effects of disclosure on lead dust levels. Consistent with the result for peeling paint, disclosure does not decrease the lead dust levels in owner occupied houses, not even for any ethnic group. This may reflect that homeowners tend to take better care of houses as a result of their longer tenure. It also could be that homeowners with children shun lead hazard by sorting into lead safe houses, instead of working to mitigate pre-existing lead hazard.

2.5.1.5 Health Here I look at how Title X affects health outcomes. Lead paint and lead dust are the most hazardous sources of lead for children in the US. Prior to being banned from residential use in 1978, lead paint was commonly in homes. Table (10) shows the positive and significant correlation between the lead dust levels in a house and the BLLs of children living in the house. Column (5) in Table (10) uses blood mercury levels as a robustness check assuring that the regression does not pick up other unobserved characteristics that affect both housing qualities and BLLs. Like lead, mercury is also a toxic element, children are exposed to mercury through environment or fish consumption. As such, the lead dust levels in a house should not affect blood mercury levels, as confirmed by the insignificant coefficients. Blood mercury level is the only other laboratory measure collected through the NHANES surveys.

The ultimate goal of Title X is to reduce children BLLs. Columns (1) to (6) in Table (11) indicate that disclosure does not affect the BLLs of children in owner occupied houses. This may be due to the fact that disclosure does not change the probability of peeling paint or the lead dust level in a house.

Results so far have shown that homeowners mitigate the lead hazard by changing their home purchasing behaviors and increasing lead inspections. Given the fact that even prior to disclosure, children of homeowners have lower BLLs as well as a lower risk of being exposed to lead hazard, it is not surprising that there are no health effects on children's health in owner occupied properties.

2.5.2 Rental Market

2.5.2.1 Rental Price Conventional wisdom suggests that the rental market is better suited for valuation studies than the owner market, as higher turnover rates and the lower financial costs of moving should allow prices to adjust more quickly. Interestingly, there is no effect on rental prices. Table (12) shows estimates of the effects of disclosure on rental prices. Similar to owner market, the coefficients are all negative - as expected - and also insignificant. Results are robust to different sample restrictions and model specifications: Columns (1) - (3) show results using the new occupants sample; columns (4) and (5) use the study sample. While capitalization effects are similar between owner and rental markets, this does not necessarily mean that owner and rental markets respond to information disclosure in the same manner.

2.5.2.2 Demographic Composition One reason that rental prices are unaffected by disclosure might be the absence of changes in renters' willingness to pay for a lead free house.

As such, according to the model predictions, we should also expect no measurable changes in demographic composition of rental properties. Table (13) shows the effects of disclosure on the incidence of pre-1978 properties occupied by families with children under six. There is no evidence that disclosure has changed the occupancy sorting patterns of parents in the rental market. Table (14) presents demographic composition in rental markets controlling for house fixed effects. I find that the probability that lead risky houses are occupied by white parents with a college degree has decreased by 0.01, while it increased by 0.02 for minority parents with no more than high school degrees. Except for these two groups, I find no evidence of sorting in renter occupied units, not even for the high income renters. Figure (7) shows that the probability of a lead risky house that being occupied by families with children stays roughly the same across income quintile groups.

Compared to owners, renters tend to be younger, have lower income and fewer years of education, are less likely to have kids, and have a shorter tenure in one property. Together with the fact that families with high income and education have more resources and capabilities to become aware of and respond to lead hazard, it is unsurprising that renters have not changed their occupancy choices due to disclosure. In addition, for parents, avoidance of lead risky houses is one option to avoid lead hazard. I now look at if renters are choosing another avoidance option, namely, by keeping paint in good condition.

2.5.2.3 Risk Mitigation Behaviors For renter occupied units, the AHS does not contain lead inspection information. Table (15) shows the effects of disclosure on presence of peeling paint in renter occupied units. Columns (1) - (3) show that disclosure has no effects on the presence of peeling paint when new tenants moved in. Usually not required, many landlords choose to paint their houses between tenants for marketing and aesthetics purpose. In terms of lead hazard, it is crucial whether the paint is well maintained when tenants live in the house. Therefore, I also show the effects of disclosure using the study sample in Columns (4) and (5). By controlling for house fixed effects, I find some evidence that disclosure has decreased the probability of peeling paint in a rental house. I further show the effects on peeling paint by controlling for house fixed effects in Table (16) using various sample restrictions. Results confirm the weak evidence of the decrease in probability of peeling paint.

Similar to procedures used in owner market analysis, I then restrict my attention to lead dust levels in renter occupied houses, with Table (17) presents the results. Consistent with the results from the AHS sample, lead dust levels fell following the information disclosure, in both houses occupied by white and Hispanic parents. The coefficient for houses occupied by black parents is also negative, but insignificant. These results provide evidence that, unlike owners who mitigate lead hazards by moving to houses with less lead hazards, renters seem to take efforts to maintain their paint in a better condition.

2.5.2.4 Health Table (18) shows the effects of information disclosure on the BLLs of children in renter occupied units. Following disclosure, BLLs of children in renter occupied houses decreased roughly 0.9 $\mu g/dL$ or 43%.[21] Interestingly, BLLs of white children decreased the most, in both level and percentage change. Column (7) provides a robustness check and shows that disclosure had no effect on blood mercury levels.

Regression results of the effect of disclosure on incidence of lead poisoning show evidence Title X has reduced lead poisoning. Owing to the recent environmental and pubic health effort, the incidence rate for lead poisoning in my study sample is about 3.4%. However, recent research has indicated that significant neurological damage to children occurs even at very low levels of lead exposure: a one unit increase in BLL when BLL is below 10 $\mu g/dL$ is associated with a 0.51 IQ point decrement; the decrement reduces to 0.19 IQ point when BLL is between 10 and 20 $\mu g/dL$. Of the 27.97 million children under six in the US in 2006, 24.7%, or 6.9 million, have BLLs between 2 and 10 $\mu g/dL$ (Gould, 2009). Drawing on recent public health literature (Gould, 2009), each IQ point loss represents a loss of $15,120 in present discounted value of lifetime earnings (in 2000 USD). That means that a 0.9 $\mu g/dL$ BLLs decrease for children living in rental properties corresponds to approximately a $7000 increase in present discounted value lifetime earnings. Notice that this benefit estimate does not include savings in costs related to healthcare, special education, and criminal activity. According to Schwartz (1994), the total social benefit tends to be about three times as

[21] I divide the interaction term by the average BLLs of children in pre-1978 houses who moved prior to information disclosure. Blood lead levels of all race, white, black and Hispanic children in pre-1978 houses who moved prior to information disclosure are 2.05, 2.74, 2.50 and 1.80 respectively.

large as the benefit in lifetime earnings, in which lead-linked crimes are not included. Based on an estimate from Nevin (2007), a one$\mu g/dL$ reduction in the average preschool BLL is associated with about a $1.5 billion reduction in direct cost of lead-linked crimes.[22]

2.6 CONCLUSION

With the recent findings of high levels of lead in the drinking water in Flint, Michigan, attention has once again focused on lead poisoning. While drinking lead tainted water is certainly dangerous, the main cause of lead poisoning in American children comes from lead paint. In this paper, I have studied the efficacy of Title X, an information disclosure policy aimed at reducing lead paint hazards. Using a difference-in-differences approach and a natural experiment, I've obtained estimates for the effects of Title X on housing prices, demographic composition, lead mitigation behavior and health. In doing so, my paper not only details the effects of Title X, but also provides evidence of the possibilities and limitations of information disclosure more broadly. This is of particular interest, as information disclosure has become a popular way to regulate environmental risks.

I find that owner and rental markets have different responses to Title X. In the owner market, sale prices do not change after Title X. Estimates of these price effects should be thought of as a lower bound of Title X's effect on buyers' willingness to pay, as sorting among heterogeneous buyers leads to an underestimate of willingness to pay. I find that parents, especially white parents, are more likely to move away from lead risky properties following disclosure; while minority parents are more likely to move into houses with lead risk. I also find that homeowners respond to information disclosure by increasing lead inspection, especially high-income and educated homeowners. By contrast, in the renter occupied units, there is no response in terms of price or demographic composition. However, in the rental market, there is evidence of effects on risk mitigation behavior, and strong evidence that the BLLs of children in lead risky rental properties have decreased as a result of Title X. I also

[22] Direct victim costs are costs related to the criminal justice system through legal proceedings and incarceration, and lost earnings to both criminal and victim.

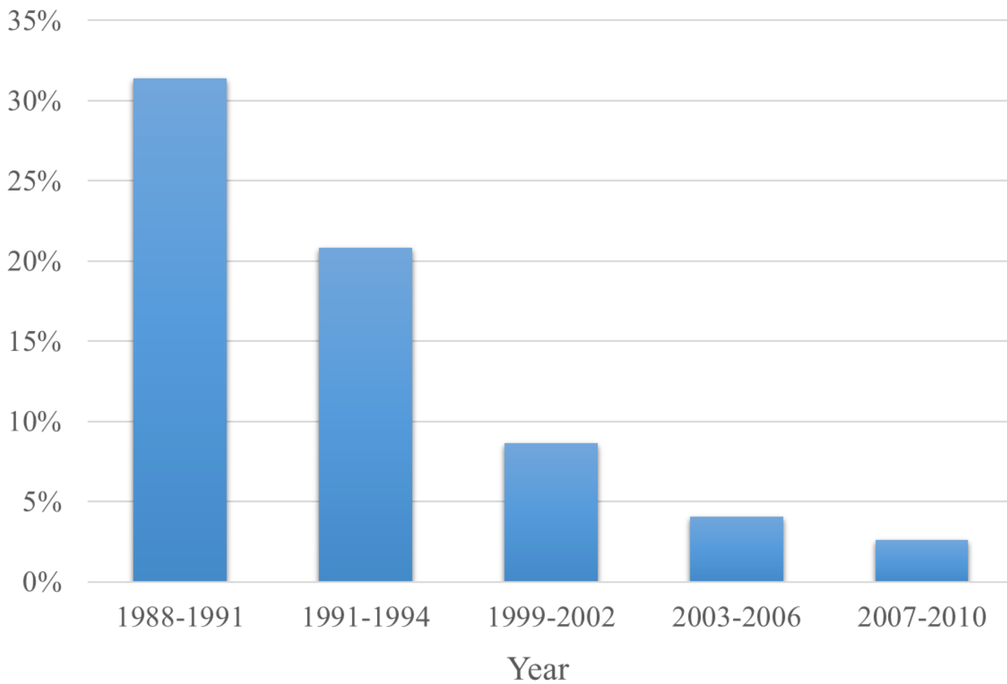
find suggestive evidence that the decrease in BLLs can be partly attributed to the reduction in lead dust.

Conventional wisdom and stylized facts suggest several possibilities for the disparities between owner and rental markets. First, there are significant housing and occupant characteristic differences between owners and renters. If these differences are somehow correlated with perceptions of lead hazard then this could drive different responses to disclosure. Owners may be more attentive of lead-based paint in their properties given their anticipated tenure and financial stake in the properties. The disparities in health effects between owners and renters may also arise from different mitigation behaviors. Owners may have already taken care of lead paint hazard before Title X was enacted. Another possibility is that children in owner occupied units have much lower BLLs than those in renter occupied units before disclosure, and therefore disclosure has little effect in the owner market, but relatively large effects in the rental market.

Disadvantaged groups may have a higher willingness to pay for cheaper houses that are lead risky. This brings up the question of whether information disclosure contributes to the disparities in childhood lead poisoning across racial and socioeconomic groups. Fortunately, although minority children are more likely to move to houses with lead risk, BLLs of minority children living in owner occupied houses did not increase following disclosure. In the rental market, disclosure resulted in increased mitigation behavior. This had the effect of sharply reducing the BLLs of all children, with white children benefiting the most. On the whole, my results suggest that regulation by information can be an effective approach to managing environmental risk, although benefits may not be shared equally across racial and socioeconomic groups.

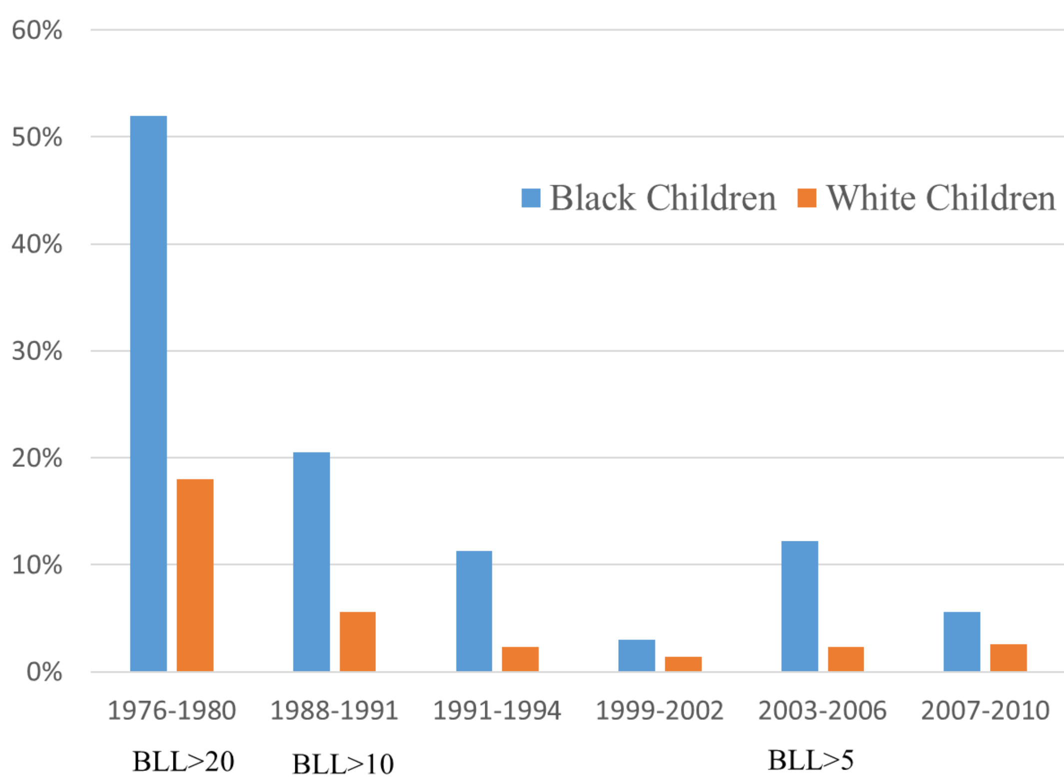
2.7 FIGURES AND TABLES

Figure 1: Percentage of Children under Six with BLLs above 5 $\mu g/dL$



Data source: National Health and Nutrition Examination Survey, various years

Figure 2: Percentage of Children under Six with Lead Poisoning



Data source: National Health and Nutrition Examination Survey, various years

Figure 3: Residual Plots with 90% Confidence Interval, Owner Market

Figure 4: Residual Plots with 90% Confidence Interval, Rental Market

Figure 5: Family with Children Sorting into Lead Risky Owner Occupied Houses, by Income Quintile

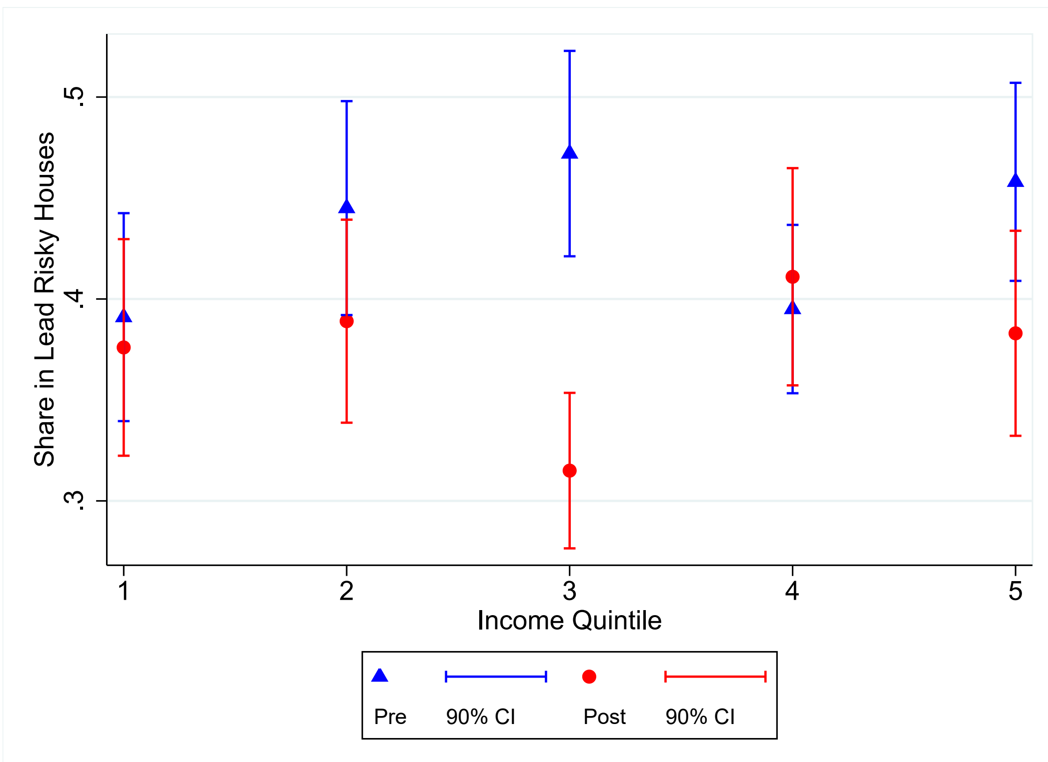


Figure 6: Family without Children Sorting into Lead Risky Owner Occupied Houses, by Income Quintile

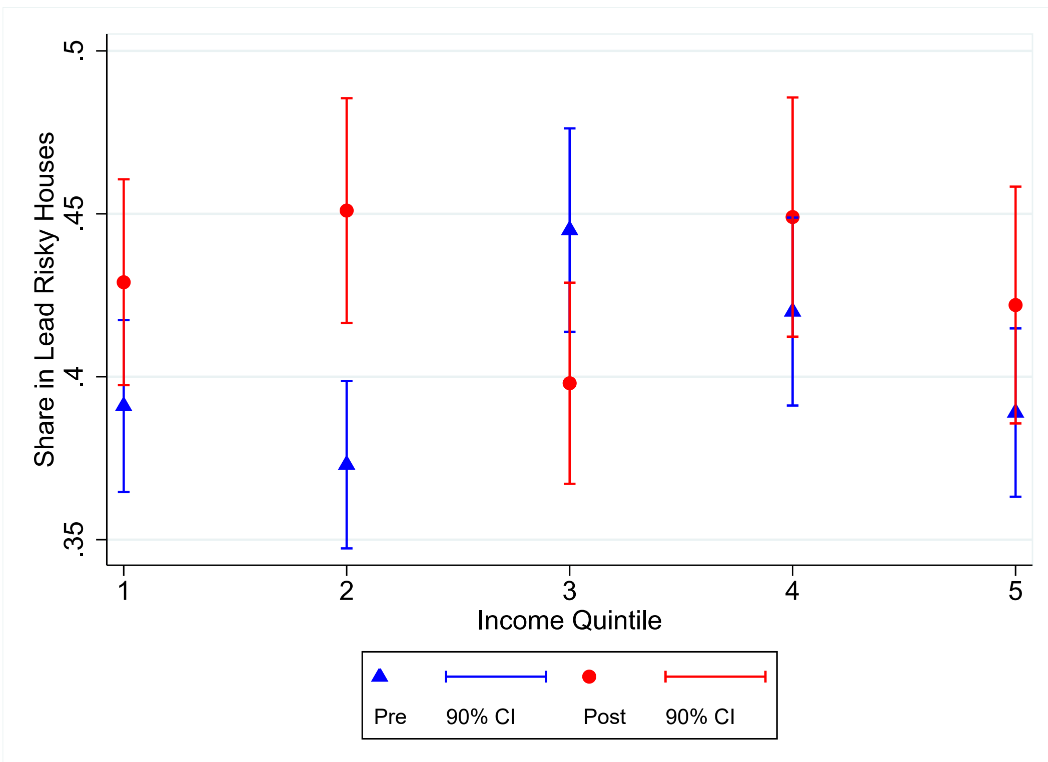


Figure 7: Family with Children Srting into Lead Risky Rental Homes, by Income Quintile

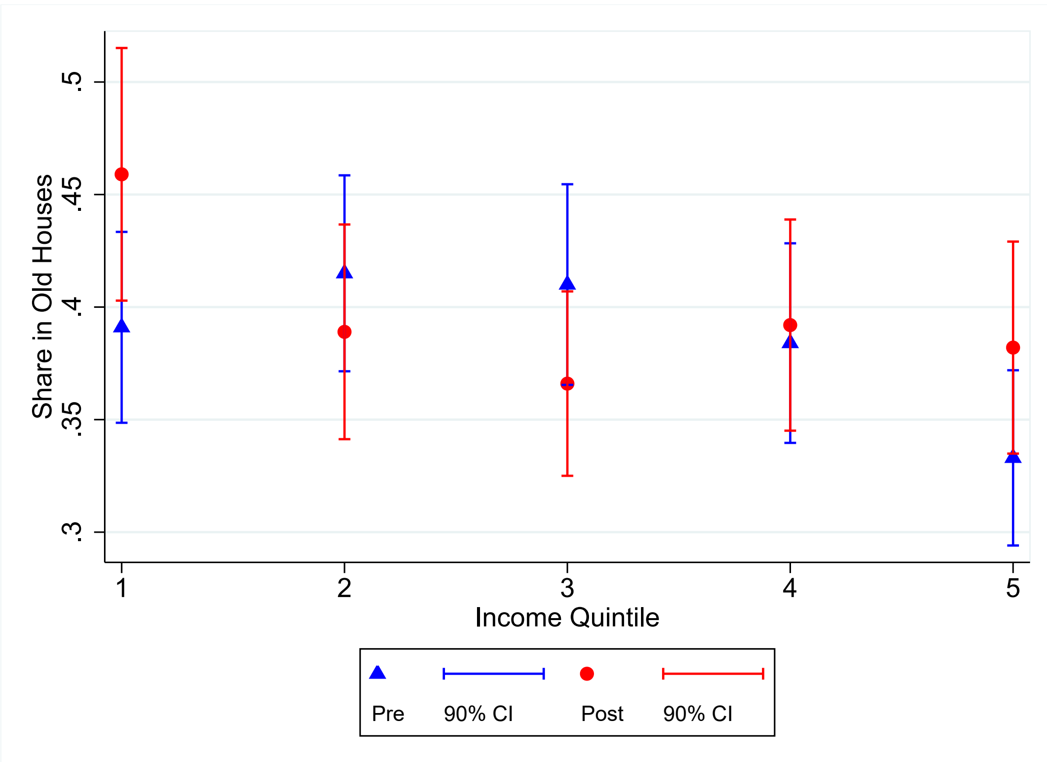


Table 1: Summary Statistics from the AHS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample Restriction	Owner	Owner	Owner	Owner	Renter	Renter	Renter	Renter
Year Built	All	75-82	75-78	79-82	All	75-82	75-78	79-82
Year Moved in	All	92-01	92-01	92-01	All	92-01	92-01	92-01
Number of Bedrooms	3.037	3.008	3.060	2.973	1.922	1.895	1.913	1.883
Number of Bathrooms	1.638	1.809	1.787	1.824	1.167	1.251	1.197	1.288
Lot Size	124,168	128,703	123,769	132,171	146,439	192,989	192,309	193,510
Square Footage	2,087	1,950	1,978	1,930	1,191	1,104	1,139	1,081
Garage	0.753	0.758	0.774	0.747	0.310	0.321	0.322	0.321
Porch	0.884	0.913	0.913	0.913	0.641	0.707	0.663	0.737
Peeling Paint [1]	0.0200	0.0102	0.0110	0.00969	0.0486	0.0242	0.0248	0.0239
Lead Test Before Purchase [2]	0.198	0.343	0.343	0.344	-	-	-	-
White Family [3]	0.822	0.823	0.835	0.815	0.601	0.655	0.650	0.658
Black Family	0.0803	0.0520	0.0484	0.0545	0.181	0.150	0.151	0.149
Hispanic Family	0.0648	0.0801	0.0750	0.0835	0.156	0.135	0.141	0.131
Kids in the House [4]	0.171	0.254	0.256	0.252	0.218	0.242	0.232	0.248
White Parents	0.128	0.193	0.206	0.184	0.0967	0.124	0.120	0.127
Black Parents	0.0144	0.0142	0.00898	0.0177	0.0482	0.0435	0.0415	0.0448
Hispanic Parents	0.0197	0.0317	0.0265	0.0352	0.0597	0.0569	0.0539	0.0589
Head with College Degree	0.286	0.328	0.336	0.322	0.208	0.218	0.198	0.231
Head with No More than HS Degree	0.171	0.254	0.256	0.252	0.218	0.242	0.232	0.248
Rent [5]	-	-	-	-	587.0	574.7	573.2	575.7
Sale Price	133,225	124,838	127,717	122,883	-	-	-	-
Number of Residents Moved in	1.437	1.482	1.519	1.462	3.118	3.222	3.168	3.258
Number of Obs [6]	234,156	10,552	4,255	6,297	100,879	6,739	2,837	3,902

Note: Each column is a different subsection of the American Housing Survey. 1. A peeling paint condition is determined by the existence of an area of peeling paint larger than 8 by 11 inches. 2. This question is prepared for those who live in owner occupied houses only. 3. The race of a family is defined by the race of the family head. 4. Kids are defined as number of occupants less than 6 years old. 5. Rent and sale price are adjusted using year 2000 dollars. 6. Some variables have fewer observations than the number denoted here due to missing information.

Table 2: Summary Statistics from the NHANES

Sample Restriction		(1) Owner	(2) Owner	(3) Owner	(4) Owner	(5) Renter	(6) Renter	(7) Renter	(8) Renter
Year Built		All	60-89	60-78	79-89	All	60-89	60-78	79-89
Age	Unweighted[1]	7.132	7.849	7.612	8.067	4.631	4.499	4.628	4.352
	Weighted	*7.300*	*8.152*	*7.924*	*8.319*	*4.988*	*4.901*	*4.867*	*4.939*
Poverty Income Ratio[2]	Unweighted	2.671	2.610	2.490	2.722	1.394	1.413	1.429	1.394
	Weighted	*3.007*	*2.977*	*2.768*	*3.132*	*1.682*	*1.707*	*1.740*	*1.672*
Blood Lead Levels	Unweighted	1.684	1.557	1.571	1.545	2.356	1.979	2.063	1.883
(µg/dL)	*Weighted*	*1.473*	*1.375*	*1.436*	*1.330*	*2.052*	*1.795*	*1.941*	*1.631*
Blood Mercury Levels	Unweighted	*3.137*	*2.915*	*2.652*	*3.199*	*2.948*	*2.595*	*2.453*	*2.757*
(µmol/L)	*Weighted*	*3.201*	*2.719*	*2.436*	*2.949*	*2.931*	*2.491*	*2.181*	*2.821*
Lead Dust Levels	Unweighted	0.662	0.634	0.745	0.512	0.835	0.635	0.715	0.559
	Weighted	*0.575*	*0.565*	*0.674*	*0.475*	*0.728*	*0.532*	*0.604*	*0.462*
White	Unweighted	0.442	0.406	0.329	0.478	0.247	0.225	0.224	0.225
	Weighted	*0.743*	*0.721*	*0.666*	*0.761*	*0.486*	*0.424*	*0.430*	*0.417*
Black	Unweighted	0.191	0.200	0.248	0.157	0.330	0.334	0.285	0.389
	Weighted	*0.069*	*0.074*	*0.101*	*0.055*	*0.183*	*0.190*	*0.166*	*0.217*
Hispanic	Unweighted	0.313	0.349	0.371	0.329	0.354	0.363	0.412	0.307
	Weighted	*0.133*	*0.153*	*0.176*	*0.135*	*0.239*	*0.256*	*0.290*	*0.219*
HH Ref with College	Unweighted	0.264	0.233	0.193	0.271	0.0849	0.0963	0.0974	0.0950
Degree[3]	*Weighted*	*0.333*	*0.316*	*0.278*	*0.344*	*0.121*	*0.106*	*0.090*	*0.125*
HH Ref with No More	Unweighted	0.463	0.494	0.515	0.475	0.615	0.583	0.562	0.607
than HS Degree	*Weighted*	*0.383*	*0.405*	*0.439*	*0.380*	*0.553*	*0.551*	*0.562*	*0.539*
Number of Obs[4]	Unweighted	3190	1203	577	626	1120	521	277	244

Note: Each column is a different subsection of the NHANES. 1. I show both weighted and unweighted summary of statistics. The weight is constructed according to the NHANES procedure. 2. This variable is a ratio of family income to the poverty threshold 3. HH Ref is the person in the household who answered the questions on behalf of the children. 4.Some variables have fewer observations than the number denoted here due to missing information.

Table 3: Price Effects: The Effect of Information Disclosure on Sale Prices

Sample Restriction	*DV: ln(price)*				
	(1)	(2)	(3)	(4)	(5)
Year Built	75-82	75-82	75-82	75-82	75-82
Year Sold	94-99	93-00	92-99	92-00	92-01
Disclosed	-0.0213	0.0590	-0.00934	0.0663	0.0910
	(0.649)	(0.251)	(0.910)	(0.241)	(0.155)
Risky	-0.0982***	-0.0479**	-0.0194	-0.0171	-0.0177
	(0.00218)	(0.0253)	(0.209)	(0.278)	(0.249)
Disclosed x Risky	0.0357	-0.0184	-0.0274	-0.0484	-0.0352
	(0.490)	(0.707)	(0.364)	(0.146)	(0.199)
Observations	2,078	2,770	2,886	3,161	3,475
R-squared	0.551	0.543	0.560	0.552	0.558

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from five separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. For example, the estimated coefficients in columns (1) correspond to the effect of the Title X on properties built between 1975 and 1982 and sold between 1994 and 1999. Year 2000 dollars are used. Other variables in regression but omitted inlcude: number of bedrooms; number of bathrooms; lot size and its square; square footage and its square; sale year interacted with MSA; and region dummies.

Table 4: Demographic Composition: Children under Six in Owner Market

Sample Restriction	***DV: Whether child under 6 presence in the house***				
	(1)	(2)	(3)	(4)	(5)
Year Built	75-82	75-82	75-82	75-82	75-82
Year Sold	94-99	93-00	92-01	92-01	92-01
Disclosed	0.00387	0.0286	0.0132	-0.0262	-0.121
	(0.889)	(0.521)	(0.743)	(0.651)	(0.243)
Risky	0.0248	0.0450***	0.0251**	0.0151**	
	(0.298)	(0.000556)	(0.0220)	(0.0297)	
Disclosed x Risky	-0.0453*	-0.0921***	-0.0497***	-0.0660***	-0.0469**
	(0.0540)	(1.45e-09)	(0.00111)	(1.40e-05)	(0.0473)
Housing Characteristics	Yes	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
MSA fixed effect	Yes	Yes	Yes	Yes	Yes
House Fixed Effects	No	No	No	No	Yes
Observations	2,064	2,751	3,450	10,093	10,093
R-squared	0.226	0.229	0.233	0.183	0.681

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from five separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. For example, the estimated coefficients in columns (1) correspond to the effect of the Title X on properties built between 1975 and 1982 and sold between 1994 and 1999. Please see Table (2) for a list of the control variables used in each specification.

Table 5: Demographic Composition: Different Family Types in Owner Market

Family Type	(1a) White Family	(1b) Minority Family	(1c) Black Family	(1d) Hispanic Family
Disclosed x Risky	0.01000	0.0219	-0.00291	0.0248*
	(0.643)	(0.252)	(0.816)	(0.0951)
Observations	10,382	10,382	10,382	10,382
Family Type	(2a) White Parents	(2b) Minority Parents	(2c) Black Parents	(2d) Hispanic Parents
Disclosed x Risky	-0.0399**	0.0230*	0.0118***	0.0112
	(0.0466)	(0.0643)	(0.000749)	(0.330)
Observations	10,382	10,382	10,382	10,382
Family Type	(3a) Family Head with College Degree	(3b) Family Head with No More than HS Degree	(3c) White Parents with College Degree	(3d) White Parents with No More than HS Degree
Disclosed x Risky	0.0245	0.0103	-0.0116	0.0282**
	(0.119)	(0.674)	(0.187)	(0.0234)
Observations	10,382	10,382	10,382	10,382
Family Type	(4a) Minority Parents with No More than HS Degree	(4b) Black Parents with No More than HS Degree	(4c) Hispanic Parents with No More than HS Degree	(4d) Seniors with Each Family Member Older than 60
Disclosed x Risky	0.0107	0.00723***	0.00348	0.000551
	(0.127)	(0)	(0.634)	(0.963)
Observations	10,382	10,382	10,382	10,382
Housing Characteristics	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes
MSA Fixed Effect	Yes	Yes	Yes	Yes
House Fixed Effect	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports effects of Title X from 16 separate regressions using same sample restriction but different dependent variables. Sample restriction: houses built between 1975 and 1982, and sold between 1992 and 2001. The type of a family is categorized by the family head. All these regressions include other independent variables, and coefficient of those variables are not reported. Please see Table (2) for a list of the control variables used in each specification.

Table 6: Risk Mitigation Behaviors: Purchaser Lead Test in Owner Market

Sample Restriction	*DV: Purchaser Lead Paint Test*			*Lead Pipe Test*	*Lead Solder Test*
	(1)	(2)	(3)	(4)	(5)
Year Built	75-82	75-82	75-82	75-82	75-82
Year Sold	94-99	93-00	92-01	92-01	92-01
Disclosed	0.0338	0.0863	0.127***	0.142*	0.188***
	(0.491)	(0.470)	(0)	(0.0749)	(0.000745)
Risky	-0.0976***	-0.131***	-0.0863**	-0.0737***	-0.0662***
	(0.00130)	(8.62e-05)	(0.0261)	(0.000113)	(0.000856)
Disclosed x Risky	0.0873***	0.101***	0.0972***	0.105***	0.0903***
	(0.00812)	(0.00789)	(0.00693)	(0.000401)	(0.00241)
Observations	854	1,135	1,432	1,403	1,387
R-squared	0.296	0.300	0.298	0.303	0.313

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from five separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. For example, the estimated coefficients in columns (1) correspond to the effect of the Title X on properties built between 1975 and 1982 and sold between 1994 and 1999. Dependent variable for Columb (1)-(4) is whether the homeowner conducted lead paint test before purchasing the house. Dependent variables for Columb (5) and (6) are whether the homeowner conducted lead pipe and solder test before purchasing the house respectively.

Table 7: Risk Mitigation Behaviors: The Presence of Peeling Paint in Owner Market

Sample Restriction	*DV: Presence of Peeling Paint*				
	(1)	(2)	(3)	(4)	(5)
Year Built	75-82	75-82	75-82	75-82	75-82
Year Sold	94-99	93-00	92-01	92-01	92-01
Disclosed	-0.00358	-0.00359	0.00499	-0.00147	-0.00980
	(0.628)	(0.721)	(0.692)	(0.784)	(0.308)
Risky	0.00969	0.0111	0.00496	0.00229	
	(0.274)	(0.146)	(0.466)	(0.451)	
Disclosed x Risky	-0.00314	-0.00802	-0.00529	-0.00141	-0.000170
	(0.770)	(0.401)	(0.550)	(0.736)	(0.983)
Housing Characteristics	Yes	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
MSA fixed effect	Yes	Yes	Yes	Yes	Yes
House Fixed Effects	No	No	No	No	Yes
Observations	2,078	2,770	3,475	10,382	10,382
R-squared	0.112	0.071	0.042	0.015	0.426

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from five separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. For example, the estimated coefficients in columns (1) correspond to the effect of the Title X on properties built between 1975 and 1982 and sold between 1994 and 1999.

Table 8: Risk Mitigation Behaviors: Subsample Analysis

	DV: Purchaser Lead Paint Test			
	(1)	(2)	(3)	(4)
Sample Restriction	High Income Families	Low Income Families	Educated Families	Less Educated Familes
Disclosed x Risky	0.115**	0.0470*	0.180***	0.0786***
	(0.0151)	(0.0742)	(0.000140)	(0.00446)
Observations	779	652	457	974
R-squared	0.421	0.332	0.422	0.332
	DV: Presence of Peeling Paint			
	(5)	(6)	(7)	(8)
Sample Restriction	High Income Families	Low Income Families	Educated Families	Less Educated Familes
Disclosed x Risky	0.00801	-0.0160	0.00440	-0.00379
	(0.556)	(0.144)	(0.712)	(0.745)
Observations	1,909	1,563	1,124	2,348
R-squared	0.074	0.104	0.163	0.063

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from 8 separate regressions. High income families are defined as families with income above median in each SMSA-Year cell. Educated families are defined as family head with at least college degree.

Table 9: Risk Mitigation Behaviors: Lead Dust in Owner Market

	DV: Lead Dust Levels(µ g/sq.ft.)				
	(1)	(2)	(3)	(4)	(5)
Sample	All	White	Non White	Black	Hispanic
Risky	0.252***	0.228**	0.234*	0.176	0.0841
	(0.00293)	(0.0430)	(0.0729)	(0.558)	(0.510)
Disclosed	0.0584	0.0993	0.147	1.327***	-0.234
	(0.666)	(0.550)	(0.501)	(0.00744)	(0.300)
Disclosed x Risky	-0.154	-0.0657	-0.276	-0.714	0.00190
	(0.147)	(0.647)	(0.114)	(0.156)	(0.992)
Observations	500	195	305	91	176
R-squared	0.095	0.186	0.126	0.415	0.082

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust cluster standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from 6 separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. For example, the estimated coefficients in columns (1) correspond to the effect of the Title X on properties built between 1975 and 1982 and sold between 1994 and 1999.

Table 10: Blood Lead Levels and Lead Dust, Owner and Rental Market

			Panel A: Owner Occupied Properties		
	(1)	(2)	(3)	(4)	(5)
Dependent Var	BLL	BLL	BLL	BLL	Mecury
Lead Dust Level	0.680***	0.629***	0.640***	0.284*	-0.105
	(0.211)	(0.215)	(0.233)	(0.158)	(0.427)
Observations	903	903	903	342	337
R-squared	0.109	0.149	0.144	0.162	0.100
			Panel B: Renter Occupied Properties		
	(1)	(2)	(3)	(4)	(5)
Dependent Var	BLL	BLL	BLL	BLL	Mecury
Lead Dust Level	0.642***	0.599***	0.569***	0.787***	0.359
	(0.125)	(0.123)	(0.120)	(0.264)	(0.320)
Observations	486	486	486	246	241
R-squared	0.232	0.291	0.297	0.315	0.160
Parental Education	No	Yes	Yes	Yes	Yes
Family Income	No	No	Yes	Yes	Yes
Race	No	No	Yes	Yes	Yes
Sample Restriction	No	No	No	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are reported in the parenthesis. This table reports regression coefficients from 10 separate regressions. Column (1) - (4) show the correlation between lead dust level and blood lead level. Column (5) shows the correlation between lead dust levels and blood mercury. Column (1) - (3) use all available data in NHANES, and column (4) and (5) use children who live in houses built between 1960 and 1989, and those were under 6 years old, when they moved into the property.

Table 11: Health Effects: Blood Lead Levels in Owner Market

	DV: Blood Lead Level (µg/dL)					
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	White	Non White	Black	Hispanic
Risky	0.0665	-0.0196	0.0253	-0.127	0.207	-0.205
	(0.365)	(0.801)	(0.783)	(0.349)	(0.342)	(0.118)
Disclosed	-0.0954	-0.170	-0.239	0.0853	-0.0504	0.371
	(0.486)	(0.201)	(0.157)	(0.730)	(0.887)	(0.354)
Disclosed x Risky	0.0954	0.194	0.238	-0.0431	-0.0217	-0.0697
	(0.556)	(0.229)	(0.236)	(0.874)	(0.959)	(0.858)
Parental Education	No	Yes	Yes	Yes	Yes	Yes
Family Income	No	Yes	Yes	Yes	Yes	Yes
Race	No	Yes	No	No	No	No
Observations	1,203	1,203	489	714	241	420
R-squared	0.131	0.188	0.209	0.170	0.357	0.183

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. P-values, which are reported in parenthesis, are based on robust standard errors. This table reports regression coefficients from 6 separate regressions. I restrict my attention to children living in owner-occupied houses built between 1960 and 1989, and those who were under six years old when they moved into the property. The regression sample changes as one moves across the columns, indicated by the column headings. Other variables in regression but omitted inlcude: year fixed effects, age fixed effects, and gender.

Table 12: Price Effects: The Effect of Information Disclosure on Rental Prices

Sample Restriction	DV: ln(rent)				
	(1)	(2)	(3)	(4)	(5)
Year Built	75-82	75-82	75-82	75-82	75-82
Year Sold	94-99	93-00	92-01	92-01	92-01
Disclosed	0.104	0.000513	0.0957	0.101**	0.107***
	(0.187)	(0.995)	(0.121)	(0.0468)	(3.47e-05)
Risky	-0.0174	-0.00460	-0.0117	0.0276**	
	(0.386)	(0.747)	(0.478)	(0.0478)	
Disclosed x Risky	-0.0156	-0.0330	-0.0287	-0.0289	-0.0229
	(0.545)	(0.122)	(0.169)	(0.224)	(0.456)
Housing Characteristics	Yes	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
MSA fixed effect	Yes	Yes	Yes	Yes	Yes
House Fixed Effects	No	No	No	No	Yes
Observations	4,504	5,960	7,417	10,815	10,815
R-squared	0.244	0.227	0.233	0.249	0.667

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from five separate regressions.Year 2000 dollars are used. Please see Table (2) for a list of the control variables used in each specification.

Table 13: Demographic Composition: Children under Six in Rental Market

Sample Restriction	***DV: Whether child under 6 presence in the house***				
	(1)	(2)	(3)	(4)	(5)
Year Built	75-82	75-82	75-82	75-82	75-82
Year Sold	94-99	93-00	92-01	92-01	92-01
Disclosed	-0.0171	-0.00144	0.00485	0.0332	0.0290
	(0.447)	(0.964)	(0.921)	(0.487)	(0.359)
Risky	-0.0497***	-0.0228*	-0.0216*	-0.0282***	
	(5.50e-05)	(0.0557)	(0.0556)	(0.00474)	
Disclosed x Risky	0.00798	-0.00632	0.00109	0.0153	-0.0247
	(0.510)	(0.599)	(0.930)	(0.274)	(0.205)
Housing Characteristics	Yes	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
MSA fixed effect	Yes	Yes	Yes	Yes	Yes
House Fixed Effects	No	No	No	No	Yes
Observations	4,477	5,929	7,379	10,649	10,649
R-squared	0.145	0.141	0.143	0.212	0.632

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust clustered standard errors at MSA and whether the house is built before and after 1978, are reported in parenthesis. This table reports regression coefficients from five separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. Please see Table (2) for a list of the control variables used in each specification.

Table 14: Demographic Composition: Different Family Types in Rental Market

Family Type	(1a) White Family	(1b) Minority Family	(1c) Black Family	(1d) Hispanic Family
Disclosed x Risky	0.006	-0.015	-0.0145	-0.0008
	(0.846)	(0.631)	(0.523)	(0.969)
Observations	10,026	10,026	10,026	10,026
Family Type	(2a) White Parents	(2b) Minority Parents	(2c) Black Parents	(2d) Hispanic Parents
Disclosed x Risky	-0.00442	-0.00160	-0.00234	0.000741
	(0.853)	(0.919)	(0.823)	(0.947)
Observations	10,026	10,026	10,026	10,026
Family Type	(3a) Family Head with College Degree	(3b) Family Head with No More than HS Degree	(3c) White Parents with College Degree	(3d) White Parents with No More than HS Degree
Disclosed x Risky	-0.0186	-0.00217	-0.0109*	-0.00724
	(0.302)	(0.921)	(0.0915)	(0.666)
Observations	10,026	10,026	10,026	10,026
Family Type	(4a) Minority Parents with No More than HS Degree	(4b) Black Parents with No More than HS Degree	(4c) Hispanic Parents with No More than HS Degree	(4d) Seniors with Each Family Member Older than 60
Disclosed x Risky	0.0189*	0.0119	0.00408	0.000551
	(0.0641)	(0.209)	(0.632)	(0.963)
Observations	10,026	10,026	10,026	10,026
Housing Characteristics	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes
MSA fixed effect	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports effects of Title X from 16 separate regressions using same sample restriction but different dependent variables. Sample restriction: houses built between 1975 and 1982, and sold between 1992 and 2001. The type of a family is categorized by the family head. All these regressions include other independent variables, and coefficient of those variables are not reported. Please see Table (2) for a list of the control variables used in each specification.

Table 15: Risk Mitigation Behaviors: The Presence of Peeling Paint in Rental Market

Sample Restriction	***DV: Presence of Peeling Paint***				
	(1)	(2)	(3)	(4)	(5)
Year Built	75-82	75-82	75-82	75-82	75-82
Year Sold	94-99	93-00	92-01	92-01	92-01
Disclosed	0.0104**	-0.00774*	0.00673***	-0.00189	0.00814
	(0.0240)	(0.0613)	(0.00153)	(0.793)	(0.397)
Risky	-0.00407	0.00709	0.00171	0.00550	
	(0.405)	(0.112)	(0.581)	(0.240)	
Disclosed x Risky	0.00618	-0.00646	-0.000734	-0.00638	-0.0177**
	(0.409)	(0.222)	(0.858)	(0.290)	(0.0470)
Housing Characteristics	Yes	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
MSA fixed effect	Yes	Yes	Yes	Yes	Yes
House Fixed Effects	No	No	No	No	Yes
Observations	4,504	5,960	7,417	10,815	10,815
R-squared	0.105	0.108	0.100	0.015	0.397

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from five separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. For example, the estimated coefficients in columns (1) correspond to the effect of the Title X on properties built between 1975 and 1982 and sold between 1994 and 1999.

Table 16: Risk Mitigation Behaviors: The Presence of Peeling Paint in Rental Market, Using Study Sample Only

Sample Restriction	DV: Presence of Peeling Paint (1)	(2)	(3)
Year Built	75-82	75-82	75-82
Year Sold	94-99	93-01	92-01
Disclosed	-0.0226	-0.00519	0.00797
	(0.152)	(0.580)	(0.403)
Disclosed x Risky	-0.00270	-0.0213**	-0.0177**
	(0.851)	(0.0348)	(0.0480)
Observations	6,722	9,838	10,815
R-squared	0.474	0.385	0.397

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust cluster standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from three separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. For example, the estimated coefficients in columns (1) correspond to the effect of the Title X on properties built between 1975 and 1982 and sold between 1994 and 1999.

Table 17: Risk Mitigation Behaviors: Lead Dust in Rental Market

	DV: Lead Dust Levels(μ g/sq.ft.)				
	(1)	(2)	(3)	(4)	(5)
Sample	All	White	Non White	Black	Hispanic
Risky	0.528***	0.483***	0.644***	0.242**	0.488**
	(0.00178)	(3.30e-06)	(0.00526)	(0.0339)	(0.0197)
Disclosed	0.280	0.388	0.538**	0.212	0.408
	(0.115)	(0.104)	(0.0487)	(0.618)	(0.241)
Disclosed x Risky	-0.438**	-0.424***	-0.549**	-0.184	-0.400*
	(0.0148)	(0.00159)	(0.0235)	(0.402)	(0.0772)
Observations	317	87	230	103	108
R-squared	0.150	0.362	0.146	0.125	0.308

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust cluster standard errors at MSA and whether the house is built before and after 1978. This table reports regression coefficients from 6 separate regressions. The regression sample changes as one moves across the columns, indicated by the column headings. For example, the estimated coefficients in columns (1) correspond to the effect of the Title X on properties built between 1975 and 1982 and sold between 1994 and 1999.

Table 18: Health Effects: Blood Lead Levels in Rental Market

	DV: Blood Lead Level (μg/dL)						***Mercury***
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	All	All	White	Non White	Black	Hispanic	All
Risky	0.939***	0.976***	1.568**	0.849***	1.121***	1.154***	-1.132
	(0.00157)	(0.000342)	(0.0270)	(0.000317)	(0.00640)	(0.000764)	(0.414)
Disclosed	0.0793	0.0921	0.391	0.186	-0.624	0.212	-1.297
	(0.757)	(0.688)	(0.481)	(0.460)	(0.263)	(0.541)	(0.231)
Disclosed x Risky	-0.868***	-0.905***	-1.621**	-0.798***	-0.824*	-0.781**	0.672
	(0.00708)	(0.00190)	(0.0378)	(0.00387)	(0.0892)	(0.0472)	(0.636)
Parental Education	No	Yes	Yes	Yes	Yes	Yes	Yes
Family Income	No	Yes	Yes	Yes	Yes	Yes	Yes
Race	No	Yes	No	No	No	No	Yes
Observations	521	521	117	404	174	189	420
R-squared	0.140	0.249	0.427	0.256	0.330	0.294	0.218

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust standard errors. This table reports regression coefficients from 6 separate regressions. I restrict my attention to children living in renter-occupied houses built between 1960 and 1989, and those were under 6 years old when they moved into the property. The regression sample changes as one moves across the columns, indicated by the column headings. Other variables in regression but omitted inlcude: year fixed effects, age fixed effects, and gender. The dependent variable for column (7) is the blood mercry level (μmol/L).

3.0 WILDFIRE AND INFANT HEALTH

(with Shawn McCoy)

Wildfires have increased in intensity and frequency. Relative to the 1980s, they are six times more likely to occur and once they ignite, they grow four times as large (Westerling et al., 2006). Roughly 100,000 wildland forest fires occur in the United States each year.[1] Part of this trend may be due to changes in global climates (Westerling et al., 2006; Gillett et al., 2004). Recent expansion of residential housing into forested lands may be another factor. As a result of population de-concentration, urban areas are increasingly interdigitating with wild and rural lands creating what has been called the Wildland-Urban Interface (WUI). As of 2005, the WUI contained 39% of the stock of residential housing units across the United States (Radeloff et al., 2005). It has been argued that the sprawling configurations of WUI developments have modified the interactions between environmental and socio-economic dynamics leading to an increase in the likelihood of severe wildfires in inhabited spaces (Radeloff et al., 2005; Spyratos et al., 2007).

Elevated concentrations of fine particulate matter ($PM_{2.5}$) is often considered to be the principal threat of wildfire to public health (Jaffe et al. 2008). While $PM_{2.5}$ is a term used to refer to fine particulates suspended in the air less than 2.5 micrometers in diameter, the size of particles found in wildfire smoke are on the lower end of this spectrum typically between .4 and .7 micrometers in diameter; the same as the spectral range of visible light and small enough to penetrate the lungs and the heart (Lipsett and Materna, 2008; Hueglin et al., 1997).

In the United States, wildfire accounts for a notable proportion of total annual $PM_{2.5}$

[1] Wildfires: Dry, hot, and windy. *National Geographic*, (2013). http://environment.national geographic.com/environment/naturaldisasters/wildfires/.

emissions. To illustrate this, in Figure (8) we plot $PM_{2.5}$ emissions trends expressed as a percentage of total annual emissions using the EPAs 1970-2014 Air Pollutant Emissions Trends Data[2]. This graph shows that wildfire has accounted for approximately 20% of total annual $PM_{2.5}$ emissions in the United States between the years 2002 and 2013[3]. In recent years, $PM_{2.5}$ emissions in the U.S. due to wildfire surpassed emissions due to highway and off-highway vehicles as well as emissions due to fuel combustion from electric utility, industrial, commercial, institutional sectors and residential use.

Collectively, these observations motivate us to ask, "What are the public health implications of wildfire?" Another question equally important to the design of economically efficient fire management policies is, "On what spatial scales do the health impacts of wildfire matter?" In this paper, we investigate the public health implications of wildfire by estimating the spatio-temporal dynamics between wildfire and infant birthweight. While birthweight is only one of the many relevant health outcomes potentially influenced by fire, it is a useful metric to consider since it has been shown to be linked to short-term health outcomes, such as one-year mortality rates, as well as longer-term outcomes such as educational attainment and earnings (Almond and Currie, 2011; Black et al., 2007). While the physiological pathway between fine particulate exposure and birthweight remains unclear, researchers hypothesize that after inhalation, the particles and toxicants contained in wildfire smoke cross through the placenta disrupting fetal nutrition and oxygen flow resulting in fetal growth retardation (Jayachandran, 2009; Berkowitz et al., 2003; Dejmek et al., 1999; Wang et al., 1997).

The existing literature points towards air pollution as one channel through which wildfire may impact public health; however, the stress placed on mothers living in close proximity to a wildfire may also play a role. As indicated by Dunkel Schetter (2011), there is a growing body of work linking maternal depressive symptoms as well as general distress during pregnancy to reduced birthweight. Motivated by this literature, we formulate our empirical strategy with the goal of quantifying the relative importance in-utero stress plays in explaining reductions in birthweight following a fire. Teasing out health effects due to stress from health effects due to ambient air pollution is a unique empirical challenge since

[2]http://www.epa.gov/air-emissions-inventories/air-pollutant-emissions-trends-data.

[3]Estimates of the percent of total annual $PM_{2.5}$ emissions due to wildfire in the United States reported by Urbanski et al. (2011), Mallia et al. (2015), and Zhang et al. (2006) range from 20% to 40%.

both effects are presumably increasing with respect to proximity to a fire. We approach this problem by utilizing a spatial, difference-in-differences estimation framework. To implement this method, we assembled spatial data for every fire in Colorado between 2002 and 2013. We then constructed a fine-scaled dataset delineating wildfire smoke plumes as well as the prevailing wind direction near each fire. Each wildfire smoke plume was re-constructed in GIS using a series of daily satellite images taken around the ignition date of each fire. These datasets were subsequently linked to a confidential database detailing the birthweights of every infant born in Colorado since 2002 using the latitude and longitude co-ordinates corresponding to the home address of each infant's mother.

Together, the data and our empirical framework allow us to explore how wildfires influence infant birthweight across multiple geo-spatial dimensions of treatment. In our baseline empirical specifications, we present models of the overall effects of fire by comparing the birhweights of infants in close proximity to a wildfire (treatment group) to the birthweights of infants in less proximate areas (control group) letting the data drive the spatial cutoff delineating the treatment and the control groups. In subsequent specifications, we partition infants in the treatment group into two sub-groups based on each infant's location relative to the presence of wildfire smoke: The set of infants located inside of a wildfire smoke plume and the set of infants located outside of a wildfire smoke plume. Estimating changes in birthweight in each of these sub-groups allows us to draw inferences regarding the significance of the roles that in-utero stress (as captured by proximity to a wildfire) and ambient air pollution play in explaining reductions in birthweight.

To preview our empirical results, model estimates show that wildfire smoke leads to statistically significant, 4% to 6% reduction in birthweight. These effects are most pronounced among mothers exposed to smoke during their second and third trimesters of pregnancy and attenuate with respect to distance to a fire. Drawing on estimates reported by Black et al. (2007), these effects translate into: a .34 to .45 centimeter reduction in height at age 18; a .54 to .72 percent decrease in full-time earnings; and a statistically significant decrease in the probability of high school completion. We find no statistically significant relationships between proximity to wildfire and the birthweights of infants located outside the path of wildfire smoke; as such, our models fail to validate in-utero stress (as captured by proximity to a fire) as a mechanism through which wildfire impacts infant health.

We proceed as follows. We begin by providing a background of the related literature in Section (4.1). We characterize our study area and the construction of our geo-spatial data in Section (4.2). We present our empirical methodology in Section (4.3) and our findings in Section (4.4).

3.1 BACKGROUND

This study contributes to three distinct literatures: An emerging literature focused on the public health implications of wildfire; a larger literature dedicated to understanding the health consequences of ambient air pollution; and a branch of the literature dealing with the links between in-utero stress and fetal health.

3.1.1 Health Impacts of Wildfire

Jayachandran (2009) explores the effects of the 1997 forest fires in Indonesia on early-life mortality. Using daily data on airborne smoke, the author estimates that fire-driven increases in air pollution leads to a significant increase in child mortality evidenced by a rise in the number of children missing in the 2000 Indonesian Census. Moeltner et al. (2013) study the effects of wildfire in the Reno/Sparks area of Northern Nevada utilizing data on daily hospital admissions. A unique feature of these authors' study is their use of data on prevailing wind direction near wildfires. Moeltner et al. (2013) estimate a significant increase in hospital patient counts downwind and as far as 300 miles away of a burn site.

Most closely related to our work, Holstius et al. (2012) study the effects of the 2003 Southern California wildfires on infant birthweight. Using information identifying the Census tract of each mother, these authors restrict attention to infants born in the South Coast Air Basin[4] for which wildfire smoke due to the 2003 wildfires was presumed to be heavily concentrated. They subsequently compare the birthweights from pregnancies before, during,

[4]This area includes Orange County and portions of Los Angeles, San Bernadino, and Riverside Counties.

and after the 2003 event. Compared to pre-fire births, Holstius et al. (2012) estimate a significant reduction in the birthweights of infants that were exposed to wildfire in their third and second trimesters of gestation. Khawand (2015) also explores the links between wildfire-fueled changes in air pollution and perinatal health. This author relates county-level health outcomes on observed concentrations of $PM_{2.5}$ at pollution monitoring stations instrumented by simulated $PM_{2.5}$ from recent wildfires. He finds that a $10\mu g/m^3$ increase in monthly $PM_{2.5}$ concentrations leads to one additional pre-mature death per 100,000 individuals; an effect which appears to be driven by deaths from cardiovascular and respiratory diseases among individuals over 65. Khawand (2015) also estimates a negative, but statistically insignificant reduction in birthweight. The health impacts of wildfire have been considered in other notable works using data on eye and respiratory symptoms, medication use, and physician visits (Kunzli et al., 2006), cardiorespiratory-related deaths (Kochi et al., 2012), and asthma (Johnston et al., 2002).

One dimension this paper helps to improve our understanding of the health impacts of fire is the advance we make with the dataset we construct. As detailed more carefully in the sections that follow, we use GIS to re-construct a series of wildfire smoke plumes that occurred across our study area; a dataset which we can fully utilize by incorporating geographic information for each individual in our sample. These data allow us to formulate an empirical strategy capable of delivering a more nuanced analysis of the spatio-temporal dynamics between fire and infant health than was previously possible in the extant literature.

3.1.2 Physiological Effects of Ambient Air Pollution.

There is a large literature focused on estimating the physiological effects of ambient air pollution from sources not including fire. Within this literature, epidemiological investigations are the most abundant. As of the year 2000, Pope III (2000) identified over 150 published epidemiological studies on the health effects of particulate air pollution. The majority of these studies focus on mortality, daily hospital admissions, respiratory symptoms, and lung function. Pope III (2000) identifies a general consensus in this literature that particulate air pollution – primarily due to combustion-source pollutants – is a leading risk factor for cardiopulmonary diseases and mortality. More recent studies have turned their attention to the effects of air pollution on fetal health outcomes. As summarized by Currie and Walker (2011), these include: Gilbert et al. (2003); Glinianaia et al. (2004); Currie et al. (2009); Huynh et al. (2006); Lee et al. (2008); Leem et al. (2006); Liu et al. (2007); Parker et al. (2008); Salam et al. (2005); Ritz et al. (2006); Woodruff et al. (2008); Wilhelm and Ritz (2003); Ponce et al. (2005); Brauer et al. (2003); Slama et al. (2007); Beatty and Shimshack (2011); Karr et al. (2009). The majority of these papers find strong correlations between ambient air pollution and fetal health. However, it has been argued that many of these studies are limited by the identification strategies they employ (Goodwin, 2015; Pope III, 2000). Even in light of these arguments, these papers set the foundation for our understanding of the spectrum of potential health effects due to air pollution. Focused more closely on the problem of identification, newer studies have emerged using quasi-experimental techniques more commonly found in the economics literature. These include: Chay and Greenstone (2003); Currie and Neidell (2005); Parker et al. (2008); Currie et al. (2009); Currie and Walker (2011); Hill (2013); Severnini (2014); Goodwin (2015), and Currie et al. (2015). The identification strategy we employ in our paper is inspired by the work of Currie et al. (2009), Currie and Walker (2011), and Currie et al. (2015).

Currie et al. (2009) estimate the effects of ambient air pollution on fetal health outcomes by linking pollutant levels from air monitoring stations to mothers using the latitude and longitude coordinates associated with each mother's home address. Restricting attention to mothers living in close proximity to monitoring stations, they find significant negative effects of exposure to CO on birth outcomes, but fail to find any significant effects due to PM_{10}. Using the introduction of the electronic toll collection (E-ZPass) – which reduced vehicle emissions near highway toll plazas – Currie and Walker (2011) estimate an 11.8% reduction in the incidence of low birthweight among infants located within 2km of a toll plaza relative to the birth outcomes of infants between 2km and 10km. Also using micro-data on infants, Currie et al. (2015) compare birth outcomes within 1 mile of a toxic plant to birth outcomes between 1 and 2 miles, before and after the opening or closure of 1600 plants. These authors report a 3% increase in the probability of low birthweight within 1 mile of a plant.

Each of these studies utilize micro-data on the location of infants; information not incorporated by previous works. One way that we contribute to this literature is by introducing a new measure of ambient air pollution. Currie et al. (2009) measure air pollution using pollutant levels from air monitoring stations. Currie and Walker (2011) and Currie et al. (2015) use proximity to the pollutant source as a proxy for exposure.

In our study, we digitally re-construct wildfire smoke plumes captured by satellite imagery. Each metric has its own advantages and dis-advantages. One advantage of our approach is that it allows us to estimate the dynamics between wildfire and infant birthweight across multiple geo-spatial scales based on each infant's proximity to a fire and location relative to wildfire smoke. One dis-advantage to our approach is that while we can proxy for the density of fine-particulate matter within wildfire smoke plumes with distance to fire, we cannot assign specific $PM_{2.5}$ levels to each infant within these plumes.

While much of the extant literature utilizes highly sophisticated methodological tools applied to novel and relevant data, Currie and Walker (2011) highlights the importance of accounting for the possibility that changes in air pollution may induce geographical sorting on the basis of household characteristics. This consideration is motivated by the earlier work of Banzhaf and Walsh (2008). Using data from the Toxics Release Inventory of the US Environmental Protection Agency (EPA), Banzhaf and Walsh (2008) identify a link between changes in environmental quality and changes in local demographics which may ultimately be correlated with health; effects that are driven by increases in the demand for lands in improving neighborhoods.

These authors' theoretical and empirical work clarifies a potential threat to identification strategy faced by researchers seeking to identify the causal effects of ambient air pollution. Namely, changes in the environmental quality of a particular region may attract new residents with poorer health outcomes. The magnitude of this potential bias is closely related to the rate of migration. In our application, the timing of a wildfire and the timing of particle emissions fueled by a wildfire coincide. Additionally, both the timing of wildfire ignitions as well as the spatial variation in wildfire smoke are plausibly random processes. These properties of wildfire – which we can effectively leverage with our data – allow us to draw new inferences regarding the health impacts of ambient air pollution with less concern of bias due to sorting.

3.1.3 Natural Disasters & Physiological Stress

Lastly, our study contributes to a small literature dealing with the effects of natural disasters on public health. For instance, Simeonova (2011) investigates the effects of natural disasters on pregnancy outcomes by relating county-level data on birth outcomes to the incidence of disasters by disaster type. Using data for the period of 1968-1988, the author shows that experience with an extreme weather event raises the chances of premature birth and lowers the gestational length of pregnancies. Utilizing birth record data in Chile, Torche (2011) finds a significant decline in the birthweight of infants located in counties that were exposed to a high-intensity earthquake. Finally, Currie and Rossin-Slater (2013) use geo-coded vital statistics records to examine the effects of hurricanes on birth outcomes. These authors compare the birth outcomes of infants living within 30km of the path of a hurricane to the outcomes of infants in the immediately adjacent area, but fail to detect any significant relationships between hurricane exposure during pregnancy and birthweight. These papers are part of a broad medical literature which considers the effects of in-utero stress on fetal health. Most notably, Aizer et al. (2009) utilize longitudinal data to study the effects of elevated levels of key stress-hormones in pregnant mothers but fail to detect any significant effects on birthweight. Even in light of these findings, Dunkel Schetter (2011) notes there still exists a general consensus among scholars that maternal depressive symptoms as well as general distress during pregnancy are strong predictors of reduced birthweight. We contribute to this literature by quantifying the relative importance in-utero stress (as captured by proximity to a fire) plays in explaining reductions in birthweight following a wildfire.

3.2 DATA

This paper studies the effects of wildfires in the State of Colorado between the years 2002 and 2013. Spatial data delineating wildfire burn scars were obtained from the Geospatial Multi-Agency Coordination Group (GeoMAC)[5] and Monitoring Trends in Burn Severity (MTBS)[6].

[5] http://www.geomac.gov/index.shtml.

[6] http://www.mtbs.gov/.

The data provided by GeoMAC and MTBS were linked to information contained in each fire's Incident Status Summary report (ICS-209) which were obtained from the National Fire and Aviation Management Web Application[7] maintained by the National Inter-agency Fire Center[8]. These reports were used to determine the date each fire ignited as well as the prevailing wind direction near each burn site. Where applicable, fire ignition dates were cross-checked with dates reported by FEMA. In a small handful of cases, we identify a one to five day discrepancy in ignition dates; to control for this, we drop observations from our sample that occurred within five days of the ignition dates reported in each fire's ICS-209 report. We identify a total of 161 fires. The extent of our study area and the location of each fire in our sample are shown in Figure (9).

We re-construct wildfire smoke plumes in GIS using a series of daily satellite images of our study area taken by the MODIS[9] instrument on board the Terra and Aqua spacecrafts. These data, which have a temporal coverage of 2007 - 2015, were provided courtesy of the University of Wisconsin-Madison Space Science and Engineering Center[10]. We overlay eight satellite images for each fire. These include four images from the Terra satellite and four images from the Aqua satellite taken on each of the first four days following the ignition date of each fire. We then we trace out the extent of visible smoke in each image and store this information in GIS. We re-construct each fire's smoke plume by dissolving each smoke polygon from each satellite image into a single polygon. We illustrate a sample fire and smoke plume in Figure (11). At a resolution of 250m, we were unable to re-construct smoke polygons for many of the smaller fires in our sample. Also, while we are able to identify prevailing wind directions for all fires dating back to 2002, the set of satellite images we use only dates back to 2007. In a small handful of cases, we were unable to detect wildfire smoke due to excessive cloud cover. We successfully constructed wildfire smoke polygons for 28 wildfires. We illustrate the final set of smoke plumes associated these fires in Figure (12).

A confidential database detailing the vital statistics and natality records for every infant born in the state of Colorado between 2002 and 2013 was obtained under a confidential data

[7]http://fam.nwcg.gov/fam-web/.
[8]http://www.nifc.gov/.
[9]http://modis.gsfc.nasa.gov/data/.
[10]http://www.ssec.wisc.edu/.

agreement with the Center for Health and Environment Data at the Colorado Department of Public Health and Environment. These data include: information on the birthweight and gestational age of each infant as well as demographic information for each infant's mother including: Race (white, black, hispanic, or other); education level (graduate or professional degree, bachelor's degree, associate degree, some college (but no degree), high school graduate or GED, 9th to 12th grade (but no diploma)); marital status; and age. These data also include the latitude and longitude coordinates associated with each mother's home address. We restrict attention to full term pregnancies (39 to 42 weeks gestation) and exclude infants with missing information regarding their birthweight, gestational age, or the demographic characteristics of their mothers.

3.3 METHODS

Our empirical analysis compares the birthweights of infants before and after wildfires across various dimensions of treatment using a difference-in-differences estimation strategy. To implement this procedure, we assign each infant, i, to its nearest fire $m \in M$ that occurred within plus or minus nine months of each infant's birth date[11] restricting attention to infants less than five miles of a fire. To minimize any confounding effects of exposure to other fires, we drop any observation from our sample that lies within five miles of more than one fire.

We report the descriptive statistics of our data in Table (13). Column (1) shows sample means for our complete sample which consists of 7,398 births; standard deviations are shown in parenthesis. Each infant included in Column (1) is located near a fire with data regarding prevailing wind direction; we refer to this sample as our *wind sample*. Column (2) is a sub-sample of Column (1). We construct the sub-sample in Column (2) by restricting attention

[11] To implement this procedure, we compute the nearest distance to the nearest point of every fire, m, for every infant, i, recording both the ignition date of the fire and the birth date of the infant. For each infant i, we rank each fire in ascending order with respect to the distance between each fire and said infant; for infant i, let $\{m_{i1},\ m_{i2}, \ldots, m_{in}\}$ denote the rank ordering of fires. We then assign infant i to fire m_{i1} conditional on m_{i1} igniting within plus or minus nine months of the infant's birth date. If fire m_{i1} failed to ignite within plus or minus nine months of the infant's birth date, we assign infant i to m_{i2}. We continue iterating using this procedure until each infant is assigned to its nearest fire that occurred within plus or minus nine months of each infant's birth date.

to the set of infants located near a fire with spatial data delineating said fire's smoke plume; we refer to this sample as our *smoke sample*.

For each treatment group, our baseline empirical specification takes the form:

$$\begin{aligned} y_{itm} &= \alpha \cdot Post_{itm} + \beta \cdot Treat_{im} \times Post_{itm} + \gamma^m \cdot Treat_{im} \\ &\quad + Z_i' \omega_1 + G_{it}' \omega_2 + \tau_{it} + \epsilon_{itm}, \end{aligned} \tag{3.1}$$

where $Post_{itm}$ is a post-fire dummy and $Treat_{im}$ is a treatment group indicator. For each treatment definition, we are interested in the estimate on the coefficient of the treatment-group by post-fire interaction term, β. To control for composition effects, we allow our main effect to vary by fire by including a full set of treatment group by fire fixed effects, $\gamma^m \cdot Treat_{im}$. Z_i' is a vector of controls which includes: an indicator variable for mothers' marital status; indicator variables for mothers' race (white, black, hispanic, or other) and education level (graduate or professional degree, bachelor's degree, associate degree, some college (but no degree), high school graduate or GED, 9th to 12th grade (but no diploma)); each mother's age; and the gestational age of each mother's infant. G_i' is a vector of fire-specific geographic controls which includes the elevation at each infant's home, the distance between each infant's home and wildfire, and the interaction between elevation and distance. Finally, τ_{it} is a set of year-quarter fixed effects.

To identify trimester-specific effects we replace $Post_{itm}$ with three post-fire indicator variables, $\{Tri_{k,itm}\}_{k=1}^{3}$, indicating the trimester of pregnancy each mother was exposed to fire. This transforms the baseline specification into:

$$\begin{aligned} y_{itm} &= \sum_{k=1}^{3} \left(\alpha^k \cdot Tri_{k,itm} + \beta_k \cdot Treat_{im} \times Tri_{k,itm} \right) + \gamma^m \cdot Treat_{im} \\ &\quad + Z_i' \omega_1 + G_{it}' \omega_2 + \tau_{it} + \epsilon_{itm}. \end{aligned} \tag{3.2}$$

For each treatment definition, we are interested in coefficient estimates for $\{\beta_k\}_{k=1}^{3}$. These coefficients correspond to the difference-in-differences estimates of fire on the birthweight of infants exposed to a fire during their k^{th} trimester of pregnancy.

3.3.1 Treatment Definitions

Our starting point for estimating the effects of wildfire on birthweight is a proximity analysis that compares the birth outcomes of infants located within a certain radius of a wildfire to the birth outcomes of infants located in the immediately adjacent (less proximate) area. We operationalize these tests by estimating variants of equation (3.2) with the treatment variable $1Mile_{im}$ which equals one for any infant located within one mile of a wildfire and zero otherwise. This approach – which might be thought of identifying the net-effect of living in close proximity to a wildfire – is motivated by its prevalence in the literature. Currie and Walker (2011), for instance, compare birth outcomes of infants within 2km of a toll plaza to the birth outcomes of infants between 2km and 10km. Currie et al. (2015) investigate the health outcomes of infants located within 1 mile of a toxic plant to health outcomes of infants located in the immediately adjacent area. As we discuss in more detail in the sections that follow, while we present our empirical models using a one-mile treatment/control cutoff, we graph estimates of each coefficient of interest in each of our models for a complete range of treatment/control cutoff values, in effect, allowing us to quantify how the impacts of fire vary across space.

As we allude to above, one empirical challenge we face is the task of estimating the extent to which changes in health outcomes identified from our proximity analysis are driven by ambient air pollution and by stress. We approach this problem by identifying the portions of the landscape surrounding each wildfire that were polluted and relatively less-polluted using information on prevailing wind direction as well as our spatial data on wildfire smoke. This requires us to determine whether each birth in our data is located upwind or downwind of a fire. We operationalize this determination in GIS by computing the angle between each birth and each fire. To identify the set of births *upwind* of a fire, we flag observations located $\pm 45^0$ of the prevailing wind direction near a fire. Using this information, we decompose infants included in the treatment group $1Mile_{im}$ into two sub-groups, $Stressed_{im}$ and $Polluted_{im}$, such that $Stressed_{im} + Polluted_{im} = 1Mile_{im}$. We perform this de-composition two different ways. For our wind sample, $Stressed_{im}$ is a dummy set equal to one for any infant located upwind of a wildfire. This variable captures the effects of living in regions in close proximity to a disaster, but that were relatively less polluted. In contrast, $Polluted_{im}$ is a dummy variable set equal to one for any infant located downwind and within one mile of wildfire. For our smoke sample, $Polluted_{im}$ is an indicator set equal to one for any infant located inside a smoke plume and within one mile of a wildfire. Likewise, $Stressed_{im}$ is an indicator set equal to one for any infant located within one mile, but outside of a wildfire smoke plume. The de-composition of the indicator variable $1Mile_{im}$ into $Stressed_{im}$ and $Polluted_{im}$ transforms the empirical specification in (3.2) into:

$$\begin{aligned} y_{itm} &= \sum_{k=1}^{3} \big(\alpha^k \cdot Tri_{k,itm} + \beta_k^{Stressed} \cdot Stressed_{im} \times Tri_{k,itm} \\ &\quad + \beta_k^{Polluted} \cdot Polluted_{im} \times Tri_{k,itm}\big) + \gamma^m \cdot Stressed_{im} \\ &\quad + \gamma^m \cdot Polluted_{im} + Z_i'\omega_1 + G_{it}'\omega_2 + \tau_{it} + \epsilon_{itm}. \end{aligned} \tag{3.3}$$

Of interest to our analysis are coefficient estimates of $\{\beta_k^{Stressed}\}_{k=1}^3$ and $\{\beta_k^{Polluted}\}_{k=1}^3$ which identify changes in the health outcomes of infants located in each treatment group exposed to fire during their k^{th} trimester of pregnancy relative to changes in the health outcomes of infants in each respective control group. In our empirical work, we compare and contrast estimates of $\beta_k^{Stresssed}$ and $\beta_k^{Polluted}$ obtained using our wind metrics and smoke metrics to assess wildfire smoke exposure.

3.4 RESULTS

We begin our formal analysis by estimating equations (3.2) and (3.3) separately for our wind sample and our smoke sample. Columns (1) and (3) of Table (3.2) present difference-in-differences estimates of these regressions with p-values in parenthesis. Columns (1) and (3) compare the birthweights of infants within one mile of a wildfire to the birthweights of infants located between one and five miles. Difference-in-differences estimates of $1Mile \times Tri\ 3$ in Columns (1) and (3) show that, on average, infants within one mile of a fire incur a 2.5% to 3.9% reduction in birthweight when fires ignite during mothers' third trimesters of pregnancy;

however, these effects are statistically insignificant with p-values of .17 and .16, respectively. Estimates of $1Mile \times Tri\ 2$ and $1Mile \times Tri\ 1$ are also statistically insignificant.

Figures (11) and (12) suggest that the concentration of wildfire smoke at any given location may be dependent upon the direction of prevailing wind. Leveraging this feature of the data to our advantage, in Column (2) we report estimates of ($Polluted \times Tri\ k$) and ($Stressed \times Tri\ k$). These estimates are obtained by applying estimating equation (3.3) to our wind sample. Model estimates of ($Polluted \times Tri\ 3$) show that infants located within one mile, but *downwind* of a wildfire, incur a statistically significant, 5.69% reduction in birthweight conditional on each infant's mother being exposed in her third trimester of pregnancy. Estimates of ($Polluted \times Tri\ 2$) also show a statistically significant, 4.5% reduction in birthweight in proximate, downwind regions of a fire as well. In contrast, referring to estimates of ($Polluted \times Tri\ 1$), we fail to detect any statistically significant effects of fire on infants exposed during their first trimester of pregnancy, even among infants located in a downwind region of a fire. Likewise, coefficient estimates of ($Stressed \times Tri\ 3$), ($Stressed \times Tri\ 2$), and ($Stressed \times Tri\ 1$) indicate no statistically significant effects of wildfire on the birthweight of infants *upwind* of a fire irrespective of the trimester in which they were exposed. Model results in Table (19) are based on the most robust version of estimating equation (3.3) which includes the complete set of demographic controls, geographic controls, and time fixed effects specified in Section (4.3)[12]. In Table (A3) in Appendix we show how the estimates in Column (2) of Table (19) change without and without the inclusion of each of these controls and fixed effects.

Next, we turn our attention to Column (4) of Table (19)[13]. Column (4) compares the outcomes of infants in polluted and non-polluted regions of a fire based on each infant's location with respect to a wildfire smoke plume. These estimates are obtained by applying estimating equation (3.3) to our smoke sample. Coefficient estimates of ($Polluted \times Tri\ 3$) indicate that infants inside a smoke plume incur a statistically significant, 4.8% reduction in birthweight conditional on each infant's mother being exposed in her third trimester. Estimates of ($Polluted \times Tri\ 2$) indicate a statistically significant, 3.8% reduction in birth-

[12]Please refer to the notes of Table (19) for a description of each variable included in these regressions.

[13]In Table (A4) in Supplementary Appendix we show how estimates in Column (2) of Table (A3) change without and without the set of of demographic controls, geographic controls, and time fixed effects.

weight. These estimates are qualitatively similar to estimates obtained from segmenting polluted regions near a wildfire on the basis of prevailing wind. Also similar to our analysis of birthweight and air pollution using prevailing wind, model estimates of ($Stressed \times Tri$ 3), ($Stressed \times Tri$ 2), and ($Stressed \times Tri$ 1), are statistically insignificant.

These results show that wildfire smoke has a detrimental impact on infant birthweight. Our findings also show that the effects of fire captured exclusively by proximity to a fire are not powerful enough to induce changes in fetal health. This finding is consistent with the earlier work of Currie and Rossin-Slater (2013) who find no statistically significant relationships between proximity to a recent hurricane and birthweight. We proceed by subjecting our model estimates to various robustness checks. We then characterize the spatial decay process between wildfire and birthweight.

3.4.1 Robustness Checks

Our empirical models based on equation (3.2) and the set of infants in our wind sample compare the outcomes of infants within one mile of a wildfire to the outcomes of infants in the immediately adjacent area, partitioning the set of infants within one mile of a fire into a polluted (downwind) group and a non-polluted (upwind) group. This approach implicitly compares the outcomes infants in each of these sub-groups to the outcomes of infants in a control group who may have resided in downwind regions of a wildfire. In Column (1) of Table (20) we replicate the baseline model reported in Column (2) of Table (19). Column (2) of Table (20) tests the sensitivity of the results shown in Column (1) to a full set of group by fire and group by trimester indicator variables for the set of infants located downwind of a fire, but *further* than one mile. We refer to observations that fall into these groups as "contaminated controls". The inclusion of these indicator variables changes the reference category for $Polluted \times Tri\ k$ and $Stressed \times Tri\ k$ from the set of infants located between one and five miles of a wildfire to the set of infants located between one and five miles and *upwind* of a fire without changing the sample of births across each model. We find no qualitative differences between the estimates in Column (2) and our baseline estimates in Column (1).

Prevailing wind direction is a useful proxy for identifying regions of the landscape near fires that experienced relatively higher concentrations of wildfire smoke and its usage is motivated by its prevalence in the extant literature. However, one might argue that the precision of this metric may be diminished if the wind patterns surrounding each fire change frequently over the course of each fire's burn. We test for this potential bias using detailed information from each fire's ICS-209 report. In addition to indicating the prevailing wind direction near each fire, these reports include a separate note indicating if the wind patterns were "erratic" or not. Column (3) re-estimates the baseline model restricting attention to fires that did not receive an erratic wind flag; coefficient estimates are also qualitatively similar to those reported in Column (1).

Our baseline models that apply equation (3.3) to our smoke sample compare the outcomes of infants within one mile of a wildfire to the outcomes of infants in the immediately adjacent area, partitioning the set of infants within one mile of a fire based on their location with respect to wildfire smoke plumes. This approach implicitly compares outcomes in each treatment group to the outcomes of infants in a control group that may have resided within a smoke plume, but located *more* than one mile of a fire. Column (1) of Table (21) replicates our baseline smoke model. Column (2) tests the sensitivity of Column (1) to a full set of group by fire and group by trimester indicator variables for the set of infants located inside the smoke plume of a fire, but more than one mile away. Coefficient estimates in Column (2) of Table (21) are similar in magnitude to each corresponding estimate in Column (1). In addition, the estimate of $Polluted \times Tri$ 3 is only marginally insignificant with a p-value of .108.

One advantage of the wildfire smoke plumes that we construct in GIS is that they allow us to identify portions of the landscape that were polluted in a relatively precise way with a high degree of confidence. One limitation of these data is that the path of smoke that we identify in GIS may represent the location of polluted areas only on a given day and time for which a fire burned. As a result, there may exist births in the control group partially exposed to wildfire smoke, even if they were located *outside* of a wildfire smoke plume. We attempt to mitigate this bias by re-constructing each plume using several images taken at different times of the day. We take an additional measure to control for this bias by incorporating information on prevailing wind into our smoke analysis. Specifically, Column (3) of Table (21) tests the sensitivity of the baseline model in Column (1) to a set of group by fire and group by trimester indicator variables for the set of infants located more than a mile away from a fire and that were located either inside the smoke plume of a fire or downwind of fire. As shown in Table (21), by including these variables, coefficient estimates for $Polluted \times Tri\ 3$ and $Polluted \times Tri\ 2$ change from -4.8% (p=.095) and -3.8% (p=.094) to -6.7% (p<.05) and -3.8% (p=.125), respectively.

3.4.2 Testing for Pre-Existing Trends

In order for our difference-in-differences estimates to represent the causal effects of wildfire on birthweight, we must assume that the average change in birthweight in each treatment group would have been proportional to the average change in birthweight in each control group, in the absence of a wildfire. We must also assume that wildfires do not coincide with any unobserved shocks differentially affecting each group. Our empirical design mitigates concerns regarding the second of these assumptions by considering the effects of multiple wildfires occurring at different points in time and that vary over a large geographic scale. We assess the validity of the first assumption by comparing the prior trends in the birthweight of infants in each treatment group leading up to the fire to the prior trends in each corresponding control.

For each sample of births in our wind and smoke analysis, we regress the natural log of birthweight on a set of year by quarter fixed effects, fire fixed effects, and demographic controls. In Figure (15), we fit treatment group-specific local polynomials on the residuals of these regressions. This approach allows us to illustrate the temporal variation in our data that is explained by the variables of interest to our analysis – the set of treatment group by trimester indicator variables – controlling for time fixed effects and differences in birthweight due to mothers' characteristics. The figure at the top of panel (a) plots the trend in the birthweight of infants located downwind and within one mile of a wildfire. This trend line shows the evolution of birthweight among infants included in the treatment group *Polluted* constructed on the basis of prevailing wind. The bottom figure of panel (a) plots the birthweight trend for the set of infants located more than a mile away of a fire. In a similar fashion, the figure at the top of panel (b) plots the birthweight of infants located inside a smoke plume and within one mile of a fire. This trend line shows the evolution of birthweight among infants included in the treatment group $Polluted$ constructed on the basis of wildfire smoke plumes. The figure at the bottom on panel (b) shows the pre-fire trend in the birthweight of infants located between one and five miles. These figures show that the birthweight of infants in the treated and control groups exhibit similar trends leading up to a fire.

Next, we turn our attention to proximate upwind / out-of-the-smoke regions of fires. At the top of panel (a) in Figure (**??**), we plot birthweight trends for infants located upwind and within one mile of a wildfire, together with a 90% confidence interval. Infants in this group are included in the treatment definition $Stressed$ constructed on the basis of prevailing wind. The trend in the birthweight of infants in the control group is illustrated in the bottom of panel (a). Likewise, the figure at the top of panel (b) shows the evolution of the birthweight of infants located outside a smoke plume, but within one mile of a wildfire; the trend in the birthweight of infants located in the control group is illustrated in the bottom of panel (b). These figures also show that the birthweight of infants in the treated and control groups exhibit similar trends leading up to a fire; however, in contrast to our previous findings, we find no systematic changes in birthweight in any treatment group following an event.

3.4.3 Model Sensitivity to Treatment Cutoff

Our baseline empirical models compare the outcomes of infants in proximate upwind / out-of-the-smoke regions of wildfires as well as proximate downwind / in-the-smoke regions of wildfires using a one mile treatment cutoff. We proceed by testing the robustness of these models to the treatment cutoff definition. To do this, we re-estimate our baseline models reported in Columns (2) and (4) of Table (19) increasing the size of the treatment cutoff in quarter mile increments. Figures (16) and (17) plot coefficient estimates for $Polluted \times Tri$ 3 and $Polluted \times Tri$ 2 obtained from each iteration, respectively[14]. Coefficient estimates

[14]Sensitivity analysis for estimates of $Polluted \times Tri$ 1, $Stressed \times Tri$ 3, $Stressed \times Tri$ 2, and $Stressed \times Tri$ 1 are shown in Figures (B1), (B2), (B3), and (B4) in Appendix, respectively.

together with their 90% confidence intervals are shown on the y-axis with the treatment cutoff (in miles) shown on the x-axis. In each figure, model estimates obtained using the wind sample are shown in panel (a) with model estimates obtained using the smoke sample in panel (b).

Referring to Figure (16), model estimates of $Polluted \times Tri\ 3$ are larger (in absolute value) when we consider infants located closer to fires. As we consider the set of infants in polluted, but less proximate regions, model estimates decay. Estimates shown in panel (a) decay towards zero after a distance of approximately 2 miles. Referring to panel (b), model estimates of $Polluted \times Tri\ 3$ decay at relatively slower rate when we identify polluted regions using wildfire smoke plumes; these coefficients converge to zero after a distance of approximately 3.5 miles. Figure (17) shows that estimates of $Polluted \times Tri\ 2$ decay more quickly than each corresponding estimate in Figure (16). Specifically, estimates based on both our wind sample (panel (a)) and our smoke sample (panel (b)) converge to zero after approximately 1.25 to 1.5 miles. We recall that estimates of $Polluted \times Tri\ 3$ constructed using wildfire smoke polygons were statistically significant (or were only marginally insignificant) up to a distance of 3 miles. This suggests that infants located between 1.5 and 3 miles of a fire, but in their second trimester of pregnancy at the time of a fire, were likely exposed to levels of fine particulate matter similar to infants in the same regions, but in their third trimester of pregnancy; the fact that we find no statistically significant reductions in birthweight among infants in the former category re-inforces the general finding in the extant literature that the health risks of ambient air pollution to infants (at least in terms of birthweight) are elevated as infants approach later stages of the pregnancy.

3.4.4 Model Sensitivity to Control Cutoff

We turn our attention to testing the sensitivity of each of our models to the control cutoff delineating treated and non-treated areas. To do this, we first replicate the baseline wind and smoke models shown in Columns (2) and (4) of Table (19). We then obtain sequential estimates of each coefficient in each model after reducing the control cutoff in quarter mile increments from an initial distance of five miles to a distance of two miles. Figure (18) plots coefficient estimates of $Polluted \times Tri\ 3$. Panel (a) reports estimates obtained under our wind specification. Panel (b) reports estimates obtained under our smoke specification. In each figure, coefficient estimates are plotted on the y-axis. The control cutoff (in miles) is shown on the x-axis. Figure (18) shows that model estimates of $Polluted \times Tri\ 3$ are robust and stable to control definitions between two and five miles. As shown in Figures (B5) to (B9) in Appendix, model estimates for $\{Polluted \times Tri\ k\}_{k=2}^{3}$ and $\{Stressed \times Tri\ k\}_{k=1}^{3}$ are also robust and stable to control cutoffs between two and five miles.

3.4.5 Testing for Changes in Demographic Composition

As we explain previously, in order for our empirical methodology to identify the causal effects of wildfire on birthweight, we must assume that the average change in birthweight among infants in each treatment group would have been proportional to the average change in outcomes in each control group, in the absence of a wildfire. Our pre-existing trend analysis in Section (3.4.2) provides evidence to suggest this requirement is met. However, while the data for infants in each treatment group exhibit trends similar to the data for infants in each corresponding control, our estimates may fail to identify the causal impacts of fire if the relative demographic composition of households in the treatment and control systematically changes following a fire on the basis of a household characteristic correlated with birthweight. In our empirical application, the timing of wildfire and the timing of particle emissions fueled by wildfire coincide. Additionally, the timing of wildfire ignitions as well as the spatial variation in wildfire smoke are plausibly random. While these properties help mitigate concerns of post-disaster demographic re-composition, we test for these potential changes explicitly by examining how demographic characteristics in each treatment and control group change following a fire.

To operationalize these tests, we re-estimate our baseline wind model and our baseline smoke model replacing the left-hand side variables with mothers' demographic characteristics. Estimates based on equation (3.3) and our wind sample are shown in Table (22). Estimates based on equation (23) and our smoke sample are shown in Table (23). Of the 60 coefficients we estimate, only five are statistically significant. Of these, only one coefficient is significant in both the wind and smoke specifications. These results provide a more direct form of evidence that the composition of infants does not systematically change after a fire.

3.4.6 Returns to Birthweight

We find that infants exposed to wildfire smoke incur a 4% to 6% reduction in birthweight; this effect translates into a 135-202 gram reduction in birthweight for the average infant in our sample. To understand the significance of our point estimates expressed in terms of longer-term health outcomes, we draw on work by Black et al. (2007) dedicated to estimating the economic returns to birthweight. Black et al. (2007) compile data on the birthweights of infants born in Norway between 1967 and 1997. The authors link each birth record to an administrative dataset covering the population of Norwegians between the ages of 16 and 74 as well as a dataset of Norwegian military records from 1984 to 2005. The authors' data include information on: educational attainment; labor market status; earnings; gender; height; weight; and IQ.

Black et al. (2007) use these data to associate differences in the birthweight of monozygotic twins to differences in their adult outcomes. On the basis of their point estimates[15], a 6 percent decrease in birthweight translates into: a .34 to .45 centimeter reduction in height at age 18; a .03 to .04 decrease in IQ (measured on a scale from one to nine); a .42 to .54 percentage point decrease in the probability of high school completion; a .54 to .72 percent decrease in full-time earnings; and a .03 to .07 decrease in BMI. Black et al. (2007) also use Center for Disease Control cutoffs for classifying an individual as overweight (BMI$\geq$25) or underweight (BMI$\leq$18.5). Using these classifications, the authors also find that decreased birthweight leads to a statistically significant increase in the probability of being underweight in adulthood.

3.4.7 Pregnancy Characteristics

The primary objective of this paper is to quantify the effects of wildfire on birthweight. However, as we allude to above, our study is limited in terms of the inferences we can draw

[15] We are referencing estimates reported by Black et al. (2007) in Columns (3) - (4) of Table III, page 422.

regarding the physiological pathways between wildfire smoke exposure and infant health. While these pathways are not well established, some researchers believe that wildfire smoke and fetal growth retardation are linked by the effects that the particles and toxicants in wildfire smoke have on fetal nutrition and oxygen flow (Jayachandran, 2009; Berkowitz et al., 2003; Dejmek et al., 1999; Wang et al., 1997). While we leave the task of determining the physiological pathways between wildfire smoke and birthweight the subject of future research, we proceed by studying the impacts of wildfire on the health characteristics of each pregnancy with the goal of shedding light on what these pathways might be. These characteristics include: The gestational length of pregnancies; the number of prenatal visits scheduled by the mother; whether the infant was in a breech position; whether the mother was diagnosed with gestational hypertension or pregnancy induced hypertension; whether the infant was presented with rupture of membranes prior to the onset of labor; whether the infant had a seizure; and whether the infant incurred an injury at birth.

We first test if wildfire has the propensity to change the gestational length of pregnancies. We do this by estimating equations (2) and (3)[16] using a single post-fire time period indicator variable ($Post$) and the log of the gestational length of pregnancies ($ln(gest)$) as the dependent variable applied to the wind and smoke samples expanded to include preterm (less than thirty-seven weeks) and early term (greater than thirty-seven weeks, less than thirty-nine weeks) pregnancies. The results of these regressions are shown in Table (24). Referring to coefficient estimates of ($Polluted$) $\times$ ($Post$) and ($Stressed$) $\times$ ($Post$), model results suggest a .38% to .74% reduction in the gestational length of pregnancies among mothers in proximate downwind / in-the-smoke regions of fires and a .27% to .52% reduction among mothers located in proximate upwind / out-of-the-smoke regions; however, in each case we fail to reject the null hypothesis that each coefficient equals zero.

We turn our attention to the other characteristics of pregnancies in Tables (25) and (26). In each table, we report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3). Of the 72 coefficients we estimate, only two are statistically significant at conventional levels. First, referring to the estimate for $Polluted \times Tri\ 3$ in

[16]Each regression includes the full-suite of demographic and geographic controls used in our analysis of birthweight. Please refer to note in Table (24) for a list of the control variables used in these regressions.

Column (1) of Table (26), we find a small and significant increase in the number of prenatal visits attended by mothers. In contrast, estimates for $Stressed \times Tri$ 2 show a small decrease in the number of prenatal visits.; however, neither of these coefficients are significant in our wind sample.

Turning attention to Column (6) of Table (25), estimates for $Polluted \times Tri$ 3 indicate a small increase in the probability infants incur injuries at birth; this effect is only marginally insignificant with a p-value of .10. The coefficient estimate for $Polluted \times Tri$ 3 is also positive and only marginally insignificant with a p-value of .119 when we apply this estimation routine to our smoke sample in Table (25). Abnormal birth injuries may include skeletal fractures, peripheral nerve injury, and/or soft tissue/solid organ hemorrhage that requires a medical intervention. Due to the fact that each estimate is only marginally insignificant, it is difficult to conclude whether or not wildfire has a causal impact on abnormal birth injuries or not. To improve our confidence in these estimates, Tables (A5) and (A6) in Appendix test the sensitivity of the estimates reported in Column (6) of Tables (25) and (26) to the set of robustness checks we presented in Section (3.4.1). While the point estimates in Columns (2) - (3) in Tables (A5) and (A6) are qualitatively similar to point estimates reported in Column (6) of Tables (25) and (26), each estimate remains statistically insignificant.

3.5 CONCLUSION

To estimate the impacts that wildfire has on infant birthweight, we construct a spatial dataset delineating wildfires in Colorado, the prevailing wind direction near each fire, and the path of wildfire smoke linked to a confidential database detailing the birthweights of infants in the study area. The extant literature identifies two plausible mechanisms through which fire may impact fetal health: Changes in ambient air pollution and stress. To gain insight into the role each mechanism plays in explaining changes in infant birthweight, we utilize a difference-in-differences estimation framework estimated across multiple, geo-spatial dimensions of treatment based on each infant's location with respect to wildfire smoke. Model estimates show that wildfire smoke leads to statistically significant, 4% to 6% reduction in birthweight; effects that are most pronounced among mothers exposed during their second and third trimesters of pregnancy and decay with respect to distance to fire. In contrast, we find no statistically significant effects of wildfire on the birthweights of infants located outside the path of wildfire smoke. These findings point to ambient air pollution as the principal mechanism by which fire impacts birthweight. While it is plausible that wildfires place undue stress on nearby residents, our models fail to validate in-utero stress (as captured by proximity to a fire) as a mechanism through which wildfire impacts infant health.

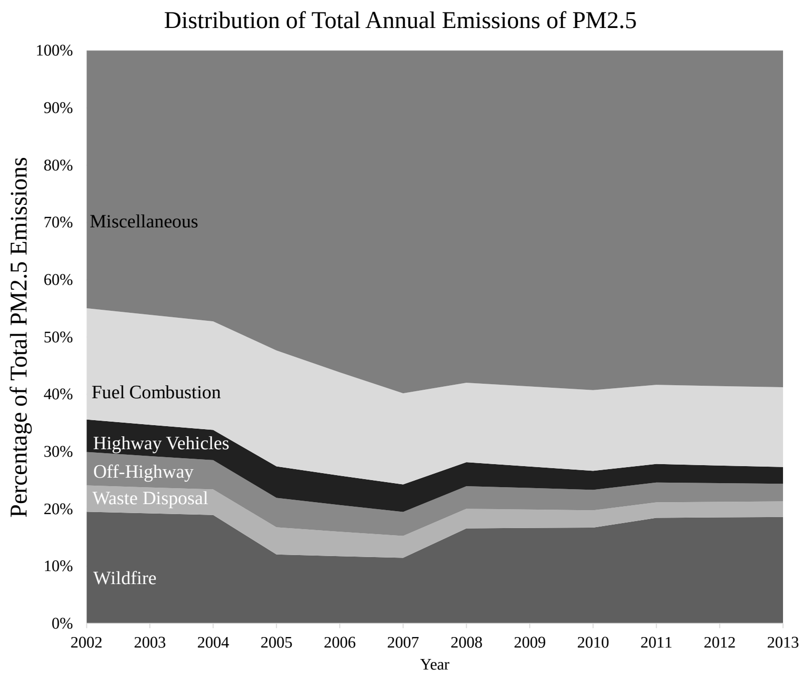
This paper provides new insight into the spatio-temporal dynamics between wildfire and infant health, but is limited in terms of identifying the physiological pathways between wildfire smoke and birthweight. We attempt to gain insight into what these pathways might be by exploring the effects of fire on pregnancy characteristics. The results of these tests further point to birthweight as the primary fetal health outcome impacted by severe fires. These findings appear to be consistent with the hypothesis advanced in previous works that wildfire smoke and fetal growth retardation are linked by the effects that the particles and toxicants in wildfire smoke have on fetal nutrition and oxygen flow; however, our study cannot validate this claim directly. We leave this determination to the subject of future research.

One advantage to studying birthweight is that many researchers agree it is a metric linked to short-term as well as longer-term health outcomes[17]. Another advantage is that infant birthweight databases are widely available and maintained at geographic scales fine enough to implement the empirical methodology we set forth in this paper. Still, efficient policy design ultimately requires an understanding of the impacts of fire across a broader spectrum of health outcomes. Avenues for fruitful work unexplored in this study are the links between wildfire, longer-term health outcomes, and cognitive impairments.

[17] See, for example, Almond and Currie (2011) and Black et al. (2007).

3.6 FIGURES AND TABLES

Figure 8: EPA Air Pollutant Emissions Trends Data: Average Annual $PM_{2.5}$ Emissions Trends (2002 - 2013)



Notes: This table is produced from the EPAs 1970-2014 Air Pollutant Emissions Trends Data. Link: http://www.epa.gov/air-emissions-inventories/air-pollutant-emissions-trends-data. Fuel Combustion category includes emissions from: electric utility, industrial, commercial, institutional sectors and residential use. Highway Vehicles category includes emissions from: light-duty gas vehicles, motor cycles, light-duty gas trucks, heavy gas vehicles and diesel vehicles. Off-Highway category includes emissions from: non-road gas and diesel use, aircraft, marine vessels, railroads, and others. Waste Disposal category includes emissions from: incineration, open burning, publicly owned treatment works, industrial waste water, treatment storage and disposal facility, landfills, and other. Finally, the Miscellaneous category includes emissions from chemical and allied product manufactures, metals processing, petroleum and related industries, other industrial processes, solvent utilization, storage and transport, natural sources, agriculture and forestry, catastrophic/accidental releases, repair shops, health services, cooling towers, and fugitive dust.

Figure 9: Study Area: Wildfires are depicted in red and black. Black is used to designate the set of wildfires with satellite images of wildfire smoke plumes.

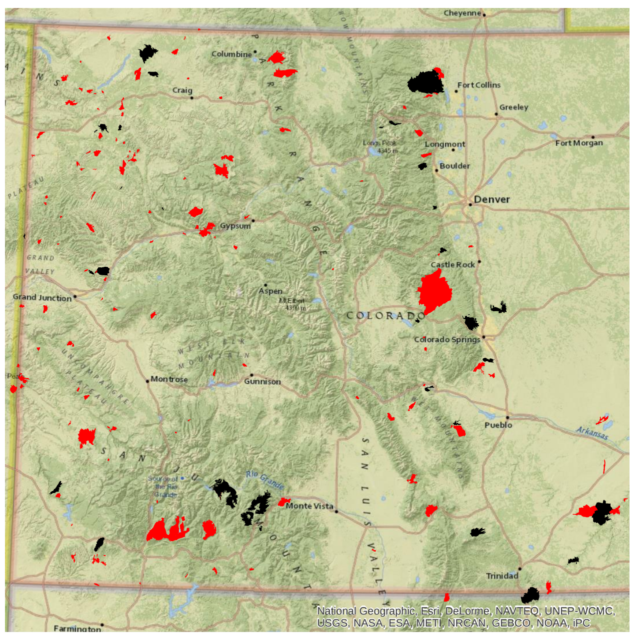


Figure 10: Study Area: Wildfires are depicted in red and black. Black is used to designate the set of wildfires with satellite images of wildfire smoke plumes.

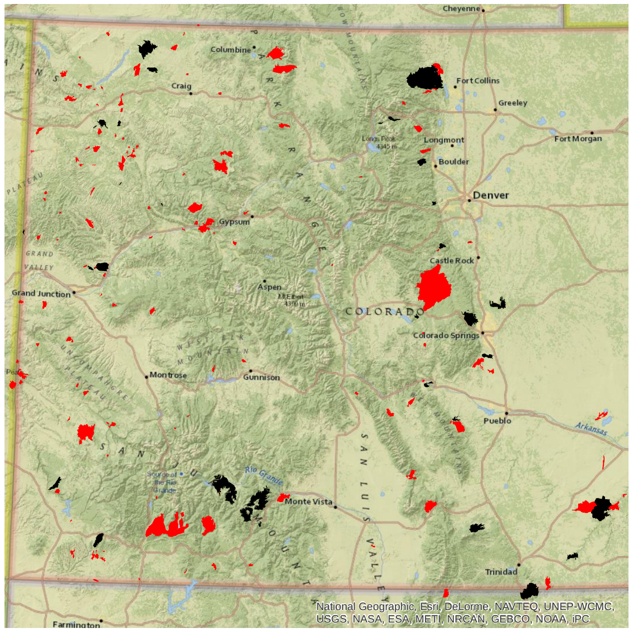


Figure 11: Sample Fire and Smoke Plume

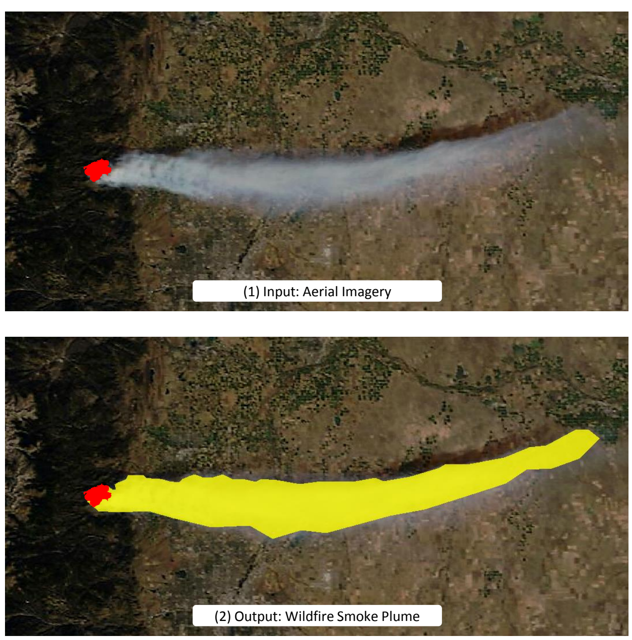


Figure 12: Wildfire Smoke Plumes: Wildfires are depicted in black. Smoke plumes are depicted in dark grey.

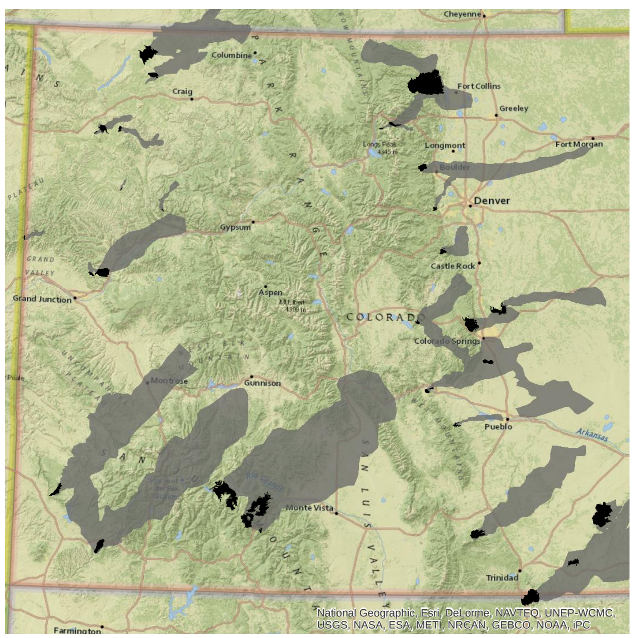


Figure 13: Descriptive Statistics.

	(1)	(2)
Variable	***Wind Sample***	***Smoke Sample***
Birth Weight (Grams)	3,381.92	3,382.61
	(413.44)	(413.86)
Gestational Age (Weeks)	39.65	39.60
	(0.76)	(0.71)
Mother's Age (Years)	29.36	29.13
	(5.77)	(5.84)
I(Married)	0.82	0.80
	(0.39)	(0.40)
I(White)	0.77	0.76
	(0.42)	(0.43)
I(Black)	0.02	0.03
	(0.13)	(0.16)
I(Hispanic)	0.15	0.14
	(0.35)	(0.35)
I(Race - Other)	0.06	0.07
	(0.24)	(0.26)
I(Doctorate or professional degree)	0.01	0.00
	(0.12)	(0.06)
I(Master's degree)	0.07	0.04
	(0.26)	(0.20)
I(Bachelor's degree)	0.20	0.14
	(0.40)	(0.34)
I(Some college, or associate degree)	0.30	0.36
	(0.46)	(0.48)
I(High school graduate or GED)	0.34	0.38
	(0.47)	(0.49)
Observations	7,398	4,736

Notes: Columns (1) and (2) report sample means and standard deviations (in parenthesis).

Table 19: Difference-in-Differences Estimates: Birthweight

	(1)	(2)	(3)	(4)
Fire Sample:	**Wind**	**Wind**	**Smoke**	**Smoke**
Dependent Variable:	*ln(bw)*	*ln(bw)*	*ln(bw)*	*ln(bw)*
(1 Mile) x (Tri 3)	-0.0254	-	-0.0389	-
	(0.170)	-	(0.163)	-
(1 Mile) x (Tri 2)	-0.0164	-	-0.0183	-
	(0.286)	-	(0.337)	-
(1 Mile) x (Tri 1)	-0.00178	-	-0.0248	-
	(0.909)	-	(0.262)	-
(Polluted) x (Tri 3)	-	-0.0569**	-	-0.0481*
	-	(0.0298)	-	(0.0955)
(Polluted) x (Tri 2)	-	-0.0450*	-	-0.0379*
	-	(0.0975)	-	(0.0940)
(Polluted) x (Tri 1)	-	-0.0281	-	-0.0129
	-	(0.488)	-	(0.643)
(Stressed) x (Tri 3)	-	-0.0115	-	-0.0281
	-	(0.615)	-	(0.604)
(Stressed) x (Tri 2)	-	-0.00206	-	0.0302
	-	(0.908)	-	(0.359)
(Stressed) x (Tri 1)	-	0.0119	-	-0.0482
	-	(0.486)	-	(0.194)
Observations	7,398	7,398	4,736	4,736

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. Columns (1) and (3) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (2) based on the data described in columns (1) and (2) of Table (1), respectively. Columns (2) and (4) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3) based on the data described in columns (1) and (2) of Table (1), respectively. Each model includes: indicators for mothers' marital status, race (white, black, hispanic, or other), and education level (doctorate or professional degree, master's degree, bachelor's degree, associate degree and some college (but no degree), high school graduate or GED, 9th to 12th grade (but no diploma)); each mother's age; the gestational age of each infant; a vector of fire-specific geographic controls including the elevation at each infant's home, the distance between each infant's home and wildfire, and the interaction between elevation and distance; year by quarter fixed effects; and treatment group by fire fixed effects.

Table 20: Robustness Checks: Wind Model

	(1)	(2)	(3)
Fire Sample:	**Wind**	**Wind**	**Wind**
Robustness Check:	Baseline Model	Contaminated Controls	Erratic Wind
Dependent Variable:	*ln(bw)*	*ln(bw)*	*ln(bw)*
(Polluted) x (Tri 3)	-0.0569**	-0.0622**	-0.0637**
	(0.0298)	(0.0198)	(0.0264)
(Polluted) x (Tri 2)	-0.0450*	-0.0428	-0.0375
	(0.0975)	(0.119)	(0.180)
(Polluted) x (Tri 1)	-0.0281	-0.0216	-0.0130
	(0.488)	(0.596)	(0.736)
(Stressed) x (Tri 3)	-0.0115	-0.0166	-0.0123
	(0.615)	(0.474)	(0.647)
(Stressed) x (Tri 2)	-0.00206	-0.000299	-0.0154
	(0.908)	(0.987)	(0.459)
(Stressed) x (Tri 1)	0.0119	0.0163	0.0174
	(0.486)	(0.349)	(0.348)
Observations	7,398	7,398	5,377

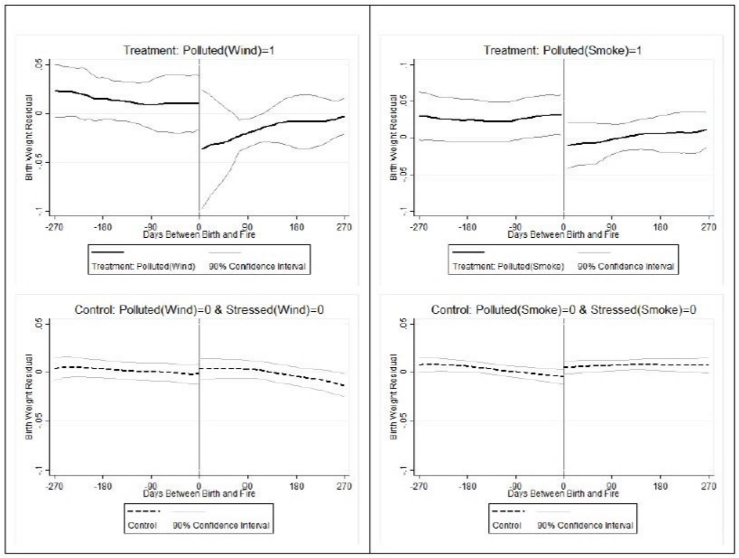
Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. The baseline model in Column (1) replicates Column (2) of Table (2). Column (2) tests the sensitivity of Column (1) to group by fire and group by trimester indicator variables for the set of infants located downwind of a fire, but further than one mile. Column (3) tests the sensitivity of Column (1) to excluding fires with an erratic wind pattern flag as described in Section 5.1.2. Please see Table (2) for a list of the control variables used in each specification.

Table 21: Robustness Checks: Smoke Model

	(1)	(2)	(3)
Fire Sample:	**Smoke**	**Smoke**	**Smoke**
Robustness Check:	Baseline Model	Contaminated Controls (Smoke)	Contaminated Controls (Smoke + Wind)
Dependent Variable:	*ln(bw)*	*ln(bw)*	*ln(bw)*
(Polluted) x (Tri 3)	-0.0481*	-0.0474	-0.0665**
	(0.0955)	(0.108)	(0.0288)
(Polluted) x (Tri 2)	-0.0379*	-0.0335	-0.0378
	(0.0940)	(0.149)	(0.125)
(Polluted) x (Tri 1)	-0.0129	-0.00812	-0.000180
	(0.643)	(0.775)	(0.995)
(Stressed) x (Tri 3)	-0.0281	-0.0265	-0.0367
	(0.604)	(0.629)	(0.502)
(Stressed) x (Tri 2)	0.0302	0.0327	0.0319
	(0.359)	(0.326)	(0.345)
(Stressed) x (Tri 1)	-0.0482	-0.0455	-0.0384
	(0.194)	(0.229)	(0.319)
Observations	4,736	4,736	4,736

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. The baseline model in Column (1) replicates Column (4) of Table (2). As described in Section 5.1.3, Column (2) tests the sensitivity of Column (1) to group by fire and group by trimester indicator variables for the set of infants within the smoke plume of a fire, but located further than one mile. Column (3) tests the sensitivity of Column (1) to group by fire and group by trimester indicator variables for the set of infants either within the smoke plume of a fire or downwind of a fire, but located further than one mile. Please see Table (2) for a list of the control variables used in each specification.

Figure 14: Trend Analysis: Stress - Proximate Upwind / Out of Smoke and Birthweight



(a) Wind Sample

(b) Smoke Sample

Figure 15: Trend Analysis: Air Pollution and Birthweight.

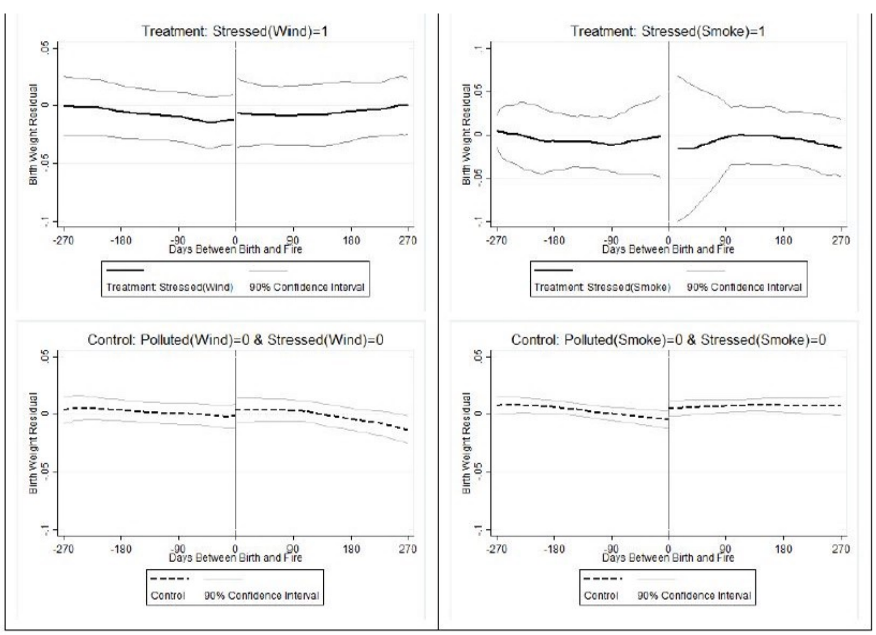
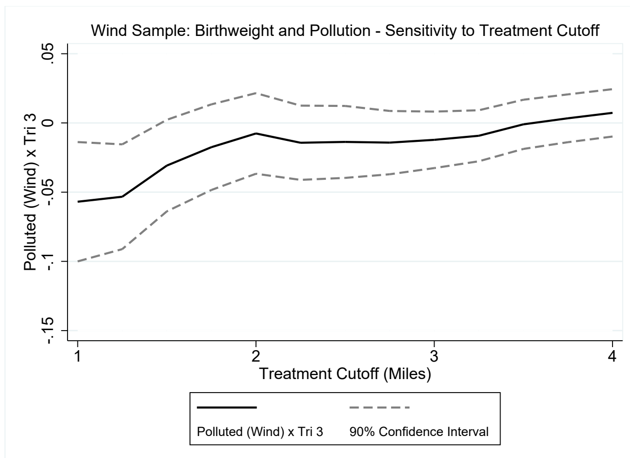
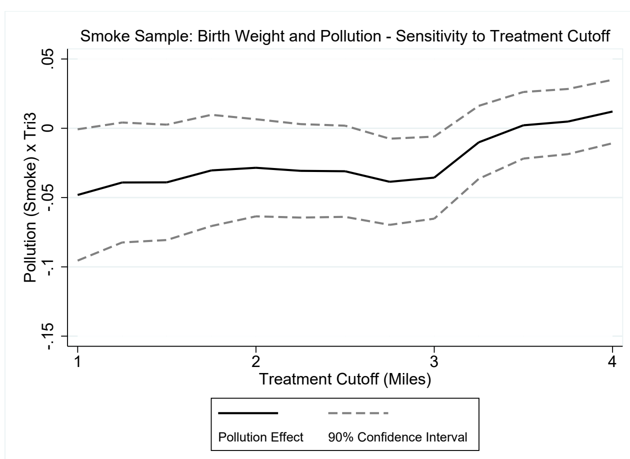


Figure 16: Sensitivity to Treatment Cutoff: Air Pollution & Birthweight (Trimester 3 Effects)

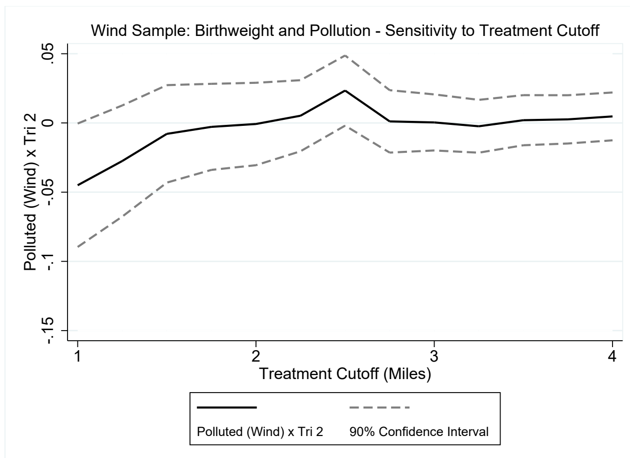


(a) Wind Model

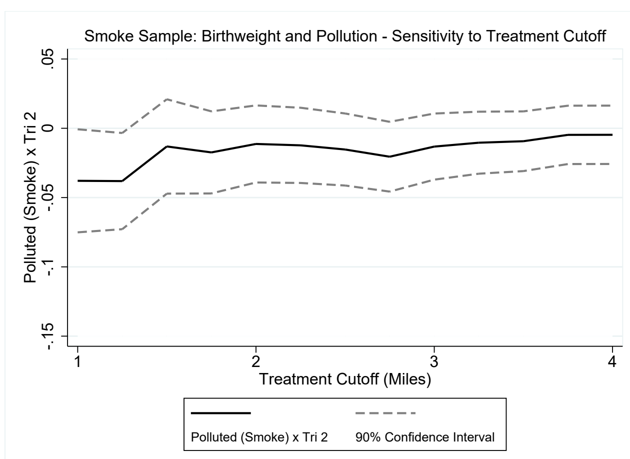


(b) Smoke Model

Figure 17: Sensitivity to Treatment Cutoff: Air Pollution & Birthweight (Trimester 2 Effects)

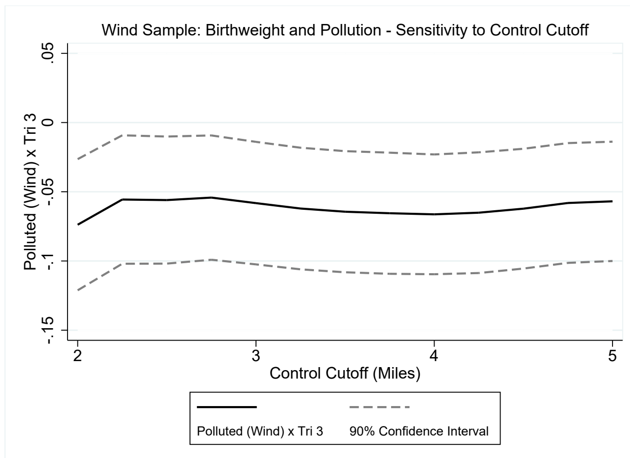


(a) Wind Model

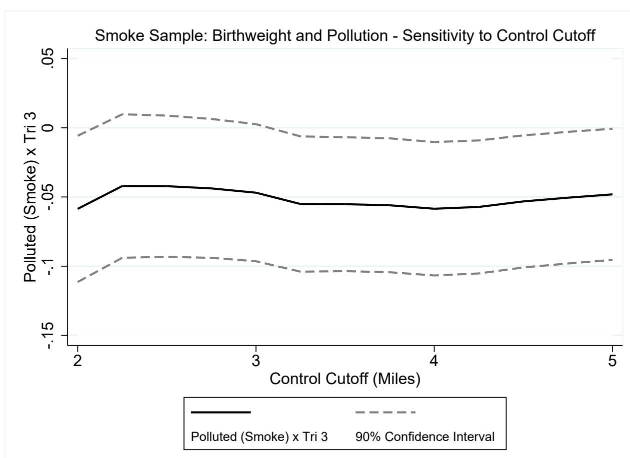


(b) Smoke Model

Figure 18: Sensitivity to Control Cutoff: Air Pollution & Birthweight (Trimester 3 Effects)



(a) Wind Model



(b) Smoke Model

Table 22: Testing for Changes in Demographic Composition: Wind Sample

	(1)	(2)	(3)	(4)	(5)
Fire Sample:	**Wind**	**Wind**	**Wind**	**Wind**	**Wind**
Dependent Variable:	*Married*	*White*	*Black*	*Hispanic*	*Age*
(Polluted) x (Tri 3)	0.117	-0.0630	-0.0273	0.0731	0.0842
	(0.148)	(0.576)	(0.118)	(0.387)	(0.945)
(Polluted) x (Tri 2)	0.0185	-0.109	-0.0185	0.0908	-0.401
	(0.810)	(0.326)	(0.458)	(0.305)	(0.771)
(Polluted) x (Tri 1)	0.00666	0.106	-0.0118	-0.0152	0.902
	(0.927)	(0.277)	(0.488)	(0.862)	(0.555)
(Stressed) x (Tri 3)	0.0379	-0.00411	-0.00347	0.0113	1.115
	(0.517)	(0.948)	(0.750)	(0.829)	(0.176)
(Stressed) x (Tri 2)	0.0434	0.0131	-0.00182	0.0173	1.169*
	(0.410)	(0.826)	(0.821)	(0.732)	(0.0970)
(Stressed) x (Tri 1)	0.0123	6.34e-05	0.00274	0.0108	-0.349
	(0.844)	(0.999)	(0.885)	(0.826)	(0.654)
Observations	7,398	7,398	7,398	7,398	7,398

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. Columns (1) - (5) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3) and are based on the data described in Column (1) of Table (1). Each model includes: Year-quarter fixed effects; treatment group by fire fixed effects; geographic controls; and the birthweight and gestational age of each mother's infant.

Table 23: Testing for Changes in Demographic Composition: Smoke Sample

	(1)	(2)	(3)	(4)	(5)
Fire Sample:	**Smoke**	**Smoke**	**Smoke**	**Smoke**	**Smoke**
Dependent Variable:	*Married*	*White*	*Black*	*Hispanic*	*Age*
(Polluted) x (Tri 3)	0.0953	0.0145	-0.000753	0.0338	0.418
	(0.277)	(0.871)	(0.973)	(0.581)	(0.688)
(Polluted) x (Tri 2)	0.00150	-0.0484	-0.00423	0.0227	0.0562
	(0.981)	(0.556)	(0.778)	(0.623)	(0.957)
(Polluted) x (Tri 1)	-0.00112	-0.0527	0.0342	0.0121	-0.874
	(0.989)	(0.587)	(0.437)	(0.843)	(0.430)
(Stressed) x (Tri 3)	-0.0245	0.0276	-0.0269	-0.0199	2.584
	(0.817)	(0.754)	(0.355)	(0.684)	(0.137)
(Stressed) x (Tri 2)	0.128**	-0.0432	-0.0208	0.105	2.156*
	(0.0305)	(0.656)	(0.441)	(0.223)	(0.0699)
(Stressed) x (Tri 1)	-0.0665	0.136**	-0.0464*	-0.0549	0.507
	(0.585)	(0.0177)	(0.0817)	(0.123)	(0.774)
Observations	4,736	4,736	4,736	4,736	4,736

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. Columns (1) - (5) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3) and are based on the data described in Column (2) of Table (1). Each model includes: Year-quarter fixed effects; treatment group by fire fixed effects; geographic controls; and the birthweight and gestational age of each mother's infant.

Table 24: Difference-in-Differences Estimates: Gestational Length of Pregnancies

	(1)	(2)	(3)	(4)
Fire Sample:	**Wind**	**Wind**	**Smoke**	**Smoke**
Dependent Variable:	*ln(gest)*	*ln(gest)*	*ln(gest)*	*ln(gest)*
(1 Mile) x (Post)	-0.00339	-	-0.00731	-
	(0.411)	-	(0.200)	-
(Polluted) x (Post)	-	-0.00383	-	-0.00735
	-	(0.574)	-	(0.291)
(Stressed) x (Post)	-	-0.00272	-	-0.00522
	-	(0.580)	-	(0.612)
Observations	11,836	11,836	7,240	7,240

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. Columns (1) and (3) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (2) with only one post-fire time period indicator variable using the data described in Columns (1) and (2) of Table (1), respectively. Likewise, Columns (2) and (4) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3) with only one post-fire time period indicator variable also using the data described in Columns (1) and (2) of Table (1). Please see Table (2) for a list of the control variables used in each specification.

Table 25: Difference-in-Differences Estimates: Pregnancy Outcomes (Smoke Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Fire Sample:	**Wind**	**Wind**	**Wind**	**Wind**	**Wind**	**Wind**
Dependent Variable:	*Number Prenatal Visits*	*Fetal Presentation Breech*	*Gestational Hypertension*	*Premature Rupture of Membranes*	*Seizure*	*Birth Injury*
(Polluted) x (Tri 3)	0.0554	-0.0189	0.0440	0.00384	-0.000205	0.00162
	(0.973)	(0.275)	(0.448)	(0.581)	(0.508)	(0.100)
(Polluted) x (Tri 2)	-0.508	-0.00612	-0.0281	0.00298	-0.00123	-0.000165
	(0.712)	(0.594)	(0.201)	(0.746)	(0.420)	(0.924)
(Polluted) x (Tri 1)	-1.659	0.0747	0.0573	-0.00290	-1.38e-07	-0.000904
	(0.480)	(0.200)	(0.422)	(0.726)	(1.000)	(0.483)
(Stressed) x (Tri 3)	0.612	0.0101	-0.0137	0.00222	0.000295	0.000707
	(0.367)	(0.269)	(0.299)	(0.929)	(0.525)	(0.350)
(Stressed) x (Tri 2)	3.490	0.0173	-0.000864	0.0181	-0.000706	-0.000206
	(0.131)	(0.346)	(0.963)	(0.505)	(0.572)	(0.878)
(Stressed) x (Tri 1)	0.400	0.0157	0.00379	-0.00463	0.000562	-0.000876
	(0.565)	(0.399)	(0.895)	(0.856)	(0.239)	(0.486)
Observations	7,336	7,398	7,398	7,398	7,398	7,398

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. Columns (1) - (6) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3) and are based on the data described in Column (1) of Table (1). Each model includes: Year-quarter fixed effects; treatment group by fire fixed effects; geographic controls; and the birthweight and gestational age of each mother's infant.

Table 26: Difference-in-Differences Estimates: Pregnancy Outcomes (Wind Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Fire Sample:	**Smoke**	**Smoke**	**Smoke**	**Smoke**	**Smoke**	**Smoke**
Dependent Variable:	*Number Prenatal Visits*	*Fetal Presentation Breech*	*Gestational Hypertension*	*Premature Rupture of Membranes*	*Seizure*	*Birth Injury*
(Polluted) x (Tri 3)	0.995*	-0.0206	0.0673	0.0199	0.000485	0.00274
	(0.0901)	(0.305)	(0.185)	(0.707)	(0.547)	(0.119)
(Polluted) x (Tri 2)	-0.148	-0.0203	0.0349	-0.0287	-0.00135	0.000435
	(0.818)	(0.275)	(0.238)	(0.185)	(0.601)	(0.878)
(Polluted) x (Tri 1)	0.0312	-0.0122	0.127*	-0.0282	0.000845	0.00130
	(0.967)	(0.478)	(0.0566)	(0.200)	(0.459)	(0.444)
(Stressed) x (Tri 3)	0.335	0.000353	-0.0301	-0.00301	-0.000200	-0.00124
	(0.693)	(0.979)	(0.398)	(0.787)	(0.794)	(0.570)
(Stressed) x (Tri 2)	-1.646*	0.0689	-0.0398	-0.00880	-0.00123	-0.00192
	(0.0725)	(0.348)	(0.229)	(0.349)	(0.335)	(0.316)
(Stressed) x (Tri 1)	-1.701	0.0587	-0.0588	2.88e-05	0.000808	-0.00146
	(0.119)	(0.378)	(0.113)	(0.998)	(0.270)	(0.448)
Observations	4,679	4,736	4,736	4,736	4,736	4,736

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. Columns (1) - (6) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3) and are based on the data described in Column (2) of Table (1). Each model includes: Year-quarter fixed effects; treatment group by fire fixed effects; geographic controls; and the birthweight and gestational age of each mother's infant.

4.0 A CITY UNDER WATER

(with Shawn McCoy)

Both the frequency and severity of natural disasters are increasing. This trend is evidenced, in part, by the fact that half of the ten most costly disasters in history occurred within just the last decade[1]. Wildfires, relative to the 1980s, are now four times more likely to occur and once they start, their burn scars are six times as large (Westerling et al., 2006). Between the 1960s and 1990s, the average number of floods rose seven-fold from 344 per year to 2,444 per year and cause roughly $6 billion in property damage annually (Brody et al., 2007; USGS, USGS). Many attribute the increasing trend in disaster frequency to changes in global climates and the rapid increase in their economic costs to changes in the number of households living in risk-prone areas. (Kunreuther and Michel-Kerjan, 2007). In the case of flood risk – which is the focus of this paper – it is generally considered that the most at risk properties are those located in special flood hazard areas (SFHAs) for which there exists a 1% chance of flooding in any given year and a 26% chance of flooding at least once over the course of 30-years. Approximately 6 million properties are located in SFHAs throughout the United States.(Burby, 2001).

Together, an increasingly larger population living in risk-prone regions and an increasingly higher rate of catastrophic events motivate us to ask, "To what degree do homeowners invest in buildings damaged by a disaster?" To answer this question, we investigate households' decisions to invest in residential homes damaged by Hurricane Sandy in affected areas of New York City. We utilize a micro-level data set which details both the location of each building in our study area as well as the timing of housing investment projects. We sub-

[1] Natural disasters: Counting the cost of calamities. *The Economist*, (2012). http://www.economist.com/node/21542755. Last accessed on September 28, 2016.

sequently link this data to information provided by FEMA which allows us to identify the locations of properties damaged by the storm.

Properties located in the SFHA differ from properties in non-SFHAs in terms of their latent flood-risk, but they also differ in terms of the policies and regulations they are subject to; most notably, regulations pertaining to flood insurance. Damage due to flooding is exempt from standard homeowner insurance policies. Instead, households must choose to purchase separate, flood insurance policies through the National Flood Insurance Program (NFIP). These policies are typically marketed to homeowners located in statutorily designated flood zones. Under the 1973 Flood Disaster Protection Act, homeowners located in SFHAs holding mortgages from federally insured or regulated lenders are *required* to purchase flood insurance. As a result, nationwide, roughly 51% of residents in SFHAs hold a flood insurance policy (Dixon et al., 2006). In contrast, less than 1% of households in non-SFHAs obtain flood insurance; a surprising statistic given that properties in non-SFHAs account for over *half* of all losses due to floods in the U.S. (Burby, 2001; Dixon et al., 2006). These observations motivate us to explore how investment decisions of owners of damaged homes located in the SFHA differ from the decisions made by owners of damaged homes outside of the SFHA.

To preview our empirical results, we estimate a statistically significant increase in the probability households living in the SFHA invest after experiencing damage to their property. In contrast, we find no change in the rate at which owners of damaged buildings outside of the SFHA invest. As we discuss in more depth below, the contrast between these empirical findings may be partially driven by differences in flood insurance take up rates between residents in and out of SFHAs.

In addition to considering determinants of post-disaster, remedial investment, we also consider factors influencing new investment in the face of risk. In particular, we ask, "Do natural disasters have the propensity to elevate households' perceptions of risk?" Homeowners' perceptions of risk are inextricably linked to their willingness to privately mitigate against risk. This may include, for instance, insuring against potential losses and deciding whether or not to develop housing in disaster-prone areas. For these reasons, *risk-salience* has been the focal point of many natural and man-made hazard disclosure policies including

California's 1998 Natural Hazards Disclosure Act[2] and the 1996 Lead Residential Lead-Based Paint Disclosure Program[3]. The ultimate goal of these types of regulations is to promote efficient behavioral outcomes by reducing the amount of asymmetry between perceived risks and underlying or latent risk levels. Henceforth, whether homeowners act on the information conveyed by a storm, or not, may speak to the potential for households to act on the information conveyed by information-based regulations.

Housing investment provides a unique context for discerning saliency dynamics since the decision to invest resembles a real-option (Downing and Wallace, 2000). Homeowners hold the option to delay an investment project into the future contingent on the arrival of new information, but once their decisions are made, they cannot easily be reversed. Thus, changes in market uncertainty will likely be reflected by changes in investment behaviors. We use Hurricane Sandy as an exogenous shock to agents' beliefs over the relative risk of living in a disaster prone area and formulate an empirical methodology that allows us to draw inferences regarding the mechanisms influencing households' evaluations of property-specific risks. More specifically, we investigate the drivers of risk-saliency by modeling relative investment decisions before and after Hurricane Sandy between properties in and out of SFHAs omitting any property that experienced physical damage from the storm from our analysis.

The empirical strategy we employ here is motivated by a growing body of work dedicated to estimating changes in risk-saliency due to natural disasters by analyzing housing price dynamics across designated disaster risk areas. The argument that housing price dynamics can reflect changes in perceived risks due to new sources of risk information attributable to a recent disaster is formalized by Hallstrom and Smith (2005) and Carbone, Hallstrom and Smith (2006). As argued by Hallstrom and Smith (2005), we posit that homeowners in designated high flood-risk zones, as well as proximate homeowners outside of these zones, are exposed to the information conveyed by a recent disaster; however, "households outside of

[2]Information regarding this policy is available at California's Department of Conversation webpage: http://www.conservation.ca.gov/cgs/shzp/Pages/shmprealdis.aspx. Last accessed on September 28, 2016. A summary of this policy is also provided by Troy and Romm (2004).

[3]Information regarding this policy is available at the US Environmental Protection Agency's webpage: http://www2.epa.gov/lead/lead-residential-lead-based-paint-disclosure-program-section-1018-title-x. Last accessed on September 28, 2016.

the high risk area are assumed to consider the information as relevant only to the designated flood zone". (542-543). Henceforth, if a natural disaster conveys new information to residents regarding the relative risk of living in a disaster-prone area, this information will ultimately be reflected by changes in the price discount homeowners place on high-risk properties.

With this conceptual framework in place, Hallstrom and Smith (2005) assess the extent to which one of the strongest hurricanes to hit the United States conveyed risk information to homeowners by estimating changes in property values in SFHAs (relative to changes in property values outside of SFHAs) using a difference-in-differences estimation strategy. Other notable works which have implemented a similar empirical strategy include: Atreya and Ferreira (2014), McCoy and Walsh (2014), Atreya et al. (2013), Bin and Landry (2013), Kousky (2010), Champ et al. (2009), Donovan et al. (2007), and Bin and Polasky (2004).

As indicated by Atreya and Ferreira (2014), there is a general consensus in the literature that recent floods increase perceived risks as evidenced by changes in the discount homeowners place on properties located in designated, flood-risk areas. From a policy perspective, it is equally important to consider whether or not saliency shocks due to disasters have the potential to promote behavioral responses across dimensions other than housing prices. The importance of this point is underscored by Gallagher (2014) who indicates that there exists a consensus in the literature that flood insurance take-up rates, for instance, appear low relative to socially optimal levels (Kunreuther et al., 2013; Kunreuther and Michel-Kerjan, 2007; Kunreuther, 1996).

By investigating the drivers of risk-saliency through the lens of a homeowner's decision to invest in a capital improvement in their home, this paper provides new insight into the propensity for information-based regulation to induce market responses across a broader set of domains that may have important implications in terms of improving the resilience of communities to natural hazards. However, one may argue that if the information conveyed through policy fails to mimic the information conveyed by a storm, a pure, information-based regulation may ultimately fall short of its intended goals. This argument motivates us to think more carefully about the channels through which homeowners may acquire new information regarding the risks they face following a catastrophic event.

Changes in local media coverage (which we might regard as a relatively *indirect* channel) may be one mechanism linking hurricanes to heightened risk-saliency. However, a homeowner's physical relationship to the spatial distribution of storm damage (which we might regard as a relatively *direct* channel) is another plausible mechanism. With the exception of Gallagher (2014), and largely due to data limitations, the distinction between saliency changes due to direct and indirect experience has received little attention in previous works. In this paper, we address the roles that direct and indirect experience play by estimating how investment decisions between SFHA and non-SFHA properties vary with respect to proximity to storm damage and by the severity of storm damage. To the best of our knowledge, our paper is the first to investigate how the spatial path of disaster damage may influence disaster risk-saliency.

To preview our empirical findings, when restricting attention to the effect of the storm on non-damaged buildings in the SFHA, we estimate a statistically significant decrease in the probability of housing investment relative to the set of non-damaged homes outside of the SFHA. Consistent with the findings in the extant literature, this result suggests that a recent disaster may heighten households' perceptions of disaster risk. Leveraging our data delineating the locations of buildings damaged by Hurricane Sandy, we also find that estimates of this effect attenuate with respect to *distance* to the spatial path of storm damage as well as the *severity* of storm damage. These results suggest that both the extent and the severity of storm damage are important mechanisms by which a recent disaster may drive changes in perceived risks.

To highlight the economic significance of this finding, we first acknowledge that, following a disaster, there exists changes in the information made available to households regarding the relative risk of living in a disaster-prone area that are arguably less correlated with homeowners' proximity to storm damage; most notably, changes in the information made available to residents through heightened media coverage. Not only do our model estimates decay with respect to homeowners' proximity to storm damage, but they become statistically insignificant as well. We find no statistical evidence that owners of properties in SFHAs located less proximately to storm damage reduce the rate at which they invest. This finding suggests that while there may exist changes in risk-saliency attributable to indirect experiences with the storm, it is unlikely that these saliency shocks are strong enough to be reflected in changes in market outcomes. In light of these findings, one could argue that information-based regulations which seek to align risk-perceptions with risk-actualities may ultimately be ineffective policy instruments in terms of their ability to induce socially-optimal, risk-mitigation behaviors if they fail to mimic the information conveyed by the direct impact of a disaster.

We begin by providing an overview of related work in Section (4.1). We characterize our study area and the details behind the construction of our data in Section (4.2). We present our empirical methodology in Section (4.3) and our findings in Section (4.4). We summarize and conclude in Section (4.5).

4.1 BACKGROUND

Residential housing investment in existing homes is often considered a relatively efficient means to improving housing standards and the primary alternative to increasing housing supply next to new construction (Plaut and Plaut, 2010; Boehm and Ihlanfeldt, 1986). The average value of home improvements expressed as a percentage of the value of new residential construction peaked in 1983 at 74% and is currently 45% (Boehm and Ihlanfeldt, 1986; Haughwout et al., 2013). Despite playing an important role in determining the size and quality of the housing stock, little research exists which considers the factors that influence housing investment decisions. Mainly due to a scarcity of data, the existing literature typically focuses on determinants of aggregate investment that vary over large, macroeconomic scales (Downing and Wallace, 2000; Poterba, 1984; Kearl, 1979).

Our paper is the first to analyze to the mechanisms linking natural disasters to changes in perceived risks through the lens of a homeowner's decision to invest in a capital improvement in their home. However, as we explain previously, there is a growing body of work that studies changes in risk-perceptions due to natural disasters using hedonic methods. For instance, Bin and Landry (2013) compare residential housing prices for properties located in SFHAs to properties located outside of these zones before and after two major hurricanes in Pitt County, North Carolina. The authors report a 5.7% to 8.8% hurricane-induced SFHA discount which lasts for 5 to 6 years. Atreya et al. (2013) perform a similar analysis after a major flood in Dougherty County, Georgia and report a post-hurricane SFHA discount of 32% which lasts for 7 to 9 years. Finally, Kousky (2010) examines pre and post-disaster housing values following the 1993 flood on the Missouri and Mississippi rivers, but fails to detect any relative price change between SFHA and non-SFHA structures.

These papers provide evidence that natural disasters may heighten households' perceptions of risk. However, without being able to distinguish between damaged and non-damaged homes, these papers may be limited in their ability to dis-entangle saliency dynamics from price effects caused by structural damage. McCoy and Walsh (2014) circumvent this difficulty using data delineating the extent of disaster damage. The authors also develop a model of preference-based sorting and underlying changes in location-specific risk perceptions which allows them to draw inferences on post-disaster saliency dynamics from changes in housing price and housing transaction rates.

Using wildfire as a natural experiment, McCoy and Walsh (2014) compare price and quantity dynamics between properties delineated by their underlying latent risk of fire, restricting their empirical analysis to properties located more than 5km from a fire and that did not have a wildfire in their viewshed. Their results suggest that natural disasters may heighten perceived risks, albeit, only temporarily. Hallstrom and Smith (2005) compare price differentials between homes in and out of the 100-year flood plain following Hurricane Andrew in 1992 using price data from Lee County, Florida which did not experience*any* damage from the storm. These authors find a 19% decline in housing prices in special flood hazard areas; a finding also suggesting that home buyers and sellers act on the information conveyed by a natural disaster.

Building off the work of Atreya et al. (2013) and Bin and Landry (2013),Atreya and Ferreira (2014) analyze home price dynamics across FEMA designated flood plains following the 1994 "flood of the century" caused by tropical storm Alberto in Albany Georgia using data delineating inundated portions of the landscape. This work is an advance over previous works in that the authors utilize flood inundation maps. These authors detect a significant, 48% housing price discount among inundated properties in SFHAs relative to non-inundated properties outside of the floodplain. However, these authors find no statistical change in the price of *non-inundated* homes in SFHAs. Other notable works which have considered the effects of natural and man-made hazards on perceived risks include Hansen et al. (2006), Gayer et al. (2002), McCluskey and Rausser (2001), Gayer et al. (2000), Tobin and Montz (1997), Bernknopf et al. (1990), Tobin and Montz (1988), and Brookshire et al. (1985).

With the exception of McCoy and Walsh (2014), the aforementioned studies focus exclusively on home price dynamics. Moving beyond the hedonic literature, Gallagher (2014) analyzes the learning process that agents use to update their beliefs over uncertain events by investigating flood insurance take-up following large regional floods. The author finds a significant spike in take-up in the year after a flood which is less pronounced, but still positive and significant, in non-flooded communities. In both flooded and non-flooded regions, take-up rates quickly decline to baseline levels after approximately one year; a finding consistent with the results presented by McCoy and Walsh (2014). Kelly et al. (2012) investigate changes in subjective hurricane risk perceptions following the provision of information from official and non-official hurricane track forecast information. A unique feature of this study is that the authors consider how agents react to new information regarding hurricane risk through an analysis of the Hurricane Futures Market prediction market. In a recent work, Anderson et al. (2014) investigate the extent to which heightened saliency due to recent disasters may influence political support for expenditures on public mitigation programs, with a particular emphasis on public spending on wildfire fuel treatment projects.

4.2 STUDY AREA AND DATA

Hurricane Sandy was the second largest Atlantic hurricane in US history. It was also the second most costliest resulting in roughly $50 billion in damage to coastal areas across the Eastern Seaboard (Blake et al., 2013). New York City – the area we study in this paper – experienced the highest storm surge with a 12.65 foot rise in water levels above normal tide levels and a 7.9 foot rise above ground level (Blake et al., 2013). About 16.6 percent of the city was under water resulting in a total loss of approximately $19 billion[4] (Furman Center for Real Estate and Urban Policy, 2013).

We identify the set of properties in NYC in and out of the SFHA by overlaying a map of the SFHA[5] with polygons delineating the lots[6] of residential structures. We utilize the most recent flood hazard maps that became effective September 5, 2007. We illustrate the study area and the extent of the SFHA in Figure (19).

Information detailing the structural characteristics of each home were acquired from NYCs Department of Finance Final Assessment Rolls[7]. These data include information on the year each structure was built, the dimensions of each property's lot, gross square footage, number of stories, and number of units. There were observations in our data with unreasonably low values with respect to year built likely reflecting transcription errors; we drop any observation lying below the first percentile with respect to year built. Finally, we limit attention to residential, 1-3 family residences[8]. Figure (20) provides an illustration of the buildings in our sample.

The extant literature has been constrained by a lack of spatial information on hurricane related damages which increased the difficulty of investigating market outcomes between damaged and non-damaged properties. In October of 2012, FEMA worked in conjunction with a team of modeling and risk analyst experts from the National Hurricane Center (NHC) and the U.S. Geological Survey to identify homes damaged by Hurricane Sandy. Referred to as FEMAs modeling task force (MOTF), these agencies utilized 157,000 images collected by the Civil Air Patrol and the National Oceanic and Atmospheric Administration in addition to 147,000 individual structural assessments to produce ground-truthed determinations of structures damaged by the Hurricane. This data, provided to us by FEMA, records the latitude and longitude coordinates of each structure damaged by Hurricane Sandy. As shown

[4] NYC Press Release PR-443-12. (November 26, 2012). Last accessed from http://www1.nyc.gov/office-of-the-mayor/news.page on September 28, 2016.

[5] Digital maps of the SFHA were obtained from the National Flood Hazard Layer: https://www.fema.gov/national-flood-hazard-layer-nfhl. Last accessed on September 28, 2016. Structures located in the 500-year floodplain were omitted.

[6] These data were obtained from NYC's Department of Planning: http://www1.nyc.gov/site/planning/index.page. Last accessed on September 28, 2016.

[7] NYC Department of Finance: http://www1.nyc.gov/site/finance/taxes/property-assessments.page#roll. Last accessed on September 28, 2016.

[8] Property Tax Class - 1.

in Figure (21), we adjoined FEMAs MOTF data to the footprints[9] of buildings in our sample in order to determine the set of properties that were and that were not damaged by the hurricane. The locations as well as the spatial density of damaged buildings are shown in panels (a) and (b) of Figure (22). These illustrations show that the locations of damaged structures tend to cluster near the SFHA. In fact, 81% of buildings in the SFHA incurred storm damage. In contrast, only 3.5% of buildings out of the SFHA incurred storm damage. While the proportion of damaged buildings out of the SFHA is small, the *scale* of this effect is not; of the 34,130 buildings in our sample that were damaged by Hurricane Sandy, 18,315 were not located in the SFHA.

In this paper, we are interested in homeowners decisions to invest in capital improvements in their homes. To this effect, we construct a micro-level dataset of housing investment projects from applications for permits to perform work on residential housing units. These data are sourced from monthly job reports provided to us by NYCs Department of Buildings[10] which contain logs of housing investment projects filed by property owners residing in NYC[11]. Among others, these projects include kitchen remodeling, removal, and or installation of non bearing partitions, installation of outdoor awnings or patios, and structural or mechanical repairs to the the interior or exterior of homes, and roof repairs and replacements[12]. Over our sampled time frame (two years before and after the Hurricane[13]), we record 17,572 housing investment projects. The locations and density of these investments are illustrated in panels (a) and (b) of Figure (23).

4.3 METHODS

Our basic empirical approach entails logistic regressions that compare housing investment outcomes before and after the hurricane estimated along various geo-spatial dimensions of

[9]These data were also obtained from NYC OpenData.

[10]NYC Department of Buildings: http://www.nyc.gov/html/dob/html/home/home.shtml.

[11]These are formally classified as Alteration Type-II investments.

[12]While each of these projects are classified as Alteration Type-II, our data does not provide a distinct indicator for each project type.

[13]Our dataset only includes residential property investments through the 2nd quarter of 2014. Therefore, while we have the complete history of investment projects for each of the 8 quarters preceding Hurricane Sandy, our data only includes investment projects for the seven quarters following the Hurricane.

treatment. Each treatment dimension is based on the extent of hurricane damage to each property and the location of each property relative to the SFHA. Contemporaneous shifts in local market conditions complicate the task of identifying the causal effect of the Hurricane on investment outcomes. As such, we compare outcomes before and after the hurricane for treated properties to the outcomes of properties in control groups that do not receive said treatment, but that are otherwise influenced by the same contemporaneous factors.

To implement our empirical models, we construct a balanced panel for each property in our assessment records using a year-quarter time increment. For each treatment definition, our logistic models take the form,

$$q_{it} = \Lambda\left(\alpha_1 \cdot Post_{it} + \alpha_2 \cdot Treat_i + \alpha_3 \cdot Treat_i \times Post_{it} + Z_{it}'\omega + \epsilon_{it}\right), \tag{4.1}$$

where $q_{it} = 1$ if household i invests in their property at time t and zero otherwise. Λ denotes the standard logistic cumulative probability distribution function. $Post_{it}$ is an indicator set equal to one for post-hurricane time periods and $Treat_i$ is an indicator set equal to one for properties belonging to the treatment group of interest. Z_{it} is a vector of structural controls including square footage and its square, age and its square, number of stories, neighborhood fixed effects, number of units, the dimensions of each parcels lot, a set of year-quarter fixed effects, and (in our more robust specifications) lagged dependent variables. Of interest to us are the marginal effects of the post-hurricane by treatment interaction terms ($Treat_i \times Post_{it}$). The functional form for these estimates is:

$$\begin{aligned} \tau\left(Treat \times Post\right) &= \Lambda\left(Post_{it} = 1,\ Treat_i = 1,\ Z_{it}'\omega\right) \\ &\quad -\Lambda\left(Post_{it} = 0,\ Treat_i = 1,\ Z_{it}'\omega\right) \\ &\quad -\Lambda\left(Post_{it} = 1,\ Treat_i = 0,\ Z_{it}'\omega\right) \\ &\quad +\Lambda\left(Post_{it} = 0,\ Treat_i = 0,\ Z_{it}'\omega\right), \end{aligned} \tag{4.2}$$

which is equivalent to,

$$\begin{aligned} \tau\left(Treat \times Post\right) &= \left[\Lambda\left(\alpha_1 + \alpha_2 + \alpha_3 + Z_{it}'\omega\right) - \Lambda\left(\alpha_2 + Z_{it}'\omega\right)\right] \\ &\quad - \left[\Lambda\left(\alpha_1 + Z_{it}'\omega\right) - \Lambda\left(Z_{it}'\omega\right)\right]. \end{aligned} \tag{4.3}$$

4.3.1 Treatment Definitions

4.3.1.1 Storm Damage We investigate the degree to which homeowners invest in damaged buildings by comparing investment outcomes between properties damaged by the hurricane to properties that were not. To understand how investment patterns differ between households in and out of SFHAs, we partition the set of damaged properties based on their location relative to the SFHA using the set of non-damaged properties outside of the SFHA as controls. Thus, for our analysis of storm damage, we estimate equation (4.1) with two treatment definitions, $Damaged_{NonSFHA,i}$ and $Damaged_{SFHA,i}$. $Damaged_{NonSFHA,i}$ is an indicator variable set equal to one for properties located outside of the SFHA that were damaged by the storm. Likewise, $Damaged_{SFHA,i}$ is an indicator set equal to one for damaged properties located inside of the SFHA. In each treatment definition, we use non-damaged, non-SFHA structures as the set of controls.

Our motivation for contrasting differences in the rates that homeowners in and out the SFHA invest stems from differences in flood insurance regulations that apply to homeowners in the floodplain. Most notably, homeowners located in the SFHA holding mortgages from federally regulated lenders are required to purchase insurance through the National Flood Insurance Program (NFIP). It has been estimated that approximately \$1.23 trillion of assets are covered by the NFIP (Michel-Kerjan et al., 2012). Nationally, it has been estimated that approximately half of all residents in SFHAs hold a flood insurance policy (Dixon et al., 2006). In contrast, less than one percent of households in non-SFHAS purchase flood insurance (Dixon et al., 2006). According to Kousky and Michel-Kerjan (2012), as of 2012, roughly 169,000 NFIP policies were in force in the state of New York. With respect to our study area, Dixon et al. (2013) estimate that 55% of the residences in New York City located in SFHAs had a flood insurance policy on the eve of Hurricane Sandy. Dixon et al. (2013) indicate that 75% of all homes in New York City located in the SFHA were subject to the mandatory insurance requirement; 66% of which held a flood insurance policy[14].

4.3.1.2 Risk Salience As we discuss previously, properties located within and proximate to SFHAs are both vulnerable to hurricane damage; albeit, conditional on a storm, property

[14]Overall, 55% of homeowners in the SFHA held flood insurance policies.Dixon et al. (2013)

owners in designated SFHAs are more likely to incur storm damage than households outside of these zones. State and Federal laws require sellers to disclose whether their property is located in an SFHA. For instance, location with respect to the floodplain is required to be disclosed to residents under New York's Property Condition Disclosure Act[15]. Community flood maps are also available online and required to be displayed publicly. Even in light of these information sources, there are reasons to suspect that homeowners do not fully acknowledge all of the risks associated with living in a floodplain. For example, using a survey of residents in Boulder County, Colorado, Chivers and Flores (2002) find evidence that homeowners living in SFHAs often fail to fully understand both the flood risks associated with their homes as well as the costs of insuring their homes when negotiating the purchase of their properties.

In the spirit of Hallstrom and Smith (2005), we use Hurricane Sandy as an exogenous shock to agents' beliefs over the likelihood of a disaster and draw inferences regarding which features of a storm influence disaster risk-saliency by comparing investment rates between properties in and out of SFHAs, before and after the storm. In order to isolate saliency effects net of the effects of storm damage, our analysis omits any property that experienced physical damage from the Hurricane. This approach helps to insure that our estimates will not reflect the direct impact of storm damage on property investment; however, as emphasized by McCoy and Walsh (2014), this specification does not necessarily rule out the possibility that homeowners may act on the dis-amenity effects of a disaster. McCoy and Walsh (2014) account for this bias by omitting properties less than 5km of a wildfire or that had a view of a wildfire burn scar. While appealing, this approach is infeasible in our application; the extent of storm damage from Hurricane Sandy was so severe, every property located in the SFHA in our sample is located within 5km of a damaged structure. This requires us to implement an alternative approach to control for dis-amenity confounds. Specifically, we restrict our salience models to the set of non-damaged properties that lie within 250 feet of a damaged structure. To the extent that properties in both the treatment and control group are similarly situated with respect to proximity to storm damage, our

[15]This information is required to be disclosed by sellers of residential real property to buyers through a property condition disclosure statement. Link to form: http://www.dos.ny.gov/forms/licensing/1614-a.pdf. Last accessed November 29, 2016.

difference-in-differences estimation strategy mitigates concerns of potential bias due to disamenity[16] effects.

To this effect, to model the salience effects of the hurricane, we first construct the treatment variable $SFHA_i$ which is an indicator variable set equal to one for non-damaged properties located in the SFHA and zero for non-damaged properties located outside of the SFHA and then estimate equation (4.1) restricting attention to properties located between 0ft - 250ft of a damaged structure.

4.4 RESULTS

4.4.1 Visual Evidence

In order for the difference-in-differences estimates obtained from equation (4.1) to represent the causal effects of the Hurricane, we must assume that the average rate of investment in each treatment group would have been proportional to the average rate in each corresponding control group, in the absence of the Hurricane. We assess the validity of this assumption by analyzing relative investment trends before the Hurricane for each treatment and control. To do this, we aggregate property investments to the treatment group by city block level using a quarterly time increment centered around the start date of Hurricane Sandy. For each treatment definition outlined in Section (4.3.1), Figures (24), (25), and (26) show group-specific, kernel-weighted local polynomial trends in housing investment before and after the Hurricane controlling for quarter and block fixed effects. Quarters elapsed since the Hurricane are shown on the x-axis. Trend lines for each treatment group are shown in dark black together with their 90% confidence intervals. Trend lines for each corresponding control are indicated by dashed lines.

Figure (24) shows that the trend in average property investment by households in the SFHA leading up to the hurricane is similar to the trend from households located outside of the SFHA. After the hurricane, we observe a systematic increase in property investment in

[16] In sections 5.3.2 and 5.3.3 we discuss in more detail instances where these assumptions may not hold and implement tests to asses their validity.

damaged, SFHA homes. We observe a small initial down-turn in investments in non-damaged homes outside of the SFHA which subsequently rebounds to its pre-hurricane level. Figure (25) provides graphical evidence that the trend in investment in damaged homes located outside of the SFHA is also similar to the trend in investment in similar homes that failed to experience any damage from the storm. However, we find no evidence of a relative increase in investment in these properties. Finally, turning our attention to Figure (26), before the hurricane, the trend in property investment in non-damaged homes in the SFHA is similar to the trend in investment in homes within the same vicinity of a damaged building, but located outside of the SFHA; this observation is reinforced by the fact that the trend line for treated observations leading up to Hurricane Sandy lies completely within the 90% confidence interval of the trend line for the control group. The difference in the size of the confidence intervals between the treatment and control reflects the difference in the number of non-damaged homes in the SFHA relative to the number of non-damaged homes out of the SFHA. After the storm, we observe an immediate and persistent decrease in investment in non-damaged, SFHA structures relative to non-damaged, non-SFHA structures; the fact that the upper confidence bound for the treated group lies strictly below the lower confidence bound for the control for all post-hurricane time periods underscores this finding.

We begin our formal analysis by estimating equation (4.1) for each treatment definition, $Damaged_{NonSFHA}$ and $Damaged_{SFHA}$. This allows us to quantify the effects of storm damage on housing investment in damaged properties both in and out of the SFHA. We then study the drivers of risk-saliency by estimating variants of equation (4.1) with the treatment definition $SFHA$.

4.4.2 Storm Damage

Table (27) presents estimates of the marginal effects of equation (4.1) comparing the outcomes of treated properties located in the SFHA that were damaged by the storm to control properties outside of the SFHA that did not experience any damage. To ensure that the control and treatment are as similar as possible, we limit the control group to properties located within 500ft of a damaged structure. Estimates of the the marginal effect corresponding to the interaction term in equation (4.1) are scaled by the baseline proportion of households that invest. To account for the possibility that previous period investment influences current period decisions, we include a lagged dependent variable in Columns (2) and (3). Finally, we report estimates obtained from a linear probability model in Column (3). Columns (1) and (2) indicate a 93% to 97% increase in the probability that a homeowner in the SFHA invests in their property after experiencing storm damage.

Turning our attention to investment in properties outside of the floodplain, we report estimates of the marginal effects of $Damaged_{NonSFHA} \times Post$ corresponding to equation (4.1) in Table (28). In contrast to our previous findings, we find no evidence of any change in the likelihood of property investment from homeowners outside of the SFHA. Specifically, referring to Columns (1) and (2), we detect a statistically insignificant -.2% to -.4% decrease in the the probability owners of damaged homes invests.

4.4.2.1 Differences in Building Characteristics & Deferred Investment The preceding analysis shows that after sustaining damages, property owners in the SFHA invest at a significantly higher rate than similarly situated households outside of the SFHA. Recalling that flood insurance take-up rates of residents inside the floodplain are higher than take-up rates of residents outside of the floodplain, our empirical results may reflect the role that insurance plays in facilitating post-disaster housing re-investment. However, the post-disaster property investment differential that we find between SFHA and non-SFHA properties may also be partially due to differences in the structural characteristics of damaged properties in and out the SFHA. We test for these differences in Table (29).

Column (1) of Table (29) indicates the structural characteristic of interest. Column (2) indicates the difference in the average level of each structural characteristic in Column (1) between damaged properties in the SFHA[17] and damaged properties out of the SFHA[18]. Column (3) shows the 95% confidence interval for each difference of means reported in Column (2).

Column (3) shows that there exists differences in the structural characteristics of each

[17]Those for which $Damaged_{SFHA} = 1$.

[18]Those for which $Damaged_{NonSFHA} = 1$.

property type in statistical terms, but Column (2) shows that these differences are small in terms of economic significance. Column (2) shows that damaged properties in the SFHA have, on average, .04 fewer stories than damaged properties out of the SFHA. They are also slightly smaller properties (by about 146 square feet), and slightly older (by approximately 1 year). The average width of their lots is .4 feet smaller than undamaged properties out of the floodplain and the average depth of their lots is 3.71 feet smaller.

Differences in post-disaster property investment between SFHA and non-SFHA properties may also be an artifact of differences in the timing, as opposed to the level, of remedial investment across these zones. In effect, homeowners outside of the SFHA may find it optimal to defer their investment decisions into the future. However, our trend analysis in Figure (25) shows that property investment fails to increase in any time period following the hurricane which seems to rule out the possibility that agents outside of the floodplain defer remedial investments, at least over the course of the seven quarters following Hurricane Sandy.

4.4.2.2 Severity of Hurricane Damage Our empirical results show that owners of damaged homes in the SFHA invest at a higher rate than owners of damaged homes out of the SFHA. In terms of understanding the policy implications of this result, it is important to determine whether these differences are due to the SFHA designation itself (and the bundle of policies and regulations that are implied by this designation), or to other, unobserved differences in the way that Hurricanes differentially impact residents in and out of flood-risk areas. One notable concern stems from the fact that the SFHA is a predictor of storm damage. As such, there is no reason, ex-ante, to assume that the severity of damage sustained by properties in the SFHA is similar to the severity of damage to properties out of the SFHA. With this said, the fact that owners of damaged homes in the SFHA invest while owners of damaged homes out of the SFHA do not might simply reflect differences in the degree to which homes in and out the SFHA sustained damage from the storm.

To investigate the role that the severity of storm damage plays, we partition our dataset of damaged structures into two sub-sets: (a) the sub-set of buildings that sustained *severe* damages, and (b) the sub-set of buildings that sustained *mild* damages. We partition properties into these sets using data provided by FEMA that was produced by New Light Technologies[19] (NLT) and ImageCat[20] from aerial imagery assessments of the landscape following Hurricane Sandy. Damaged homes that were classified as *mild* incurred superficial damage missing less than 20% of their roof coverings or had a flood depth less than five feet. Damaged homes that were classified as *severe* sustained major exterior damage missing more than 20% of their roof coverings, had collapsed exterior walls, or had a flood depth greater than five feet.

Using these classifications, we partition the treatment group $Damaged_{SFHA}$ into two sub-groups: $Damaged(severe)_{SFHA}$ and $Damaged(mild)_{SFHA}$. First, $Damaged(severe)_{SFHA}$ is an indicator set equal to one for any damaged structure in the SFHA that incurred *severe* damages. Second, $Damaged(mild)_{SFHA}$ is an indicator set equal to one for any damaged structure in the SFHA that sustained only *mild* damages. In a similar fashion, we also partition the treatment group $Damaged_{NonSFHA}$ into two sub-groups: $Damaged(severe)_{NonSFHA}$ and $Damaged(mild)_{NonSFHA}$[21].

We present the marginal effects for each treatment-group by post-hurricane interaction term in Tables (30) and (31). We first direct our attention to the effects of storm damage in the SFHA shown in Table (30). Column (1) of Table (30) replicates Column (2) of Table (27). In Columns (2) and (3), we de-compose the estimate shown in Column (1). Estimates of $\tau\,(Damaged(severe)_{SFHA} \times Post)$ indicate a statistically significant, 75% to 86% increase in the probability that households re-invest in homes that sustained severe damages. Likewise, estimates of $\tau\,(Damaged(mild)_{SFHA} \times Post)$ indicate a statistically significant, 82% to 97% increase in the likelihood that owners of homes that sustained mild damages invest. Finally, we turn our attention to investment out of the SFHA in Table (31). Column (1) of Table (31) replicates Column (2) of Table (28). Referring to Columns (2) and (3), we find no evidence of a statistically significant increase in the probability of investment in damaged homes out of the SFHA, irrespective of the severity of damage.

The results presented in Tables (30) and (31) fail to validate the hypothesis that owners

[19] NLT website: https://newlighttechnologies.com. Last accessed September 28, 2016.

[20] ImageCat website: http://www.imagecatinc.com/. Last accessed September 28, 2016.

[21] Specifically, $Damaged(severe)_{NonSFHA}$ is an indicator set equal to one for any damaged structure outside of the SFHA that incurred severe damages and $Damaged(mild)_{NonSFHA}$ is an indicator set equal to one for any damaged structure outside of the SFHA that incurred mild damages.

of damaged homes in the SFHA invest at a more significant rate than owners of damaged homes out of the SFHA because of differences in the severity in which their homes sustained damage. Put another way, if the differences in the results presented in Tables (27) and (28) were due exclusively to differences in the severity of damage sustained to the structures in each treatment group, we would expect these differences to attenuate when we compare the rates of investment between the set owners of damaged homes in and out of the SFHA, restricting attention to homes that sustained qualitatively similar levels of damage; the results presented in Tables (30) and (31) suggest this is not the case.

4.4.2.3 The Role of the Severity of Storm Damage: Looking within Flood-Risk Zones The preceding analysis shows that differences in the severity of damage to homes in and out of the SFHA do not explain differences in the rate at which owners of these homes invest. Our motivation for this analysis was to explore the extent to which our main empirical finding – again, which is that owners of damaged homes in the SFHA invest at a higher rate that owners of damaged homes out of the SFHA – might be explained by differences in the comparability of damaged structures in and out the SFHA. To further address this concern relating to comparability, we proceed by testing for differences in the rate of investment across damage types (severe vs. mild) within the SFHA and within the non-SFHA. Our motivation for this analysis is that we have a high degree of confidence that a "severely" damaged inside the SFHA sustained more damage than a "mildly" damaged home *also* in the SFHA. Put another way, a severely damaged home in the SFHA certainly is *not* comparable to a mildly damaged home also inside the SFHA. Likewise, a severely damaged home out of the SFHA certainly is not not comparable to a mildly damaged home also outside of the SFHA. Hence, if differences in the comparability of storm damage to homes in and out the SFHA can rationalize our main empirical findings, we ought to detect differences between the rate at which owners invest in severely and mildly damaged homes when we restrict attention to homes lying completely within and completely outside of the SFHA.

We operationalize these tests with a series of *one-tailed* tests for differences between estimates of $\tau(Damaged(severe)_{SFHA})$ and $\tau(Damaged(mild)_{SFHA})$ as well as tests for differences between estimates of $\tau(Damaged(severe)_{NonSFHA})$ and $\tau(Damaged(mild)_{NonSFHA})$. For each model, in Columns (2) and (3) of Tables (30) and (31), we report the p-values associated with these tests. Referring to Columns (2) and (3) of Table (31), the p-values associated with the one-tailed tests,

$$\tau(Damaged(severe)_{NonSFHA}) > \tau(Damaged(mild)_{NonSFHA}),$$

are .372 and .359, respectively which shows that owners of severely damaged homes out of the SFHA do not invest at a statistically higher rate than owners of mildly damaged homes out of the SFHA.

Turning attention to Columns (2) and (3) of Table (30), the p-values associated with the one-tailed tests, $\tau(Damaged(severe)_{SFHA}) > \tau(Damaged(mild)_{SFHA})$, are .385 and .42, respectively, which also shows that owners of severely damaged homes in the SFHA do not invest at statistically different rates than owners of mildly damaged homes. It is important to note that coefficient estimates in Columns (2) and (3) of Table (30) suggest, if anything, that *mild* damages induce a marginally *higher* rate of investment. However, coefficient estimates in Table (31) suggest the opposite. This inconsistency, coupled with the fact that we fail to detect any statistical differences in the rates of investment across damage types within the SFHA and within the non-SFHA is strong evidence that the discrepancy in the estimated rate of post-disaster investment between SFHA and non-SFHA households is not driven by differences in the comparability of these homes.

4.4.3 Risk Salience

Table (32) presents estimates of the marginal effects of equation (4.1) comparing the outcomes of treated properties located in the SFHA that were not damaged by the storm to control properties outside of the SFHA. Each model in panel (a) restricts attention to non-damaged buildings located between 0ft to 250ft of a damaged structure. Each model in panel (b) restricts attention to non-damaged buildings located between 250ft and 500ft of a damaged structure. To make the treatment and control groups more comparable, we restrict attention to properties that lie within a 1km buffer of the SFHA boundary.

Referring to panel (a), estimates of $\tau(SFHA \times Post)$ suggest a 40% to 41% *decrease* in the probability a homeowner in the SFHA invests in their property following the storm relative to households outside of the SFHA. In contrast, referring to panel (b), model estimates of $\tau(SFHA \times Post)$ become statistically insignificant when we restrict attention to non-damaged buildings located more than 250ft away of a damaged building. In Figure (27), we report sequential estimates of $\tau(SFHA \times Post)$, together with their 90% confidence intervals, obtained by increasing the lower and upper thresholds of our sampling window in 50ft. increments. The results presented in Figure (27) suggest that homeowners in the SFHA located between 0ft and 400ft of a damaged structure reduce the rate at which they invest on the order of roughly 35% to 50%; however, this effect is statistically significant only up to a distance of 300ft. We find no statistically significant changes in investment rates among properties between 300ft and 500ft.

These findings provide evidence that a natural disaster may work to heighten perceived risks. However, our finding that relative investment differentials decay with respect to distance to damaged buildings further shows that these changes in risk-saliency may be driven largely, and perhaps exclusively, by exposure to storm damage. Two factors may render this conclusion invalid. These include changes in investment induced by changes in flood-insurance premiums as well as localized dis-amenity effects. We address each of these in turn.

4.4.3.1 Flood-insurance Premiums One methodological concern that we don't explicitly account for are potential increases in flood insurance premiums after the Hurricane which may work to drive down a homeowner's willingness to invest. However, any changes in the cost of insurance would apply to *any* structure in the special flood hazard area. If these changes were strong enough to completely rationalize our empirical findings, we ought to detect falling investment in areas of the floodplain less proximate to storm damage, but we don't.

4.4.3.2 Dis-amenity Confounds Our empirical results indicate a decrease in property investment in the SFHA; however, only when we restrict attention to parcels in the immediate vicinity of a damaged structure. The presence of spatial decay may reflect localized spillover effects a la Campbell et al. (2011) due to the potential dis-amenities associated with proximity to storm damage.

The approach we utilize to mitigate this concern involves comparing outcomes in each treatment group to the outcomes of properties in each corresponding control group located within the same 250ft bandwidth of damaged structures. By estimating relative investment probabilities between each treatment and control, our estimation strategy mitigates bias due to the presence of dis-amenity effects.

To improve our confidence that our saliency estimates do not completely reflect localized spillover effects, we proceed by estimating the relationship between proximity to damaged buildings and investment probabilities separately for properties in and out of the SFHA. To do this, we construct two new treatment definitions, $1[x, x+250]_{NonSFHA}$ and $1[x, x+250]_{SFHA}$, and estimate the marginal effects for each variable interacted with a post-hurricane indicator. $1[x, x+250]_{NonSFHA}$ is an indicator variable set equal to one for any non-damaged, non-SFHA property located between $x$ft. and $x+250$ft. of a damaged building. We use the set of non-damaged, non-SFHA properties located between 500ft and 1000ft as the set of controls. As we explain in more depth below, we construct the set of control properties in this way so that we can estimate how spillover effects captured by coefficient estimates on $1[x, x+250]_{NonSFHA}$ vary with proximity to storm damage without changing the composition of the control group in each iteration.

Likewise, $1[x, x+250]_{SFHA}$ is an indicator variable set equal to one for any SFHA property located between $x$ft. and $x+250$ft. of a damaged building. We construct this variable using the same set of control properties used in the construction of $1[x, x+250]_{NonSFHA}$. Thus, marginal effects corresponding to the interaction terms, $1[x, x+250]_{SFHA} \times Post$, might be thought of as including a component due to changes in risk-saliency and a component due to spatial dis-amenities; in effect, capturing the cumulative effect of the Hurricane on non-damaged, SFHA structures.

We present the marginal effects for each treatment group by post-hurricane interaction term in Table (33). Table (33) shows estimates restricting attention to treated properties between 0ft. and 250ft. of a damaged building. Estimates obtained under the logit specification are shown in Columns (1) and (2). Estimates obtained from a linear probability model are shown in Column (3).

Referring to model estimates of $\tau(1[0ft., 250ft.]_{NonSFHA} \times Post)$, we find no evidence that proximity to a damaged building influences the probability of investment *outside* of the SFHA; neither in terms of statistical significance or magnitude. In contrast, estimates of $\tau(1[0ft.,\ 250ft.]_{SFHA} \times Post)$ show a statistically significant decrease in the probability of investment.

Next, we quantify how each estimate varies with proximity to damaged structures. Specifically, we iterate the results shown in Table (33) increasing x in 50ft. increments. We report the marginal effects of each estimate in Figures (28) and (29), respectively.

As shown in Figure (28) estimates of $\tau(1[x, x+250]_{SFHA} \times Post)$ decay at approximately the same rate as estimates of $\tau(SFHA \times Post)$. In addition, referring to Figure (29), we find no relationship between proximity to storm damage and changes in the investment decisions of homeowners outside of the SFHA. Together, these findings show that local dis-amenity shocks associated with damaged buildings are insufficient in and of themselves to fully explain our main findings regarding the saliency effects of Hurricane Sandy.

4.4.3.3 The Severity of Hurricane Damage as a Driver of Risk-Saliency The preceding analysis shows that estimates of $\tau(SFHA \times Post)$ attenuate with respect to proximity to damaged structures. This result points to storm damage as key mechanism through which hurricanes may induce changes in risk-saliency. To further highlight the significance of direct experience with a storm on disaster risk-saliency, we explore how the magnitude of the effects we estimate vary with respect to the severity of storm damage. To do this, we partition the treatment group $SFHA$ into two sub-groups: $SFHA(severe)$ and $SFHA(mild)$. $SFHA(severe)$ is an indicator set equal to one for any non-damaged structure in the SFHA near a damaged structure classified as *severe*. Likewise, $SFHA(mild)$ is an indicator set equal to one for any non-damaged structure in the SFHA near a damaged structure classified as *mild*.

We present model estimates of each treatment group by post-hurricane interaction term in Table (34). Columns (1) of panels (a) and (b) replicate Columns (2) of panels (a) and (b) of Table (32). We recall that the estimate of $\tau(SFHA \times Post)$ in Column (1) of panel (a) indicates a statistically significant decrease in the probability a homeowner in the SFHA invests in their property following the storm. However, model estimates of $\tau(SFHA(severe) \times Post)$ and $\tau(SFHA(mild) \times Post)$ in Columns (2) and (3) show that the overall effect captured by estimates of $\tau(SFHA \times Post)$ is driven primarily by severely damaged structures. Not only are estimates of $\tau(SFHA(severe) \times Post)$ and $\tau(SFHA(mild) \times Post)$ different in magnitude, but they are also statistically different as evidenced by the p-values associated with the tests: $\tau(SFHA(severe) \times Post) < \tau(SFHA(mild) \times Post)$. However, similar to our previous findings – and as shown in Columns (2) and (3) of panel (b) – model estimates of $\tau(SFHA(severe) \times Post)$ and $\tau(SFHA(mild) \times Post)$ attenuate towards zero and become statistically insignificant when we consider buildings less proximate to storm damage.

4.4.3.4 Bias Due to Differences in Storm Damage Density The results we present in the previous section further point to storm damage as an important mechanism through which hurricanes may induce changes in risk-saliency. However, as we explain previously, our empirical framework assumes that households in undamaged homes inside and outside of the SFHA are exposed to qualitatively similar storm damage dis-amenities. Our empirical methodology mitigates this bias by comparing outcomes in each treatment group to a set of control properties located within the same bandwidth of damaged structure. However, we might be concerned that undamaged homes in the SFHA near severely damaged structures (those for which $SFHA(severe) = 1$) are potentially exposed to a higher *density* of storm damage than properties included in the control, even if each property in the sample (treatment or control) is within the same [0ft., 250ft.] bandwidth of a damaged building. To asses the degree of this bias, we proceed by testing for differences in storm damage density between the treatment group and the control group.

We use two different approaches to quantify the spatial variation of storm damage density in our study area. In our first approach, we generate a *Kernel Density*[22] surface of damaged buildings using ArcGIS. We implement this methodology due its prevalence in related works, but there are a few important limitations to note. First, this methodology effectively divides

[22]Information for this tool is available here: http://help.arcgis.com/en/arcgisdesktop/10.0/help/index.html#/How_Kernel_Density_works/009z00000011000000/

our study area into a rectangular array of grid cells and then calculates the density of damaged buildings located within a *neighborhood* of each cell. In some ways this might be ideal since the resultant output values for each cell represent a measure of storm damage density based on the number of damaged buildings located completely within, but also within a small neighborhood, of each cell. However, for this very reason, this approach may lead us to *underestimate* the actual level of storm damage density for cells that have a high number of damaged buildings within them, but that are proximate to other cells with a relatively low number of damaged buildings. Second, we can control the output cell size of the Kernel Density surface, but we cannot control the orientation of the cells. This is problematic since this approach requires us to assign a density value from the Kernel Density surface to properties in our sample by determining which grid cell each property lies within. Henceforth, the density values we assign may be potentially misleading if the property of interest does not lie near the center of the cell it is located within.

We advance an alternative approach that circumvents each of the limitations of the Kernel Density analysis that we mention above. This latter approach involves computing the *point density* of damaged buildings located within a given radius of each *property* in our sample. As we explain in more depth below, this approach directly informs the validity of our empirical strategy – in terms of quantifying the degree to which estimates of $\tau(SFHA(severe) \times Post)$ are biased – since it allows us to compute the density of storm damage within 250ft. neighborhoods centered around properties in our treatment and in our control groups. We proceed by briefly discussing the details behind the computations of each spatial metric.

Kernel Density Analysis. We created a *Kernel Density* surface of damaged buildings using the *Kernel Density* tool in *ArcMap 10.4* choosing an output cell size of 250ft. and setting the search radius to the default setting.[23] The resultant map that we generated delineates storm damage density – expressed in terms of the number of damaged buildings per square mile – for a rectangular array of 250ft. by 250ft. grid cells completely spanning our study area. We subsequently computed the density of storm damage that each *property*, i, was exposed to by assigning a density value, KD_i, to each property based on the cell said property was located within.

[23] The default search radius is computed using a spatial variant of Silverman's Rule of Thumb.

Point Density Analysis. To alleviate the methodological concerns we highlight regarding the validity of the *Kernel Density* analysis, we compute a measure of the point density of damaged buildings in 250ft. neighborhoods centered around each property in our sample. We operationalized this in GIS by drawing 250ft. buffer zones around each property in our study area. We then overlayed each buffer zone with our map of damaged buildings to determine the number of damaged buildings located within 250ft. of each home. These counts effectively represent the magnitude of storm damage per the unit area of a circle with a radius of 250ft., which we denote by PD_i. In order to make comparisons between our point density metric and our kernel density metric, we normalized[24] the units of PD_i to represent the number of damaged buildings per *square mile.*

These data allow us to asses the extent to which differences in storm damage density bias estimates of $\tau(SFHA(severe) \times Post)$ by testing for mean differences in the levels of $ln(KD_i)$ and $ln(PD_i)$ between properties in the treatment group and in the control group as delineated by the treatment definition $SFHA(severe)$, $\mu[ln(KD_i|i \in Treatment]$ - $\mu[ln(KD_i|i \in Control)]$ and $\mu[ln(PD_i|i \in Treatment] - \mu[ln(PD_i|i \in Control)]$ restricting attention to properties within 250ft. of a severely damaged structure; the results of these tests are shown in equations (4) and (5).

$$\begin{aligned} &\mu[ln(KD_i|i \in Treatment)] - \mu[ln(KD_i|i \in Control)] = 1.795 \ (p < .01) \\ &\text{Treatment Definition: } SFHA(severe), \text{ Sample: [0ft, 250ft]} \end{aligned} \tag{4.4}$$

$$\begin{aligned} &\mu[ln(PD_i|i \in Treatment)] - \mu[ln(PD_i|i \in Control)] = 1.93 \ (p < .01) \\ &\text{Treatment Definition: } SFHA(severe), \text{ Sample: [0ft, 250ft]} \end{aligned} \tag{4.5}$$

Equation (4) indicates that the average level of storm damage density in the treatment group *exceeds* the average level of storm damage density in the control group by approximately 180%; this effect is statistically significant at the 1% level. Equation (5) suggests

[24] The scale factor we used was the ratio of the area of a square mile in square feet and the area of a circle with a radius of 250ft.

a larger difference of approximately 193%. These estimates suggest that our model estimates of $\tau(SFHA(severe) \times Post)$ are likely biased due to the differences in storm damage dis-amenities between the treatment and the control group.

One reason these findings are particularly problematic is that it becomes less clear what component of $\tau(SFHA(severe) \times Post)$ reflects heightened saliency. To identify the component of $\tau(SFHA(severe) \times Post)$ due to changes in risk-saliency net of relative changes in storm damage dis-amenities, we refine the control group to a set of properties with similar storm damage density levels. To do this, we first consider various threshold values for $ln(KD_i)$ and $ln(PD_i)$: γ_{KD} and γ_{PD}. For each value of γ_{KD} and γ_{PD}, we omit any property from the control group for which $ln(KD_i) < \gamma_{KD}$ or $ln(PD_i) < \gamma_{PD}$. As shown in equations (6) and (7), we then estimate differences in storm damage density between the treatment group and the resultant control group:

$$\begin{aligned}&\mu[ln(KD_i|i \in Treatment)] - \mu[ln(KD_i|i \in Control,\ ln(KD_i) \geq \gamma_{KD})]\\&\text{Treatment Definition: } SFHA(severe),\ \text{Sample: [0ft., 250ft.]}\end{aligned} \tag{4.6}$$

$$\begin{aligned}&\mu[ln(PD_i|i \in Treatment)] - \mu[ln(PD_i|i \in Control,\ ln(PD_i) \geq \gamma_{PD})]\\&\text{Treatment Definition: } SFHA(severe),\ \text{Sample: [0ft., 250ft.]}\end{aligned} \tag{4.7}$$

In Figures (30) and (31), we plot estimates of the differences of means specified in equations (6) and (7) together with a 90% confidence interval for each estimate. The x-axis in Figures (30) and (31) begin at .92 and 4.95, respectively; these points correspond to the minimum values of $ln(KD_i)$ and $ln(PD_i)$ in the data. We also note that values of $ln(KD_i)$ generated from the Kernel Density tool in ArcGIS are continuous. In contrast, values of $ln(PD_i)$ are discrete, since they are derived from the count of damaged buildings within 250ft. of each property in our sample.

As shown in Figure (30), as we increase the threshold value of γ_{KD}, the difference in storm damage density between the treatment group and the control group decreases. At a threshold value of $\gamma_{KD} = \gamma^1_{KD} = 6.78$, we can no longer detect a statistically significant difference in the level of storm damage density between the treatment group and the control group. However, while this difference is not *statistically* significant, the level of storm damage density in the treatment group exceeds the level of storm damage density in the control by 6.16%. In contrast, at a threshold value of $\gamma_{KD}^2 = 6.91$, the difference in storm damage density between the treatment and the control is approximately zero.[25] When, $\gamma_{KD}^3 = 7$, the sign of this difference flips; the level of storm damage density in the treatment group is 4.86% *lower*than the level of storm damage density in the control. We identify three similar values of γ_{PD} in Figure (31), $\gamma_{PD}^1 = 7.664$, $\gamma_{PD}^2 = 7.728$, and $\gamma_{PD}^3 = 7.789$. When $\gamma_{PD}^1 = 7.664$, we can no longer detect a statistically significant difference in storm damage density between the treatment and the control. When $\gamma_{PD}^2 = 7.728$, the difference is approximately zero.[26] Likewise,when $\gamma_{PD}^3 = 7.789$, the sign of the difference flips to -4.4%.

We proceed by testing the sensitivity of our coefficient estimates of $\tau(SFHA\ (severe) \times Post)$ shown in Table (34) to storm damage density threshold values of γ_{KD} and γ_{PD}. We report the results of these robustness checks in Table (35). Panel (a) of Table (35) tests the sensitivity of the coefficient estimate we report in Column (2) of panel (a) of Table (34). More specifically, Columns (1), (2), and (3) in sub-panel (a.1), test the robustness of our results to the Kernel Density threshold values that we identify in Figure (30) by dropping observations from the control group for which $\ln(KD_i) < \gamma_{KD}^1$, $\ln(KD_i) < \gamma_{KD}^2$, and $\ln(KD_i) < \gamma_{KD}^3$, respectively. The resultant number of observations each regression is based on is shown in brackets. In a similar fashion, Columns (1), (2), and (3) in sub-panel (a.2) test the robustness of our estimate of $\tau(SFHA(severe) \times Post)$ to the Point Density threshold values that we identify in Figure (31). Finally, Columns (1), (2), and (3) in sub-panel (a.3) test the robustness of our results to imposing both the Kernel Density and the Point Density threshold values specified above each coefficient estimate. Coefficient estimates obtained from a linear probability model are shown panel (b).[27]

Turning our attention to panel (a.1), we observe that for each value of γ_{KD}, coefficient estimates of $\tau(SFHA(severe) \times Post)$ are smaller in absolute value than the estimates we

[25] At a threshold value of $\gamma_{KD}^2 = 6.91$, the actual difference between the treatment and the control is -.0442%.

[26] The actual difference is -.242%

[27] Although coefficient estimates for $\tau(SFHA(mild) \times Post)$ were statistically insignificant in every specification in Table (34), we also tested the robustness of each estimate after refining the control group to make the treated and control observations similar in terms of their exposure to storm damage density, again, as captured by values of $ln(KD_i)$ and $ln(PD_i)$. In each case, estimates of $\tau(SFHA(mild) \times Post)$ remain statistically insignificant and attenuate towards zero.

report in Table (34). As shown in panel (a.2), the coefficient estimates that we obtain after imposing the point density thresholds are similar to the estimates that we obtain from imposing the kernel density thresholds. Moreover, consistent with the concerns we advance regarding the validity if the Kernel Density analysis, the reduction in the magnitude of our estimates is slightly larger using the point density threshold values. Finally, as shown in panel (a.3), we observe the largest reductions in the magnitude of our estimates after restricting the treatment group and the control group to be similar in terms of both storm damage density measures. While we observe reductions in the magnitude of our coefficient estimates of $\tau(SFHA(severe) \times Post)$, model estimates remain negative and statistically significant.

4.5 CONCLUSION

We estimate a statistically significant increase in the probability that homeowners in statutorily designated, special flood hazard areas invest in damaged structures. In contrast, we find no corresponding increase in the probability that homeowners invest in damaged properties located outside of the SFHA. Next to latent flood-risks, some of the most notable differences between properties in and out of SFHAs are the policies and regulations regarding flood insurance that apply exclusively to SFHA homeowners. Specifically, homeowners located in SFHAs holding mortgages from federally regulated lenders are required to purchase insurance. Ostensibly due to these requirements, flood insurance take-up rates in the SFHA significantly exceed take-up rates by residents outside of these zones.

With this said, our empirical findings may suggest that flood-insurance plays a significant role in promoting post-disaster investment. In many ways, this finding might be regarded as a positive result, but in other ways, this finding might be regarded as a negative result. We recall that many of the insurance policies in force in the SFHA are mandated under the 1973 Flood Disaster Protection Act and are typically provided at subsidized rates. This raises the question, "To what extent might these regulations lead to housing market distortions?" More to the point, "Do these regulations have the potential to promote investment projects in damaged homes in SFHAs that would have otherwise not have taken place in the absence of the regulatory framework currently in place?"

We cannot asses these questions directly, but our empirical results provide some evidence that warrant future investigation of these issues. Specifically, we recall that we do not find any change in the rate of investment in damaged homes *outside* of the SFHA nor do we find evidence that homeowners in these regions defer remedial investments into the future. One could advance the argument that if it were economically efficient to re-invest in damaged non-SFHA properties, we would anticipate some degree of post-disaster investment in them, but we do not. On this front, detailed parcel-level data on flood-insurance take-up could be one avenue for obtaining a more precise description of the roles flood insurance plays.

Building on the growing literature dedicated to understanding the links between natural disasters and disaster risk-saliency, we also use Hurricane Sandy as an exogenous shock to agents' beliefs regarding the relative risk of living in a disaster prone area. We infer changes in agents' beliefs by tracking investment decisions following the hurricane between non-damaged properties in SFHAs and non-damaged properties outside of these zones (but similarly situated in terms of their exposure to storm damage). Our model results indicate a statistically significant decrease in investment in homes in the SFHA. This finding, which is consistent with the findings in the extant literature, suggests that a recent disaster may heighten perceived risks. However, we show that this effect attenuates towards zero and becomes statistically insignificant with respect to *distance* to the spatial path of storm damage as well as the *severity* of storm damage.

Together, these results suggest that the spatial path of storm damage may be an important source of new information regarding the risks associated with living in a disaster-prone area. Here is why we think this finding is economically relevant. Following a catastrophic event, homeowners are bombarded with multiple sources of information which, on the one hand, might plausibly heighten perceived risks, but, on the other hand, are less likely to be correlated with proximity to storm damage. This includes, for instance, information garnered from increased coverage of natural disasters in the media. These sources of information may influence the degree to which households think about flood-risk, but our empirical results suggest the saliency effects due to these information sources, if present, are not strong enough to be reflected in market outcomes. An important implication of this result is that policies which seek to align households' subjective risk assessments with underlying or latent risk levels through information based regulation may induce changes in risk-saliency, but may ultimately be ineffective at promoting socially-optimal *behavioral* modifications if they fail to mimic the saliency effects attributable to the damage created by a storm.

Figure 19: Illustration of the study area and the SFHA (in green).

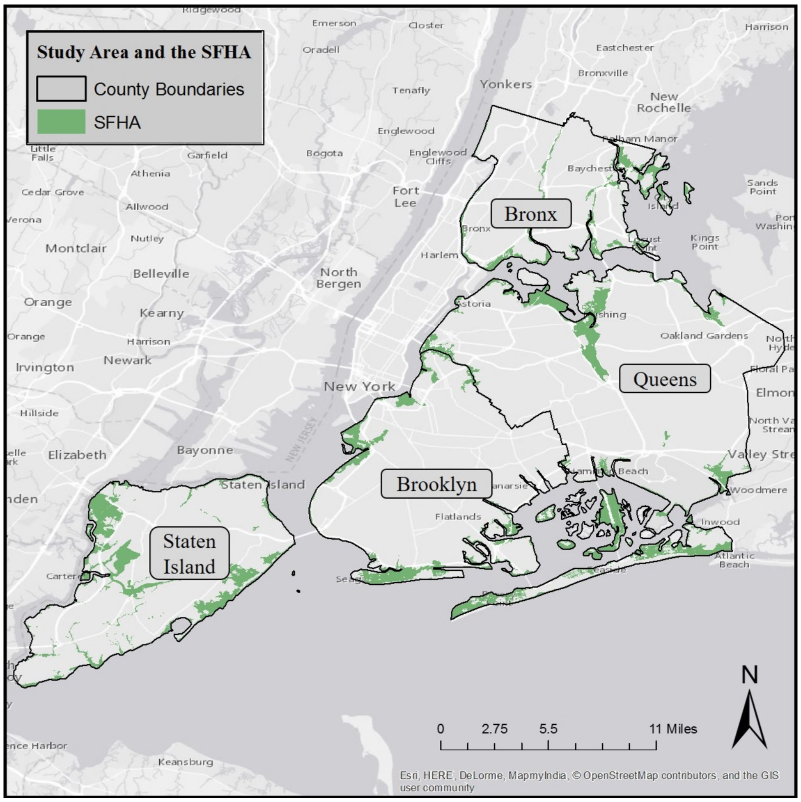


Figure 20: Illustration of the study area, building density, and the extent of the SFHA (in crosshatch).

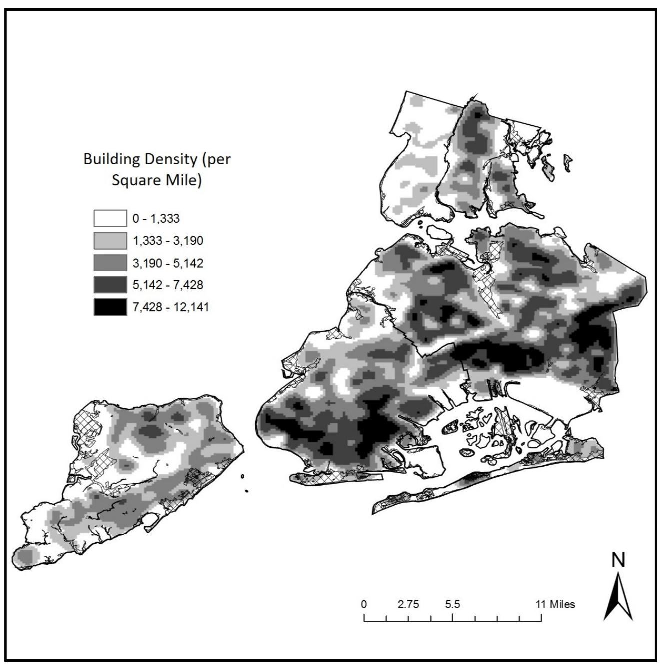


Figure 21: Illustration of residential structures, the floodplain, and flood damage. The footprints of damaged buildings are indicated by dark grey with black dots. Light grey indicates the footprints of non-damaged buildings. The extent of the SFHA is shown in crosshatch.

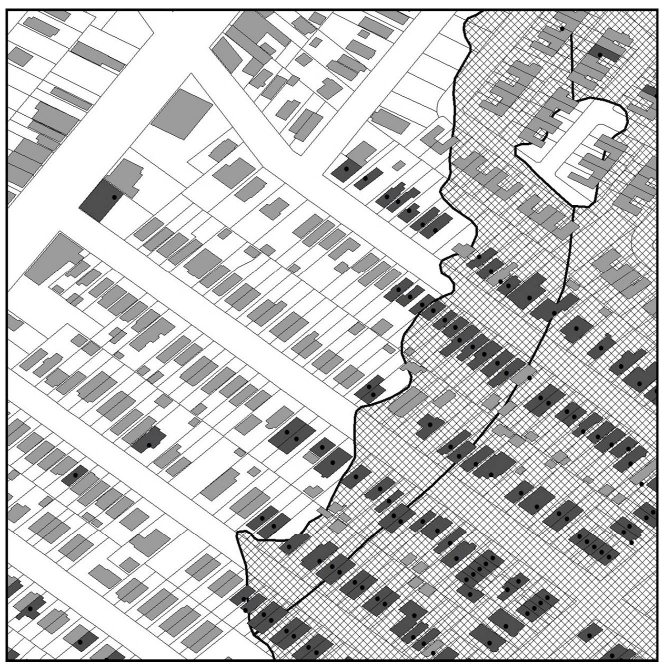
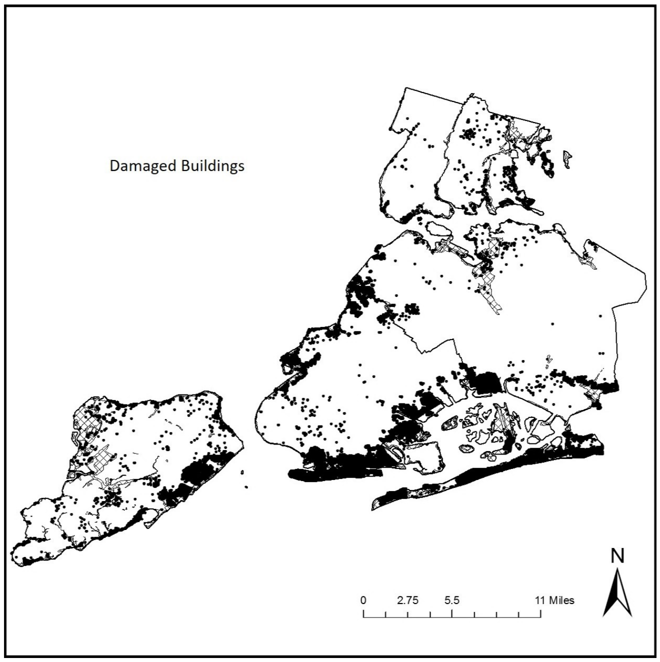
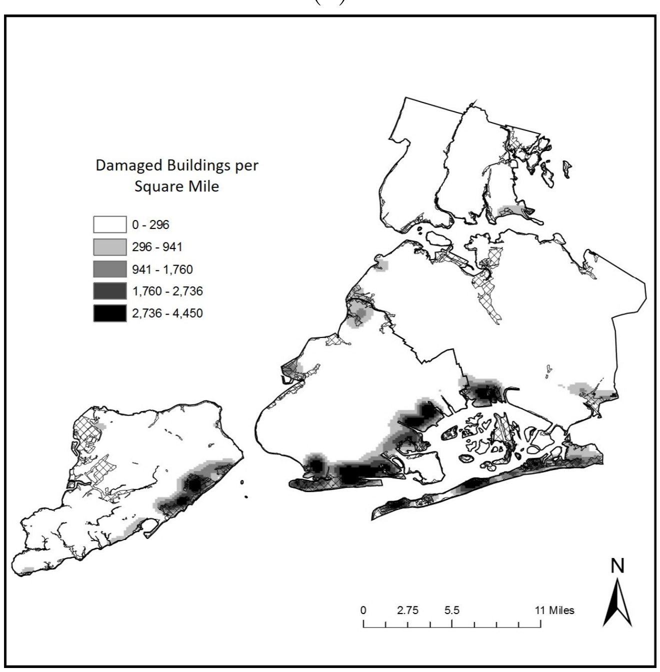


Figure 22: Panel (a) indicates the locations of damaged structures (in black dots) and the extent of the SFHA (in crosshatch). Panel (b) illustrates the density of damaged structures and the extent of the SFHA.

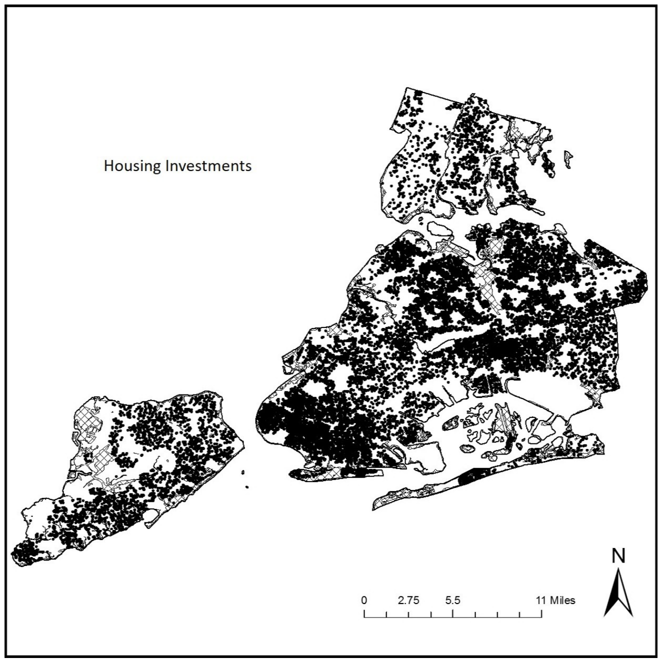


(a)

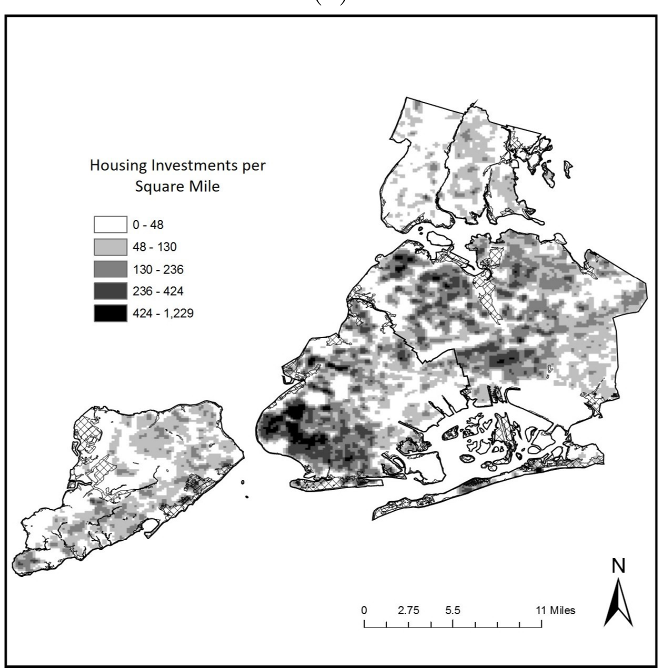


(b)

Figure 23: Panel (a) indicates the locations of property investments (in black dots) in our sample and the extent of the SFHA (in crosshatch). Panel (b) illustrates the density of property investments and the extent of the SFHA.



(a)



(b)

Figure 24: Trend Analysis: Treatment Definition - $Damaged_{SFHA}$.

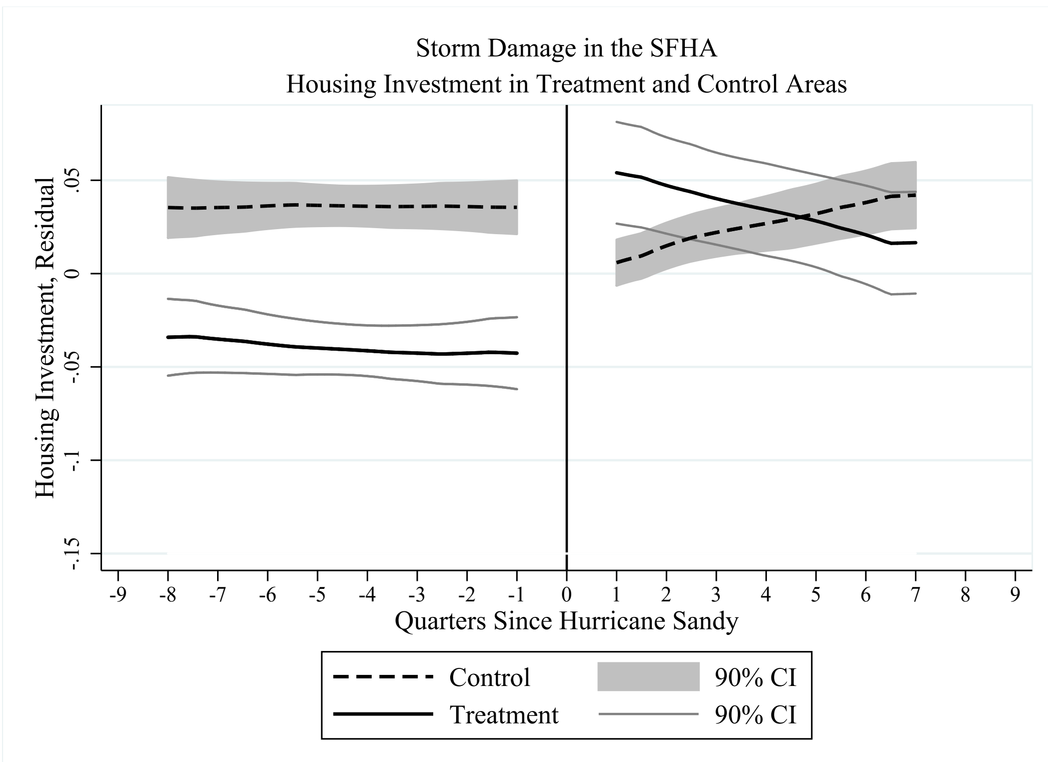


Figure 25: Trend Analysis: Treatment Definition - $Damaged_{SFHA}$.

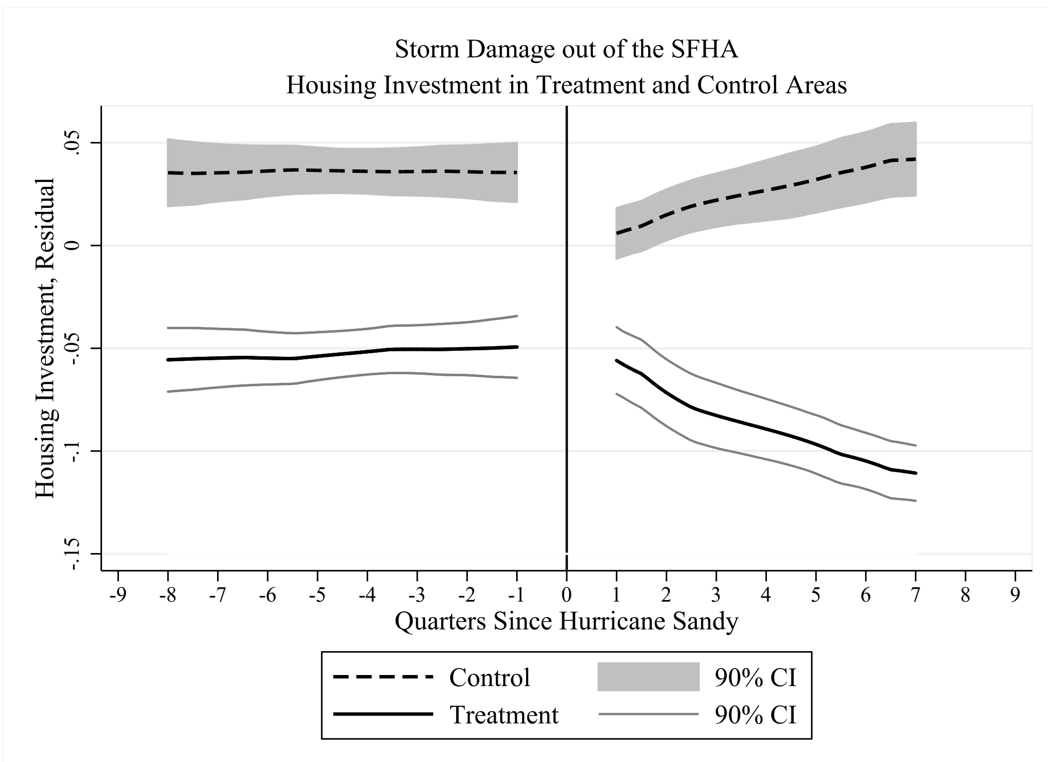


Figure 26: Trend Analysis: Treatment Definition - $Damaged_{SFHA}$.

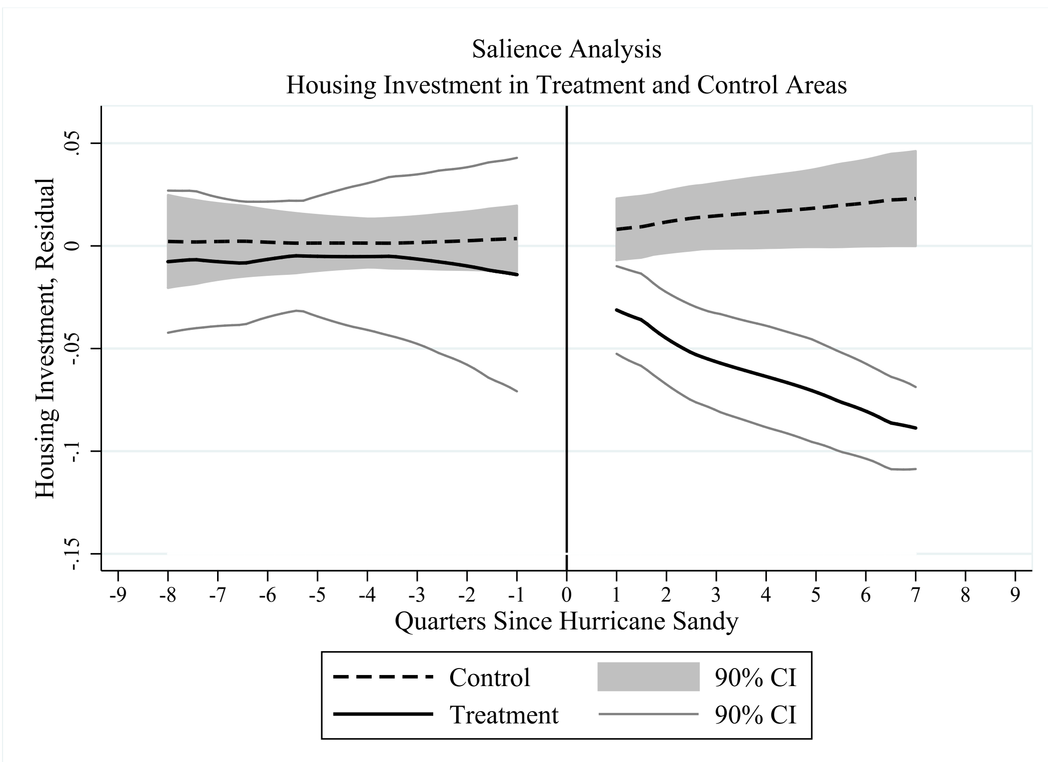


Table 27: Storm Damage and Investment: Damaged Properties in the SFHA

	(1) *Logit* *q*	(2) *Logit* *q*	(3) *LPM* *q*
$\tau(Damaged_{SFHA}\ x\ Post)$	0.974***	0.931***	0.803***
	(0.00)	(0.00)	(0.00)
Observations	1,242,025	1,242,025	1,242,025
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*n*	*y*	*y*

Notes: P-values in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in Columns (1), (2), and (3) include year by quarter fixed effects, indicator variables for number of units, neighborhood fixed effects, lot frontage, lot depth, and second order polynomials in square footage and age.

Table 28: Storm Damage and Investment: Damaged Properties out of the SFHA

	(1) *Logit* *q*	(2) *Logit* *q*	(3) *LPM* *q*
$\tau(Damaged_{NonSFHA}\ x\ Post)$	-0.002	-0.004	-0.052
	(0.99)	(0.97)	(0.61)
Observations	1,279,556	1,279,556	1,279,556
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*n*	*y*	*y*

Notes: P-values in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in columns (1), (2), and (3) include year by quarter fixed effects, indicator variables for number of units, neighborhood fixed effects, lot frontage, lot depth, and second order polynomials in square footage and age.

Table 29: Differences in the Structural Characteristics of Damaged Homes in the SFHA and Damaged Homes out of the SFHA

(1)	(2)	(3)
Structural Characteristic:	$\mu(Characteristic \mid Damaged_{SFHA} = 1) - \mu(Characteristic \mid Damaged_{NonSFHA} = 1)$	*95% Confidence Interval*
Number of Stories	-0.01	[-.02, .004]
Square Footage	-126.20	[-148.65, -103.75]
Lot Frontage (feet)	0.47	[-.05, .99]
Lot Depth (feet)	-3.31	[-3.92, -2.70]
Year Built	-3.32	[-3.93, -2.72]

Notes: Column (2) indicates the differences in the average levels of the structural characteristics listed in Column (1) between damaged properties in the SFHA and damaged properties out of the SFHA. Column (3) indicates the 95% confidence interval for each difference of means in Column (2).

Table 30: Storm Damage and Investment: Damaged Properties in the SFHA (Sensitivity to the Severity of Hurricane Damage)

	(1) *Logit* q	(2) *Logit* q	(3) *LPM* q
$\tau(Damaged_{SFHA}\ x\ Post)$	0.931***	-	-
	(0.00)	-	-
$\tau(Damaged(severe)_{SFHA}\ x\ Post)$	-	0.858***	0.754***
	-	(0.01)	(0.01)
$\tau(Damaged(mild)_{SFHA}\ x\ Post)$	-	0.972***	0.821***
	-	(0.00)	(0.00)
Observations	1,242,025	1,242,025	1,242,025
$P[\tau(Damaged(severe)_{SFHA}\ x\ Post) < \tau(Damaged(mild)_{SFHA}\ x\ Post)]$	-	0.385	0.420
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*y*	*y*	*y*

Notes: P-values in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in columns (1), (2), and (3) include year by quarter fixed effects, indicator variables for number of units, neighborhood fixed effects, lot frontage, lot depth, and second order polynomials in square footage and age.

Table 31: Storm Damage and Investment: Damaged Properties out of the SFHA (Sensitivity to the Severity of Hurricane Damage)

	(1) *Logit* q	(2) *Logit* q	(3) *LPM* q
$\tau(Damaged_{NonSFHA}\ x\ Post)$	-0.004	-	-
	(0.97)	-	-
$\tau(Damaged(severe)_{NonSFHA}\ x\ Post)$	-	0.245	0.203
	-	(0.75)	(0.78)
$\tau(Damaged(mild)_{NonSFHA}\ x\ Post)$	-	-0.011	-0.057
	-	(0.93)	(0.57)
Observations	1,279,556	1,279,556	1,279,556
$P[\tau(Damaged(severe)_{NonSFHA}\ x\ Post) > \tau(Damaged(mild)_{NonSFHA}\ x\ Post)]$	-	0.372	0.359
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*y*	*y*	*y*

Notes: P-values in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in columns (1), (2), and (3) include year by quarter fixed effects, indicator variables for number of units, neighborhood fixed effects, lot frontage, lot depth, and second order polynomials in square footage and age.

Table 32: Salience Analysis

(a) Sample Definition: [0ft, 250ft]

	(1)	(2)	(3)
	Logit	*Logit*	*LPM*
	q	*q*	*q*
$\tau(SFHA\ x\ Post)$	-0.408**	-0.397**	-0.404**
	(0.02)	(0.02)	(0.01)
Observations	347,839	347,839	347,839
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*n*	*y*	*y*

(b) Sample Definition: [250ft, 500ft]

	(1)	(2)	(3)
	Logit	*Logit*	*LPM*
	q	*q*	*q*
$\tau(SFHA\ x\ Post)$	0.183	0.165	0.164
	(0.72)	(0.74)	(0.74)
Observations	370,507	370,507	370,507
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*n*	*y*	*y*

Notes: P-values in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in columns (1), (2), and (3) include year by quarter fixed effects, indicator variables for number of units, neighborhood fixed effects, lot frontage, lot depth, and second order polynomials in square footage and age.

Figure 27: Salience Analysis: Sensitivity to Sample Definition

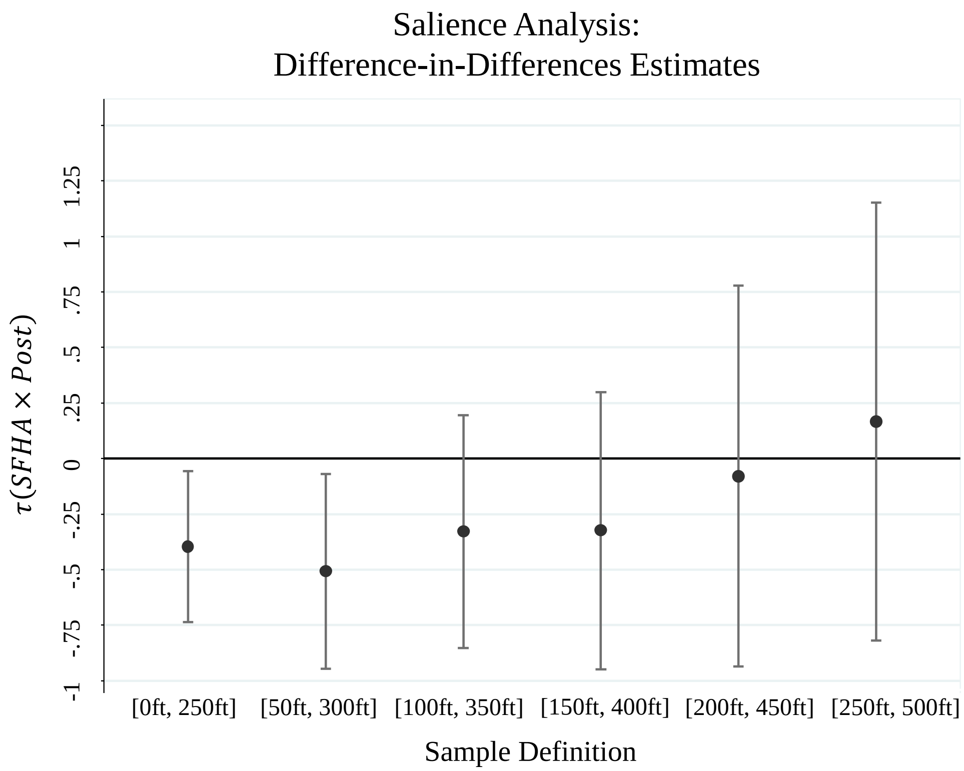


Table 33: Spillover Effects

	(1) *Logit* q	(2) *Logit* q	(3) *LPM* q
$\tau(1[0ft, 250ft]_{NonSFHA}\ x\ Post)$	0.04	0.039	-0.075
	(0.76)	(0.77)	(0.52)
$\tau(1[0ft, 250ft]_{SFHA}\ x\ Post)$	-0.395**	-0.387**	-0.404***
	(0.02)	(0.02)	(0.01)
Observations	940,195	940,195	940,195
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*n*	*y*	*y*

Notes: P-values in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in columns (1), (2), and (3) include year by quarter fixed effects, indicator variables for number of units, neighborhood fixed effects, lot frontage, lot depth, and second order polynomials in square footage and age.

Figure 28: Spillover Effects in the SFHA: Sensitivity to Sample Definition

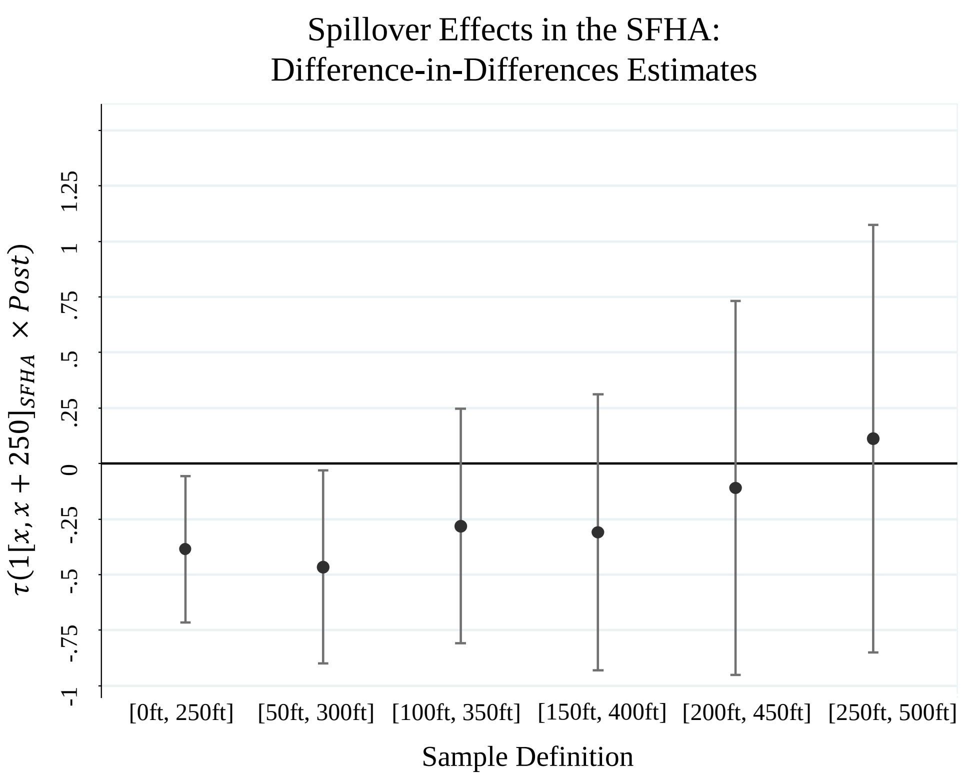


Figure 29: Spillover Effects out of the SFHA: Sensitivity to Sample Definition

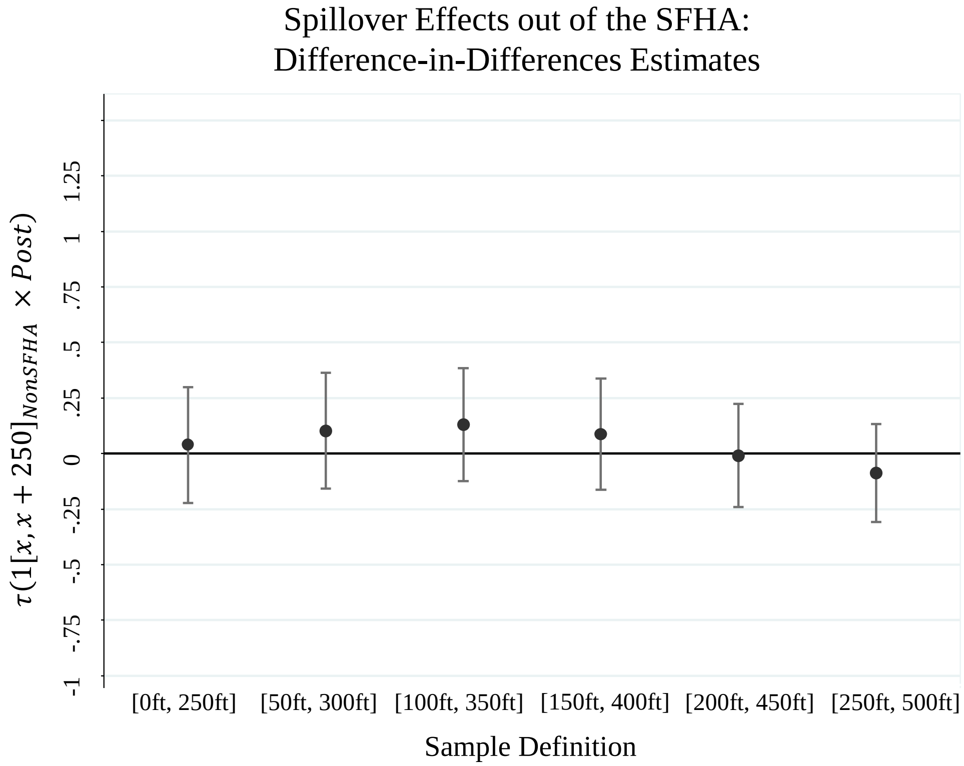


Table 34: Salience Analysis: Sensitivity to the Severity of Hurricane Damage

(a) Sample Definition: [0ft, 250ft]

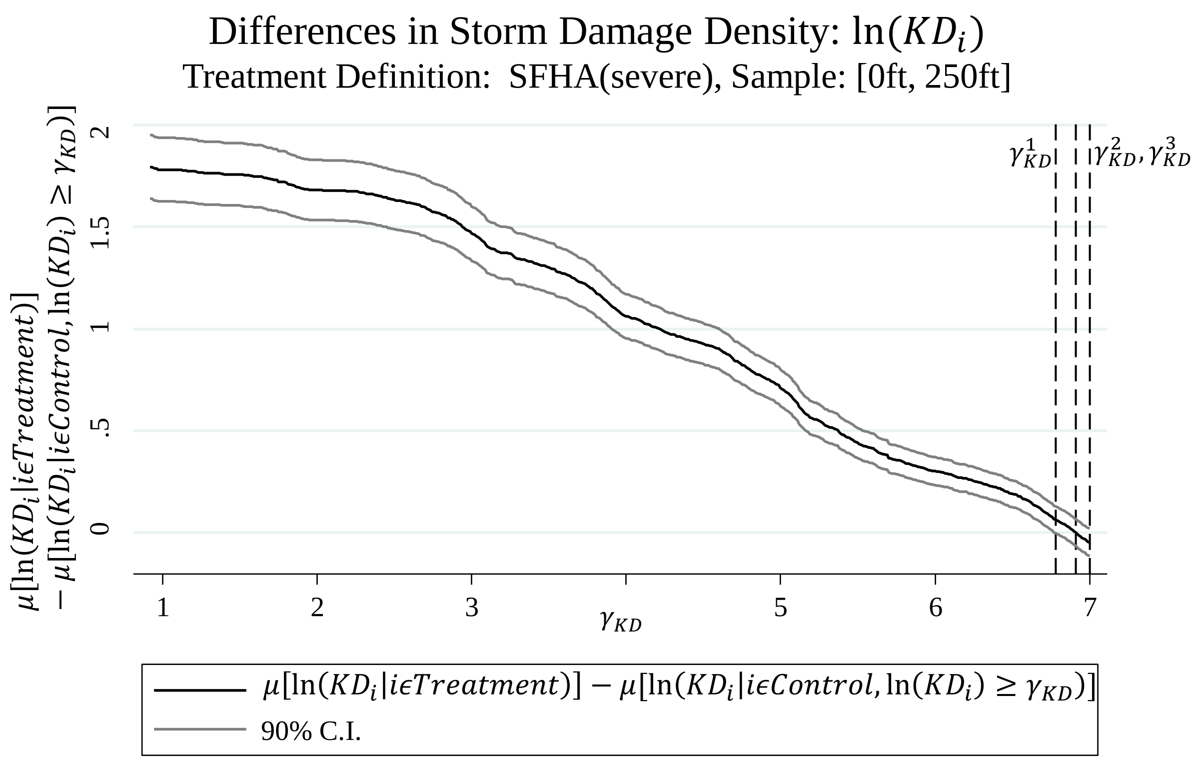
	(1) *Logit* *q*	(2) *Logit* *q*	(3) *LPM* *q*
$\tau(SFHA\ x\ Post)$	-0.397**	-	-
	(0.02)	-	-
$\tau(SFHA(severe)\ x\ Post)$	-	-0.83***	-0.87***
	-	(0.00)	(0.00)
$\tau(SFHA(mild)\ x\ Post)$	-	-0.177	-0.18
	-	(0.48)	(0.43)
Observations	347,839	347,839	347,839
$P[\ \tau(SFHA(severe)\ x\ Post) < \tau(SFHA(mild)\ x\ Post)]$	-	0.011	0.006
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*y*	*y*	*y*

(b) Sample Definition: [250ft, 500ft]

	(1) *Logit* *q*	(2) *Logit* *q*	(3) *LPM* *q*
$\tau(SFHA\ x\ Post)$	0.165	-	-
	(0.74)	-	-
$\tau(SFHA(severe)\ x\ Post)$	-	0.039	0.095
	-	(0.97)	(0.94)
$\tau(SFHA(mild)\ x\ Post)$	-	0.192	0.15
	-	(0.74)	(0.78)
Observations	370,507	370,507	370,507
$P[\ \tau(SFHA(severe)\ x\ Post) < \tau(SFHA(mild)\ x\ Post)]$	-	0.45	0.48
Year-Quarter Fixed Effects	*y*	*y*	*y*
Lagged Dependent Variable	*y*	*y*	*y*

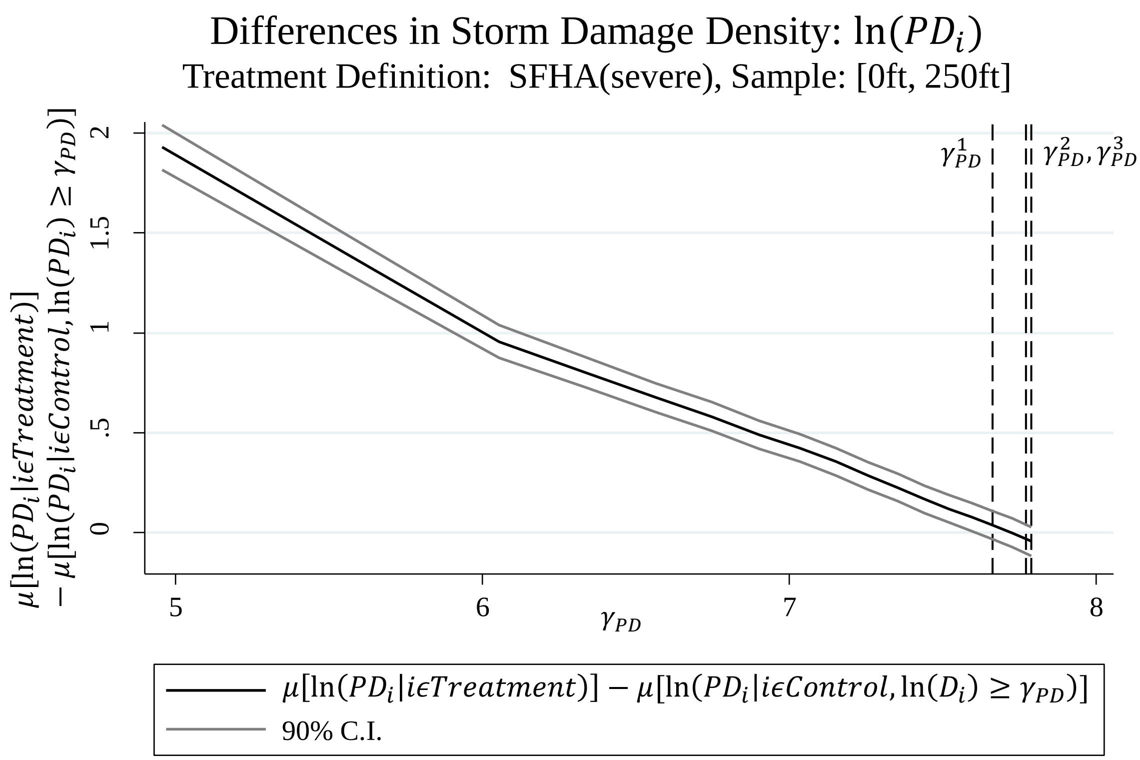
Notes: P-values in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in columns (1), (2), and (3) include year by quarter fixed effects, indicator variables for number of units, neighborhood fixed effects, lot frontage, lot depth, and second order polynomials in square footage and age.

Figure 30: Kernel Density Analysis



Notes: $\gamma_{KD}^1 = 6.78, \gamma_{KD}^2 = 6.91$, $\gamma_{KD}^3 = 7$.

Figure 31: Point Density Analysis



Notes: $\gamma^1_{PD} = 7.664, \gamma^2_{PD} = 7.728$, $\gamma^3_{PD} = 7.789$.

Table 35: Salience Analysis: Sensitivity of Results to Storm Damage Density Thresholds

(a) Sample Definition: [0ft., 250ft.]

	(1)	(2)	(3)
	Logit	*Logit*	*Logit*
(a.1)	$\gamma^1_{KD} = 6.78$	$\gamma^2_{KD} = 6.91$	$\gamma^3_{KD} = 7$
$\tau(SFHA(severe)\ x\ Post)$	-0.764***	-0.748***	-0.737***
	(0.00)	(0.00)	(0.00)
	[157,018]	[145,548]	[136,713]
(a.2)	$\gamma^1_{PD} = 7.66$	$\gamma^2_{PD} = 7.73$	$\gamma^3_{PD} = 7.79$
$\tau(SFHA(severe)\ x\ Post)$	-0.755***	-0.74***	-0.722***
	(0.00)	(0.00)	(0.00)
	[93,435]	[89,377]	[85,437]
(a.3)	$(\gamma^1_{KD} = 6.78, \gamma^1_{PD} = 7.66)$	$(\gamma^2_{KD} = 6.91, \gamma^2_{PD} = 7.73)$	$(\gamma^3_{KD} = 7, \gamma^3_{PD} = 7.79)$
$\tau(SFHA(severe)\ x\ Post)$	-0.674***	-0.653***	-0.633***
	(0.00)	(0.00)	(0.00)
	[83,610]	[77,583]	[73,268]

(b) Sample Definition: [0ft., 250ft.]

	(1)	(2)	(3)
	LPM	*LPM*	*LPM*
(b.1)	$\gamma^1_{KD} = 6.78$	$\gamma^2_{KD} = 6.91$	$\gamma^3_{KD} = 7$
$\tau(SFHA(severe)\ x\ Post)$	-0.812***	-0.789***	-0.773***
	(0.00)	(0.00)	(0.00)
	[157,018]	[145,548]	[136,713]
(b.2)	$\gamma^1_{PD} = 7.66$	$\gamma^2_{PD} = 7.73$	$\gamma^3_{PD} = 7.79$
$\tau(SFHA(severe)\ x\ Post)$	-0.79***	-0.777***	-0.761***
	(0.00)	(0.00)	(0.00)
	[93,435]	[89,377]	[85,437]
(b.3)	$(\gamma^1_{KD} = 6.78, \gamma^1_{PD} = 7.66)$	$(\gamma^2_{KD} = 6.91, \gamma^2_{PD} = 7.73)$	$(\gamma^3_{KD} = 7, \gamma^3_{PD} = 7.79)$
$\tau(SFHA(severe)\ x\ Post)$	-0.724***	-0.701***	-0.682***
	(0.00)	(0.00)	(0.00)
	[83,610]	[77,583]	[73,268]

Notes: P-values in parenthesis. *** p<.01, ** p<.05, * p<.1. Each model in each cell shows a coefficient estimate from a separate regression. The number of observations of each regression are shown beneath the p-value for each coefficient estimate in brackets. Models in panels (a) and (b) of columns (1), (2), and (3) include year by quarter fixed effects, indicator variables for number of units, neighborhood fixed effects, lot frontage, lot depth, second order polynomials in square footage and age, and a lagged dependent variable.

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APPENDIX A

LIST OF TABLES

Table A1: Demographic Composition: Different Family Types in Owner Market

Family Type	(1a) White Family	(1b) Minority Family	(1c) Black Family	(1d) Hispanic Family
Disclosed x Risky	-0.0308*	0.0311**	0.00709	0.0240**
	(0.0643)	(0.0329)	(0.397)	(0.0258)
Observations	3,475	3,475	3,475	3,475
Family Type	(2a) White Parents	(2b) Minority Parents	(2c) Black Parents	(2d) Hispanic Parents
Disclosed x Risky	-0.0609***	0.0164*	0.0189***	-0.00257
	(0.000199)	(0.0568)	(0.000372)	(0.666)
Observations	3,475	3,475	3,475	3,475
Family Type	(3a) Family Head with College Degree	(3b) Family Head with No More than HS Degree	(3c) White Parents with College Degree	(3d) White Parents with No More than HS Degree
Disclosed x Risky	-0.00802	-0.00272	-0.0149	-0.0265***
	(0.728)	(0.867)	(0.254)	(0.00171)
Observations	3,475	3,475	3,475	3,475
Family Type	(4a) Minority Parents with No More than HS Degree	(4b) Black Parents with No More than HS Degree	(4c) Hispanic Parents with No More than HS Degree	(4d) Seniors with Each Family Member Older than 60
Disclosed x Risky	0.0162**	0.00851**	0.00769	0.000551
	(0.0137)	(0.0461)	(0.116)	(0.963)
Observations	3,475	3,475	3,475	3,475
Housing Characteristics	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes
MSA fixed effect	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports effects of Title X from 16 separate regressions using same sample restriction but different dependent variables. Sample restriction: houses built between 1975 and 1982, and sold between 1992 and 2001. The type of a family is categorized by the family head. All these regressions include other independent variables, and coefficient of those variables are not reported. Please see Table (2) for a list of the control variables used in each specification.

Table A2: Demographic Composition: Different Family Type in Rental Market

	(1a)	(1b)	(1c)	(1d)
Family Type	White Family	Minority Family	Black Family	Hispanic Family
Disclosed x Risky	-0.0469*	0.0293	-0.000927	0.0302*
	(0.0936)	(0.311)	(0.953)	(0.0789)
Observations	6,852	6,852	6,852	6,852
	(2a)	(2b)	(2c)	(2d)
Family Type	White Parents	Minority Parents	Black Parents	Hispanic Parents
Disclosed x Risky	-0.0231	0.0183	-0.00148	0.0198**
	(0.132)	(0.256)	(0.871)	(0.0430)
Observations	6,852	6,852	6,852	6,852
	(3a)	(3b)	(3c)	(3d)
Family Type	Family Head with College Degree	Family Head with No More than HS Degree	White Parents with College Degree	White Parents with No More than HS Degree
Disclosed x Risky	0.00522	-0.0166	-0.00373	-0.0206*
	(0.674)	(0.246)	(0.406)	(0.0572)
Observations	6,852	6,852	6,852	6,852
	(4a)	(4b)	(4c)	(4d)
Family Type	Minority Parents with No More than HS Degree	Black Parents with No More than HS Degree	Hispanic Parents with No More than HS Degree	Seniors with Each Family Member Older than 60
Disclosed x Risky	0.0192*	0.00300	0.0162**	0.00755
	(0.0812)	(0.648)	(0.0248)	(0.504)
Observations	6,852	6,852	6,852	6,852
Housing Characteristics	Yes	Yes	Yes	Yes
Sale Year Fixed Effect	Yes	Yes	Yes	Yes
MSA fixed effect	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. P-values, which are reported in parenthesis, are based on robust clustered standard errors at MSA and whether the house is built before and after 1978. This table reports effects of Title X from 16 separate regressions using same sample restriction but different dependent variables. Sample restriction: houses built between 1975 and 1982, and sold between 1992 and 2001. The type of a family is categorized by the family head. All these regressions include other independent variables, and coefficient of those variables are not reported. Please see Table (2) for a list of the control variables used in each specification.

Table A3: Difference-in-Differences Estimates: Birthweight (Wind Sample)

	(1)	(2)	(3)	(4)	(5)
Fire Sample:	**Wind**	**Wind**	**Wind**	**Wind**	**Wind**
Dependent Variable:	*ln(bw)*	*ln(bw)*	*ln(bw)*	*ln(bw)*	*ln(bw)*
(Polluted) x (Tri 3)	-0.0436	-0.0465*	-0.0532**	-0.0543**	-0.0569**
	(0.130)	(0.0808)	(0.0489)	(0.0395)	(0.0298)
(Polluted) x (Tri 2)	-0.0343	-0.0397	-0.0544*	-0.0450	-0.0450*
	(0.276)	(0.205)	(0.0505)	(0.105)	(0.0975)
(Polluted) x (Tri 1)	-0.00545	-0.0114	-0.0270	-0.0237	-0.0281
	(0.888)	(0.767)	(0.492)	(0.551)	(0.488)
(Stressed) x (Tri 3)	-0.0110	-0.00811	-0.0120	-0.0102	-0.0115
	(0.619)	(0.707)	(0.593)	(0.658)	(0.615)
(Stressed) x (Tri 2)	-0.00114	-0.00265	-0.00285	-0.000147	-0.00206
	(0.946)	(0.879)	(0.873)	(0.993)	(0.908)
(Stressed) x (Tri 1)	0.00756	0.00630	0.00597	0.0113	0.0119
	(0.643)	(0.694)	(0.714)	(0.508)	(0.486)
Gestational Age	*no*	*yes*	*yes*	*yes*	*yes*
Geographic Controls	*no*	*no*	*yes*	*yes*	*yes*
Year-Quarter Fixed Effects	*no*	*no*	*no*	*yes*	*yes*
Demographic Controls	*no*	*no*	*no*	*no*	*yes*
Observations	7,398	7,398	7,398	7,398	7,398

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. Columns (1) - (5) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3) based on the data described in column (1) of Table (1); Column (5) replicates Column (2) of Table (2). Please refer to Table (2) for a description of the geographic and demographic controls included in these models.

Table A4: Difference-in-Differences Estimates: Birthweight (Smoke Sample)

	(1)	(2)	(3)	(4)	(5)
Fire Sample:	**Smoke**	**Smoke**	**Smoke**	**Smoke**	**Smoke**
Dependent Variable:	*ln(bw)*	*ln(bw)*	*ln(bw)*	*ln(bw)*	*ln(bw)*
(Polluted) x (Tri 3)	-0.0171	-0.0343	-0.0448	-0.0476	-0.0481*
	(0.561)	(0.213)	(0.129)	(0.106)	(0.0955)
(Polluted) x (Tri 2)	-0.0351*	-0.0371*	-0.0382*	-0.0408*	-0.0379*
	(0.0994)	(0.0873)	(0.0853)	(0.0725)	(0.0940)
(Polluted) x (Tri 1)	-0.00146	-0.00913	-0.0115	-0.0156	-0.0129
	(0.959)	(0.739)	(0.675)	(0.574)	(0.643)
(Stressed) x (Tri 3)	-0.0217	-0.0255	-0.0279	-0.0232	-0.0281
	(0.689)	(0.631)	(0.603)	(0.668)	(0.604)
(Stressed) x (Tri 2)	0.0306	0.0325	0.0326	0.0383	0.0302
	(0.355)	(0.312)	(0.318)	(0.236)	(0.359)
(Stressed) x (Tri 1)	-0.0443	-0.0460	-0.0486	-0.0492	-0.0482
	(0.243)	(0.226)	(0.183)	(0.198)	(0.194)
Gestational Age	*no*	*yes*	*yes*	*yes*	*yes*
Geographic Controls	*no*	*no*	*yes*	*yes*	*yes*
Year-Quarter Fixed Effects	*no*	*no*	*no*	*yes*	*yes*
Demographic Controls	*no*	*no*	*no*	*no*	*yes*
Observations	4,736	4,736	4,736	4,736	4,736

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. Columns (1) - (5) report coefficient estimates of the treatment group by post-fire interaction terms specified in equation (3) based on the data described in column (2) of Table (1); Column (5) replicates Column (4) of Table (2). Please refer to Table (2) for a description of the geographic and demographic controls included in these models.

Table A5: Robustness Checks (Birth Injuries): Smoke Sample

	(1)	(2)	(3)
Fire Sample:	**Smoke**	**Smoke**	**Smoke**
Robustness Check:	Baseline Model	Contaminated Controls (Smoke)	Contaminated Controls (Smoke + Wind)
Dependent Variable:	*Birth Injury*	*Birth Injury*	*Birth Injury*
(Polluted) x (Tri 3)	0.00274	0.00256	0.00394
	(0.119)	(0.165)	(0.133)
(Polluted) x (Tri 2)	0.000435	0.00124	0.00164
	(0.878)	(0.313)	(0.338)
(Polluted) x (Tri 1)	0.00130	0.00130	0.00218
	(0.444)	(0.202)	(0.139)
(Stressed) x (Tri 3)	-0.00124	-0.00164	-0.000927
	(0.570)	(0.388)	(0.570)
(Stressed) x (Tri 2)	-0.00192	-0.00133	-0.00126
	(0.316)	(0.382)	(0.359)
(Stressed) x (Tri 1)	-0.00146	-0.00171	-0.00113
	(0.448)	(0.231)	(0.341)
Observations	4,736	4,736	4,736

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. The baseline model in Column (1) replicates Column (6) of Table (11). As described in Section 5.1.3, Column (2) tests the sensitivity of Column (1) to group by fire and group by trimester indicator variables for the set of infants within the smoke plume of a fire, but located further than one mile. Column (3) tests the sensitivity of Column (1) to group by fire and group by trimester indicator variables for the set of infants either within the smoke plume of a fire or downwind of a fire, but located further than one mile. Each model includes: Year-quarter fixed effects; treatment group by fire fixed effects; geographic controls; and the birthweight and gestational age of each mother's infant.

Table A6: Robustness Checks (Birth Injuries): Wind Sample

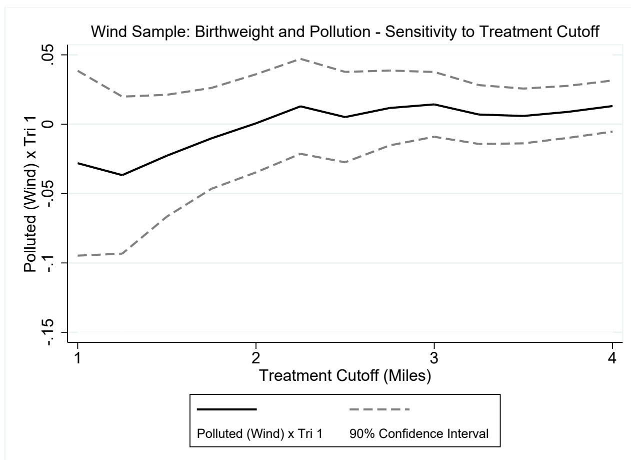
Fire Sample	**Wind**	**Wind**	**Wind**
Robustness Check:	Baseline Model	Contaminated Controls	Erratic Wind
Dependent Variable:	*Birth Injury*	*Birth Injury*	*Birth Injury*
(Polluted) x (Tri 3)	0.00162	0.00187	0.00145
	(0.100)	(0.127)	(0.187)
(Polluted) x (Tri 2)	-0.000165	-0.000590	-0.00103
	(0.924)	(0.816)	(0.622)
(Polluted) x (Tri 1)	-0.000904	-0.000128	-0.000511
	(0.483)	(0.949)	(0.630)
(Stressed) x (Tri 3)	0.000707	0.000989	0.000841
	(0.350)	(0.241)	(0.289)
(Stressed) x (Tri 2)	-0.000206	-0.000550	-0.000817
	(0.878)	(0.792)	(0.646)
(Stressed) x (Tri 1)	-0.000876	-0.000181	-0.00114
	(0.486)	(0.922)	(0.542)
Observations	7,398	7,398	5,377

Notes: ***p<.01, **p<.05, and *p<.1. P-values, which are reported in parenthesis, are based on robust (Huber-White) standard errors. The baseline model in Column (1) replicates Column (6) of Table (10). Column (2) tests the sensitivity of Column (1) to group by fire and group by trimester indicator variables for the set of infants located downwind of a fire, but further than one mile. Column (3) tests the sensitivity of Column (1) to excluding fires with an erratic wind pattern flag as described in Section 5.1.2. Each model includes: Year-quarter fixed effects; treatment group by fire fixed effects; geographic controls; and the birthweight and gestational age of each mother's infant.

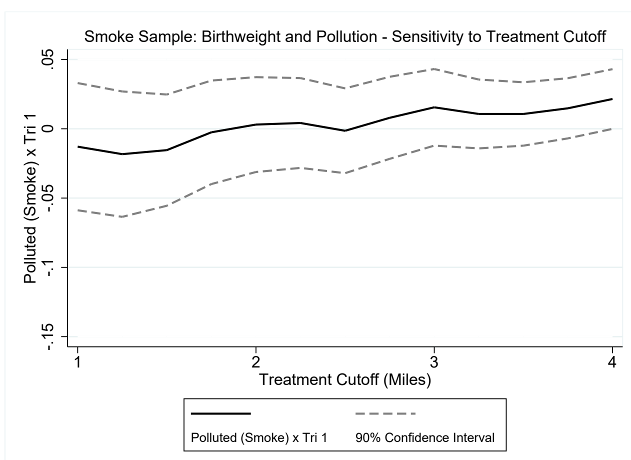
APPENDIX B

LIST OF FIGURES

Figure B1: Sensitivity to Treatment Cutoff: Air Pollution & Birthweight (Trimester 1 Effects)

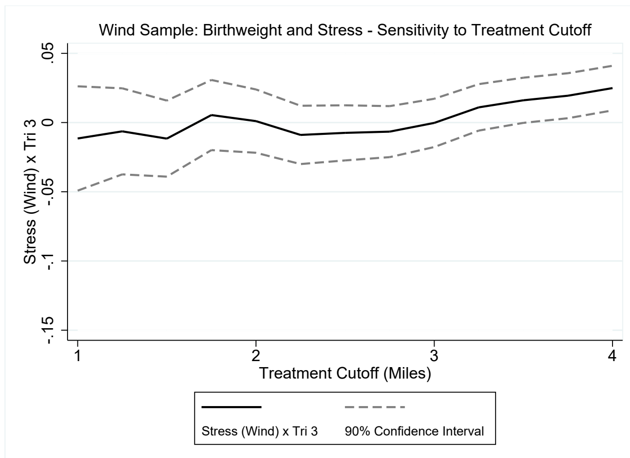


(a) Wind Model

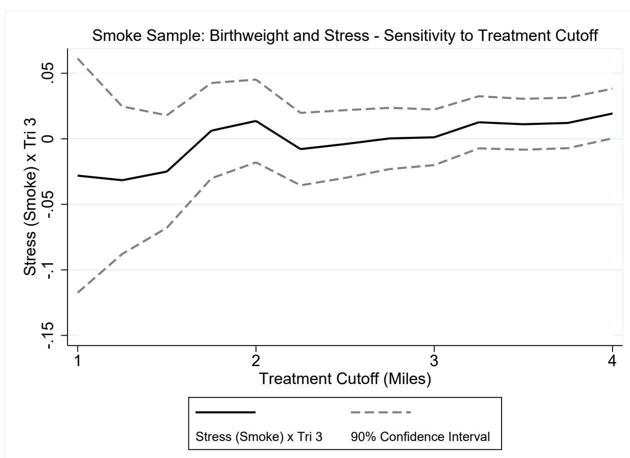


(b) Smoke Model

Figure B2: Sensitivity to Treatment Cutoff: Stress & Birthweight (Trimester 3 Effects)

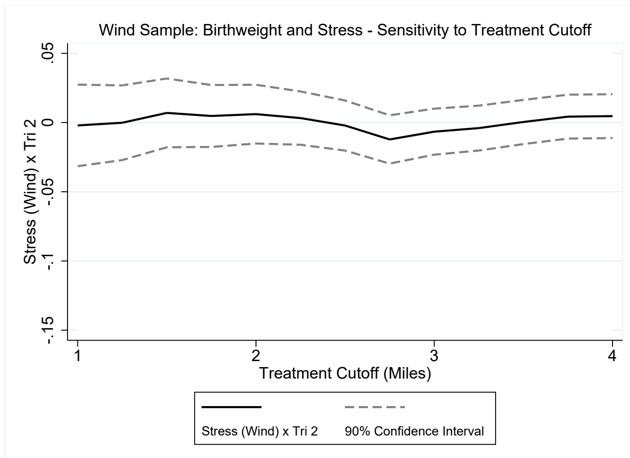


(a) Wind Model

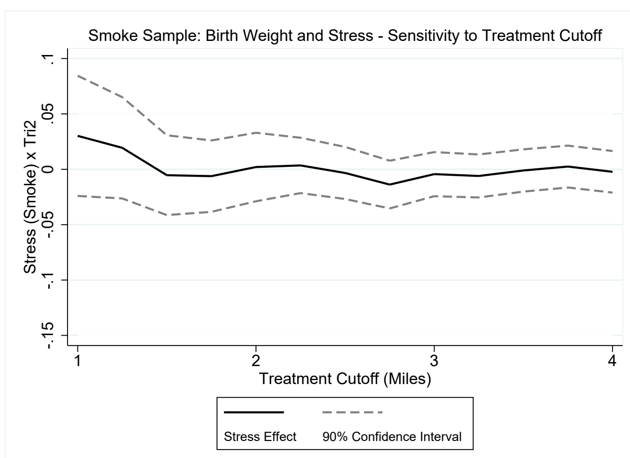


(b) Smoke Model

Figure B3: Sensitivity to Treatment Cutoff: Stress & Birthweight (Trimester 2 Effects)

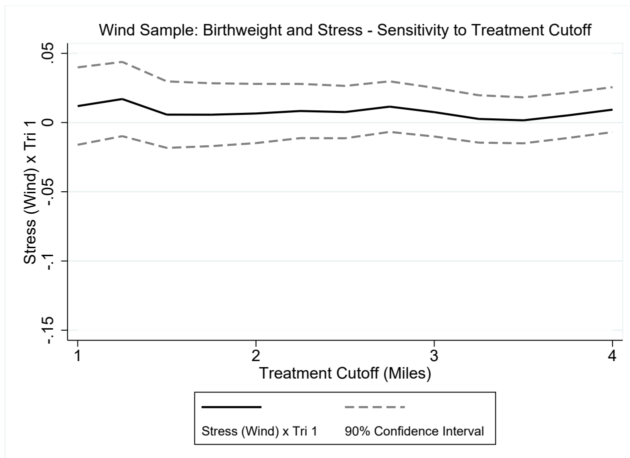


(a) Wind Model

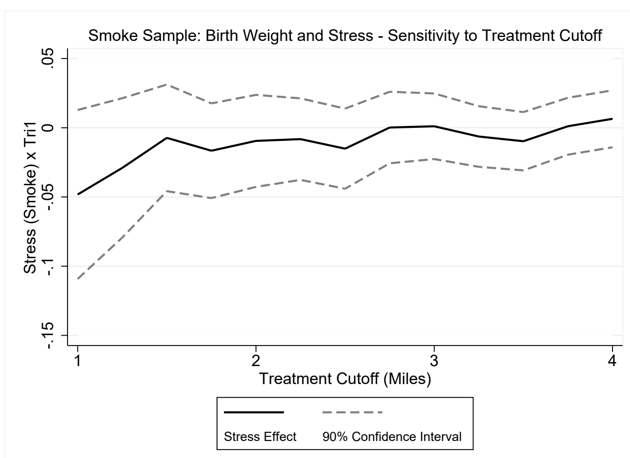


(b) Smoke Model

Figure B4: Sensitivity to Treatment Cutoff: Stress & Birthweight (Trimester 1 Effects)

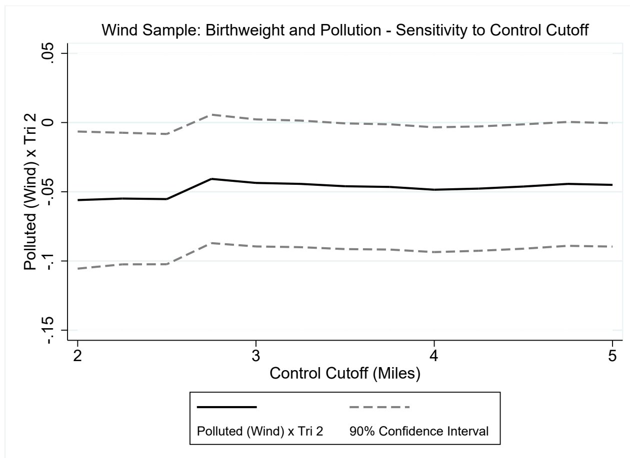


(a) Wind Model

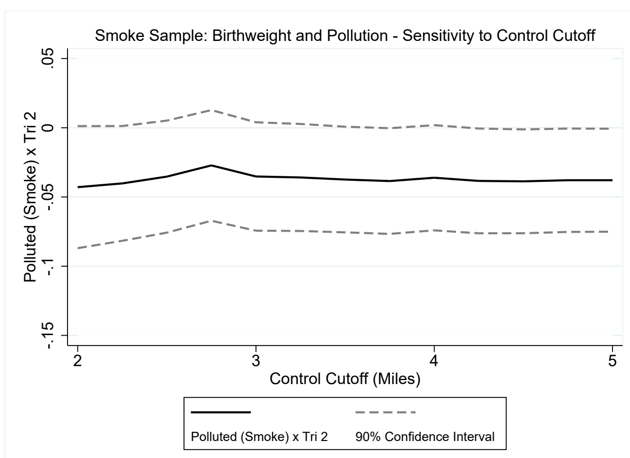


(b) Smoke Model

Figure B5: Sensitivity to Control Cutoff: Air Pollution & Birthweight (Trimester 2 Effects)

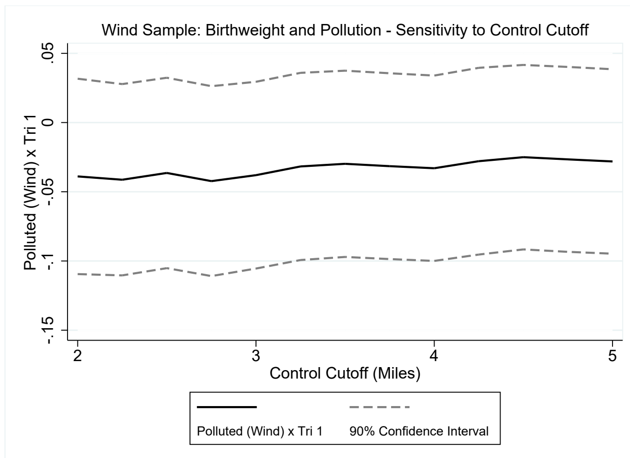


(a) Wind Model

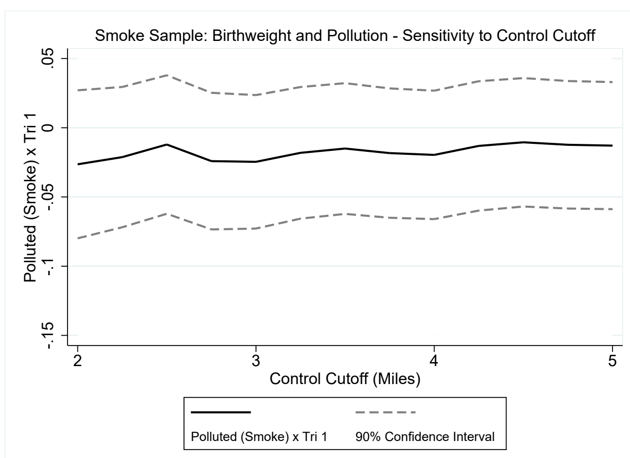


(b) Smoke Model

Figure B6: Sensitivity to Control Cutoff: Air Pollution & Birthweight (Trimester 1 Effects)

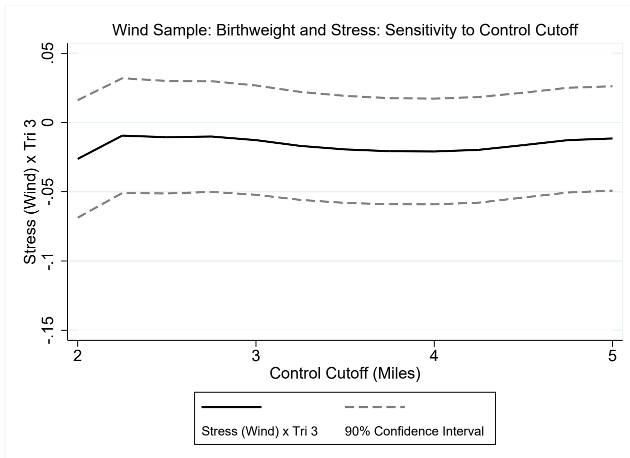


(a) Wind Model

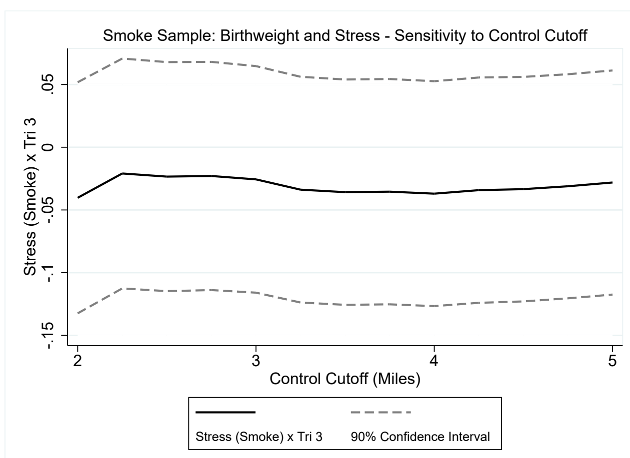


(b) Smoke Model

Figure B7: Sensitivity to Control Cutoff: Stress & Birthweight (Trimester 3 Effects)

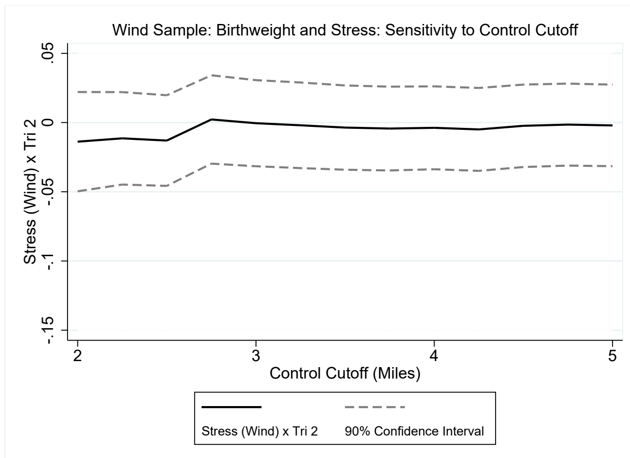


(a) Wind Model

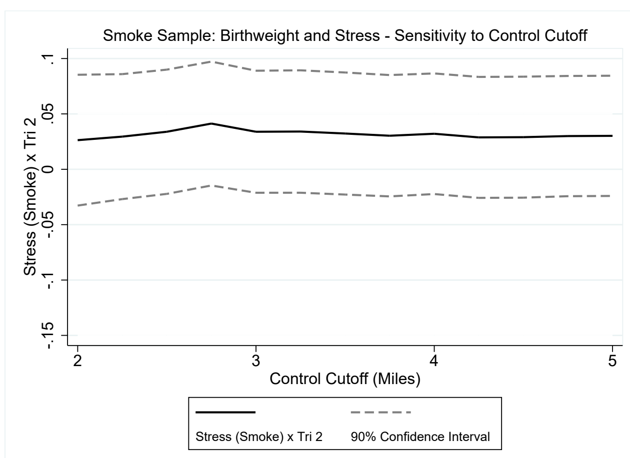


(b) Smoke Model

Figure B8: Sensitivity to Control Cutoff: Stress & Birthweight (Trimester 2 Effects)

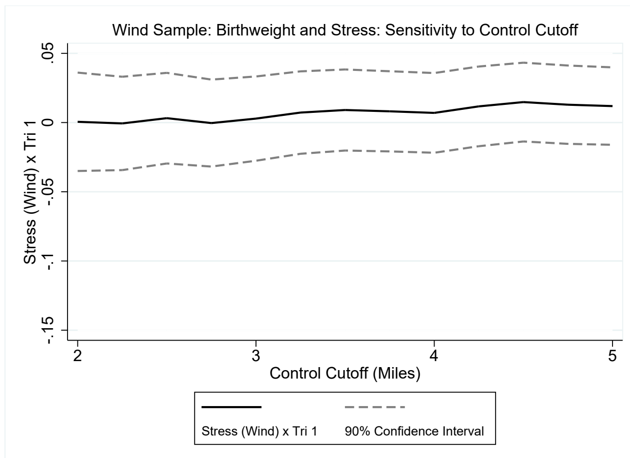


(a) Wind Model

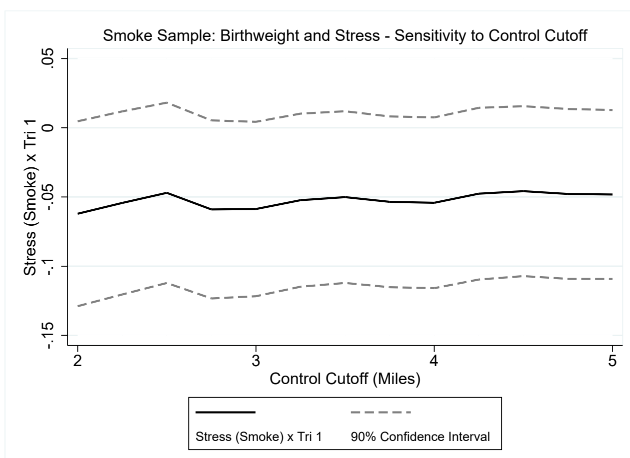


(b) Smoke Model

Figure B9: Sensitivity to Control Cutoff: Stress & Birthweight (Trimester 1 Effects)



(a) Wind Model



(b) Smoke Model