NATURAL DISASTERS, RISK-SALIENCE, AND PUBLIC HEALTH

by

Shawn James McCoy

B.A. in Economics, University of Nevada, Las Vegas, 2009

M.A. in Economics, University of Pittsburgh, 2012

Submitted to the Graduate Faculty of

the Kenneth P. Dietrich School of Arts and Sciences

in partial fulfillment of the requirements

for the degree of PhD in Economics

University of Pittsburgh

2016

UNIVERSITY OF PITTSBURGH

DIETRICH SCHOOL OF ARTS AND SCIENCES

This dissertation was presented

by

Shawn James McCoy

It was defended on

April 8, 2016

and approved by

Randall P. Walsh, PhD, Associate Professor

Werner Troesken, PhD, Professor

Arie Beresteanu, PhD, Associate Professor

Jeremy Weber, PhD, Assistant Professor

Dissertation Director: Randall P. Walsh, PhD, Associate Professor

NATURAL DISASTERS, RISK-SALIENCE, AND PUBLIC HEALTH

Shawn James McCoy, PhD

University of Pittsburgh, 2016

This dissertation explores the dynamic links between natural disasters, human decision making, and risk perceptions as well as the public health implications of wildfire. In Chapter 1, we develop a model that links underlying changes in location-specific risk perceptions to housing market dynamics. We apply the model's predictions to an empirical analysis of the influence of severe wildfires on housing markets. Interpreted in the context of the model, our empirical results suggest that the evolution of risk perceptions following a natural disaster depend both on the characteristics of the property (relationship to the disaster and latent risk) and the location of the individual whose risk perceptions we are considering (potential seller vs. potential buyer). In Chapter 2, we examine the relationship between hurricanes, the salience of flood risk, and residential property investment. Utilizing a difference-in-differences estimation strategy, we find a significant increase in the probability a homeowner invests in a damaged building located in a statutorily designated flood risk area. However, we find no change in the rate of property investment in damaged homes located outside of these areas. We estimate changes in households' perceptions of risk by modeling relative changes in investment between properties in designated risk areas and properties directly outside of these zones restricting attention to the set of structures that failed to experience any damage by the storm. Model 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. Finally, in Chapter 3 we estimate the effects of wildfire on infant health. Model results show that wildfires lead to a statistically significant 4% to 6% reduction in birthweight conditional on the mother being exposed in her second or third trimester and located inside a wildfire smoke plume or downwind of a wildfire burn area. We find no statistically significant effects of wildfire on the health of infants located more than 3 miles away from a burn scar, living outside of smoky areas, or upwind of a wildfire.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

INTRODUCTION

This dissertation explores the dynamic links between natural disasters, human decision making, and risk perceptions. Two observations motivate this work. First, with climate change, the frequency and severity of natural disasters has increased. Second, for many natural disasters, an increasing share of the world's population is living in at risk areas. These observations raise questions about how to best foster societies that are resilient to catastrophic events. What factors, for example, influence households' perceptions of climate risks? Do households respond optimally to changes in risk, and if not, what might be done to promote more efficient outcomes? Similarly, what are the public health implications of natural hazards and what might be done to mitigate the health risks and damages associated with natural disasters? These questions form the central themes of this dissertation which we explore by integrating theory and econometric methods from urban and regional economics with highly refined geo-spatial data.

The structure of this dissertation is divided into three chapters that cover different aspects of the impacts and risks associated with natural disasters. In Chapter 1 – a co-authored project with Randy Walsh[1] – we investigate the link between severe wildfires and changes in agents' perceptions of wildfire risk by considering housing price and housing transaction rate dynamics between properties in and out of wildfire risk areas following severe fires. Using GIS, we control for dis-amenity confounds by omitting properties within a certain radius or that had a view of a wildfire burn area. In addition, we formulate a theoretical model

[1]email: walshr@pitt.edu. website: http://www.randallwalsh.com/

of preference-based sorting which links housing price and housing transaction dynamics to underlying changes in risk perceptions. Our theoretical and empirical results show that wildfires heighten households' perception of wildfire risk. However, these effects are short-lived. Changes in risk-saliency attenuate over the course of two years following a disaster. This result shows that in the absence of re-curring disasters or persistent information shocks, we cannot necessarily expect potential gaps between risk-perceptions and underlying latent risk levels to close over time.

Chapter 1 explores the link between natural disasters and disaster-risk saliency by investigating residential housing price dynamics to wildfire. In Chapter 2 – a co-authored project with Xiaoxi Zhao[2] – we investigate saliency dynamics by modeling homeowners' decisions to invest in their homes in response to Hurricane Sandy. In this project, we utilize spatial data delineating flood hazard zones (often referred to as special flood hazard areas or SFHAs) and the locations of buildings damaged by the Hurricane. These data allow us to draw inferences regarding changes in risk-saliency by estimating changes in the likelihood homeowners in SFHAs invest before and after the hurricane, relative to similar households directly outside of these areas. We show that in the case of flood-risk; changes in risk-perceptions following a storm are pronounced, but are driven heavily by homeowners' exposure to storm damage; in areas less-proximate to storm damage we find *no* change in the rate at which homeowners in the SFHA invest. We interpret our findings in light of the fact that following a natural disaster, there exists other sources of information available to households regarding changes the relative risk of living in a disaster-prone area that are not correlated with homeowners' proximity to storm damage; most notably, information garnered from increased media coverage. However, to the extent that homeowners in less-proximate regions of storm damage fail to modify their behaviors (as captured by their decisions to invest their homes), our results show that households fail to react to these alternative sources of information. As a result, our findings cast doubt on the ability of an information-based regulation to align

[2]email: x.zhao@pitt.edu

risk-perceptions with risk-actualities.

The data we construct in Chapter 2 also allow us to estimate factors influencing post-disaster remedial investment decisions. We estimate a significant increase in the rate at which owners of damaged homes located in SFHAs invest, but we find no such change in investment rates in damaged homes located directly outsize of these areas. We show that one of the key differences between homes in and out of flood hazard areas is the rate at which homeowners acquire flood insurance. 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 even though properties in non-SFHAs account for over *half* of all losses due to floods in the U.S. (Burby, 2001; Dixon et al., 2006). Our results show that the provision of flood-insurance plays a significant role in facilitating post-disaster home re-investment. However, it is difficult to explain why homeowners outside of the SFHA fail to invest at all. If it were economically efficient to invest in a damaged building outside the SFHA, we ought expect some increase in investment rates in these homes, but we do not. Moreover, 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. These facts provide evidence suggesting we cannot rule out the possibility that current flood-insurance regulations have the unintended effect of promoting economically inefficient investment projects in disaster prone regions.

In Chapter 1 we show that the magnitude of the population affected by recent wildfires – as well as the size of the population at risk from future events – is non-trivial. In the year 2011 alone, roughly two-thirds of households in the United States lived in counties affected by wildfire smoke. (Knowlton, 2013). Motivated by these observations, in Chapter 3 – co-authored with Xiaoxi Zhao – we estimate the impact of wildfires on fetal health outcomes. We use vital statistics records for the universe of infants born in Colorado linked to the latitude and longitude coordinates of each infants home. Using daily satellite imagery, we create maps of the spatial extent of wildfire smoke plumes in GIS. These maps allow us to compare the birth outcomes of infants in and out of polluted areas before and after severe wildfires. Using a difference-in-differences estimation strategy, we find that wildfires lead to a 4% to 6% reduction in birthweight conditional on the mother being exposed in her second or third trimester. We find no statistically significant effects of wildfire on infants located more than 3 miles away from a wildfire or living outside of wildfire smoke plumes. These findings link short-term changes in air pollution to reduced birthweight. In addition, we show that the physical and psychological stress of living in close proximity to a natural disaster aren't strong enough to translate into fetal health outcomes.

1. WUI ON FIRE: RISK, SALIENCE AND HOUSING DEMAND

(with Randy Walsh)

1.1. INTRODUCTION

Building on the early work of Tversky and Kahnemann (Tversky and Kahnemann,1974; Kahnemann and Tversky,1979), social scientists increasingly focus on the role that salience plays in explaining individual behavior in the face of risk. Formally defined, salience is "the phenomenon that when one's attention is differentially directed to one portion of the environment rather than others, the information contained in that portion will receive disproportionate weighting in subsequent judgments." (Taylor and Thompson, 1982). In recent work, Bordalo et al. (2012) rationalize salience with a theory of choice over lotteries where agents replace true or objective probabilities over states with subjective, decision weights. Their model can effectively rationalize many ostensible inconsistencies in decision making including preference reversals and frequent risk-seeking behavior. While well understood at a theoretical level, direct empirical evidence of saliency dynamics, and how they translate into behavioral outcomes, is limited. From a policy perspective, saliency dynamics are particularly relevant for understanding market and individual behaviors in the face of natural hazards risks as households perceptions of risk are inextricably linked to their willingness to mitigate against risk as well as their preference for living in disaster prone areas. These observations motivate us to ask, "To what extent do natural disasters impact risk salience?"

While well understood at a theoretical level, direct empirical evidence of saliency dynamics, and how they translate into behavioral outcomes, is limited. From a policy perspective, saliency dynamics are particularly relevant for understanding market and individual behaviors in the face of natural hazards risks since households perceptions of risk are inextricably linked to their willingness to mitigate against risk as well as their preference for living in disaster prone areas. These observations motivate us to ask, "To what extent do natural disasters impact risk salience and how do saliency dynamics subsequently evolve over time?"

Natural disasters are an apt context within which to investigate salience dynamics, for a number of reasons. First, they are plausibly exogenous shocks to agents beliefs over disaster risk. After witnessing a natural disaster agents may re-weight their perceived probability of a catastrophic event occurring in the future. Second, saliency dynamics in the face of natural disaster risk have important real world consequences. In particular, when households hold inaccurate beliefs, we may observe sub-optimal private risk mitigation strategies and an inefficient level of public support for disaster management policies. Finally, both the frequency and severity of natural disasters is increasing. Half of the ten most costly natural disasters in history have occurred in the last decade alone.[1] This trend is particularly strong in the case of wildfires which have seen a four-fold increase in their frequency and a six-fold increase in the average size of their burn scars since 1986. (Westerling et al., 2006). Currently, the United States experiences over 100,000 wildland forest fires each year.[2] In 2012, a single Colorado fire burned more than 87,000 acres. Nationwide, wildfires cost federal agencies $2.9 billion annually. (GAO, 2013). Thus, this is both fertile and relevant ground for studying saliency dynamics.

[1] Natural disasters: Counting the cost of calamities. *The Economist*, (2012). http://www.economist.com/-node/21542755.

[2] Wildfires: Dry, hot, and windy. *National Geographic*, (2013). http://environment.nationalgeographic.com/environment/natural-disasters/wildfires/.

In this paper, we develop a new approach to investigating the saliency dynamics of a natural disaster by formulating a simple theoretical model of preference-based sorting which links housing price and housing transaction dynamics to underlying changes in risk perceptions. We then empirically model the link between wildfire occurrences and housing market dynamics – using the theoretical framework as a lens through which we can gain inference on the underlying shifts in risk perceptions that arise as a result of these wildfires.

In our model, residents choose between two communities which may experience potentially differential shocks to risk saliency following the occurrence of a natural disaster. The model allows us to interpret relative price and quantity dynamics in terms of the relative strength of salience shocks between extant residents located in "high-risk" communities (which we refer to as treated locations) and potential buyers initially located in "zero-risk" communities (which we refer to as control regions). If risk-saliency following a disaster doesn't vary across extant residents and potential buyers our model predicts a decrease in prices but no change in the probability of transacting; all agents update their subjective beliefs about the probability of a fire, but the relative preference ordering of agents living in the fire prone area (as opposed to zero risk locations) remains unchanged. In contrast, negative price shocks coincide with positive quantity shocks when post-disaster saliency varies by the initial allocation of individuals. We explore these observations more formally below and then link the models predictions to an empirical analysis of wildfire.

In addition to the climate-driven increase in wildfire events, social dynamics are also playing a role in increasing the societal costs associated with wildfire. 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) which, as of 2005, contained 39% of the stock of residential housing across the United States. (Travis et al., 2002; Conroy et al., 2003; Radeloff et al., 2005). The sprawling configurations of WUI developments have modified the interactions between environmental and socio-economic dynamics leading to a sharp increase in the likelihood of severe wildfires impacting inhabited spaces. (Radeloff et al., 2005; Spyratos et al., 2007). On a second margin, private mitigation behaviors, such as investment in fire-resistant building materials and fuel reduction treatments around one's property, which may reduce property-specific risks as well as the overall risk of fire in forested lands, appear to occur at much lower levels than would be socially optimal. (Shafran, 2008; Steelman, 2008). Both the decision to develop in disaster-prone areas as well as the decision to privately mitigate against risk are influenced by household perceptions of disaster risk.

We center our empirical analysis on 18 wildfires which occurred in 8 counties spanning WUI areas of the Colorado Front Range (COFR) and utilize the universe of housing transactions data for 358,823 unique residential properties between the years 2000-2012. Using geo-spatial data on wildfire burn scars and latitude and longitude co-ordinates for each property in our sample, we implement GIS routines to produce multiple measures reflecting potential drivers of risk saliency. These include *proximity* to wildfire and *view* of wildfire burn scars – which may also capture the dis-amenity effects of fire – in addition to property-specific indexes of actual *latent risk* of wildfire which may be associated with susceptibility to saliency shocks. Our measures for latent risk represent the probability of a wildfire occurring or burning into an area based on the physical attributes of the terrain surrounding each property such as slope, aspect, elevation and vegetation fuel type. As we discuss below, this last treatment definition is the most robust for identifying saliency effects because in this analysis we focus on properties distant enough from the fire that direct, fire-driven dis-amenity effects are unlikely to be of concern.

To preview our empirical results, we find that home prices in close proximity to a recent wildfire incur a significant negative price shock in the first year following a fire which attenuates after three years. Our duration analysis finds a lagged increase in transaction rates in the third year following a wildfire (with no significant increase predicted in years one and two). Properties with a view of a wildfire burn scar incur an immediate price loss, relative to properties without a view, which remains persistent even after three years. We find no relative change in property turnover along this dimension. Finally, we find that housing values in high-risk zones, relative to housing values in low-risk zones, incur an immediate price shock in the year immediately following a wildfire which is associated with a significant increase in the likelihood of transacting. Interpreted in the context of our theoretical model, these results suggest that the evolution of risk perceptions following a major fire depends both on the characteristics of the property itself (relationship to the fire and latent risk) and the location of the individual whose risk perception we are considering (potential seller, potential buyer).

We proceed as follows. We begin by providing background on the existing work on housing markets and natural hazards risk in Section (1.2). We summarize our theoretical model of price-capitalization and preference-based sorting in response to changing risk perceptions in Section (1.3). We then characterize our study area and the details behind the construction of our geo-spatial data in Section (1.4). We present our empirical methodology in Section (1.5) and our findings in Section (1.6). We summarize and conclude in section (1.7).

1.2. BACKGROUND

At its core this work utilizes a basic theoretical model as a lens through which the impact of wildfires on risk salience can be inferred from housing market dynamics. Our conception of risk salience arises from the early work of Tversky and Kahnemann (see for instance: Tversky and Kahnemann,1974; Kahnemann and Tversky,1979). These authors provided new insight into how agents make decisions in the face of risk. They suggested that in the presence of uncertainty, decision-makers will often resort to simple heuristic principles in order to reduce the computational burden of predicting or assessing the likelihood of events. Specifically, Tversky and Kahnemann's *Availability Heuristic* posits that agents may "assess the frequency of a class or the probability of an event by the ease with which instances or occurrences can be brought to mind." (Tversky and Kahneman, 1974). As a result, while simplifying the computational burden, agents may find themselves acting on a set of beliefs that are systematically inaccurate and biased towards information provided by more recent or poignant events. This early work continues to resonate as social scientists focus increased attention on the role that salience plays in explaining individual behavior in the face of risk.

We link conceptions of risk saliency to an empirical analysis of housing markets and wildfire – considering both prices and transaction rates. While transaction rates remain largely unexplored in this context, there is a large extant literature on the effects of wildfire on housing prices. Examples include Loomis (2004); Troy and Romm (2004); Donovan et al. (2007); Mueller and Loomis (2008); Huggett Jr et al. (2008); Mueller et al. (2009); Champ et al. (2009); Stetler et al. (2010) and Mueller and Loomis (2014). Loomis (2004) finds that housing values in an unburned town two miles from a major wildfire dropped on the order of 15% based on housing transactions data five years after the fire. Mueller et al. (2009) analyze housing market responses to repeated wildfires in Southern California which occurred at different points in time but within a small geographic area and find that repeated events lead to increasingly negative effects on home prices. Donovan et al. (2007) evaluate the role of information shocks on risk perceptions by analyzing the relationship between housing prices and wildfire risk after a website was made available which enabled residents in the city of Colorado Springs to view their risk-rating. They found that households generally placed a premium on higher risk properties (largely due to positive amenity effects associated with drivers of risk) before the website was available but not after. This finding is consistent with the notion we advance in our paper that the provision of information may elevate risk perceptions. These extant papers differ from our work in that they do not have an explicit focus on the impact of a wildfire on risk-salience, generally study a limited geographic area with a small number of fires, and fail to consider the connection between risk perceptions and transaction rates.

In terms of price effects, our empirical work is in some ways closest to that of Kousky (2010), Bin and Landry (2012) and Atreya et al. (2013) who analyze the effects of major floods on housing prices[3]. Bin and Landry (2012) compare residential housing prices for properties located in FEMA designated flood zones to those properties located outside of flood zones, before and after two major hurricanes in Pitt County, North Carolina. The authors report a 5.7% to 8.8% hurricane-induced flood-risk 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 flood-risk discount of 32% which lasts for 7 to 9 years. Kousky (2010) finds no significant change in property prices in the 100-year floodplain but does report a 2% - 5% reduction in property prices in the 500-year floodplain following the 1993 flood on the Missouri and Mississippi rivers. From a risk saliency perspective, the potential for inference from the extant hedonic work on floods and fires is limited. To demonstrate that changes in risk perceptions underlie the observed price changes, we would want to be certain that other, more direct channels are not responsible. Three specific areas of potential concern are: 1) proximate neighborhood infrastructure was harmed by the event; 2) having damaged properties nearby generates a spillover effect a la Campbell et al. (2011); and 3) the presence of composition effects – driven by differences in structural characteristics of houses that sell before and after fire. The one exception that we are aware of is work by Hallstrom and Smith (2005). They compare price differentials between properties in and out of the 100-year flood plain following Hurricane Andrew in 1992. They base their analysis on 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 suggesting that home buyers and sellers act on the information conveyed by a severe storm.

Going beyond the hedonic literature, Anderson et al. (2014) suggests that, through their influence on political support for expenditures on public mitigation programs, salient events may lead to an inefficiently high levels of public spending on programs such as fuels treatments. Finally, using national data on regional floods and flood insurance policies, Gallagher

[3]In other works, the impact of additional environmental hazards and risks have been considered using housing price data associated with the rupture and explosion of a major pipeline (Hansen et al., 2006), hazardous waste (McCluskey and Rausser, 2001), levee breaks (Tobin and Montz, 1988, 1997), and earthquakes (Naoi et al., 2009).

(2014) finds that flood insurance take-up increases the year after a flood, but steadily decreases to baseline levels thereafter.

1.3. A MODEL OF NATURAL DISASTERS, RISK-SALIENCE AND PREFERENCE-BASED SORTING

We consider an economy comprised of a measure 1 continuum of individuals who choose to live in one of two locations $j \in \{t, c\}$. We conceptualize t as a region that is prone to treatment by a natural disaster and c as a control area which has zero risk of a natural disaster. In the context of wildfire, for example, t is an area providing amenity values to some, but with heightened wildfire risk. Formally, for individual i, we denote the relative amenity value of t as a_i which is distributed according to the cumulative distribution function $F_a(.)$. Should a fire occur, individuals in location t experience damages d. We assume that agents hold heterogeneous beliefs over the probability of a natural disaster, π_i, whose distribution in the population is described by the cumulative distribution function $F_\pi(\cdot)$ which is assumed to be independent of F_a.

Conditional on choosing location j, each individual consumes a fixed quantity of housing at price p_j. We fix the price level in c at $\bar{p}_c$ and allow the price level in the treated area (p_t) to adjust endogenously in order to clear both housing markets. All individuals are endowed with the identical income level y.

Individuals choosing to live in the control region receive a utility level given by:

$$u_{c,i} = y - \bar{p}_c.$$

Utility from choosing to live in t depends on whether or not a fire occurs. In the non-disaster state, utility is given by:

$$u_{t,i}^{nd} = y - p_t + a_i,$$

while utility conditional on a disaster occurring is given by:

$$u_{t,i}^{d} = y - p_t + a_i - d.$$

Thus, agent $\omega's$ subjective expected utility from choosing t is given by:

$$u_{t,i} = y - p_t + a_i - \pi_i \cdot d.$$

We denote the individual specific component of utility by $\omega_i = a_i - \pi_i \cdot d$ whose distribution is given by:

$$F_w(w) = \int F_a(w + \pi d) dF_\pi.$$

Finally, we assume that a unit measure of housing supply q is split across the two communities so that $q_t + q_c = 1$ with $q_t,\ q_c > 0$.

In equilibrium, individuals choose the location which maximizes their subjective utility, giving rise to stratification around a critical value of $\omega,\ \omega_0^\star$; with individuals choosing location t when:

$$\omega \geq p_t - \bar{p}_c = \omega_0^\star. \tag{1.1}$$

The equilibrium price level in t, p_t, is then identified by the requirement that land markets clear which is expressed in equation (1.2):

$$F_\omega\left(\omega_0^\star\right) = q_c. \tag{1.2}$$

That is, p_t adjusts such that the proportion of individuals satisfying $\omega < \omega_0^\star$ exactly equals the proportion of the housing supply located in c. We denote by p_t^0 the market clearing price in the baseline equilibrium. Finally, we conceptualize the salience-effects of a natural disaster by assuming that when a disaster occurs in the treatment region agents experience a non-decreasing update to their subjective beliefs about the probability of a disaster. This approach is motivated by the model of Bordalo et al. (2012) under which decision makers overemphasize states that draw attention, in effect, weighting states of the world with more salient payoffs more heavily. Assuming that this salience-effect may be stronger for those living in t at the time of the fire, we allow for heterogeneity in the size of the probability shift, $\Delta\pi$, across individuals based on their location in the baseline equilibrium:

$$\Delta\pi_t \geq \Delta\pi_c \geq 0,\ \ \Delta\pi_t > 0.$$

We also assume that a disaster leads to a non-increasing shift in the relative amenity value of region t, Δa, that is homogeneous across individuals. Thus, following a disaster, the utility achieved in location t may now also depend on an individuals location in the initial equilibrium:

$$u_{t|c} \quad = \quad y - p_t + \omega - \Delta\pi_c d + \Delta a \tag{1.3}$$

$$u_{t|t} \quad = \quad y - p_t + \omega - \Delta\pi_t d + \Delta a. \tag{1.4}$$

With this framework in place, we make several observations regarding how the baseline equilibrium changes following a disaster.

OBSERVATION 1: Conditional Stratification and Dis-Amenity Confounds.

In equilibrium, conditional on their realized relative amenity value for t, individuals completely stratify based on subjective probability beliefs, with all of those with subjective beliefs below some threshold level $\bar{\pi}$ locating in region t. Similarly, conditional on realized subjective risk probabilities, individuals completely stratify based on amenity values, with all of those with amenity values above some threshold level $\bar{a}$ locating in the region t as well. Additionally, *non-zero amenity effects from a disaster will potentially confound empirical identification of saliency effects.*

Conditional stratification arises directly from the equilibrium sorting condition in equation (1.1) while the potential for dis-amenity confounds are apparent from equations (1.3) and (1.4).

OBSERVATION 2: Positive Saliency Shocks Reduce p_t.

The post-disaster equilibrium price in t is strictly less than the pre-disaster equilibrium price: $p_t^1 < p_t^0$.

Observation 1 follows directly from the following. First, because $\Delta\pi > 0$ and $\Delta a \leqslant 0$, for any $p_t \geq p_t^0$, there exists $\delta > 0$ such that for any $\omega \in [\omega_0^\star,\ \omega_0^\star + \delta)$, $y - p_t + \omega - \Delta\pi_t + \Delta a < y - \bar{p}_c$. Because $F_\omega(\cdot)$ is strictly increasing, the set of $\omega \in [\omega_0^\star,\ \omega_0^\star + \delta)$ has positive measure. Thus, post-fire if $p_t \geq p_t^0$ the set of individuals with $\omega \geq \omega_0^\star$ who prefer t over c will be strictly smaller than prior to the fire. Second, it follows immediately from the baseline equilibrium condition that, because $\Delta\pi_c \geq 0$, any individual with $\omega < \omega_0^\star$ will strictly prefer community c if $p_t \geq p_t^0$. Since there will be excess supply in t if $p_t \geq p_t^0$, under the new equilibrium it must be the case that $p_t^1 < p_t^0$.

In the remaining observations, we focus exclusively on the impact of shocks to risk salience and thus for parsimony, and without loss of generality, suppress the dis-amenity effects.

OBSERVATION 3: No Resorting Under Equal Shocks to Risk Salience.

If the disaster saliency doesn't vary with baseline equilibrium location choice ($\Delta\pi_t = \Delta\pi_c = \Delta\pi$) then the post-fire equilibrium sorting of individuals is identical to that of the baseline equilibrium. Further, the size of the fire-driven price drop identified in Observation 1 is increasing in $\Delta\pi$. Specifically: $\partial p_t^1 / \partial \Delta\pi = -d$.

The first half of Observation 2 stems from the fact that when $\Delta\pi_t = \Delta\pi_c$ all individual preferences for locating in t have shifted by an identical distance. We can simply re-cast the problem in terms of a newly defined distribution of types $\hat{F}_\omega(\omega) = F_\omega(\omega + \Delta\pi d)$ where each individual's value of ω has essentially been shifted down by $\Delta\pi$. Thus, in equilibrium, the sorting of individuals across the two locations must be preserved. The second half of Observation 2 follows from totally differentiating the post-disaster equivalent of equation (1.2):

$$\hat{F}_\omega\left(p_t^1 - \bar{p}_c\right) = F_\omega(p_t^1 - \bar{p}_c + \Delta\pi d) = q_c.$$

OBSERVATION 4: Unequal Shocks to Risk Salience Lead to Resorting.

If disaster saliency is higher for individuals initially located in t $(\Delta\pi_t > \Delta\pi_c)$ then there will exist $\delta_t,\ \delta_c > 0$ such that following the disaster the new equilibrium reallocates individuals with $\omega_0^\star \leq \omega < \delta_t$ from t to c and all individuals with $\delta_c \leq \omega < \omega_0^\star$ from c to t.

The logic behind Observation 4 is as follows. First, note that because $\Delta\pi_t > \Delta\pi_c$ if it is optimal for all individuals with $\omega \geq \omega_0^\star$ to choose t post-disaster then there exists $\delta > 0$ such that for any $\omega \in [\omega_0^\star - \delta,\ \omega_0^\star)$,

$$y - p_t^1 - \Delta\pi_c d + \omega > y - p_t^1 - \Delta\pi_t d + \omega_0^\star \geq y - \bar{p}_c.$$

In other words, if p_t^1 is such that all individuals who were initially located in t choose to remain in t post-disaster, then for some values of $\omega < \omega_0^\star$ it will now be optimal to locate in t post-disaster as well. However, by construction, the measure of $\{\omega|\omega \geq \omega_0^\star - \delta\}$ is greater than q_t and this can't be an equilibrium because there would be excess demand in t. Thus, to clear the housing market in the post-fire equilibrium it must be the case that over some positive measure set of $\omega \geq \omega_0^\star$ it must hold that $y - p_t^1 - \Delta\pi_t d + \omega < y - \bar{p}_c$. Further, it is straightforward to demonstrate that this set must be continuous and include $\omega_0^\star$ as its lower bound. The complimentary result can be derived by similar logic.

The bounds of these two sets $(\delta_t,\ \delta_c)$ are identified by the optimality conditions. The range of $\omega \geq \omega_0^\star$ values for which region c is optimal in the post-disaster equilibrium must satisfy:

$$y - p_t^1 - \Delta\pi_t d + \omega < y - \bar{p}_c.$$

Thus, the relevant range for ω is:

$$\omega_0^\star \leq \omega < p_t^1 - \bar{p}_c + \Delta\pi_t d = \delta_t.$$

Similarly, the set of $\omega < \omega_0^\star$ value for which t is optimal post-fire must satisfy:

$$y - p_t^1 - \Delta\pi_c d + \omega > y - \bar{p}_c.$$

And the relevant range for ω is:

$$\delta_c = p_t^1 - \bar{p}_c + \Delta\pi_c d \leq \omega < \omega_0^\star.$$

The new market clearing price is determined by the requirement that for housing market equilibrium to hold, it must be the case that the measure of these two sets be equal:

$$F_\omega\left(p_t^1 - \bar{p}_c + \Delta\pi_t d\right) - F_\omega\left(\omega_0^\star\right) = F_\omega\left(\omega_0^\star\right) - F_\omega\left(p_t^1 - \bar{p}_c + \Delta\pi_c d\right). \tag{1.5}$$

Recalling that $F_\omega\left(\omega_0^\star\right) = q_c$, the new market clearing price is implicitly defined by:

$$\frac{F_\omega\left(p_t^1 - \bar{p}_c + \Delta\pi_t d\right) + F_\omega\left(p_t^1 - \bar{p}_c + \Delta\pi_c d\right)}{2} = q_c. \tag{1.6}$$

Total differentiation of the market clearing condition in (1.6) and equation (1.5) indicates that the magnitude of the price adjustment and the measure of residents who sort between t and c vary proportionally to the magnitude of each locations salience shock. We summarize these formally in Observations (5) and (6).

OBSERVATION 5: Characterizing Price Effects.

The post-disaster price drop in t is increasing in both location's risk-saliency shock. Specifically:

$$\frac{\partial p_t^1}{\partial \Delta\pi_t} = \frac{-F'_\omega\left(p_t^1 - \bar{p}_c + \Delta\pi_t d\right)}{F'_\omega\left(p_t^1 - \bar{p}_c + \Delta\pi_t d\right) + F'_\omega\left(p_t^1 - \bar{p}_c + \Delta\pi_c d\right)},$$

and

$$\frac{\partial p_t^1}{\partial \Delta\pi_c} = \frac{-F_0'\left(p_t^1 - \bar{p}_c + \Delta\pi_c d\right)}{F_\omega'\left(p_t^1 - \bar{p}_c + \Delta\pi_t d\right) + F_\omega'\left(p_t^1 - \bar{p}_c + \Delta\pi_c d\right)}.$$

OBSERVATION 6: Characterizing Quantity Effects.

The size of the post-disaster relocation – measure of $\{\omega|\delta_c \leq \omega < \omega_0^\star\}$ = *measure of* $\{\omega|\omega_0^\star \leq \omega < \delta_t\}$ *– is increasing in* $\Delta\pi_t$ *and decreasing in* $\Delta\pi_c$*. Specifically, this change is given by:*

$$F_\omega(\omega_0^\star) - F_\omega(\delta_c) \;=\; F_\omega(\delta_t) - F_\omega(\omega_0^\star) \;=\; \frac{F_\omega'\left(p_t^1 - \bar{p}_c + \Delta\pi_t d\right) \cdot F_\omega'\left(p_t^1 - \bar{p}_c + \Delta\pi_c d\right)}{F_\omega'\left(p_t^1 - \bar{p}_c + \Delta\pi_t d\right) + F_\omega'\left(p_t^1 - \bar{p}_c + \Delta\pi_c d\right)}.$$

To summarize our theoretical results, the treated and control regions in our model delineate locations based on resident's experience with or their perceived likelihood of a natural disaster. The predictions of our theoretical model allow us to interpret price and quantity responses in terms of differential saliency between extant residents and potential buyers. If risk-saliency changes following a disaster don't vary across extant residents and potential buyers our model predicts a decrease in prices but no change in the probability of transacting. Negative price shocks coincide with positive quantity shocks only when post-disaster saliency varies between potential sellers located in the treated area and potential buyers located outside the treated area; that is, when one group experiences a stronger shock than the other. As such, we can approach the task of discerning saliency dynamics by investigating the evolution of prices and quantities through the lens of our theoretical framework.

While we focus on saliency shocks, our theoretical results carry through for amenity shocks as well. Amenity shocks contrast to saliency changes in that they are observable, are likely to be relatively more persistent, and may serve to reinforce the initial salience shock of a disaster. In such cases, it may be difficult to disentangle amenity changes from saliency dynamics. As we discuss below, this potential confound motivates our empirical analysis of latent risk which involves identifying portions of the landscape where the dis-amenity effects of wildfire are plausibly absent.

1.4. STUDY AREA AND DATA

The Colorado Front Range forms a barrier between the easternmost range of the Rocky Mountains and the Great Plains regions of eastern Colorado. The region's population increased by 30% from 1990 - 2000 with the growth predominantly concentrated in the interface and intermix communities of the WUI. (Travis et al. 2002). As depicted in Figure (1.1), we conduct our analysis across 8 counties spanning the COFR: Boulder, Douglas, Larimer, Pueblo, El Paso, Jefferson, Teller and Fremont. We identify WUI properties in these locations based on GIS data provided by the Silvis Lab[4]. (Radeloff et al., 2005). The WUI is composed of interface and intermix regions. In both types of WUI regions, housing density must exceed one structure per 40 acres while intermix areas must also be at least 50% vegetated and lie within 1.5 miles of an area at least 1,325 acres large that is at least 75% vegetated.

We obtained a list of wildfire incidents from FEMA's disaster declaration web-page[5]. We use FEMA as a reference point for identifying severe wildfires. FEMA records each fire's start-date, end-date and the total dollars obligated in public assistance grants. We cross-check these dates with the information contained in each fire's Incident Status Summary (ICS-209) report which we obtained from the National Fire and Aviation Management Web Application[6] maintained by the National Inter-agency Fire Center[7].

Spatial data-sets for each fire's burn scar were acquired from the Geospatial Multi-Agency

[4]http://silvis.forest.wisc.edu/
[5]http://www.fema.gov/disasters
[6]https://fam.nwcg.gov/fam-web/
[7]http://www.nifc.gov/

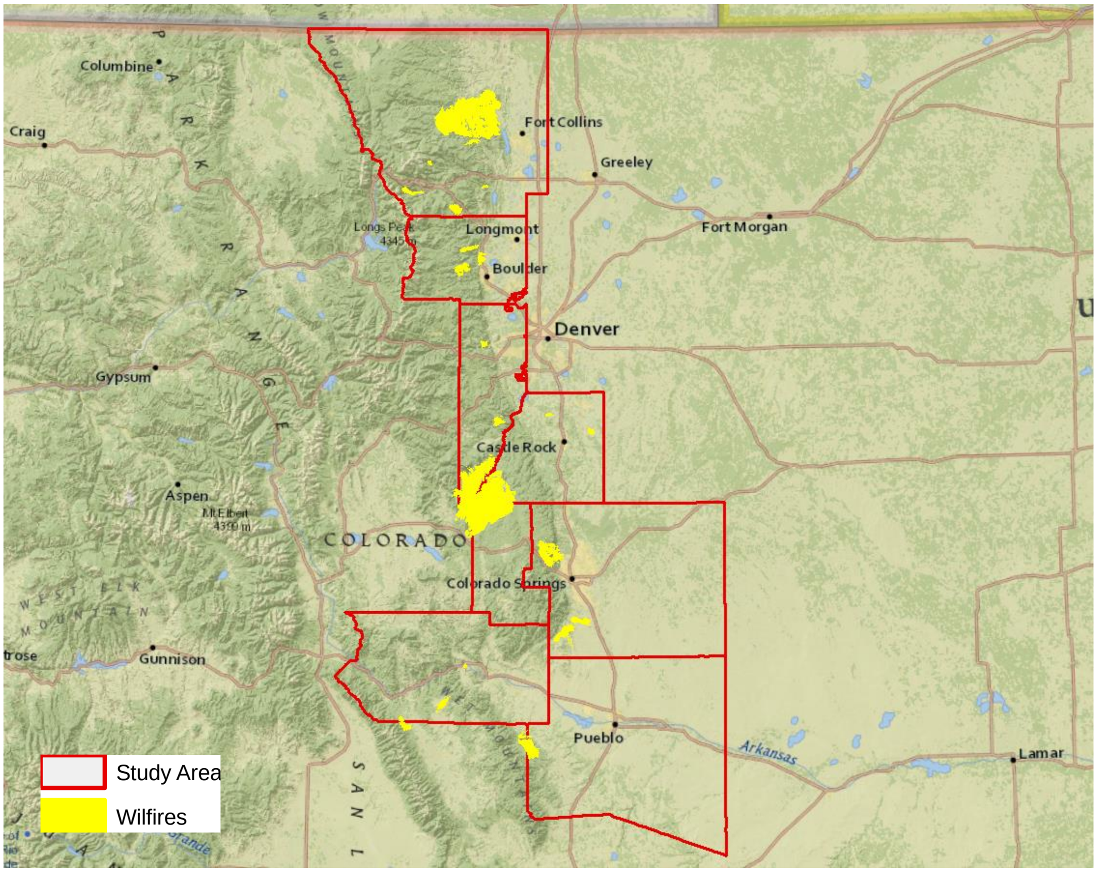


Figure 1.1: Illustration of the Study Area and Wildfire Burn Scars

Coordination Group (GeoMAC)[8] and Monitoring Trends in Burn Severity (MTBS)[9]. We include in our analysis any fire with a burn area exceeding 500 acres which appears in either the GeoMAC or MTBS data-sets. We summarize the set of fires included in our empirical work in Table (1.1)[10]. The spatial distribution of the wildfires in our sample are depicted in Figure (1.1). Their size varies from 606 to 87,505 acres and the costs of suppressing them range from $250 thousand to $38 million.

Our housing transactions data is provided by DataQuick Information Systems, used under a license agreement with the Social Science Research Institute at Duke University. In the 8 counties of interest to our study, we observe repeated transaction histories for 358,823 unique

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

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

[10] Other notable fires which occurred in the COFR but whose burn areas which extend beyond either the spatial or temporal coverage of our housing price data are the Hayman Fire of 2002, the Mason Fire of 2005 and the Wetmore fire of 2012.

Table 1.1: Colorado Wildfires

Fire Name	*Start Date*	*End Date*	*Received FEMA Declaration*	*Acres*	*Structures Lost*	*Suppression Costs*
Big Elk	7/17/2002	7/26/2002	y	4,344	1	3,700,000
Overland	10/29/2003	10/30/2003	y	3,230	62	400,000
Cherokee Ranch	10/29/2003	10/31/2003	y	1,042	3	300,000
Picnic Rock	3/30/2004	4/7/2004	y	9,006	2	2,200,000
Olde Stage	1/7/2009	1/8/2009	y	3,167	3	-
Quarry	3/6/2009	3/9/2009	n	5,137	4	250,000
Parkdale Canyon	6/21/2010	6/25/2010	n	606	4	1,400,000
Cow Creek	6/24/2010	7/3/2010	n	969	0	2,100,000
Reservoir	9/12/2010	9/16/2010	y	778	6	2,000,000
Four Mile Canyon	9/13/2010	9/17/2010	y	5,861	172	9,500,000
Indian Gulch	3/20/2011	3/25/2011	y	1,570	0	2,100,000
Burning Tree	3/24/2011	3/25/2011	n	1,662	0	-
Crystal	4/1/2011	4/11/2011	y	2,937	13	2,800,000
Duckett	6/12/2011	6/24/2011	y	4,610	0	6,600,000
Lower North Fork	3/26/2012	4/2/2012	y	3,218	27	4,400,000
Hewlett	5/14/2012	5/22/2012	n	7,685	0	3,400,000
High Park	6/9/2012	6/30/2012	y	87,505	371	38,400,000
Waldo Canyon	6/23/2012	7/10/2012	y	18,248	347	15,700,000

residential properties between the years 2000 and 2012. The data records information on: type of sale (newly constructed, re-sale, refinance or equity dealings, timeshare, or subdivision sale); transaction-level information including sale price and sale date; building characteristics from the most recent tax assessment including square footage, lot size, number of bedrooms, number of bathrooms and the number of stories; and the site address. In order to obtain precise Geo-referenced locations for each property, we ran a batch geo-coding routine[11] in ArcMap10 which returns the latitude and longitude coordinates for each properties roof-top or parcel-centroid.

We limit transactions to arms length sales of owner occupied, residential single family residences. Properties lying in the 1^{st} or 99^{th} percentile with respect to square footage or sale price, or the 99^{th} percentile with respect to the number stories, baths, beds, units or

[11] The 10.0 North America Geocode Service Locator, updated as of June 2012, was used to generate latitude and longitude coordinates.

rooms were dropped. Houses with a negative age[12] were removed as well.

To determine the portion of the landscape visible from each property in our sample, we perform a Viewshed Analysis[13] in ArcMap10. This method has been used in hedonic models to address the visual impacts of shale gas wells (Muehlenbachs et al. 2014) wind turbines (Sunak and Madlener, 2012), natural landscapes (Walls et al., 2013) and wildfire (Stetler et al., 2010). Given a Digital Elevation Model (DEM) of the terrain which we obtained from the National Map[14], we compute the visible area from each property as determined by the line-of-sight between each observer point and every cell in the DEM. To determine fire-visibility, we overlay and intersect each property's viewshed with each fire's burn scar.

We measure latent wildfire risk with the Wildfire Threat Index (WTI) developed by the Colorado Wildfire Risk Assessment Project (CO-WRAP[15]) which represents the likelihood of a wildfire occurring or burning into an area. (CO-WRAP, 2013). The WTI takes as inputs: surface fuels, canopy characteristics, land cover, terrain, slope, and elevation. The threat index, which ranges from "Lowest Threat" to "Highest Threat", is compiled to a resolution of 30m and allows for consistent comparison of wildfire risk between different parts of the State.

1.5. EMPIRICAL METHODOLOGY

Our basic empirical approach entails hedonic models of residential housing prices and duration models of housing transaction rates estimated along multiple dimensions of potential salience. Contemporaneous shifts in local and macroeconomic housing markets complicate the task of identifying the causal effects of a natural disaster using our housing transac-

[12] We calculate age as year sold minus year built.

[13] To increase the computational speed of this algorithm, we limit the search over the DEM to a radius of 20km of each property.

[14] http://nationalmap.gov/

[15] http://www.coloradowildfirerisk.com/

tion data. To overcome this empirical challenge, we implement a difference-in-differences estimation strategy which identifies treatment groups based upon multiple geo-spatial measures of saliency and compares market dynamics in each group to the outcomes of properties in control groups that do not receive said treatment, but that are otherwise influenced by the same contemporaneous factors. Our treatment groups are *proximity* (treatment equals properties located within a 2km ring of a wildfire, with immediately adjacent properties used as controls), *view* (treatment equals proximate properties with a view of a burn scar, with similarly proximate properties lacking a view used as controls), and *latent risk* (treatment equals location in a high latent risk-zones, with proximate homes located in low latent risk areas used as controls).

As potential drivers of differential risk salience, these three treatment definitions differ along several dimensions. The *proximity* treatment definition is motivated both by its prevalence in the hedonics literature and by the notion that increased proximity likely translates to increased saliency. In terms of identifying the pure saliency effect of a single wildfire event, this treatment definition comes with two caveats. First, immediate proximity to the resultant burn scar will likely be associated with regular experience with the burn scar and thus may serve to reinforce the fire's saliency shock as time goes by. This reinforcing impact could hold for both potential sellers and potential buyers, although it is possible that because they live in the area that it could be more relevant for potential sellers. Second, proximity to the burn scar could be associated with other direct property value impacts such as damaged infrastructure, the loss of proximate recreation opportunities and a long-term reduction in viewshed related amenities. These potential direct effects could serve to mask (and possibly exaggerate) variation in housing mark dynamics that are driven by a pure saliency effect. The *visibility* treatment is similar to the proximity treatment. Further, the reinforcing effect of a burn scar view will likely be even more evident, as will the dis-amenity effect. However, because a potential buyer will also view the burn scar from the property, we expect that it would be less likely for buyers and sellers to experience differential saliency shocks.

The *latent risk* treatment seeks to identify a salience shock that would arise due to an awareness by buyers and sellers of the relative latent risk associated with the topography and land cover of a given location. To the extent that owners who are living in the WUI are more aware of these topography and landcover related risk factors than are potential buyers, who do not typically live in the area, they may be expected to experience a greater saliency shock relative to potential non-resident buyers. Further, by choosing treatment and control parcels that are relatively distant from (and have no view of) the burn scar, this analysis greatly diminishes concerns about the potential for differences between treatment and control parcels in terms of the saliency reinforcing effect or direct effects, dis-amenity and otherwise, associated with proximity and view treatments.. Thus, this treatment is best suited for identifying a pure saliency effect.

To implement our estimation procedure, we assign each property i to its nearest fire $m \in M$. To minimize the potential confounding effects of exposure to multiple fires we drop from our sample any observations that lie within 7 km of multiple fires. For each treatment group, our hedonic models take the form:

$$\begin{aligned} \ln p_{itm} &= \alpha \cdot Post_{itm} + \beta \cdot Treat_{im} \times Post_{itm} + \gamma^m \cdot Treat_{im} \\ &\quad + \delta^m \cdot \tau_t + \pi^m \cdot Treat_{im} \times \tau_t + Z_i' \omega_1 \\ &\quad + G_{it}' \omega_2 + \epsilon_{itm}, \end{aligned} \tag{1.7}$$

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. Moreover, in order to understand how our estimate for β varies in each year following a wildfire, we replace $Post_{itm}$ with 1, 2 and 3-year post-fire indicator variables $\left\{Year_{itm}^{k}\right\}_{k=1}^{3}$. This transforms the baseline specification in (1.7) into:

$$\begin{aligned}
\ln p_{itm} \quad = \quad & \sum_{k=1}^{3}\left(\alpha^{k}\cdot Year_{itm}^{k}+\beta^{k}\cdot Treat_{im}\times Year_{itm}^{k}\right)+\gamma^{m}\cdot Treat_{im} \\
& +\delta^{m}\cdot\tau_{t}+\pi^{m}\cdot Treat_{im}\times\tau_{t}+Z_{i}'\omega_{1} \\
& +G_{it}'\omega_{2}+\epsilon_{itm},
\end{aligned} \tag{1.8}$$

Thus, the estimate of β_k may be interpreted as the difference-in-differences estimate of β restricting attention to post-fire transactions which occur between $k-1$ and k years of a wildfire. To control for composition effects, we allow our main effects to vary by fire by including a full-set of group by fire interaction terms, $\gamma^m \cdot Treat_{im}$. To account for trends in housing prices which may vary over time and space, we include fire-specific trends which can vary by treatment group, $\delta^m \cdot \tau_t + \pi^m \cdot Treat_{im} \times \tau_t$. Our set of structural controls, Z_i', include: second-order polynomials in square footage and age; basement square footage; indicator variables for number of bathrooms and bedrooms; and a variable indicating if a property has a swimming pool. Our set of geographic characteristics, G_i', include second-order polynomials in viewshed size, slope, county fixed effects, year by quarter fixed effects, and, in our most robust specifications, year by quarter by fire fixed effects.

For transaction rates, the probability that a property sells at any given point in time is conditional on whether it sold in the previous period. Moreover, properties which fail to transact in the time-fame of our property data are censored. For these reasons, we model the conditional probability of a property transacting as a continuous time duration process and estimate the relative increase or decrease in the transaction-hazard between each treatment and control group following a wildfire. In addition to our data being censored from the right, which we account for in our maximum likelihood estimation, a second issue is left censoring which occurs whenever ownership of a property initiates prior to the window of our sample. Archer et al. (2010), who estimate a Cox model of ownership duration to understand the effects of household characteristics, neighborhood factors and tenure on housing turnover rates, are also restricted by left-censored data. They argue in their paper, as do we in ours, that to the extent that the window of our transactions data is random, left censoring should not lead to biased estimates.

Letting t denote the elapsed time since property i last sold and $\lambda_0(t)$ the non-parametric baseline hazard function at time t, we estimate the coefficients of:

$$\lambda\left(t|z_i(t)\right) = \lambda_0(t)e^{z_i(t)}. \tag{1.9}$$

where,

$$\begin{aligned} z_i(t) &= \sum_{k=1}^{3}\left(\alpha^k \cdot Year_{itm}^k + \beta^k \cdot Treat_{im} \times Year_{itm}^k\right) + \gamma^m \cdot Treat_{im} \\ &\quad + Z_i'\omega_1 + G_{it}' \cdot \omega_2 + \epsilon_{itm}. \end{aligned} \tag{1.10}$$

In this specification, $\lambda\left(t|z_i(t)\right)$ represents the probability a property turns over at t conditional on its time-varying co-variates $z_i(t)$ and the non-parametric baseline hazard rate $\lambda_0(t)$.

1.6. RESULTS

1.6.1 Visual Evidence and Identification

The difference-in-differences estimates of equation (1.8) will identify the causal effects of wildfire if the average change in housing prices for treated properties would have been proportional to the average change in outcomes for the non-treated in the absence of treatment. In addition, wildfires must not coincide with any other shock differentially affecting each group. We are less concerned with the second of these assumptions since we consider the effects of multiple disasters which occur at different points in time and space; however, since we do not observe counter-factual outcomes, we cannot explicitly test for the first. Instead, we provide graphical evidence that the evolution of prices in the periods immediately preceding wildfire are similar between treated and non-treated properties. After limiting our analysis to the WUI, we regress log-prices on a set of year-by-quarter fixed effects, county fixed effects, and structural control variables. For each treatment definition outlined in Section (1.5), Figure (1.2), Figure (1.3), and Figure (1.4) fit group-specific, kernel-weighted local polynomials on the residuals of these regressions[16].

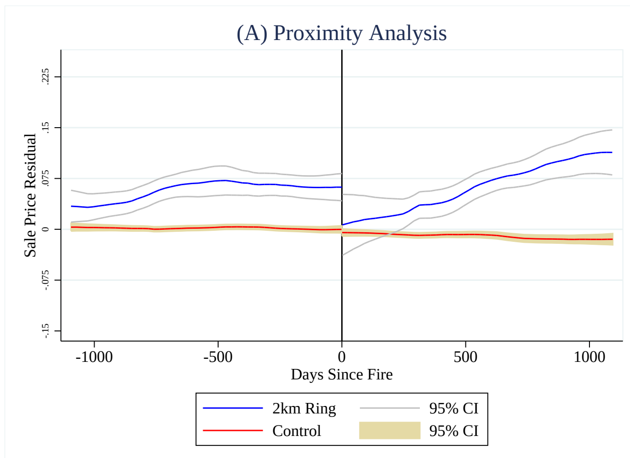


Figure 1.2: Sale Price Residual Plot: Proximity Analysis

In the visibility and risk plots presented in Figure (1.3) and Figure (1.4), the pre-fire trends of each treatment group are generally similar to each control group, but we do detect a slight upward price trend for properties located in 2km wildfire rings which we account for in our empirical analysis by fitting fire-specific trends which which vary by treatment group. The visibility plot suggests that homeowners pay a premium to have a view of fire-prone landscapes prior to a wildfire. The risk plots also suggest a pre-fire premium for properties located in fire-prone areas. These results suggest a positive amenity value for being situated in an area with (or that has a view of) ridge lines, dense vegetation and other determinants of wildfire threat. Donovan et al. (2007) report a similar finding.

[16]These regressions vary with respect to the sample definition for each model. The residual plot for proximity is limited to properties within 10km of a wildfire while the plots for visibility and latent risk are restricted properties within 5km and 30km, respectively. The graphical results using other sample definitions are qualitatively similar.

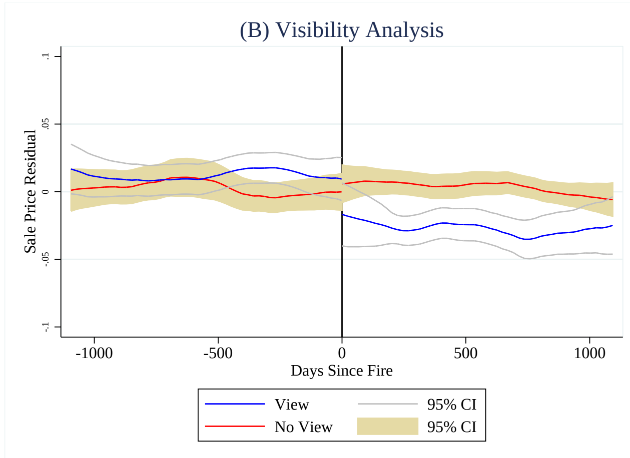


Figure 1.3: Sale Price Residual Plot: Visibility Analysis

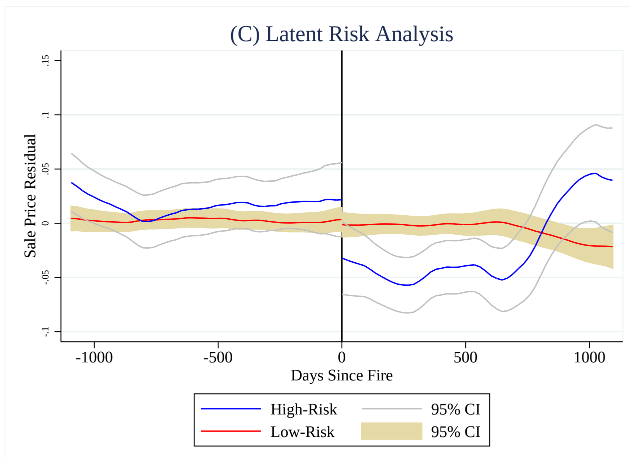


Figure 1.4: Sale Price Residual Plot: Latent Risk Analysis

These graphs also provide visual evidence of the short and long term effects of wildfire on home values. In the years following a wildfire, we observe that each control group continues on their pre-existing trend while each treatment group experiences a sharp drop. Following the initial decline, prices of properties in high latent risk zones decay quickly toward their pre-fire level. Housing values in 2km rings are also initially discounted, but subsequently return to their pre-fire trend. In contrast, properties with a view of a burn scar incur immediate and persistent losses.

1.6.2 Hedonic Property Models

We begin our formal analysis by estimating equation (1.8) along two dimensions*: Proximity* to wildfire and *view* of wildfire burn scars. These treatment measures likely incorporate both saliency effects and direct dis-amenity driven price effects. We determine the extent to which our difference-in-differences estimates diminish towards zero over time along these dimensions. We then estimate our models of latent risk on the spatial extent where the potentially correlated amenity effects of wildfire are less of a concern.

1.6.2.1 Proximity Table (1.2) presents coefficient estimates of equation (1.8) comparing the outcomes of treated properties located within a 2km ring of a wildfire to control properties in the immediately adjacent area. The coefficient estimates in column (1) indicate an immediate and highly significant -8.5% post-fire discount in the first year following a fire. This effect decreases in magnitude towards -7.6% in two years and to approximately -6.7% in year three. As reflected in columns (2) and (3), these results are robust to a smaller set of control properties[17]. Each specification in Table (1.2) also reports the p-value associated with the test: (2km Ring) $\times$ (Year 3) $\leq$ (2km Ring) $\times$ (Year 1). We fail to reject the null hypothesis at conventional levels of significance, however the p-values associated with this test in the two largest samples provide some evidence that the small decrease in magnitude of the first year estimates is not due to statistical error alone.

To test the sensitivity of our model to the cutoff delineating treated and non-treated areas, we limit our sample to properties within 30km of a wildfire burn scar and, starting with a 1km ring, estimate equation (1.8) as we increase the size of the treatment ring in 250m increments. Figure (1.5) plots the first-year coefficient estimates together with their 95% confidence intervals. We take note that the magnitudes of these effects are pronounced and increase into the range of -15% within 1km. Beyond 2km, our coefficient estimates and

[17]These results are also qualitatively similar to Mueller et al. (2009) who finds that house prices located within 1.75 miles of a wildfire drop approximately -9.7% in the year immediately following a fire.

Table 1.2: Difference-in-Differences: Proximity

	(1) ln(price)	(2) ln(price)	(3) ln(price)
Sample Restrictions:	<30km	<20km	<10km
(2km Ring) x (Year 1)	-0.0849***	-0.0855***	-0.0829***
	(0.0238)	(0.0237)	(0.0219)
(2km Ring) x (Year 2)	-0.0759***	-0.0751***	-0.0779***
	(0.0263)	(0.0261)	(0.0249)
(2km Ring) x (Year 3)	-0.0667**	-0.0660**	-0.0677**
	(0.0338)	(0.0335)	(0.0319)
Observations	90,955	87,183	53,904
R-squared	0.730	0.736	0.767
P[(2km Ring x Year 3)>(2km Ring x Year 1)]	0.1725	0.1535	0.2125

Note: Robust standard errors in parentheses. ***p<.01, **p<0.05, *p<0.1. Geographic controls include: Second order polynomials in viewshed size, slope and elevation in addition to county fixed effects, year-quarter-fire fixed effects, and treatment group by fire trends. Structural controls include second order polynomials in square footage and building age as well as basement square footage and indicators for number of bedrooms, number of bathrooms. Models are limited to W.U.I. properties located within 30km of wildfire burn scars which transact within (+/-) 3 years of the fire in their region unless otherwise noted.

our confidence in them rapidly diminish to zero and beyond 5km they are zero.

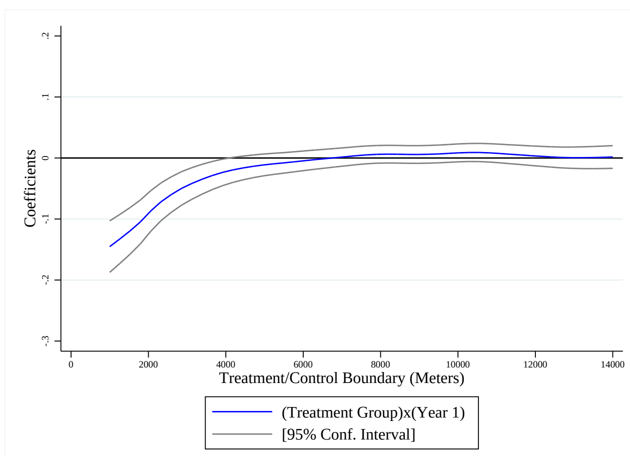


Figure 1.5: Proximity: Sensitivity to Treatment / Control Boundary

1.6.2.2 Visibility Table (1.3) presents the coefficient estimates of equation (1.8) comparing prices between properties with and without a view of a wildfire burn scar. By default, each property's viewshed calculation will extend to the limits of our DEM. Visible areas may include portions of the terrain that are in the observers line-of-sight, but too distant for the observer to be able to discern temporal variations in the landscape. To account for this potential issue, we limit our analysis to properties located within 5km of each fires burn scar. Referring to the coefficient estimates for the view of fire, post-fire interaction terms in columns (1) of Table (1.3), (View of Fire)×(Year k), we find that having a view of a burned area results in a highly significant 4% drop in price immediately following a wildfire. This effect remains unchanged even after three years have passed and, as shown in column (2), robust to second order polynomials with respect to distance to fire fit separately before and after each event. Including these variables reduces our coefficient estimates on view by approximately 1 percentage point in all three years; however, the effects of view still remain persistent over time.

Table 1.3: Difference-in-Differences: Visibility

	(1) *ln(price)*	(2) *ln(price)*
(View of Fire) x (Year 1)	-0.0396**	-0.0313**
	(0.0156)	(0.0148)
(View of Fire) x (Year 2)	-0.0445**	-0.0350*
	(0.0200)	(0.0193)
(View of Fire) x (Year 3)	-0.0473*	-0.0396
	(0.0262)	(0.0251)
Observations	15,911	15,911
R-squared	0.824	0.834
Distance Controls	N	Y

Note: Robust standard errors in parentheses. ***p<.01, **p<0.05, *p<0.1. Geographic controls include: Second order polynomials in viewshed size, slope and elevation in addition in addition to county fixed effects, year-quarter-fire fixed effects, and treatment group by fire trends. Structural controls include second order polynomials in square footage and building age as well as basement square footage and indicators for number of bedrooms, number of bathrooms. Models are limited to W.U.I. properties located within 5km of wildfire burn scars which transact within (+/-) 3 years of the fire in their region unless otherwise noted.

Finally, to test the sensitivity of our model to the 5km cutoff we impose, we re-estimate

Column (1) of Table (1.3) in 250m increments starting with a 1km cutoff and ending with a 14km cutoff. The coefficient estimates for each of these regressions together with their 95% confidence intervals are plotted in Figure (1.6). The figure shows that the effect of view diminishes gradually with distance but remains at approximately -4% between 1km and 5km.

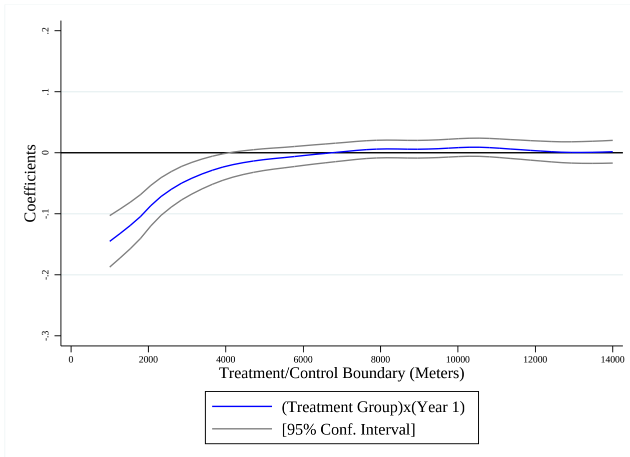


Figure 1.6: Visibility: Sensitivity to Sample Definition

1.6.2.3 Latent Wildfire Risk The price adjustments with respect to proximity and visibility are only weak evidence that households update risk perceptions following a natural disaster; these estimates may be conflated with the dis-amenity effects of fire. To more cleanly identify saliency effects, we estimate the wildfire impact on price differentials between properties in high and low risk areas that are not immediately proximate to a fire (i.e $>$ 5km) and that do not have a view of a wildfire burn scar. Here, one might expect wildfire exposure to lead to an increased level of attention on wildfire risk in the context of those properties whose landscapes make them particularly prone to wildfire. We report the estimation results of the latent risk interactions, which are also based on equation (1.8), in Table (1.4). The coefficients of interest are the estimates of the latent risk, post-fire interaction terms, (High Latent Risk)$\times$(Year k).

Column (1) in Table (1.4) presents model estimates based on properties located at a

Table 1.4: Difference-in-Differences: Latent Risk

	(1)	(2)	(3)
	ln(price)	*ln(price)*	*ln(price)*
Sample Restrictions:	*<30km*	*<20km*	*<10km*
(High Latent Risk) x (Year 1)	-0.0716**	-0.0917**	-0.0878**
	(0.0320)	(0.0357)	(0.0430)
(High Latent Risk) x (Year 2)	-0.0604	-0.0422	-0.0359
	(0.0439)	(0.0493)	(0.0585)
(High Latent Risk) x (Year 3)	0.0155	0.0384	-0.0123
	(0.0757)	(0.0854)	(0.0886)
Observations	15,712	12,733	6,987
R-squared	0.693	0.692	0.655
P[(H.L.R. Year 3) < (H.L.R. Year 1)]	0.09285	0.0409	0.1546

Note: Robust standard errors in parentheses. ***p<.01, **p<0.05, *p<0.1. Geographic controls include: Second order polynomials in viewshed size, slope and elevation in addition to county fixed effects, year-quarter-fire fixed effects, and treatment group by fire trends. Structural controls include second order polynomials in square footage and building age as well as basement square footage and indicators for number of bedrooms, number of bathrooms. Models are limited to W.U.I. properties located within 30km of wildfire burn scars which transact within (+/-) 3 years of the fire in their region unless otherwise noted. Models exclude properties located within 5km or that have a view of a wildfire.

burn scar distance between 5km and 30 km and without a view of a wildfire burn scar. Columns (2) and (3) utilize more restrictive sample – limiting the outer boundary of the sample to 20km and 10km, respectively. Referring to Column (1), we find we find a -7.2% latent risk discount in the year immediately following a wildfire. This effect is significant at the 5% level and equates to an approximately $25,000 discount for an average-priced home in our sample. The magnitude of the first-year effect remains significant and increases to approximately -9% when we limit our sample cutoff to 10-20km. In each model, coefficients decrease in magnitude towards zero and become insignificant in the second year. To evaluate the statistical robustness of this finding, each specification in Table (1.4) also reports the p-value associated with the test: (High Latent Risk) $\times$ (Year 3) $\leq$ (High Latent Risk) $\times$ (Year 1). In two of the three specifications we reject the null hypothesis at either the 10% or 5% level. When we substantially shrink the sample, the p-value for the test is .15%.

1.6.3 Duration Analysis

The conceptual model presented in section (1.3) allows us to draw inferences regarding the saliency dynamics of wildfire by investigating the evolution of prices *and* quantities. We now turn to the quantity side of the market. As discussed above, we analyze transaction rates using a proportional hazards model. In what follows, we report hazard ratios corresponding to each of our three treatment definitions with p-values – relative to a no-effect level of one – reported in brackets.

Results for *proximity* are reported in Table (1.5). The sample definitions in columns (1) - (3) correspond to the samples used in columns (1) - (3) of Table (1.2). In each specification, the estimated hazard ratios for (2km Ring)×(Year 1) and (2km Ring)×(Year 2) are insignificant despite corresponding to years which experienced negative price shocks. However, as shown in columns (1) and (2) we find that a statistically significant 21% increase in transaction probability occurs in year three; when we shrink the sample, this effect decreases in magnitude to approximately 13%.

Our theoretical analysis (Observation 3) predicts that prices fall with quantities remaining unchanged when sellers and buyers both experience the same shift in risk perceptions. Through this lens, our empirical results suggest that the spatial effects of wildfire – which may ultimately incorporate amenity changes – are equally salient to both extant residents in close proximity to a wildfire *and* to potential buyers; but only in the first two years. The theoretical model (Observation 4) predicts price decreases in association with transaction rate increases when negative saliency shocks are greater for those located in the treated area. Thus, the subsequent increase in housing transaction probabilities in the third year provides evidence that the spatial effects of fire become relatively less pronounced for individuals not living in immediate proximity of the fire after two years have elapsed.

Turning to the effect of *view* as presented in Table (1.6), in contrast with the results for proximity, we find no evidence of an uptick in transaction rates. The estimated hazard

Table 1.5: Duration Analysis: Proximity

	Cox Model: Hazard Ratios		
Sample Restrictions	(1) *<30km*	(2) *<20km*	(3) *<10km*
(2km Ring) x (Year 1)	1.053	1.042	1.026
	[0.442]	[0.539]	[0.711]
(2km Ring) x (Year 2)	1.107	1.107	1.06
	[0.226]	[0.226]	[0.495]
(2km Ring) x (Year 3)	1.215**	1.207*	1.136
	[0.046]	[0.055]	[0.197]
Observations	598,956	580,901	378,485
P[(2km Ring x Year 3)>(2km Ring x Year 1)]	0.085	0.0805	0.1685

Note: P-values are reported in brackets, ***p<.01, **p<0.05, *p<0.1. Geographic controls include: Second order polynomials in viewshed size, slope and elevation in addition to year-quarter-fire, county, and fire by treatment group fixed effects. Structural controls include second order polynomials in square footage as well as basement square footage and indicators for number of bedrooms, number of bathrooms. Models are limited to W.U.I. properties located within 30km of wildfire burn scars unless otherwise noted.

ratios (View of Fire)×(Year k) are insignificant in all years with point estimates less than one. In addition, recall that we find persistent price effects over this time frame. From the perspective of the model, this finding suggests that the visual-effects of wildfire are as relevant for the average buyer as they are for the average owner, even after three years have passed. The contrast between the results for *proximity* and *view* could arise because a property's burn scar view acts as a continuous saliency shock for both buyers *and* sellers.

As discussed above, a confounding issue that may partially explain the failure of prices to return quickly to their pre-fire levels is the dis-amenity associated with close proximity to, or view of, a burn scar. To ameliorate this concern and isolate a pure salience effect, we consider the relative impact of fire on transaction rates for high-risk and low-risk properties located between 5 and 30 km of a wildfire without a fire in their viewshed. To the extent that dis-amenity effects exist, both control and treatment groups in this final set of analyses should experience identical amenity impacts.

Table (1.7) presents transaction rate results for high latent risk properties relative to

Table 1.6: Duration Analysis: Visibility

	Cox Model: Hazard Ratios	
	(1)	(2)
(View of Fire) x (Year 1)	0.896	0.883
	[0.159]	[0.116]
(View of Fire) x (Year 2)	0.882	0.882
	[0.147]	[0.151]
(View of Fire) x (Year 3)	0.924	0.914
	[0.421]	[0.368]
Observations	97,916	97,916
Distance Controls	N	Y

Note: P-values are reported in brackets, ***p<.01, **p<0.05, *p<0.1. Geographic controls include: Second order polynomials in viewshed size, slope and elevation in addition to year-quarter-fire, county, and fire by treatment group fixed effects. Structural controls include second order polynomials in square footage as well as basement square footage and indicators for number of bedrooms, number of bathrooms. Models are limited to W.U.I. properties located within 5km of wildfire burn scars unless otherwise noted.

low latent risk properties. The sample definitions in columns (1) - (3) correspond to the samples used in columns (1) - (3) of Table (1.4). Referring to column (1), the estimated hazard ratio for (High Latent Risk)×(Year 1) shows a statistically significant, 19% increase in transaction probabilities in the first year following a wildfire. When we shrink the sample size in columns (2) and (3), first year estimates remain positive in the range of 14% - 24% however the coefficient estimates are insignificant with p-values of .24 & .19, respectively. P-values associated with the test: (High Latent Risk) $\times$ (Year 3) $\leq$ (High Latent Risk) $\times$ (Year 1) are .064 for the full sample, increasing to .15 and .11 when we shrink the sample in columns (2) and (3). Collectively, these findings are suggestive of a short term increase in transaction rates.

Given that we detect no measurable effects on prices after the first year, this short-run price decline which is associated with a similarly short-lived increase in transaction rates suggests that, following a wildfire, potential sellers located on high risk lots that are in the general area of the fire, but not so close as to be directly affected by the fire, experience an

Table 1.7: Duration Analysis: Latent Risk

	Cox Model: Hazard Ratios		
Sample Restrictions	(1) *<30km*	(2) *<20km*	(3) *<10km*
(High Latent Risk) x (Year 1)	1.19*	1.139	1.235
	[0.071]	[0.238]	[0.186]
(High Latent Risk) x (Year 2)	1.088	1.0004	0.899
	[0.521]	[0.998]	[0.605]
(High Latent Risk) x (Year 3)	0.884	0.91	0.877
	[0.517]	[0.657]	[0.633]
Observations	280,739	263,227	151,332
P[(H.L.R. Year 3) < (H.L.R. Year 1)]	0.0635	0.1525	0.109

Note: P-values are reported in brackets, ***p<.01, **p<0.05, *p<0.1. Geographic controls include: Second order polynomials in viewshed size, slope and elevation in addition to year-quarter-fire, county, and fire by treatment group fixed effects. Structural controls include second order polynomials in square footage as well as basement square footage and indicators for number of bedrooms, number of bathrooms. Models are limited to W.U.I. properties located within 30km of wildfire burn scars unless otherwise noted.

increase in their perception of the fire risk associated with their house which exceeds that experienced by potential buyers. Without immediate proximity to, or view of, the burn scar to reinforce this shock, the increase in fire saliency attenuates quickly.

1.6.3.1 Testing for Composition Effects. Finally, one potential concern is that our results are driven by changes in the composition of houses that go on the market following a fire. To test for this possibility, we compare the mean characteristics of houses sold in each treatment and control region pre and post fire. For parsimony, we report comparisons along a single dimension quantity-index constructed for each property based on a linear combination of its structural characteristics[18]. We construct weights for the quantity-index (Q_i) using the coefficients from a single *pre-fire* regression of logged prices on the full suite of structural characteristics.

[18] There are no qualitative differences in the results of the composition analysis when implemented across individual structural characteristics.

In Table (1.8) we report tests for differences in pre and post-treatment Quantity Index means. In rows one, two, and three we compute the difference of the mean quality-adjusted index of properties that sell one, two, and three years after a fire and the mean index of properties that sell before a fire restricting attention to treated parcels. In Column (1), we evaluate this difference for properties located within 2km of a wildfire burn scar while in Columns (2) and (3) we consider properties with a view of a wildfire burn scar and those located in a wildfire risk area, respectively; P-values of differences are reported in brackets. Rows four, five, and six report mean differences across time for each corresponding control group. These results provide no evidence to suggest that the composition of residential units that transact after a fire systematically differs from the composition of properties that transact before a fire.

1.7. DISCUSSION AND SUMMARY OF FINDINGS

In this paper we develop a parsimonious model that links underlying changes in location-specific risk perceptions to housing market dynamics. In particular, given estimates of both the price and quantity effects associated with significant natural disasters the model allows us to draw inferences about the underlying changes in risk perceptions that gave rise to the observed housing market impacts. This approach is an advance over the existing literature which has focused almost exclusively on the price effects of natural disasters and is thus limited in terms of the inferences it can draw regarding the impact of these events on underlying risk perceptions.

In our empirical work, by considering several different dimensions along which the saliency effects of wildfire may vary, we are able to more clearly identify a pure saliency effect. Our results suggest that, for properties located very close to a significant wildfire, both potential buyers and sellers experienced increases in the perceived fire risks associated with these

Table 1.8: Testing for Composition Effects

	Proximity	Visibility	Latent Risk
$\bar{Q}_{Treat,\,Year\,1} - \bar{Q}_{Treat,\,Pre-Fire}$	0.002	-0.0213	-0.0313
	[0.91]	[0.39]	[0.41]
$\bar{Q}_{Treat,\,Year\,2} - \bar{Q}_{Treat,\,Pre-Fire}$	0.0017	-0.0274	-0.0544
	[0.94]	[0.43]	[0.33]
$\bar{Q}_{Treat,\,Year\,3} - \bar{Q}_{Treat,\,Pre-Fire}$	-0.0206	-0.0564	-0.0384
	[0.49]	[0.19]	[0.6]
$\bar{Q}_{Control,\,Year\,1} - \bar{Q}_{Control,\,Pre-Fire}$	-0.0146	0.0255	-0.016
	[0.22]	[0.28]	[0.63]
$\bar{Q}_{Control,\,Year\,2} - \bar{Q}_{Control,\,Pre-Fire}$	-0.0364**	0.0146	-0.0459
	[0.04]	[0.67]	[0.36]
$\bar{Q}_{Control,\,Year\,3} - \bar{Q}_{Control,\,Pre-Fire}$	-0.034	-0.0025	-0.0184
	[0.14]	[0.95]	[0.78]

P-values reported in brackets. ***p<.01, **p<0.05, *p<0.1. Differences of means reported in Columns (1), (2), and (3) are based on the sample of housing transactions included in model estimates reported in Columns (1) of Tables (2) - (4).

locations. For close locations with no view of a burn scar we find suggestive evidence that after two years have elapsed, these heightened risk perceptions attenuate for potential buyers – relative to those of potential sellers. However, for locations with a burn scar view, no such relative attenuation in risk perception occurs, even 3 years out. These findings suggest that proximity to, and view of, a burn scar may serve to reinforce an initial saliency shock. Of course, in these first two cases, we also can't rule out the possibility that agents are responding to a dis-amenity effect as well.

Finally, by focusing on differences in housing dynamics that are driven by variation in a given location's underlying latent fire risk, we are able to identify a much purer saliency effect. Here our empirical results suggest that potential sellers in high risk locations experience an increase in perceived risk that is not shared by potential buyers. This short-lived (one to two year) increase in relative risk saliency experienced by households living in the general vicinity of, but not immediately proximate to, a wildfire suggests that households in high risk areas may be particularly sensitive to information shocks about fire risk.

These results provide insight into the potential for information treatments to impact risk salience and market behavior in the context of natural hazards. Our analysis suggests that households update their risk beliefs and market behavior in response to disaster-driven information shocks – with households living in high risk areas being more responsive than those in low risk areas. However, the impact of these information treatments may be short lived. For the Colorado wildfires considered in our study, saliency effects appear to attenuate over the course of one or two years in locations that aren't located in immediate proximity to a burn scar.

Our finding that disasters may heighten risk perceptions which, in the absence of recurring events, diminish quickly to their pre-disaster levels, lends credence to the insights set forth by Tversky and Kahnemann's *Availability Heuristic.*(Tversky and Kahneman, 1974). Unexplored in this study, and a fruitful avenue for future work, is the feedback loop between the cognitive factors that influence the temporal dynamics of agents beliefs and the decisions agents ultimately have to make in an uncertain environment.

2. RISK, SALIENCE, AND INVESTMENT IN RESIDENTIAL HOUSING

(with Xiaoxi Zhao)

2.1. INTRODUCTION

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 six-fold from 344 to 2,444 per year and now cause roughly $6 billion in property damage annually. (Brody et al., 2007; USGS, 2006).

Many attribute the increasing trend in disaster frequency to changes in global climates and the rapid increase in their economic costs to increases 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

[1]Natural disasters: Counting the cost of calamities. *The Economist*, (2012). http://www.economist.com/-node/21542755.

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 from a disaster?" To answer this question, we investigate households' decisions to invest in homes damaged by Hurricane Sandy in 2012 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 and the complete history of alterations made to each structure registered with NYC's Department of Buildings. We subsequently link this data to information provided by FEMA which allows us to identify the locations of properties damaged by the storm.

In addition to considering post-disaster remedial investment, we also consider factors influencing new investment in the face of risk. In particular, we ask, "To what extent do natural disasters heighten 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 to develop housing in disaster-prone areas or not. For these reasons, *risk-salience* has been the focal point of many natural and man-made hazard 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 households risk-perceptions 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.

[2]Link: http://www.conservation.ca.gov/cgs/shzp/Pages/shmprealdis.aspx. Also, see Troy and Romm (2004) for a background of this policy.

[3]http://www2.epa.gov/lead/lead-residential-lead-based-paint-disclosure-program-section-1018-title-x

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 housing investment projects 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 ultimately 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 subsequently draw inferences regarding how the information effects of the storm change a household's evaluation of property-specific risk. We estimate saliency changes by investigating relative investment decisions before and after the hurricane between properties in and out of SFHAs (omitting any property that experienced physical damage from the storm from our analysis). The spatial distribution as well as the overall magnitude of storm damage may send a stronger signal to households regarding the likelihood of a disaster occurring in the future. To determine the extent to which this is true, we estimate how relative investment decisions between the set of SFHA and non-SFHA properties vary with respect to proximity to damaged structures. To the best of our knowledge, our paper is the first to consider the factors influencing micro-level housing investment decisions in the context of natural disasters, storm damage, and changing beliefs over the likelihood of property specific risk.

One component largely unexplored in previous works that we consider here is the role of homeowners 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 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, 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). This variation allows us to examine the role insurance plays in promoting post-disaster home re-investment by comparing the rates at which homeowners invest in damaged structures in and out of special flood hazard areas.

To preview our empirical results, we find a significant increase in the probability of investment by households living in the SFHA 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. These findings show that flood-insurance plays a significant role in facilitating post-disaster re-investment. Although, if it were economically efficient to invest in a damaged building outside the SFHA, we should expect some increase in investment rates in these homes, but we do not. Moreover, 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 evidence suggests we cannot rule out the possibility that these regulations have the unintended effect of promoting economically inefficient investment projects in disaster prone regions.

When restricting attention to the effect of the storm on non-damaged buildings in the SFHA, we find a significant *decrease* in the probability of housing investment relative to the set of non-damaged homes outside of the SFHA. This result suggests that a recent disaster may heighten households' perceptions of disaster risk. However, this result is *conditional* on households living in close proximity to a damaged building. We find no evidence that owners of properties less-proximate to storm damage reduce the rate at which they invest. Additionally, we find no evidence that this finding is driven by local spillover or dis-amenity effects which may decay through space. These results effectively show that exposure to storm damage is one of the key mechanisms driving changes in risk-perceptions. However, there exists other sources of information to households regarding the relative risk of living in a disaster-prone area that are not correlated with homeowners' proximity to storm damage including, for instance, information garnered from increased media coverage. To the extent that homeowners in less-proximate regions of storm damage fail to modify their behaviors, our results show that households fail to react to these alternative sources of information. As a result, our findings cast doubt on the ability of an information-based regulation to align risk-perceptions with risk-actualities.

We begin by providing an overview of related work in Section (2.2). We characterize our study area and the details behind the construction of our data in Section (2.3). We present our empirical methodology in Section (2.4) and our findings in Section (2.5). We summarize and conclude in Section (2.6).

2.2. BACKGROUND

Residential housing investment in existing homes is considered a relatively efficient means to improving housing standards. It is also considered 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 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, very little research exists which considers the factors that influence 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 link between housing investment decisions, natural disasters, and disaster risk-saliency. However, there is a growing body of work that studies changes in risk-perceptions due to natural disasters by analyzing housing price dynamics across designated disaster risk areas. 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 appears to last for 5 to 6 years. Atreya et al. (2012) 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.

With the exception of Kousky (2010), these papers provide some evidence that natural disasters may heighten households' perceptions of risk. However, without being able to distinguish damaged and non-damaged buildings, these papers are limited in their ability to dis-entangle the effects a disaster on the salience of disaster risk from price effects caused by structural damage to homes. McCoy and Walsh (2014) circumvent this difficulty using geo-spatial 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 between properties in and out of disaster risk zones. 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 temporarily heighten agents' perceptions of risk which decay to their pre-disaster levels after 1-2 years. 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 which also suggests that home buyers and sellers act on the information conveyed by a natural disaster.

Building off the work of Atreya et al. (2012) and Bin and Landry (2013), Atreya and

Ferreira (2014) estimate 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 advance over previous works in that the authors utilize flood inundation maps; however, the authors note that their maps represent modeled flood heights based on digital elevation models calibrated to high water marks that were available at the time of creation. These authors detect a significant, 48% post-storm price discount among inundated properties in SFHAs relative to non-inundated properties outside of the floodplain; a result that is consistent with the earlier work of Atreya et al. (2012) and Bin and Landry (2013). However, they find no statistical change in the price of *non-inundated* homes in SFHAs relative to non-inundated properties outside of the floodplain.

With the exception of McCoy and Walsh (2014), the aforementioned studies focus exclusively on home price dynamics. 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).

We make a number of significant advances over the extant literature. First, we utilize fine-scaled geographic data delineating the set of structures damaged by a hurricane. These data allow us to draw inferences regarding changing risk-perceptions effectively controlling for dis-amenity confounds. Second, we are the first to estimate the extent to which changing risk-perceptions are driven by the spatial distribution of storm damage. Third, we are the first to study risk-saliency following a natural disaster using data indicating both the timing and the location of residential property investment projects. Fourth, we are the first to address the extent to which homeowners re-invest in homes damaged by a natural disaster. In addition, by exploiting variation in flood-insurance take-up rates across statutorily designated flood-risk zones, we provide new insight into the role insurance plays in facilitating these decisions.

2.3. 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 residential properties in and out of the SFHA by overlaying a map of the SFHA boundary[5] with polygons delineating the lots of residential structures obtained from NYCs Department of Planning[6]. We utilize the most recent flood hazard maps that became effective September 5, 2007[7]. We illustrate the study area and the extent of the SFHA in Figure (2.1).

Information detailing the structural characteristics of each home were acquired from NYCs Department of Finance Final Assessment Roll[8]. These data include information on the year each structure was built, the dimensions of each properties lot, gross square footage,

[4]NYC Press Release PR -443-12: http://www.nyc.gov/portal/site/nycgov/menu-item.c0935b9a57bb4ef3daf2f-1c701c7-89a0/index.jsp?pageID=mayor_press_release-&catID=1194&doc_name=http://www.nyc.gov/html/om/html/2012b/pr443-12.html&cc=unused1978&rc=1194&ndi=1. (November 26, 2012).

[5]Digital maps of the SFHA were obtained from the National Flood Hazard Layer: https://www.fema.gov/national-flood-insurance-program-flood-hazard-mapping/national-flood-hazard-layer-nfhl. Structures located in the 500-year floodplain were omitted.

[6]NYC Department of Planning: http://www.nyc.gov/html/dcp/html/bytes/applbyte.shtml

[7]http://www.nyc.gov/html/dob/html/codes_and_reference_materials/code_internet.shtml

[8]NYC Dept. of Finance Assessment Archives: http://www1.nyc.gov/site/finance/taxes/property-assessments.page#roll.

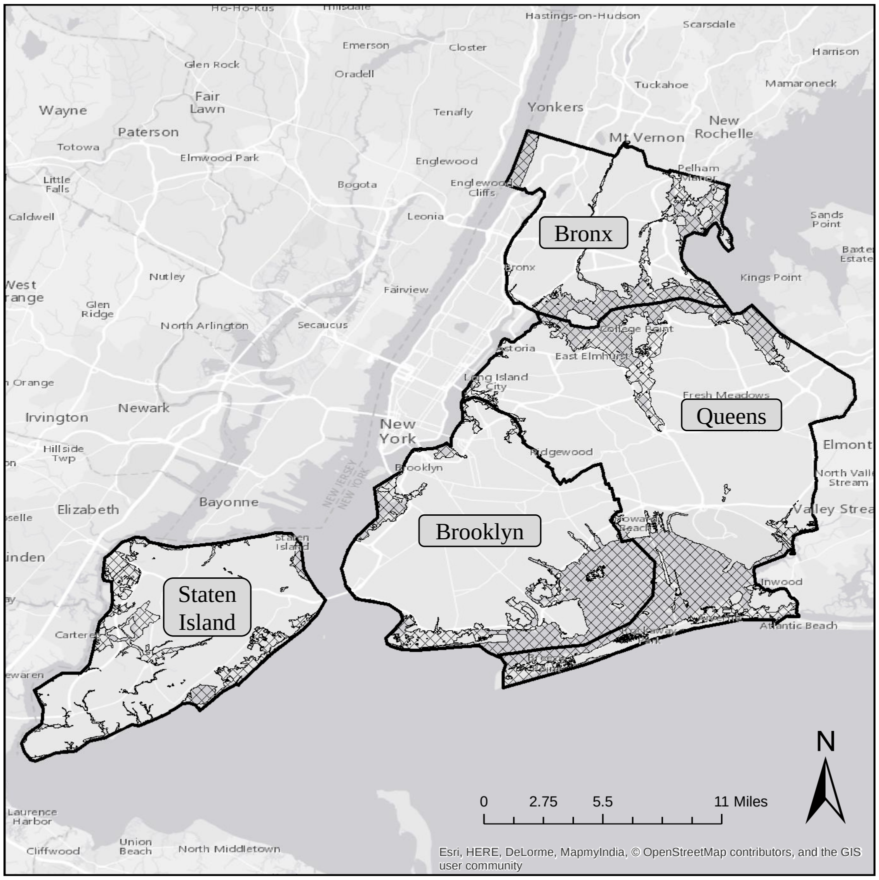


Figure 2.1: Illustration of the study area with the extent of the SFHA shown in crosshatch.

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 our attention to residential, 1-3 family residences[9].

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

[9]Property Tax Class - 1.

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 for the universe of structures damaged by Hurricane Sandy. As shown in Figure (2.2), we adjoined FEMAs MOTF data to the footprints[10] of buildings in our sample in order to determine the set of properties that were and 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 (2.3). These illustrations show that while the locations of damaged structures cluster near the floodplain, a substantial amount of property damage occurred to properties located immediately outside of these zones.

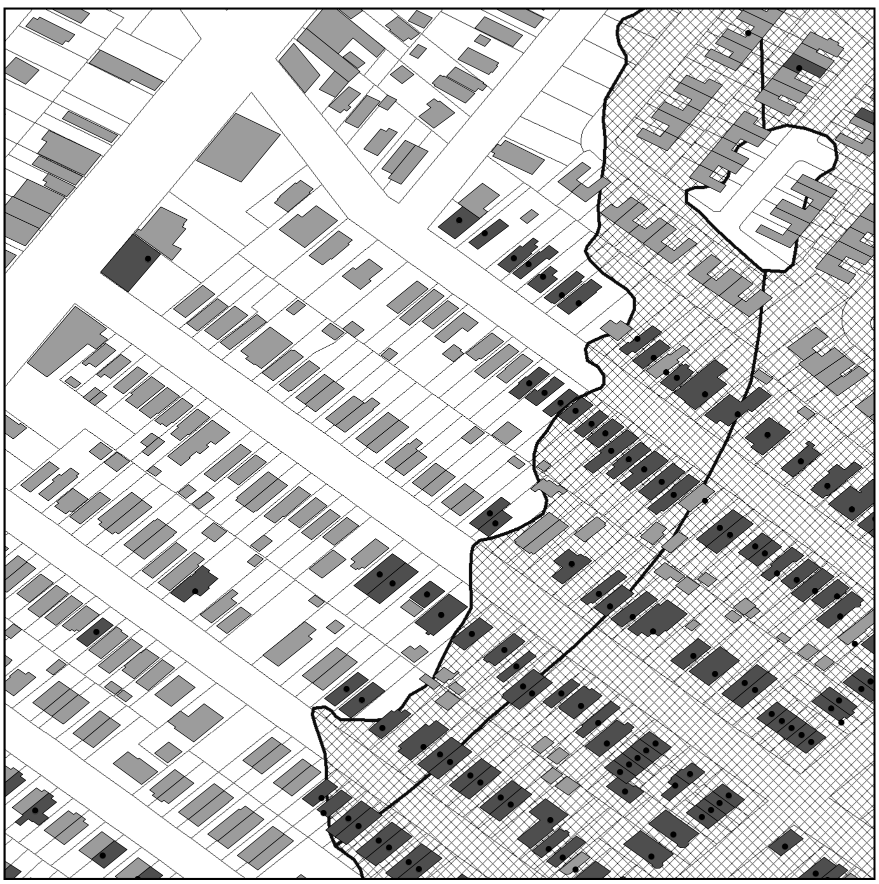
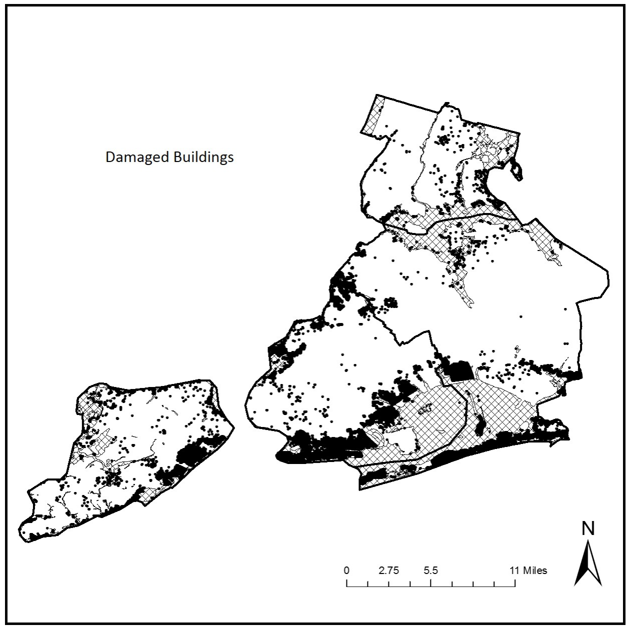
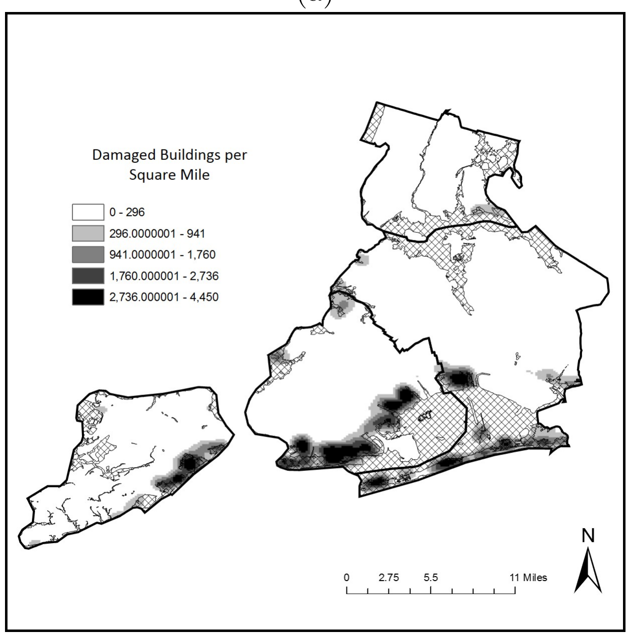


Figure 2.2: Illustration of residential structures, the floodplain, and flood damage. Dark gray indicates the footprints of damaged buildings. Light grey indicates the footprints of non-damaged buildings. The extent of the SFHA is shown in crosshatch.

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



(a)



(b)

Figure 2.3: Damaged structures with the extent of the SFHA shown in crosshatch.

In this paper we are interested in explaining household level decisions to repair, renovate or invest in homes. To this effect, we construct a micro-level dataset of housing modifications 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[11] which contain logs of housing investment projects filed by property owners residing in NYC. These include information on the filing date, job type, and a brief description of the alteration. We restrict attention to housing investment projects classified as Alteration Type-II which are those that do not change the use or occupancy of a building. Among others, 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, roof repairs and replacements, fall into this category. Over our sampled time frame (two years before and after the Hurricane[12]), we record 23,362 housing investments projects. The locations and density of these investments are illustrated in panels (a) and (b) of Figure (**??**).

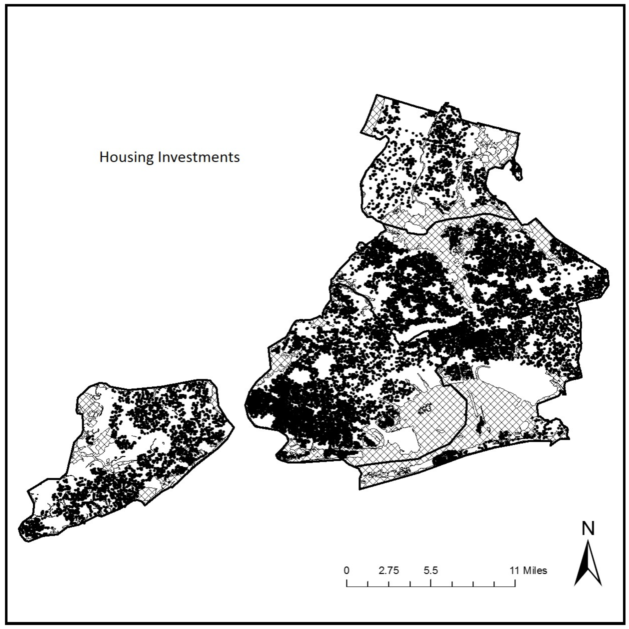
2.4. METHODS

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

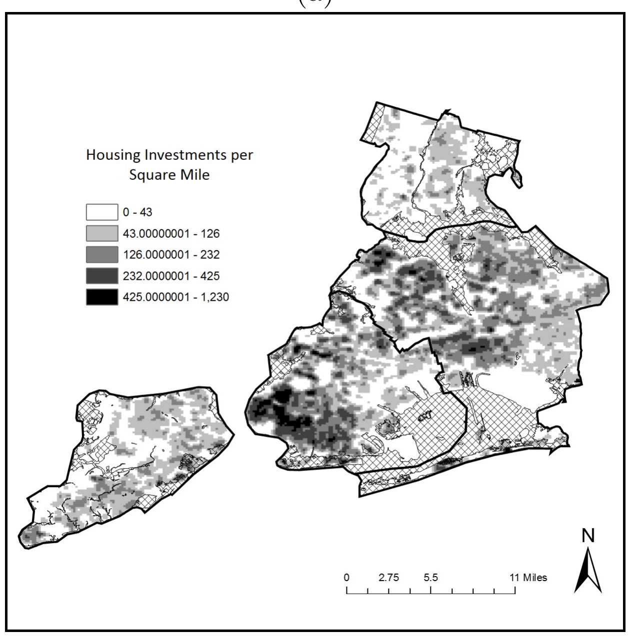
To implement our empirical models, we construct a balanced panel for each property in

[11] http://www.nyc.gov/html/dob/html/home/home.shtml.

[12] 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.



(a)



(b)

Figure 2.4: Property investments with the extent of the SFHA shown in crosshatch.

our assessment records using a year-quarter time increment. For each treatment definition, our logistic models take the form:

$$q_{it} = \Lambda\left(\alpha_1 \cdot Post_{it} + \alpha_2 \cdot Treat_i + \alpha_3 \cdot Treat_i \times Post_{it} + Z_{it}'\omega + \epsilon_{it}\right), \qquad (2.1)$$

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} includes 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. The functional form for these estimates is:

$$\begin{aligned}\tau\left(Treat \times Post\right) \quad = \quad & \Lambda\left(Post_{it} = 1,\ Treat_i = 1,\ Z'_{it}\omega\right) \\ & -\Lambda\left(Post_{it} = 0,\ Treat_i = 1,\ Z'_{it}\omega\right) \\ & -\Lambda\left(Post_{it} = 1,\ Treat_i = 0,\ Z'_{it}\omega\right) \\ & +\Lambda\left(Post_{it} = 0,\ Treat_i = 0,\ Z'_{it}\omega\right), \end{aligned} \tag{2.2}$$

which is equivalent to,

$$\begin{aligned}\tau\left(Treat \times Post\right) \quad = \quad & \left[\Lambda\left(\alpha_1 + \alpha_2 + \alpha_3 + Z'_{it}\omega\right) - \Lambda\left(\alpha_2 + Z'_{it}\omega\right)\right] \\ & - \left[\Lambda\left(\alpha_1 + Z'_{it}\omega\right) - \Lambda\left(Z'_{it}\omega\right)\right]. \end{aligned} \tag{2.3}$$

2.4.1 Treatment Definitions

2.4.1.1 Storm Damage We investigate the degree to which homeowners re-invest in damaged buildings by comparing investment outcomes between properties damaged by the hurricane to properties that were not. To understand how restoration patterns differ between households in and out of SFHAs, we partition the set of (treated) 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 (2.1) 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.

2.4.1.2 Risk Salience Properties located within and directly proximate to SFHAs are *both* vulnerable to hurricane damage; however, owners of homes in non-SFHAs are disadvantaged in the sense that it is more difficult for them to asses their risk level. State and Federal laws, for instance, require sellers to disclose whether their property is located in an SFHA. Community flood maps are also available online and required to be displayed publicly. We use Hurricane Sandy as an exogenous shock to agents' beliefs over the relative risk of living in a disaster prone area. To infer these changes in risk-saliency, we compare investment rates between properties in and out of SFHAs before and after the storm.

In order to isolate the saliency effects of the storm 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 effects of storm damage on property investment; however, as emphasized by McCoy and Walsh (2014), this specification does not 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 from 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 difference-in-differences approach mitigates the concern for potential bias due to dis-amenity effects. To this effect, 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 subsequently estimate equation (2.1) restricting attention to properties located between 0ft - 250ft of a damaged structure.

2.5. RESULTS

2.5.1 Visual Evidence and Identification

In order for the difference-in-differences estimates obtained from equation (2.1) to represent the the causal effects of the Hurricane on investment, 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 the number of property investments at 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 (2.4.1), Figures (2.5), (2.6), and (2.7) show group-specific, kernel-weighted local polynomial trends in the number housing investments before and after the Hurricane controlling for quarter 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 (2.5) shows that the trend in the average number of housing investments by property owners in the SFHA leading up to the hurricane is generally similar to the average

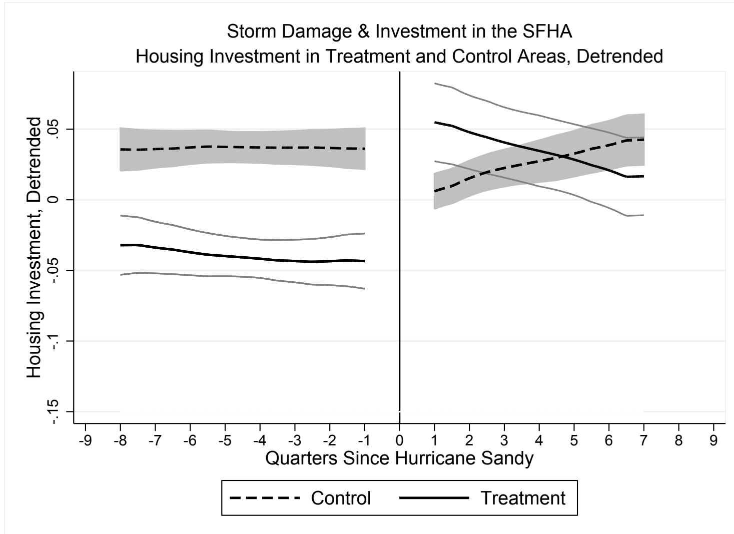


Figure 2.5: Trend Analysis: Treatment Definition - $Damaged_{SFHA}$

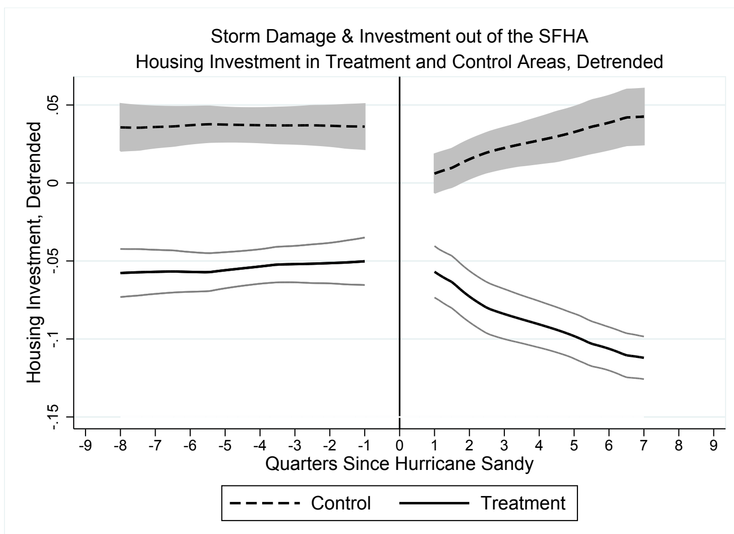


Figure 2.6: Trend Analysis: Treatment Definition - $Damaged_{NonSFHA}$

number investments made by households located outside of the SFHA. After the hurricane, we observe a systematic increase in property investment in damaged, SFHA homes. We observe a small initial down-turn in the number of investments in non-damaged homes outside of the SFHA which subsequently rebounds to its pre-hurricane level. Figure (2.6) provides graphical evidence that the trend in the number of investments in damaged homes located outside of the SFHA is also parallel to the trend in the amount of investment in similar homes that failed to experience any damage from the storm. However, we find no evidence of a relative increase in the number of investments made in these properties. Turning our attention to Figure (2.7), before to the hurricane, the trend in the overall level of property investment in non-damaged homes in the SFHA located within 250 feet of a damaged structure is similar to the trend of investment in homes within the same vicinity of a damaged building, but located directly outside of the SFHA. After the storm, we observe an immediate and persistent decrease in the number of investments made in treated properties.

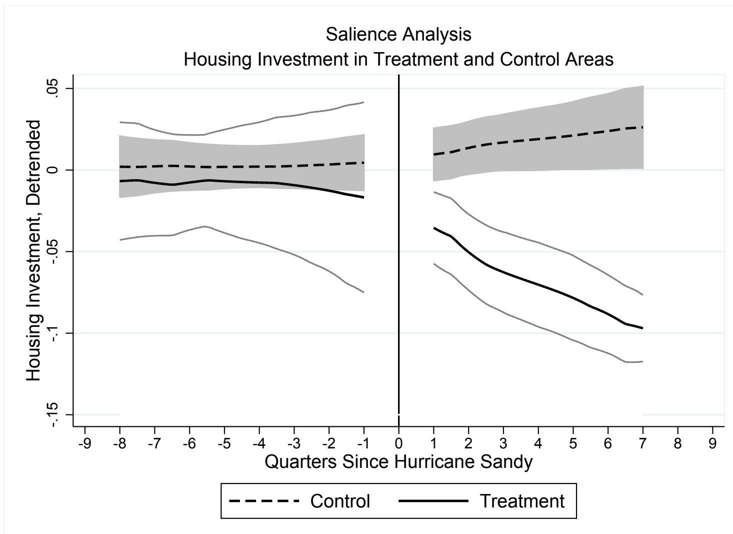


Figure 2.7: Trend Analysis: Treatment Definition - $SFHA$

We begin our formal analysis by estimating equation (2.1) for each treatment definition, $Damaged_{NonSFHA}$ and $Damaged_{SFHA}$. This allows us to quantify the effects of storm damage on housing investment among the set of damaged properties both in and out of the SFHA. We then study the effect of Hurricane Sandy on households' risk perceptions by estimating equation (2.1) using the treatment definition $SFHA$.

2.5.2 Storm Damage

Panel (a) of Table (2.1) presents estimates of the marginal effects of equation (2.1) 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 (2.1) ($\tau(Damaged_{SFHA} \times Post)$) 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 Column (2). Finally, we report estimates obtained from a linear probability model in Columns (1) and (2) of panel (b). Referring to panel (a), estimates range from .93 and .98 suggesting 93% to 98% increase in the probability that a homeowner in the SFHA invests in their property after experiencing storm damage.

Table 2.1: DID Estimates - Damaged Properties in the SFHA

	(1)	(2)
(a)		
	Logit	Logit
	q	q
$\tau(Damaged_{SFHA}\ x\ Post)$	0.975***	0.934***
	(0.202)	(0.198)
(b)		
	LPM	LPM
	q	q
$\tau(Damaged_{SFHA}\ x\ Post)$	0.822***	0.797***
	(0.175)	(0.173)
Observations	1,240,915	1,240,915
Year-Quarter FE	Y	Y
Lagged Dep. Var.	N	Y

Notes: Robust standard errors in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in panels (a) and (b) 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.

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 (2.1) in panel (a) of Table (2.2). We find no evidence of any change in the likelihood of property restoration among homeowners outside of the SFHA. Specifically, we detect a statistically insignificant 1.1% to 1.4% increase in the the probability an owner of a damaged home invests relative to households whose homes were unaffected.

Table 2.2: DID Estimates - Damaged Properties out of the SFHA

(a)

	(1)	(2)
	Logit	Logit
	q	q
$\tau(Damaged_{NonSFHA}\ x\ Post)$	0.0141	0.0114
	(0.1250)	(0.1247)

(b)

	(1)	(2)
	LPM	LPM
	q	q
$\tau(Damaged_{NonSFHA}\ x\ Post)$	-0.0396	-0.0419
	(0.1021)	(0.1020)
Observations	1,278,506	1,278,506
Year-Quarter FE	Y	Y
Lagged Dep. Var.	N	Y

Notes: Robust standard errors in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in panels (a) and (b) 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.

The preceding analysis shows that after experiencing damage to one's home, property owners in the SFHA invest at a significantly higher rate than similarly situated households outside of these zones. Recalling that flood insurance take-up rates by residents inside the floodplain are significantly higher than take-up rates by residents outside of the floodplain, our empirical results appear to 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 simply be an artifact of the 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 (2.6) shows that property investment fails to increase in any time period following the hurricane which rules out the possibility that agents outside of the floodplain defer remedial investments.

2.5.3 Risk Salience

Table (2.3) presents estimates of the marginal effects of equation (2.1) 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 Table (2.3) restricts attention to non-damaged buildings located between 0ft to 250ft of a damaged structure. To make the treatment and control groups more comparable, we also restrict attention to properties that lie within a 1km buffer of the SFHA boundary.

Table 2.3: DID Estimates - Salience Analysis

(a)

		(1)	(2)
	Sample	Logit q	Logit q
$\tau(SFHA\ x\ Post)$	[0ft, 250ft]	-0.4171** (0.171)	-0.4075** (0.173)

(b)

		(1)	(2)
	Sample	LPM q	LPM q
$\tau(SFHA\ x\ Post)$	[0ft, 250ft]	-0.4134** (0.161)	-0.4069** (0.164)
Observations		347299	347299
Year-Quarter FE		Y	Y
Lagged Dep. Var		N	Y

Notes: Robust standard errors in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in panels (a) and (b) 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.

Referring to panel (a), estimates of $\tau(SFHA \times Post)$ suggest a 41% to 42% *decrease* in the probability a homeowner in the SFHA invests in their property following the storm relative to households outside of the SFHA. Next, we characterize how estimates of $\tau(SFHA \times Post)$ vary with respect to proximity to damaged buildings. In Figure (2.8), 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 second coefficient estimate in Figure (2.8) shows, for instance, estimates of $\tau(SFHA \times Post)$ restricting attention to properties located between 50ft and 300ft. of a damaged building. The results presented in Figure (2.8) 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 54%; 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.

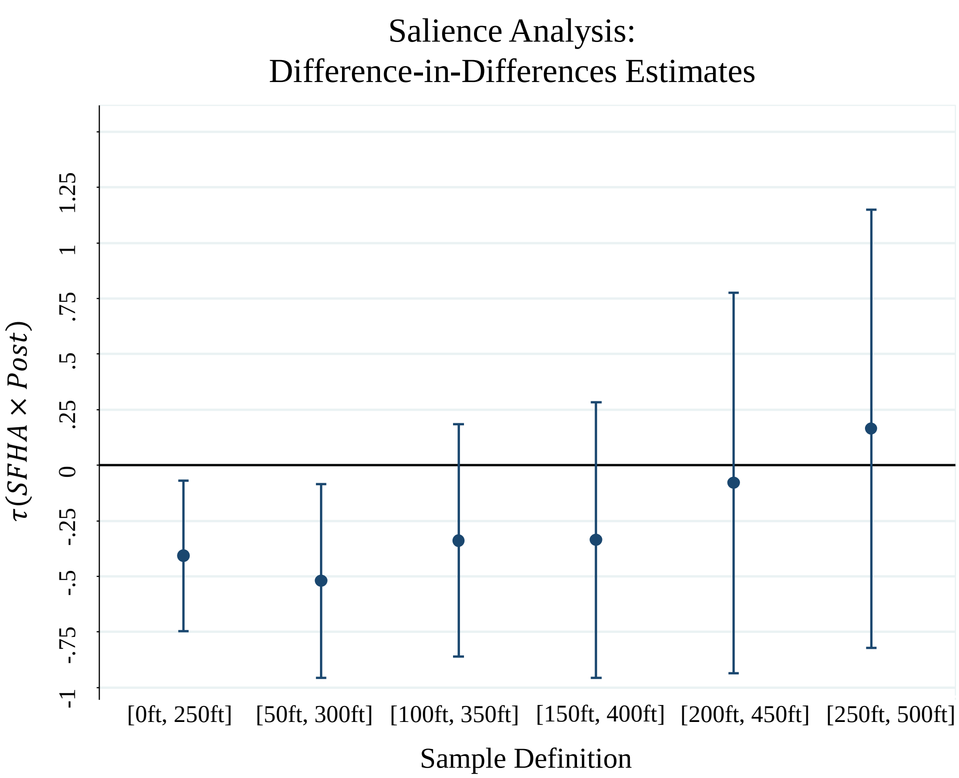


Figure 2.8: Spatial Effects: DID Estimates - Salience Analysis

These findings provide evidence that a natural disaster may work to heighten agents' perceptions of risk. However, our finding that relative investment differentials decay quickly with distance to damaged buildings suggests that changes in risk-saliency are 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.

2.5.3.1 Flood-insurance Premiums One methodological concern that we don't explicitly account for is potential increases in flood insurance premiums after the Hurricane which may work to drive down homeowners 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 in and of themselves to drive down property investment, we ought to detect falling investment in areas of the landscape less proximate to storm damage, but we don't.

2.5.3.2 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 gives the appearance that our estimates reflect localized spillover effects due to the potential dis-amenities associated with proximity to storm damage.

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

To rule out the possibility that our saliency estimates reflect localized dis-amenity 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, $1[x, x + 250]_{SFHA} \times Post$, should 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, post-hurricane interaction term obtained under estimating equation (2.1) in Table (2.4). Table (2.4) shows estimates restricting attention to treated properties within 0ft. and 250ft. of a damaged building. Estimates obtained under the logit specification are shown in panel (a). Estimates obtained from a linear probability model are shown in panel (b).

Referring to model estimates of $\tau(1[0, 0 + 250]_{NonSFHA} \times Post)$, 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 relative magnitude. In contrast, estimates of $\tau(1[0, 0+250]_{SFHA} \times Post)$ show a statistically significant 39% to 40% decrease in the probability of investment.

Next, we quantify how each estimate varies with proximity to damaged structures. Specif-

Table 2.4: DID Estimates - Spillover Effects

(a)

	(1)	(2)
	Logit	Logit
	q	q
$\tau(1[0,250]_{NonSFHA}\ x\ Post)$	0.0636	0.0624
	(0.1380)	(0.1371)
$\tau(1[0,250]_{SFHA}\ x\ Post)$	-0.396**	-0.3883**
	(0.1670)	(0.1681)

(b)

	(1)	(2)
	LPM	LPM
	q	q
$\tau(1[0,250]_{NonSFHA}\ x\ Post)$	0.1457	-0.0616
	(0.1539)	(0.1181)
$\tau(1[0,250]_{SFHA}\ x\ Post)$	-0.3463*	-0.4066*
	(0.1812)	(0.1508)
Observations	722,081	722,081
Year-Quarter FE	Y	Y
Lagged Dep. Var.	N	Y

Notes: Robust standard errors in parenthesis. *** p<.01, ** p<.05, * p<.1. Models in panels (a) and (b) 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.

ically, we iterate the results shown in Table (2.4) increasing x in 50ft. increments. We report the marginal effects of each estimate in Figures (2.9) and (2.10), respectively.

As shown in Figure (2.9) 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 (2.10), 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 disamenity shocks associated with damaged buildings are insufficient in and of themselves to drive down investment behaviors.

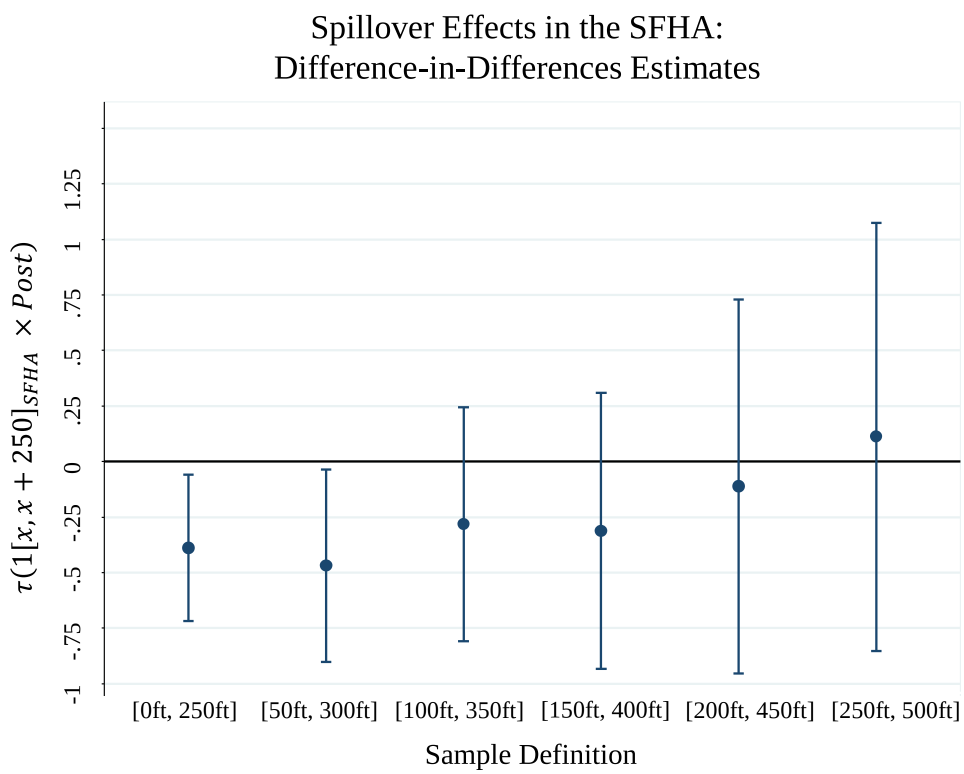


Figure 2.9: Spatial Effects: Spillover Analysis in the SFHA

2.6. SUMMARY OF FINDINGS

We estimate a statistically significant increase in the probability homeowners in statutorily designated 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. This disparity is likely driven by differences in flood-insurance take-up rates between these regions. Henceforth, flood insurance appears to be an effective tool for facilitating post-disaster home re-investment. However, we find no change in the rate of investment in damaged homes outside of the floodplain nor do we find evidence that homeowners in these regions defer remedial investments into the future. One might suspect that if it were economically efficient to re-invest in this latter set of properties, we ought

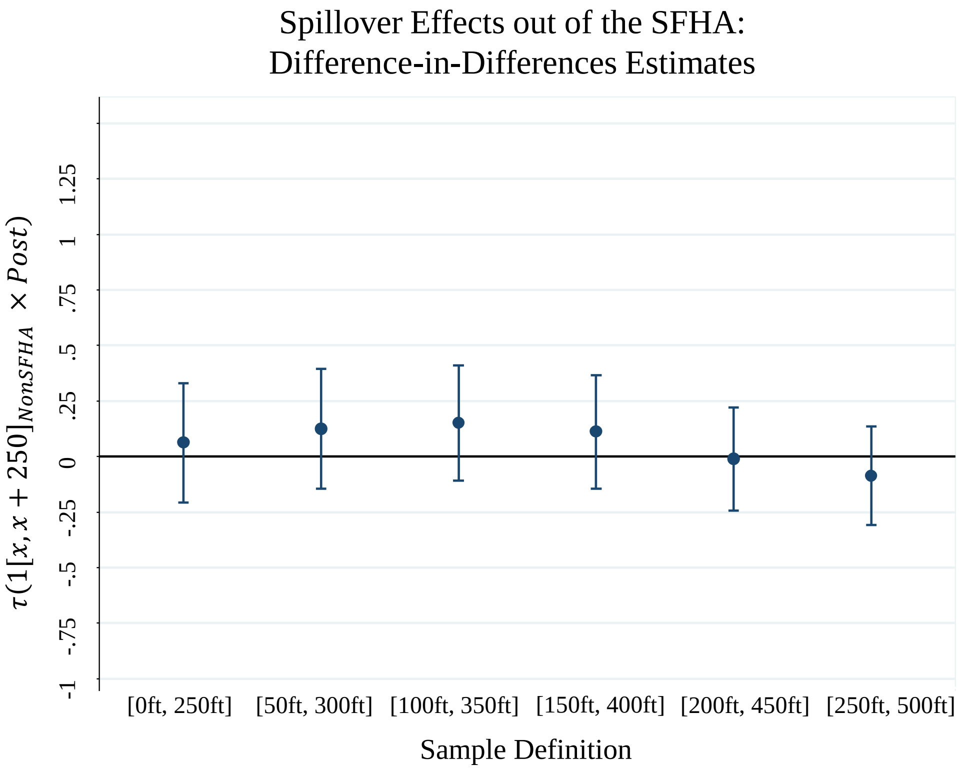


Figure 2.10: Spatial Effects: Spillover Analysis in the SFHA

to expect some degree of post-disaster investment in them, but we do not. The potential significance of this argument is further highlighted by the fact that a substantial portion of the insurance policies held by homeowners in the SFHA are a direct result of mandatory insurance laws and are typically provided to homeowners at subsidized rates. Our findings cannot rule out the possibility that current flood-insurance regulations have an unintended distortionary effect on the market for residential property investments; a potential reality which warrants attention given an increasing trend in the frequency and severity of natural disasters as well as the rate at which households migrate into disaster-prone regions.

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 properties outside of these zones (but similarly situated in terms of their exposure to storm damage). We find a statistically significant decrease in investment in homes in the SFHA, but only among the set of structures that in the immediate vicinity of a damaged building. We find no statistically significant change in the rate at which homeowners invest when we restrict our attention to properties less proximate to damaged buildings. These results show that while a recent storm may heighten households' risk-perceptions, a primary mechanism through which these changes are achieved is the spatial distribution of storm damage.

Following a catastrophic event, homeowners are bombarded with multiple sources of information which may heighten their awareness of the relative risk of living in a disaster prone area. These saliency shocks include factors that are not correlated with proximity to storm damage such as 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; however, our results show that these effects are not strong enough to translate into behavioral modifications. Our findings, therefore, cast doubt on the ability of an information-based regulation to effectively align risk-perceptions with risk-actualities.

3. WILDFIRE AND INFANT HEALTH

(with Xiaoxi Zhao)

3.1. INTRODUCTION

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 is a result of changes in global climates. (Westerling et al., 2006; Gillett et al., 2004). Recent expansion of residential housing into forested lands is 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. (Travis et al., 2002; Conroy et al., 2003; Radeloff et al., 2005). Sprawling configurations of WUI developments have modified the interactions between environmental and socio-economic dynamics resulting in an increase in the likelihood of severe wildfires in inhabited spaces. (Radeloff et al., 2005; Spyratos et al., 2007).

Residents in or near the WUI aren't the only ones at risk. Recent work by the Natural Resources Defense Council suggests that in 2011 alone, out of a population of 311 million,

[1]Wildfires: Dry, hot, and windy. *National Geographic*, (2013). http://environment.nationalgeographic.com/environment/natural-disasters/wildfires/.

roughly 212 million people lived in counties affected by wildfire smoke. Motivated by these observations, we assess the public health implications of wildfire. To do this, we assemble data for every fire in Colorado between 2002 and 2013 and construct a fine-scaled geo-spatial data set delineating both the path of wildfire smoke and the prevailing wind direction near burn sites. These data are subsequently combined with a restricted access database detailing the vital statistics as well as the latitutde and longitude co-ordinates corresponding to the home address of the universe of infants born in Colorado.

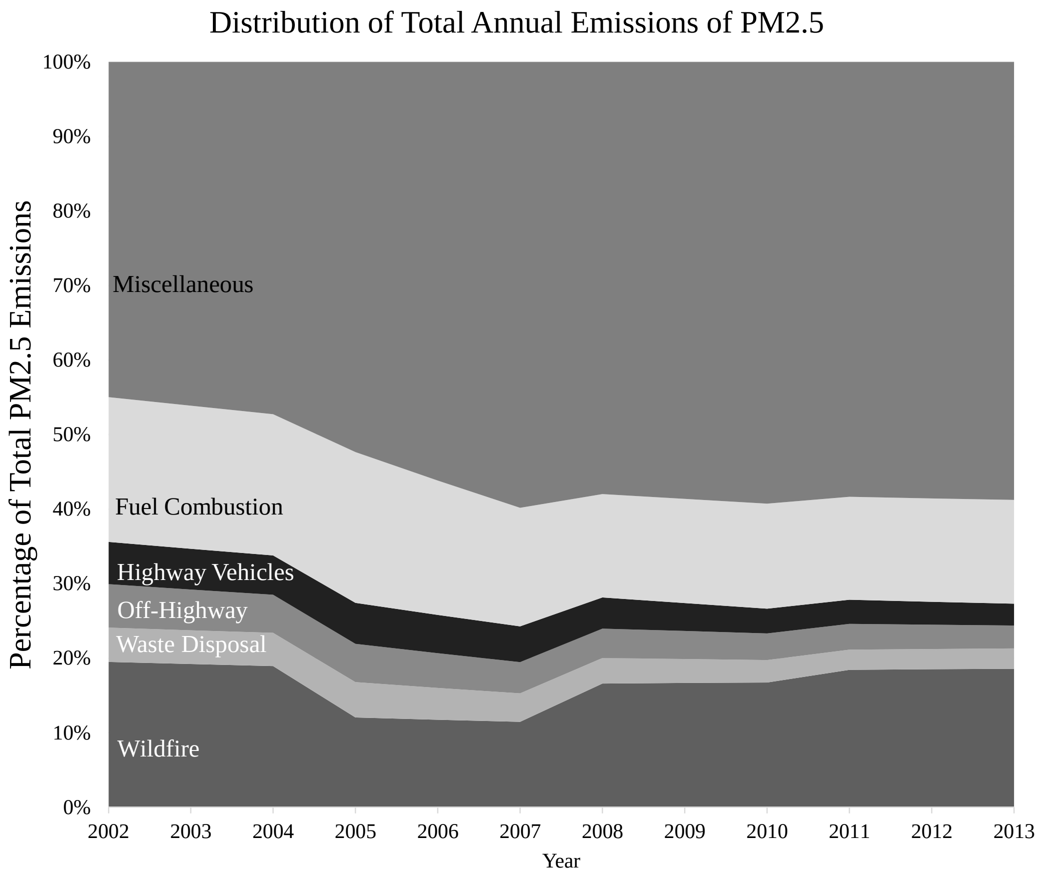
Elevated concentrations of fine particulate matter ($PM_{2.5}$) is the principal health threat of wildfire. (Jaffe et al. 2008). While $PM_{2.5}$ is a term used to refer to fine particulates suspended in the air less than 2.5 micrometers in diameter, the size of particles found in wildfire smoke are on the lower end of this spectrum with diameters typically between .4 and .7 micrometers; 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). $PM_{2.5}$ emissions from wildfire account for a strikingly large proportion of total annual $PM_{2.5}$ emissions in the United States. To get a sense of the relative magnitude, in Figure (3.1), we plot $PM_{2.5}$ emissions trends expressed as a percentage of total annual emissions. Figure (3.1), which is constructed from the EPAs 1970-2014 Air Pollutant Emissions Trends Data[2], 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, the percentage of total annual $PM_{2.5}$ emissions in the U.S. due to wildfire has surpassed the proportion of total 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. These trends bring *wildfire* to the forefront of the debate on air-quality, public health, and wildland fire management policy.

While the physiological pathway between fine particulate exposure and fetal health out-

[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%.



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 3.1: EPA Air Pollutant Emissions Trends Data: Average Annual $PM_{2.5}$ emissions trends (2002 - 2013).

comes remains unclear, it is hypothesized that the particles and toxicants in wildfire smoke cross through the placenta disrupting fetal nutrition and oxygen flow leading to fetal growth retardation and reduced gestational length (Jayachandran, 2009; Berkowitz et al., 2003; Dejmek et al., 1999; Wang et al., 1997). It has also been argued that exposure to fine particulate matter may cause an inflammatory response weakening the immune system in the body. (Currie and Neidell, 2005; Seaton et al., 1995).

Very little research exists which considers the effects of wildfire smoke on infant health.

Additionally, pollution is only one of the potential health threats of wildfire. The physical and psychological stress placed on mothers living in close proximity to a natural disaster may be another. 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. This literature suggests that the stress associated with living through a natural disaster may translate into lower birthweight; however, no study to date has explored the link between severe wildfires, in-utero stress, and public health.

Disentangling health effects due to stress from the effects of ambient air pollution is one of the more significant empirical challenges we face. Our baseline empirical models compare the birth outcomes of infants within one mile of wildfire to the birth outcomes of infants between one and five miles using a difference-in-differences estimation strategy. In subsequent specifications, we isolate the effects of stress and ambient air pollution by analyzing birth outcomes in upwind regions of the landscape separately from the birth outcomes of infants in downwind regions. This approach allows us to examine the relationship between smoke intensity and health by varying the spatial cutoff delineating treated and non-treated infants. However, one potential drawback to this approach is that one might consider prevailing wind to be only a coarse measure of exposure to particle emissions. This may lead us to underestimate both the level effect of pollution on infant health as well as the spatial decay process between ambient air pollution and proximity to wildfire. To overcome this limitation, we construct a dataset set delineating the spatial path of wildfire smoke which we produce from a series of daily satellite images taken around the ignition date of each fire. This allows us to refine our definition of exposure to pollution by determining each birth's distance to a wildfire as well as each birth's exact location relative to the actual path of wildfire smoke.

We find that fire reduces the birthweight of infants that were exposed to wildfire smoke in their third trimesters of gestation and located within 3 miles of a wildfire burn by 4% to 6%. Drawing on estimates by Black et al. (2007), this effect translates 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. Infants exposed to wildfire smoke in their second trimester of gestation are also at risk of lower birthweight on the order of 3% to 4%; albeit, only if they are located within 1.25 miles of a fire. We find no evidence that exposure to wildfire reduces the birthweight of infants located upwind or outside of a wildfire smoke plume. Our results link exogenous changes in ambient air pollution to decreased birthweight, but we fail to detect any changes in fetal health due to physical and psychological stress.

To the best of our knowledge, this is the first study to integrate fine-scaled spatial data on the dispersion of ambient air pollution with health data geocoded to the *individual* level. These data allow us to utilize a simple, difference-in-differences empirical strategy to estimate the effects of short-term exposure to fine particulate matter on fetal health outcomes. Using wildfire as an exogenous shock to ambient air pollution, our empirical approach allows us to mitigate many of the identification problems faced by researchers in the extant literature including imprecise air-quality assessments and potential bias due to geographical sorting.

We proceed as follows. We begin by providing background on the existing work on air pollution, in-utero stress, natural disasters, and health outcomes in Section (3.2). We characterize our study area and the construction of our geo-spatial data in section (3.3). We present our empirical methodology in Section (3.4) and our findings in Section (3.5).

3.2. BACKGROUND

3.2.1 Health Impacts of Wildfire

Kunzli et al. (2006) study the impact of the 2003 Southern California wildfires on child health using self-reported data on the occurrence of health symptoms among 873 high-school students and 5,551 elementary-school children. Using community-level data identifying fire-related PM_{10} levels, the authors identify a link between wildfire, increased eye and respiratory symptoms, medication use, and physician visits. Using zip-code level data on daily death counts, Kochi et al. (2012) estimate that these fires led to 133 excess cardiorespiratory-related deaths. Johnston et al. (2002) examine the relationship between fine particle emissions due to bushfire smoke and asthma. They find a significant association between higher levels of atmospheric PM_{10} concentration and the daily number of presentations for asthma in a particular hospital.

Jayachandran (2009) explores the effects of the 1997 forest fires in Indonesia on early-life mortality. This author infers fetal, infant, and child mortality from missing children in the 2000 Indonesian Census. Combining this information with daily data on airborne smoke, the author estimates that fire-driven increases in air pollution led to a significant increase in child mortality evidenced by a rise in the number of children missing in the Indonesian Census.

More recently, Moeltner et al. (2013) study the effects of wildfire in the Reno/Sparks area of Northern Nevada. These authors utilize data on daily hospital admissions for illnesses related to exposure to air pollution. Their study improves upon previous works by incorporating spatial data on prevailing wind direction. Their strategy involves linking acreage burned to daily data on ambient air pollutants near hospital sites and local hospital admission counts. They estimate a significant increase in hospital patient counts downwind and as far as 300 miles away of a burn site.

More closely related to our work, Holstius et al. (2012) study the effects of wildfire on birthweight following the 2003 Southern California wildfires. Using information identifying the Census tract of each mother, these authors restrict attention to infants born in the South Coast Air Basin – which includes Orange County and portions of Los Angeles, San Bernadino, and Riverside Counties – for which wildfire smoke due to the 2003 wildfires was argued to be the most heavily concentrated. They subsequently compare the birthweights from pregnancies before, during, and after the wildfire event. Compared to pre-fire births, they estimate a significant reduction in the birthweight 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. Specifically, this author regresses 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. In addition, he finds a negative, but statistically insignificant reduction in birthweight.

We improve on the existing literature in three notable ways. First, we construct fine-scaled spatial data of the actual path of wildfire smoke. Air quality assessments based on air monitoring stations are being increasingly considered an imprecise tool to determine ground level concentrations of air pollutants over large geographic scales. In addition, wildfires are very short-term sources of ambient air pollution. We show that the concentration of wildfire smoke in any given region is heavily dependent upon the prevailing winds near the burn site as well as the distance to the burn source. Second, to the best of our knowledge, our study is the first to combine spatial data on ambient air pollution with a micro-level health dataset. The geographic quality of our data allows us to estimate the health impacts of ambient air pollution by comparing fetal health outcomes both in and out of polluted areas. Additionally, we can estimate the spatial decay process between pollution and proximity to wildfire. In our empirical work, we show that improperly accounting for the spatial concentration of pollution near the pollution source leads to a substantial downward bias in our coefficient estimates. Finally, we estimate the health effects from multiple fires on the *universe* of infants born in the state of Colorado over the past 12 years considerably mitigating any concerns regarding composition effects due to sample attrition.

3.2.2 Physiological Effects of Ambient Air Pollution.

Among the vast literature dedicated to estimating the relationship between air pollution and public health, 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 bulk 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; although, many of these studies face notable identification issues stemming primarily from co-pollutant associations and coarse geographic measures of air pollution. (Goodwin, 2015; Pope III, 2000).

A more recent literature estimates the effects of ambient air pollution on health using quasi-experimental techniques more commonly found in the economics literature. For example, Parker et al. (2008) investigate the effects of pollution on pre-term births before, during, and after the closure of an open-hearth steel mill in Utah Valley which was identified as a source of PM_{10} emissions. They find a reduction in the likelihood of mothers delivering pre-maturely during the closure, relative to mothers delivering before or after. Chay and Greenstone (2003) look at infant mortality rates in counties that experienced significant reductions in pollution due to the 1981-1982 recession. They find that a 1 $\mu g/m^3$ reduction in suspended particles results in 4-8 fewer deaths per 100,000 live births. Currie and Neidell

(2005) examine the impacts of air pollutants on fetal health using data on atmospheric pollution from the California Environmental Protection Agency's air monitoring stations. The authors impute pollution levels for each zip code in California by taking distance weighted averages of weekly pollution measures at all stations within a twenty-mile radius. These authors find that high levels of postnatal exposure to CO can have a significant effect on infant mortality.

Goodwin (2015) investigates the effects of air quality on birth outcomes using the eruption of Mount St. Helen as a natural experiment. Comparing birth outcomes in "ashed" counties to birth outcomes in "non-ashed" counties, the author finds no evidence linking fine particulate exposure to infant mortality. Finally, using a difference-in-differences approach, Severnini (2014) shows that infants born in counties with elevated levels of pollution from coal-fired power plants due to nuclear plant shutdowns have lower birthweight and lower gestational age. Utilizing quasi-experimental techniques, these more recent studies make a considerable advance over much of the extant work on air pollution and public health. However, they are limited by the geographic quality of the data they utilize; typically relying on coarse measures of ambient air pollution and health outcomes aggregated to the county or zip-code level.

Currie et al. (2009) improve the accuracy of air quality exposure 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 the birth outcomes of infants, but fail to find any significant effect of PM_{10} on fetal health. 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 2-10km. Also using micro-data on infants, Currie et al. (2015) compare birth outcomes within one mile of a toxic plant to birth outcomes between one and two miles, before and after the opening or closure of 1600 plants. These authors report a 3% increase in the probability of low birthweight within one mile of a plant.

Each of these studies utilizes micro-data on the location of infants which in and of itself represents a significant advance over earlier works. One methodological limitation – which forms the focal point of our study of the health impacts of wildfire – is that much of the extant literature is limited to relatively coarse spatial metrics or proxies of ambient air pollutants. Currie and Walker (2011) identify a second limitation rooted in the propensity for changes in air pollution to induce geographical sorting on the basis of demographic characteristics potentially correlated with health outcomes; a critique 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 significant threat to identification 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 the potential bias due to geographical sorting is closely related to the rate of out-migration. To the extent that the timing of a wildfire and the timing of particle emissions fueled *by* a wildfire coincide; we can substantially mitigate this form of bias by investigating the health impacts of fire immediately after wildfires ignite. Additionally, the timing of wildfire ignitions as well as the spatial variation in wildfire smoke are random. These properties of wildfire – which we can effectively leverage with our data – allow us to address many of the notable identification problems faced by researchers in the extant literature.

3.2.3 Natural Disasters, Physiological and Psychological Stress

Our work on wildfire also contributes to a small literature which studies the effects of natural disasters on public health. Xiong et al. (2008), for instance, study the effects of hurricane exposure on pregnancy outcomes in New Orleans and Baton Rouge. Utilizing data collected from interviews with 301 women regarding their experiences during a hurricane, the authors find that the frequency of preterm birth was higher in women that reported "high" levels of hurricane exposure relative to women in less-exposed areas. Utilizing a larger sample of births, 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 age of infants. Finally, utilizing birth record data in Chile, Torche (2011) finds a significant decline in the mean birthweight of infants located in counties that were exposed to a high-intensity earthquake.

One limitation of these studies is that they rely on indirect measures of disaster exposure. In this respect, Currie and Rossin-Slater (2013) make a considerable advance using 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 relationship between hurricane exposure during pregnancy and birthweight.

These papers contribute to a broad medical literature which considers the effects of in-utero stress on fetal health. Mulder et al. (2002) evaluate the effects of prenatal stress on pregnancy outcomes by synthesizing findings reported in controlled, human studies. They find an association between self-reported stress levels in pregnant women and an increased incidence of low birthweight. 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 authors' 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. Even in light of Dunkel Schetter's (2011) work, the findings reported in the set of more recent studies which seek to identify the causal effects of stress on health are generally inconclusive.

3.3. DATA

3.3.1 Study Area, Wildfire Burn Scars, and Prevailing Winds

We study the effects of wildfires in the State of Colorado between the years 2002 and 2013. We obtained spatial data delineating wildfire burn scars from the Geospatial Multi-Agency Coordination Group (GeoMAC)[4] and Monitoring Trends in Burn Severity (MTBS)[5]. We subsequently linked the data provided by GeoMAC and MTBS to information contained in each fire's Incident Status Summary report (ICS-209) which we obtained from the National Fire and Aviation Management Web Application[6] maintained by the National Inter-agency Fire Center[7]. We use these reports to determine the date each fire ignited as well as the prevailing wind direction around each burn site. Where applicable, we cross-checked the date of each fire with ignition dates reported by the Federal Emergency Management Agency[8]. In a small handful of cases, we identified a one to five day discrepancy in ignition dates; to control for this, we drop birth records from our sample that occurred within a five day window of the dates reported in each ICS-209 report. We identify a total of 161 fires. Our study area and the location of each fire in our sample are shown in Figure (3.2).

[4]http://www.geomac.gov/index.shtml
[5]http://www.mtbs.gov/
[6]https://fam.nwcg.gov/fam-web/
[7]http://www.nifc.gov/
[8]https://www.fema.gov/disasters

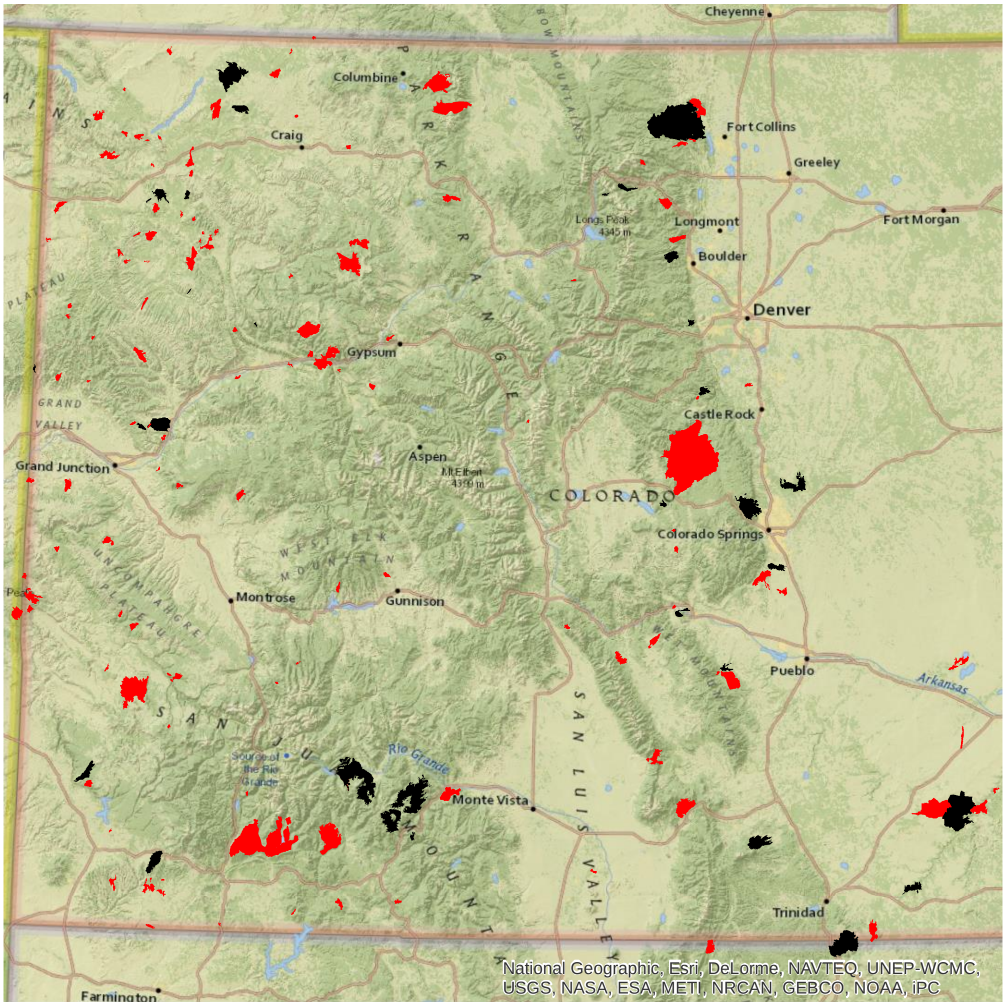


Figure 3.2: Study Area: Wildfires are depicted in red and black. Black is used to designate the set of wildfires with satellite images of wildfire smoke plumes.

3.3.2 Wildfire Smoke

To identify wildfire smoke plumes, we collected daily satellite images of our study area taken by the MODIS[9] instrument on board the Terra and Aqua spacecrafts. The local equatorial crossing times of the Terra and Aqua satellites are approximately 10:30 a.m. and 1:30 p.m., respectively. 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].

To map wildfire smoke plumes, we first overlay eight satellite images for each fire with

[9] http://modis.gsfc.nasa.gov/data/
[10] http://www.ssec.wisc.edu/

each fire's burn scar. These include four images from the Terra satellite and four images from the Aqua satellite corresponding to the first four days following the ignition date of each fire. Second, we trace out the extent of visible smoke in each image and store this information in GIS. We construct each fires 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 (3.3).

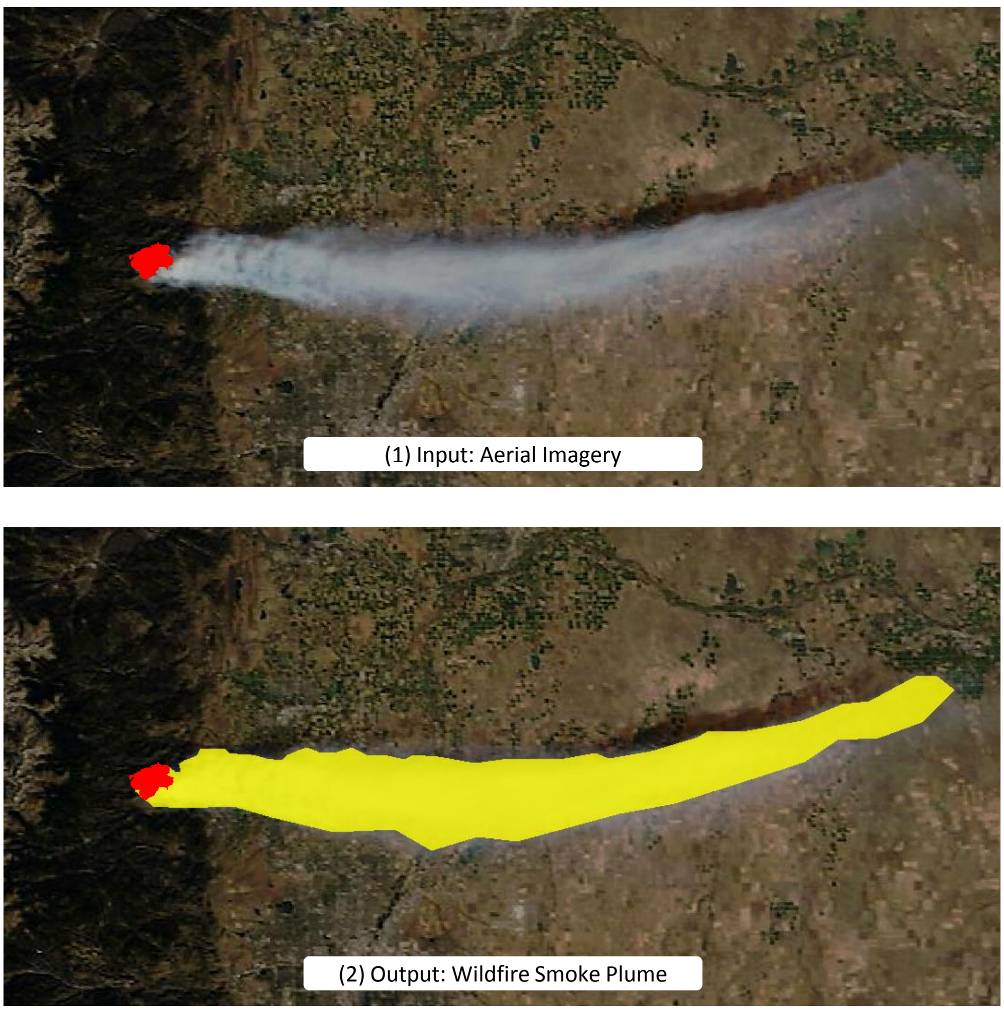


Figure 3.3: Sample Fire and Smoke Plume

One limitation of our smoke data is the relatively poor image resolution of our satellite imagery. At a resolution of 250m, we were unable to code many of the smaller fires in our sample. The temporal coverage of our satellite imagery is another limiting factor; while we have wind data for all fires dating back to 2002, our satellite imagery only dates back to 2007. In addition, we were unable to construct plumes for many of the fires due to excessive cloud cover. As a result, we successfully constructed wildfire smoke polygons for 28 of the 161 wildfires in our sample. We illustrate the smoke plumes associated these fires in Figure (3.4).

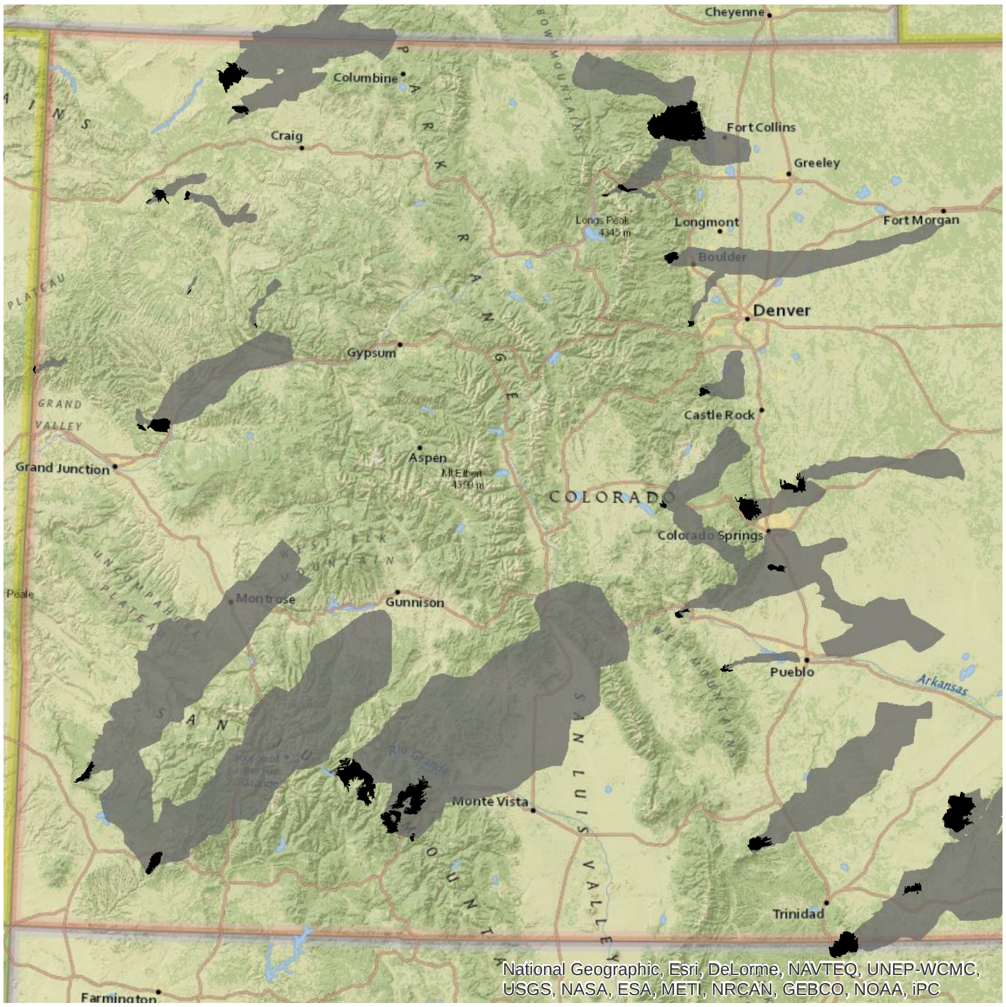


Figure 3.4: Wildfire Smoke Plumes: Wildfires are depicted in black. Smoke plumes are depicted in dark grey.

3.3.3 Infant Health

We utilize a restricted-access data set detailing the vital statistics and natility records for the universe of infants born in the state of Colorado between 2002 and 2013. These data were obtained under a confidential data 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; the demographic information of each infant's mother including race, education, marital status and age. These data also include the latitude and longitude coordinates associated with each mother's home address.

3.4. METHODS

Our empirical analysis compares the birth outcomes 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 birth, i, to its nearest fire $m \in M$ that occurred within an eighteen month window of each infant's birth date. To minimize confounding effects of exposure to other fires, we drop any observation from our sample that lies within five miles of multiple fires that occurred within an eighteen month window of an infant's birth date. For each treatment group, our baseline empirical specification takes the form:

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

where $Post_{itm}$ is a post-fire dummy and $Treat_{im}$ is a treatment group indicator. We use this empirical model to study the effects of fire on the gestational length of pregnancies. 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 effects 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 mothers' marital status, race, age, and education. G_i' is a vector of fire-specific geographic controls which includes elevation, distance to fire, as well as the interaction between elevation and distance. Finally, we include a set of year-quarter fixed effects. To identify trimester-specific effects for our analysis of birthweight 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:

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

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 gestation.

3.4.1 Treatment Definitions

Our starting point for investigating the effects of wildfire on infant health involves comparing birth outcomes of infants located within a certain radius of a wildfire to the birth outcomes of infants located in the immediately adjacent areas. Specifically, we estimate variants of equation (3.2) with the treatment variable $1Mile_{im}$ which equals one for any birth located within one mile of a wildfire (using the set of infants located in the immediately adjacent area as controls). This approach – which identifies the net-effect of living living in close proximity to a wildfire relative to less proximate areas – is motivated by its prevalence in the literature. Currie and Walker (2011), for instance, study birth outcomes of infants within 2 kilometers of a toll plaza to the birth outcomes of infants in the surrounding 2 - 10 kilometer areas. In a related work, Currie et al. (2015) investigate health outcomes of infants located within 1 mile of a toxic plant to health outcomes of births located in the immediately adjacent area. As we explain below, we test the sensitivity of each of our models to various treatment / control definitions and ultimately let the data drive the selection of these parameters.

The principal empirical challenge we face involves separately identifying the extent to which changes in health outcomes along the $1Mile_{im}$ treatment dimension are driven by ambient air pollution and stress. We solve this problem by identifying the portions of the landscape surrounding each wildfire that were polluted and less-polluted using information on prevailing wind directions 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 fire. To identify the set of births *upwind* of a fire, we flag any observation located between $\pm 45^0$ of the prevailing wind direction near each fire. Using this information, we construct two treatment variables $Stressed_{im}$ and $Polluted_{im}$, such that $Stressed_{im} + Polluted_{im} = 1Mile_{im}$. Specifically, for our sample of fires with *wind* data, $Stressed_{im}$ is a dummy set equal to one for any infant located upwind of a wildfire burn scar. This variable captures the effects of living in regions in close proximity to a disaster area, but that were relatively less likely to be 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 sample of fires with *smoke* data, $Polluted_{im}$ is an indicator set equal to one for any birth located inside the smoke plume and within one mile of a wildfire. Likewise, $Stressed_{im}$ is an indicator set equal to one for any birth located within one mile, but outside of the smoke plume of a given wildfire. This transforms the empirical specification in (3.2) into:

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

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 gestation relative to changes in the health outcomes of infants in each respective control group.

3.5. RESULTS

3.5.1 Descriptive Statistics

We report the descriptive statistics of our data in Table (3.1). Columns (1) - (3) report sample means for births which match to a fire with wind data. Columns (4) - (6) report sample means for births which match to a fire with smoke data. Columns (1) and (4) show the descriptive statistics for the entire sample of births which match to fires with wind and smoke data, respectively. We identify a total of 430,444 births which match to a fire with wind data and 157,293 births which match to a fire with smoke data. The mean birthweight in our wind sample is 3196.83 grams and the mean birth weight in our smoke sample is 3199.57 grams. In Columns (2) and (5), we restrict attention to births located within five miles of a wildfire; Columns (3) and (6) further restrict attention to full-term births (those reaching a gestational age of at least 39 weeks). The summary statistics of each variable are qualitatively similar across each sample except that roughly 75% to 77% of residents within five miles of a wildfire are white compared to 60% to 61% of residents across the state; this reflects differences in the demographic composition of households living in forested areas where fires occur and urban and city areas where fires do not occur.

Table 3.1: Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
Fire Sample		***Wind***			***Smoke***	
Sample Restrictions						
Distance to Fire	-	<5 Miles	<5 Miles	-	<5 Miles	<5 Miles
Gestational Age (Weeks)	-	-	39 to 43	-	-	39 to 43
Birth Weight (g)	3196.83	3200.98	3381.42	3199.57	3192.83	3382.61
	(558.66)	(547.56)	(413.37)	(557.54)	(558.42)	(413.86)
Gestational Age (Weeks)	38.63	38.60	39.67	38.63	38.60	39.60
	(2.28)	(2.11)	(0.81)	(2.04)	(2.03)	(0.71)
Age	28.07	29.38	29.34	28.53	29.02	29.13
	(6.06)	(5.87)	(5.78)	(5.99)	(5.94)	(5.84)
I(Married)	0.74	0.82	0.82	0.75	0.80	0.80
	(0.44)	(0.39)	(0.39)	(0.44)	(0.4)	(0.4)
I(White)	0.61	0.77	0.77	0.60	0.75	0.76
	(0.49)	(0.42)	(0.42)	(0.49)	(0.43)	(0.43)
I(Black)	0.04	0.02	0.02	0.04	0.03	0.03
	(0.2)	(0.14)	(0.13)	(0.2)	(0.16)	(0.16)
I(Hispanic)	0.29	0.15	0.15	0.29	0.15	0.14
	(0.46)	(0.36)	(0.35)	(0.45)	(0.35)	(0.35)
I(Race - Other)	0.06	0.06	0.06	0.07	0.07	0.07
	(0.23)	(0.24)	(0.24)	(0.26)	(0.26)	(0.26)
Observations	430,444	11,929	7,444	157,293	7,241	4,736

3.5.2 Birth Weight

We begin our formal analysis by investigating the effects of wildfire on birthweight. Specifically, we estimate equations (3.2) and (3.3) separately for our sample of births that match to fires with wind data and for the sample of births that match to fires with smoke data. These results are reported in Table (3.2). Columns (1) and (3) report difference-in-differences estimates obtained by estimating equation (3.2) using our wind and our smoke sample, respectively; p-values are reported in parenthesis. These models compare the outcomes of infants located within a one mile radius of wildfire to the outcomes of infants located in immediately adjacent, one to five mile rings. In order to identify the effects of fire on birthweight independent of the effects of fire on the gestational age, each model restricts attention to full term births.

Table 3.2: Difference-in-Differences Estimates: Birth Weight

Fire Sample	(1)	(2)	(3)	(4)
	Wind		**Smoke**	
	ln(bw)	*ln(bw)*	*ln(bw)*	*ln(bw)*
(1 Mile) x (Tri 3)	-0.0254	-	-0.0394	-
	(0.173)	-	(0.160)	-
(1 Mile) x (Tri 2)	-0.0135	-	-0.0193	-
	(0.375)	-	(0.311)	-
(1 Mile) x (Tri 1)	-0.00328	-	-0.0256	-
	(0.838)	-	(0.247)	-
(Polluted) x (Tri 3)	-	-0.0544**	-	-0.0493*
	-	(0.0393)	-	(0.0905)
(Polluted) x (Tri 2)	-	-0.0437	-	-0.0399*
	-	(0.106)	-	(0.0761)
(Polluted) x (Tri 1)	-	-0.0432	-	-0.0128
	-	(0.308)	-	(0.644)
(Stressed) x (Tri3)	-	-0.0121	-	-0.0282
	-	(0.598)	-	(0.606)
(Stressed) x (Tri 2)	-	0.00142	-	0.0319
	-	(0.937)	-	(0.336)
(Stressed) x (Tri 1)	-	0.0146	-	-0.0504
	-	(0.391)	-	(0.180)
P[Polluted x Tri 3 - Stressed x Tri 3]	-	0.212	-	0.730
P[Polluted x Tri 2 - Stressed x Tri 2]	-	0.155	-	0.070
P[Polluted x Tri 1 - Stressed x Tri 1]	-	0.200	-	0.418
Observations	7,444	7,444	4,736	4,736

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Model include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, and gestational age.

Difference-in-Differences estimates corresponding to $1Mile \times Tri$ 3 shown in Columns (1) and (3) suggest a 2.5% to 3.9% reduction in birthweight conditional on each infant's mother being exposed to wildfire in her third trimester of pregnancy; however, these effects are statistically *insignificant* with p-values of .173 and .16 respectively. Estimates of $1Mile \times Tri$ 2 and $1Mile \times Tri$ 1 are insignificant in each model as well. As we explain previously, the concentration of wildfire smoke at a given location is heavily dependent on the direction of the prevailing winds; a fact that is readily apparent from Figures (3.3) and (3.4). Henceforth, comparing the outcomes of births located less than one mile of a fire to birth outcomes in less proximate regions ignores the fact that there exists infants located within one mile of a wildfire that were not exposed to high concentrations of wildfire smoke. Leveraging this fact to our advantage, in Column (2) we report estimates of ($Polluted \times Tri\ k$) and ($Stressed \times Tri\ k$) corresponding to our sample of births which match to fires with wind data. Model estimates of ($Polluted \times Tri\ 3$) show that infants located in a one mile radius *downwind* of a wildfire incur a statistically significant 5.44% reduction in birthweight conditional on each infants' mother being exposed in her third trimester of pregnancy. Estimates of ($Polluted \times Tri\ 2$) also suggest a weakly insignificant ($p < .106$), 4.4% reduction in birthweight in proximate, downwind regions of a wildfire as well. Estimates of ($Polluted \times Tri\ 1$) show that wildfire has no statistically significant effect on infants exposed during their first trimester of gestation, even when they are located in a *downwind* region of a wildfire. Referring to coefficient estimates of ($Stressed \times Tri\ 3$), ($Stressed \times Tri\ 2$), and ($Stressed \times Tri\ 1$), we find no statistically significant effects of wildfire on the birthweight of infants *upwind* of a fire regardless of the trimester in which they were exposed.

Next, we turn our attention to Column (4) which compares the outcomes of infants in polluted and non-polluted regions identified based on each infant's location with respect to the actual smoke plume of each fire. Coefficient estimates of ($Polluted \times Tri\ 3$) indicate that infants inside a smoke plume incur a statistically significant 4.9% reduction in birthweight conditional on each infants mother being exposed in her third trimester. Estimates of ($Polluted \times Tri\ 2$) also suggest a significant 4% reduction in birthweight. These estimates are qualitatively similar to our estimates obtained from identifying 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. Finally, we test the relative effect of living in proximate downwind / in-the-smoke regions of a wildfire to proximate upwind / out-of-the-smoke regions by reporting p-values associated with the two-sided test: (Polluted) $\times$ (Tri k) $-$ (Stressed) $\times$ (Tri k). We fail to reject the null hypothesis for each test at conventional levels of significance; however, restricting attention to trimester 2 and 3 effects, these p-values show we can reject the null hypothesis associated with the one-tailed tests (Polluted) $\times$ (Tri k) $\leq$ (Stressed) $\times$ (Tri k) in three out of the four tests.

Collectively, these results indicate a strong link between decreased birthweight and ambient air pollution. Further, our findings show that exposure to a wildfire in and of itself is not identified as a strong enough force to translate into fetal health outcomes; a finding consistent with the earlier work of Currie and Rossin-Slater (2013) who also fail to link exposure to a recent hurricane to birth outcomes. The fact that coefficient estimates of $1Mile \times Tri\ k$ are insignificant in *every* specification shows that proximity to a polluting source can be a relatively imprecise measure of exposure to ambient air pollution and may explain why many of the earlier studies which estimate the effects of wildfire on fetal health fail to identify a significant relationship between birthweight and county level $PM_{2.5}$ concentrations. The importance of properly accounting for the spatial concentration of wildfire smoke is highlighted by the fact that we find a statistically significant reduction in the birthweight of infants in proximate downwind / in-the-smoke regions but *no* statistically significant effects in proximate upwind / out-of-the-smoke regions.

We proceed by subjecting our model estimates to various robustness checks. We then characterize the spatial decay process of wildfire on infant health.

3.5.2.1 Contaminated Controls & Erratic Wind Patterns

Contaminated Control Group: *Wind Analysis*. Our baseline model using the sample of births which match to a fire with wind data compares the outcomes of infants within one mile of a wildfire to the outcomes of infants in the immediately adjacent area, partitioning the set of treated infants into a polluted (downwind) group and a non-polluted (upwind) group. This approach implicitly compares outcomes in each treatment group to the outcomes of infants in a control group; some of which resided in *downwind* regions of a wildfire. In Column (1) of Table

(3.3) we replicate our baseline wind model initially reported in Column (2) of Table (3.2). In Column (2) of Table (3.3), we re-estimate our baseline model including a full set of group by fire and group by trimester indicator variables for the set 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 variables effectively 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 two and five miles *upwind* of a fire, without changing the sample of births across each model. We find no significant differences between coefficient estimates shown in Column (2) of Table (3.3) and our baseline estimates in Column (1).

Table 3.3: Robustness Checks: Wind Model

	(1)	(2)	(3)	(4)
Fire Sample	**Wind**	**Wind**	**Wind**	**Wind**
Robustness Check:	Baseline Model	Contaminated Controls	Erratic Wind	Opposite Wind Direction (Placebo)
	ln(bw)	*ln(bw)*	*ln(bw)*	*ln(bw)*
(Polluted) x (Tri 3)	-0.0544**	-0.0589**	-0.0617**	-0.0413
	(0.0393)	(0.0285)	(0.0338)	(0.263)
(Polluted) x (Tri 2)	-0.0437	-0.0409	-0.0372	-0.00789
	(0.106)	(0.136)	(0.182)	(0.773)
(Polluted) x (Tri 1)	-0.0432	-0.0362	-0.0317	0.0318
	(0.308)	(0.396)	(0.456)	(0.237)
(Stressed) x (Tri3)	-0.0121	-0.0163	-0.0130	-0.0134
	(0.598)	(0.483)	(0.626)	(0.487)
(Stressed) x (Tri 2)	0.00142	0.00387	-0.0105	-0.0124
	(0.937)	(0.833)	(0.611)	(0.472)
(Stressed) x (Tri 1)	0.0146	0.0195	0.0215	-0.00833
	(0.391)	(0.265)	(0.245)	(0.650)
P[Polluted x Tri 3 - Stressed x Tri 3]	0.212	0.212	0.202	0.493
P[Polluted x Tri 2 - Stressed x Tri 2]	0.155	0.160	0.432	0.887
P[Polluted x Tri 1 - Stressed x Tri 1]	0.200	0.218	0.245	0.205
Observations	7,444	7,444	5,416	7,444

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, and gestational age.

Erratic Wind Patterns: *Wind Analysis*. Prevailing wind direction is a useful metric for identifying more heavily polluted regions of the landscape and its usage is motivated by its prevalence in the extant literature. However, the precision of this metric is diminished if the wind patterns surround each fire change frequently over the course of each fires burn. We account for this potential bias using detailed information from each fire's ICS-209 report. In addition to indicating the prevailing wind direction at each burn site, these reports include a separate note indicating if the wind patterns on the day the fire begun were "erratic" or not. In Column (3) we re-estimate our baseline model restricting attention to the set of fires that did not receive an erratic wind flag. Coefficient estimates are qualitatively similar to those shown in our baseline model.

Contaminated Control Group: *Smoke Analysis*. Our baseline model using the sample of births which match to a fire with smoke data compares the outcomes of infants within one mile of a wildfire to the outcomes of infants in the immediately adjacent area, partitioning the set of treated infants based on their location with respect to each fire's smoke plume. This approach implicitly compares outcomes in each treatment group to the outcomes of births in a control group; some of which resided within the boundaries of a smoke plume located more than one mile of a wildfire. In Column (1) of Table (3.4) we replicate our baseline smoke model. In Column (2), we re-estimate the baseline model including a full set of group by fire and group by trimester indicator variables for the set infants located inside the smoke plume of a fire but located further than one mile. Coefficient estimates shown in Column (2) of Table (3.4) are similar to each corresponding coefficient estimate in our baseline specification; coefficient estimates of $Polluted \times Tri\ 3$ and $Polluted \times Tri\ 2$ are only marginally insignificant with p-values of .101 and .119, respectively.

The advantage of using wildfire smoke plumes is that they allow us to precisely identify portions of the landscape that were polluted. 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 each fire burned. Thus, there may exist births in the control group that may have been partially exposed to wildfire smoke, even if they were located outside of the smoke plumes that we identify. We mitigate this bias by constructing each plume in

Table 3.4: Robustness Checks: Smoke Model

	(1)	(2)	(3)
Fire Sample	**Smoke**	**Smoke**	**Smoke**
Robustness Check:	Baseline Model	Contaminated Controls (Smoke)	Contaminated Controls (Smoke + Wind)
	ln(bw)	*ln(bw)*	*ln(bw)*
(Polluted) x (Tri 3)	-0.0493*	-0.0488	-0.0649**
	(0.0905)	(0.101)	(0.0345)
(Polluted) x (Tri 2)	-0.0399*	-0.0360	-0.0394
	(0.0761)	(0.119)	(0.107)
(Polluted) x (Tri 1)	-0.0128	-0.00808	-0.00101
	(0.644)	(0.774)	(0.973)
(Stressed) x (Tri3)	-0.0282	-0.0267	-0.0176
	(0.606)	(0.628)	(0.756)
(Stressed) x (Tri 2)	0.0319	0.0341	0.0256
	(0.336)	(0.309)	(0.400)
(Stressed) x (Tri 1)	-0.0504	-0.0477	-0.0561
	(0.180)	(0.213)	(0.162)
P[Polluted x Tri 3 - Stressed x Tri 3]	0.730	0.719	0.465
P[Polluted x Tri 2 - Stressed x Tri 2]	0.070	0.079	0.097
P[Polluted x Tri 1 - Stressed x Tri 1]	0.418	0.398	0.278
Observations	4,736	4,736	4,736

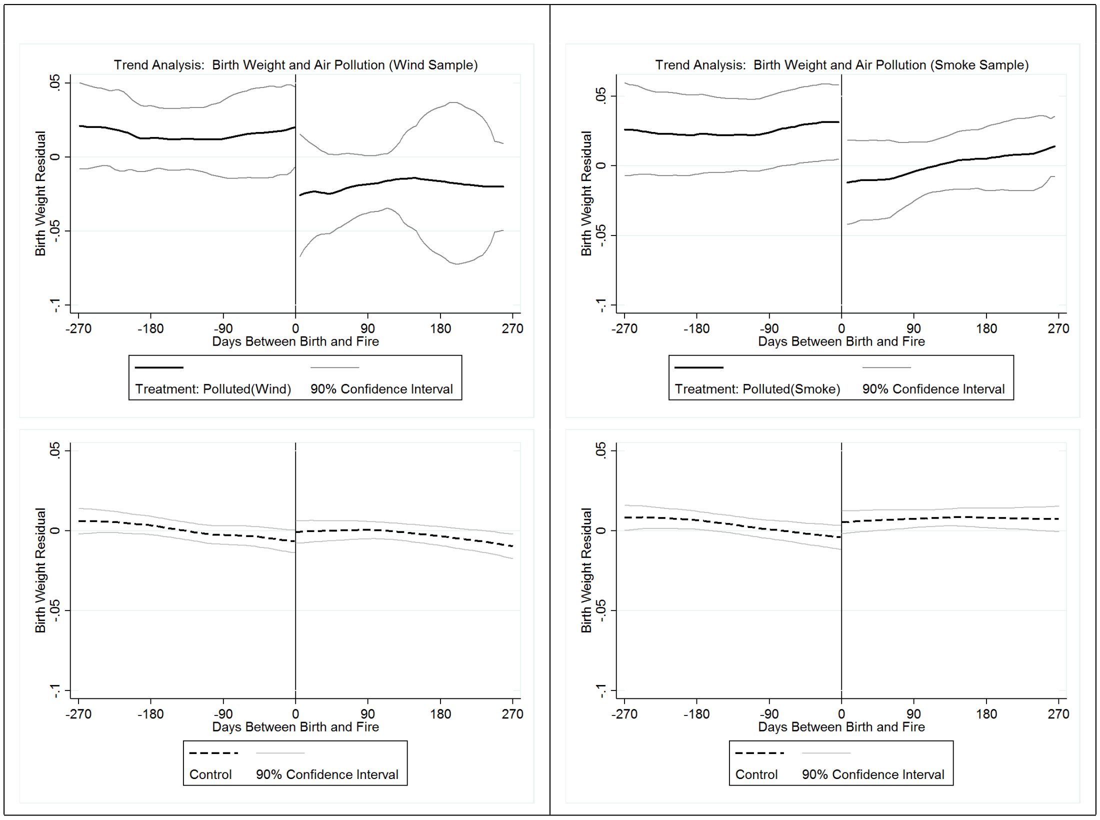
Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, and gestational age.

GIS on the basis of several images taken at different times of the day. We take an additional measure to control for this contamination by incorporating information on prevailing wind into our smoke analysis. Specifically, in Column (3) we re-estimate our baseline model in Column (1) including a full set of group by fire and group by trimester indicator variables for the set of infants located within a one to five mile ring of a wildfire that were either inside the smoke plume of the fire or that were downwind of the fire. As shown in Table (3.4), by including these variables, coefficient estimates for $Polluted \times Tri\ 3$ and $Polluted \times Tri\ 2$ increase (in absolute terms) from -4.8% and -3.6% to -6.5% and -4.0%, respectively.

3.5.2.2 Prior Trends In order for each of our difference-in-differences estimates to represent the causal effects of wildfire on health, 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. We must also assume that wildfires do not coincide with any unobserved shock 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.

Air Pollution: ***Wind & Smoke Analysis***. For each sub-sample of births in our wind and smoke analysis, we regress log birthweight on a set of year by quarter fixed effects, fire fixed effects, and demographic controls. In Figure (3.5), we fit treatment group-specific local polynomials on the residuals of these regressions. The figure at the top of panel (a) indicates the trends in the birthweight of infants located downwind and within one mile of a wildfire burn scar, together with a 90% confidence interval. This trend line shows the evolution of birthweight from infants included in the treatment group *Polluted* constructed based on prevailing wind. The bottom figure of panel (a) illustrates the birthweight trend for the set of infants located between one and five miles of a wildfire. In a similar fashion, the figure at the top of panel (b) plots the birthweight of infants located inside the smoke plume and within one mile of a wildfire. This trend line shows the evolution of birthweight for infants included in the treatment group *Polluted* constructed with wildfire smoke polygons. 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.

Stress: ***Wind and Smoke Analysis***. Next, we turn our attention to proximate upwind / out-of-the-smoke regions of fires. At the top of panel (a) in Figure (3.6), we plot birthweight trends for infants located upwind and within one mile of a wildfire, together with a 90% confidence interval. Births in this group are included in the treatment definition *Stressed* constructed on the basis of prevailing wind. The trend in the birthweight of infants



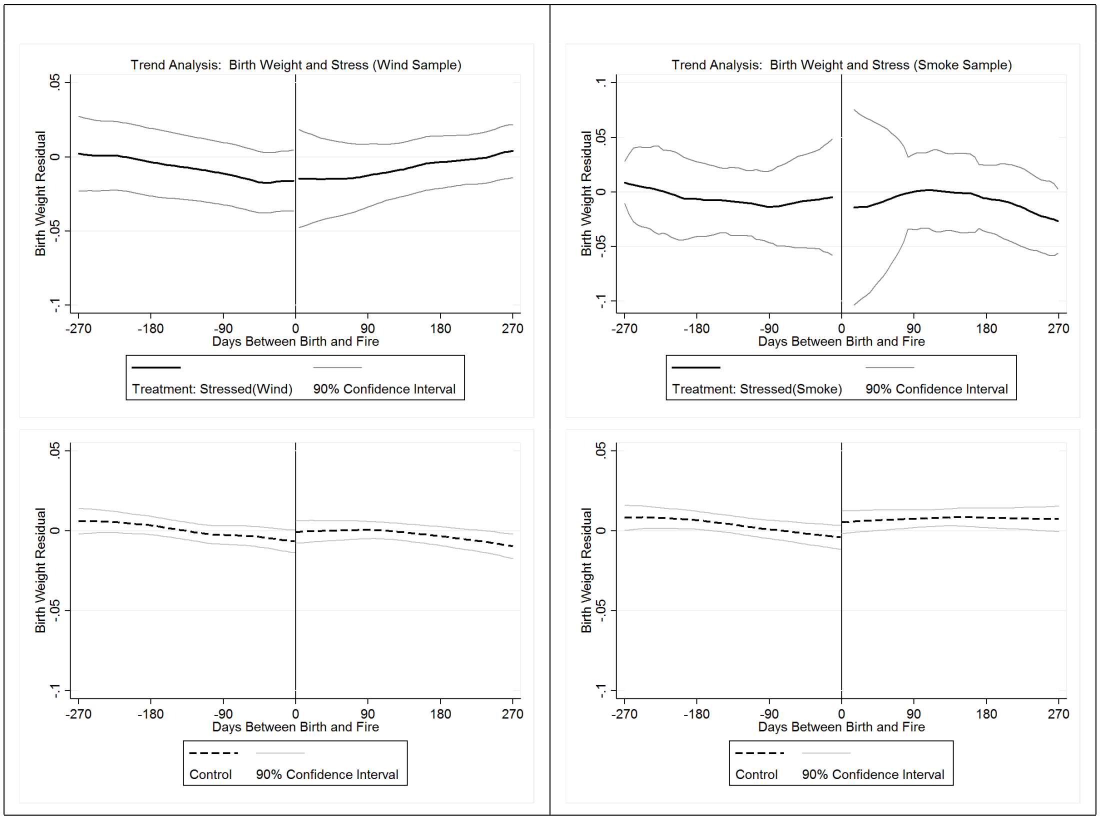
(a) Wind Sample (b) Smoke Sample

Figure 3.5: Trend Analysis: Air Pollution and Birth Weight

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 the smoke plume but within one mile of a wildfire; the trend in the birthweight of infants located in the control is illustrated in the bottom of panel (b). These figures also provide graphical evidence suggesting that the birthweight of infants in the treated and control groups follow similar trends leading up to a fire.

3.5.2.3 Model Sensitivity to Treatment Cutoff and the Presence of Spatial Decay

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 our models to the one mile definition. To do this, we re-estimate our baseline models as we in-



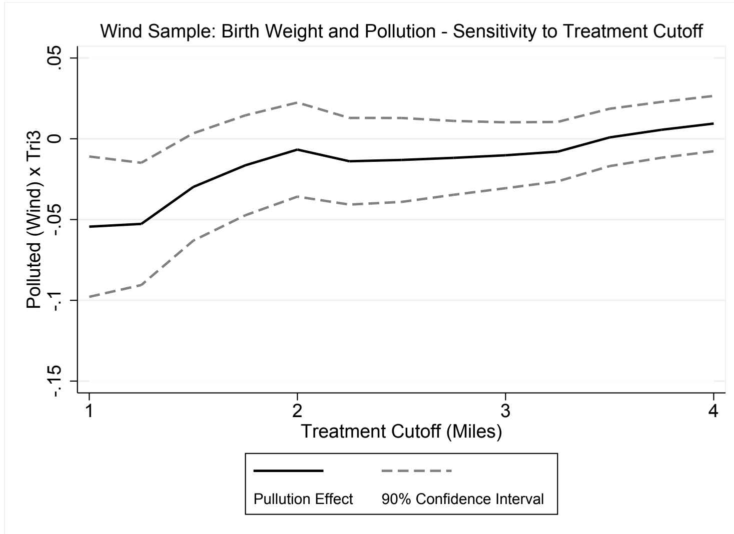
(a) Wind Sample

(b) Smoke Sample

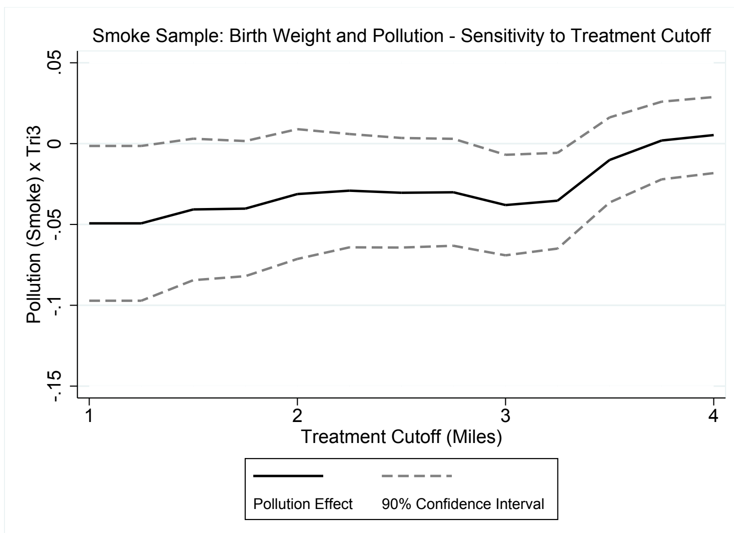
Figure 3.6: Trend Analysis: Stress - Proximate Upwind / Out of Smoke and Birth Weight

crease the size of the treatment cutoff in quarter mile increments. Figures (3.7) to (3.9) plot coefficient estimates for $\{Polluted \times Tri\ k\}_{k=1}^{3}$ obtained under each iteration, respectively. Coefficient estimates together with their 90% confidence intervals are shown on the x-axis with the treatment cutoff (in miles) shown on the y-axis. In each figure, model estimates obtained under our wind specification are shown in Panel (a) with model estimates obtained under our smoke specification shown in Panel (b).

Referring to Figure (3.7), model estimates of $Polluted \times Tri\ 3$ are larger (in absolute value) when we consider infants closer to fires, but as we consider the set of infants in polluted, but less proximate regions, model estimates decay. Estimates shown in panel (a) remain statistically significant up to a distance of 1.5 miles after which they quickly converge to zero. Beyond 3.5 miles, they are zero. Referring to panel (b), model estimates



(a) Wind Model



(b) Smoke Model

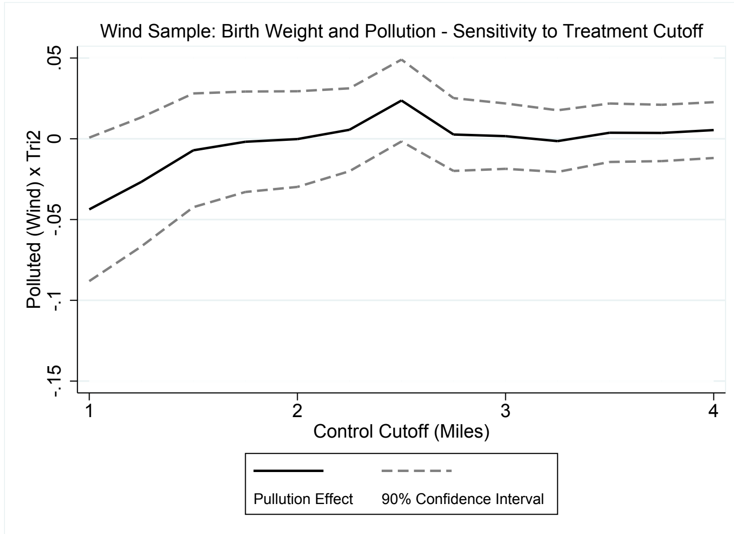
Figure 3.7: Sensitivity to Treatment Cutoff: Air Pollution & Birth Weight (Trimester 3 Effects)

of $Polluted \times Tri$ 3 decay at a slower rate when we identify polluted regions using wildfire smoke plumes. In general, these estimates are significant up to a distance of 3 miles. In both our wind and smoke analysis, trimester 3 effects decay to zero after 3.5 miles. This shows that while wildfire induced particle emissions result in decreased birthweight, these effects are highly localized.

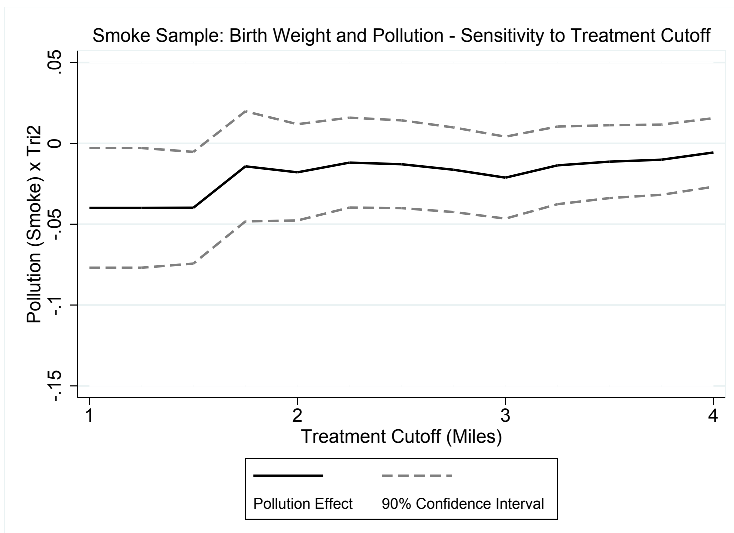
Referring to Figure (3.8), we see that coefficient estimates of $Polluted \times Tri$ 2 are statistically significant only when we consider the set of infants within 1 to 1.25 miles of a wildfire. In both our wind and smoke models, coefficient estimates converge to zero after two miles. We recall that estimates of $Polluted \times Tri$ 3 constructed using wildfire smoke polygons are significant up to a distance of 3 miles which suggests that infants between 1.25 and 3 miles are exposed to ambient air particles; hence, our finding that trimester 2 effects are insignificant in these regions shows that ambient air pollution may affect children in their second trimester of gestation at the time of a fire; albeit, only when they are exposed to more concentrated levels of particle emissions. Turning attention to the effects of pollution on infants exposed in their first trimester of gestation, Figure (3.9) shows that model estimates of $Polluted \times Tri$ 1 are statistically insignificant irrespective of which treatment definition we impose.

3.5.2.4 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 replicate the wind and smoke models shown in Columns (2) and (4) of Table (3.2) starting with a control cutoff of 5 miles. We then obtain sequential estimates of each coefficient in each model from reducing the control cutoff in quarter mile increments down to a distance of two miles. In Figure (3.10), we plot coefficient estimates of $Polluted \times Tri$ 3. Panel (a) reports coefficient estimates obtained under our wind specification. Panel (b) reports coefficient estimates obtained under our smoke specification. In each figure, coefficient estimates are plotted on the y-axis, together with their 90% confidence intervals. The control cutoff (in miles) is shown on the y-axis.

Figure (3.10) shows that model estimates of $Polluted \times Tri$ 3 are robust and stable to control definitions between two and five miles. 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.



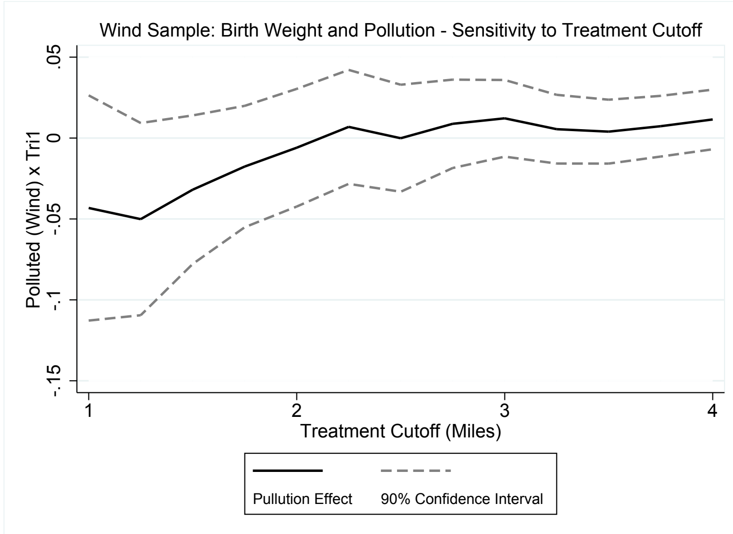
(a) Wind Model



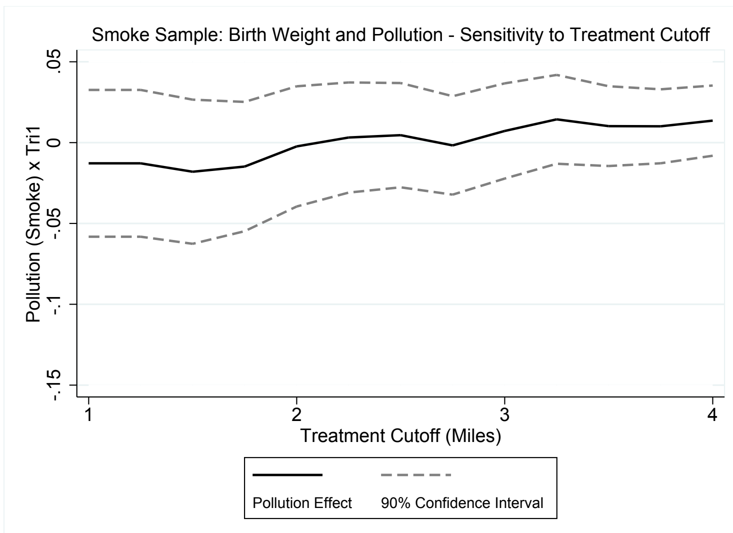
(b) Smoke Model

Figure 3.8: Sensitivity to Treatment Cutoff: Air Pollution & Birth Weight (Trimester 2 Effects)

3.5.2.5 Placebo Effects and Demographic Composition Our identification strategy requires that the composition of children born in each treatment and control group before fire is similar to the the composition of children born after fire. To test this assumption, we examine how mean demographic characteristics of infants in each treatment and control group change following a fire. To do this, we re-estimate our baseline wind model and our



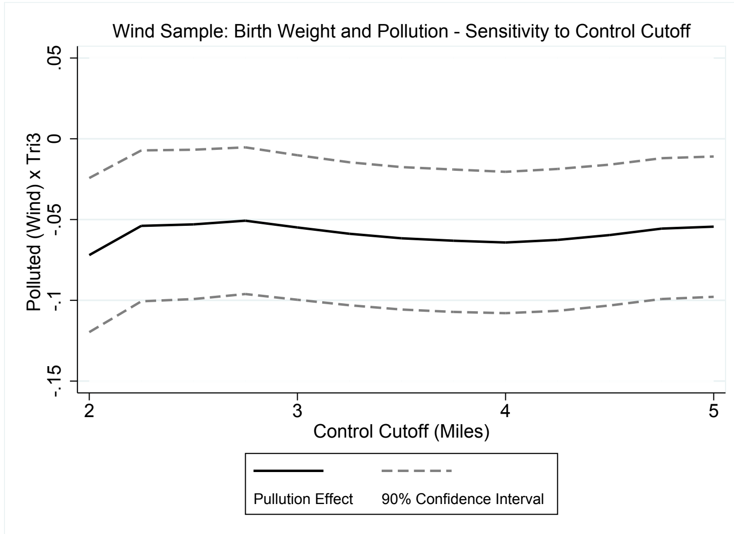
(a) Wind Model



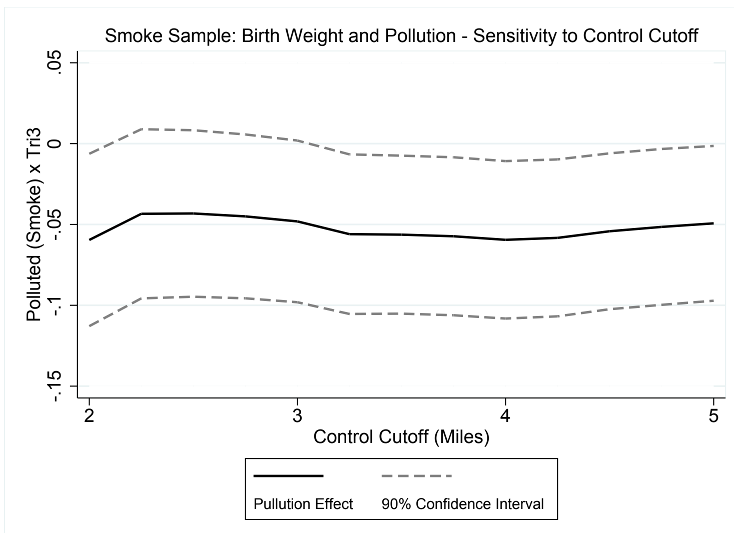
(b) Smoke Model

Figure 3.9: Sensitivity to Treatment Cutoff: Air Pollution & Birth Weight (Trimester 1 Effects)

baseline smoke model replacing birthweight with each demographic characteristic. Estimates based on equation (3.3) using wind to identify polluted areas are shown in Table (3.5). Estimates based on equation (3.6) using smoke plumes to identify polluted areas are shown in Table (3.6). Of the 60 coefficients we estimate, only five are statistically significant; only one of which is significant in both the wind and smoke specifications. These results provide no evidence to suggest that the composition of infants systematically changes following a



(a) Wind Model



(b) Smoke Model

Figure 3.10: Sensitivity to Control Cutoff: Air Pollution & Birthweight (Trimester 3 Effects)

fire.

3.5.3 Returns to Birthweight

We find that exposure to wildfire smoke in the third trimester of gestation leads to a 4% to 6% reduction in birthweight which translates into a 135-202 grams for the average infant in our sample. From a policy perspective, we are certainly interested in the long-run effects of

Table 3.5: Placebo Effects & Demographic Composition: Wind Model

Fire Sample	(1) **Wind**	(2) **Wind**	(3) **Wind**	(4) **Wind**	(5) **Wind**	(6) **Wind**
	Married	*White*	*Black*	*Hispanic*	*Education*	*Age*
(Polluted) x (Tri 3)	0.120	-0.0644	-0.0271	0.0738	-0.0266	0.0886
	(0.138)	(0.569)	(0.120)	(0.383)	(0.898)	(0.942)
(Polluted) x (Tri 2)	0.0182	-0.110	-0.0184	0.0914	0.0619	-0.397
	(0.813)	(0.322)	(0.459)	(0.301)	(0.787)	(0.773)
(Polluted) x (Tri 1)	0.0116	0.110	-0.0123	-0.0197	-0.177	0.862
	(0.869)	(0.246)	(0.460)	(0.815)	(0.448)	(0.559)
(Stressed) x (Tri3)	0.0387	0.0102	-0.00337	-0.00278	0.0321	1.027
	(0.510)	(0.871)	(0.751)	(0.957)	(0.833)	(0.204)
(Stressed) x (Tri 2)	0.0457	0.0112	-0.00178	0.0193	0.171	1.194*
	(0.384)	(0.850)	(0.824)	(0.702)	(0.247)	(0.0896)
(Stressed) x (Tri 1)	0.0143	0.000626	0.00284	0.0100	0.00536	-0.288
	(0.819)	(0.992)	(0.880)	(0.838)	(0.970)	(0.711)
Observations	7,444	7,444	7,444	7,444	7,444	7,444

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, birthweight, and gestational age.

Table 3.6: Placebo Effects & Demographic Composition: Smoke Model

Fire Sample	(1) **Smoke**	(2) **Smoke**	(3) **Smoke**	(4) **Smoke**	(5) **Smoke**	(6) **Smoke**
	Married	*White*	*Black*	*Hispanic*	*Education*	*Age*
(Polluted) x (Tri 3)	0.0953	0.0145	-0.000753	0.0338	0.186	0.418
	(0.277)	(0.871)	(0.973)	(0.581)	(0.418)	(0.688)
(Polluted) x (Tri 2)	0.00150	-0.0484	-0.00423	0.0227	0.120	0.0562
	(0.981)	(0.556)	(0.778)	(0.623)	(0.548)	(0.957)
(Polluted) x (Tri 1)	-0.00112	-0.0527	0.0342	0.0121	-0.0926	-0.874
	(0.989)	(0.587)	(0.437)	(0.843)	(0.602)	(0.430)
(Stressed) x (Tri3)	-0.0245	0.0276	-0.0269	-0.0199	-0.357	2.584
	(0.817)	(0.754)	(0.355)	(0.684)	(0.114)	(0.137)
(Stressed) x (Tri 2)	0.128**	-0.0432	-0.0208	0.105	0.242	2.156*
	(0.0305)	(0.656)	(0.441)	(0.223)	(0.271)	(0.0699)
(Stressed) x (Tri 1)	-0.0665	0.136**	-0.0464*	-0.0549	0.240	0.507
	(0.585)	(0.0177)	(0.0817)	(0.123)	(0.472)	(0.774)
Observations	4,736	4,736	4,736	4,736	4,736	4,736

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, birthweight, and gestational age.

ambient air pollution; however, our data are limited exclusively to short-run health metrics. 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 subsequently 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[11], 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.

3.5.4 Alternative Health Metrics

We now test if wildfire has the propensity to reduce the gestational age of infants by estimating variants of equation (3.1) using log gestational age as our dependent variable in Table (3.7). Referring to our results in Table (3.7), in no specification do we find evidence of a reduction in the gestational age of infants.

In Tables (3.8) and (3.9), we report estimates of the effects of the health characteristics of the pregnancies. These include: (1) the number of prenatal visits scheduled by each mother; (2) whether the infant was in a breech position; (3) whether the mother was diagnosed with gestational hypertension or pregnancy induced hypertension; (4) whether the infant was presented with rupture of membranes prior to the onset of labor; (5) whether the infants

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

Table 3.7: Difference-in-Differences Estimates: Gestational Age

	(1)	(2)	(3)	(4)
Fire Sample	**Wind**		**Smoke**	
	ln(gest)	*ln(gest)*	*ln(gest)*	*ln(gest)*
(1 Mile) x (Post)	-0.00391	-	-0.00749	-
	(0.344)	-	(0.189)	-
(Polluted) x (Post)	-	-0.00315	-	-0.00747
	-	(0.646)	-	(0.282)
(Stressed) x (Post)	-	-0.00370	-	-0.00553
	-	(0.453)	-	(0.592)
Observations	11,929	11,929	7,241	7,241

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, and year-quarter fixed effects.

had a seizure; and (6) whether the infant incurred a birth injury.

Table 3.8: Difference-in-Differences Estimates: Pregnancy Outcomes (Smoke Analysis)

	(1)	(2)	(3)	(4)	(5)	(6)
Fire Sample	**Wind**	**Wind**	**Wind**	**Wind**	**Wind**	**Wind**
	Number Prenatal Visits	*Fetal Presentation Breech*	*Gestational Hypertension*	*Premature Rupture of Membranes*	*Seizure*	*Birth Injury*
(Polluted) x (Tri 3)	0.132	-0.0197	0.0439	0.00563	-8.58e-05	0.00177*
	(0.937)	(0.249)	(0.450)	(0.400)	(0.741)	(0.0662)
(Polluted) x (Tri 2)	-0.414	-0.00509	-0.0270	0.00422	-0.00126	-9.76e-05
	(0.761)	(0.657)	(0.220)	(0.642)	(0.431)	(0.954)
(Polluted) x (Tri 1)	-1.678	0.0711	0.0556	0.000277	2.42e-05	-0.000236
	(0.485)	(0.196)	(0.418)	(0.972)	(0.923)	(0.854)
(Stressed) x (Tri3)	0.697	0.00732	-0.0123	0.00164	0.000299	0.000858
	(0.304)	(0.423)	(0.350)	(0.947)	(0.470)	(0.250)
(Stressed) x (Tri 2)	3.405	0.0152	5.81e-05	0.0179	-0.000723	-0.000177
	(0.137)	(0.406)	(0.998)	(0.515)	(0.560)	(0.894)
(Stressed) x (Tri 1)	0.349	0.0126	0.00372	-0.00504	0.000546	-0.000747
	(0.610)	(0.496)	(0.897)	(0.842)	(0.239)	(0.557)
Observations	7,382	7,444	7,444	7,444	7,444	7,444

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, birthweight, and gestational age.

Of the 72 coefficients we estimate, only three are significant. Referring to the estimate for $Polluted \times Tri\ 3$ in Column (1) of Table (3.9), we find a small and significant increase in the

Table 3.9: Difference-in-Differences Estimates: Pregnancy Outcomes (Wind Analysis)

Fire Sample	(1) **Smoke**	(2) **Smoke**	(3) **Smoke**	(4) **Smoke**	(5) **Smoke**	(6) **Smoke**
	Number Prenatal Visits	*Fetal Presentation Breech*	*Gestational Hypertension*	*Premature Rupture of Membranes*	*Seizure*	*Birth Injury*
(Polluted) x (Tri 3)	0.999*	-0.0207	0.0675	0.0197	0.000312	0.00269
	(0.0882)	(0.299)	(0.185)	(0.711)	(0.682)	(0.104)
(Polluted) x (Tri 2)	-0.179	-0.0209	0.0346	-0.0298	-0.00146	0.000321
	(0.781)	(0.261)	(0.241)	(0.168)	(0.581)	(0.906)
(Polluted) x (Tri 1)	0.0592	-0.0115	0.126*	-0.0271	0.000749	0.00134
	(0.937)	(0.504)	(0.0553)	(0.218)	(0.484)	(0.427)
(Stressed) x (Tri3)	0.373	-0.00109	-0.0343	-0.00251	0.000112	-0.00110
	(0.661)	(0.934)	(0.337)	(0.809)	(0.835)	(0.566)
(Stressed) x (Tri 2)	-1.594*	0.0706	-0.0409	-0.00736	-0.00109	-0.00184
	(0.0813)	(0.336)	(0.216)	(0.425)	(0.361)	(0.329)
(Stressed) x (Tri 1)	-1.729	0.0560	-0.0594	-0.00243	0.000825	-0.00155
	(0.114)	(0.400)	(0.112)	(0.840)	(0.260)	(0.403)
Observations	4,679	4,736	4,736	4,736	4,736	4,736

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, birthweight, and gestational age.

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 attended by mothers. However, neither of these coefficients are significant in our larger sample of fires with wind data. Specifically, referring to Column (1) of Table (3.8), coefficient estimates for $Polluted \times Tri\ 3$ and $Stressed \times Tri\ 2$ are .132 ($p < .937$) and 3.405 ($p < .137$), respectively. This discrepancy casts doubt on the validity of the findings reported in Column (1) of Table (3.9).

Turning attention to Column (6) of Table (3.8), estimates for $Polluted \times Tri\ 3$ suggest a small, but significant increase in the probability an infant incurred a birth injury. Abnormal birth injuries may include skeletal fractures, peripheral nerve injury, and/or soft tissue/solid organ hemorrhage that requires a medical intervention; however, our data do not include separate indicators for each of these factors. Coefficient estimates for $Polluted \times Tri\ 3$ are positive and insignificant when we consider our extended sample of fires with wind data in Table (3.9). Although, this estimate is only marginally insignificant with a p-value of .104.

To improve our confidence in these findings, we test the sensitivity of each estimate to more robust empirical specifications. In Column (1) of Tables (3.10) and (3.11) we replicate our baseline estimates of wildfire on birth injuries shown in Column (1) of Tables (3.8) and (3.9), respectively. Referring to the robust specifications in Columns (2) and (3) of Tables (3.10) and (3.11), coefficient estimates of $Polluted \times Tri\ 3$ become statistically insignificant in *every* model. These findings seem to rule out the possibility that exposure to ambient air pollution increases the risk a child experiences an injury at birth.

Table 3.10: Robustness Checks (Birth Injuries): Smoke Model

	(1)	(2)	(3)
Fire Sample	**Smoke**	**Smoke**	**Smoke**
Robustness Check:	Baseline Model	Contaminated Controls (Smoke)	Contaminated Controls (Smoke + Wind)
	ln(bw)	*ln(bw)*	*ln(bw)*
(Polluted) x (Tri 3)	0.00269	0.00249	0.00277
	(0.104)	(0.156)	(0.164)
(Polluted) x (Tri 2)	0.000321	0.00109	0.00154
	(0.906)	(0.315)	(0.527)
(Polluted) x (Tri 1)	0.00134	0.00132	0.00324
	(0.427)	(0.173)	(0.227)
(Stressed) x (Tri3)	-0.00110	-0.00148	-0.00124
	(0.566)	(0.366)	(0.427)
(Stressed) x (Tri 2)	-0.00184	-0.00127	-0.00109
	(0.329)	(0.394)	(0.350)
(Stressed) x (Tri 1)	-0.00155	-0.00177	-0.000547
	(0.403)	(0.203)	(0.510)
Observations	4,736	4,736	4,736

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, birthweight, and gestational age.

3.6. CONCLUSIONS

We construct a novel data set which combines geocoded vital statistics records with geospatial data delineating the location of wildfires, the prevailing winds surrounding each fire,

Table 3.11: Robustness Checks (Birth Injuries): Wind Model

	(1)	(2)	(3)
Fire Sample	**Wind**	**Wind**	**Wind**
Robustness Check:	Baseline Model	Contaminated Controls	Erratic Wind
	birth injury	*birth injury*	*birth injury*
(Polluted) x (Tri 3)	0.00177*	0.00180	0.00173
	(0.0662)	(0.134)	(0.142)
(Polluted) x (Tri 2)	-9.76e-05	-0.000659	-0.000864
	(0.954)	(0.789)	(0.679)
(Polluted) x (Tri 1)	-0.000236	0.000348	0.000408
	(0.854)	(0.862)	(0.767)
(Stressed) x (Tri3)	0.000858	0.000971	0.00107
	(0.250)	(0.240)	(0.195)
(Stressed) x (Tri 2)	-0.000177	-0.000631	-0.000816
	(0.894)	(0.757)	(0.646)
(Stressed) x (Tri 1)	-0.000747	-0.000239	-0.000669
	(0.557)	(0.895)	(0.718)
Observations	7,444	7,444	5,416

Notes: P-values are reported in parenthesis with ***p<.01, **p<.05, and *p<.1 calculated using robust standard errors. Models include demographic and geographic controls as specified in Section 4, year-quarter fixed effects, birthweight, and gestational age.

and the actual path of wildfire smoke. These data allow us to disentangle the health impacts of ambient air pollution from the health effects of stress. They also facilitate our analysis of the spatial decay process between ambient air pollution, proximity to wildfire, and fetal health. Our study provides new insight into the physiological impacts of air pollution and addresses many of the identification problems faced by researchers in the extant literature including imprecise air-quality assessments and potential bias due to geographical sorting.

Our empirical results suggest that wildfire reduces the birthweight of infants that were exposed to smoke in their third trimester of gestation and located within 3 miles of a wildfire on the order of 4% to 6%. On the basis of our point estimates, exposure to wildfire smoke leads to a statistically significant 135g to 202g reduction in birthweight. Estimates in the extant literature show that this effect will likely translate into poorer long-run health outcomes including lower educational attainment, lower adult-height, and lower full-time earnings. Infants exposed to wildfire smoke in their second trimester of gestation are also at risk of lower birthweight on the order of 3% to 4%; albeit, only if they are located within 1.25 miles of a burn area. We find no evidence that exposure to wildfire reduces the birthweight of infants located upwind of a fire or outside of a wildfire smoke plume. These results do not rule out the possibility that natural disasters place significant physical and psychological stress on nearby residents. However, these findings effectively show that if these stressors are present, they are not strong enough to translate into poorer fetal health outcomes. Finally, with respect to the extant literature concerned with estimating the physiological effects of ambient air pollution, our results highlight the importance of properly assessing the spatial variation of fine-particulate matter around polluting sites. Avenues for future research unexplored in this study are the links between exposure to wildfire smoke, longer-term health outcomes and cognitive impairments.

BIBLIOGRAPHY

Aizer, A., Stroud, L., and Buka, S. (2009). Maternal stress and child well-being: Evidence from siblings. *Unpublished manuscript, Brown University, Providence, RI*.

Anderson, S., Plantinga, A. J., Wibbenmeyer, M., and Hodges, H. (2014). Salience of wildfire risk and the management of public lands. *Paper prepared for presentation at The Politics and Economics of Wildfire Conference, Bren School, University of California, Santa Barbara. October 26-28, 2014*.

Archer, W. R., Ling, D. C., and Smith, B. C. (2010). Ownership duration in the residential housing market: The influence of structure, tenure, household and neighborhood factors. *The Journal of Real Estate Finance and Economics*, 40(1):41–61.

Atreya, A. and Ferreira, S. (2014). Seeing is believing? evidence from property prices in inundated areas. *Risk Analysis*.

Atreya, A., Ferreira, S., and Kriesel, W. (2012). Forgetting the flood: Changes in flood risk perceptions over time. *" Flood Risk and Homeowners' Flood Risk Perceptions: Evidence from Property Prices in Georgia*, page 5.

Atreya, A., Ferreira, S., and Kriesel, W. (2013). Forgetting the flood? An analysis of the flood risk discount over time. *Land Economics*, 89(4):577–596.

Banzhaf, H. S. and Walsh, R. P. (2008). Do people vote with their feet? an empirical test of tiebout's mechanism. *The American Economic Review*, pages 843–863.

Beatty, T. K. and Shimshack, J. P. (2011). School buses, diesel emissions, and respiratory health. *Journal of Health Economics*, 30(5):987–999.

Berkowitz, G. S., Wolff, M. S., Janevic, T. M., Holzman, I. R., Yehuda, R., and Landrigan, P. J. (2003). The world trade center disaster and intrauterine growth restriction. *Jama*, 290(5):595–596.

Bin, O. and Landry, C. E. (2012). Changes in implicit flood risk premiums: Empirical evidence from the housing market. *Journal of Environmental Economics and Management*.

Bin, O. and Landry, C. E. (2013). Changes in implicit flood risk premiums: Empirical evidence from the housing market. *Journal of Environmental Economics and Management*, 65(3):361–376.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2007). From the cradle to the labor market? the effect of birth weight on adult outcomes. *The Quarterly Journal of Economics*, 122(1):409–439.

Blake, E. S., Kimerlain, T. B., Berg, R. J., Cangialosi, J. P., and Beven II, J. L. (2013). Tropical cyclone report hurricane sandy al182012. Technical report, National Hurricane Center.

Boehm, T. P. and Ihlanfeldt, K. R. (1986). The improvement expenditures of urban homeowners: An empirical analysis. *Real Estate Economics*, 14(1):48–60.

Bordalo, P., Gennaioli, N., and Shleifer, A. (2012). Salience theory of choice under risk. *The Quarterly Journal of Economics*, 127(3):1243–1285.

Brauer, M., Hoek, G., van Vliet, P., Meliefste, K., Fischer, P., Gehring, U., Heinrich, J., Cyrys, J., Bellander, T., Lewne, M., et al. (2003). Estimating long-term average particulate air pollution concentrations: application of traffic indicators and geographic information systems. *Epidemiology*, 14(2):228–239.

Brody, S. D., Zahran, S., Maghelal, P., Grover, H., and Highfield, W. E. (2007). The rising costs of floods: Examining the impact of planning and development decisions on property damage in florida. *Journal of the American Planning Association*, 73(3):330–345.

Burby, R. J. (2001). Flood insurance and floodplain management: the us experience. *Global Environmental Change Part B: Environmental Hazards*, 3(3):111–122.

Campbell, J. Y., Giglio, S., and Pathak, P. (2011). Forced sales and house prices. *American Economic Review*, 101(5):2108–31.

Champ, P. A., Donovan, G. H., and Barth, C. M. (2009). Homebuyers and wildfire risk: A Colorado Springs case study. *Society & Natural Resources*, 23(1):58–70.

Chay, K. Y. and Greenstone, M. (2003). The impact of air pollution on infant mortality: Evidence from geographic variation in pollution shocks induced by a recession. *Quarterly Journal of Economics*, 118(3):1121.

Colorado Wildfire Risk Assessment Project: Final Report (2013). Colorado State Forest Service.

Conroy, M. J., Allen, C. R., Pritchard Jr, L., Moore, C. T., and Peterson, J. T. (2003). Landscape change in the southern Piedmont: Challenges, solutions, and uncertainty across scales. *Conservation Ecology (11955449)*, 8(2).

Currie, J., Davis, L., Greenstone, M., Walker, R., et al. (2015). Environmental health risks and housing values: Evidence from 1,600 toxic plant openings and closings. *American Economic Review*, 105(2):678–709.

Currie, J. and Neidell, M. (2005). Air pollution and infant health: What can we learn from california's recent experience? *Quarterly Journal of Economics*, 120(3).

Currie, J., Neidell, M., and Schmieder, J. F. (2009). Air pollution and infant health: Lessons from new jersey. *Journal of health economics*, 28(3):688–703.

Currie, J. and Rossin-Slater, M. (2013). Weathering the storm: Hurricanes and birth outcomes. *Journal of health economics*, 32(3):487–503.

Currie, J. and Walker, R. (2011). Traffic congestion and infant health: Evidence from e-zpass. *American Economic Journal. Applied Economics*, 3(1):65.

Dejmek, J., Selevan, S. G., Benes, I., Solanskỳ, I., and Srám, R. J. (1999). Fetal growth and maternal exposure to particulate matter during pregnancy. *Environmental Health Perspectives*, 107(6):475.

Dixon, L. S., Turner, S., Clancy, N., Seabury, S. A., and Overton, A. (2006). *The National Flood Insurance Program's market penetration rate: Estimates and policy implications.* RAND Santa Monica, CA.

Donovan, G. H., Champ, P. A., and Butry, D. T. (2007). Wildfire risk and housing prices: A case study from Colorado Springs. *Land Economics*, 83(2):217–233.

Downing, C. and Wallace, N. (2000). *A real options approach to housing investment.* Divisions of Research & Statistics and Monetary Affairs, Federal Reserve Board.

Dunkel Schetter, C. (2011). Psychological science on pregnancy: stress processes, biopsychosocial models, and emerging research issues. *Annual review of psychology*, 62:531–558.

Furman Center for Real Estate and Urban Policy (2013). Sandy's effects on housing in new york city. *Furman Center and the Moelis Institute for Affordable Housing Policy Fact Brief*.

Gallagher, J. (2014). Learning about an infrequent event: evidence from flood insurance take-up in the united states. *American Economic Journal: Applied Economics*, 6(3):206–233.

Gilbert, D., Coussens, C., Wilson, S., Mattison, D. R., et al. (2003). *The Role of Environmental Hazards in Premature Birth:: Workshop Summary*. National Academies Press.

Gillett, N., Weaver, A., Zwiers, F., and Flannigan, M. (2004). Detecting the effect of climate change on canadian forest fires. *Geophysical Research Letters*, 31(18).

Glinianaia, S. V., Rankin, J., Bell, R., Pless-Mulloli, T., and Howel, D. (2004). Particulate air pollution and fetal health: a systematic review of the epidemiologic evidence. *Epidemiology*, 15(1):36–45.

Goodwin, C. (2015). Air quality and birth outcomes, the mount st. helens eruption, a natural experiment. Technical report, Job Market Papers.

Hallstrom, D. G. and Smith, V. K. (2005). Market responses to hurricanes. *Journal of Environmental Economics and Management*, 50(3):541–561.

Hansen, J. L., Benson, E. D., and Hagen, D. A. (2006). Environmental hazards and residential property values: Evidence from a major pipeline event. *Land Economics*, 82(4):529–541.

Haughwout, A., Sutherland, S., and Tracy, J. (2013). Negative equity and housing investment. Technical report, Staff Report, Federal Reserve Bank of New York.

Holstius, D. M., Reid, C. E., Jesdale, B. M., and Morello-Frosch, R. (2012). Birth weight following pregnancy during the 2003 southern california wildfires. *Environmental health perspectives*, 120(9):1340–1345.

Hueglin, C., Gaegauf, C., Künzel, S., and Burtscher, H. (1997). Characterization of wood combustion particles: morphology, mobility, and photoelectric activity. *Environmental science & technology*, 31(12):3439–3447.

Huggett Jr, R. J., Murphy, E. A., and Holmes, T. P. (2008). Forest disturbance impacts on residential property values. In *The Economics of Forest Disturbances*, pages 209–228. Springer.

Huynh, M., Woodruff, T. J., Parker, J. D., and Schoendorf, K. C. (2006). Relationships between air pollution and preterm birth in california. *Paediatric and perinatal epidemiology*, 20(6):454–461.

Jaffe, D., Hafner, W., Chand, D., Westerling, A., and Spracklen, D. (2008). Interannual variations in pm2. 5 due to wildfires in the western united states. *Environmental science & technology*, 42(8):2812–2818.

Jayachandran, S. (2009). Air quality and early-life mortality evidence from indonesia's wildfires. *Journal of Human Resources*, 44(4):916–954.

Johnston, F. H., Kavanagh, A. M., Bowman, D., and Scott, R. K. (2002). Exposure to bushfire smoke and asthma: an ecological study. *The Medical Journal of Australia*, 176(11):535–538.

Kahneman, D. and Tversky, A. (1979). Prospect theory: An analysis of decision under risk. *Econometrica: Journal of the Econometric Society*, pages 263–291.

Karr, C. J., Rudra, C. B., Miller, K. A., Gould, T. R., Larson, T., Sathyanarayana, S., and Koenig, J. Q. (2009). Infant exposure to fine particulate matter and traffic and risk of hospitalization for rsv bronchiolitis in a region with lower ambient air pollution. *Environmental research*, 109(3):321–327.

Kearl, J. R. (1979). Inflation, mortgage, and housing. *The Journal of Political Economy*, pages 1115–1138.

Khawand, C. (2015). Air Quality, Mortality, and Perinatal Health: Causal Evidence from Wildfires. 2015 Papers pkh318, Job Market Papers.

Knowlton, K. (2013). Where there's fire, there's smoke: Wildfire smoke affects communities distant from deadly flames. *NRDC Issue Brief*.

Kochi, I., Champ, P. A., Loomis, J. B., and Donovan, G. H. (2012). Valuing mortality impacts of smoke exposure from major southern california wildfires. *Journal of forest economics*, 18(1):61–75.

Kousky, C. (2010). Learning from extreme events: Risk perceptions after the flood. *Land Economics*, 86(3):395–422.

Kunreuther, H. C. and Michel-Kerjan, E. O. (2007). Climate change, insurability of large-scale disasters and the emerging liability challenge. Technical report, National Bureau of Economic Research.

Kunzli, N., Avol, E., Wu, J., Gauderman, W. J., Rappaport, E., Millstein, J., Bennion, J., McConnell, R., Gilliland, F. D., Berhane, K., et al. (2006). Health effects of the 2003 southern california wildfires on children. *American journal of respiratory and critical care medicine*, 174(11):1221–1228.

Lee, S. J., Hajat, S., Steer, P. J., and Filippi, V. (2008). A time-series analysis of any short-term effects of meteorological and air pollution factors on preterm births in london, uk. *Environmental research*, 106(2):185–194.

Leem, J.-H., Kaplan, B. M., Shim, Y. K., Pohl, H. R., Gotway, C. A., Bullard, S. M., Rogers, J. F., Smith, M. M., and Tylenda, C. A. (2006). Exposures to air pollutants during pregnancy and preterm delivery. *Environmental health perspectives*, pages 905–910.

Lipsett, M. and Materna, B. (2008). *Wildfire smoke: a guide for public health officials*. Office of Environmental Health Hazard Assessment.

Liu, S., Krewski, D., Shi, Y., Chen, Y., and Burnett, R. T. (2007). Association between maternal exposure to ambient air pollutants during pregnancy and fetal growth restriction. *Journal of Exposure Science and Environmental Epidemiology*, 17(5):426–432.

Loomis, J. (2004). Do nearby forest fires cause a reduction in residential property values? *Journal of Forest Economics*, 10(3):149–157.

Mallia, D., Lin, J., Urbanski, S., Ehleringer, J., and Nehrkorn, T. (2015). Impacts of upwind wildfire emissions on co, co2, and pm2. 5 concentrations in salt lake city, utah. *Journal of Geophysical Research: Atmospheres*, 120(1):147–166.

McCluskey, J. J. and Rausser, G. C. (2001). Estimation of perceived risk and its effect on property values. *Land Economics*, 77(1):42–55.

McCoy, S. J. and Walsh, R. P. (2014). Wui on fire: Risk, salience & housing demand. Technical report, National Bureau of Economic Research.

Moeltner, K., Kim, M.-K., Zhu, E., and Yang, W. (2013). Wildfire smoke and health impacts: A closer look at fire attributes and their marginal effects. *Journal of Environmental Economics and Management*, 66(3):476–496.

Muehlenbachs, L., Spiller, E., and Timmins, C. (2014). The housing market impacts of shale gas development. *NBER Working Paper No. 19796*.

Mueller, J., Loomis, J., and Gonzalez-Caban, A. (2009). Do repeated wildfires change homebuyers demand for homes in high-risk areas? A hedonic analysis of the short and long-term effects of repeated wildfires on house prices in Southern California. *The Journal of Real Estate Finance and Economics*, 38(2):155–172.

Mueller, J. M. and Loomis, J. B. (2008). Spatial dependence in hedonic property models: Do different corrections for spatial dependence result in economically significant differences in estimated implicit prices? *Journal of Agricultural and Resource Economics*, pages 212–231.

Mueller, J. M. and Loomis, J. B. (2014). Does the estimated impact of wildfires vary with the housing price distribution? A quantile regression approach. *Land Use Policy*, 41(0):121 – 127.

Mulder, E., De Medina, P. R., Huizink, A., Van den Bergh, B., Buitelaar, J., and Visser, G. (2002). Prenatal maternal stress: effects on pregnancy and the (unborn) child. *Early human development*, 70(1):3–14.

Naoi, M., Seko, M., and Sumita, K. (2009). Earthquake risk and housing prices in Japan: Evidence before and after massive earthquakes. *Regional Science and Urban Economics*, 39(6):658 – 669.

Parker, J. D., Mendola, P., and Woodruff, T. J. (2008). Preterm birth after the utah valley steel mill closure: a natural experiment. *Epidemiology*, 19(6):820–823.

Plaut, P. O. and Plaut, S. E. (2010). Decisions to renovate and to move. *Journal of Real Estate Research*, 32(4):461–484.

Ponce, N. A., Hoggatt, K. J., Wilhelm, M., and Ritz, B. (2005). Preterm birth: the interaction of traffic-related air pollution with economic hardship in los angeles neighborhoods. *American journal of epidemiology*, 162(2):140–148.

Pope III, C. A. (2000). Review: epidemiological basis for particulate air pollution health standards. *Aerosol Science & Technology*, 32(1):4–14.

Poterba, J. M. (1984). Tax subsidies to owner-occupied housing: an asset-market approach. *The quarterly journal of economics*, pages 729–752.

Radeloff, V. C., Hammer, R. B., Stewart, S. I., Fried, J. S., Holcomb, S. S., and McKeefry, J. F. (2005). The wildland-urban interface in the United States. *Ecological Applications*, 15(3):799–805.

Ritz, B., Wilhelm, M., and Zhao, Y. (2006). Air pollution and infant death in southern california, 1989–2000. *Pediatrics*, 118(2):493–502.

Salam, M. T., Millstein, J., Li, Y.-F., Lurmann, F. W., Margolis, H. G., and Gilliland, F. D. (2005). Birth outcomes and prenatal exposure to ozone, carbon monoxide, and particulate matter: results from the children's health study. *Environmental health perspectives*, pages 1638–1644.

Seaton, A., Godden, D., MacNee, W., and Donaldson, K. (1995). Particulate air pollution and acute health effects. *The Lancet*, 345(8943):176–178.

Severnini, E. R. (2014). Air pollution, power grid, and infant health: Evidence from the shutdown of tva nuclear power plants in the 1980s.

Shafran, A. P. (2008). Risk externalities and the problem of wildfire risk. *Journal of Urban Economics*, 64(2):488–495.

Simeonova, E. (2011). Out of sight, out of mind? natural disasters and pregnancy outcomes in the usa. *CESifo Economic Studies*, 57(3):403–431.

Slama, R., Morgenstern, V., Cyrys, J., Zutavern, A., Herbarth, O., Wichmann, H.-E., Heinrich, J., Group, L. S., et al. (2007). Traffic-related atmospheric pollutants levels during pregnancy and offspring's term birth weight: a study relying on a land-use regression exposure model. *Environmental Health Perspectives*, pages 1283–1292.

Spyratos, V., Bourgeron, P. S., and Ghil, M. (2007). Development at the wildland–urban interface and the mitigation of forest-fire risk. *Proceedings of the National Academy of Sciences*, 104(36):14272–14276.

Steelman, T. A. (2008). Addressing the mitigation paradox at the community level. *Wildfire risk: Human perceptions and management implications. (Eds W Martin, C Raish, B Kent) pp*, pages 64–80.

Stetler, K. M., Venn, T. J., and Calkin, D. E. (2010). The effects of wildfire and environmental amenities on property values in Northwest Montana, USA. *Ecological Economics*, 69(11):2233–2243.

Sunak, Y. and Madlener, R. (2012). The impact of wind farms on property values: A geographically weighted hedonic pricing model. *FCN Working Paper No. 3/2012*.

Taylor, S. E. and Thompson, S. C. (1982). Stalking the elusive "vividness" effect. *Psychological Review*, 89(2):155.

Tobin, G. A. and Montz, B. E. (1988). Catastrophic flooding and the response of the real estate market. *The Social Science Journal*, 25(2):167–177.

Tobin, G. A. and Montz, B. E. (1997). The impacts of a second catastrophic flood on property values in Linda and Olivehurst, California. *Natural Hazards Research and Applications Center, University of Colorado, Boulder*.

Torche, F. (2011). The effect of maternal stress on birth outcomes: exploiting a natural experiment. *Demography*, 48(4):1473–1491.

Travis, W. R., Theobald, D. M., and Fagre, D. (2002). Transforming the rockies: Human forces, settlement patterns, and ecosystem effects. *Rocky Mountain futures: An Ecological Perspective. Island Press, Washington, DC*, pages 1–24.

Troy, A. and Romm, J. (2004). Assessing the price effects of flood hazard disclosure under the California natural hazard disclosure law (AB 1195). *Journal of Environmental Planning and Management*, 47(1):137–162.

Tversky, A. and Kahneman, D. (1974). Judgment under uncertainty: Heuristics and biases. *Science*, 185(4157):1124–1131.

Urbanski, S. P., Hao, W. M., and Nordgren, B. (2011). The wildland fire emission inventory: western united states emission estimates and an evaluation of uncertainty. *Atmospheric Chemistry and Physics*, 11(24):12973–13000.

U.S. Geological Survey (USGS) (2006). Flood hazards: A national threat. *U.S. Geological Survey Fact Sheet 2006-3026*.

Walls, M., Kousky, C., and Chu, Z. (2013). Is what you see what you get? The value of natural landscape views (August 2013). *Resources for the Future Discussion Paper*, (13-25).

Wang, X., Ding, H., Ryan, L., and Xu, X. (1997). Association between air pollution and low birth weight: a community-based study. *Environmental Health Perspectives*, 105(5):514.

Westerling, A. L., Hidalgo, H. G., Cayan, D. R., and Swetnam, T. W. (2006). Warming and earlier spring increase western US forest wildfire activity. *Science*, 313(5789):940–943.

Wildland Fire Management: Federal agencies have taken steps forward but additional action is needed to address remaining challenges (2013). GAO.

Wilhelm, M. and Ritz, B. (2003). Residential proximity to traffic and adverse birth outcomes in los angeles county, california, 1994-1996. *Environmental Health Perspectives*, 111(2):207.

Woodruff, T. J., Darrow, L. A., and Parker, J. D. (2008). Air pollution and postneonatal infant mortality in the united states, 1999-2002. *Environmental Health Perspectives*, pages 110–115.

Xiong, X., Harville, E. W., Mattison, D. R., Elkind-Hirsch, K., Pridjian, G., and Buekens, P. (2008). Exposure to hurricane katrina, post-traumatic stress disorder and birth outcomes. *The American journal of the medical sciences*, 336(2):111.

Zhang, X., Kondragunta, S., Kogan, F., Tarpley, J. D., Guo, W., and Schmidt, C. (2006). Satellite-derived pm2. 5 emissions from wildfires for air quality forecast. In *15th Annual Emission Inventory Conference, May 16–18, 2006, in New Orleans*. Citeseer.