Essays in Applied Health Economics

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I would like to dedicate this dissertation

To my mother, father, and sister who always guide me with love in the right direction.

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Abstract

I study how change in environmental factors and information provision about environmental risk effect on human capitals and individuals' behavior.

Psychological maternal stress is thought to be a factor in poor infant health, but direct evidence is difficult to obtain. In first chapter, we posit that the 1994 Northridge earthquake in Los Angeles, California provides a natural test of the effect of mothers' stress on infants' birth weight and gestation. The Northridge disaster featured a low rate of injury and a quick recovery, but long-lasting and well documented consequences for mental health. Difference-in-difference results show that infants born closest to the epicenter were 0.2 percentage points more likely to be born with low birth weight.

In second chapter, I examine the effect of air quality information on population change in counties of California, Oregon, and Washington State by measuring change in the number of air alerts days. Based on panel data between 1999-2014, the results find a negative relationship between more frequent air quality alerts and change in the total population of a county. In particular, only "Unhealthy" which is the air quality alert category that contains more polluted days than "Unhealthy for sensitive groups", has significant effect on the change in population. Those results are consistent with the idea that individuals are responsive to air quality and willing to move.

Providing information is an important policy tool to encourage individuals' behavioral response. In my third paper, I focus on the role of prevalence and design of information by examining the effect of the EPA's map of radon zones, which is characterized as non-pervasive and unsophisticated in terms of its information on housing price in California counties. Using difference-in-difference estimator, I find that after the map is released, there is no difference in the house price between high radon level counties and low radon level

counties. These results are consistent with the previous studies showing that individuals change their behavior when the information is well-specified as well as is widely available to the public.

Table of Contents

List of Tables

List of Figures

Chapter 1

Maternal stress and birth outcomes: Evidence from the 1994 Northridge earthquake

1.1 Introduction

> "It moved into every corner of the landscape, into every home, into every life." [67]

Poor health at birth, often signaled by low birth weight or prematurity, is strongly linked to weaker health and development later in life [29, 12, 51]. There are many known determinants of low birth weight and preterm birth, including maternal health, economic deprivation, and limited access to proper nutrition and prenatal care during pregnancy. Maternal psychological stress is also thought to be a key risk factor in birth outcomes. In clinical research, changes in hormone levels due to stress are linked to an increased probability of low birth weight and prematurity [54, 59, 61, 38].[1] It is very difficult, however, to identify stress as a causal factor in infant health. The stylized correlation between mothers' stress and birth outcomes is complicated by the fact that many of the same conditions that result

[1] Stress can also indirectly lead to low birth weight via smoking, drinking, unhealthy dietary habits, or other compensating behaviors by expectant mothers.

in stress (e.g., financial uncertainty, substance abuse, or single motherhood) likely have independent effects on infant health.

We are not the first to investigate the effects of plausibly exogenous and stressful events on birth outcomes. Armed conflict [73], proximity to landmines [20], the September 11^{th} terrorist attacks [15], job loss [70], economic collapse (Bozzoli and Quintana-Domeque, 2014), and blackouts [16] have all been shown to harm newborn health. A subset of this literature assesses the effect of natural disasters on infant outcomes, including hurricanes and tropical storms [31], a major earthquake in a developing country [89], and other adverse weather events including smaller earthquakes [86]. While estimated effects from these events vary considerably, the consensus is that disasters – both man made and natural – affect birth weight and gestation length. However, attributing a causal role for psychological stress alone is difficult because most disasters impose both physical and psychological stress by limiting access to nutrition, clean water, safe living conditions, prenatal care, or other inputs to maternal or fetal health. To the extent that any of these events are anticipated, results may in part reflect compositional changes in birth cohorts *prior* to the disaster in question.

We join Brown [15] in assessing the *in utero* effects of a disaster that entailed far more psychological stress than bodily harm, but in a context where difference-in-difference identification is possible. The 1994 Northridge earthquake, registering a 6.7 moment magnitude and centered near Los Angeles, California, offers a source of exogenous variation in large-scale maternal stress that is minimally confounded by physical stress or endogenous *ex ante* migration. An earthquake is a purely unanticipated disaster that elicits stress (in varying degrees) among individuals in the affected area regardless of their socioeconomic status.

Using 1992-1995 data from the National Center for Health Statistics (NCHS), we analyze birth outcomes before and after the earthquake.[2] We evaluate how intertemporal variation in infants' birth weight and gestation differed by mothers' likely proximity to the epicenter. The Los Angeles area population tends to be more socioeconomically and environmentally challenged, but our empirical strategy balances characteristics of parents and expectations of

[2]We do not consider births after 1995 because of the possibility of endogenous longer-term mobility following the earthquake. Results are broadly robust to including 1991 births or excluding 1995 births (see Section 1.5.1).

newborn health with rich controls for city and seasonal fixed effects. Results of this difference-in-difference exercise show that infants born closest to the epicenter were 0.2 percentage points more likely to be born with low birth weight (less than 2,500 grams, or about 5 pounds and 8 ounces). These treatment effect estimates are approximately 11 percent of the estimated effect of smoking on birth weight [1]. One might expect that effects of a stressful event would be strongest among those who are more susceptible to stress, such as single and first-time mothers [19]. Indeed, for these women we find that the earthquake raised the likelihood of low birth weight by 0.5 percentage points, or 21 percent of the estimated effect of maternal smoking. Estimated effects are strongest for mothers who experienced the earthquake in their first or third trimester. Findings are limited to mild degrees of low birth weight and are broadly robust to changes in the delineation of treatment and the sample period in question. Under some specifications of the sample and estimating equations, we find that the Northridge earthquake affected preterm birth, although results are not as robust as estimates for low birth weight. These findings underscore psychological and medical research pointing to stress as an important factor in low birth weight, both at and below full gestation [50, 40]. We rule out the idea that reactive mobility after the earthquake could be driving results. Permutation tests strongly highlight the actual epicenter over placebo locations.

The challenge with attributing disaster-induced changes in birth outcomes to stress is that higher stress coincides with other barriers to physical health and health care. Much of the related literature on the effects of disasters on birth outcomes either abstracts from specific causal mechanisms or relies on contextual information to claim that factors other than stress are unlikely to drive the result. In most disasters, however, at least some of the aftermath includes event-specific non-psychological factors, making it difficult to quantify the effect of stress alone. There is considerable variation in the severity of disasters reviewed by this literature. It is therefore unsurprising that estimated effects on the probability of low birth weight range from zero to 2.7 percentage points, or that estimated changes in birth weight range from zero to 70 grams (See Table 1.1 in Section 1.2.2). While we also rely on contextual knowledge to understand the possible extent of physical mechanisms, features of the Northridge earthquake make it uniquely well suited to isolate the effect of stress following a natural disaster. The earthquake occurred recently in a large and developed metropolitan area, and there was extensive media coverage and scientific documentation of both the damage and recovery. The record indicates that access to nutrition and basic needs were largely unaffected, infrastructure in the area was constrained but intact, and the retail and grocery sectors were similarly spared. With plentiful access to food, water, medical care, and safe housing, remaining candidates for why disasters may affect birth outcomes include physical injury, stress, and changes in physical health as a result of lasting structural damage (which may plausibly affect mental health as well). In some models, we control for spatial variation in damages and injuries to more tightly bound the effect of stress within this single event, a departure from the related literature.[3] Spatial variation in injury and structural damage reduces the precision of estimates but leaves at least 45 percent of the effect of proximity unexplained. We attribute this residual effect to psychological stress, acknowledging that the true effect may be larger if we account for the effect of damage itself on maternal stress.

Evidence from the Northridge earthquake suggests that physical harm to expectant mothers was limited. Relief efforts were immediate and successful, reducing the possibility that material deprivation, access to nutritious food and clean water, or unsanitary conditions contributed to the results to follow. Although the immediate aftermath of the earthquake saw power outages, gas leaks, crowded hospitals, and road and overpass destruction, normal business activity resumed within a week. Utilities were restored within four days and the vast majority of surface streets were navigable, evidenced by the fact that public transit saw little to no disruption [46, 66]. We find no evidence that access to prenatal care was hindered. On the contrary, mothers may have consumed more prenatal care as compensatory behavior (see Table A5 and related discussion). As discussed in Section 1.2.2, injuries resulting from the Northridge earthquake were too infrequent to explain our findings. Results are impervious to controls for air quality during pregnancy, ruling out particulate matter, smoke,

[3] A close exception is provided by Simeonova [86], who controls for property damage and fatalities across a wide variety of disasters. Differences in damages across disasters, however, do not necessarily align with differences in the physical challenges faced by expectant mothers. By way of example, Hurricane Ike wrought $29 billion in damages in 2008, much less than Northridge's $44 billion before correcting for inflation. But Ike entailed weeks of power outages for millions of people in the late summer alongside widespread difficulties obtaining food and medical care [23], whereas the Los Angeles area returned to normal daily activities within days of the Northridge earthquake.

and pollution as mechanisms.[4] The scope of structural damage included eleven hospitals and health facilities where one or more buildings were "red-tagged" and deemed unsafe to enter.[5] Nevertheless, we find no differential change in the likelihood of a hospital birth among more affected mothers, and results to follow are robust to controls for hospital birth (see Table A5).

Though casualties were relatively low and there was little disruption to health care or basic needs, damages from the earthquake were extraordinarily large compared to other natural disasters in the United States. The financial measure of damages was a nominal $44 billion and 90,000 buildings were damaged or destroyed [43]. Destruction of this degree, even with minimal accompanying effects on physical health, may have exacerbated and extended the short-term stress mothers experienced during and immediately after the earthquake. Exposure to destruction from natural disasters is associated with lasting psychological stress, including symptoms of depression and post-traumatic stress disorder [79, 75]. Just ten of about 2,000 bridges and overpasses were damaged beyond repair in the region, but this still affected major freeways and created "severe hardships for the traveling public" [27]. Kahneman et al. [65] found commuting to be the least satisfying of all daily activities in a sample of working women, associated with elevated levels of impatience and fatigue. Heightened traffic has been shown to increase physical markers of stress across non-experimental and experimental settings [47, 37].

The context of the Northridge earthquake enables us to contribute to the literature on disasters, stress, and birth outcomes in two ways. First, this paper serves as a bridge between two literatures: largely medical research on maternal stress and newborn health, and predominantly economic research on disasters and the same.[6] Documentation from the disaster and recovery enables us to eliminate many alternative physical mechanisms

[4]After Northridge, airborne fungal spores from landslides resulted in a noted increase in coccidioidomycosis or "valley fever" in Ventura county.

[5]Six of these facilities provided labor and delivery care; three were fully operational within two weeks, and three were closed in whole or part for an extended period of time.

[6]In addition to studies cited in Section 1, Simeonova [86] studies a broad set of adverse weather events in the United States, including some smaller earthquakes, and finds that experiencing a natural disaster in the second or third trimester decreases county-level gestational age and birth weight. Others examining the impact of negative *in utero* shocks on long term individual outcomes include Almond [2], Chen and Zhou [26], Almond et al. [3], Field et al. [48], Sanders [83], Lee [69], Bharadwaj et al. [11].

(i.e., access to lifelines and nutrition) and more directly control for physical stressors, thus we are better able to bound the role of psychological stress in affecting neonatal health. The estimated impact of Northridge is similar in magnitude to estimated impacts of purely psychological events – the loss of a parent [13] or the announcement of future layoffs [21] – and smaller than the effects of more sweeping disasters. These comparisons imply that much of the variation in estimated impacts of large disasters on birth outcomes stems from event-specific factors.

Second, we are among the first to examine the impact of a massive earthquake in an urban, developed environment on newborn outcomes. Our analyses benefit from data on over 2 million California births as well as both spatial and temporal variation in earthquake exposure.[7] Results speak to the importance of infrastructure, relief, and recovery in mitigating the health consequences of disasters. Our findings, when viewed through the lens of a quick recovery, stand in marked contrast to the 2005 Tarapaca earthquake in Chile, which Torche [89] credits with a 1.7 percentage point rise in the likelihood of low birth weight and a 2.6 percentage point higher probability of preterm delivery.[8]

1.2 Background

1.2.1 Mechanisms connecting maternal stress and birth outcomes

The pathophysiology connecting maternal stress and birth outcomes is not definitive, but is thought to involve mothers' hypothalamic-pituitary-adrenocortical axis and/or early

[7] See Harville et al. [58] for a review of medical research on the impact of earthquakes and other disasters on birth outcomes. Earthquake studies therein focus on small samples of births ($N \leq 115$), the impact of direct injury (e.g., being buried in rubble), and/or inferences from intertemporal variation alone. This literature includes work by Glynn et al. [54], a clinical analysis of 40 pregnant or post-partum women who experienced the Northridge earthquake. Results indicate that stress was strongest among women who experienced the tremor in their first trimester, who tended to have shorter gestation by 1.2 weeks. In a larger hospital-based study of 13,003 births, Tan et al. [88] found that neonates *in utero* during the Wenchuan, China earthquake of May 2008 gestated 0.6 fewer weeks than infants born before the earthquake. In contrast to these two pre/post analyses, our difference-in-difference findings for the impact of Northridge on gestation are considerably smaller (from Table 2.3: 0.02 - 0.05 fewer weeks).

[8] The 7.8-magnitude Tarapaca earthquake affected a rural area in the Andes and entailed infrastructure damage so severe that power and water services were interrupted. Thirty power generating substations were destroyed, meaning water could not be pumped to the nearby town of Iquique. Relief supplies had to be delivered by air [82].

placental development [39]. In response to psychological stress, the body engages a hormonal chain reaction to enhance metabolic function in support of a "fight or flight" response. Specifically, the neuropeptide corticotrophin-releasing hormone (CRH) activates the adrenocorticotrophic hormone (ACTH), which initiates cortisol production. Cortisol has been shown to increase metabolic rates, suppress the immune system, and decrease bone formation [85, 34]. To the question at hand, CRH is particularly active during pregnancy, and elevated placental CRH levels, measured as early as the second trimester, are consistently linked to preterm birth [60]. Preterm birth is also associated with the presence of abnormally high rates of placental lesions, which may have roots in stress and poor placental development early in pregnancy.

Preterm birth clearly goes hand in hand with low birth weight, but there are several other possible links between maternal stress and low birth weight outside of gestation length. Chronic stress leads to the release of catecholamines, which hinder uterine perfusion and reduce fetal growth [81]. Another hypothesis suggests that increased cortisol levels from chronic stress lead to increases in glucocorticoids, which diminish the cardiovascular and metabolic function of the fetus.[9] Lobel et al. [71] find empirical support for the idea that maternal behaviors (e.g., smoking, caffeine use, etc.) mediate the effect of stress on birth weight. In a review of the literature, Dunkel Schetter and Lobel [40] concluded that maternal depression and distress are consistently tied to low birth weight (also see Field et al. [50]).

Looking beyond birth weight and gestation, Currie and Rossin-Slater [31] report robust impacts of exposure to Texas hurricanes on extended ventilator use or meconium aspiration syndrome (MAS) in newborns. The pathway between stress and ventilator use or MAS is not well studied in the medical literature. Breathing support is often, but not exclusively, used for preterm or smaller newborns with underdeveloped lungs. MAS has several potential causes, not all of them independently linked to maternal stress (e.g., high blood pressure, maternal diabetes, long gestation, long labor, low amniotic fluid index values). Both abnormalities are potentially serious, often tied to comorbidities, and considerably less common than preterm birth or low birth weight, suggesting that hurricane exposure was most detrimental

[9] See Hobel and Culhane [59], Hobel et al. [61], and Dunkel Schetter [38], among others, for further discussion.

to the least healthy infants. Here, available data on birth measures are limited to gestation and birth weight, but after assessing overall impacts we look for heterogeneous impacts at different degrees of severity for both outcomes. Methodologically, [31] show that estimates for birth weight and gestation are sensitive to specification, aggregation, and measurement. For our purposes, the primary lesson from their work is the notion that infant outcomes may be endogenous to maternal mobility between the disaster and birth. Sensitivity checks described in Section 1.5.3 rule out maternal mobility as a major contributing factor to our main results.

1.2.2 Northridge earthquake

On January 17, 1994, at 4:31 AM , the Northridge earthquake shook the greater Los Angeles area with a moment-magnitude of 6.7. It was felt over approximately 21,400 square kilometers in Southern California, Nevada, and Arizona (Dewey et. al, 1995). According to the U.S. Geological Survey (USGS), 57 people were killed, more than 9,000 were injured, and 20,000 people were left temporarily homeless.[10] At least 90,000 buildings were damaged.

The particular circumstances of the Northridge catastrophe suggest that psychological stress and its physiological consequences were more damaging to the earthquake's survivors than injury, debris, poor nutrition, or inadequate health care. Perhaps 2.7 percent of the Los Angeles County population was pregnant at the time of the earthquake,[11] and assuming that injuries were distributed uniformly, about 243 of the injured would have been pregnant. Even if every injured and pregnant woman delivered a low birth weight child (a 100 percent incidence rate that far exceeds the 6 percent norm), results discussed in Section 1.4 suggest that injury would explain just 62 percent of the conditional rise in low birth weight. In spite of large economic losses, little of the structural damage threatened lives. Most insurance claims in the Los Angeles area were for residential damage (Roth and Van, 1997; Comerio, 1998). In addition to federal aid for home repair, there were programs for mental health counseling,

[10] Durkin [41] counts 72 fatalities and up to 11,800 injuries as direct or immediate indirect consequences of the earthquake. Heart attacks account for much of the difference between the official count of 57 deaths and Durkin's higher figure. Kloner et al. (1997) also report an elevated frequency of heart attacks in the Northridge aftermath.

[11] In 1990, 23.5 percent of the Los Angeles County population were women of childbearing age (15 - 44), and national pregnancy rates at the time measured 115.8 women per 1,000 of childbearing age.

job training, and unemployment assistance (Bolin and Stanford, 1998).[12] Importantly, these relief programs began immediately: traffic volumes steadily increased as work and daily life resumed in the week following the earthquake (U.S. Department of Transportation, 2002), train and bus transit lines saw little to no interruption, utilities were restored quickly,[13] and most temporary shelters were closed within a month [46]. Several hospitals were evacuated or shut down in the short run [25], though as we show in Section 1.5.3 this appears to have had no effect on the likelihood of a hospital birth among affected mothers. Holding access to hospitals constant, it is possible that an altered composition of labor and delivery providers could affect our results through the measurement of birth weight and gestation in vital statistics data [6]. In specification checks discussed in Section 1.5.1 we show that inferences are not affected by the exclusion of infants with round birth weights.

Hurricanes are also thought to elicit maternal stress (Dunkel Schetter and Glynn, 2010; Currie and Rossin-Slater, 2013), though by nature they entail one empirical and one conceptual challenge for studies of *in utero* stress on newborn health. Hurricanes are anticipated with enough time, plausibly, to elicit endogenous avoidance, and when they entail flooding they tend to severely alter the landscape, disrupt public services, and endanger the basic needs of pregnant women.[14]

[12]Homeowners whose dwellings sustained less than $10,000 in damages (as determined by FEMA inspectors) were eligible for grants from the agency's Minimum Home Repair Program. Victims who could not qualify for other federal assistance programs or who had unmet needs after receiving federal assistance could receive aid through the joint OES-FEMA Individual and Family Grant Program.

[13]Eight hours after the earthquake, nearly half of the customers in the Los Angeles Department of Public Works had their power restored. Eleven hours after the earthquake, Southern California Edison had power restored to all but 150,000 homes and businesses. Four days later, power was reconnected to all houses but 7,500 in the San Fernando Valley. At the start of the fifth day, all but 10,000 houses had their water service restored [66]

[14]Maternal dehydration can lead directly to fetal stress, decreased amniotic fluid index values, and therefore abnormal birth outcomes. When Hurricane Ike struck Houston in September 2008, over 4.5 million customers in the greater Houston-Galveston area lost power for three weeks [44]. During this time, the daily high temperature averaged 89 degrees Fahrenheit. A rapid needs assessment by the CDC found that 27% of surveyed households required assistance obtaining food, and 11% could not access medical care or prescription medication. As late as September 25, 18% of households reported having no garbage pick up and 7% of households reported not having drinking water [23]. In June 2001, Storm Allison carried up to 37 inches of rain in 24 hours and resulted in severe residential flooding for over 850,000 residences. One week after the heaviest rainfall, 33% of the surveyed homes in Houston still had floodwaters in their homes with a mean depth of 16 inches, 15% did not have power, 15% did not have sewage service, and 6% had no running water [22]. June 2001 had an average high temperature of 89 degrees Fahrenheit.

Northridge's impact on daily life and health care were short-lived, but contemporary accounts and psychiatric research suggest that the earthquake had more lasting effects on mental well being. Stress in this setting includes immediate stress from the earthquake and its powerful aftershocks, persistent anxieties from living and working in damaged structures, anticipation of future disasters, and changes in the emotional health of communities. In a psychiatric study of 130 Northridge survivors three months after the earthquake, McMillen et al. [75] report that 61 percent met two out of three criteria defining post-traumatic stress disorder. In the months following the Northridge earthquake, the Los Angeles area received at least $35.1 million for earthquake-related mental health counseling [63]. Stress was sufficiently severe and widespread that over 241,000 children received crisis counseling in a FEMA-funded program from January 17 through November, 1994 [56]. On-campus counseling centers and outreach programs were opened and operated by California State University, Northridge [9, 18]. The non-profit Valley Trauma Center opened its doors for disaster counseling for months after the earthquake [17]. Studies of the Northridge aftermath found that earthquake-induced stress affected immune systems and cardiovascular health [87, 36]. One year after Northridge, crisis centers in the Los Angeles area were reportedly flooded with calls from people who were triggered by prominent news coverage of an earthquake in Kobe, Japan [74]. Earthquake-induced PTSD is not unique to Northridge. See Goenjian et al. [55], [68], and [7] for evidence of elevated rates of PTSD among earthquake survivors in, respectively, Armenia, Taiwan, and Turkey. In the context of the Northridge earthquake, other physical stresses (i.e., dehydration) were minimized by the speedy restoration of utilities and the mid-winter Mediterranean climate of Southern California.

Table 1.1 contains a summary of results from prior work evaluating the effects of harmful events on birth outcomes, spanning large disasters, national crisis, family crisis, and violent conflict. The last three listed studies pertain to events with more potent psychological than physical consequences, and it is telling that their estimated effects tend to be smaller, less variable, and similar in magnitude to our main findings. While the studies summarized in Table 1.1 differ in their econometric strategies, it is sensible that events yielding widespread physical *and* psychological damage (nearby violence, a 20 percent decline in economic

Table 1.1: Related research on disasters and birth weigh

Study	Event	Geography	Result
Mansour and Rees [73]	Fatalities from al-Aqsa Intifada	Palestine (West Bank)	P(LBW) increased 0.001-0.0027 per fatality (3-10% marginal effect)
Camacho [20]	Landmine explosions in city	Columbia	Birth weight decreased 9g if in 1st and 2nd trimester
Torche [89]	Earthquake (Tarapaca)	Chile	P(LBW) increased by 0.017
Bozzoli and Quintana-Domeque [14]	Macroeconomic Crisis	Argentina	Birth weight decreased 30g
Currie and Rossin-Slater [31]	Hurricane/Tropical Storm	United States	P(LBW) increased .016, in specification closest to ours
Burlando [16]	Blackouts	Zanzibar (Tanzania)	Birth weight decreased 60-70g
Simeonova [86]	Natural Disasters, varying	United States	Birth weight decreased up to 18g, (floods most harmful)
Kim, Carruthers, and Harris (2017)	Northridge Earthquake	United States	Birthweight decreased 9-11g P(LBW) increased .002-.005
Brown [15]	In-utero during 9/11 not in NY/DC	United States	P(LBW) increased by 0.002
Black et al. [12]	Loss of (grand)parent	United States	Birth weight decreased 15g
Carlson [21]	News of future layoff event	United States	Birth weight decreased 15-20g P(LBW) increased .0021-.0092

activity, hurricanes, blackouts, and flooding) result in bigger declines in birthweight. We contribute to this literature by demonstrating that the effect of maternal stress from a short-lived natural disaster is comparable to the effect of more personal loss and economic anxiety.

1.3 Data and model

1.3.1 Data

California birth records over the years 1992-1995 are taken from the National Center for Health Statistics (NCHS). Our primary identification strategy proxies for earthquake exposure using mothers' city of residence and the radial distance between that city and the epicenter. An alternate model relies on a "community decimal intensity" (CDI) scale derived from survey responses. In all cases, we know the county of birth and the mother's county of residence. There are two circumstances describing 42 percent of births where we do not observe mothers' city of residence. When the county of birth differs from the mother's county of residence, we observe her home county but not her city. Second, births in cities with populations fewer than 100,000 are treated as having occurred in an aggregate residual location in the home county. For both cases we have a less accurate idea of where the mother lived when the earthquake struck, but not so inaccurate as to justify omitting these births. When we do not observe mothers' home city, we randomly assign one within her home county. This imputation creates a small degree of measurement error under our preferred specification of proximity, which (given these uncertainties about birth cities) is a simple indicator for birth within about 80 kilometers of the epicenter.[15]

The main analytical sample includes 2,167,895 live births. Primary outcomes of interest are two binary indicators: one for preterm birth, defined as birth before 37 weeks gestation, and another for low birth weight, defined as less than 2,500 grams. Birth records also provide the infant's gender and a number of variables describing parents and siblings: mother's and father's age and race; mother's education (years of schooling); marital status; mother's count of previous live births still living; total birth order; plurality of the current birth; and mother's number of prenatal care appointments.[16] Additional controls describing mother's city of residence include the Bureau of Labor Statistics annual unemployment rate in the year of birth and annual measures of pollution and air quality drawn from the U.S. Environmental Protection Agency (EPA).[17]

Figure 1.1 depicts California cities and their distance from the epicenter, dividing the 62 cities listed in the NCHS data into quartiles. The first quartile consists of "highly affected cities," that is, those that are closest to the epicenter (up to 77.8 kilometers). This region experienced the greatest degree, by far, of shaking and destruction during the earthquake. Compare our definition of highly affected cities with the "shakemap" reproduced in Figure 1.2. As a point of reference, Bakersfield is 34 minutes north of Gorman. In Figure 1.1,

[15] Orange County is the only county where some cities are within 80 kilometers and some are more than 80 kilometers from the epicenter. See Table A3 in Appendix A for results when the sample excludes imputed Orange County births. Point estimates for low birth weight are slightly larger than they are otherwise, which is consistent with classical measurement error, though these differences may also be driven by treatment effect heterogeneity across Orange County and other affected areas. See Table A4 for results excluding all births with imputed cities of residence. Degrees of freedom are somewhat weaker but results are generally consistent with those reported for all births.

[16] Prenatal care may have been affected by the earthquake (a) if mothers compensated for stress or injury with additional prenatal visits or (b) if access to prenatal care was hindered by the earthquake's destruction. We show in Section 1.5.3 that the former case is more plausible.

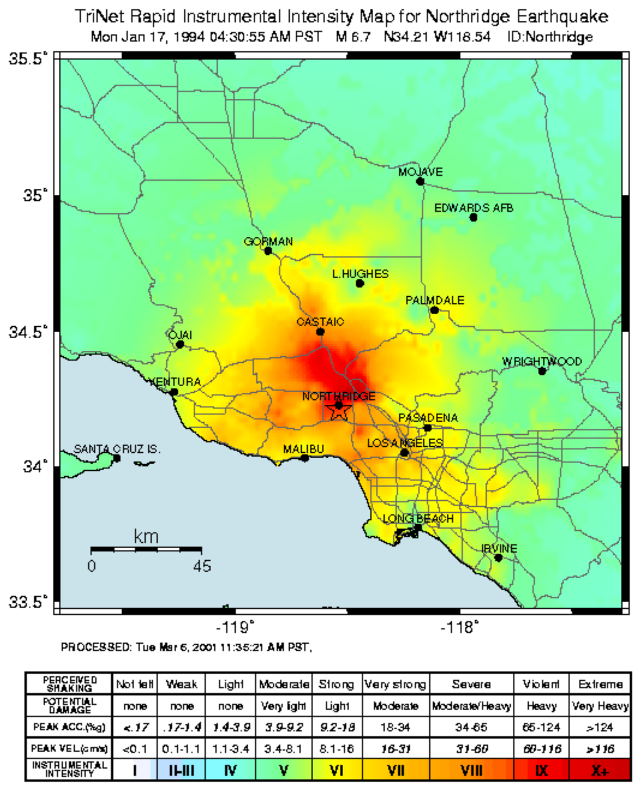
[17] Air quality controls include the second-highest 24-hour recording of particulate matter (PM-10) in the infant's birth year and city of residence, the second-highest 8-hour recording of carbon monoxide, and the second-highest 8-hour recording of ozone. If data on these pollutants do not exist for a given city, we substitute values for the closest city. When pollutant data for a given city are drawn from more than one EPA monitor, we take the average of all monitors in the city. Graphical evidence suggests that there were no abnormal changes in pollution or air quality levels in the highly affected cities at the time of the earthquake. Graphs of monthly levels of PM10 (Particulate matter smaller than 10 microns in diameter), Ozone, and Carbon Monoxide are available in Appendix A, Figure A1.

Figure 1.1: California cities by distance from the epicenter

Gorman would be on the approximate boundary between the "highly affected" group of cities and second-quartile cities.[18] Difference-in-difference analyses to follow compare pre-post differences in birth outcomes of infants born in highly affected cities with pre-post differences among infants born elsewhere in California. One alternative specification defines a continuous earthquake dosage as a function of distance (in kilometers) from the epicenter, and another defines exposure as a binary variable for cities above a particular CDI threshold.

Descriptive statistics of key variables in highly affected and other cities are shown in Table 1.2. Six percent of newborns are classified as having low birth weight, and 9 - 10 percent are born preterm. Newborns in highly affected cities exhibit significantly lower

[18]For an eastern comparison, note that both maps categorize Pasadena as highly affected. Additionally, Pomona (Figure 1.1) is due south of Wrightwood (Figure 1.2), putting it in the less affected category. Moreno Valley (Figure 1.1) is southeast of Wrightwood in Figure 1.2.



Source: California Integrated Seismic Network

Figure 1.2: Northridge earthquake intensity

mean birth weight and significantly shorter gestational age, on average. Of course, part of the cross-city difference may be driven by socioeconomic factors that existed before the earthquake. For example, parents in highly affected cities are less educated, on average, and 6.4 percentage points less likely to be married. As shown in Table 1.3, highly affected cities have substantially poorer air quality. PM-10 readings are 15 percent higher than in places further from the epicenter, carbon monoxide readings are 34 percent higher, and ozone readings are 31 percent higher.

1.3.2 Methods

We exploit the combination of cross sectional variation in the intensity of the earthquake between cities and intertemporal variation in birth outcomes before and after the earthquake to estimate the causal effect of the earthquake on low birth weight and preterm delivery. Our

Table 1.2: Summary Statistics of California Births, 1992-1995

	(1) Highly affected cities	(2) Other cities	(3) Difference, (1)-(2)
Birth weight (grams)	3,351.576	3,371.113	-19.537***
	(575.245)	(583.065)	(0.799)
Birth weight under 2,500 grams (%)	6.014	5.967	0.047
	(23.775)	(23.687)	(0.033)
Gestation (weeks)	39.038	39.115	-0.077***
	(2.490)	(2.502)	(0.003)
Gestation under 37 weeks (%)	9.692	9.422	0.270***
	(29.213)	(29.585)	(0.040)
Age of mother (years)	27.103	27.029	0.074***
	(6.117)	(6.132)	(0.008)
Nonwhite, non-black mother (%)	9.674	11.839	-2.165***
	(29.560)	(32.307)	(0.043)
Black mother (%)	8.213	7.256	0.957***
	(27.456)	(25.941)	(0.037)
Education of mother (years)	11.301	11.890	-0.589***
	(3.675)	(3.509)	(0.005)
Married mother (%)	62.013	68.403	-6.390***
	(48.535)	(46.490)	(0.065)
Number of live births now living	1.126	1.127	-0.001
	(1.308)	(1.335)	(0.002)
Age of father (years)	29.654	29.531	0.123***
	(6.882)	(6.888)	(0.009)
Imputed age of father (%)	7.632	5.886	1.746***
	(26.551)	(23.537)	(0.034)
Nonwhite, non-black father (%)	11.775	14.035	-2.260***
	(32.231)	(34.735)	(0.046)
Black father (%)	8.776	8.588	0.187***
	(28.294)	(28.019)	(0.039)
Number of prenatal visits	11.264	11.277	-0.013**
	(4.226)	(4.119)	(0.006)
Male newborn (%)	51.138	51.203	-0.066
	(49.987)	(49.986)	(0.069)
Plurality	1.023	1.024	-0.001
	(0.156)	(0.160)	(2.2E-04)
Total birth order	2.367	2.391	-0.024***
	(1.533)	(1.582)	(0.002)
Births	904,998	1,262,897	

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.3: Summary Statistics of California Cities, 1992-1995

		(1) Highly affected cities	(2) Other cities	(3) Difference, (1)-(2)
Unemployment rate	8.460	9.356	-0.897*	
	(0.753)	(3.890)	(0.506)	
PM-10	91.867	79.735	12.132***	
	(26.518)	(29.402)	(4.304)	
Carbon Monoxide	7.182	5.358	1.824***	
	(2.453)	(2.389)	(0.361)	
Ozone	0.119	0.091	0.028***	
	(0.024)	(0.032)	(0.005)	
City-years	60	171		
Cities	15	45		

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

model is a difference-in-difference estimator:

$$\begin{aligned} BirthOutcome_{ijmt} &= \beta_0 + \beta_1 1994_{ijmt} + \beta_2(1994_{ijmt} * HA_{ijmt}) \\ &\quad + \beta_3 X_{ijmt} + HA_{ijmt}\delta_{jm} + M_m + T_t + \theta_j + \epsilon_{ijmt} \end{aligned} \tag{1.1}$$

where i is an infant, j is the city of residence, and m,t is the month and year that infant i is born. *BirthOutcome* is an indicator for low birth weight or prematurity in our main results (specifications for continuous measures of birth weight and gestation are also reported), *1994* is an indicator for infants born between January and October of 1994,[19] *HA* is an indicator for whether the city of birth is in the "highly affected" zone (whose seasonality is permitted to differ by δ_{jm}), and X is a vector of controls summarized in Tables 1.2 and 1.3.[20] Lastly, M is a fixed effect for month of the year to control for seasonal shocks, T contains fixed effects for birth years 1993, late 1994 (November or December), and 1995, θ is a city fixed effect, and ϵ is the error term. In Equation 1.1, β_2 is the difference-in-difference estimate of the effect of the earthquake on birth outcomes.

To investigate effects by trimester, we modify Equation 1.1 as follows:

$$\begin{aligned} BirthOutcome_{ijmt} &= \alpha_0 + \alpha_1 1st_{ijmt} + \alpha_2 2nd_{ijmt} + \alpha_3 3rd_{ijmt} \\ &\quad + \alpha_4(1st_{ijmt} * HA_{ijmt}) + \alpha_5(2nd_{ijmt} * HA_{ijmt}) \\ &\quad + \alpha_6(3rd_{ijmt} * HA_{ijmt}) + \alpha_7 X_{ijmt} + HA_{ijmt}\delta_{jm} + M_m + T_t + \theta_j + \epsilon_{ijmt} \end{aligned} \tag{1.2}$$

where *1st* is an indicator for infants who experienced the earthquake during their first trimester *in utero* (and were conceived between October 1993 and January 17th, 1994) *2nd* is an indicator for infants who were in their second trimester when the earthquake struck (conceived between September and July 1993), and *3rd* is an indicator for infants were were conceived between June and January 1993, thereby experiencing the earthquake in their third trimester.[21] The rest of the variables are defined as in Equation 1.1. In both

[19]Births between January 1 and January 16 are included in this indicator as we are not able to observe the precise day of birth.

[20]In addition to these controls, we also include an indicator for whether an infant is missing data on the father's age, which affects 6 - 8 percent of births. Where missing, father's age is imputed as the mean value by city, birth year, and mother's age.

[21]Additionally, *3rd* applies to a small number of infants who were conceived between January and March 1993 and born in 1994.

specifications, the reference group is the set of infants who are born before or conceived after the Northridge earthquake.

1.4 Results

Table 1.4 shows the results of estimating Equation 1.1. Panel A contains results from the full sample. Panel B contains results when the sample is restricted to mothers more susceptible to high levels of stress: single first-time mothers. [22] If the earthquake affects birth outcomes through maternal stress (and not other channels, e.g., physical stress on the mother and fetus), we would expect the effects of the earthquake to be strongest in the group more susceptible to stress. In each column, we report the results for the coefficient on the interaction $1994 * HA$. All specifications include covariates summarized in Table 1.2, month fixed effects, year fixed effects, and month-by-HA effects. We cluster robust standard errors by county of birth, allowing for the possibility that birth outcomes are correlated within public health service areas.[23]

In each panel of Table 1.4, columns (1) - (3) display results for low birth weight outcomes. Here, low birth weight (LBW) is an indicator for whether an infant weighs 2,499 grams or less at birth. We also separately model the probability that an infant is born "Very LBW," defined as birth weight less than 1,500 grams, or "Mild LBW" defined as birth weight between 1,500 and 2,499 grams.

In panel A, the coefficient on the interaction term in column (1) is 0.0020 and statistically significant. This implies that infants born in cities within 77.8 km of the epicenter were 0.20 percentage points more likely to be born weighing less than 2,500 grams. Based on the mean percent of low birth weight in highly affected cities before the earthquake (5.9%), this effect is equivalent to a 3.4% increase in the probability of low birth weight, or about 393 additional LBW infants.[24] Columns (2) and (3) show that increases in the probability of low birth weight are driven by mild LBW rather than more severe LBW. For comparison, the

[22]We also run results by race of the mothers. As shown at A1, there are larger effects for black mothers compared to white mothers and other races mothers.

[23]Results are robust to clustering standard errors at the mother's city of residence.

[24]There were 194,434 births in the 77.8-kilometer vicinity of Northridge over the January - October 1993 period; 0.2 percent of this sum is 393.

Table 1.4: Equation 1.1 results - The effect of the earthquake on birth outcomes

	(1) LBW	(2) Very LBW	(3) Mild LBW	(4) Prematurity	(5) Extreme Prematurity	(6) Mild Prematurity
Panel A (Full sample)						
1994*Highly Affected City	0.0020**	0.0007**	0.0014*	0.0006	0.0000	0.0006
	(0.0008)	(0.0003)	(0.0007)	(0.0010)	(0.0002)	(0.0010)
N	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895
R^2	0.104	0.032	0.075	0.058	0.017	0.048
Panel B (Restricted sample)						
1994*Highly Affected City	0.0054**	-0.0002	0.0056***	0.0036	-0.0018***	0.0054**
	(0.0020)	(0.0010)	(0.0018)	(0.0026)	(0.0006)	(0.0024)
N	294,332	294,332	294,332	294,332	294,332	294,332
R^2	0.019	0.015	0.010	0.023	0.011	0.017

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

estimated effect of the earthquake on the probability of low birth weight is approximately 11 percent of the estimated effect of maternal smoking [1].

Panel B of Table 1.4 shows that the estimated effect of the earthquake on low birth weight is over twice as large for single first-time mothers, who are assumed to be more susceptible to stress. Among these mothers, the earthquake led to a 0.54 percentage point increase in the probability of a low birth weight infant, equivalent to an additional 155 LBW infants in this population, or approximately 21 percent of the effect of smoking on the probability of low birth weight. Consistent with the broader sample of births, these impacts are concentrated among milder LBW newborns.

The column (4) specification estimates the linear probability of birth before 37 weeks' gestation, and columns (5) and (6) contain results for extreme prematurity (births less than 28 weeks) and mild prematurity (between 28 and 37 weeks), respectively. In the full sample specification (Panel A), we do not detect a significant preterm birth gap between highly affected cities and more removed cities along any of these margins. In the restricted sample of single, first-time mothers, however, we find that a decline in extreme prematurity of 0.18 percentage points offsets a 0.54 percentage point rise in milder prematurity, such that the net effect of proximity on preterm birth is statistically insignificant. It is possible that the earthquake led to fewer infants carried to the point of a live but extremely premature birth.

Our characterization of cities as "highly affected" is fairly accurate but arbitrary. Figure 1.2 shows that while there are marked discontinuities in the perceived impact of the earthquake, the strength of tremors lessens with distance from the epicenter. With this

Table 1.5: Equation 1.1 results - Linear proximity to the earthquake and birth outcomes

	(1) LBW	(2) Very LBW	(3) Mild LBW	(4) Prematurity	(5) Extreme Prematurity	(6) Mild Prematurity
Panel A (Full sample)						
1994*Inverse KM	0.0790**	0.0485***	0.0305	0.0388	0.0112*	0.0276
	(0.0292)	(0.0094)	(0.0277)	(0.0348)	(0.0063)	(0.0371)
N	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895
R^2	0.104	0.032	0.075	0.058	0.017	0.048
Panel B (Restricted sample)						
1994*Inverse KM	0.224***	0.0280	0.196***	0.302***	-0.0276	0.329***
	(0.0549)	(0.0405)	(0.0515)	(0.0785)	(0.0239)	(0.0774)
N	294,332	294,332	294,332	294,332	294,332	294,332
R^2	0.019	0.015	0.010	0.023	0.011	0.017

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

in mind, Table 1.5 reports results from a version of Equation 1.1 in which the binary indicator of proximity within 77.8 kilometers ($HA = \mathbf{1}(distance \leq 77.8km)$) is replaced with a continuous measure of proximity equal to $1/distance$, where $distance$ is equal to kilometers from the epicenter.[25]

Point estimates in Table 1.5 can be interpreted, approximately, as the conditional gap in birth outcomes between a city one kilometer from the epicenter ($1/distance = 1$) and the city farthest from the earthquake (884 kilometers, such that $1/distance \approx 0.001$). For the full sample, we find this gap to be quite large: 7.9 percentage points with respect to low birth weight. Echoing results from the binary measure of proximity, column (2) of Table 1.5 results suggest a significant but much larger impact on very low birth weight. For the high-stress subsample in Panel B, the estimated impact on low birth weight is implausibly large at 20 - 22 percentage points. Large point estimates in Table 1.5 and visual evidence from Figure 1.2 support $HA = \mathbf{1}(distance \leq 77.8km)$ as the preferred specification. Nevertheless, results in Table 1.5 demonstrate that inferences from a binary proximity measure are robust in sign and significance to more continuous measures of exposure to the Northridge earthquake.

Table 1.6 reports results from a third exposure measure that is less arbitrary than our 77.8 kilometer radius and more grounded in perceptions of shaking and damage. The community decimal intensity (CDI) scale aggregates telephone survey responses from people who experienced an earthquake. The measure, widely used today, was developed by Dengler and Dewey [35] following the Northridge earthquake using survey data from about 6,000

[25] Results are qualitatively robust to alternative functional forms, e.g., exponential or piece-wise.

affected adults. Examples of survey questions include "Did you feel it?"; "Was it difficult to stand or walk?"; "Was there damage to the building?" Responses are quantified, weighted (with more weight given to recollections of moving furniture or structural damage), and aggregated by community. The range is 1-7 for cities in the Northridge vicinity. Dengler and Dewey [35] and others show that spatial CDI distributions are smoother than moment magnitude readings, and for our purposes, CDI responses may better reflect variation in the stress of experiencing an earthquake.

One drawback of using city-level CDI data over the simple kilometer radius is that we introduce additional measurement error when birth cities are imputed. Another drawback is that CDI variation is lumpy.[26] Accordingly, we limit the sample to unimputed birth cities and define a binary indicator of high CDI for cities rating 4 or higher on the 7-point scale. Four-fifths of births in the 77.8-kilometer radius were in high CDI cities, and no births in high CDI cities were outside the 77.8-kilometer radius.

The Table 1.6 specification of Equation 1.1 is therefore a logical test of whether infants meeting our broad, radial measure of proximity to the earthquake were more affected in areas where citizens reported more shaking. We find this to be the case, with the incidence of LBW rising 0.27 percentage points in the full sample and 0.79 percentage points in the high-stress restricted subsample. These point estimates are 35-48 percent larger than analogous estimates using radial proximity, and as before, appear to be driven by mild LBW.[27] The CDI measure also points to higher rates of mild preterm birth, by 0.35 - 1.1 percentage points, respectively, across the full and restricted samples. Table 1.6 results support the causal inferences we make from simpler proximity measures, but at the cost of omitting 42 percent of infants who were born in smaller towns and cities. We maintain radial proximity as our preferred measure of earthquake exposure, emphasizing that this broader treatment may understate the effect of experiencing the earthquake *in utero* on newborn health.

Turning to results by trimester in Table 1.7, we find effects of the earthquake on low birth weight to be strongest in the first and third trimesters in the full sample. A mother

[26] Among January-October 1994 births in the 77.8-kilometer radius, 93 percent were born in (unimputed) cities that rated 3 or 4 on the CDI scale, and more intense ratings were rare.

[27] Table 1.6 point estimates are also larger in magnitude than estimated effects of radial proximity among the subsample of births in non-imputed cities. See Table A4 in the appendix.

Table 1.6: Equation 1.1 results - CDI and birth outcomes

	(1) LBW	(2) Very LBW	(3) Mild LBW	(4) Prematurity	(5) Extreme Prematurity	(6) Mild Prematurity
Panel A (Full sample)						
CDI≥4	0.0027***	0.0008	0.0020***	0.0037***	0.0002	0.0035**
	(0.0007)	(0.0006)	(0.0007)	(0.0013)	(0.0003)	(0.0013)
N	1,269,963	1,269,963	1,269,963	1,269,963	1,269,963	1,269,963
R^2	0.104	0.033	0.074	0.058	0.017	0.048
Panel B (Restricted sample)						
CDI≥4	0.0080***	0.0008	0.0071***	0.0102***	-0.0011	0.0113***
	(0.0019)	(0.0013)	(0.0021)	(0.0023)	(0.0010)	(0.0020)
N	179,043	179,043	179,043	179,043	179,043	179,043
R^2	0.021	0.016	0.011	0.023	0.012	0.017

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

living in cities designated as "highly affected" who experienced the earthquake in her first (third) trimester was 0.33 (0.42) percentage points more likely to have an LBW infant. An important new insight in Table 1.7 is the marginally significant effect on severe LBW for full-sample infants experiencing the earthquake in their third trimester (one sixth of one percentage point). Echoing some of our earlier findings, none of the Panel A point estimates for prematurity are statistically significant at conventional levels.

It is perhaps not surprising that the earthquake had larger effects on birth weight when it coincided with the first or third trimester. The first trimester is the most important period for fetal development and establishes the trajectory for weight gain over the balance of the pregnancy [76]. The third trimester, however, is when the majority of fetal weight gain actually occurs. Low birth weight and Intrauterine Growth Restriction have been linked to several risk factors, including maternal hypertension [72], which is in turn plausibly linked to stress.

That being said, findings in Table 1.7 Panel B indicate that mothers who were more susceptible to stress were more affected if they were in their second or third trimester on January 17th, 1994. The incidence of mild LBW was significantly more likely regardless of trimester for the stress-susceptible group.

Table 1.7: Equation 1.2 results - The effect of the earthquake on birth outcomes, by trimester

	(1)	(2)	(3)	(4)	(5)	(6)
	LBW	Very LBW	Mild LBW	Prematurity	Extreme Prematurity	Mild Prematurity
Panel A (Full sample)						
Earthquake during 1st trimester	-0.0113***	-0.0051***	-0.0062***	-0.0124**	-0.0020**	-0.0104**
	(0.0035)	(0.0016)	(0.0021)	(0.0051)	(0.0008)	(0.0044)
Earthquake during 2nd trimester	-0.0131***	-0.0060***	-0.0071***	-0.0141**	-0.0028***	-0.0114**
	(0.0039)	(0.0019)	(0.0023)	(0.0057)	(0.0010)	(0.0049)
Earthquake during 3rd trimester	-0.0316***	-0.0140***	-0.0176***	-0.0585***	-0.0084***	-0.0501***
	(0.0040)	(0.0020)	(0.0022)	(0.0056)	(0.0010)	(0.0047)
1st trimester*Highly affected	0.0033*	0.0013	0.0020	0.0038	0.0005	0.0034
	(0.0016)	(0.0009)	(0.0013)	(0.0026)	(0.0006)	(0.0023)
2nd trimester*Highly affected	0.0025	0.0005	0.0020	0.0014	-0.0002	0.0015
	(0.0023)	(0.0010)	(0.0015)	(0.0029)	(0.0005)	(0.0026)
3rd trimester*Highly affected	0.0042**	0.0016*	0.0026**	0.0018	0.0002	0.0017
	(0.0017)	(0.0009)	(0.0012)	(0.0033)	(0.0007)	(0.0027)
N	2,136,038	2,136,038	2,136,038	2,136,038	2,136,038	2,136,038
R^2	0.104	0.033	0.075	0.061	0.017	0.050
Panel B (Restricted sample)						
Earthquake during 1st trimester	-0.0130**	-0.0055**	-0.0075**	-0.0186**	-0.0018	-0.0168***
	(0.0053)	(0.0025)	(0.0033)	(0.0071)	(0.0015)	(0.0060)
Earthquake during 2nd trimester	-0.0194***	-0.0083***	-0.0111**	-0.0209**	-0.0030*	-0.0179**
	(0.0060)	(0.0028)	(0.0041)	(0.0081)	(0.0017)	(0.0070)
Earthquake during 3rd trimester	-0.0399***	-0.0166***	-0.0233***	-0.0720***	-0.0112***	-0.0608***
	(0.0062)	(0.0030)	(0.0038)	(0.0078)	(0.0016)	(0.0066)
1st trimester*Highly affected	0.0043	-0.0009	0.0052*	0.0047	-0.0025**	0.00717**
	(0.0033)	(0.0014)	(0.0026)	(0.0038)	(0.0010)	(0.0033)
2nd trimester*Highly affected	0.0088*	0.0011	0.0077*	0.0064	-0.0018	0.0081*
	(0.0045)	(0.0019)	(0.0038)	(0.0044)	(0.0013)	(0.0041)
3rd trimester*Highly affected	0.0072**	0.0014	0.0058**	0.0035	0.0003	0.0032
	(0.0034)	(0.0016)	(0.0022)	(0.0037)	(0.0009)	(0.0034)
N	289,928	289,928	289,928	289,928	289,928	289,928
R^2	0.020	0.015	0.010	0.026	0.012	0.019

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

1.5 Robustness checks

1.5.1 Changes in Specification

Table 2.3 reports on our tests of the sensitivity of results to specification changes, with specifications for LBW (panel A1 and A2) and specifications for prematurity (panel B1 and B2). Baseline point estimates are repeated in columns (1).

The column (2) model redefines low birth weight or prematurity as a weak inequality condition rather than a strict inequality condition, i.e., birth weights $\leq$ 2500g, rather than $<$ 2500g, are categorized as low birth weight and gestation $\leq$ 37 weeks is defined as preterm. The magnitude of the estimated effect on LBW in panels A and B are almost identical with baseline results. Including 37-week gestations with preterm births dramatically increases the estimated effect of the earthquake, increasing the differential from an imprecise zero to 0.3 percentage points in the full sample and 0.5 percentage points in the restricted sample. This, along with inconsistent evidence of a higher rate of mild preterm birth under various specifications (see Tables 1.4, 1.5, and 1.6), indicates that the earthquake had a small effect on gestation, most noticeable at the 37-week medical definition of early term gestation.

Specifications reported in column (3) omits newborns whose birth weight was measured in round multiples of 100 grams. Barreca et al. [6] note that less healthy newborns and newborns of lower socioeconomic status are overrepresented at round birth weights, in part because hospitals serving these populations are less likely to have precise neonatal scales. The earthquake may have shifted the composition of births across hospitals in a way that inadvertently increased the recorded rate of LBW. Comparing columns (1) with (3) does not suggest that this possibility had bearing on our results. Point estimates for LBW are within one standard error of the baseline, and inferences do not change for preterm birth. Unreported analyses find no change in the likelihood of having a round birthweight in cities most affected by the earthquake.

Column (4) reports results from a specification where dependent variables are continuous measures of birth weight and gestational age rather than binary designations of LBW or preterm birth. Results in these specifications are consistent with those from table 1.4. For the full sample, the earthquake reduced birth weight by 9.8 grams and reduced gestation by

Table 1.8: Robustness and specification tests

	(1) Baseline	(2) Birth weight <=2500g	(3) Round weights Omitted	(4) Birth weight (Continuous)	(5) City FE Excluded
Panel A1 (Full sample- Low Birth Weight)					
1994*Highly affected	0.0020**	0.0021**	0.0018**	-9.837***	0.0023**
	(0.0008)	(0.0008)	(0.0007)	(2.231)	(0.0009)
N	2,167,895	2,167,895	2,077,740	2,167,579	2,167,895
R^2	0.104	0.104	0.104	0.123	0.103
Panel A2 (Restricted sample-Low Birth Weight)					
1994*Highly affected	0.0054**	0.0056***	0.0057***	-11.39**	0.0058***
	(0.0020)	(0.0020)	(0.0020)	(5.530)	(0.00199)
N	294,332	294,332	281,658	294,296	294,332
R^2	0.019	0.020	0.019	0.049	0.019
Panel B1 (Full sample- Prematurity)					
1994*Highly affected	0.0006	0.0034**	0.0002	-0.0258***	-0.0004
	(0.0010)	(0.0013)	(0.0010)	(0.0085)	(0.0013)
N	2,167,895	2,167,895	2,077,740	2,092,896	2,167,895
R^2	0.058	0.054	0.058	0.070	0.058
Panel B2 (Restricted sample- Prematurity)					
1994*Highly affected	0.0036	0.0052***	0.0036	-0.0471**	0.0030
	(0.0026)	(0.0013)	(0.0025)	(0.0194)	(0.0030)
N	294,332	294,332	281,658	284,090	294,332
R^2	0.023	0.022	0.023	0.040	0.022

Note: NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

0.026 weeks. Focusing on mothers more susceptible to stress, we estimate that the earthquake reduced birth weight by 11.4 grams and reduced gestation by 0.047 weeks.

Specifications reported in columns (5) estimate Equation 1.1 without fixed effects for city of residence. Results are not substantially different from baseline findings repeated in column (1).

Further sensitivity tests are included in Table 1.9, where we deviate from the baseline sample window of 1992 - 1995. As above, full sample results are displayed in Panel A1 and A2, and Panel B1 and B2 results pertain to the restricted sample of single, first-time mothers. Baseline results are repeated in columns (1). Columns (2) expands the analytical sample to birth years 1991 - 1995, columns (3) contracts the sample to 1992 - 1994, and columns (4) lists results for a 1991 - 1994 sample. Looking across columns 1-4, we show that results from the baseline 1992 - 1995 sample are conservative with respect to low birth weight and preterm outcomes. Excluding 1995 births increases the magnitude and precision with which we estimate prematurity in the full sample.

Columns (5) of Table 1.9 contains results for a falsification test, where we limit the sample to 1992 - 1993 births and control for the interaction $1993 * HA$ in place of the causal difference-in-difference estimate. This is an indirect test of our identifying assumption that affected cities would have followed the same trajectory of birth outcomes as less affected

Table 1.9: Alternative sample windows

Baseline	(1) 1991-1995	(2) 1992-1994	(3) 1991-1994	(4) 1991-1993	(5)
Panel A1 (Full sample- Low Birth Weight)					
1994*Highly affected	0.0020**	0.0025***	0.0033**	0.0035***	
	(0.0008)	(0.0009)	(0.0012)	(0.0012)	
1993*Highly Affected					0.0000
					(0.0016)
N	2,167,895	2,740,817	1,653,188	2,226,110	1,113,006
R^2	0.104	0.103	0.102	0.102	0.102
Panel A2 (Restricted sample- Low Birth Weight)					
1994*Highly affected	0.0054**	0.0063***	0.0052**	0.0061**	
	(0.0020)	(0.0020)	(0.0024)	(0.0023)	
1993*Highly affected					-0.00629
					(0.00408)
N	294,332	371,983	228,613	306,264	152,461
R^2	0.019	0.020	0.021	0.021	0.021
Panel B1 (Full sample- Prematurity)					
1994*Highly affected	0.0006	0.0012	0.0022*	0.0026**	
	(0.0010)	(0.0010)	(0.0012)	(0.00107)	
1993*Highly affected					0.0017
					(0.0023)
N	2,167,895	2,740,817	1,653,188	2,226,110	1,113,006
R^2	0.058	0.057	0.058	0.057	0.057
Panel B2 (Restricted sample- Prematurity)					
1994*Highly affected	0.0036	0.0042	0.0039	0.0047	
	(0.0026)	(0.0027)	(0.0033)	(0.0031)	
1993*Highly affected					-0.00332
					(0.00555)
N	294,332	371,983	228,613	306,264	152,461
R^2	0.023	0.023	0.024	0.024	0.024

Note: NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

cities in the absence of the Northridge earthquake, which would be questionable if highly affected cities exhibited different rates of low birth weight or prematurity in 1993, before the earthquake. As we see in columns (5), however, we do not observe a significant change in low birth weight or preterm delivery in 1993.

Next, we explore the extent to which additional controls for injury and structural damage rates could explain the inferred effect of proximity to the Northridge earthquake. In Table 1.10 we report results from Equation 1.1 with the addition of two control variables: city-level injury rates and a structural damage index. Injury data are the number of persons per thousand who were treated and released from area hospitals, as estimated by Durkin [41]. Structural damage indices are computed from the same survey of affected households that yielded the CDI [35]. These city-level controls can be thought of as the aggregate likelihood

Table 1.10: Equation 1.1 results with city injury and structural damage rates included as additional controls

	(1) LBW	(2) Very LBW	(3) Mild LBW	(4) Prematurity	(5) Extreme Prematurity	(6) Mild Prematurity
Panel A (Radial proximity indicator)						
1994*Highly Affected	0.0009*	-0.0004	0.0014	-0.0012	-0.0004	-0.0008
	(0.0005)	(0.0006)	(0.0008)	(0.0031)	(0.0006)	(0.0026)
N	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895
R^2	0.104	0.032	0.075	0.058	0.017	0.048
Panel B (High CDI indicator)						
CDI $\geq$ 4	0.0059***	-0.0013	0.0072***	0.0082*	-0.0006*	0.0089*
	(0.0013)	(0.0015)	(0.0019)	(0.0048)	(0.0003)	(0.0046)
N	1,269,963	1,269,963	1,269,963	1,269,963	1,269,963	1,269,963
R^2	0.104	0.033	0.074	0.058	0.017	0.048

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

that a mother or her building was physically harmed because of the earthquake.[28] Any remaining effect of proximity is therefore a stronger indication of non-physical, psychological mechanisms connecting Northridge to infant health. We emphasize, however, that variation in aggregate structural and personal harm likely soaks up much of the variation in stress that we seek to quantify. Destruction itself is stressful [79], even without injury. We view the Table 1.10 model, therefore, as strong test of the residual effect of proximity.

Panel A of Table 1.10 lists results of Equation 1.1 when the HA treatment of interest is the radial indicator for cities within 77.8 kilometers of the epicenter. There, we show that LBW rates are 0.09 percentage points higher in more affected areas, with weak statistical significance. This means that 45 percent of the baseline 0.20 percentage point estimate remains unexplained by spatial variation in injury and structural damage. Echoing earlier findings, the positive point estimate appears to be driven by mild LBW, which exhibits the same percentage point gain as in the baseline Table 1.4 specification but with a larger standard error. Panel B lists results for the specification of Equation 1.1 where CDI greater than 3 is used to indicate highly affected cities. As in Table 1.6, we limit the sample to non-imputed birth cities. Results for LBW indicate a strong residual effect of high CDI, one that is actually more than twice as large as the corresponding estimate in Table 1.6. Since

[28]Reported injury rates were too infrequent to explain our findings (refer back to our computations in Section 1.2.2), but we acknowledge the possibility that unreported injuries played a role. Citywide aggregates should approximate geographic variation in all degrees of injury.

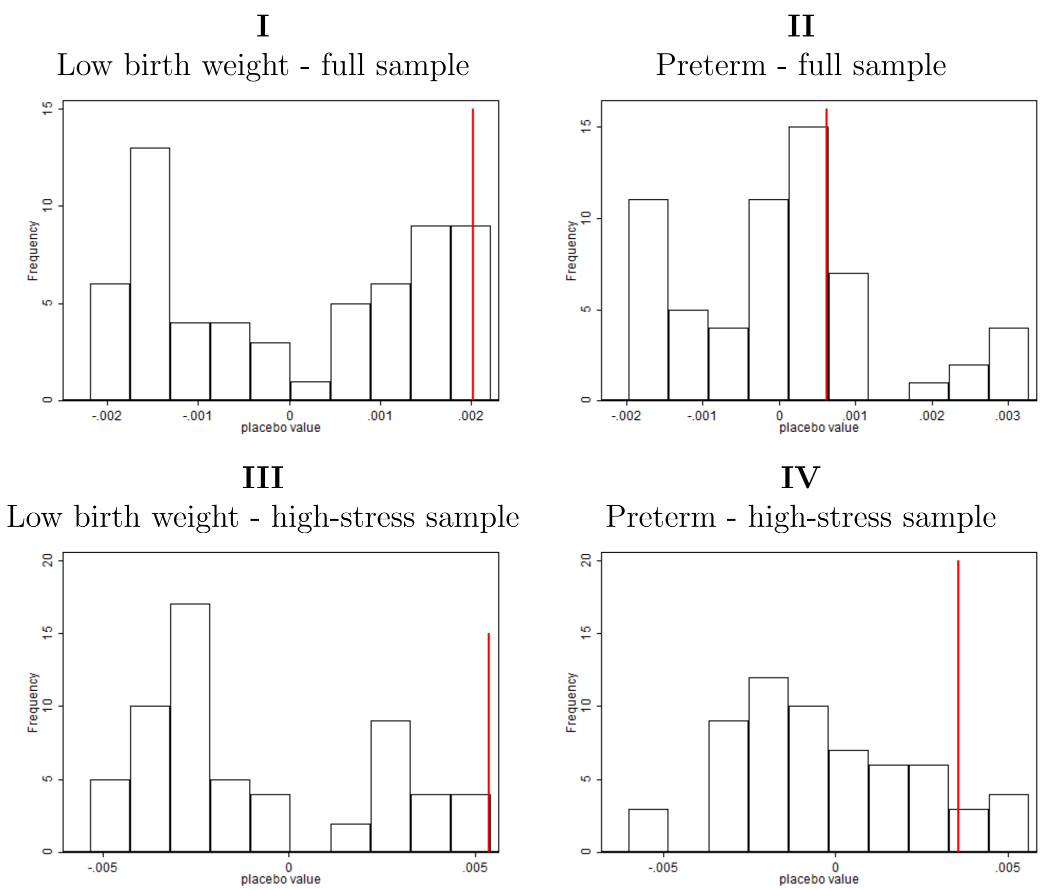
the structural damage component was one element of CDI, this attributes an outsized role to recollections of pictures shaking, furniture moving, and so forth.

1.5.2 Placebo test: Moving the epicenter

Main results discussed in Section 1.4 show that birth weight in close proximity to Northridge noticeably deviated from expectations. It is possible that this was merely due to sampling variation. If several locations deviated from expectations by even greater degrees, that would cast doubt on the earthquake as a primary factor in weaker infant outcomes. We assess this threat to internal validity by estimating a distribution of "treatment effects" across every city in the NCHS birth records and then computing the probability that we observe a point estimate as large as the one associated with the true epicenter. Specifically, for each city further than 30 kilometers from the epicenter, we re-estimate our main specification as if the alternative city was the actual epicenter of the earthquake.[29] We then compare the estimated effect from the true epicenter to the distribution of false estimates arising from this series of placebo tests.

Figure 1.3 depicts results. For the full sample, the estimated effect of the earthquake on low birth weight at the true epicenter is greater than all but four placebo estimates (Panel I), one of which was plausibly affected by the earthquake (Glendale). For the high stress subsample, the estimated effect at the true epicenter is higher than all but the Oxnard coefficient (Panel III). Regarding preterm birth, however, Panels II and IV of Figure 1.3 show that baseline effect estimates are not exceptional relative to the distribution of falsification tests. This is expected as our initial results in Table 1.4 indicate the earthquake did not significantly affect the likelihood of preterm birth. Results for the placebo distribution of t-statistics (not shown) follow the same pattern.

[29]Analytical samples for each false epicenter omit births within 30 kilometers of Northridge. Having already established that proximate cities realized a differential change in birth outcomes, excluding them will give false epicenters a better chance at meeting the treatment effect estimated for the true epicenter. Results are robust to wider or narrower radial exclusions.



NOTE: Each figure depicts a distribution of β_2 estimates from Equation 1.1 for the given outcome and sample, where the distribution is generated by "moving" the epicenter from Northridge to 60 alternative cities. Solid lines signify the β_2 estimate for the true epicenter, which can also be found in Table 1.4

Figure 1.3: Placebo permutation tests for the full sample

Table 1.11: Equation 1.1 results for parental characteristics and aggregate birth outcomes

Panel A (Full sample)						
	(1a) Years of education	(2a) Black mother	(3a) Mother age < 25	(4a) Mother age > 40	(5a) Black father	(6a) Father age < 25
1994*Highly affected	0.0352	-0.0013**	-0.0006	-0.0004	-0.0001	-0.0004
	(0.0501)	(0.0005)	(0.0016)	(0.0003)	(0.0007)	(0.0018)
N	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895
R^2	0.078	0.050	0.020	0.001	0.044	0.015
Panel B (Restricted sample)						
	(1b) Years of education	(2b) Black mother	(3b) Mother age < 25	(4b) Mother age > 40	(5b) Black father	(6b) Father age < 25
1994*Highly affected	0.0630	0.0005	-0.0013	0.0001	0.0006	-0.0044*
	(0.0398)	(0.0028)	(0.0025)	(0.0005)	(0.0030)	(0.0026)
N	294,332	294,332	294,332	294,332	294,332	294,332
R^2	0.049	0.075	0.027	0.002	0.064	0.022
Panel C (City-level Aggregates)						
	(9) births	(10) LBW births	(11) white births	(12) white LBW births	(13) black births	(14) black LBW births
1994*Highly affected	-126.9	-7.128	-106.6	-14.80	-5.194	-1.599
	(164.1)	(10.07)	(137.6)	(20.21)	(7.622)	(2.408)
N	3,588	3,588	3,588	3,588	3,588	3,588
R^2	0.286	0.290	0.285	0.363	0.281	0.360

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

1.5.3 Migration and Access to Care

One concern with our empirical strategy is endogenous migration after the earthquake, i.e., the idea that some expectant mothers moved outside the radius of "highly affected" cities after January 17th, 1994 and before delivery. If the expectant mothers who migrate after the earthquake are not representative of the full sample, then difference-in-difference results may reflect compositional changes in mothers more so than the impact of stress on fetal health and development. Plausibly, mothers from more advantaged backgrounds, who are less likely to deliver a preterm or LBW infant, may be more able and likely to leave their community than disadvantaged, higher-risk mothers. This migration pattern would bias results away from zero, overstating the deleterious effect of the earthquake on newborn outcomes. To investigate this possibility, we regress mothers' and fathers' characteristics on other covariates in Equation 1.1.

Results are found in Table 1.11. As in previous tables, Panel A contains results for the full sample and Panel B contains results for our high-stress subsample. The coefficient on the interaction term estimates the post-earthquake change in parental characteristics in highly affected cities, relative to the same difference across other cities. For both the full and high-stress subsample, proximity to Northridge in 1994 does not appear to affect the average level of mothers' education, mothers' age, or fathers' race. Relative to the rest of the full sample, mothers in highly affected cities were 0.1 percentage points less likely to be black (1.5 percent of the area mean prior to 1994), and in the restricted sample fathers were 0.4 percentage points less likely to be under 25 (0.7 percent of the restricted sample mean prior to 1994). Both differentials are small and correlated with lower rather than higher rates of LBW and preterm, and by conditioning on parental characteristics in Equation 1.1, we control for any *ex ante* differences between black and white mothers or between younger and older fathers.

We also assess mothers' migration by examining changes in the total number of births and the total number of LBW births in each city by month and year, following [86]. If results are driven by outmigration of more advantaged mothers, we might expect to observe a decrease in total 1994 births in highly affected cities but a relative increase in the number of LBW births. We empirically test this notion by aggregating births to a city-month panel and estimating Equation 1.3:

$$\begin{aligned}\overline{TotalBirth}_{js} = \gamma_0 + \gamma_1 \overline{1994}_{js} + \gamma_2 \overline{Treatment}_{js} \\ + \gamma_3(\overline{1994}_{js} * \overline{Treatment}_{js}) + \gamma_4 \overline{X}_{js} + \overline{M_s} + \psi_{js},\end{aligned} \tag{1.3}$$

where $\overline{TotalBirth}$ is either the total number of births in city j during month s or total number of low birth weight newborns.[30] If the coefficient γ_3 in equation (3) is negative when modeling the total birth count, we should be concerned about endogenous migration. However, as the results in Panel C, column (9) of Table 1.11 show, we do not detect a significant change in total births. Highly affected cities saw an average of 126.9 fewer births per month in 1994, but the standard error is large and we cannot reject the hypothesis of no change in total births. Additionally, note that the population share of LBW newborns is approximately 0.06. With this in mind, we would about expect 7-8 fewer LBW infants per month, which coincides with the insignificant point estimate reported in column (10). This implies our main findings from microdata are not driven by a disproportionate outflow of healthy infants. Columns (3)-(6) in panel C examine whether the earthquake affected aggregate births by race and LBW status. None of these difference-in-difference results are statistically significant.

[30]The rest of the variables are defined as before, except that their values are monthly averages.

Table 1.12 lists difference-in-difference estimates for two obstetric outcomes that one might expect to have been affected by proximity to Northridge. First, we estimate the impact of the earthquake on the number of prenatal care visits that mothers' report. The main finding in column (1) is that being pregnant in a highly affected area during the earthquake led to a statistically significant but economically small increase in prenatal care visits. Compared to a sample mean of 11.26, the earthquake led to a 2.5 percent increase in prenatal care visits for the full sample and no distinguishable change in prenatal care for the restricted sample. This is important for two reasons. First, the increase in overall prenatal care after the earthquake in highly affected areas contradicts the notion that reduced access to care is responsible for our main result. Second, but equally important, the increased number of prenatal visits is evidence of at least a small degree of compensating behavior on the part of affected mothers (but not, it would seem, mothers most susceptible to stress). Note that prenatal care is included as a control variable in Equation 1.1, meaning that results are net of such compensating behavior.[31]

Another issue related to health care is the likelihood of giving birth in a hospital since several hospital facilities in the affected area were temporarily or permanently closed as a result of the earthquake. Difference-in-difference results for the linear probability of a hospital birth indicate no significant change among mothers overall (column 2a) or among mothers in the stress-susceptible group (column 2b). Over 99 percent of births are recorded as occurring in a hospital in both the full and restricted samples. Main results are virtually unchanged if we include "hospital birth" as an additional control variable (see Table A5 in Appendix A). While we acknowledge the hospital system was disrupted (in places, severely) by the earthquake, results for prenatal care indicate that affected mothers accessed care above and beyond expectations. Finally, low birth weight is more reasonable as a precursor to a hospital birth than vice versa. It is possible that accessible hospitals could reduce low birth weight by stopping early labor. If this was driving our results, however, we would also expect the earthquake to have a stronger and more precise effect on prematurity.

[31] We acknowledge the possibility that damage to some medical facilities and the increased demand for prenatal care may have had an adverse effect on the *quality* of prenatal care after the earthquake. However, we have not found qualitative evidence to support this hypothesis, nor do we have any data to empirically evaluate it.

Table 1.12: Equation 1.1 results for health care decisions

Panel A (Full sample)		
	(1) Num prenatal visits	(2) Hospital birth
1994*Highly affected	0.287***	-0.0037
	(0.0586)	(0.0032)
N	2167,895	2,167,895
R^2	0.125	0.009
Panel B (Restricted sample)		
	(1) Num prenatal visits	(2) Hospital birth
1994*Highly affected	-1.46E-17	-0.0037
	(5.72E-17)	(0.0023)
N	294,332	2,167,895
R^2	0.009	0.009

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Thus, we conclude that endogenous migration and access to health care did not play a substantial role in our main findings. The composition of mothers was relatively unchanged after the earthquake, access to health care had compensating rather than restricting effects, and aggregate changes in births do not support a story of selective outmigration.

1.6 Conclusion

We provide evidence that maternal stress leads to a higher probability of low birth weight, a small decline in gestation age, but no robust change in the likelihood of preterm birth. We rely on identification from intertemporal and spatial variation in stress caused by the 1994 Northridge earthquake in California. Infants born in highly affected cities were 0.20 percentage points more likely to weigh less than 2,500 grams at birth. Among single and first-time mothers, presumed to be more susceptible to stress, the effect of the earthquake was 2.6 times larger at 0.54 percentage points. Most specifications indicate that the earthquake led to increases in the probability of mild low birth weight, rather than very low birth weight. This last observation is consistent with the idea that the earthquake operated mainly through psychological stress. Had we found that the earthquake led to more severe birth weight and gestation changes, we would give more credence to mediating and unobserved factors like access to nutrition, shelter, and water.

When disasters serve as natural quasi-experiments in stress, results must be viewed through the lens of the geographic and institutional context surrounding these events, as well as unique circumstances of the event itself. Earthquakes have idiosyncratic impacts, as do natural disasters more generally. Particularly for pregnant women, a natural disaster may impose immediate physical and psychological stress but may also lead to physical hardship if lifeline services are disrupted or access to nutrition or medical care is limited. A holistic view of the disaster in question is of paramount importance when comparing results discussed here to those of related work.

For example, our estimate of the Northridge earthquake's impact on infant outcomes is easily less than half of that estimated for a 2005 earthquake in Tarapaca, Chile [89]. But the destruction caused by the Tarapaca earthquake was much more comprehensive in nature. Rock slides blocked roads to the point where relief materials had to be air-dropped. Power and water were disrupted for several weeks. Following the Northridge earthquake, all utilities were reconnected within four days. While there was extensive damage to freeway overpasses, the infrastructure was sufficient to allow navigation to essential goods and supplies via surface streets.

The effect of the Northridge earthquake on low birth weight, though economically small, is greater than that reported in Currie and Rossin-Slater's (2013) study of severe storms and birth outcomes. The earlier study discerned no precise impact on birth weight or gestation alongside significant impacts on more severe outcomes. Earthquakes and hurricanes differ on several dimensions, and our findings may be reconciled by the duration of disruption, climate, and other contextual factors. Northridge was an unanticipated disaster followed by a quick recovery in terms of transportation, access to food and water, and normal daily activities. Contemporary accounts suggest that stress and mental health were the most lasting consequences of the Northridge earthquake. Our findings are most similar in magnitude to those of Carlson [21] and Black et al. [13], who study the effect of other psychological shocks on newborn health.

Results point to the potential value of counselors and other resources specifically focused on helping expectant mothers cope with stress in the wake of a natural disaster. More generally, we provide quasi-experimental evidence that maternal psychological stress does indeed lead to low birth weight, which has been previously linked to adverse outcomes over the life course. Incorporating stress management and behavioral counseling into high-risk pregnancy and maternal fetal medicine practices may yield substantial benefits in averting low birth weight and associated consequences.

Chapter 2

Do Air Quality Alerts Affect Population?

2.1 Introduction

The air quality alert program managed by the Environmental Protection Agency (EPA) is one of the most pervasive alert programs in the U.S. The purpose of the program is to minimize public exposure to air pollution by providing daily air quality information.[1] A number of studies have shown that people respond to air quality alerts by reducing the amount of time they spend outdoors (Zivin and Neidell [92]; Neidell [78]; Noonan [80]; Ward and Beatty [91]). For example, Neidell [78] shows that when smog alerts are announced, there is a 12-15 percent decrease in daily attendance at a zoo in Los Angeles. Simple rationale behind avoidance behavior is that there are greater costs of health damage due to exposure to pollutants than benefits from outdoor activities. Providing information is one of the popular policies to encourage avoidance behavior. What if the benefits lost from changing daily activities become large due to an increase in air quality alerts? Those who benefit from outdoor activities are still greater than the costs may engage in outdoor activities even during bad air quality days. But others whose costs exceed the benefits from bad air quality

[1] There is a body of evidence that air pollution negatively effects health outcomes, such as infant health (Chay and Greenstone [24]; Currie and Walker [32]), cognitive ability (Ebenstein et al. [42];Bharadwaj et al. [10]), and hospitalization (Moretti and Neidell [77]) as well as school absentism (Currie et al. [28]) and labor productivity (Graff Zivin and Neidell [57]).

may move to higher air quality areas. In other words, migration can be viewed as long run avoidance behavior. In particular, for families who are on the margin about staying in an area, air quality information may be the deciding factor.

In this paper, I analyze the effect of air quality information on population change in counties of West coast states- California, Oregon and Washington -by measuring change in the number of air alerts days from 1999-2014. Importantly, county fixed effects models controlling for the number of alert days, as well as the median air quality index (AQI), allows me to exploit marginal variation in AQI near the threshold of each air quality alert within same counties as a key influential factor. In other words, those population changes are driven by information about air quality more than air quality itself. I examine the effect of relationship between change in two different types of air alert- "Unhealthy for sensitive groups," which is defined as a day with AQI over 100, and "Unhealthy," which is defined as a day with AQI over 150, in last two previous years and percentage change in population between previous and current years (e.g., effect of change in number of unhealthy days between 2004 and 2005 on percentage change in population between 2005 and 2006).

Results show that a 10 day increase in "Unhealthy" days is associated with a 0.05 percent decrease in total population in that county. This result is robust even when I subtract birth, death, and net international migration from total population change,[2] since each of them can be affected by air quality alerts. The impact is largest for groups aged 0-9, 25-39, and 60-older, suggesting that the elderly and households having young children are most likely to respond to changes in air alerts. I also find marginally significant effects on groups aged 10-19 and 40-59, but find no significant effect on groups aged 20-24. When I look at the heterogeneous effect by state, however, individuals aged 20-24 in Oregon and Washington are more likely to move as a result of air quality alerts compared with those in California. Lastly, there is little evidence of effects from "Unhealthy for sensitive groups" days for all age groups, indicating that individuals do not response to increases in "mild" air pollution days. In particular, these results are robust when county-specific time trends are included

[2]More specifically, total population change =(birth-death)+(international migration+domestic migration)+residual changes.

in the models to account for a linear trend in the error term which might be correlated with trends in air quality and migration, possibly leading to spurious results.

To my knowledge, few papers empirically examine the effect of air quality or information about air quality on household migration. Kahn [64] examines the relationship between population growth in Los Angeles suburbs and decline in ozone during 1980-1994. He finds that a 10 day reduction in high ozone days is related to a 7.8 percent population increase compared to a county whose level remained unchanged. Based on a theoretical model that predicts relative increases in population and income for neighborhoods experiencing an exogenous marginal improvement in public goods, Banzhaf and Walsh [5] examine the impact of entry and exit of toxic facilities on population and household income between 1990-2000. Using "community" as a geographical area which is defined by half-mile-diameter circles in California, they find a 4.3-6.3 percent increase in population for exit communities and a 8.3-12.1 percent decrease in population for entrance communities. Using household level data from Public Use Micro Area (PUMA), Finney et al. [53] investigate the relationship between ozone levels and the probability of migration into the area. In particular, they examine heterogeneous effects by household's income level. Their results show that a 10 percentage point increase in an area's acceptable air quality days is associated with a 1.2 percent increase in probability that high income households move into the area, alongside a 0.4 percent decrease in the probability for low income households. Lastly, Finney [52] divides urban areas in California into those with frequent air alerts and those with few or no alerts and studies how households' relocation decisions differ across these areas. Using Public Use Micro Sample (PUMS) in the 2000 Census, he finds that air quality alerts deter households from relocating within the same high air alert area, but do not prevent households from moving from one high air alert area to a different high air alert area. He interprets these results as evidence that information affects the decision of the household to move only if the household is also knowledgeable about the local environmental conditions of the new location.

This paper complements and extends previous work in several ways. Most importantly, unlike many previous studies using AQI scores or pollution levels as key explanatory variables, this study uses the number of air alert days as an explanatory variable to directly test the effect of air quality information on household migration. As described below, air alerts are issued with health risk categories and AQI scores. Since individuals more intuitively understand air quality information through risk categories, they are also more likely to respond to changes in the number of each type of air alert if both risk alerts and AQI score are available. Thus, individuals' response to number of air alert days conditional on air quality level can be viewed as additional information on behavioral change. Second, I examine heterogeneous effects by age group. On one hand, since Neidell [78] shows that the effect of ozone on daily activities is significantly larger for children and the elderly, so perhaps these age groups are also more likely to switch residential areas in response to air alerts. On the other hand, because households having young children and the elderly have relatively lower income than households with other age groups, they may be less likely to move.[3] I examine these possibilities by dividing total population into each specific age group. Third, while previous studies use cross sectional or decennial panel data, I use yearly panel data across more than ten periods which allows us to exploit more variation in air quality and population change, thus enabling more exact estimates. In addition, while the geographic area in most previous studies is limited to California, I extend the area to include Oregon, and Washington. Results from a specific region may not be generalized to other areas in the U.S., or different patterns in population change might exist between California and other states. Lastly, more econometric control variables are included. In particular, I control for each county's meteorological conditions (temperature and precipitation), which are some of the most important confounders in estimating the relationship between ambient pollution level and migration.[4]

The remainder of the paper proceeds as follows. In section 2, I provide background information on the air quality alert program. Data and empirical strategy are laid out in section 3. The results are presented in section 4. Robustness checks are given in section 5. Section 6 concludes.

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[3] According to the U.S. census, in 2011 median income of householders aged between 25 and 34, 35 and 44, 45 and 54, 55 and 64, and 65 years and over was $50,774, $61,916, $63,861, $55,937, and $33,118, respectively.

[4] In particular, there is a strong positive correlation between temperatures and ground-level ozone. Also, there is evidence that air pollution suppresses precipitation level by creating a narrow cloud droplet spectrum. For details, refer to Union of Concerned Scientists [90] and Jirak and Cotton [62].

2.2 Air quality alert program

Concentrations of ozone, particulate matter 2.5 ($PM_{2.5}$), particulate matter 10 (PM_{10}), carbon monoxide (CO), and sulfur dioxide (SO_2) are used to calculate AQI from a formula developed by EPA. These air pollutants are collected from individual monitoring sites daily.[5] Of the five AQIs calculated each day, the highest value is issued as the AQI for that day. The AQI report contains the reporting area, reporting period, critical pollutant, AQI, category descriptor, and the associated color. Figure 2.1 shows each category descriptor and its color. National air quality standards for each pollutant generally lie between AQI of 0-100. When AQI exceeds 100, reporting agencies are expected to share an alert with all major news media. When "Unhealthy for sensitive groups" is issued, members of sensitive groups may experience health effects, but the general public is not likely to be affected. When "Unhealthy" is issued, everyone may begin to experience health effects, and in particular, members of sensitive groups may experience more serious health effects.[6] The AQI report may also contain health effects and cautionary language, but these are not required.[7]

The feature which most distinguishes the air quality alert program from other alert programs is its pervasiveness. As Finney [52] mentioned, individuals may have little reason to seek out AQI unless they are in an area with air quality alerts. According to the Environmental Protection Agency [45], the law states that metropolitan statistical areas (MSAs) with populations exceeding 350,000 are required to report AQI to the general public daily through local media (newspapers, radio, television) or internet sites. Many smaller communities also report the AQI as a public health service. Moreover, as shown in Figure 2.2, the EPA website provides software allowing the public to compare air quality between counties in terms of the number of air quality alert days.[8] The mandatory alerts issued

[5] Figure B1 in appendix A shows the distribution of monitoring sites in CA, OR, and WA.

[6] https://airnow.gov/index.cfm?action=aqibasics.aqi During 2000-2014, up to 136 "Unhealthy for sensitive groups" alerts per year were issued from California, versus maximum yearly "Unhealthy for sensitive groups" alerts of 30 for Oregon and 27 for Washington. From 2000-2014, 76.40 percent and 86.25 percent of days in Oregon and Washington, respectively, did not have any "Unhealthy" alerts, compared to only 50.83 percent of days in California.

[7] For example, the health effects of an "Unhealthy" day driven by ozone are stated as "Greater likelihood of respiratory symptoms and breathing difficulty in active children and adults and people with lung disease, such as asthma; possible respiratory effects in general population."

[8] https://www3.epa.gov/aircompare/compare.htm

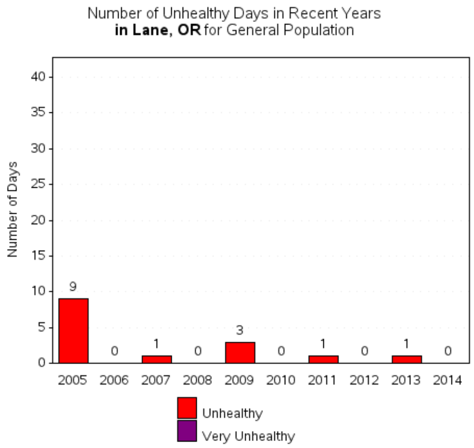
Air Quality Index (AQI) Values	Levels of Health Concern	Colors
When the AQI is in this range:	*..air quality conditions are:*	*...as symbolized by this color:*
0-50	Good	Green
51-100	Moderate	Yellow
101-150	Unhealthy for Sensitive Groups	Orange
151 to 200	Unhealthy	Red
201 to 300	Very Unhealthy	Purple
301 to 500	Hazardous	Maroon

Figure 2.1: Air quality index

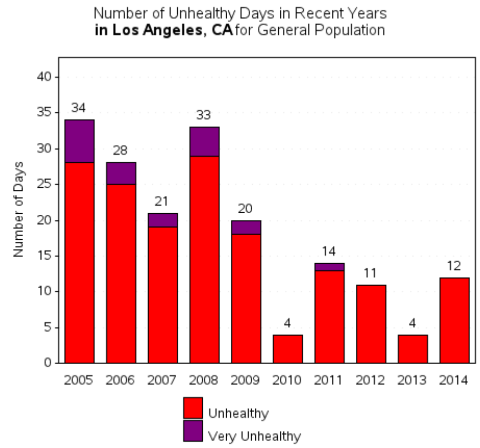
through a variety of mass media along with the easily accessible EPA website tool make the public well informed about local air quality.[9]

A couple points must be mentioned before the next section. First, EPA distinguishes "Unhealthy for Sensitive Groups" days as those with AQI between 101-150, but this category in this study is defined as a day with any AQI over 100 because "Unhealthy for Sensitive Groups" is automatically included in a higher category (e.g., if a day is "Unhealthy", then that day is also "Unhealthy for Sensitive Groups"). Similarly, I define an "Unhealthy" day as one with AQI over 150. Although most of study areas in this study use the federal standard, for few areas, they use a slightly different standard. For example, the San Joaquin Valley Air Pollution Control District in California uses the latest hourly data, instead of the 8-hour ozone or 24-hour PM to calculate the index. Also, the threshold to show health effects of $PM_{2.5}$ exposure for the Washington Air Quality Advisory (WAQA) is less than the EPA standard. However, because these areas also report air quality using the same AQI scale from EPA and the differences are marginal, I use each county's number of air alert days as redefined in this study to compare air quality across study areas.

[9]Information about air quality alerts are also obtained from a variety of sources such as variable message sign on highway or weather application in smart phone.



(a) Number of air alerts days in Lane, OR



(b) Number of air alerts days in LA, CA

Figure 2.2: Comparison of air quality between different counties from EPA website

2.3 Data and Model

2.3.1 Data

The geographic units of this study are counties in California, Oregon, and Washington state. While most previous studies limit their study area to California, I extend the area to Oregon and Washington, which are included in "Pacific states division."[10] Data are merged from several sources. AQI scores, the number of each category of unhealthy days, and median annual AQI over the years 1995-2014 are drawn from the EPA Air Quality Index Report. In particular, the report contains the number of days on which measurements from any monitoring site in the county were reported to the air quality system database. Since the number of AQI reporting days is different across counties and years, in a baseline model I drop counties and years which report AQI less than 300 days per year. These observations account for 13.72 percent of the beginning sample. Since median AQI of the excluded samples is lower than that of non-excluded samples,[11] this might significantly change the baseline model results. Thus, in a robustness check, I add back counties that report AQI less than 300 days per year. The main analytical sample includes an unbalanced panel of 112 counties. Table B1 in Appendix B shows the list of counties and number of panel years

[10]The other two states in Pacific states division are Alaska and Hawaii.

[11]Mean AQI of excluded samples is 23.31 whereas it is 39.13 for samples in baseline model.

used in this study. Following the Banzhaf and Walsh [5] model showing that improvement in environmental quality leads to an increase in the population of a community, I construct the explanatory variable as the difference in the number of each type of unhealthy day between t-2 and t-1, which is more appropriate for capturing individuals' behavioral response to information about air quality than using AQI level under the circumstance that the public is widely informed about the air quality alert program.

Data describing population and economic activities in a county come from the U.S. Census. The population and population density come from the intercensal and vintage estimates. I use percentage change in population from t-1 to t as a dependent variable which takes values between 0 and 100. Population density per square mile of each county is calculated by dividing total population by land area. Poverty rate and median household income are from small area income and poverty estimates. Percentage of manufacturing firms, total number of establishment, and total employment in a county come from county business patterns, and total number of building unit permits are from building permit estimates. Unemployment rate comes from the Bureau of Labor Statistics, and pupil to teacher ratio and number of primary and secondary school come from common core of data by the Nation Center for Education Statistics. Temperature and precipitation data come from the National Centers for Environmental Information by the National Oceanic and Atmospheric Administration, and median home price per square foot is from Zillow.com.[12] Table B2 and B3 in appendix B show lists of variables and summary statistics.

Before describing the econometric model, I draw percentage change in population by improvement in air alert days in Figure 2.3 to check whether there is a visual correlation between air quality alert and population change. I define counties' air alert days as improved if numbers of each type of unhealthy day at t-1 are less than those at t-2.[13] In particular, to make the comparison clear, I divide counties into largely improved counties and counties which became worse throughout the sample years. Largely improved counties are those in which the number of air quality days improved for more that 3 years after 2007. For

[12]If data on median price of house per square foot, and temperature and precipitation variables do not exist for a given county, I substitute values for the closest county. Results are robust without controlling for median home price per square foot.

[13]For example, if county i has 20 "Unhealthy" days in 2005 and 10 "Unhealthy" days in 2006, this county is defined as an improved county.

(a) Unhealthy for Sensitive Groups (b) Unhealthy

Figure 2.3: Population change trend by improvement in air alert days

poorly improved counties, the opposite criteria is used.[14] For "Unhealthy for Sensitive Groups", there is clearly a larger population increase in largely improved counties compared to the poorly improved counties throughout the sample years. For "Unhealthy", even though population increase in poorly improved counties is generally larger than in largely improved counties until 2005 and in 2010, for rest of the sample years, largely improved counties experience higher population change. In sum, Figure 2.3 suggests that individuals may respond when they obtain information about air quality alerts by relocating residence. In the next section, I will examine this trend more systematically by controlling for a large set of potential confounders in the econometric model, including unobserved county heterogeneity.

2.3.2 Method

In order to examine how additional information about air quality affects households' relocation, in the following estimation, I assume that individuals are well informed with regards to the air alert program, and that the observe "change" in air alerts between the previous two years, t-2 and t-1. Based on information at time t-1, they make decisions about whether move into a better air quality area or stay in the current area at time t. For baseline

[14] From this criteria, there are 7 largely improved counties (Contra Costa, Inyo, Placer, San Mateo, Josephine, Chelan, and Yakima) and 3 poorly improved counties (Humboldt, Santa Cruz, and Sonoma) in (a), and 3 largely improved counties (Los Angeles, Linn, and Snohomish) and 5 poorly improved counties (Imperial, Solano, Tuolumne, Ventura, and Walla Walla) in (b).

results, I estimate equation (1) using a county fixed effect model:

$$\triangle\%Pop_{it} = \beta_0 + \beta_1 \triangle UHD_{it-1} + \beta_2 MedAQI_{it-1} + \beta_3 \mathbf{X}_{it-1} + I_i + \gamma_t + \sum_{T=1}^{14} T * I_i + \epsilon_{it} \tag{2.1}$$

where i indicates county and t is year. $\triangle\%Pop_{it}$, which is a dependent variable, is the percentage change in population, $\triangle UHD_{it-1}$ is the lagged change in number of days with AQI over 100 or 150,[15] $MedAQI_{it-1}$ is the median AQI score, X is vector of covariates, γ_t are year dummies, I_i are county dummies so that $\sum_{T=1}^{14} T * I_i$ captures a county-specific time trend, and ϵ_{it} is error of the model. In particular, county specific time trend controls for a linear trend in the error term which might be correlated with trends in air quality and migration. For $\triangle\%Pop_{it}$, in addition to total population, age groups 0-9 and 25-39, 10-19 and 40-59, 20-24, and 60-older are examined. I bundle age 0-9 and 25-39, and 10-19 and 40-59 together to recognize typical family compositions. All regressions are also weighted by population at each age category. Again, to the extent that median AQI is controlled in the model, and the variation in air quality alerts is driven by small changes in AQI near the threshold of each air quality alert within same counties, the parameter β_1 captures the effect of changes in information about air quality, not changes in air quality level. I expect that β_1 will show significantly negative for each type of unhealthy date if individuals respond to air alerts, other things being equal.

In order to examine the different effects of air pollution across states, I modify equation (1) to be:

$$\begin{aligned} \triangle\%Pop_{it} = {} & \beta_0 + \beta_1 \triangle UHD_{it-1} + \beta_2(\triangle UHD_{it-1} \times OR) + \beta_3(\triangle UHD_{it-1} \times WA) \\ & + \beta_4 MedAQI_{it-1} + \beta_5 \mathbf{X}_{it-1} + I_i + \gamma_t + \sum_{T=1}^{14} T * I_i + \epsilon_{it} \end{aligned} \tag{2.2}$$

where OR is an indicator for Oregon state and WA is an indicator for Washington state. In equation (2), β_1 is the coefficient of non-interaction, $\triangle UHD_{it-1}$ represents the effect of

[15]For baseline results, equation (1) is separately estimated by "Unhealthy for Sensitive Groups" and "Unhealthy" because interpretation of coefficients in equation (2) is more intuitive. However, single estimation equation results using both explanatory variables simultaneously are also similar with the baseline results (see Table B4).

California, and β_2 and β_3 represent relative effects of Oregon and Washington to California, respectively.

2.4 Results

Table 2.1 shows the results of estimating equation (1). Panel A contains results for change in the number of "Unhealthy for Sensitive Groups" days, and panel B contains results for change in the number of "Unhealthy" days. Since "Unhealthy" indicates worse air quality relative to "Unhealthy for Sensitive Groups", I would expect the effects to be stronger in panel B. In panel column of 2.1, column (2)-(5) display results for subgroups of the total population.

In panel A, none of the coefficients are significant. Thus, I can infer that individuals do not respond to increases in information about "mild" air pollution days. On the other hand, the coefficient of column (1) in panel B is negative and significant at 1 percent level. This implies that a 10 day increase in "Unhealthy" days between two previous years is associated with an approximately 0.05 percent decrease in total population. Based on the mean percent of total population change (0.9 percent), this effect is equivalent to a 5.6 percent decrease in total population. In terms of the effect on each subgroup, columns (2) and (5) show significantly negative coefficients. Coefficient of age group 0-9 and 25-39 is -0.006, which is the largest, suggesting that households having young children are more likely to respond to changes in air quality. In particular, the coefficient on age group 60-older is also significant at 1% level and the results correspond to Neidell [78], showing that the effect of ozone on daily activities is significantly larger for children and the elderly.[16] Moreover, similar to Ebenstein et al. [42], the results show a small impact for modest pollution days, but a much larger impact for more polluted days.[17] Even though I don't find a significant effect on age groups 10-19 and 40-59, the t-values of the coefficient are -1.65 indicating it is very close to significance at 10% level.

[16] Table B5 in appendix B which shows results when I use each age group as a dependent variable also confirms this.

[17] Even though the modest pollution days and very polluted days are defined at lower AQI scores than standard from U.S. EPA in their study, they show non-linear impacts of air pollution on cognitive ability.

Table 2.1: Baseline results

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Panel A					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 100)	-0.0012	-0.0019	-0.0016	0.0041	-0.0009
	(0.0022)	(0.0030)	(0.0018)	(0.0053)	(0.0013)
N	1310	1310	1310	1310	1310
R^2	0.823	0.808	0.937	0.719	0.908
Panel B					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.0047***	-0.0062**	-0.0029	-0.0067	-0.0045***
	(0.0018)	(0.0027)	(0.0018)	(0.0054)	(0.0015)
N	1310	1310	1310	1310	1310
R^2	0.824	0.809	0.937	0.719	0.909

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Even though results in Table 2.1 show an insignificant effect of change in "Unhealthy for Sensitive Groups," and a significant effect of change in "Unhealthy" on each age group, the results can be different by state. Table 2.2 shows the results of estimating equation (2). In panel A, change in "Unhealthy for Sensitive Groups" effects on all age groups are still not significant for counties in California. However, for counties in Oregon and Washington, the effects on some sub-age groups are significant now. Coefficients in column (4) and (5) for Oregon indicate that for counties in Oregon, relative effects of a 10 day increase in "Unhealthy for Sensitive Groups" between two previous years on decrease in population age 20-24 and 60-older are 0.53 and 0.07 percent points larger than the effects on counties in California, respectively. Similarly, for counties in Washington, the relative effects on increase in population age 10-19 and 40-59 and on decrease in population age 20-24 are 0.08 and 0.35 percent points greater than the effects on counties in California, respectively. A similar pattern of heterogeneous effects for change in "Unhealthy" on ages between 20 and 24 are also observed in panel B. Another interesting fact is that the results for counties in California in panel A and B are almost identical to the results of Table 2.1. This suggests that the baseline results are mainly driven by results from California counties. These results can be explained by the fact that, as mentioned at section 2, "Unhealthy" days in Oregon and Washington are rare whereas there are relatively many "Unhealthy" days in California.

Table 2.2: Heterogenous effects by state

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Panel A					
Change in "Unhealthy for Sensitive Groups"	-0.0012	-0.0023	-0.0016	0.0060	-0.0007
	(0.0023)	(0.0031)	(0.0018)	(0.0052)	(0.0013)
Change in "Unhealthy for Sensitive Groups" × OR	-0.0025	0.0080	-0.0004	-0.0533**	-0.0065*
	(0.0046)	(0.0083)	(0.0047)	(0.0241)	(0.0037)
Change in "Unhealthy for Sensitive Groups" × WA	0.0010	0.0077*	0.0007	-0.0352**	0.0017
	(0.0021)	(0.0043)	(0.0027)	(0.0140)	(0.0057)
N	1310	1310	1310	1310	1310
R^2	0.823	0.809	0.937	0.721	0.908
Panel B					
Change in "Unhealthy"	-0.0047**	-0.0063**	-0.0029	-0.0059	-0.0044***
	(0.0018)	(0.0027)	(0.0018)	(0.0057)	(0.0015)
Change in "Unhealthy" × OR	-0.0017	0.0088	-0.0006	-0.0467*	-0.0056
	(0.0046)	(0.0083)	(0.0045)	(0.0238)	(0.0039)
Change in "Unhealthy" × WA	-0.0001	0.0055	-0.0008	-0.0304**	0.0012
	(0.0025)	(0.0048)	(0.0028)	(0.0135)	(0.0055)
N	1310	1310	1310	1310	1310
R^2	0.824	0.809	0.937	0.721	0.909

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.5 Robustness checks

2.5.1 Change in Specification

I test whether the findings are robust to several specification changes. To alleviate concerns about change in the results by choice of number of AQI days, I examine the results when all counties are included regardless of the number of AQI days, and when only complete AQI days are included in the sample. In addition, I include the square term of an explanatory variable to check whether there is a non-linear relationship between air pollution change and population change. Moreover, in order to capture lagged response of individuals, I include an average explanatory variable for the previous three years. Finally, I exclude the number of births, deaths, and international migration which could be also affected by air quality alerts. This ensures that the change in population results solely from households' migration.

Table 2.3 contains tests of the sensitivity of the results to specification changes, with specifications for "Unhealthy for Sensitive Groups" in first four rows (panel A1-A5) and specifications for "Unhealthy" in last four rows (panel B1-B5). Rows A1 and B1 show the results from all observations regardless of the number of AQI days. Remember that results in Table 2.1 are from observations with AQI for more than 300 days, and they have higher mean

AQI than that of observations with AQI days less 300 days. 176 observations are included in rows A1 and B1 that are not included in the baseline. The magnitude of the estimated effects in both panel A1 and B1 are almost identical, as shown in Table 2.1. When I use more stringent observations with only complete AQI days, 336 observations are dropped, as shown in panels A2 and B2. The effects are very similar to those in Table 2.1, except there is a marginally significant effect on ages 10-19 and 40-59 in panel B2. When I include the square term of lagged change in the number of days with AQI over 100 or 150, in panels A3 and B3, the effect of "Unhealthy" days on age group 10-19 and 40-59 is also marginally significant, and estimated coefficients on age group 0-9 and 25-39 and 60-older are still significant, while estimated coefficients for "Unhealthy for Sensitive Groups" remain insignificant. Also, I find no non-linear relationships between air pollution change and population change in any case. In panel A4 and B4, I report results when I include the previous three years average lagged change in the number of days with AQI over 100 or 150. Nevertheless, although 47 observations are dropped from the sample because several counties do not have AQI data between 1995 and 1998, the results are almost identical with the baseline results at Table 2.1. Lastly, rows A5 and B5 show the results with net domestic migration as a dependent variable. I run regression only for total population because the data are not available by age group. The magnitude of the estimated effects in B1 becomes larger and still significant indicating the baseline results are largely due to households' migration. In sum, results in Table 2.3 show that the baseline results in Table 2.1 are robust to changes in model specification.[18]

2.5.2 Effect of "Moderate" and "Good" days

If individuals migrate from poor air quality areas to cleaner air areas in response to information about air alerts, I might observe that the population in counties experiencing improved air quality alert category increases. To test this, I estimate equation (1) using "Moderate" days, which are defined as AQI below 100, and "Good" days, which are defined as AQI below 50, as the explanatory variables. Even though these days are not as widely

[18] I also examine the results without Los Angeles county or Riverside county to check whether a large or unusual county may be driving result. Moreover, in an attempt to take into account the distribution of air quality in a given county-year, I run the model including 90^{th} percentile AQI or maximum AQI as well as median AQI. Overall results are similar with results in Table 2.1. See Table B6 and Table B7 in appendix.

Table 2.3: Robustness and specification tests

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Panel A1 (All counties)					
Change in "Unhealthy for sensitive groups"	-0.0011	-0.0020	-0.0015	0.0045	-0.0006
	(0.0022)	(0.0030)	(0.0017)	(0.0052)	(0.0014)
N	1486	1486	1486	1486	1486
R^2	0.818	0.801	0.935	0.717	0.902
Panel A2 (Counties with complete days with AQI)					
Change in "Unhealthy for sensitive groups"	-0.0013	-0.0019	-0.0017	0.0031	-0.0010
	(0.0023)	(0.0031)	(0.0019)	(0.0053)	(0.0013)
N	974	974	974	974	974
R^2	0.830	0.819	0.940	0.740	0.914
Panel A3 (Include square term)					
Change in "Unhealthy for sensitive groups"	-0.0012	-0.0018	-0.0016	0.0041	-0.0008
	(0.0023)	(0.0031)	(0.0018)	(0.0055)	(0.0013)
(Change in "Unhealthy for sensitive groups")^2	0.0000	0.0001	0.0000	0.0000	0.0000
	(0.0000)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
N	1310	1310	1310	1310	1310
R^2	0.823	0.809	0.937	0.719	0.908
Panel A4 (Include previous 3 years average)					
Change in "Unhealthy for sensitive groups"	-0.0017	-0.0037	-0.0011	0.0047	-0.0011
	(0.0016)	(0.0027)	(0.0015)	(0.0050)	(0.0014)
N	1263	1263	1263	1263	1263
R^2	0.823	0.809	0.937	0.723	0.909
Panel A5 (Domestic migration as a dependent variable)					
Change in "Unhealthy for sensitive groups"	-0.0002				
	(0.0026)				
N	1310				
R^2	0.854				
Panel B1 (All counties)					
Change in "Unhealthy"	-0.0045**	-0.0057**	-0.0029	-0.0060	-0.0047***
	(0.0018)	(0.0027)	(0.0018)	(0.0052)	(0.0015)
N	1486	1486	1486	1486	1486
R^2	0.819	0.802	0.935	0.717	0.903
Panel B2 (Counties with complete days with AQI)					
Change in "Unhealthy"	-0.0050***	-0.0066**	-0.0033*	-0.0074	-0.0041***
	(0.0018)	(0.0027)	(0.0018)	(0.0058)	(0.0015)
N	974	974	974	974	974
R^2	0.832	0.820	0.940	0.741	0.914
Panel B3 (Include square term)					
Change in "Unhealthy"	-0.0048**	-0.0058*	-0.0033*	-0.0073	-0.0045***
	(0.0019)	(0.0030)	(0.0019)	(0.0055)	(0.0015)
(Change in "Unhealthy")^2	0.00000	0.0000	0.0000	0.0001	0.0000
	(0.0000)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
N	1310	1310	1310	1310	1310
R^2	0.824	0.809	0.937	0.719	0.909
Panel B4 (Include previous 3 years average)					
Change in "Unhealthy"	-0.0048***	-0.0061**	-0.0031*	-0.0067	-0.0046***
	(0.0017)	(0.0029)	(0.0018)	(0.0051)	(0.0015)
N	1263	1263	1263	1263	1263
R^2	0.824	0.809	0.937	0.723	0.909
Panel B5 (Domestic migration as a dependent variable)					
Change in "Unhealthy"	-0.0057**				
	(0.0022)				
N	1310				
R^2	0.855				

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: Effect of "Moderate" and "Good" days

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Panel A					
Change in "Moderate"					
(Change in number of days with AQI below 100)	0.0011	0.0020	0.0011	-0.0021	0.0004
	(0.0012)	(0.0018)	(0.0009)	(0.0031)	(0.0007)
N	1310	1310	1310	1310	1310
R^2	0.823	0.809	0.937	0.719	0.908
Panel B					
Change in "Good"					
(Change in number of days with AQI below 50)	0.0011*	0.0019*	0.0014***	-0.0011	-0.0003
	(0.0007)	(0.0011)	(0.0005)	(0.0020)	(0.0006)
N	1310	1310	1310	1310	1310
R^2	0.824	0.809	0.937	0.719	0.908

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

announced as days with AQI exceeding 100, if information about air quality alert is an important factor of individuals' migration, they are likely to search the information on "Moderate" and "Good" days for a new location through a variety of sources. The results are shown in Table 2.4. Panel A shows the effects of change in "Moderate". The effects are not significant for all age groups. This result is similar to results shown in Table 2.1 to the extent that people do not respond to "mild" change in air quality. For "Good", however, results in panel B show that change in "Good" days is related to increase in population. More specifically, a 10 day increase in "Good" between two previous years is associated with a 0.01 percent increase in total population in ages 10-19 and 40-59, with a 0.02 percent increase in ages 0-9 and 25-39, respectively. Although the coefficient on the age 60-older group is negative, it is insignificant.

2.5.3 Heterogeneous effect by household income

As Finney et al. [53] show, household income is one of many dimensions of the heterogeneous effect. Since migration may be considered a luxury good,[19] higher income households are more likely to respond to air alerts by changing their residence. One way to examine this with county level data is to classify counties by median household income level. Following, Finney et al. [53], I generate indicators for low (below the 25th percentile), middle (between the 25th and 75th percentiles) and high (above the 75th percentile) median income counties and

[19] As shown at Table B8 in appendix where median household income is used as a dependent variable in equation (1), median household income of counties falls slightly following a rise in "Unhealthy" days.

Table 2.5: Heterogenous effects by household income of counties

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Panel A					
Change in "Unhealthy for Sensitive Groups"	0.0030	0.0062	0.0038	-0.0037	-0.0021
	(0.0019)	(0.0039)	(0.0030)	(0.0149)	(0.0024)
Change in "Unhealthy for Sensitive Groups" × Middle income	-0.0037	-0.0071	-0.0052	0.0088	0.0005
	(0.0033)	(0.0049)	(0.0040)	(0.0159)	(0.0026)
Change in "Unhealthy for Sensitive Groups" × High income	-0.0068**	-0.0130**	-0.0075**	0.0048	0.0030
	(0.0032)	(0.0057)	(0.0034)	(0.0161)	(0.0032)
N	1310	1310	1310	1310	1310
R^2	0.825	0.810	0.937	0.723	0.908
Panel B					
Change in "Unhealthy"	0.0005	0.0011	0.0020	0.0060	-0.0069**
	(0.0032)	(0.0052)	(0.0056)	(0.0218)	(0.0035)
Change in "Unhealthy" × Middle income	-0.0054	-0.0071	-0.0050	-0.0159	0.0023
	(0.0039)	(0.0062)	(0.0063)	(0.0224)	(0.0038)
Change in "Unhealthy" × High income	-0.0034	-0.0098	-0.0058	0.0222	0.0054
	(0.0107)	(0.0159)	(0.0103)	(0.0299)	(0.0101)
N	1310	1310	1310	1310	1310
R^2	0.826	0.810	0.937	0.724	0.909

Note: Low, middle, and high income counties are defined as counties with median household income below 25th percentile, between 25th and 75th, and above 75th percentile in sample, respectively. Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

estimate the heterogeneous effect by including interaction terms with each type of unhealthy day, like in equation (2). As shown at panel B and panel A in Table 2.5, compared with low income counties, coefficients for groups aged 0-9 and 25-39 and 10-19 and 40-59 in middle and high income counties are negative. However, the only significant result is interaction term between "Unhealthy for Sensitive Groups" and high income counties. This is probably because county level data are aggregated with individual household, and thus they are not as precise as individual households' income data. With individual income data, I expect that more precise results can be estimated. For age group 60-older, however, low income counties show negative coefficients compared to middle and high income counties. As noted in footnote 3, there is significant difference in median income of householders between the elderly and other groups. If the income gap is relatively small between low income counties and middle and high income counties, the elderly could respond more to air alerts than other age groups. Again, through individual income data, I could examine this possibility.

2.6 Conclusion

In this paper, I provide evidence that individuals respond to information about relatively strong air quality alerts by changing their residential area. Exploiting attributes of panel data, which allow us to control many unobservable time invariant confonders of counties, the estimation shows that a 10 day increase in “Unhealthy” days between the last two previous years is related to a 0.05 percent decrease in total population of a county, and that the effects are greatest in age groups having young children and elders. Moreover, compared to the same age group in California, individuals in the age 20-24 group in Oregon and Washington are strongly affected by the air alerts. By controlling for air quality level in the model, these effects capture the effect of information about air quality, rather than air quality level.

Even though I include county specific time trends as well as many control variables in the model, and conduct several robustness checks to identify causal effect, some issues need to be addressed further. There are still potential confounders related to air pollution levels that are not included in the model. In particular, local economic characteristics maybe not be fully captured by unemployment rate, poverty rate, percentage of manufacturing firms, total number of establishments, and employment. One way to alleviate this problem is using instrumental variables which affect local economics only through air pollution levels, but in presence of data restrictions, it is hard to find those variables in this study.[20] However, to the extent that air pollution is likely to be positively correlated with local economic activities, the estimation can be considered as a lower bound to the impact of air quality alerts on migration. Moreover, the main results do not show whether population change occurs as a result of immigration or outmigration. Although in robustness checks I showed that the baseline results are still valid when I use domestic migration as a dependent variable, I can also classify domestic migration by immigration and outmigration. Unfortunately, intercensal and vintage estimates of the U.S. Census do not contain information about immigration and outmigration by county level. The only source of this information is the American

[20]For example, Bayer et al. [8] use a contribution of distance sources calculated from a source-receptor matrix as an instrumental variable for local air quality. However, while their study areas include whole parts of U.S., including the midwest and northeast, the study area is restricted to western states where distance emission matters less matter due to weather patterns. Moreover, for this study, I need panel version of distance source data set across 10 periods which is computationally intensive.

Community Survey (ACS) 5-year estimates, but this is cross sectional data which average variables across 5 years. Using ACS data, I show some evidence in Table B9 in appendix that an increase in "Unhealthy" days is negatively correlated with immigration, but these results are not conclusive because of the cross sectional variation with small sample size. I need to look further at this issue when relevant data are available.

Overall, in addition to previous studies showing avoidance behavior based on daily activities, the results in this paper are consistent with the idea that individuals are responsive to air quality and willing to move.

Chapter 3

Effect of Radon Map on Housing Price

3.1 Introduction

Providing information is commonly seen as one of the most cost effective policies for influencing individuals' behavior. Due to its significant effect on health in short term and the accumulation of human capital such as educational attainment and employment in the long term,[1] policy makers try to minimize individuals' exposure to pollution through a variety of programs, such as The Toxics Release Inventory and the Air Quality Alert Program from Environmental Protection Agency (EPA). Several papers examine how people behave when they obtain new information about pollution (Madajewicz et al., 2007; Shimshack, Ward and Beatty 2007; Neidell 2009; Graff Zivin, Neidell and Schlenker 2011). These studies show that individuals actually respond to new information by changing their behavior. For example, using purchase data from a national grocery chain, Graff Zivin, Neidell and Schlenker (2011) find that after introducing Safe Drinking Water Act (SDWA) the bottled water sales increased by 17-22 percent in the Northern California and Nevada areas. In spite of the importance, however, there are only a small amount of empirical studies focusing on how prevalence and design of information play an important role in influencing individuals' behavior.

[1] See Currie et al. [30], Almond et al. [4], Field et al. [49], Currie and Walker [33], and Sanders [84].

In this paper, I investigate whether the release of a map of radon zones from the EPA affects individual behavior by looking at the house price changes in California counties between 1991 and 1995. Radon gas, which is considered second leading cause of lung cancer in United States, typically moves through the ground into the home. In September 1993, the EPA released a county-by-county basis map dividing 3,141 counties into 3 zones according to their potential for elevated indoor radon levels. If new information from this map was well known to the public and the residents were sensitive to health effect of radon exposure, I might observe a change in house price between high radon level counties and low level counties due to either decreased demand or decreased willingness to pay for house in high level zones before and after the map is released. Exploiting variation in radon level across counties and timing of map release in difference-in-difference estimator, I find that, after the map is released, there is no significant difference in house price between high radon level counties (zone 1 and zone 2 counties) and low level counties (zone 3 counties). In particular, even when I define only zone 1 as the high radon level counties, the difference still not observable. To the extent that the primary purpose of the map is to assist to government administration, and the map is designed in an unsophisticated format without containing detailed information on health effects, the results are consistent with the previous studies showing that individuals change their behavior when the information is well-specified as well as is widely known to the public.

The remainder of the paper proceeds as follows. In section 2, I provide background information on Map of Radon zones. The data and empirical strategy are laid out section 3. The results are presented in section 4. Robustness checks are given in section 5. Section 6 concludes.

3.2 Background

Radon is a colorless, odorless, radioactive gas released from decaying of uranium in nearly all soils. It is the second leading cause of lung cancer after smoking in United States.[2]

[2] There has been a suggestion of increased risk of leukemia associated with radon exposure in adults and children; however, the evidence is not conclusive (National Cancer Institute).

According to EPA, radon is responsible for about 21,000 lung cancer deaths every year. It typically moves through the ground to the air above and within homes through cracks and openings in the foundation (Gundersen and Schumann 1993). In September 1993, as a consequence of 1988 Indoor Radon Abatement Act (IRAA), EPA released county-by-county basis map dividing 3,141 counties into 3 zones. Based on geological data, aerial radiometric data, soil characteristics data, indoor radon data, and building architectural data, counties are classified to zone 1 which indicates the highest potential for elevated indoor radon levels if they have a predicted average indoor screening level greater than 4 pCi/L, to zone 2 if they have the level between 4 pCi/L and 2 pCi/L, and to zone 3 if they have the level less than 2 pCi/L.

In fact, the map is primarily designed to help in radon reduction programs or in implementation of radon resistant building codes of local and state governments without any regulatory requirement, not to inform to the public. Moreover, as shown at Figure A1, the map is designed in an unsophisticated format in terms of information. Even though the map recommends all homes to test indoor radon level regardless of zone designation, there is no further information such as health effects across different zones. These features are in obvious contrast with other mandatory programs such as Air Quality Alert Program which information has to be reported to the general public along with the health concern through a variety of mass media.[3]

3.3 Data and model

3.3.1 Data

The time window for the analysis is between 1991 and 1995. Monthly median house price of single-family detached homes in each county is publicly available from California Association of Realtors. I use yearly averages of median house price, which is adjusted to 2000 price

[3] The law states that metropolitan statistical areas (MSAs) with populations exceeding 350,000 are required to report AQI to the general public daily through local media (newspapers, radio, television) or internet sites. Many smaller communities also report the AQI as a public health service.

level, as the dependent variable. The house price for twenty four counties[4] is not available during the time window, so I exclude these counties from the analysis. According to the map of radon zones for California, two counties are classified as zone 1, fifteen counties are classified as zone 2 and seventeen counties are classified as zone 3 in the sample.[5] Since there are only two counties in zone 1, I assigned both zone 1 and zone 2 counties as "high radon level" counties, which are defined as treatment group, and zone 3 counties as "low radon level" counties, which are defined as the control group in our basic estimation. Economic characteristics and particulate matter (PM10) level data of each county are from U.S. Census and EPA.

Descriptive statistics of variables by group are shown in Table 1. The yearly average median house price is approximately $48,000 larger in treatment group than control group. In terms of racial composition, there is larger percentage of non-white people living in treatment group. Individuals in the treatment group have a higher level of personal income than individuals in control groups and the number of crimes per capita in treatment group is higher. Lastly, counties in the treatment group have both a higher population density, a larger land area, a higher population change, and a poorer air quality in terms of PM10 than counties in control group. However, there is no statistically significant difference in unemployment rate, number of public school enrollment per capita, and personal income change between treatment and control group. Since the differences between the treatment and the control group can affect the outcomes potentially, I will control these variables in the estimation.

Figure 1 shows the average median house price over time separately by treatment and control group. I can see the parallel downward trend in house prices before 1993 in both groups suggesting there is decrease in house price all across California. This downward trend continued in 1994 and 1995, but the drop in house prices of the treatment group at 1995 is significantly larger than control group. In next section, I will examine this trend more

[4]Alpine, Amardor, Calaveras, Colusa, Del Norte, El Dorado, Glenn, Imperial, Inyo, Lassen, Madera, Mariposa, Modoc, Mono, Nevada, Plumas, San Denito, Sierra, Sutter, Tehama, Trinity, Tuolumne, Yolo, Yuba

[5]Zone 1: Santa Barbara and Ventura. Zone 2: Alameda, Contra Costa, Fresno, Kern, Los Angeles, Monterey, Placer, Riverside, San Bernardino, San Francisco, San Luis Obispo, San Mateo, Santa Clara, Santa Cruz and Tulare. Zone 3: Butte, Humboldt, Kings, Lake, Marin, Mendocino, Merced, Napa, Orange, Sacramento, San Diego, San Joaquin, Shasta, Siskiyou, Solano, Sonoma, and Stanislaus.

Table 3.1: Descriptive statistics by controlled and treated groups

	(1) Control groups	(2) Treatment group	(3) Difference ((1)-(2))
Yearly averages of median house price (Thousand dollar)	184.408	232.127	-47.719***
	(9.805)	(98.684)	(13.094)
Percentage of Asian	0.065	0.101	-0.036***
	(0.006)	(0.009)	(0.011)
Percentage of Black	0.036	0.062	-0.026***
	(0.004)	(0.005)	(0.007)
Percentage of Hispanic	0.163	0.256	-0.094***
	(0.011)	(0.011)	(0.016)
Percentage of White	0.880	0.827	0.053***
	(0.008)	(0.012)	(0.015)
Personal income (Thousand dollar)	25.901	28.025	-2.124*
	(0.929)	(0.680)	(1.125)
Unemployment rate	9.807	9.198	0.610
	(0.481)	(0.392)	(0.614)
Number of crimes per capita	0.006	0.008	-0.002***
	(0.000)	(0.000)	(0.001)
Population density	512.547	1491.104	-978.557**
	(100.378)	(391.896)	(450.607)
Public school enrollment per population	0.178	0.173	0.005
	(0.004)	(0.004)	(0.006)
Land area (mi^2)	2083.029	3914.739	-1831.71***
	(190.595)	(511.126)	(600.405)
PM10 level of county	81.242	98.131	-16.889*
	(3.787)	(6.116)	(7.831)
Population change (%)	0.009	0.012	-0.003*
	(0.001)	(0.001)	(0.002)
Personal income change (%)	0.031	0.033	-0.001
	(0.002)	(0.002)	(0.003)

Standard errors are in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

systematically by controlling for a set of potential confounding factors in the econometric model.

3.3.2 Model

In order to estimate the causal relationship between release of radon zones and the house price, variations in the timing of map release and the radon level across counties are exploited in a difference-in-difference estimator.

$$\begin{aligned} Hprice_{jt} = \beta_0 + \beta_1 Post_{jt} + \beta_2 Treatment_{ijt} \\ + \beta_3(Post_{jt} * Treatment_{jt}) + \beta_4 X_{jt} + M_t + C_j + \epsilon_{jt} \end{aligned} \tag{3.1}$$

where j indicates individual county and t is the year of observation. *Hprice* is an average median house price, which is adjusted to 2000 price level. I use level of the house price, rather than log of the house price, because the it is closer to a normal distribution. *Post* is an indicator for whether counties are observed after 1993, *Treatment* is an indicator for whether counties are assigned to zone 1 or 2, X is a vector of covariates, M is a vector of yearly fixed effect, C is a vector of county fixed effect and ϵ is error term of model. Standard errors are clustered by county. In equation (1), the value of β_3 represents the average treatment effect (ATE) of the information in the radon map on house price.

I assume that households were not endogenously sorted into high level counties before the map is released. Even though individuals may already have information about the level of radon in their residence or community through the individual test, it seems unlikely that they obtain the information on cut off radon level for each zone as well as the level of near counties before the map is released [6]. Thus, it seems reasonable to assume that the house price is not adjusted by level of radon gas before 1993.

[6] Although EPA and USGS released the map entitled "Areas with Potentially High Radon Levels" in 1987, this map was based on limited geologic information only.

Table 3.2: Effect of radon zones map on house price

	(1) House Price	(2) House Price	(3) House Price	(4) House Price
Panel A (1991-1995)				
After 1993	-27.29***	-47.03***	-16.29	-178.8
	(4.487)	(6.161)	(25.82)	(147.9)
High level counties	29.22***	30.09***		
	(8.368)	(8.370)		
After 1993*High level counties	-2.909	-2.003	-2.885	1.676
	(4.873)	(4.736)	(5.008)	(6.138)
N	146	146	146	146
R^2	0.939	0.952	0.989	0.996
Panel B (Excluding 1993)				
After 1993	-36.39***	-46.07***	-14.48	-266.1
	(4.977)	(6.518)	(29.09)	(214.9)
High level counties	31.62***	30.85***		
	(8.344)	(8.255)		
After 1993*High level counties	-2.236	-2.718	-2.214	4.952
	(5.896)	(5.945)	(6.735)	(7.138)
N	117	117	117	117
R^2	0.948	0.952	0.988	0.997
Covariates	Controlled	Controlled	Controlled	Controlled
Year FE	No	Yes	Yes	Yes
County FE	No	No	Yes	Yes
County Time Trend	No	No	No	Yes

Note: Standard errors in parentheses. Standard errors are clustered by counties.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.4 Result

Table 2 shows the results of estimation of equation (1). Panel A shows the results when we use the full sample, and panel B represents the results when sample from 1993 is excluded. The reason I exclude sample from 1993 is that, because the map is released at September 1993, including 1993 data introduces noise in estimation. In each column, the *Post* indicator, the *Treatment* indicator, and the interaction term of these two variables in equation (1) are represented.

In panel A, the coefficient of interaction term is -2.909 without significance in column (1), where covariates are controlled, but year and county fixed effect are omitted. Since I do not take unobserved heterogeneity into account over different year periods and counties, estimate may be biased. Once I control for these factors, as shown in column (3), the coefficient is still not significant and equals -2.885. In column (4), where I additionally include county specific time trend to control for linear trend in the error term which might be correlated to house price during sample periods in the model, the coefficient shows positive values as 1.676. Compared with the results in panel A, the negative effect of the interaction term in panel B is smaller overall, but still none of coefficients are significant.

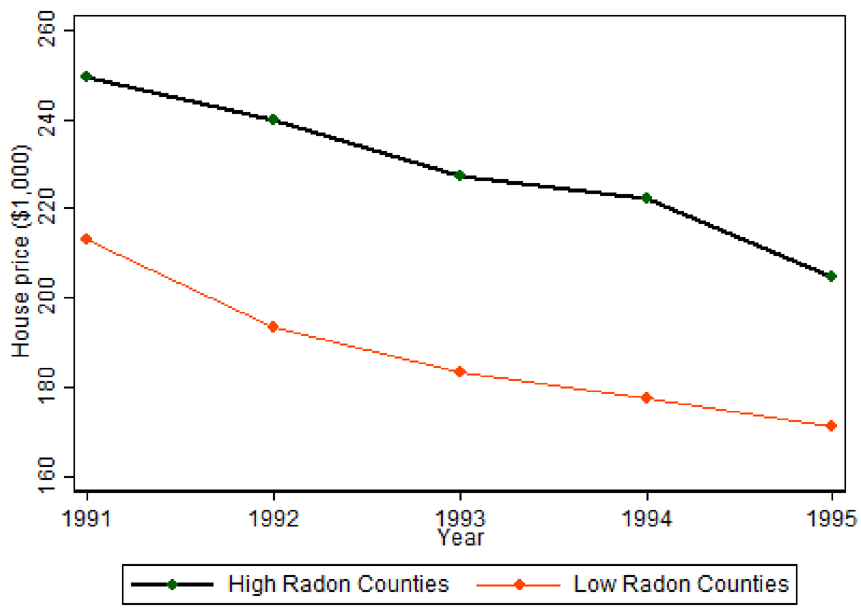


Figure 3.1: Average median house price trend

Table 3.3: Difference-in-difference estimates of Radon zones map on average median house price: zone 2 and zone 3

	(1) House Price	(2) House Price	(3) House Price	(4) House Price
Panel A (1991-1995)				
After 1993	-27.46***	-45.85***	-23.03	-172.6
	(4.543)	(6.299)	(24.92)	(148.1)
High level counties	30.11***	30.91***		
	(8.473)	(8.554)		
After 1993*High level counties	-2.602	-1.821	-2.301	0.467
	(5.334)	(5.216)	(5.335)	(6.907)
N	136	136	136	136
R^2	0.940	0.951	0.989	0.996
Panel B (Excluding 1993)				
After 1993	-36.48***	-44.70***	-22.15	-237.3
	(5.037)	(6.629)	(27.78)	(207.2)
High level counties	32.69***	32.04***		
	(8.445)	(8.430)		
After 1993*High level counties	-1.950	-2.437	-1.182	3.862
	(6.443)	(6.546)	(7.061)	(7.693)
N	109	109	109	109
R^2	0.948	0.951	0.988	0.997
Covariates	Controlled	Controlled	Controlled	Controlled
Year FE	No	Yes	Yes	Yes
County FE	No	No	Yes	Yes
County Time Trend	No	No	No	Yes

Note: Standard errors in parentheses. Standard errors are clustered by counties.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.4: Difference-in-difference estimates of Radon zones map on average median house price: zone 1 and zone 3

	(1) House Price	(2) House Price	(3) House Price	(4) House Price
Panel A (1991-1995)				
After 1993	-24.38***	-45.23***	-6.031	97.66
	(4.734)	(6.759)	(17.59)	(101.7)
High level counties	21.32	22.26		
	(17.36)	(17.83)		
After 1993*High level counties	-11.35	-7.223	-7.960	13.17
	(10.59)	(9.727)	(5.882)	(11.23)
N	72	72	72	72
R^2	0.966	0.976	0.998	0.999
Panel B (Excluding 1993)				
After 1993	-32.14***	-44.79***	-17.44	77.51
	(5.330)	(6.373)	(13.29)	(197.3)
High level counties	20.82	21.12		
	(18.09)	(18.73)		
After 1993*High level counties	-12.42	-11.16	-20.20***	23.91
	(9.814)	(10.08)	(4.281)	(18.93)
N	58	58	58	58
R^2	0.971	0.976	0.999	1.000
Covariates	Controlled	Controlled	Controlled	Controlled
Year FE	No	Yes	Yes	Yes
County FE	No	No	Yes	Yes
County Time Trend	No	No	No	Yes

Note: Standard errors in parentheses. Standard errors are clustered by counties.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Remember that the high level counties in Table 2 include both zone 1 and zone 2. In order to examine the difference in house price between each zone, first I include only zone 2 counties in the treatment group. As the results show in Table 3, there are no significant effects in either panel A or panel B. When I include only zone 1 counties in the treatment group and estimate the result, the coefficient of interaction term in Table 4 becomes much lager than Table 3. In particular, the coefficient in panel B is significant at 1% level for column (3) indicating that average of median house price in zone 1 counties is about $20,000 lower than zone 3 counties after the map is released. However, there are no significant effects for other specifications, and the small sample size in the treatment group makes statistical inference limited. In sum, the results in Table 2- Table 4 show that the map has no effect on the average of the median house price between high radon level counties and low radon level counties.[7]

There are two possible explanations for these results. First explanation is that people may not understand information on the map. As mentioned earlier, the main purpose of this map is to assist national, state and local organizations to implement radon resistant building codes, not to inform the public. If most individuals did not recognize the existence

[7] The results without Los Angeles county show smaller magnitude of coefficient, but they are similar with results in Table 2 in terms of sign and significance. See Table A1 in appendix.

of the new information, release of the map could not have any impact on their behavior. Second explanation is that information in the map may be not enough to induce behavioral response. Even though I allow the possibility that many people could be exposed to the map through local media or other sources, as Madajewicz et al. (2007) mentioned, without well-specified information, individuals do not change their behavior. In this aspect, the map which simply divides regions into three zones without further information such as health effect might have limited impact on change in behavior.

3.5 Robustness checks

In order to check sensitivity of the results to a change in the specifications of the model, I i) expand the time window in our analysis and ii) restrict the sample to the adjacent counties of different zones.

One characteristic of housing markets is that transaction costs are high and there are investment incentives in buying and selling. These properties may cause the effect to appear after several years rather than immediately. In order to check this possibility, I include the years of 1996-1998 after the release. The results are shown in Table 5. Even though coefficients on the interaction term show negative for column (4) where county time trend is controlled, there are still no significant results in either panel A or panel B.

It is possible that residents do not change their residence dramatically but tend to move into their neighbouring counties. In this scenario, I might observe the change in house price more clear when sample is restricted to adjacent counties of different zones. To check this possibility, I exclude the counties that are surrounded by same level zone. For example, in Figure 2, I exclude Mendocino county, which is surrounded by zone 3 counties. From this process, a total of ten counties (Humboldt, Lake, Marin, Mendocino, Napa, San Francisco, San Mateo, Santa Cruz, Siskiyou, and Sonoma) are excluded from the sample. Table 6 shows the results. Compared with the results in Table 2, the magnitude of coefficients becomes larger overall in Table 6, but the effects are still not significant.

Table 3.5: Robustness checks: Including 1996-1998

	(1) House Price	(2) House Price	(3) House Price	(4) House Price
Panel A (1991-1995)				
After 1993	-30.40***	-30.40***	-74.12***	18.11
	(5.471)	(5.471)	(22.56)	(65.28)
High level counties	29.56***	29.56***		
	(8.184)	(8.184)		
After 1993*High level counties	0.853	0.853	-2.097	-0.856
	(5.599)	(5.599)	(5.219)	(4.835)
N	234	234	234	234
R^2	0.936	0.936	0.983	0.993
Panel B (Excluding 1993)				
After 1993	-37.26***	-46.67***	-72.87***	41.76
	(6.190)	(10.16)	(22.39)	(70.48)
High level counties	31.45***	30.17***		
	(8.187)	(8.529)		
After 1993*High level counties	-0.419	-0.447	-3.639	-0.297
	(6.320)	(6.392)	(6.762)	(7.888)
N	205	205	205	205
R^2	0.940	0.944	0.983	0.993
Covariates	Controlled	Controlled	Controlled	Controlled
Year FE	No	Yes	Yes	Yes
County FE	No	No	Yes	Yes
County Time Trend	No	No	No	Yes

Note: Standard errors in parentheses. Standard errors are clustered by counties.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.6: Robustness checks: Neighbour counties that are assigned to different zone

	(1) House Price	(2) House Price	(3) House Price	(4) House Price
Panel A (1991-1995)				
After 1993	-15.48***	-34.36***	-36.11	-75.21
	(5.291)	(6.062)	(35.56)	(54.80)
High level counties	19.16***	19.17***		
	(5.458)	(5.030)		
After 1993*High level counties	-3.799	-3.605	-2.965	-1.114
	(4.402)	(3.974)	(4.856)	(3.576)
N	106	106	106	106
R^2	0.952	0.970	0.992	0.999
Panel B (Excluding 1993)				
After 1993	-25.25***	-34.10***	-35.35	-95.26
	(5.423)	(6.394)	(42.53)	(176.4)
High level counties	20.16***	19.37***		
	(5.925)	(5.438)		
After 1993*High level counties	-1.861	-4.195	-2.055	3.853
	(5.439)	(5.380)	(6.998)	(5.547)
N	85	85	85	85
R^2	0.965	0.971	0.991	0.999
Covariates	Controlled	Controlled	Controlled	Controlled
Year FE	No	Yes	Yes	Yes
County FE	No	No	Yes	Yes
County Time Trend	No	No	No	Yes

Note: Standard errors in parentheses. Standard errors are clustered by counties.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

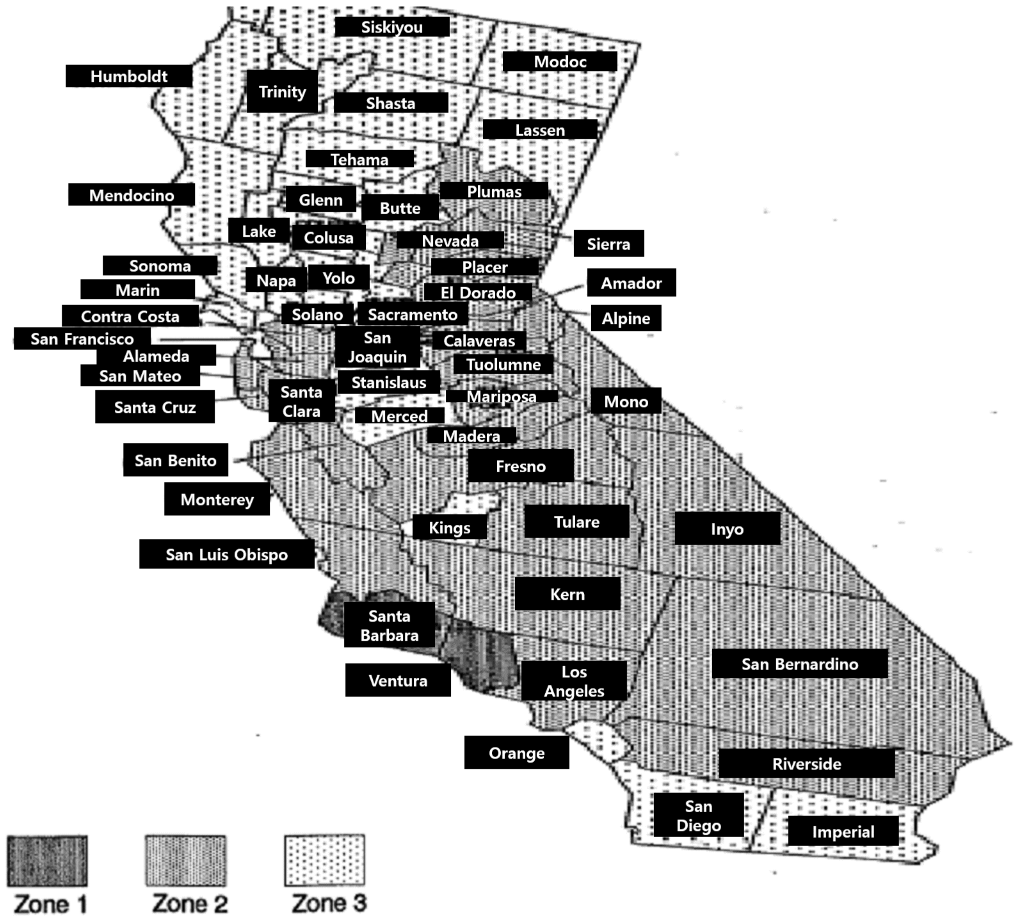


Figure 3.2: California Map of Radon Zones

3.6 Conclusion

This paper shows that EPA's map of radon zones did not have an impact on the house price in California counties. There is no significant change in average of median house price between high level radon counties (zone 1 and zone 2) and low level radon counties (zone 3) after the map is released. The results are robust even when I compare very high level counties to low level radon counties, expand sample years, and restrict the sample to adjacent counties of different zones. These results support the idea that individuals change their behavior when the information is well-specified as well as is widely available to the public.

There are caveats I do not address. First, the results are valid under the assumption that there are no other exogenous events affecting house price except the map release between 1991 and 1995. There may be a spurious relationship in the results unless other possibilities are fully ruled out. Moreover, even though EPA's map of radon zones is available for all states in U.S., because publicly available house price data starting from early 1990's are only able to be obtained for California, the geographic area is restricted to this area. I need to extend the geographic area to other states when relevant data are available.

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Appendices

A Appendix A

Table A1: The effect of the earthquake on birth outcomes by race of mothers

	(1) LBW	(2) Very LBW	(3) Mild LBW	(4) Prematurity	(5) Extreme Prematurity	(6) Mild Prematurity
Panel A (Full sample- White)						
1994*Highly Affected City	0.0018***	0.0006*	0.0012*	0.0009	0.0002	0.0007
	(0.0007)	(0.0004)	(0.0007)	(0.0010)	(0.0002)	(0.0010)
N	1,764,878	1,764,878	1,764,878	1,764,878	1,764,878	1,764,878
R^2	0.102	0.028	0.077	0.057	0.014	0.048
Panel B (Restricted sample- White)						
1994*Highly Affected City	0.0028	-0.0005	0.0033	0.0065**	-0.0013*	0.0078***
	(0.0023)	(0.0010)	(0.0022)	(0.0027)	(0.0007)	(0.0023)
N	242,725	242,725	242,725	242,725	242,725	242,725
R^2	0.015	0.011	0.007	0.021	0.009	0.016
Panel C (Full sample- Black)						
1994*Highly Affected City	0.0049*	0.0024	0.0026	-0.0066	-0.0011	-0.0055
	(0.0029)	(0.0015)	(0.0027)	(0.0042)	(0.0014)	(0.0038)
N	165,960	165,960	165,960	165,960	165,960	165,960
R^2	0.112	0.053	0.066	0.065	0.030	0.045
Panel D (Restricted sample- Black)						
1994*Highly Affected City	0.0213***	0.0027	0.0186***	-0.0063	-0.0044**	-0.0019
	(0.0056)	(0.0033)	(0.0059)	(0.0087)	(0.0020)	(0.0080)
N	34,719	34,719	34,719	34,719	34,719	34,719
R^2	0.031	0.030	0.015	0.031	0.023	0.020
Panel E (Full sample- Others)						
1994*Highly Affected City	0.0020**	0.0007**	0.0014*	0.0006	0.0000	0.0006
	(0.0008)	(0.0003)	(0.0007)	(0.0010)	(0.0002)	(0.0010)
N	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895
R^2	0.104	0.032	0.075	0.058	0.017	0.048
Panel F (Restricted sample- Others)						
1994*Highly Affected City	0.0054**	-0.0002	0.0056***	0.0036	-0.0018***	0.0054**
	(0.0020)	(0.0010)	(0.0018)	(0.0026)	(0.0006)	(0.0024)
N	294,332	294,332	294,332	294,332	294,332	294,332
R^2	0.019	0.015	0.010	0.023	0.011	0.017

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Equation 1.1 results without imputed Orange County births

	(1) LBW	(2) Very LBW	(3) Mild LBW	(4) Prematurity	(5) Extreme Prematurity	(6) Mild Prematurity
Panel A (Full sample)						
1994*Highly Affected	0.0020**	0.001	0.0015**	0.0002	-0.0002	0.0004
	(0.0008)	(0.0003)	(0.0007)	(0.0009)	(0.0002)	(0.0010)
N	2,079,193	2,079,193	2,079,193	2,079,193	2,079,193	2,079,193
R^2	0.103	0.032	0.074	0.057	0.017	0.047
Panel B (Restricted sample)						
1994*Highly Affected	0.0054**	0.0001	0.0052**	0.0047**	-0.0019***	0.0066***
	(0.0021)	(0.0009)	(0.0020)	(0.0021)	(0.0006)	(0.0019)
N	286,294	286,294	286,294	286,294	286,294	286,294
R^2	0.019	0.014	0.010	0.023	0.011	0.017

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Equation 1.1 results without imputed Orange County births

	(1)	(2)	(3)	(4)	(5)	(6)
	LBW	Very LBW	Mild LBW	Prematurity	Extreme Prematurity	Mild Prematurity
Panel A (Full sample)						
1994*Highly Affected	0.0020**	0.0006	0.0015**	0.0002	-0.0002	0.0004
	(0.0008)	(0.0003)	(0.0007)	(0.0009)	(0.0002)	(0.0010)
N	2,079,193	2,079,193	2,079,193	2,079,193	2,079,193	2,079,193
R^2	0.103	0.032	0.074	0.057	0.017	0.047
Panel B (Restricted sample)						
1994*Highly Affected	0.0054**	0.0001	0.0052**	0.0047**	-0.0019***	0.0066***
	(0.0021)	(0.0009)	(0.0020)	(0.0021)	(0.0006)	(0.0019)
N	286,294	286,294	286,294	286,294	286,294	286,294
R^2	0.019	0.014	0.010	0.023	0.011	0.017

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Equation 1.1 results excluding births with imputed cities of residence

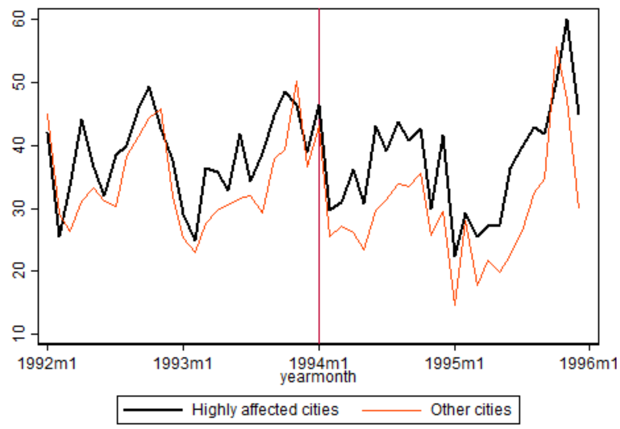
	(1)	(2)	(3)	(4)	(5)	(6)
	LBW	Very LBW	Mild LBW	Prematurity	Extreme Prematurity	Mild Prematurity
Panel A (Full sample)						
1994*Highly affected	0.0017	-0.0002	0.0018**	0.0015	-0.0007**	0.0022
	(0.0010)	(0.0005)	(0.0008)	(0.0015)	(0.0003)	(0.0017)
N	1,009,903	1,009,903	1,009,903	1,009,903	1,009,903	1,009,903
R^2	0.103	0.033	0.074	0.057	0.017	0.046
Panel B (Restricted sample)						
1994*Highly affected	0.0071**	0.0004	0.0067***	0.0077***	-0.0021*	0.0098***
	(0.0026)	(0.0011)	(0.0021)	(0.0024)	(0.0011)	(0.0019)
N	148,554	148,554	148,554	148,554	148,554	148,554
R^2	0.021	0.016	0.011	0.023	0.013	0.017

NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

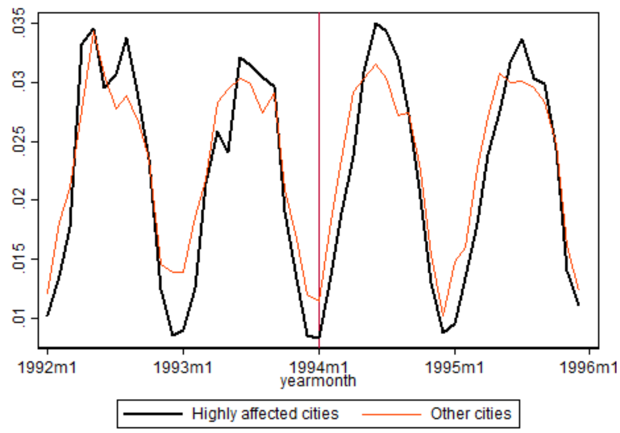
Table A5: Equation 1.1 results with hospital birth included as an additional control

	(1)	(2)	(3)	(4)	(5)	(6)
	LBW	Very LBW	Mild LBW	Prematurity	Extreme Prematurity	Mild Prematurity
Panel A (Full sample)						
1994*Highly affected	0.0020**	0.0007**	0.0014*	0.0006	0.0000	0.0007
	(0.0008)	(0.0003)	(0.0007)	(0.0010)	(0.0002)	(0.0010)
N	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895	2,167,895
R^2	0.104	0.032	0.075	0.058	0.017	0.048
Panel B (Restricted sample)						
1994*Highly affected	0.0053**	-0.0002	0.0055***	0.0036	-0.0019***	0.0054**
	(0.0020)	(0.0010)	(0.0018)	(0.0026)	(0.0006)	(0.0023)
N	294,332	294,332	294,332	294,332	294,332	294,332
R^2	0.019	0.015	0.010	0.023	0.011	0.017

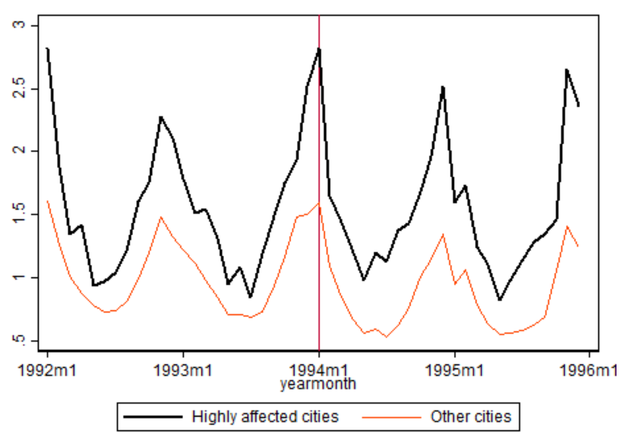
NOTE: Standard errors in parentheses. Standard errors are clustered by county of residence.
"Very LBW" refers to infants born under 1,500 grams. "Mild LBW" refers to infants 1,500 - 2,499 grams.
"Extreme prematurity" describes gestation under 28 weeks. "Mild prematurity" is gestation at 28 - 36 weeks.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.



(a) Particulate matter less than 10 microns in diameter (PM10)



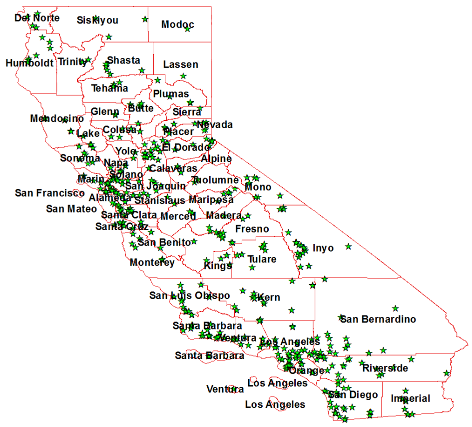
(b) Ozone



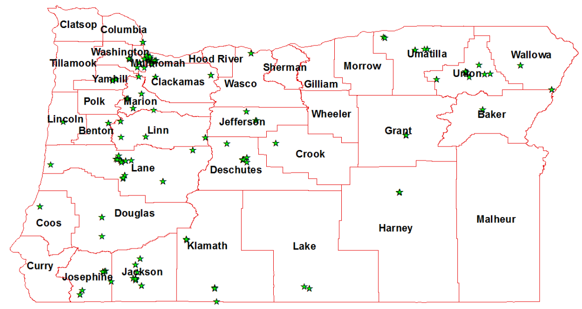
(c) Carbon Monoxide

Figure A1: California pollutant trends by month

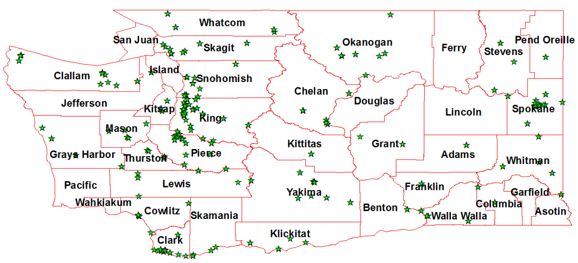
B Appendix B



California



Oregon



Washington

Figure B1: Distribution of monitoring stations

Table B1: Lists of counties and number of panel years

State	County
California	Alameda (16), Amador(16), Butte(16), Calaveras(16), Colusa(16), Contra(16), Del Norte(16), El Dorado(16), Fresno(16), Glenn(16), Humboldt(16), Imperial(16), Inyo(16), Kern(16), Kings(16), Lake(16), Lassen(3), Los Angeles(16), Madera(16), Marin(16), Mariposa(16), Mendocino(16), Merced(16), Modoc(5), Mono(16), Monterey(16), Napa(16), Nevada(16), Orange(16), Placer(16), Plumas(16), Riverside(16), Sacramento(16), San Benito(16), San Bernardino(16), San Diego(16), San Francisco(16), San Joaquin(16), San Luis Obispo(16), San Mateo(16), Santa Barbara(16), Santa Clara(16), Santa Cruz(16), Shasta(16), Sierra(2), Siskiyou(16), Solano(16), Sonoma(16), Stanislaus(16), Sutter(16), Tehama(16), Trinity(16), Tulare(16), Tuolumne(16), Ventura(16), Yolo(16)
Oregon	Baker(10), Benton(15), Clackamas(16), Columbia(16), Coos(3), Crook(7), Deschutes(16), Douglas(10), Grant(10), Harney(15), Jackson(16), Jefferson(7), Josephine(16), Klamath(16), Lake(16), Lane(16), Lincoln(2), Linn(16), Marion(16), Multnomah(16), Umatilla(16), Union(16), Wallowa(15), Wasco(16), Washington(16), Yamhill(3)
Washington	Adams(15), Asotin(16), Benton(16), Chelan(16), Clallam(16), Clark(16), Columbia(12), Cowlitz(16), Franklin(12), Grant(12), Grays Harbor(13), Jefferson(15), King(16), Kitsap(13), Kittitas(16), Klickitat(16), Lewis(16), Mason(14), Okanogan(14), Pend Oreille(6), Pierce(16), Skagit(16), Skamania(4), Snohomish(16), Spokane(16), Stevens(16), Thurston(16), Walla Walla(16), Whatcom(16), Whitman(15), Yakima(16)

Note: Parenthesis indicates number of panel years.

Table B2: List of variables

	Variable
Dependent variable	Percentage change in population
Explanatory variable	Changed number of each type of unhealthy days
Covariates	Unemployment rate
	Poverty rate (all ages)
	Percentage of manufacturing firms
	Number of establishment
	Number of employment
	Median price of house per square feet
	Median household income
	Number of building permits
	Number of primary and secondary school
	Pupil to teacher ratio of primary and secondary
	Mean and square of mean temperature
	Mean and square of mean precipitation
	Population density

Table B3: Summary Statistics

Variable	CA	OR	WA	Total
Total population change (%)	0.937	0.655	1.183	0.947
	(1.054)	(1.057)	(1.128)	(1.088)
Age 0-9 and 25-39 population change (%)	0.08	0.31	0.823	0.317
	(1.566)	(1.969)	(1.75)	(1.725)
Age 10-19 and 40-59 population change (%)	0.647	-0.473	0.243	0.328
	(1.929)	(1.798)	(1.744)	(1.905)
Age 20-24 population change (%)	1.988	0.883	1.514	1.654
	(2.819)	(3.019)	(2.987)	(2.931)
Age 60 and older population change (%)	2.766	3.126	3.497	3.025
	(1.374)	(1.365)	(1.348)	(1.4)
"Good" days (%)	225.876	288.638	299.135	256.212
	(91.75)	(34.866)	(42.78)	(81.108)
"Moderate" days (%)	103.961	66.887	58.07	85.373
	(61.215)	(30.44)	(41.32)	(56.113)
"Unhealthy for Sensitive Groups" days (%)	27.221	4.444	2.482	16.684
	(32.465)	(6.114)	(4.12)	(27.239)
"Unhealthy" days (%)	6.198	0.903	0.372	3.729
	(12.95)	(2.407)	(1.211)	(10.148)
"Very Unhealthy" days (%)	0.885	0.058	0.025	0.513
	(3.328)	(0.476)	(0.174)	(2.533)
Median AQI (%)	47.688	28.315	28.223	39.129
	(16.86)	(7.401)	(8.569)	(16.758)
Unemployment rate	8.738	8.785	7.807	8.509
	(4.05)	(2.815)	(2.179)	(3.465)
Poverty rate (all age)	14.14	15.846	14.868	14.642
	(4.92)	(3.12)	(4.305)	(4.525)
Manufacturing firms (%)	4.522	4.829	4.424	4.554
.	(1.533)	(1.272)	(1.301)	(1.436)
Median price of house per square feet ($)	213.339	118.018	128.447	174.006
	(118.623)	(32.748)	(32.337)	(101.533)
Median household income ($)	54,565.865	43,904.733	48,998.226	51,170.312
	(13,510.792)	(7,015.584)	(8,654.763)	(12,152.035)
Number of building unit permits	2,371.135	762.409	1,487.851	1,847.731
	(4,266.534)	(1,140.355)	(2,557.416)	(3,534.06)
Number of employment	265,297.108	63,648.148	98,978.344	185,479.294
	(591,404.118)	(99,954.19)	(213,248.494)	(466,059.629)
Number of establishment	17,294.09	4,702.883	7,057.256	12,348.074
	(37,206.205)	(6,257.803)	(12,932.055)	(29,248.661)
Number of primary and secondary school	7.852	7.009	7.574	7.625
	(6.646)	(5.396)	(5.449)	(6.143)
Pupil to teacher ratio of primary and secondary	20.559	19.277	18.808	19.875
	(2.069)	(3.236)	(1.306)	(2.319)
Mean temperature (F°)	59.344	49.673	50.107	55.194
	(5.146)	(3.337)	(2.397)	(6.346)
Mean precipitation (inches)	21.884	30.575	38.732	27.798
	(13.609)	(19.485)	(26.353)	(20.057)
Population density (person/square miles)	769.02	179.668	174.257	507.966
	(2,454.631)	(406.024)	(237.135)	(1,870.801)
N	777	257	355	1,389

Table B4: Results from single estimation equation

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Panel A					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 100)	-0.0005	-0.0010	-0.0012	0.0054	-0.0002
	(0.0021)	(0.0028)	(0.0017)	(0.0052)	(0.0013)
Panel B					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.0046**	-0.0058**	-0.0025	-0.0085	-0.0045***
	(0.0018)	(0.0026)	(0.0017)	(0.0054)	(0.0015)
N	1310	1310	1310	1310	1310
R^2	0.824	0.809	0.937	0.720	0.909

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: Baseline results by each age group

	(1) Age0-9	(2) Age10-19	(3) Age20-24	(4) Age25-39	(5) Age40-59	(6) Age60-older
Panel A						
Change in "Unhealthy for Sensitive Groups"						
(Change in number of days with AQI over 100)	0.0030	-0.0001	0.0041	-0.0054*	-0.0028	-0.0009
	(0.0035)	(0.0021)	(0.0054)	(0.0030)	(0.0017)	(0.0013)
N	1310	1310	1310	1310	1310	1310
R^2	0.805	0.903	0.732	0.785	0.937	0.908
Panel B						
Change in "Unhealthy"						
(Change in number of days with AQI over 150)	-0.0095***	-0.0040	-0.0071	-0.0031	-0.0024	-0.0045***
	(0.0029)	(0.0031)	(0.0053)	(0.0039)	(0.0016)	(0.0015)
N	1310	1310	1310	1310	1310	1310
R^2	0.806	0.903	0.733	0.784	0.937	0.909

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B6: Results without Los Angeles county and Riverside county

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Panel A: Without LA county					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 100)	-0.00458**	-0.00631**	-0.00389**	-0.00374	-0.00187
	(0.00185)	(0.00259)	(0.00177)	(0.00413)	(0.00144)
N	1295	1295	1295	1295	1295
R^2	0.823	0.794	0.934	0.706	0.896
Panel B: Without LA county					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.00708***	-0.0110***	-0.00538**	-0.00142	-0.00360
	(0.00263)	(0.00404)	(0.00228)	(0.00813)	(0.00248)
N	1295	1295	1295	1295	1295
R^2	0.822	0.794	0.933	0.706	0.896
Panel C: Without Riverside county					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 150)	-0.000691	-0.00124	-0.00117	0.00461	-0.000457
	(0.00183)	(0.00262)	(0.00140)	(0.00513)	(0.00125)
N	1295	1295	1295	1295	1295
R^2	0.797	0.809	0.932	0.695	0.914
Panel D: Without Riverside county					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.00398**	-0.00528*	-0.00218	-0.00657	-0.00414***
	(0.00178)	(0.00273)	(0.00161)	(0.00555)	(0.00143)
N	1295	1295	1295	1295	1295
R^2	0.798	0.809	0.932	0.695	0.914

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B7: Results controlling for 90^{th} percentile AQI or maxiumum AQI

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Panel A: 90^{th} percentile AQI					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 100)	-0.00128	-0.00152	-0.00213	0.00338	-0.000354
	(0.00250)	(0.00316)	(0.00206)	(0.00585)	(0.00134)
N	1310	1310	1310	1310	1310
R^2	0.823	0.809	0.937	0.719	0.909
Panel B: 90^{th} percentile AQI					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.00616**	-0.00541	-0.00610**	-0.0126*	-0.00331*
	(0.00253)	(0.00414)	(0.00240)	(0.00733)	(0.00196)
N	1310	1310	1310	1310	1310
R^2	0.825	0.809	0.937	0.720	0.909
Panel C: Maximum AQI					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 150)	-0.000941	-0.00161	-0.00135	0.00463	-0.000540
	(0.00218)	(0.00299)	(0.00169)	(0.00518)	(0.00136)
N	1486	1486	1486	1486	1486
R^2	0.819	0.802	0.935	0.717	0.902
Panel D: Maximum AQI					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.00437**	-0.00541**	-0.00279	-0.00587	-0.00469***
	(0.00177)	(0.00271)	(0.00176)	(0.00529)	(0.00153)
N	1486	1486	1486	1486	1486
R^2	0.820	0.803	0.935	0.717	0.903

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B8: Effect of air quality alerts on median household income

	(1) Median Household Income	(2) Median Household Income
Panel A		
Change in "Unhealthy for Sensitive Groups"	1.874	
	(4.855)	
Change in "Unhealthy"		-13.08*
		(7.618)
N	1310	1310
R^2	0.988	0.988

Note: Low, middle, and high income counties are defined as counties with median household income below 25th percentile, between 25th and 75th, and above 75th percentile in sample, respectively. Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: Effect of air quality alerts on population change by migration status using ACS 5 year estimates (2006-2010)

	(1) Total	(2) Age 0-9 and 25-39	(3) Age 10-19 and 40-59	(4) Age 20-24	(5) Age 60-older
Immigration					
Panel A					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 100)	0.0425	-0.0864	0.0569	0.0480	-0.0147
	(0.0836)	(0.212)	(0.150)	(0.205)	(0.0277)
N	107	107	107	107	107
R^2	0.743	0.769	0.755	0.773	0.849
Panel B					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.344***	-1.234***	-0.435**	-0.557	-0.179***
	(0.0897)	(0.211)	(0.186)	(0.339)	(0.0518)
N	107	107	107	107	107
R^2	0.768	0.809	0.764	0.783	0.864
Outmigration					
Panel A					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 100)	0.0193	0.0282	-0.0681	-0.0945	-0.0273
	(0.0445)	(0.133)	(0.0769)	(0.124)	(0.0246)
N	106	106	106	106	106
R^2	0.722	0.676	0.649	0.757	0.453
Panel B					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.174*	-0.485**	-0.491***	-0.170	-0.169***
	(0.0888)	(0.238)	(0.155)	(0.321)	(0.0639)
N	106	106	106	106	106
R^2	0.738	0.691	0.690	0.757	0.494
Immigration-Outmigration					
Panel A					
Change in "Unhealthy for Sensitive Groups"					
(Change in number of days with AQI over 100)	0.0223	-0.116	0.123	0.144	0.0234
	(0.0496)	(0.137)	(0.132)	(0.178)	(0.0233)
N	106	106	106	106	106
R^2	0.681	0.748	0.607	0.403	0.635
Panel B					
Change in "Unhealthy"					
(Change in number of days with AQI over 150)	-0.169**	-0.749***	0.0577	-0.388	0.0726
	(0.0831)	(0.218)	(0.146)	(0.432)	(0.0510)
N	106	106	106	106	106
R^2	0.700	0.774	0.602	0.410	0.639

Note: Standard errors are in parentheses. Standard errors are clustered by county. Number of population in each group is used as weight in each regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Appendix C

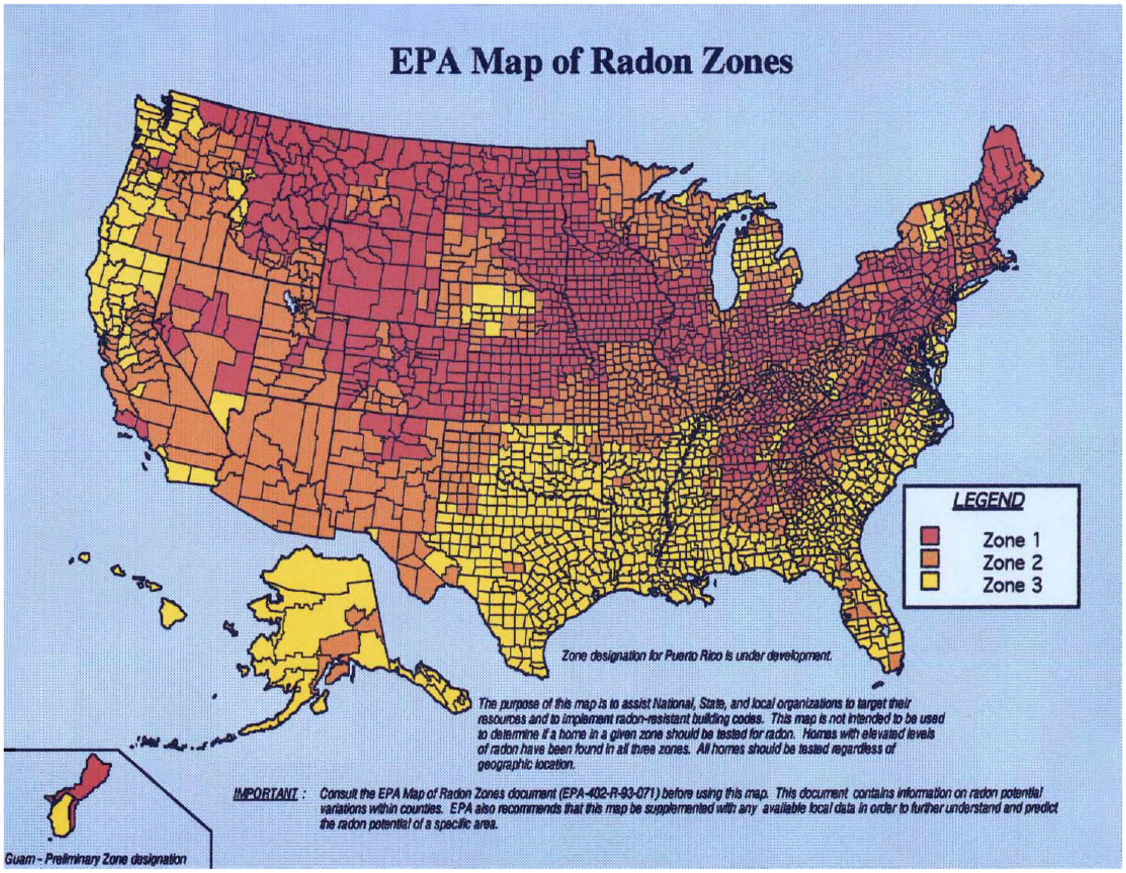


Figure C1: EPA Map of Radon Zones

Table C1: Difference-in-difference estimates of Radon zones map on average median house price: Without Los Angeles County

	(1) House Price	(2) House Price	(3) House Price	(4) House Price
Panel A (1991-1995)				
After 1993	-27.18***	-46.84***	-4.708	-181.6
	(4.601)	(6.352)	(25.21)	(154.6)
High level counties	28.77***	29.69***		
	(8.432)	(8.468)		
After 1993*High level counties	-1.998	-1.166	-1.874	1.671
	(5.042)	(4.890)	(5.002)	(6.336)
N	141	141	141	141
R^2	0.940	0.952	0.989	0.996
Panel B (Excluding 1993)				
After 1993	-36.26***	-46.05***	-0.282	-246.4
	(5.072)	(6.603)	(28.81)	(236.4)
High level counties	31.12***	30.31***		
	(8.420)	(8.353)		
After 1993*High level counties	-1.188	-1.582	-0.790	5.692
	(6.048)	(6.106)	(6.664)	(8.558)
N	113	113	113	113
R^2	0.949	0.952	0.989	0.997
Covariates	Controlled	Controlled	Controlled	Controlled
Year FE	No	Yes	Yes	Yes
County FE	No	No	Yes	Yes
County Time Trend	No	No	No	Yes

Note: Standard errors in parentheses. Standard errors are clustered by counties.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Vita

Bongkyun Kim was born in Incheon, Republic of Korea on May 6, 1981. He grew up in Gangseo-gu, Seoul, and graduated from Kwangyoung High School in February 2000. He attended Inha University in Incheon, where he received a Bachelor of Arts degree in Economics in February 2006 and Master of Arts degree in Economics in February 2008. He then studied at the University of Colorado Denver, and received a Master of Arts degree in Economics in May 2012. He moved to Knoxville, Tennessee the following July to study at the University of Tennessee. He received his Doctorate of Philosophy degree in Economics in May 2017.