ESSAYS ON HEALTH ECONOMICS

A Dissertation

Presented to

The Faculty of the Department

of Economics

University of Houston

In Partial Fulfillment

Of the Requirements for the Degree of

Doctor of Philosophy

By

Tzu-Yin Hazel Tseng

May 2016

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Abstract

The dissertation consists of two applied health economics studies on early childhood environment and the short- and long-term health outcomes. The first study uses detailed birth registries and health insurance claims records from Taiwan to examine the effects of exposure to adverse events while *in utero* on pregnancy outcomes. My coauthors and I study the impact of the 1999 Taiwan earthquake on fetal mortality and pregnancy outcomes. We compare the pregnancy outcomes of women who resided in areas with high earthquake intensity (i.e., higher on the Seismic scale) to those who resided in areas with low earthquake intensity, and compare pregnancies that were exposed to the earthquake to those pregnancies that were not exposed to the earthquake. Our results suggest that the incidence of fetal mortality increases by 4.4 and 3.2 percent for those who have *in utero* exposure to the earthquake in the most earthquake-affected regions during the first and second trimesters, respectively. We find that almost all of the losses that occur during first-trimester exposure are due to the loss of male fetuses.

The second study explores the relationship between early childhood environment and mental health later in life. I examine the impact on psychological well-being later in life of poor intrauterine environment caused by severe typhoons that took place in Taiwan. By exploiting time and regional variation, I compare the mental health of individuals who were exposed to severe typhoons while *in utero* in landfall counties to those who had no fetal exposure to severe typhoons. I find that the likelihood of mental disorders in adulthood resulting from fetal exposure to severe typhoons increased by 11%. Exposed individuals are also more likely to use psychiatric drugs and have more psychiatric-related healthcare utilization. The effects are most prominent for women. My results suggest that natural disasters could have adverse impacts beyond infant health and adult physical health.

Acknowledgements

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A special thanks to my family in Taiwan. I would like to express appreciation to my parents for their belief in me and allowing me to follow my dreams. I am truly blessed to have their love and support in my life. Last, but certainly not least, I would like to thank my beloved boyfriend Dr. Yen-Tien Lu for his companionship all these years. He has been my favorite non-economist audience of my research, and I always have his support in the moments when there was no one to answer my queries.

"Anything that happens, happens.

Anything that, in happening, causes something else to happen,

causes something else to happen.

Anything that, in happening,

causes itself to happen again, happens again.

It doesn't necessarily do it in chronological order, though."

—Douglas Adams, *Mostly Harmless*

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Chapter 1

Introduction

In Grossman's (1972) model, health capital depends on health stocks in the previous time period and investment in the current time period. The model implies that the effects of health shocks gradually dissipated, and early-life events are less influential on long-term health outcomes. Yet, a growing body of literature has documented the impacts of early childhood environment on short-term and long-term outcomes in the past two decades. Studies show supporting evidence on the persistent effects of health shocks during early childhood and the *in utero* period. The common finding is that adverse events during the early stage of life are associated with worse birth outcomes, lower education attainments and wages, and higher likelihood of chronic diseases. Building on the literature, this dissertation further examines whether *in utero* environment affects fetal losses and adult mental health.

The majority of the literature in this line relies on data from the surviving (fetuses) population, supposing fetal mortality increases as a result of worse *in utero* environment such that the unhealthiest fetuses are now less likely to survive. The existing estimates could underestimate the true effects due to survivor bias. To see how serious the selection

problem is, chapter 2 of this dissertation, titled "The Impact of a Natural Disaster on the Incidence of Fetal Losses and Pregnancy Outcomes," examines the extent to which fetal losses occur more frequently resulting from poor *in utero* environment. My coauthors and I study the impact of the 1999 Taiwan earthquake on fetal mortality, pregnancy complications, and outcomes including birth weight, gestational lengths, and sex ratio using detailed birth registries and health insurance claims records. My identification strategy is a difference-in-differences method. I compare the pregnancy outcomes of women who resided in areas with high earthquake intensity to those who resided in areas with low earthquake intensity, and compare pregnancies that were exposed to the earthquake to those pregnancies that were not exposed to the earthquake.

Using universal birth registries from 1998 to 2001 from Taiwan, we construct cohort size for each township and month of conception. After controlling for time-invariant regional effects and time effects that are common across regions, the changes in cohort size could reflect the size of fetal losses as a result of the earthquake. Our results suggest that the incidence of fetal mortality increases by 4.4 and 3.2 percent for those who have *in utero* exposure to the earthquake in the most earthquake-affected regions during the first and second trimesters, respectively. We find that almost all of the losses that occur during first-trimester exposure are driven by the loss of male fetuses. I also provide evidence of positive selection on health.

Although the literature has provided extensive evidence on the impacts on pregnancy outcomes, physical health, and human capital formation, the evidence of effects of *in utero* environment on adult mental health is relatively scant. The third chapter of this dissertation, titled "The Hidden Costs of Natural Disasters: *In Utero* Environment and Mental

Health in Adulthood," studies the impact of poor intrauterine environment on psychological well-being later in life caused by severe typhoons that took place in Taiwan. I use detailed health insurance claim records of a 5 percent population sample to identify mental disorders and calculate psychiatric-related healthcare utilization. The clinical measures from this dataset are less susceptible to misreporting compared to self-reported mental health as used in many other studies. In order to map individuals with their *in utero* exposure to severe typhoons, I constructed a typhoon data set which includes detailed information on every typhoon starting in 1958. Individuals were mapped with their *in utero* exposure to severe typhoons based on the year and month of their birth and the county of residence. To minimize potential bias of migration, I restrict the analysis to rural areas where migration occurs less frequently.

By exploiting time and regional variation, I compare the mental health of individuals who were exposed to severe typhoons while *in utero* in landfall counties to those who had no fetal exposure to severe typhoons. My analysis suggests that the likelihood of mental illness and use of psychiatric drugs increased by 11% for individuals who were exposed to severe typhoons while *in utero* relative to individuals who had no fetal exposure to severe typhoons. I also find that fetal exposure to a severe typhoon increases the number of psychiatric-related visits and healthcare expenditures. The negative effects on mental health are almost exclusively found among women. The results also suggest that the timing of exposure is extremely crucial, with *in utero* exposure having the greatest impacts on mental health relative to the effects of exposure in the first few years of life. My findings suggest that natural disasters could have long-term consequences on mental health in addition to massive economic disruption. With timely preventive services provided to affected pregnant women, social welfare may be improved.

Keywords: early childhood, health, fetal origins, natural disasters

JEL: I12, I19, I31, N35

Chapter 2

The Impact of a Natural Disaster on the Incidence of Fetal Losses and Pregnancy Outcomes (with Elaine M. Liu and Jin-Tan Liu)

2.1 Introduction

Studies have shown that *in utero* exposure to illness and adverse events is predictive of many negative outcomes such as shorter gestational length, low birth weight, higher infant mortality, lower education level, and higher likelihood of diabetes and cardiovascular disease (see Almond and Currie 2011; Currie and Vogl 2013 for an overview of recent literature). Other than the randomized control trial experiments done on animals, much of this line of literature exploits natural experiments (e.g., the 1918 flu pandemic, famine, earthquake, and extreme weather events) to compare the health or cognitive outcomes of groups that are affected to those who are unaffected by negative shocks. One important thing to note is that these existing findings are estimated with the surviving (fetus) population. If experiencing

adverse events *in utero* increases fetal mortality and if there is positive selection of fetuses, such that the weakest are culled, then previous estimates of the impact provide a lower bound estimate of the true impact, known as survivor bias (Bozzoli, Deaton, and Quintana-Domeque 2009). This challenge posed by positive mortality selection is widely recognized in both economics and epidemiology. The main objectives of this paper are to understand the extent to which fetal losses occur as a result of poor *in utero* environment and to examine whether there is evidence of positive selection. We will also discuss the causes of fetal losses. To the best of our knowledge, this is one of the first papers that attempts to estimate the increasing likelihood of fetal losses caused by a natural disaster and one of the handful of papers that provides evidence of positive selection (Bhalotra, Valente, and van Soest 2010; Bozzoli, Deaton, and Quintana-Domeque 2009; Gorgens, Meng, and Vaithianathan 2012).

The closer examination of the impact of adverse events on fetal losses is of high importance for researchers. First, recent papers in this literature have tried to address the issue of survivor bias using a bounding exercise, following Lee's procedure (Lee 2009)[1]. However, while the bounding exercise is appealing and informative, this method may not always be feasible since the size of culling is often unknown to authors. The finding from this paper, which aims to estimate the size of culling as a result of a natural disaster, may be useful as a benchmark for future authors who wish to conduct similar bounding exercises. Second, from the policymaker's perspective, the true cost of a negative shock could be underestimated if pregnancy losses are unaccounted for.

In this project, we study the impact of the 1999 Taiwan earthquake on fetal mortality, pregnancy complications, and outcomes including birth weight, gestational lengths, and

[1]Recent papers that use this exercise in the related literature include Bharadwaj, Lken, and Neilson (2013), Lin and Liu (2014), Halla and Zweimller (2014), Isen, Rossin-Slater, and Walker (2014)

sex ratio. This earthquake caused more than 2,400 deaths, and created aftershocks that lasted a month. We expect the earthquake to affect fertility outcomes and fetal mortality since recent papers have found that maternal stress caused by natural disasters is harmful to birth outcomes (e.g., Currie and Rossin-Slater 2013; Simeonova 2011; Torche 2011), but none of these papers examines the incidence of fetal losses. Our main identification strategy is a difference-in-differences method. We compare the pregnancy outcomes of women who resided in areas with high earthquake intensity (i.e. higher on the Seismic scale) to those who resided in areas with low earthquake intensity, and compare pregnancies that were exposed to the earthquake to those pregnancies that were not exposed to the earthquake.

While fetal mortality that includes miscarriages and stillbirths is extremely common, the difficult part of estimating pregnancy losses is the issue of underreporting. Most existing papers that examine fetal losses often rely on the reporting of miscarriages/stillbirths in birth registries (Black, Devereux, and Salvanes 2014; Laszlo et al. 2013; Persson and Rossin-Slater 2014). One caveat of using the record from birth registries is that most countries' administrative birth registries require reports of any outcomes of pregnancies of later gestation period.[2] Medical studies have found the incidence of fetal losses to be around 30% (Nepomnaschy et al. 2006; Wilcox et al. 1988, but most fetal losses occur during the first trimester, which is before 12 weeks of gestational length (Goldhaber and

[2]For example, all births taking place at 12 weeks of gestation or later are registered in the Norwegian birth registry used by Black, Devereux, and Salvanes (2014). All births taking place at 28 weeks of gestation or later in Sweden are recorded in the birth registry used by Laszlo et al. (2013).

Fireman 1991; Nepomnaschy et al. 2006)[3]. Early miscarriages will not be reported in administrative vital statistics. Other studies use household survey recall data reporting on miscarriages and fertility outcomes (Hernandez-Julian, Mansour, and Peters 2014). Recalling errors issue aside, according to the National Institutes of Health[4], which cites Michels and Tiu (2007), nearly half of fertilized eggs are aborted even before women realize that they are pregnant (Wang et al. 2003; Wilcox et al. 1988). The lack of knowledge of possible miscarriages could further dampen the issue of under-reporting of very early miscarriages. Our study tries to overcome this issue in the following ways.

We use birth registries from 1998 to 2001 from Taiwan to construct cohort size. Using gestational lengths (reported in weeks) and birth date allows us to infer the week of conception and whether a given child was exposed to the earthquake or not. We construct cohort size for each township and month of conception. After controlling for various fixed effects, the changes in cohort size could reflect the size of fetal losses as a result of the earthquake. Another advantage of the Taiwanese data is that the township was identified based on the mother's permanent residence, not based on the birth location; thus, the issue of migration (as a result of the earthquake) is less of a concern.

Furthermore, we utilize detailed health insurance claim records of a 5 percent population sample from 1998 to 2001. The claim record provides information such as reason for visiting the hospital/clinic and the location of the hospital. These health insurance records allow

[3]This fetal loss rate (which includes both miscarriages and stillbirths) is highly variable. The ACOG practice bulletin suggests that an estimated 15–20 percent of *known pregnancies* result in miscarriage, which happens within 20 weeks of gestation, and 80% of miscarriages happen during the first trimester But the rate of miscarriage can be even higher if one accounts for the *unknown pregnancies*. Early miscarriages that occur within the first couple weeks of conception have similar symptoms as the woman's delayed period, and thus are often unknown to women. One of the studies that has tried to overcome the issue of under-reporting is Wilcox et al. (1988). They tracked women's hormone levels every day to detect very early pregnancy losses and found a total fetal losses rate of 31 percent of all pregnancies.

[4]NIH. *Pregnancy loss*. Retrieved from http://m.nichd.nih.gov/topics/pregnancyloss/conditioninfo/Pages/risk.aspx

us to identify whether each pregnancy resulted in labor complications, or a normal delivery, as long as these events occur in a hospital or clinic.

Our analysis based on the birth registry data suggests that the incidence of fetal mortality increases by 4.4 and 3.2 percent for those who have *in utero* exposure to the earthquake in the most earthquake-affected regions during the first and second trimesters, respectively. We find that almost all of the losses that occur during first-trimester exposure are due to the loss of male fetuses. Using the health insurance claim data, we also observe a small increase the use of dilation and curettage (D&C) procedure, which is a procedure used for removing tissues after a miscarriage/abortion or treating abnormal bleeding within 9 months after the earthquake among those who reside in high intensity area.

We also find reductions in birth weight of 13 and15 grams for females with *in utero* exposure to the earthquake during the first and third trimesters, respectively. There is no difference in birth weight for males who are exposed during the first trimester compared to those with no exposure to the earthquake. The lack of results for male birth weight can be due to the positive (fetal) selection. Next, we apply Lee's bounding technique (Lee 2009) to our analysis, and it paints a very different picture. We find that male exposure to the earthquake during the first trimester would lower birth weight by 48 grams, which is significantly higher than the impact of exposure during the second and third trimesters and higher than the impact on females. This is extremely important since this exercise illustrates how positive selection may bias our findings. In sum, exposure to the earthquake during the first trimester resulted in higher fetal mortality; exposure during later trimesters resulted in a lighter birth weight.

Lastly, we discuss various channels of why this earthquake leads to an increase in miscarriages, and we provide some suggestive evidence that maternal stress caused by

the earthquake may be a key reason for the increase in miscarriages and poor pregnancy outcomes.

This chapter is organized as follows. Section 2 discusses Background and Data. Section 3 presents results from our empirical analysis. Section 4 discusses various channels of why the earthquake can lead to poor pregnancy outcomes. Section 5 concludes.

2.2 Background and Data

2.2.1 Background on the Earthquake Intensity

On September 21, 1999, the most destructive earthquake in the past few decades struck central Taiwan. 2,415 people were killed and 11,305 injured, with 51,711 buildings destroyed.[5] The earthquake had a magnitude of 7.3 on the Richter scale and was classified as a major earthquake. Townships experienced the earthquake at various levels of intensity. The intensity scale ranged from 3 in the least severe area to 7 in the most affected area (see Figure 2.1).[6] As indicated in Figure 2.2, the aftershocks from this earthquake persisted for a month. Figure 2.2 shows the distribution of the number and maximum Richter scale reading of detectable earthquakes before and after the earthquake.

[Insert Figure 2.1 about here]

[Insert Figure 2.2 about here]

[5] While Taiwan is in an earthquake zone, most of the earthquakes occur on the east coast and cause little damage. The 921 earthquake was one of the most catastrophic earthquakes since the epicenter was in the center of Taiwan, and the western part of the island, which is densely populated, was much more affected (Institute 1999).

[6] The Richter Scale reflects the magnitude of an earthquake. Richter scale 7.3 is measured at the epicenter. Our dataset contains Shindo intensity scale, which reflects the intensity of the earthquake that is felt by humans. Unlike the Richter scale, the Shindo Intensity scale is ordinal, and the intensity is inversely related to the distance to the epicenter.

2.2.2 Birth Registries

One of the main datasets we use in this study is the national birth registries from 1998 to 2001 in Taiwan. The records include birth weight, gestational age, gender, county of birth, multiple birth, birth order, parental education levels, age, township of permanent residence (hukou), and marital status. We focus on only singleton births in this study.[7]

Having gestational age is extremely helpful. Based on gestational length and one's birth date, we can infer the week that one was conceived and the timing of exposure to earthquake. For example, suppose an individual was conceived in July 1999 (two months before the earthquake) and was born in February 2000. If we use birth months to identify the timing of earthquake exposure, we would infer that the person was exposed to the earthquake during the second trimester, instead of the first trimester. Given that gestational lengths in the birth registry are reported in weeks, we could infer the week of conception. However, we worry about measurement-error issues in gestational length, thus, we use months of conception. We then collapse the data so that each unit of observation is at the conceiving-month-township level. We have birth records between January 1, 1998 and December 31, 2001, so conceiving year-months ranges from mid-1997 to the early part of 2001, with a total of 43 conceiving year-months. Table 2.1 shows the summary statistics at the cohort level. Townships with an intensity level of 6 have lower birth weights even prior to the earthquake. The time-invariant characteristics would be absorbed by township fixed effects in our analysis later.

[7] We also restrict the sample to births with birth weight between 500 g and 6,000 g and gestational length between 20 and 44 weeks. We find that mothers with missing age or education information tend to be of foreign origins. Based on a paper by Edlund, Liu, and Liu (2013) discussing foreign bride phenomenon in Taiwan, we would code those births from foreign mothers with missing age as aged between 25–34 and education level 9 years or below. If the birth is from domestic mothers, we replace the missing education level with 10–12 years of education and age between 25–34.

[Insert Table 2.1 about here]

2.2.3 Health Insurance Claim Records

The second main dataset we use in this study are the detailed claim records of a sample of 5 percent of the Taiwanese population from 1998 to 2001. After implementing universal health insurance in 1995, Taiwan's coverage rate reached 96% by 1997. The claim data records include outpatient and inpatient visits and drug prescriptions that were covered by government insurance during this period. We use International Classification of Diseases (ICD-9) and Diagnosis-Related Group (DRG) codes to identify the reasons for their visits. We construct a dataset of women between the ages of 16 and 45 in which each pregnancy contains the reason for their visit, and pregnancy outcome, with associated ICD-9 codes. Here pregnancy outcome is either normal delivery or delivery with complications (reported by physicians). Since health claim records do not contain residence information for individuals, we infer their pre-earthquake residences based on the township of their most frequently visited outpatient hospitals/clinics prior to the earthquake.[8]

Table 2.2 shows the summary statistics of insurance claim records. Earthquake intensity levels are based on the township of the most frequently visited hospital prior to the earthquake. Conditional on giving birth, about 17–19% of them experienced labor complications (Currie and Rossin-Slater (2013) reported a rate of 16% in their sample). Upon first examination, pregnant women in level 5+ areas seem to be healthier, with a lower rate of labor complications. One should note that the difference could also be due to

[8]While this dataset provides some information on miscarriages, we do not use it as an outcome for two reasons. First, as discussed in the introduction, most fetal losses occurred within a month of conception, and they are not known to mothers, so mothers might not visit hospitals/clinics for miscarriages. Second, as shown in Figure 2.5, immediately after the earthquake, there was a sharp drop in the number of outpatient visits. Those residing in high-intensity areas avoid visiting hospitals/clinics for less-urgent care as a result of the earthquake. For these two reasons combined, using post-earthquake miscarriages to proxy for fetal losses as a result of earthquake would underestimate the true impact of the earthquake.

the differences in norm, to patient access to hospitals, and to physicians' billing practices. As long as these differences in patient/physician behaviors remain time-invariant, it would not threaten our identification.

[Insert Table 2.2 about here]

2.3 Empirical Analysis

We exploit the variation in earthquake intensity and pregnancy timing and apply a difference-in-differences approach to investigate the effect of the earthquake. The first difference is between the outcomes of those residing in low-intensity regions and those residing in high-intensity regions. The second difference is based on the timing of pregnancy to determine whether there is *in utero* exposure to the earthquake or not. The key identifying assumption is that the pregnancy outcomes between high- and low-intensity areas would have followed similar trends had there not been an earthquake. In a later section, we will show some evidence of these assumptions.

Our analysis consists of two parts. First, we use birth registries to test whether cohort sizes are smaller and have worse outcomes for those who were exposed to the earthquake in high-intensity areas. Later, we use health insurance claim records to examine whether the likelihood of birth complications increases for those pregnancies that were exposed to the earthquake in high-intensity areas.

2.3.1 Fetal Losses and Birth Outcomes Using National Birth Registries

We first examine the differences in birth outcomes across birth cohorts. Each birth cohort is defined by the month of conception and the township of the mother's permanent residence. This definition has two important features. First, we use the month of conception rather than the month of birth since gestational lengths can be shortened as a result of a negative shock: using the month of birth would misidentify the timing of exposure to the earthquake (Currie and Rossin-Slater 2013) (for the method of imputing month of conception, see Section 3.2). Second, earthquake intensity was defined based on the township of the mother's permanent residence (hukou) instead of the location of the birth place. This reduces the issue of endogenous migration discussed in Currie and Rossin-Slater (2013).

For a given cohort conceived in year-month t in township w, our primary specification is as follows:[9]

[9] We have birth records between 1998 and 2001, so conceiving year-months t ranges from the early part of 1997 to the early part of 2001, with a total of 43 conceiving year-months (specifically September of 1997 to March of 2001).

$$
\begin{aligned}
Y_{wt} = & \beta_1 * I(Intensity \geq 6)_w * I(Conceivedin0 - 3MonthsBeforetheearthquake)_t + \\
& \beta_2 * I(Intensity \geq 6)_w * I(Conceivedin4 - 6MonthsBeforetheEarthquake)_t + \\
& \beta_3 * I(Intensity \geq 6)_w * I(Conceivedin7 - 9MonthsBeforetheEarthquake)_t + \\
& \alpha_1 * I(Intensity = 5)_w * I(Conceivedin0 - 3MonthsBeforetheEarthquake)_t + \\
& \alpha_2 * I(Intensity = 5)_w * I(Conceivedin4 - 6MonthsBeforetheEarthquake)_t + \\
& \alpha_3 * I(Intensity = 5)_w * I(Conceivedin7 - 9MonthsBeforetheEarthquake)_t + \\
& I(IntensityX)_w * I(Conceivedin1 - 3MonthsAftertheEarthquake)_t + \\
& I(IntensityX)_w * I(Conceivedin4 - 6MonthsAftertheEarthquake)_t + \\
& I(IntensityX)_w * I(Conceivedin7 - 9MonthsAftertheEarthquake)_t + \\
& I(IntensityX)_w * I(Conceivedin10 - 12MonthsAftertheEarthquake)_t + \\
& \delta_w + \mu_t + \epsilon
\end{aligned}
\tag{2.1}
$$

δ_w and μ_t capture the township and year-month fixed effects, respectively. β_1, β_2, β_3 capture the impact for exposure to the earthquake with intensity6 during the first, second, and third trimesters, respectively; α_1, α_2, α_3 capture the impact of the earthquake with intensity level 5 during the first , second, and third trimesters, respectively. Other coefficients capture the variation in outcomes for those conceived 1–3 months after the earthquake, 4–6 months after the earthquake, 7–9 months after earthquake, and 10–12 months after the earthquake. We include these post-earthquake dummies because there can be a negative selection issue for women who decide to conceive immediately after the earthquake. Outcomes of interest include natural log of cohort size, cohort male-to-female ratio, average gestational length, and birth weight. There are only 9 townships with an intensity level of 7, so we group townships with level 6 intensity and level 7 intensity

together. The reference groups in this regression are those in townships with the lowest intensity level, i.e. intensity levels of 4 or below, and those births that were not exposed to the earthquake. We decide not to use a continuous measure of earthquake intensity in our analysis, since the earthquake intensity scale we have is ordinal. Each observation may have a very different sample size, so we adjusted it by weighting the cell by township female population at the end of December 1998.[10]

The regression results for fetal losses and sex ratio are shown in Table 2.3. We find that the cohort sizes for those who were exposed to the earthquake *in utero* during the first trimester (conceived 0–3 months prior to the earthquake) in a high-intensity area (level 6+) are about 4.4% smaller relative to those who experienced the earthquake in the low-intensity areas (level 4 or below). Exposure to the earthquake (in level 6+) during the second trimester would cause a 3.2% drop in cohort size.[11] Impact on the third-trimester exposure, albeit negative, is not statistically different from zero. The size of fetal losses in the level 5 areas is smaller in magnitude compared to the level 6+ areas.

Recently, there have been some works suggesting that male fetuses can be more fragile than female fetuses under poor intrauterine conditions (Almond and Edlund 2007; Kraemer 2000). If the earthquake caused more loss of male fetuses, we would see a decrease in the male-to-female ratio for the affected cohorts. Table 2.3, Column 2 suggests that almost all the losses that occurred during the first trimester were caused by the loss of male fetuses. This finding of a skewed sex ratio as a result of poor *in utero* environment is also supported by several papers, including Hernandez-Julian, Mansour, and Peters (2014), Torche and

[10]In the regression with log cohort size/sex ratio as outcomes, we do not weight it by cohort size since cohort size is endogenous and is an outcome of interest. Hence, we use a pre-earthquake township female population as the weight. For other birth outcomes such as gestational lengths and birth weight, we use cohort size as weight since it will provide us with the average treatment effect for those who were born.

[11]Dinkleman (2013) finds drought exposure reduces cohort size by 2 percent. Laszlo et al. (2013) show that maternal bereavement increases the probability of stillbirth by 18 percent. Valente (2015) finds civil conflict increases the likelihood of miscarriage by 12 percent.

Kleinhaus (2012), Trivers and Willard (1973); Almond and Edlund (2007), Almond and Mazumder (2011), Mu and Zhang (2011), and Valente (2015).

[Insert Table 2.3 about here]

[Insert Table 2.4 about here]

Given that we find a difference in fetal mortality rates between genders, in Table 2.4, we will present birth outcomes separated by gender. Table 2.4 shows that birth weights are lower for both male and female infants. Females with first- and third-trimester exposure are about 16 g lighter (0.5% reduction), and gestational length is only marginally shorter (0.07 weeks) in high-intensity areas (relative to low-intensity areas). The impact on gestational lengths is extremely small. Interestingly, we find that males with first-trimester exposure to the earthquake seem to be relatively unaffected by the earthquake. It might also appear that females are more affected by the earthquake since the effects in the first trimester are statistically greater for females than for males. This can be due to positive selection. Since nearly 4% of male fetuses in the left tail of health distribution may be culled, it is not surprising that we do not find much effect. In Section 4.2, we will perform a bounding exercise based on Lee's procedure Lee 2009, which will allow us to compare the impact for males versus for females. We are also interested in exploring whether the fetal mortality rates can differ by the socioeconomic status of the mother or the age of the mother. In the birth registry data, we do not have household income information, thus, we examine the variation in outcomes by mother's education level. In these regressions, we do not find mothers with lower socioeconomic status to be any more likely to experience fetal losses.[12]

We perform robustness checks using alternative proxies for earthquake intensity. First, the Shindo intensity level we use has one decimal point. In our main specification, we round

[12]Regression results are available upon request.

it to the closest integer. As a robustness check, we round down the number to the integer, and the results are presented in Table 2.5, Column 2. In Column 3, we have measures of building collapsed rates in each township. We define highest intensity as those townships with 5% or more of the buildings collapsed, and 2nd highest intensity for those townships with greater than 0 to 5% of buildings collapsed. Across these alternative specifications, we can see that the magnitudes of fetal loss for first-trimester earthquake exposure in high-intensity areas are quite consistent.

[Insert Table 2.5 about here]

2.3.2 Fetal Losses Using Health Insurance Claim Records

Next, we use health insurance claim records to examine the effects on fetal losses. The health insurance claim records provide two ways to identify pregnancy losses, one is based on ICD-9 codes of miscarriages and stillbirths and the other relies on the DRG codes of D&C procedures. In the data set, we observe some women who had multiple OB/GYN visits in a short period of time, and all of the visits had ICD-9 codes of miscarriage. We suspect that these visits are likely due to one pregnancy loss, and the subsequent visits could be follow-up visits. Thus, we focus on D&C procedure in the analysis since the procedure can only be performed once after a miscarriage. In Section 4.1, we find that the number of outpatient visits dropped as a result of the earthquake. It is likely that individuals avoid going to hospitals for less urgent care after the earthquake. To avoid underestimating the incidence of D&C procedures in post-earthquake periods, we use the proportion of number of D&C procedure visits to number of OB/GYN visits as an outcome. For township w and year-month t, we estimate the following specification:

$$
\begin{aligned}
Y_{wt} = & \sum_{k=1998M2}^{k=2001M12} I(YearMonth = k)_t + \\
& \sum_{k=1998M2}^{k=2001M12} \beta_k * I(YearMonth = k)_t * I(Intensity \geq 6)_w + \\
& \delta_w + \epsilon_{wt}
\end{aligned} \tag{2.2}
$$

Y_{wt} represents the D&C rate that is calculated based on pre-earthquake residence. The coefficient of interest, β_k, shows the difference in D&C procedure rate between high intensity areas (intensity level 6+) and low intensity areas (intensity level 4) after controlling for time-invariant township effects and time effects that common across townships. Figure 2.3 shows the estimation results of β_k in Equation 2.2. We find that D&C rate increases by 0.2 to 0.6 percentage points in high intensity areas relative to low intensity areas after the earthquake. Recall that Table 2.3, Column 1 finds an effect of 4% fetal losses for those who had first-trimester exposure to the earthquake. Given that the majority of miscarriages are likely to happen during the first trimester (Nepomnaschy 2006), D&C procedure is most commonly performed between the 10th week and 20th week of pregnancy, and it would not be able to capture those very early miscarriages. Thus, it is not surprising that we do not see similar magnitude of effects here compared to Table 2.3. Although the universal health insurance claim records seem to provide some suggestive evidence on pregnancy loss, it is likely to be a lower bound of the true effects.

[Insert Figure 2.3 about here]

2.3.3 Labor Complication Using Health Insurance Claim Records

We examine the impact of the earthquake on incidences of pregnancy complications using health insurance claim records. The advantage of this dataset is that it provides us with the ICD-9 and DRG codes, which allow us to identify the reason for the woman's visit (labor delivery) and the physician's diagnostic codes for relevant pregnancy outcomes (e.g., labor/pregnancy complications).

Unlike the birth registry data, we do not know the gestational length, we only observe the timing of diagnosis. If a pregnant woman went to the hospital eight months after the earthquake for a delivery, we would not be able to know whether the pregnancy was conceived before or after the earthquake. Thus, in the following specification, unlike the birth registry data, we show results based on the timing of the diagnosis rather than the timing of the conception. Our main regression specification is listed as below, for a birth delivery i from mother who resides in township w at year-month t:

$$\begin{aligned} I(LaborComplication)_{iwt} = & \sum_{k=1998M2}^{k=2001M12} I(YearMonth = k)_t + \\ & \sum_{k=1998M2}^{k=2001M12} \beta_k * I(YearMonth = k)_t * I(Intensity \geq 5)_w + \\ & Age_{it} + \delta_w + \epsilon_{iwt} \end{aligned} \tag{2.3}$$

$I(LaborComplication)_{iwt}$ equals 1 if a physician reported a labor complication during delivery. The coefficients β_k capture the differences in labor complication in areas with an intensity of 5 and above relative to areas with an intensity of 4 or below and relative to the omitted month (1998M1). We decided to combine all areas with an intensity of 5 or

more as one group since there are only 4,752 observations in areas with intensity levels of 6 and 7.

[Insert Figure 2.4 about here]

The regression results of Equation 2.3 are presented in Figure 2.4. The shaded areas indicate those who probably had *in utero* exposure to the earthquake and its month-long aftershock. In the period leading up to the earthquake, we do not find any systematic differences between the high-intensity areas and the low-intensity areas, which provide evidence to the parallel trends assumption required by difference-in-differences analysis. In the period immediately after the earthquake (October–November, 1999), the likelihood of complications increases by 7.4–9.5 percentage points (approximately 30% increase) in areas with intensity of 5 and above relative to low-intensity areas, but this impact disappears after two months. Combining this result with the previous results on fetal losses suggests that exposure to the earthquake during the third trimester increases the likelihood of pregnancy/labor complications, whereas exposure during the first and second trimesters increases the likelihood of fetal losses.

2.3.4 Evidence of Positive Selection

In this section we examine whether there is evidence of positive selection. We will attempt to do so by examining subgroups that potentially experience greater treatment from the earthquake and compare the pregnancy outcomes of these groups. If we find that the ones that experience more fetal losses have better outcomes than those that do not, then it could be supporting evidence for positive selection.

First, we examine the likelihood of labor complications in level 6+ areas compared to

level 5 areas. We rewrite Equation 2.3 as below. For a birth delivery i from a mother who resides in township w at year-month t,

$$I(LaborComplication)_{iwt} = \pi_1 I(Intensity \geq 5)_w * I(< 3MonthsPostEarthquake)_t + \sum_{k=1998M2}^{k=2001M12} I(YearMonth = k)_t + Age_{it} + \delta_w + \epsilon_{iwt} \tag{2.4}$$

$$I(LaborComplication)_{iwt} = \pi_1 I(Intensity \geq 5)_w * I(< 3MonthsPostEarthquake)_t + \pi_2 I(Intensity \geq 6)_w * I(Intensity \geq 5)_w * I(< 3MonthsPostEarthquake)_t + \sum_{k=1998M2}^{k=2001M12} I(YearMonth = k)_t + Age_{it} + \delta_w + \epsilon_{iwt} \tag{2.5}$$

[Insert Table 2.6 about here]

Results can be found in Table 2.6. Column 1 shows the regression results of Equation 2.4. We only focus on less than 3 months after the earthquake since in Figure 2.4, we find that this short-term effect π_1 captures the causal effect of the earthquake on labor complications within the first two months post-earthquake, which is an increase of about 4.0 percentage points. Column 2 presents the regression results of Equation 2.5. In Column 2, we can see that most of the negative effects on labor complications are being driven by women residing in the intensity 5 areas, rather than those from the intensity 6 areas. In particular, π_2+π_1 , the total earthquake effect on level 6+ intensity area is not statistically different from zero. This is not surprising, given that in the earlier analysis, we find that most fetal losses occur in the level 6+ areas (instead of level 5 area). This

pattern is consistent with the positive selection in level 6+ areas. Appendix Figure A1 illustrates this pattern of positive selection.

Second, we identify those pregnant women with chronic conditions prior to the earthquake.[13] In general, we find that women with chronic conditions are more susceptible to miscarriages, thus has a higher rate of fetal losses even without the presence of the earthquake.[14] We examine whether they are more likely to experience labor complications as a result of earthquake in the regression specification below.

$$\begin{aligned} I(LaborComplication)_{iwt} =& I(Chronic)_{it} + I(Chronic)_{it} * I(Intensity \geq 5)_w + \\ & \pi_1 I(Intensity \geq 5)_w * I(< 3MonthsPostEarthquake)_t + \\ & \pi_3 I(Chronic)_{it} * I(Intensity \geq 5)_w * I(< 3MonthsPostEarthquake)_t + \\ & \sum_{k=1998M2}^{k=2001M12} \beta_k * I(YearMonth = k)_t + \\ & Age_{it} + \delta_w + \epsilon_{iwt} \end{aligned} \tag{2.6}$$

Results are shown in Table 2.6, Column 3. Similar to before, π_1 is the causal estimate of the impact of the earthquake on labor complications is positive and significant. The casual impact of the earthquake on women with chronic condition is $\pi_1+\pi_3$, which is not statistically different from zero. Again similar to the finding before, given that women with

[13]Patients with chronic conditions have been identified by the National Health Insurance Bureau. It is recorded in our dataset. The chronic conditions include diabetes, hypertension, hyperlipidemia, cancer, mental illness, cardiovascular disease, chronic renal failure, chronic obstructive pulmonary disease, etc. Being identified with chronic conditions would allow the patients to pick up drugs for their conditions for longer duration.

[14]We use only 1998 health insurance claim data to estimate the relationship between chronic illness and miscarriages that are recorded in health insurance claims. We find that those with chronic illnesses are associated with a 9 percentage point increase in miscarriage and a 1.8 percentage point increase in labor complications even without the presence of the earthquake.

chronic conditions probably experience more fetal losses as a result of the earthquake, the pattern in Column 3 is consistent with a positive selection of fetuses. We illustrate this pattern in Appendix Figure A2.

2.4 Discussion of Channels and Bounding Exercise

2.4.1 Channels

There can be many reasons why a major earthquake could increase the likelihood of fetal losses and worsen pregnancy outcomes. Following is a list of reasons that we try to tackle them one by one: the earthquake could destroy health infrastructure or increase the overall crowdedness of hospitals; the earthquake could damage public infrastructure, affecting the food and water supplies; the earthquake could affect the frequency of prenatal visits; and lastly, a major earthquake could increase maternal stress.

[Insert Figure 2.5 about here]

The increase in fetal loss may be due to the crowdedness of the hospitals or the closure of the hospitals. In Figure 2.5, we plot the residual of inpatient and outpatient visits by earthquake intensity level after the township dummies and the month and year dummies included in our models have been controlled for. Surprisingly, we find a dramatic drop in the use of outpatient visits immediately following the earthquake. It is likely that individuals are avoiding going to hospitals for less urgent care post-earthquake. In addition, given the magnitude of this earthquake, this particular earthquake caused relatively few casualties compared to other earthquakes of similar intensity level (e.g., the Turkish earthquake in 1999 had approximately 17,000 deaths, and the Kobe earthquake in Japan in 1995 had 6,400 reported deaths). Even in the highest-intensity areas, with intensity levels of 6 and

7, the fatality rates are 1.0/1000 and 2.4/1000, respectively.[15] Thus, hospitals may not be as crowded as one expects. Our data also allows us to observe the number of hospitals that are actively treating patients. In Figure 2.6, we do not see evidence of hospital closures following the earthquake.

[Insert Figure 2.6 about here]

[Insert Table 2.7 about here]

Next, we examine whether the results are being driven by infrastructure damages. There are a lot of variations in the share of building collapses even within the same intensity level. Among level 6 and 7 intensity areas (41 townships), some have no building collapses in the township while some townships have nearly half of their buildings collapsed.[16] To distinguish whether the effect is from the damaged infrastructure, we focus on the subgroups that have high intensity (level 6 and level 7) but little damage (less than 5% buildings completely collapsed). The results are presented in Table 2.7, Column 2. We do observe that the coefficient has shrunk compared to the original specification, which is expected since we dropped all the most-severely damaged areas, but in this relatively undamaged and high-intensity area, the rate of fetal loses is still statistically different from zero. In addition, one possible channel is dirty water as a result of infrastructure damages. We examine gastrointestinal-related visits to hospitals/clinics using Equation 2.3, and we do not find a statistical significant increase in these visits. Overall, the finding seems to

[15] We do not have the breakdown of injuries by townships, so we do not know the rate of casualties by earthquake intensity. However, there are about 5 times more casualties than deaths resulting from this earthquake. If we assume the proportions between intensity levels is the same between casualties and deaths, it suggests that level 7 areas have about a 12/1000 casualty rate and level 6 areas have casualty rates of about 5/1000.

[16] One might worry that within a given earthquake intensity, townships with higher income may have fewer buildings collapsed. We run a regression and township average education level is not predictive of the share of buildings collapsed. This suggests that the share of buildings collapsed could be random within a given intensity level.

point to the reason beyond the infrastructure damages.[17]

Given that we find fewer non-urgent outpatient visits, it is possible that pregnant women may go to the hospital for prenatal visits fewer times as a result of the earthquake. Universal health insurance in Taiwan covers 10 free prenatal visits per pregnancy, and every prenatal visit is recorded in the health insurance claim data (but cannot be linked to the birth registry). Thus, for every birth/delivery, we can impute the number of prenatal visits. In Figure 2.4, we show that births in October-November 1999 are more likely to have labor complications. One possibility is that these births had fewer prenatal visits, which may result in a higher likelihood of labor complications. So we use the same specification as Equation 2.3: for each delivery i in township w and year-month t, we examine a number of prenatal visits as an outcome, and coefficients β_k are presented in Appendix Figure A3. We do not find any statistical difference in the number of prenatal visits (among those births immediately following the earthquake). This suggests that missing prenatal visits is not a reason for the increase in labor complications for those who had earthquake exposure during the third trimester. On the other hand, can missing prenatal visit explain the fetal losses that occurred for those with first-trimester exposure?[18] We think it is unlikely since the medical literature suggests that most fetal losses occur within the first two weeks of conception, which is earlier than the first prenatal visit—occurring around 8 weeks into pregnancy.

In addition to number of prenatal visits, we also look at outpatient medical expenditures

[17] We tried to use a repeated-cross-section dataset on labor force participation to examine the earthquake's effects on labor participation and hours of work. We find no significant impacts on unemployment, hourly wages, and working hours. It is possible that the dataset does not have enough power to detect the impact.

[18] We can look at frequencies of prenatal visits by intensity level; this is illustrated in Figure 2.7. Similar to the non-urgent outpatient visits, the number of prenatal visits immediately dropped after the earthquake for those who reside in intensity 6+ and this drop lasted for few quarters. There can be a few reasons of this drop. One is that for each given birth, pregnant women visiting the OB/GYN less frequently post-earthquake, and another possibility is that there are simply fewer births (either due to changes in conception or fetal losses). We cannot distinguish one reason from the other.

during pregnancy (9 months prior to birth delivery). Using a specification similar to equation 2.2, we examine the average outpatient expenditures among pregnant women residing in township w and giving birth in year-month t between high intensity areas and low intensity areas. The results do not show statistical significant changes in average outpatient medical expenditures for those pregnant women who gave births immediately after the earthquake.[19] This implies that lack of access to health care may not explain an increase in labor complication for births that were delivered shortly after earthquake.

We propose that one of the main explanations is due to maternal stress. First, there is an existing literature suggesting an association between maternal stress and gestational lengths and birth outcomes. The hypothesized biological mechanism is that maternal stress activates a higher level of cortisol that stimulates the release of placental corticotrophin-releasing hormone (CRH), which could affect birth outcomes (Copper et al. 1996; Dole et al. 2003; Hobel and Culhane 2003; McLean et al. 1995; Wadhwa et al. 2001; Wisborg et al. 2008). Some economists have exploited exogenous shocks to identify the effects of maternal stress on birth outcomes, including natural disasters (Frankenberg et al. 2013; Glynn et al. 2001; Torche 2011), the threat of terrorist attacks (Brown 2014; Camacho 2008; Eccleston 2011; Quintana-Domeque and Rdenas-Serrano 2014), armed conflict (Mansour and Rees 2012), and post-9/11 treatment of Arab women (Lauderdale 2006).

A major earthquake like this can increase one's stress level. Constant fear of aftershocks could last for months and the traumatic experience could even lead to post-traumatic stress disorder (Dimsdale 2008; Leor, Poole, and Kloner 1996; Siegel 2000). We examine psychiatric-related visits post-earthquake for males who are between age 16-45, and we find that those who reside in areas with high intensity increase depression-related outpatient visits by about 10% within half a year after the earthquake. It suggests that maternal

[19] Results are available upon request.

stress could be a possible explanation for our findings.

We cannot rule out the difference in nutrition intake: the first trimester, especially, is a critical period for the formation of organs. Overall, the fetal losses we estimate can possibly be due to the exposure to poor nutritional environment or maternal stress.

Last, one might suspect that the differences in cohort size in the birth registry could be due to the change in conception or even selective abortion post-earthquake. However, it is unlikely that parents were planning to avoid giving birth because of an unexpected earthquake. Furthermore, either of the alternative hypotheses would not be able to explain the drop in male-to-female sex ratio.

2.4.2 Bounding Exercise

As we have discussed before, a positive selection of survivors would lead to an underestimate of the true impact of the earthquake. We try to address the issue of survivor bias using a bounding exercise, following Lee's procedure (Lee 2009). In previous sections, we find that nearly 4.4% of fetuses who had first trimester-exposure to a high-intensity earthquake were culled and they are almost all male, and we show some evidence of positive selection. In the exercise, we drop the bottom 5% of observations by birth weight for each township-conception month cohort for males and females separately (males who had first-trimester exposure in intensity 6+ areas are excluded from this exercise) and rerun the regressions. The results are presented in Table 2.8, Columns 3 and 4. We find that birth weight for males who were exposed to the earthquake during the first trimester in high-intensity areas compared to those in low-intensity areas would drop by 48 grams and see their gestational length shorten by 0.10 week, both of which are higher than the impact on females. In Table 2.8, we also present the results without bounding exercise in Columns 1 and 2 for

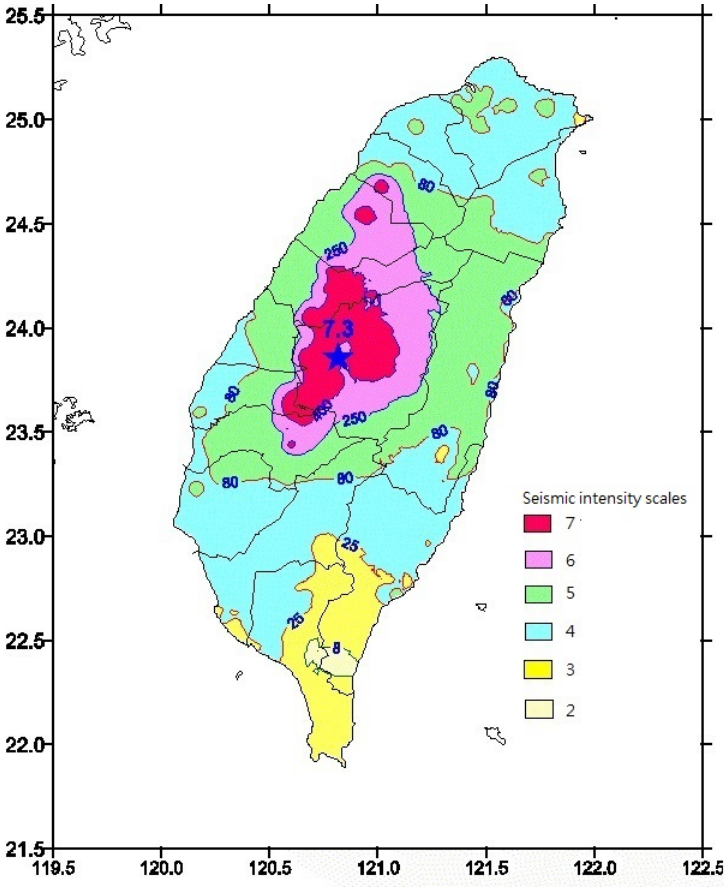
easy comparison. Before the bounding exercise, one might conclude that the earthquake has a greater impact on females, but once we correct for the survivor bias, it would suggest that the earthquake has a stronger, if not similar, impact on males.

[Insert Table 2.8 about here]

2.5 Conclusion

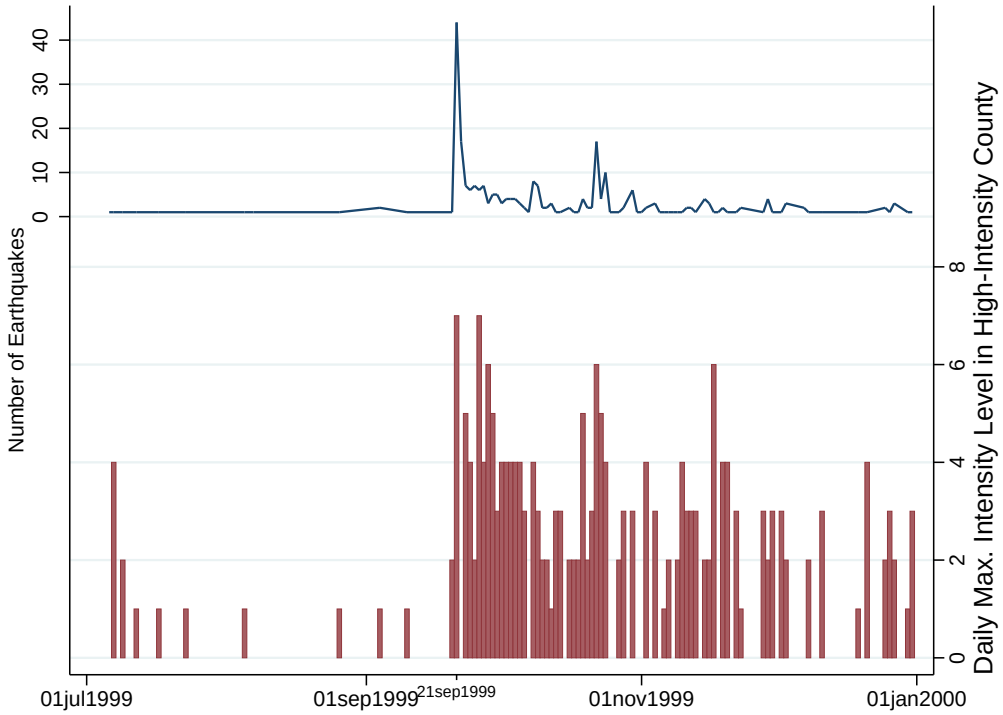
We use a major earthquake in Taiwan to examine the effects of a natural disaster on birth outcomes and incidences of pregnancy loss. We find evidence that early *in utero* exposure to the earthquake led to fetal losses and that exposure to the earthquake during the last trimester led to more labor complications and worse pregnancy outcomes. Almost all the pregnancy losses were driven by loss of a male fetus. We find evidence of positive selection on health. In sum, our findings on fetal losses suggest that the existing literature based on surviving past a certain gestational length is likely to have underestimated the impacts of natural disasters on pregnancy outcomes. Without the bounding exercise and the results on fetal losses, some may mistakenly conclude that the earthquake has a bigger impact on female fetus health, and draw the wrong conclusion, e.g. female fetuses suffer more because the son preference in Taiwan. The bounding exercise in this paper demonstrates that it is extremely important to consider survivor bias especially if poor intrauterine environment substantially affects the likelihood of fetal losses of one subgroup more than others.

Figure 2.1: Seismic Intensity Map of the Earthquake on September 21, 1999



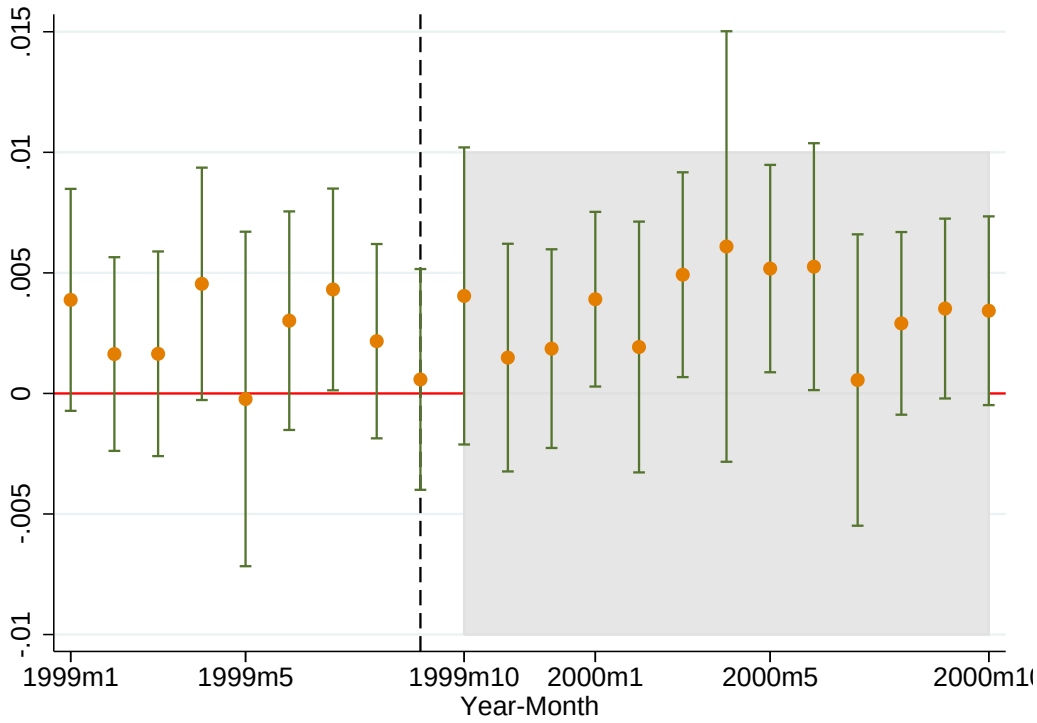
Notes: Map source: Central Weather Bureau, Taiwan.

Figure 2.2: Distribution of Detectable Earthquakes



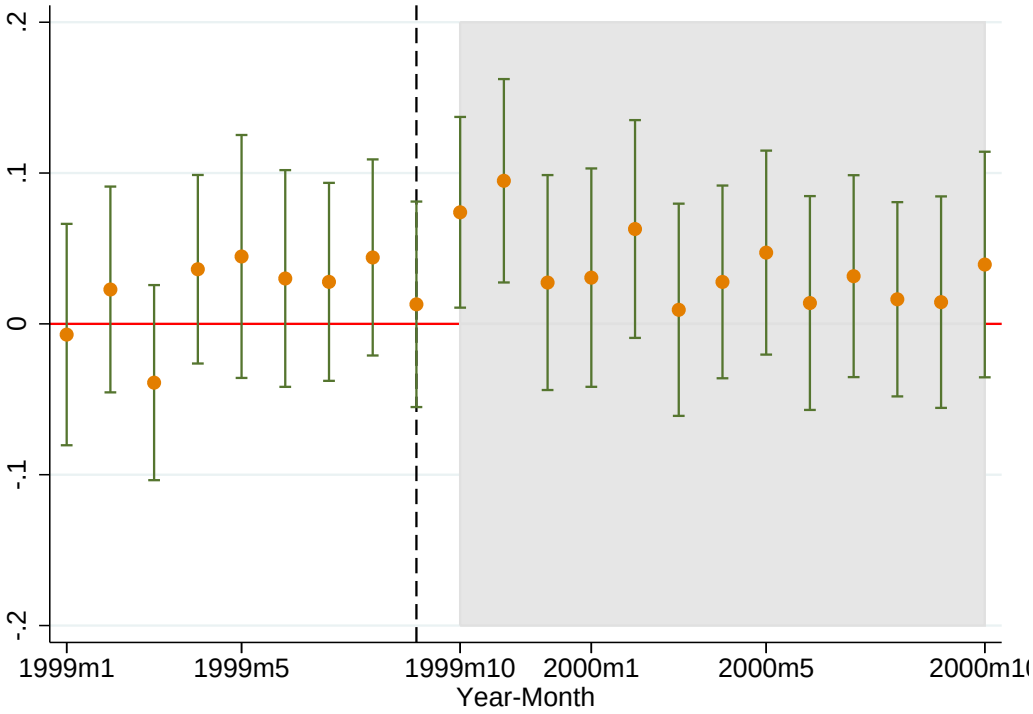
Notes: Data source: Central Weather Bureau, Taiwan.

Figure 2.3: Impact of Earthquake on Dilation and Curettage Procedure Rates



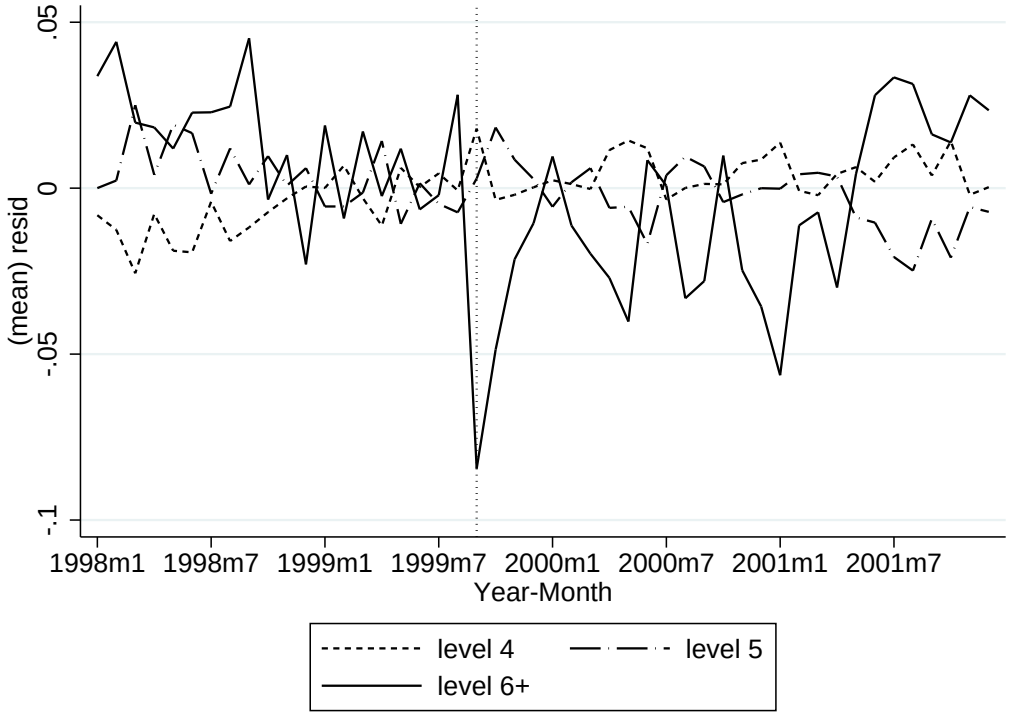
Notes: Regression estimates from Equation 2.2 are plotted. The dot and the bar correspond to the coefficient estimates with 90% confidence intervals. The shaded areas indicate those who probably had *in utero* exposure to the earthquake and its month-long aftershock. The dotted vertical line indicates the month when the earthquake occurred. Regression estimates are plotted. The covariates include year-month fixed effects, and township fixed effects.

Figure 2.4: Impact of Earthquake on the Likelihood of Labor Complications



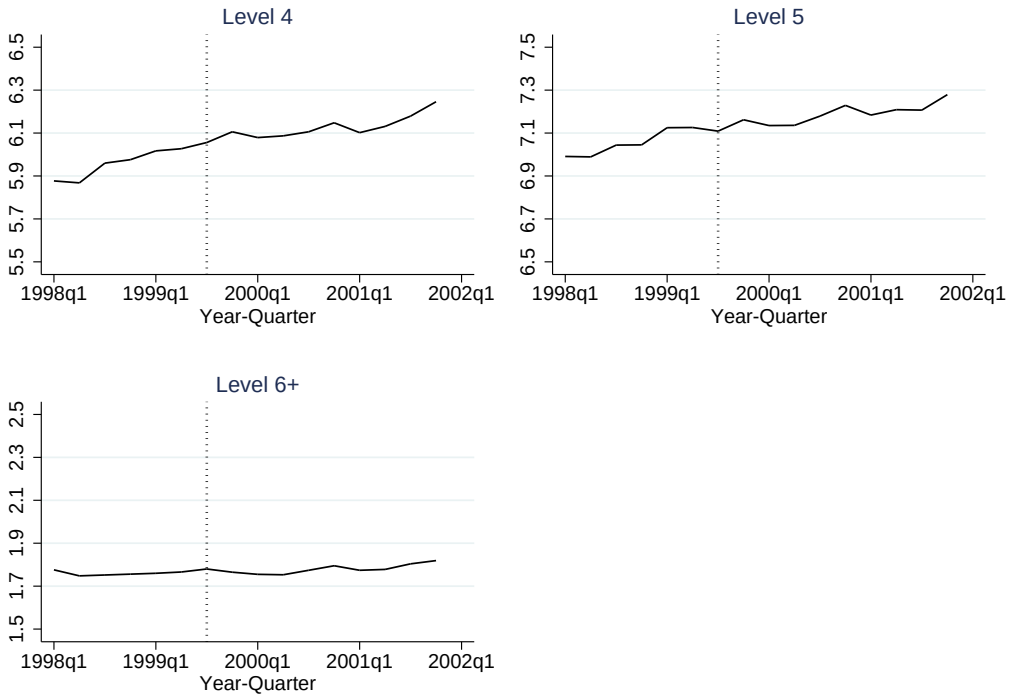
Notes: Regression estimates from Equation 2.3 are plotted. The dot and the bar correspond to the coefficient estimates with 90% confidence intervals. The shaded areas indicate those who probably had *in utero* exposure to the earthquake and its month-long aftershock. The dotted vertical line indicates the month when the earthquake occurred. Regression estimates are plotted. The covariates include year-month fixed effects, township fixed effects, and a set of age dummies.

Figure 2.5: Residuals of Numbers of Outpatient Visits by Intensity Level, 1998–2001



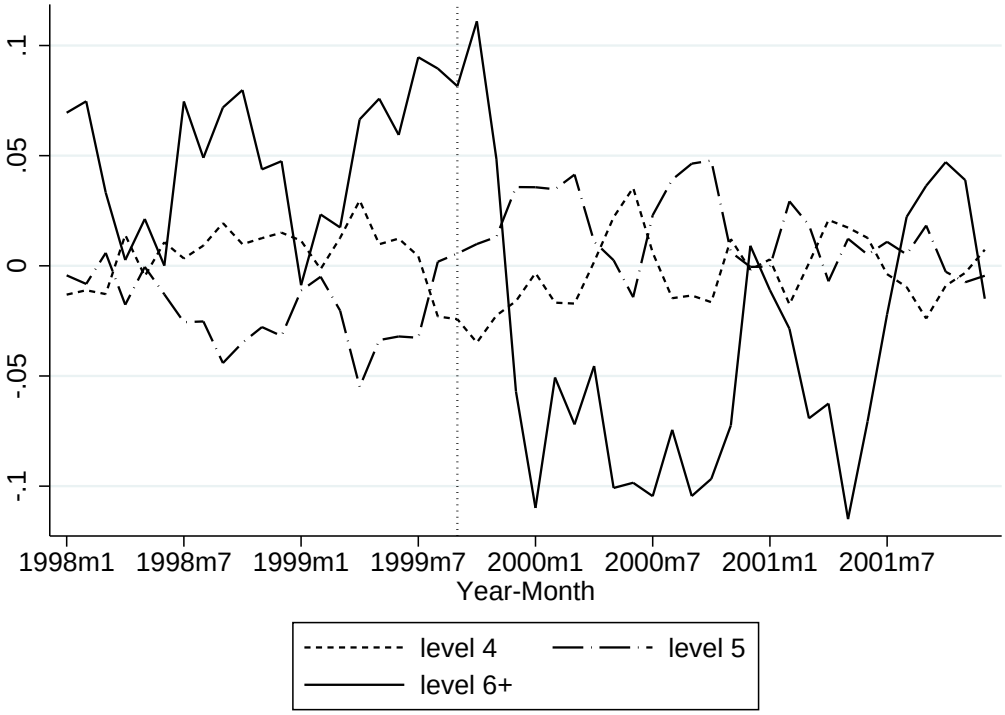
Note: This figure plots residual levels of log number of outpatients visits after the township dummies, month and year dummies included in our models have been controlled for. Unit of observations: township-months. We exclude OB/GYN and prenatal visits. The vertical dotted line correspond to the month when the earthquake occur. Level 6+ include both townships with level 6 and level 7 intensity. Level 4 include those townships with intensity level 4 or below.

Figure 2.6: Number of Hospitals/Clinics by Earthquake Intensity



Note: Data source Health Insurance Claim from 1998–2001. The vertical dotted line correspond to the month when the earthquake occur. Level 6+ include both townships with level 6 and level 7 intensity. Level 4 include those townships with intensity level 4 or below.

Figure 2.7: Number of Prenatal Visits by Earthquake Intensity, 1998–2001



Note: This figure plots residual levels of log number of prenatal visits by township intensity level after month and year dummies have been controlled for. The vertical dotted line correspond to the month when the earthquake occur. Level 6+ include both townships with level 6 and level 7 intensity. Level 4 include those townships with intensity level 4 or below.

Table 2.1: Descriptive Statistics of Birth Registry by Township Earthquake Intensity Level, 1998-2001

Intensity level	4	5	6+
	(1)	(2)	(3)
Cohort size	56.336	76.803***	56.157
	(65.744)	(80.044)	(54.371)
Male-to-female ratio	1.064*	1.082	1.088
	(0.459)	(0.382)	(0.443)
Infant mortality rate	6.006	6.36	6.337
(per 1,000 live births)	(20.172)	(17.520)	(16.988)
Preterm	0.068	0.066	0.067
(less than 37 weeks)	(0.063)	(0.050)	(0.055)
Birth weight	3132.328***	3139.275***	3121.117
	(117.803)	(98.570)	(103.816)
Gestational length	38.633	38.638	38.627
	(0.450)	(0.358)	(0.381)
Number of townships	170	142	41
Number of observations (conceiving month-township)	7,310	6,106	1,763

Note: Data source: Birth Registry, 1998–2001. Earthquake intensity level is based on the township of mother's permanent residence registration (hukou). Intensity 6+ includes 9 townships with level 7 earthquake shock and intensity 4 includes 17 townships with level 3 earthquake shock. Standard deviations are reported in parentheses. *, **, *** denote the p-values at 1%, 5%, 10% levels from t-test of the equality between the given number and the reported number for level 6.

Table 2.2: Descriptive Statistics of Health Insurance Claim Records By Township Earthquake Intensity Level, 1998–2001

Intensity level	<=4	>=5
	(1)	(2)
Mother with pre-existing health conditions	0.233***	0.258
	(0.423)	(0.437)
Conditional on giving birth:		
Complications during delivery	0.172***	0.190
	(0.378)	(0.393)
Age of mother	28.004***	27.722
	(4.819)	(4.817)
Number of observations (pregnancy)	16,240	24,825

Note: Data source: 5% Health Insurance Claim Records, 1998-2001. Pre-existing conditions include heart conditions, hypertension, stroke, diabetes, asthma, cancer, and high cholesterol. We use all outpatient visits prior to the earthquake to identify the most frequently visited township as mother's residence. Earthquake intensity is based on the township of the mother's residence. *, **, *** denote the p-values at 1%, 5%, 10% levels from t-test of the equality between the given number and the reported number for level 6.

Table 2.3: Impact of Intrauterine Earthquake Exposure on Fetal Losses and Sex Ratio

Dependent Variable	Log Cohort Size	M/F Ratio
Mean across Cohort Average	3.665	1.08
	(1)	(2)
(Intensity≥6) *(Conceived 7-9M before Earthquake)	-0.027	-0.028
	(0.018)	(0.038)
*(Conceived 4-6M before Earthquake)	-0.032*	-0.024
	(0.017)	(0.029)
*(Conceived 0-3M before Earthquake)	**-0.044*** **	**-0.082*** **
	(0.013)	**(0.027)**
(Intensity= 5)*(Conceived 7-9M before Earthquake)	-0.002	-0.000
	(0.009)	(0.020)
*(Conceived 4-6M before Earthquake)	-0.024**	-0.008
	(0.010)	(0.018)
*(Conceived 0-3M before Earthquake)	-0.007	-0.021
	(0.009)	(0.017)

Note: N=15,179. Data source: Birth Registry, 1998–2001. This table presents the estimation results of specification (1). Cohort is defined as those conceived in the same month and same township. Each column is from a single regression, weighted by township female population size. All regressions include a set of post-earthquake dummies interacting with intensity level, conceiving month FE and township FE. Std. Errors are clustered at township level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 2.4: Impact of Intrauterine Exposure to Earthquake on Birth Outcomes by Gender

Dependent Variable	Birth Weight		Gestational Length	
	Male	Female	Male	Female
Mean across Cohort Average	3178.518	3085.831	38.563	38.712
	(1)	(2)	(3)	(4)
(Intensity≥6)*(Conceived 7-9M before Earthquake)	-6.273	-15.744**	0.015	-0.031
	(7.353)	(7.015)	(0.036)	(0.034)
* (Conceived 4-6M before Earthquake)	-25.378***	-11.295	-0.047	-0.068**
	(8.870)	(8.342)	(0.035)	(0.033)
* (Conceived 0-3M before Earthquake)	-3.887	-13.654*	0.003	-0.022
	(7.987)	(7.949)	(0.028)	(0.030)
(Intensity=5)* (Conceived 7-9M before Earthquake)	-8.614	-4.776	-0.008	-0.019
	(5.496)	(6.237)	(0.019)	(0.025)
* (Conceived 4-6M before Earthquake)	-7.461	-3.525	-0.050**	-0.040*
	(5.697)	(6.117)	(0.022)	(0.021)
* (Conceived 0-3M before Earthquake)	-7.558	-9.138*	-0.042**	-0.009
	(4.753)	(5.012)	(0.019)	(0.018)

Note: Data source: Birth Registry, 1998–2001. This table presents the estimation results of specification (1). Cohort is defined as those conceived in the same month and same township. Each column is from a single regression, weighted by cohort size. All regressions include conceiving month FE and township FE and a set of interaction terms between township intensity level and conceived X months after earthquake (same as Table 3). Std. Errors are clustered at township level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 2.5: Robustness Check for Impact of Earthquake Exposure on Fetal Losses Using Alternative Definition of High Intensity

	Original	Alternative I	Alternative II
	(1)	(2)	(3)
(Highest Intensity Level) *(Conceived 7-9M before Earthquake)	-0.027	-0.026*	-0.032*
	(0.018)	(0.013)	(0.017)
*(Conceived 4-6M before Earthquake)	-0.032*	-0.017	-0.026*
	(0.017)	(0.018)	(0.016)
*(Conceived 0-3M before Earthquake)	**-0.044*** **	**-0.059*** **	**-0.049*** **
	(0.013)	**(0.016)**	**(0.017)**
(2nd Highest Intensity Level)*(Conceived 7-9M before Earthquake)	-0.002	-0.000	0.001
	(0.009)	(0.011)	(0.011)
*(Conceived 4-6M before Earthquake)	-0.024**	-0.023**	-0.002
	(0.010)	(0.012)	(0.012)
*(Conceived 0-3M before Earthquake)	-0.007	-0.006	0.005
	(0.009)	(0.009)	(0.009)

Note: N=15,179. Data source: Birth Registry, 1998–2001. This table presents the estimation results of specification (1). Cohort is defined as those conceived in the same month and same township. Each column is from a single regression, weighted by township population size. Column 1 reports the results of original specification from Table 3 Column 1. As an alternative measure I use the rounding down of the intensity level. Highest intensity is still those areas with level 6.0 and higher and second-highest intensity level includes those townships with intensity between 5.0–5.9. All regressions include conceiving month FE and township FE. Std. Errors are clustered at township level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 2.6: Impact of Intrauterine Exposure to Earthquake on Labor Complication

	(1)	(2)	(3)
I(<3 months Post Eqk)*I(Intensity≥5) (π1)	0.040***	0.043**	0.045***
	(0.015)	(0.017)	(0.017)
I(<3 months Post Eqk)*I(Intensity≥6)*I(Intensity≥5) (π2)		-0.012	
		(0.027)	
I(<3 months Post Eqk)*I(Chronic illness)*I(Intensity≥5) (π3)			-0.027
			(0.046)
I(Chronic illness)*I(Intensity≥5)			0.012
			(0.011)
I(<3 months Post Eqk)*I(Chronic illness)			0.023
			(0.035)
Chronic Illness			-0.002
			(0.009)
H0: π1+π2=0		0.19	
H0: π1+π3=0			0.673

Note: N=41,065. Data source: 5% Health Insurance Claim Records, 1998–2001. This table presents the estimation result of specifications 3a, 3b and 4 in Columns 1, 2 and 3, respectively. I(Chronic Illness) indicates whether one has chronic illness prior to earthquake. I(¡3 months Post Eqk) indicates whether a birth occurred within three months after earthquake.I(IntensityX) indicates whether one resides in areas with earthquake intensity greater or equal to X level. We use all outpatient visits prior to the earthquake to identify the most frequently visited township as mother's residence. All regressions include mother's age FE, township FE, month FE. Std. Errors are clustered at the township level. *** p¡0.01, ** p¡0.05, * p¡0.1.

Table 2.7: Robustness Check for Impact of Intrauterine Earthquake Exposure on Fetal Losses

	Original	Dropping>5%
	(1)	(2)
(Intensity Level 6+) *(Conceived 7-9M before Earthquake)	-0.027	-0.014
	(0.018)	(0.026)
*(Conceived 4-6M before Earthquake)	-0.032*	-0.022
	(0.017)	(0.027)
*(Conceived 0-3M before Earthquake)	**-0.044*****	-0.028*
	(0.013)	(0.015)
(Intensity Level 5)*(Conceived 7-9M before Earthquake)	-0.002	-0.002
	(0.009)	(0.009)
*(Conceived 4-6M before Earthquake)	-0.024**	-0.024**
	(0.010)	(0.010)
*(Conceived 0-3M before Earthquake)	-0.007	-0.008
	(0.009)	(0.008)
N	15,179	14,276

Note: Data source: Birth Registry, 1998–2001. This table presents the estimation results of specification (1). Column 1 is the same as Table 3 Column 1. Column 2 drops all townships with more than 5% of building collapsed.Cohort is defined as those conceived in the same month and same township. Each column is from a single regression, weighted by township population size. All regressions include conceiving month FE and township FE and a set of interaction terms between township intensity level and conceived X months after earthquake (same as Table 3). Std. Errors are clustered at township level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 2.8: Impact of Intrauterine Exposure to Earthquake on Birth Outcomes with Bounding Exercise

	Male original	Female original	Male Bounding	Female Bounding
	(1)	(2)	(3)	(4)
Panel A: Dependent Variable Birth Weight (grams)				
(Intensity≥6)*(Conceived 7-9M before Earthquake)	-6.273	-15.744**	-11.047	-21.017**
	(7.353)	(7.015)	(9.705)	(8.141)
* (Conceived 4-6M before Earthquake)	-25.378***	-11.295	-16.035	-9.343
	(8.870)	(8.342)	(10.830)	(11.582)
* (Conceived 0-3M before Earthquake)	-3.887	-13.654*	-48.375***	-20.887**
	(7.987)	(7.949)	(11.950)	(8.664)
(Intensity=5)* (Conceived 7-9M before Earthquake)	-8.614	-4.776	-11.883*	-9.348
	(5.496)	(6.237)	(6.332)	(6.157)
*(Conceived 4-6M before Earthquake)	-7.461	-3.525	-9.206	-9.452
	(5.697)	(6.117)	(6.392)	(7.589)
*(Conceived 0-3M before Earthquake)	-7.558	-9.138*	-15.113***	-11.879**
	(4.753)	(5.012)	(5.097)	(5.543)
Panel B: Dependent Variable Gestation Length (weeks)				
(Intensity≥6)*(Conceived 7-9M before Earthquake)				
	0.015	-0.031	-0.014	-0.090**
	(0.036)	(0.034)	(0.040)	(0.036)
* (Conceived 4-6M before Earthquake)	-0.047	-0.068**	-0.037	-0.076**
	(0.035)	(0.033)	(0.036)	(0.036)
* (Conceived 0-3M before Earthquake)	0.003	-0.022	-0.105***	-0.037
	(0.028)	(0.030)	(0.036)	(0.039)
(Intensity=5)* (Conceived 7-9M before Earthquake)	-0.008	-0.019	0.016	-0.081***
	(0.019)	(0.025)	(0.022)	(0.022)
*(Conceived 4-6M before Earthquake)	-0.050**	-0.040*	-0.065***	-0.048**
	(0.022)	(0.021)	(0.023)	(0.024)
*(Conceived 0-3M before Earthquake)	-0.042**	-0.009	-0.021	-0.028
	(0.019)	(0.018)	(0.021)	(0.021)

Note: Data source: Birth Registry, 1998–2001. We apply Lee's bounding method (Lee 2009) in Columns 3/4 by dropping the bottom 5% of observations by birth weight for each cohort (the only exception are males in Intensity 6*Conceived 0–3M before earthquake). All regressions include conceiving month FE and township FE and a set of interaction terms between township intensity level and conceived X months after earthquake (same as Table 3). Std. Errors are clustered at township level. *** p¡0.01, ** p¡0.05, * p¡0.1

Chapter 3

The Hidden Costs of Natural Disasters: *In Utero* Environment and Mental Health in Adulthood

3.1 Introduction

Mental illness affects hundreds of millions of people across the world. An estimated loss of global output associated with mental health conditions will reach $6 trillion in the next two decades, second only to cardiovascular diseases (Bloom et al. 2011). In 2004, one in four U.S. adults was estimated to experience some mental disorders (CDC 2011). Earlier studies document contemporaneous effects of adverse events on psychological well-being. Recent literature in this line points out that the determinants of mental illness can be traced back to the critical period while *in utero* (Abel et al. 2014; Adhvaryu et al. 2015; Adhvaryu, Fenske, and Nyshadham 2014; Almond and Mazumder 2011; Class et al. 2013; Dinkelman 2015; Persson and Rossin-Slater 2014). The objective of this study is to understand the impacts of intrauterine environment on adult mental health and related health care utilization. This paper contributes to the literature documenting a causal

relationship between poor intrauterine environment and mental health in adulthood. To the best of my knowledge, this is the first paper that uses outpatient visits to identify adult mental illness over a broader range of severity, not necessarily requiring hospitalization or drug treatment to investigate the long-term impacts of *in utero* environment. This study is also one of the first few papers that attempts to examine the full aspects of the long-term effects on mental illness as a result of natural disasters.

In this project, I study the impact of severe typhoons in Taiwan on the incidence of mental disorders, psychiatric drug use, and psychiatric-related health care utilization in adulthood.[1] While Taiwan is in a typhoon zone, and experiences typhoons every year, most of the typhoons caused limited damages. Here I focus my analysis using severe typhoons, defined as causing more than 50 deaths. A severe typhoon occurred in only five of the years between 1958 and 1970. Importantly, the timing and landfall regions of severe typhoons are exogenous to the decision to have a child in a particular year and month. The clear trajectory of typhoons can be used to identify high intensity regions, specifically the landfall counties of severe typhoons. My main identification strategy is an interrupted time series design. By exploiting time and regional variation, I compare the mental health of individuals who were exposed to severe typhoons while *in utero* in landfall counties to those who had no fetal exposure to severe typhoons.

There are several advantages to using data from Taiwan. One key advantage of examining Taiwan is the availability of data. I use detailed health insurance claim records of a 5 percent population sample (approximately 1 million individuals) drawn in 2000 to identify mental disorders and calculate psychiatric-related health care utilization. The claim records provide physicians' diagnoses codes and comprehensive prescription drugs for each outpatient and inpatient visit that is covered by the insurance between 1998 and 2002.

[1] Kessler et al. (2007) points out that 75% of mental disorders occur by the mid-20s.

The clinical measures from this dataset are less susceptible to misreporting compared to self-reported mental health status as used in many other studies. Another advantage of this dataset is that I can identify psychiatric-related medical use from visits to psychiatrists as well as to other physicians. Visiting a psychiatrist is not common in Taiwan. For example, 25% of antidepressants were prescribed by non-psychiatric physicians and the majority of those were prescribed by doctors in family medicine and internal medicine.[2] Using prescriptions from all physicians will help to alleviate under-reporting issues that may arise due to social stigma attached to seeing a psychiatrist.

In order to map individuals with their *in utero* exposure to severe typhoons, I constructed a typhoon dataset using the Typhoon Database of the Central Weather Bureau and the 2014 Annual Disaster Report from the National Fire Agency. Each typhoon event includes dates, landfall regions, casualties, and property damage. Ideally, individuals' prenatal exposures to severe typhoons are identified based on date of birth and county of birth. Unfortunately, county of birth is not available in the health insurance claim records dataset, since the dataset only provides township/county of current residence. To minimize potential bias of migration, I restrict the analysis to rural areas where migration occurs less frequently.[3] Using census data from 2000, I verified that 77% of residents in rural areas were born in the county of residence. To the extent that I miscode individuals' birth places, measurement errors will attenuate estimates, therefore, leaving it more difficult to detect an effect of severe typhoons. As a robustness check, I further address migration issues by examining the effects of typhoons on farmers and fishermen, whose migration rates are extremely low.[4] Additionally, I calculate in-migration and out-migration rates

[2]The statistics was calculated based on individuals who were born between 1959 and 1970.

[3]More specifically, rural areas refer to Shiang and Zhen (township). *Shiang* and *Zen* are the third-level administrative subdivisions. This classification is based on population.

[4]Farmers and fishermen are identified based on their insurance status.

based on census data from 2000, and focus the analysis on those areas where movement of population is not common.

My analysis suggests that the likelihood of mental illness and the use of psychiatric drugs increased by 11% for individuals who were exposed to severe typhoons while *in utero* relative to individuals who had no fetal exposure to severe typhoons. I also find fetal exposure to a severe typhoon is associated with more psychiatric-related visits and health care expenditures. The negative effects on mental health are almost exclusively found among women. I find some evidence of positive mortality selection, which suggests that weaker male fetuses could be selected out during pregnancies in the presence of severe typhoons. Thus, it is likely that the male survivors may be healthier than their female counterparts. Moreover, I show that the main findings are robust to several alternative specifications including using different measures of typhoon severity, restricting to subsamples to reduce doubts about the comparability of control and treatment groups, adding additional controls for temperature and rainfall, using alternative specifications that control for county-cohort specific trends and region-birth year fixed effects, and conducting placebo tests. Lastly, I find that *in utero* exposure has the greatest impacts on mental health relative to the effects of exposure in the first few years of life. This suggests that it may be beneficial if there are disaster assistance programs targeting pregnant women.

The chapter is organized as follows. Section 2 discusses the related literature. Section 3 describes the newly constructed severe typhoon dataset and the health insurance claim records. Section 4 outlines the empirical strategy, and section 5 presents the results. Section 6 discusses the mechanisms and section 7 concludes.

3.2 Related Literature

Mental well-being is believed to be determined by various dimensions including individual characteristics, socioeconomic circumstances, and environmental factors (WHO 2012). Literature has explored the contemporaneous effects of adverse events on mental health such as natural disasters (Edwards, Gray, and Hunter 2015; Shoaf et al. 2004), war and terrorist attacks (Brattia, Mendolab, and Mirandac, 2014; Galea et al., 2003; Schlenger et al., 2002), and recession and job loss (Bradford and Lastrapes, 2014; Currie and Tekin, 2015; Kuhn, Lalive, and Zweimuller, 2009; McInerney, Mellor, and Nicholas, 2013). A common finding is that the prevalence of post-traumatic stress disorder (PTSD) and the use of psychiatric drugs increased, and self-reported mental health is worse.

In addition, events that occurred in prenatal life can also influence psychological well-being (WHO 2012). Medical literature proposes the "Neurodevelopment Hypothesis," which argues that *in utero* exposure to adverse events damages one's neural development, which in turn leads to mental illness later in life (Bennet and Gunn 2006). One hypothesized mechanism focuses on elevating the activity of neuroendocrine systems, particularly the hypothalamic-pituitary-adrenal and automatic nervous systems (Phillips 2007). Fetuses are supposed to be protected from maternal glucocorticoids (Edwards et al. 1993). An animal study shows that the deficiency of a placental enzyme (11 β-hydroxysteroid dehydrogenase type 2) allows excess amounts of maternal glucocorticoids to be exposed to fetuses resulting in affecting the fetal brain (Holmes et al. 2006). Khalife et al. (2013) points out the association between prenatal exposure to synthetic glucocorticoids and deterioration in child and adolescent mental health. Some clinical studies show that higher levels of maternal cortisol and prenatal anxiety and stress are associated with increased affective problems and lower mental development (Buss et al. 2012; DiPietro et al. 2006;

Huizink et al. 2003).

Building on the literature, this study is relevant to a large body of literature that investigates the short- and long-term effects of exposure to poor intrauterine environment (see Almond and Currie 2011 and Currie and Vogl 2013 for a review).[5] Although the literature has provided extensive evidence on the impacts on pregnancy outcomes, physical health, and human capital formation, the causal relationship of *in utero* environment and adult mental health is relatively under-studied.

In an effort to investigate the causal impacts, studies use exogenous events or shocks to examine the effects.[6] Almond and Mazumder (2011) find that adults who experienced Ramadan in the early stage of their fetal life were more likely to report mental/learning disabilities. Dinkelman (2015) shows that early childhood exposure to droughts increases mental disability, and the effects of the droughts are larger for males relative to females. Adhvaryu, Fenske, and Nyshadham (2014) use a Ghanaian survey to investigate the effects of *in utero* exposure to income shocks on adult mental health. They found that individuals who experienced a positive income shock while *in utero* were less likely to report mental problems. While these studies are novel and interesting, they are based on self-reported mental health. Survey measures are likely to suffer from the issue of misreporting. Bharadwaj, Pai, and Suziedelyte (2015) match individuals' self-reported mental health status and prescriptions for depression, and find that survey measures of mental conditions are significantly under-reported. They suggest that the mis-reporting is likely due to social stigma.

[5]Fetal Origin Hypothesis, which suggests that adverse *in utero* events can lead to worse health conditions later in life, was first introduced by Barker (1990).

[6]Natural experiments or exogenous shocks is widely used to examine the impacts of poor *in utero* environment: famine (Almond et al. 2010; Hernandez-Julian, Mansour, and Peters 2014), influenza (Almond 2006; Kelly 2011; Lin and Liu 2014, extreme weather events and natural disasters (Currie and Rossin-Slater 2013; Dinkelman 2015; Frankenberg et al. 2013; Glynn et al. 2001; Hernandez-Julian, Mansour, and Peters 2014; Liu, Liu, and Tseng 2015; Simeonova 2011; Torche 2011), and armed conflicts (Camacho 2008; Lauderdale 2006; Mansour and Rees 2012).

It is possible that individuals' characteristics may influence their responses.

Some researchers resort to psychiatry admission data to investigate the long-term impacts on mental health. Os and Selten (1998) compare the prevalence rate of individuals who were exposed to the German invasion of the Netherlands while *in utero* to individuals who were born before and after the event. They find that birth cohorts who had first-trimester exposure to the German invasion had 1.15 times the risk of schizophrenia relative to unexposed birth cohorts. Watson et al. (1999) suggest that exposure to influenza and earthquake during the second trimester of gestation increases the likelihood of adult schizophrenia and depression. Although these two studies find negative impacts of adverse events, they are based on psychiatric hospital admissions, which may represent the most extreme outcome and may not capture the full effects of war, earthquake, and influenza. Using Swedish administrative data, recent studies examine the impacts of bereavement during pregnancy on adult mental illness (Abel et al. 2014; Class et al. 2013; Persson and Rossin-Slater 2014). Abel et al. (2014) find prenatal bereavement has no effects on hospital admissions related to non-affective and affective psychoses among adults aged between 20 and 33. Similarly, Class et al. (2013) do not find increased schizophrenia and bipolar disorder among individuals of age 12–36 as a result of bereavement during pregnancy, yet they find that third-trimester exposure to loss of a close relative increases risk of outpatient and inpatient diagnoses of autism and attention-deficit/hyperactivity disorder (ADHD). Notably, the prevalence rates of adult mental illness in Abel et al. (2014) and Class et al. (2013) are strikingly low given that their findings on adult mental illness rely on hospital admissions.

In addition to inpatient records, Persson and Rossin-Slater (2014) further use prescription drugs to examine the effects of bereavement on child ADHD and adult anxiety and

depression. Their results suggest prenatal bereavement raises the likelihood of using ADHD drugs by 18 percent among children aged between 9 and 11. They also find that the likelihood of using anxiety and depression drugs increased by 11 and 7 percent, respectively, among adults aged 34 to 36. The mixed results on adult mental illness between these three Swedish studies could be due to the fact that Persson and Rossin-Slater (2014) focus on middle-aged adults, and they also use detailed prescription data.

This paper uses universal health insurance claim records, including inpatient and outpatient visits and drug prescriptions, to identify mental illness by diagnostic codes. This would permit measuring incidences of mental illness over a broader range of severity, not necessarily requiring hospitalization or drug treatment. The particular shock I use to the intrauterine environment is severe typhoons. One particular advantage of using typhoons is the detailed information on the typhoon's timing and path. Based on the detailed health insurance claim records from Taiwan, I am able to use outpatient and inpatient and prescription data to provide a better understanding of the extent to which poor *in utero* environment affects mental health.

3.3 Data

3.3.1 Typhoon Severity Dataset

The historical typhoon data used in this study was drawn from the Typhoon Database of the Central Weather Bureau and the 2014 Annual Disaster Report from the National Fire Agency.[7] Each typhoon event consists of information on dates, landfall counties, casualties,

[7]Typhoon data is only available for the periods after 1958. Together with the available periods of health insurance claim records, the study sample restricts to individuals who were born between 1959 and 1970. Retrieved from http://rdc28.cwb.gov.tw/.

and property damage.[8] Taiwan is in a typhoon zone, and it is typically affected every year by at least one typhoon, which usually occurs during summer. Having detailed data on each typhoon event is extremely crucial since it allows me to separate extreme typhoon events from regular ones, and also to identify high-intensity regions from low-intensity regions. In general, a typhoon will cause more damage when it initially makes landfall and its structure will then weaken quickly.[9]

Figure 3.1 depicts the number of typhoons and maximum casualties of a unique typhoon in a given year and month. Although Taiwan usually experiences several typhoons a year, the most severe events do not occur annually. Since the majority of mental illnesses are not identified until the individual's mid-twenties (Kessler et al. 2007), I focus on individuals who were born between 1959 and 1970. During the periods of 1959 and 1970, 41 out of 119 typhoons resulted in casualties, and the conditional mean of the death toll from a given typhoon was 46.[10] In this study a severe typhoon is defined as one causing more than 50 deaths, so I use five typhoon incidents in the main analysis. To confirm that the results are not sensitive to the definition of severe typhoons, I will use alternative definitions for severity including using casualty rates as well as the number of collapsed buildings in the robustness section.

[Insert Figure 3.1 about here]

[8] Casualties and property damage are reported at the national level.

[9] During the time periods, there were four typhoons that caused more than 50 deaths and did not make landfall. Individuals who were exposed to those typhoons are coded as exposed in non-landfall counties. When I use a specification which separates all three exposure status, and the results between exposure of non-landfall counties and exposure to non-landfall typhoons are not statistically different. Thus, I decide to combine these two exposure status for the sake of simplicity.

[10] The medium and mean of death toll from a given typhoon were zero and fifteen respectively.

3.3.2 Health Insurance Claim Records

The second dataset used in this study includes detailed health insurance claim records of a 5 percent Taiwanese population sample (approximately 1 million individuals) drawn in 2000. Universal health insurance in Taiwan was first introduced in 1995, and the coverage rate was more than 96% within two years of implementation. The dataset contains all outpatient and inpatient visits and drug prescriptions that were covered by universal health insurance between 1998 and 2002 for these individuals. International Classification of Diseases (ICD-9) codes were used to identify the reasons for each medical visit.[11] Mental illness is identified if physicians made a diagnosis of mental disorders including psychoses, organic psychotic conditions, other psychoses, neurotic disorders, personality disorders, and other nonpsychotic mental disorders.

Drug prescriptions include Anatomical Therapeutic Chemical (ATC) codes for every prescribed drug. ATC codes were used to identify psychiatric drugs including antidepressants, antipsychotics, and anxiolytics.[12] The use of psychiatric drugs is identified if an ATC code of psychiatric drugs was recorded and the visit was psychiatric-related based on ICD-9 codes. Since health claim records do not contain the county of birth for individuals, I proxy the birth county based on the county of each individual's most frequently visited outpatient hospitals/clinics in 2000.[13] According to 2000 census data, nearly half of residents of urban areas reside outside of their county of permanent residence (hukou,

[11] See Appendix A.1 for a list of ICD-9 codes of mental disorders and other illness that are examined in this study.

[12] ATC system classification system is published by World Health Organization Collaboration Center for Drug Statistics Methodology. To the best of my knowledge, it is very difficult to identify off-label drug use from the claim records, therefore, I follow ATC classification for the primary use of drugs. See Appendix A.2 for a list of ATC codes that were used to identify psychiatric and other drugs.

[13] For individuals who were insured through township government or farmer's & fisherman's unions, I used their insured township as residence. The township of most respiratory-related visits was used as residence for the rest of individuals.

usually the birth county), while 77% of rural residents stay in their birth county. In order to reduce migration issues, I focus on individuals of rural townships in the empirical analysis.[14] Individuals were mapped with their *in utero* exposure to severe typhoons based on the year and month of their birth and the county of residence.

Table 3.1 presents the summary statistics at the individual level. Overall, one in five individuals had a visit related to any mental illness. The prevalence rate is comparable to more developed countries such as the U.S., U.K., and Canada.[15] Due to the features of Taiwan's single-payer health care system, total expenditure on health care may seem low as compared to countries with multi-payers systems such as the U.S. On average, individuals who were exposed to severe typhoons are slightly older because three out of five severe typhoons occurred in the first few years of the study periods. These individuals also have lower socio-economic status than their unexposed counterparts. Those individuals who were exposed to severe typhoons while *in utero* and were residing in landfall counties have the highest prevalence rates of mental disorders and usage of psychiatric drugs, followed by exposed individuals of non-landfall counties, with the unexposed individuals having the lowest prevalence rates. Prenatal exposure to severe typhoons in landfall county is also associated with more health care utilization and expenditures. However, these differences may be due to factors other than fetal exposure to severe typhoons, such as cohort differences. In the next section, I will use rigorous specifications to estimate the causal effects of severe typhoons.

[Insert Table 3.1 about here]

[14] In the main analysis, rural townships are defined by administrative divisions. As a robustness check, I will show the results of using migration rates to identify rural townships.

[15] The one-year prevalence rate of mental illness is 21.5% among U.S. adults aged between 26 and 49 in 2013 (Substance Abuse and Mental Health Services Administration 2014). Nearly 25% of British adults aged between 16 and 74 experienced a mental illness in a given year (Singleton et al. 2003). The lifetime prevalence rate of mental illness in Canada is 20% (Canadian Mental Health Association 2012)

3.4 Empirical Strategy

The objective of this study is to estimate the long-term impacts of prenatal exposure to severe typhoons on mental health. In this section, I first examine the relationship between the *in utero* exposure and mental illness later in life using a descriptive graph. In the second part, I discuss the empirical specification used to uncover the causal impacts of severe typhoons.

3.4.1 Motivation

Figure 3.2 shows the prevalence of adult mental illness by birth cohort and by *in utero* exposure to severe typhoon status. The dotted lines reflect the timing of severe typhoons. Prevalence of mental illness is aggregated to year-month of birth level and separated by *in utero* exposure status. The figure suggests that individuals who resided in landfall counties and were born immediately after a severe typhoon seem to have higher rates of mental illness compared to individuals who were not exposed to severe typhoons while *in utero*. Overall, the figure points out a persistent relationship between prenatal exposure to severe typhoons and worse mental health. In the rest of the section, I outline the empirical strategy that is used to rigorously examine the casual impact of *in utero* exposure to severe typhoon on mental well-being.

[Insert Figure 3.2 about here]

3.4.2 Empirical Specification

In this study, I exploit two sources of variation, geographical variation and the timing of births. An interrupted time series design is applied to examine the effects of severe

typhoons. This approach is used to identify treatment effects by examining the patterns of outcomes in pretreatment and posttreatment periods. A discontinuity in the patterns could suggest the existence of treatment effects, as nature tends to be smooth. Thus, a deviation from the smooth trend that is sharply timed after the typhoon's landfall and especially pronounced in counties hardest hit by the typhoon would be interpreted as an effect of the typhoon. I compare the mental health of individuals who were exposed to severe typhoons while *in utero* in landfall counties and non-landfall counties to those who had no fetal exposure to severe typhoons, respectively. *In utero* period is defined as 9 months (252 days) prior to the date of birth.[16] Those individuals who were not exposed to severe typhoons while *in utero* are the comparison group, and this group consists of cohorts who were older and younger than those in the treatment groups. The key assumption of this approach is that the trends of the incidence of mental disorders would be similar in exposed and unexposed individuals had severe typhoons not occurred. Since severe typhoons are not predictable, they serve as natural experiments to examine the impacts of extreme events.[17] In a later section, I will show supporting evidence for this assumption. The effects of severe typhoons are modeled at the individual level.

3.4.2.1 Main Strategy

I first examine the difference in mental disorders and the use of psychiatric drugs. For individual i born in year t and month m residing in county c, the primary specification is

[16]The results are similar when I use 8 months and 10 months prior as alternate definitions for *in utero* period

[17]Although a typhoon typically takes place between April and November, the timing and landfall location of a severe typhoon is random. During 1958 to 1970, typhoons made landfall in five out of 22 counties.

as follows:

$$
\begin{aligned}
Y_{icmt} = \alpha &+ \beta_1 * I(AnyInUteroExposuretoSevereTyphoon)_{mt} * I(LandfallCounty)_{cmt} \\
&+ \beta_2 * I(AnyInUteroExposuretoSevereTyphoon)_{mt} * I(NonlandfallCounty)_{cmt} \\
&+ \delta * Male_i + \delta_t + \zeta_m + \eta_c + u_{icmt}
\end{aligned}
\tag{3.1}
$$

I include county-, birth year-, and birth month-fixed effects to control regional time-invariant determinants and a time effect that is common across counties. I(Any In Utero Exposure to Severe Typhoon) is an indicator variable, 1 if individual i was exposed to a severe typhoon while *in utero*. I(Landfall County) and I(Nonlandfall County) are indicator variables, if individual i resides in the landfall and non-landfall county for the given severe typhoon respectively. The estimate β_1 captures any differences in the prevalence of mental disorders between individuals who were exposed to a severe typhoon while *in utero* and residing in a landfall county and individuals who had no *in utero* exposure to severe typhoon from the trends. Similarly, the estimate β_2 captures the differences in the prevalence of mental disorders between individuals who were exposed to a severe typhoon while *in utero* and residing in a non-landfall county, and individuals who had no *in utero* exposure to a severe typhoon. β_1 and β_2 are interpreted as the total effects of a severe typhoon in landfall and non-landfall counties, respectively. Standard errors are clustered at the county level to allow for the possibility of within-county correlation.

I propose two predictions related to the estimates of interests, β_1 and β_2. First, based on early childhood literature, we would expect that a poor *in utero* environment may deteriorate mental health later in life. Thus, if a severe typhoon leads to worse adult mental health, then we would expect to find $\beta_1 > 0$. Second, major destruction of severe typhoons is more likely to fall in landfall counties in general. If severe typhoon insults are

greater in the landfall county, meaning that individuals residing in the landfall county are most affected by severe typhoons, we would expect to see β_1 - $\beta_2 > 0$.

A potential concern is that certain counties are more likely to get typhoons; however, different counties may have different trends in mental health outcomes, such that the estimated β_1 and β_2 reflect not only the effect of the typhoon but also the trend differences. I take several approaches to address this concern. First, I estimate another specification which is Equation 3.1, with the addition of county-specific birth cohort trends. Second, in the robustness section, I present results controlling for region-specific cohort effects (in the analytical sample, Taiwan as 15 counties divided into five regions), which controls very flexibly for differences in cohorts across regions. Finally, if one were still concerned that within a region, different counties have rather different cohort effects, making it difficult to disentangle the effect of typhoon exposure from district-specific cohort effects, then it is possible to get a difference-in-differences estimate of the effect of greater exposure to severe typhoon as follows. β_1 gives the effect of high exposure to a severe typhoon but is potentially confounded by a district-specific cohort effect, and β_2 gives the effect of low exposure to a severe typhoon but is potentially confounded by a district-specific cohort effect. By taking the difference between β_1 and β_2 the district-specific cohort effect is removed, and so regardless of whether district-specific cohort effects are present, the difference represents the effect of greater exposure to a severe typhoon.

3.4.2.2 Special Considerations for Discrete and Continuous Outcomes (Number of Visits and Expenditures)

In order to see whether severe typhoons affect other aspects of health care utilization, I also examine the effects on the number of psychiatric-related medical visits and health care expenditures. Two common features of these outcomes are the existence of many zeros and

skewed data. Thus, although I use the same empirical strategy for identifying the causal effect of typhoon exposure as discussed above, some additional comments must be made on how to deal with outcome measures of this type.

The nature of a count variable such as number of visits is that it consists of non-negative integer values and mostly takes on a few values. Since the normality assumption is not suitable for this type of data, more appropriate approaches to estimate the effects are the Poisson and Negative Binomial regression models. Although the Poisson quasi-maximum likelihood estimation allows for cases when the data generating process is misspecified, the Poisson variance assumption is required. When there is overdispersion (variance is greater than the mean) or underdispersion (the mean is greater than the variance), a common practice is to adopt the Negative Binomial regression model. I will show the estimation results from both models.

A conventional approach to deal with the issues of expenditure data is to apply log transformation and assume some positive values to all observations, typically adding one to all observations (to avoid dropping the observations with an observed value of zero). However, variance of the data will be distorted after the conventional transformation. I overcome these problems by employing the inverse hyperbolic sine transformation method. The method was first introduced by Johnson (1949), and has been used in recent literature in household wealth and saving, trade, and parental transfer data (Chen 2013; Gelber 2011; Hochguertel and Ohlsson 2009; Pence 2006; Rotunno, Vezina, and Wang 2013).[18] One particular advantage of the method is that it can be used to estimate a specification of percentage change without distorting the data since the inverse hyperbolic sine function is defined at zero. Figure 3.3 displays the histograms of health care expenditures before

[18]See Burbidge, Magee, and Robb (1988) for a discussion on the advantage of inverse hyperbolic sine transformation.

and after applying the inverse hyperbolic sine transformation method. The specification is of the following form:

$$
\begin{aligned}
Log(Y_{icmt} + (Y_{icmt}^2 + 1)^{0.5}) = \alpha \\
&+ \beta_1 * IAnyInUteroExposuretoSevereTyphoon)_{mt} * I(LandfallCounty)_{cmt} \\
&+ \beta_2 * I(AnyInUteroExposuretoSevereTyphoon)_{mt} * I(NonlandfallCounty)_{cmt} \\
&+ \delta * Male_i + \delta_t + \zeta_m + \eta_c + u_{icmt}
\end{aligned}
\tag{3.2}
$$

Y_{icmt} represents the psychiatric-related expenditures for individual i born in year t and month m residing in county c. The estimate β_1 is interpreted as individuals who had prenatal exposure to severe typhoons are likely to have (100*β_1)% more psychiatric-related expenditures as compared to individuals who had no prenatal exposure.[19] In addition, I also estimate the effects of severe typhoons using the conventional log transformation approach and a non-linear tobit regression model.

[Insert Figure 3.3 about here]

3.4.2.3 Event Study (Timing of Exposure)

In addition, I conduct an event study specification to examine the impact of exposure to severe typhoons at various ages, specifically from three years before birth to three years after birth. By estimating age-specific effects of severe typhoon exposure, we can more immediately see whether the pattern of mental illness is indeed smooth, and if the

[19]The interpretation of the coefficient is similar to standard log transformation since the transformed dependent variable is approximately $log(2y)$ or $log(2) + log(y)$ with exceptions for small values (Chen, 2013).

deviation from the smooth trend is indeed sharply timed after the typhoon incident. The specification is as follows:

$$
\begin{aligned}
Y_{icmt} = & \alpha \\
& + \sum_{x=-2}^{3} \beta_{1x} * I(AnyExposuretoSevereTyphoonatAgeX)_{mt} * I(LandfallCounty)_{cmt} \\
& + \sum_{x=-2}^{3} \beta_{2x} * I(AnyExposuretoSevereTyphoonatAgeX)_{mt} * I(NonlandfallCounty)_{cmt} \\
& + \delta * Male_i + \delta_t + \zeta_m + \eta_c + u_{icmt}
\end{aligned}
\tag{3.3}
$$

The estimate β_{1x} captures the difference in mental illness in individuals who were exposed to severe typhoons at age x to individuals who were not exposed to severe typhoons at age x; x ranges from -2 to 3. The reference group consists of individuals who had no exposure to severe typhoons between two years before birth to three years after birth. These individuals include birth cohorts that are older or younger than the treatment groups.

3.5 Results

3.5.1 Mental Illness and Use of Psychiatric Drugs

The regression results for Equation 3.1 are shown in Table 3.2. I find that the likelihood of being diagnosed with mental illness within the five years for those who were exposed to severe typhoons while *in utero* in landfall county increases 2 percentage points (roughly an 11% increase) relative to individuals who did not have any prenatal exposure to severe

typhoons.[20] After controlling for county-specific cohort trends, the estimation results remain similar. Mental illness is a broad term that includes psychoses, organic psychotic conditions, other psychoses, neurotic disorders, personality disorders, and other nonpsychotic mental disorders. In Columns 3–8 of Panel A, I break down mental illness into the three most common mental disorders: anxiety disorders, mood disorders, and schizophrenia. The results show an increase in the prevalence of anxiety and mood disorders, and schizophrenia increases by 0.4–1.9 percentage points as a result of prenatal exposure to a severe typhoon.

I further investigate the likelihood of psychiatric drug use in Panel B of Table 3.2. Some literature suggests that off-label use of psychiatric drugs is common (Chien et al. 2007; Wittich, Burkle, and Lanier 2012). In order to avoid off-label prescribing of psychiatric drugs, I identify psychiatric drug use *only if* any mental illness was diagnosed in the associated visits. Individuals with *in utero* exposure to severe typhoons in landfall counties are more likely to use any psychiatric drugs as compared to unexposed individuals, an increase of 1.8 percentage points.[21] The results also indicate that the effects on psychiatric drugs are concentrated on antidepressants, a 50% increase in the likelihood of using antidepressants. The impacts on use of anxiety and psychosis drugs are positive although the estimates are statistically insignificant. In the robustness section, I will show results of specifications, which include region-by-year fixed effects, and also present results using a non-linear logistic regression model.[22]

These estimation results support the aforementioned hypotheses (see section 3.4.2). *In*

[20] Adhvaryu, Fenske, and Nyshadham (2014) find the likelihood of mental severe distress reduced by 3 percentage points (50% of the mean) resulting from one standard deviation increase in cocoa price.

[21] Persson and Rossin-Slater (2014) find a 0.7 percentage point increase (or 7-11% of the mean) in the use of anxiety and depression drugs resulting from prenatal bereavement.

[22] The effects of main outcomes remain statistical significant at the conventional level when I employ wild bootstrap techniques proposed by Cameron, Gelbach, and Miller (2008) and Webb (2014) (results are available upon request).

utero exposure to severe typhoons results in worse adult mental health. Moreover, the impacts of severe typhoons are indeed more pronounced for exposed individuals of landfall counties relative to those residing in non-landfall counties. These results suggest that the main findings cannot simply be explained by unobserved secular changes between birth cohorts.

[Insert Table 3.2 about here]

3.5.2 Psychiatric-Related Health Care Utilization and Expenditures

I have shown the effects of severe typhoon on the incidence of mental illness. It is of interest to see if *in utero* exposure to severe typhoons also increases psychiatric-related visits and associated medical expenditures. Table 3.3 presents the estimation results from Poisson and Negative Binomial regression models. Although the Poisson variance assumption is not supported by the data, the estimation results from Poisson and Negative Binomial regression models are similar. The results show that prenatal exposure to severe typhoons in a landfall county is likely to increase psychiatric-related outpatient visits by 35% (=exp(0.30)-1). I also find that prenatal exposure to severe typhoon in landfall counties increases the likelihood of using inpatient care in a psychiatry department although the effect is not statistically significant.[23]

[Insert Table 3.3 about here]

Additionally, I examine the effects on psychiatric-related health care expenditures in

[23]Table A2 presents the estimation results on psychiatric hospitalization. Similar to the results on inpatient utilization, I find little effects on psychiatric hospitalization.

Table 3.4. Consistent with the results in Table 3.3, I find that fetal exposure to severe typhoons increases the medical expenditures on psychiatric-related visits. Columns 1 and 4 show the results of Equation 3.2, in which inverse hyperbolic transformation is applied to the outcomes. The results suggest that individuals who were exposed to severe typhoons in landfall counties are likely to spend 12% more on psychiatric-related outpatient care. I also find a positive insignificant effect on psychiatric-related inpatient care. It is likely that very few people had been admitted to psychiatry units, thus I may not have enough statistical power.[24] The results using two log transformation methods are very comparable. Considering that the OLS estimates are interpreted as the intent-to-treat estimate, the treatment-on-the-treated estimate would suggest an even larger effect on psychiatric-related health care expenditures. The magnitude of the effects on out-of-pocket expenditures is much smaller than that of total expenditures although the effects maintain statistical significance. This is likely due to the fact that the universal health care insurance in Taiwan is a single-payer system: the government can be fairly generous, so out-of-pocket expenditures tend to be much lower compared to those in a multi-payer system. Also the universal health care insurance granted waivers to out-of-pocket expenditures for patients with catastrophic illnesses (including some severe mental illness).[25]

Alternatively, I employ Tobit regression to model the causal effects. Columns 3 and 6 of Table 3.4 present the average partial effect (APE) from a Tobit regression model. The Tobit estimates generally have the same sign as that of OLS estimates; however, they are not directly comparable. Tobit estimates also imply much larger effects in terms of magnitude. The Tobit APE would suggest that *in utero* exposure to a severe typhoon is likely to increase psychiatric-related health care expenditures by 107 USD.

[24]The result on inpatient care is consistent with Abel et al. (2014) and Class et al. (2013) which they also find little effects based on hospital admission data.

[25]Waivers are exclusive to the specific catastrophic illnesses.

[Insert Table 3.4 about here]

3.5.3 Robustness of the Results

Table A1 presents the estimation results of Equation 3.1 using a non-linear logistic model. The results show that the marginal effects based on a non-linear logistic model is similar to the baseline results. Thus, biases associated with a linear probability model are negligible. For the purpose of easier interpretation, my preferred specification is the linear probability model with county-specific cohort trends.

For the sake of simplicity, the rest of this section presents the robustness results on the main outcome, the prevalence of mental illness. In the Appendix Tables A3–A6, I present the same series of robustness checks for other outcomes. To see whether the baseline results are sensitive to the definition of severity, I use alternative proxies of severity of typhoons, and the results are presented in Table 3.5. Columns 2 and 3 of Table 3.5 consider different cutoffs of death tolls, and columns 4 and 5 use numbers of collapsed buildings. The number of severe typhoons incidents vary under each alternative measure, ranging from four to eight. The results are robust to various definitions of severe typhoons. Interestingly, the results seem to suggest the magnitudes of effects are closely related to the degrees of severity.

[Insert Table 3.5 about here]

Further, Table 3.6 examines the effects on various subgroups to verify that the impacts are not due to different characteristics between exposed and unexposed birth cohorts. First

Taiwan is an island and a typhoon is unlikely to make landfall in the north and central regions due to geographical features. Thus, Column 2 excludes counties that did not experience landfall of a severe typhoon between 1958 and 1970. The results are consistent with the baseline results. Second, Figure 3.1 shows that since most of the severe typhoons occurred in earlier years, it appears that those who were exposed to severe typhoons are older than those who are not affected by severe typhoons. Thus, in Column 3, I exclude individuals who were born after 1966 to confirm that the impacts are not resulting from a slightly older treatment group. As compared to the baseline results, the effect is larger. It could be the case that some mental illness has not been diagnosed yet among the younger cohorts relative to older cohorts, although the majority of mental illness is likely to be diagnosed by the individual's mid-twenties. As mentioned in section 3, there were a few severe typhoons that did not make landfall. In column 4, I estimate the effect excluding those individuals who were exposed to the no-landfall severe typhoons. The estimation results remain comparable to the baseline.

To even further reduce the migration issue, instead of relying on administrative division, column 5 identifies rural townships based on migration rate as of the 2000 census. I exclude counties with migration rates are greater than 20%. Column 6 restricts the sample to farmers/fishermen since I find that the migration in and out of county among farmers and fishermen is relatively low, around 85% based on census data from 2000. It is notable that the coefficients become larger than my baseline estimate when I restrict the sample to those with little migration.

Additionally, columns 7 and 8 of Table 3.6 include region-by-year fixed effects to further control trends differences in mental health outcomes. The results are consistent with the main results. Column 8 adds further control for average temperature and rainfall while *in*

utero, respectively.[26] The results are consistent with the main results. Column 9 addresses concerns of within-year-of-birth correlation by applying the multiway clustering approach proposed by Cameron, Gelbach, and Miller (2011). There is no difference between standard errors obtained from this approach and the main specification. Furthermore, I examine whether the negative effects of severe typhoons were driven by a particular episode of a severe typhoon event. Table A7 compares the results, which exclude individuals who were exposed to severe typhoon events one by one to the baseline result. The estimation results do not support the claim that one event dominates the impacts.

[Insert Table 3.6 about here]

As a placebo test, I conduct a permutation test in which the timing of severe typhoon and landfall counties are randomly drawn without replacement. For each permutation, the timing and landfall location of severe typhoons are randomly chosen. Individuals' prenatal exposures are then assigned accordingly.[27] I then estimate the effects of severe typhoons based on placebo exposure status. Figure 3.4 displays the empirical distributions of the placebo treatment effects on outpatient psychiatric-related visits from 1,000 permutation tests. The fact that the distribution is centered at zero is comforting as these placebo tests are expected to find no impacts. When I compare the treatment effects that are based on actual exposure, the results indicate that less than 1% of the time permutation estimates are larger than the estimates of actual treatment. This result based on permutation tests reassures that the effect of severe typhoon is statistically significant.

[26] Rainfall and temperature data were obtained from the Data Bank for Atmospheric Research of the Taiwan Typhoon and Flood Research Institute.

[27] Permutation tests have been used recently in the following papers: Agarwal et al. (2015), Bloom et al. (2013), Chetty et al. (2011).

[Insert Figure 3.4 about here]

3.5.4 Heterogeneous Effects by Gender

I next probe whether the effects are different by gender. Table 3.7 presents the estimation results of Equation 3.1 and 3.2 by gender. Column 2 and column 4 show the estimates of β_1 for mental health outcomes for males and females, respectively. Overall, the results indicate that prenatal exposure to a severe typhoon has a larger impact on women. Yet the estimates for males on anxiety and mood disorders, schizophrenia, antidepressant use, and number of psychiatric-related visits are not statistically different from that of females.[28] There are a few possible explanations for why I find larger effects for females. First, I cannot rule out the possibility that women and men may have different health care seeking behaviors. For example, both males and females experience the same depression symptoms but females may be more likely to report it to physicians. It is evident from the baseline differences in means. Additionally, early childhood literature suggests that weaker fetuses could be selected out during pregnancy in the presence of adverse events (Bhalotra, Valente, and van Soest 2010; Bozzoli, Deaton, and Quintana-Domeque 2009; Gorgens, Meng, and Vaithianathan 2012; Liu, Liu, and Tseng 2015). To examine whether there is positive mortality selection, I investigate the effects of prenatal exposure to severe typhoons on cohort male-to-female ratio. Using census data from 1980, I find that exposure to severe typhoons during the early stage of pregnancy has a negative while statistically insignificant effect on sex ratio. The results suggest that fetal exposure to severe typhoons is likely to increase mortality selection; therefore, on average men could be healthier than women

[28] Dinkelman (2015) uses South African census data and finds that drought exposure affects men more than women.

among the survivors. Lastly, with the same *in utero* shock, the negative effects on females may be reinforced under the practice of son preference.[29]

[Insert Table 3.7 about here]

3.5.5 Physical Health Outcomes

Although this study focuses on mental health, it is likely that physical health deteriorates as a result of *in utero* exposure to a severe typhoon. Table 3.8 examines various aspects of physical health including whether the individual has ever been diagnosed with coronary heart diseases, hypertension, diabetes, stroke, and cancer. The results show that *in utero* exposure to a severe typhoon slightly increases the incidence of stroke by 0.7 percentage points for men. In contrast, I find very little effects on women's physical health. It is noteworthy that these individuals were of the ages between 28 and 43. It is possible that these individuals are relatively young to detect the onset of these chronic physical illnesses. In general, I do not find that prenatal exposure to severe typhoons damages physical health. Nonetheless, there is a possibility of negative impacts on physical health during older age.[30]

[Insert Table 3.8 about here]

3.5.6 Timing of Exposure

Although this study focuses on poor intrauterine environment, it is reasonable to ask whether exposures to severe typhoons at other ages also affects one's mental health later in

[29] In the context of Asian countries, some studies find women are more sensitive to environmental shocks such as Maccini and Yang (2009) and Pathania (2007).

[30] Persson and Rossin-Slater (2014) find little effects on the use of prescription drugs that target cardiovascular diseases, diabetes, and hypertension while they find that prenatal bereavement leads to worse birth outcomes.

life. Since the main finding is concentrated on women, I explore the effects of the timing of exposure to severe typhoons among women. Since three out of five severe typhoons occurred in consecutive years of the earlier period, this part of the analysis focuses on women who were born between 1964 and 1970 to avoid overlapping of severe typhoon exposure at different ages. Figure 3.5 presents the estimation results of 3.3. First, the results provide supporting evidence on placebo tests in which I find no effects on the exposure before the possible timing of *in utero* period —two years before birth. According to historical newspapers, reconstruction after the five severe typhoon incidents lasted no more than one year. Thus, individuals who were born two years after a severe typhoon were unlikely to have been exposed to the aftermath of a severe typhoon. Second, although exposure to a severe typhoon at age one has some positive yet statistically insignificant effects on mental illness, exposure in the fetal life has the most striking impacts as compared to exposure during the first few years of life.[31]

[Insert Figure 3.5 about here]

3.6 Discussion

During the 1950s, the infant mortality rate was around 40 per 1,000 live births in Taiwan. It is similar to some developing countries such as India, Myanmar, and Rwanda as of 2013. There are many possible reasons why *in utero* exposure to a severe typhoon can cause poor outcomes. A severe typhoon can lead to worse sanitation environment, lack of access to

[31] I also conduct an event study which uses finer bins, specifically born 1, 2, 3, and 4 quarters before and after the severe typhoons. Individuals who were born within two quarters after a severe typhoon had higher likelihood of mental illness, whereas the effects of exposed to severe typhoons at the ages of 1-12 months were relatively small and statistically insignificant.

health care, household income shocks, worse parental health, maternal stress, and disruption of nutritional intake. The remaining part of this section discusses some of the potential channels for why severe typhoons could cause mental well-being to deteriorate: personal hygiene and sanitation, education, and employment. Suppose that severe typhoons result in a worse sanitation environment. The lack of access to clean water may facilitate the spread of communicable diseases. Many studies have provided evidence on the long-term effects of fetal exposure to communicable diseases (Barreca 2010; Bleakley 2007; Case and Paxson 2008). Thus, I investigate whether communicable diseases can explain the negative effects of severe typhoons. Ideally I would like to have information on whether the individuals' mothers contracted any communicable diseases during pregnancy; however, communicable disease data is only available at aggregated county and year level between 1958 and 1965.[32] Table 3.9 presents the estimation results of a specification controlling for the communicable disease rate. The results are the same as baseline results, therefore suggesting that the main channel of severe typhoons may not be communicable diseases.

[Insert Table 3.9 about here]

Furthermore, it is possible that prenatal exposure to severe typhoons leads to lower educational attainments and a higher probability of unemployment and, in turn, worsens mental health. I tried to use census data from 2000 to examine the impacts of severe typhoons on these outcomes. I found no effects on employment and a statistically insignificant effect on education attainments. Since census data from 2000 provides individuals' ages instead of year and month of birth, and age heaping is a known issue of census data,

[32] Communicable disease rate were obtained from annual health statistical abstract of the years between 1958 and 1965. The rate was mapped to individuals by county and year-month of birth.

the results could be partly affected by measurement errors of birth timing. Finally, I could not rule out other possible mechanisms such as maternal stress, nutritional intake, and income shocks. However, the event study results seem to suggest that the mechanisms must be very specific to intrauterine environment given that *in utero* exposure has the largest impact.

3.7 Conclusion

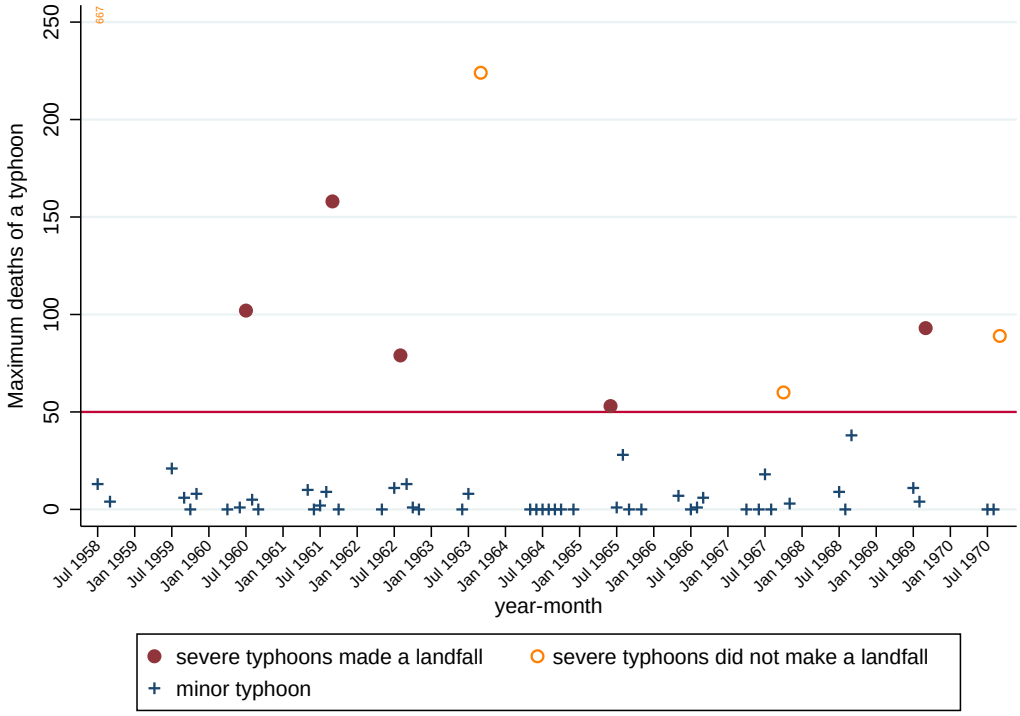
This study examines the extent to which natural disasters negatively affect mental health later in life. I exploit time and regional variation of severe typhoons in Taiwan to estimate the effects of intrauterine exposure to severe typhoons. I find that the likelihood of mental illness increases by 11% for individuals who had *in utero* exposure to severe typhoons in landfall counties as compared to their unexposed peers. These individuals also tend to have more psychiatric-related health care utilization. Compared to the effects of exposure in the first few years of life, I find that *in utero* exposure has the greatest impacts on mental health. The negative effects on mental health are largest for women.

There are several possible channels for why severe typhoon could lead to worse psychological well-being, including maternal stress, typhoon-related income shocks, maternal malnutrition, etc. Although my result does not support communicable diseases as one of the main channels, my approach does not allow me to fully separate the effects from these potential channels. Hence, I would cautiously interpret my findings as reduced form effects of poor *in utero* environment.

This study complements the existing literature to present a broader understating of how poor intrauterine environment can have long-lasting effects on mental health. My results suggest that, despite the massive economic disruption caused by natural disasters, there

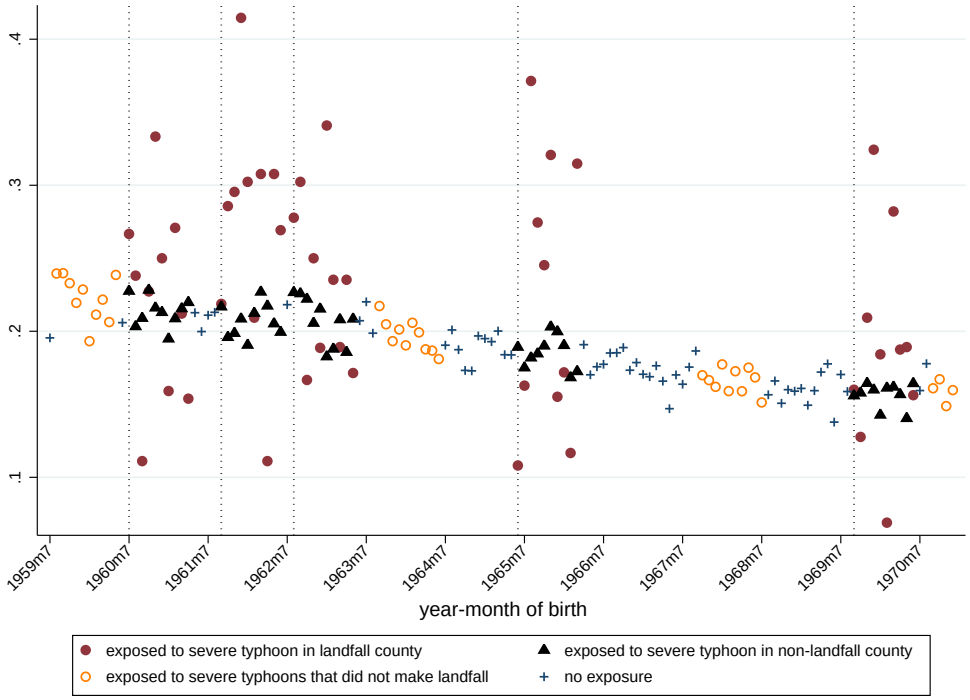
could be consequences that are not immediately noticeable. Given the tremendous costs that are associated with mental disorders, welfare may be largely improved by providing timely prevention services to affected pregnant women.

Figure 3.1: Death Tolls from Typhoons by Year-Month, 1958–1970



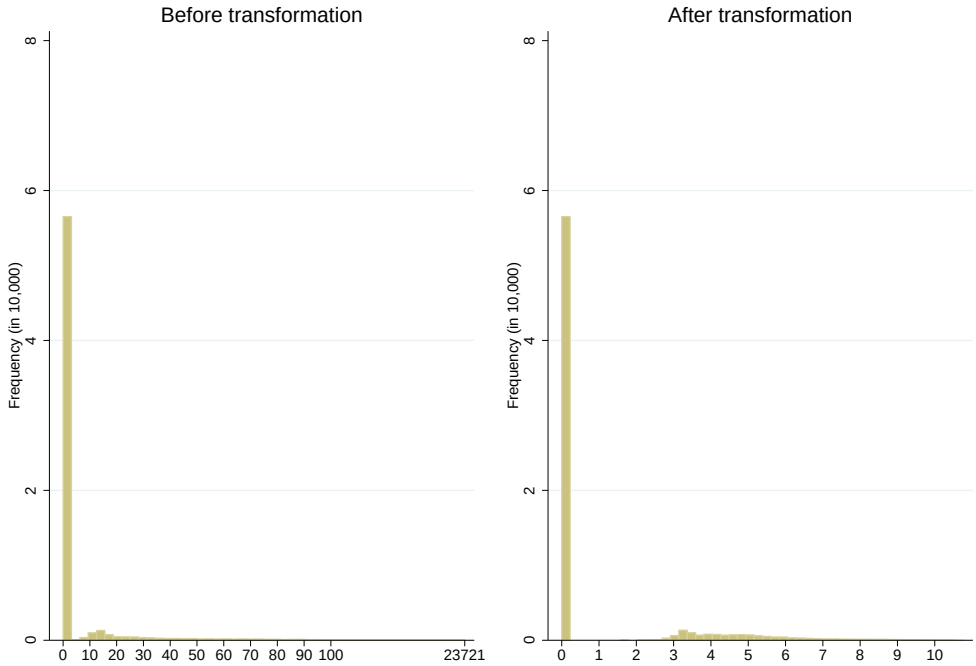
Notes: Data Source: The Typhoon Database of the Central Weather Bureau and the 2013 Annual Disaster Report from the National Fire Agency. Each point represents the number of deaths caused by a given typhoon at the national level. Solid circles indicate typhoons that made landfall and caused more than 50 deaths. Hollow circles show the typhoons that did not make landfall and caused more than 50 deaths. Pluses refer to the typhoons that caused less than 50 deaths. In the main analysis, a severe typhoon is defined as one causing more than 50 deaths. Y-axis represents the deaths toll for a given typhoon. X-axis indicates year and month.

Figure 3.2: The Likelihood of Mental Illness by Intrauterine Exposure to Severe Typhoon



Notes: Data source: 5% Health Insurance Claim Records, 1998–2002. Mental illness is measured as ever been diagnosed with mental disorders based on ICD-9 codes (physician diagnosis). Likelihood of mental illness is aggregated to year-month of birth and *in utero* exposure to severe typhoons status. Severe typhoon is defined as a typhoon that caused 50 deaths. Each point represents a given birth cohort (at the year-month level) and its exposure to severe typhoon. The dotted lines show when severe typhoons made landfall. Solid circles refer to the cohorts that had fetal exposure to severe typhoons in landfall county. Triangles show the cohorts that had fetal exposure to severe typhoons in non-landfall county. Hollow circles indicate the cohorts that had fetal exposure to severe typhoons that did not made landfall. Pluses show the cohorts that had no fetal exposure to severe typhoons. Y-axis represents the share of mental illness for a given birth cohort. X-axis indicates year-month of birth.

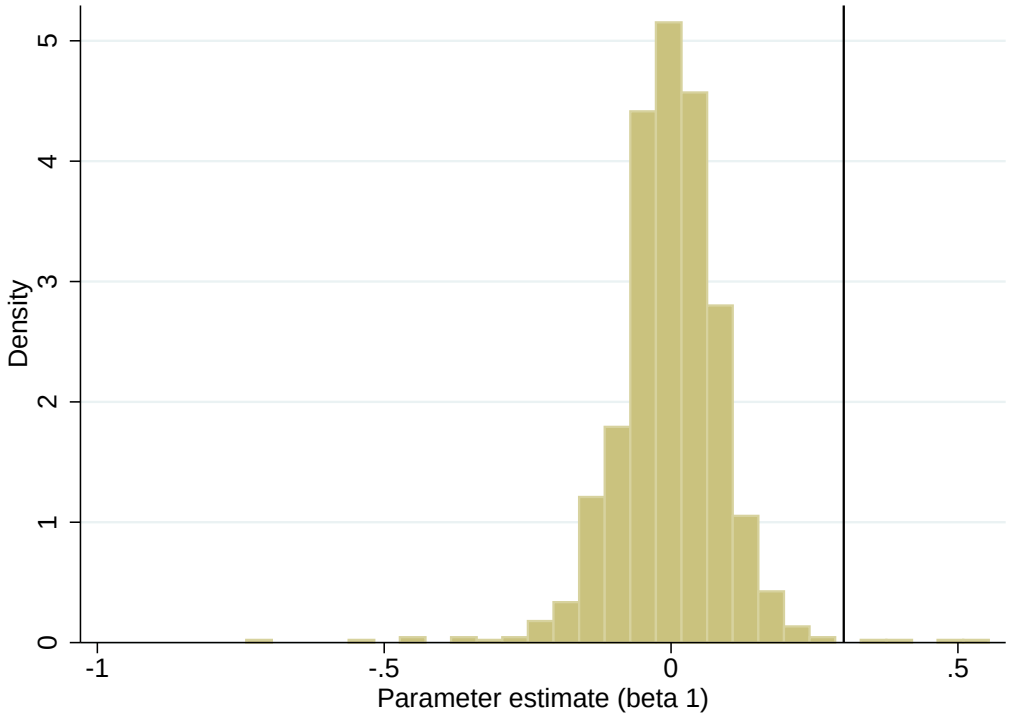
Figure 3.3: Histograms of Psychiatric-Related Outpatient Health Care Expenditures (in 2011 USD)



Notes: Data source: 5% Health Insurance Claim Records, 1998–2002. The histograms on the left and right display respectively psychiatric-related expenditures before and after applying inverse hyperbolic sine transformation method ($log(y + (y^2 + 1)^{0.5})$). Expenditures are inflation-adjusted and in 2011 USD. Y-axis represents the frequency of a given amount of expenditures. X-axis indicates total psychiatric-related outpatient expenditures over the five years.

Figure 3.4: Permutation Test Result for Outpatient Psychiatric-Related Visits

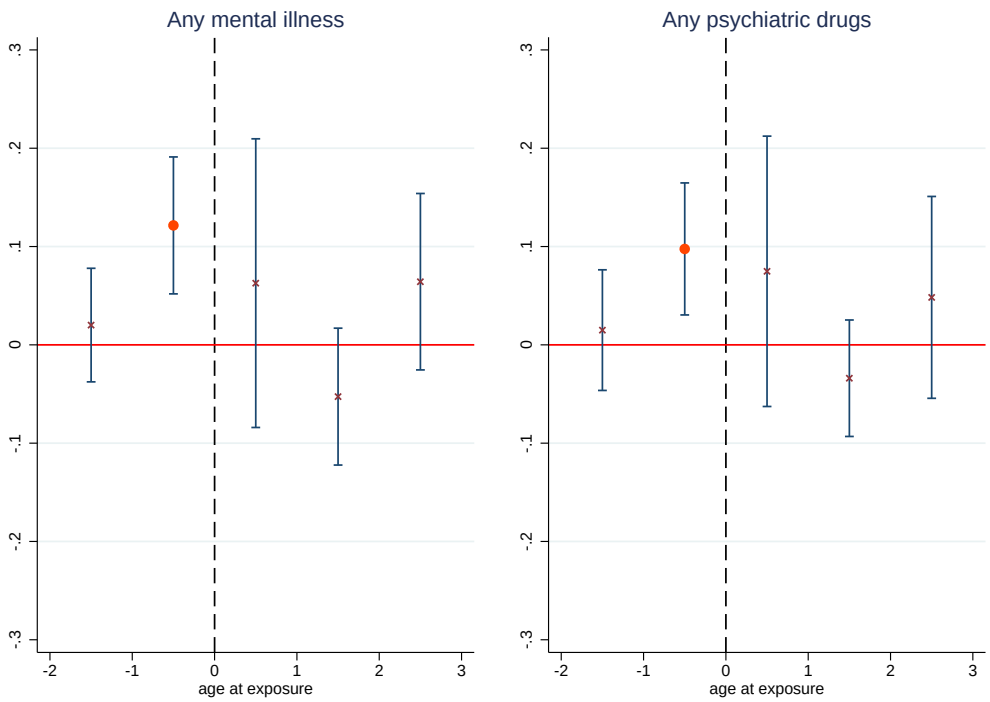
Coefficient of (Exposed)*(Landfall County), β_1



Notes: I assigned placebo treatment (prenatal exposure to severe typhoons) in randomly selected year-month and county drawn without replacement. The histogram displays the coefficient estimates of an interaction term between *in utero* exposure to severe typhoon and landfall county from 1,000 permutations. The vertical line shows the estimates of the actual prenatal exposure. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Omitted group is individuals who were not exposed to a severe typhoon while *in utero*. The results show that 4 out of 1,000 permutation estimates are greater than that of actual treatment.

Figure 3.5: Impacts of Exposure to Severe Typhoon by Birth Cohort

Coefficient of (Exposed)*(Landfall County), β_{1x}



Notes: Sample is comprised of female individuals who were born between 1964 and 1970. Regression estimates of Equation 3.3 from linear probability models are plotted. The dots, crosses, and bars correspond to the coefficient estimates with 95% confidence intervals. The estimate illustrates the difference in outcome variables between those individuals who were exposed to severe typhoons at age x relative to individuals who did not exposed to severe typhoons at age x. The dot represents the differences in outcomes between individuals who were exposed to severe typhoons and individuals who were not exposed to severe typhoons within one year before birth, which covers *in utero* period. Omitted group is individuals who did not have severe typhoon exposure between two years before and three years after birth. The covariates include year of birth fixed effects, month of birth fixed effects, county fixed effects, county-specific cohort trends, and a set of interaction terms between non-landfall county and exposure at age x to severe typhoon (see Equation 3.3). Exposure to severe typhoons is a dummy variable, which equals 1 if one was at age x during a severe typhoon. Severe typhoon is defined as a typhoon that cause 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. X-axis measures age at exposure.

Table 3.1: Descriptive Statistics, Health Insurance Claim Records 1998–2002

	All	w/ in-utero exposure		w/o in utero exposure
		landfall county	non-landfall county	
	(1)	(2)	(3)	(4)
Individual characteristics				
Age	37.5	38.0	38.0	37.1
	(3.42)	(3.17)	(3.55)	(3.23)
Male	0.50	0.51	0.50	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Farmer/fisherman	0.23	0.31	0.23	0.23
	(0.42)	(0.46)	(0.42)	(0.42)
Low income	0.0055	0.011	0.0052	0.0056
	(0.074)	(0.11)	(0.072)	(0.075)
Health Outcomes				
Ever had any mental disorders	0.20	0.25	0.21	0.20
	(0.40)	(0.43)	(0.41)	(0.40)
Ever been prescribed psychiatric drugs	0.16	0.21	0.17	0.16
	(0.37)	(0.40)	(0.38)	(0.36)
Ever been hospitalized in psychiatry	0.007	0.016	0.006	0.007
	(0.082)	(0.13)	(0.078)	(0.084)
Health care utilization				
Total psychiatric-related outpatient visits	1.58	2.66	1.56	1.58
	(8.29)	(11.6)	(7.64)	(8.78)
Total bed-days in psychiatry	1.66	4.52	1.72	1.52
	(35.6)	(53.4)	(38.2)	(32.1)
Total psychiatric-related outpatient expenditures	61.3	115.9	58.8	62.1
	(460.1)	(679.5)	(431.7)	(478.5)
Total inpatient expenditures in psychiatry	68.9	158.5	67.4	67.6
	(1287.6)	(1703.1)	(1329.6)	(1229.2)
N	69,549	1,056	34,047	34,446

Note: Data source: 5% Health Insurance Claim Records, 1998–2002. Unit of observation is individual. Analytical sample includes individuals who were born between 1959 and 1970 and currently reside in rural townships. Individual characteristics are observed in 2002, and health outcomes are aggregated across 1998–2002 based on claim records. Standard deviation are reported in parentheses.

Table 3.2: Impact of Intrauterine Exposure to Severe Typhoon on Mental Health

Panel A	Ever been diagnosed with							
	Any mental disorders (mean=0.20)		Anxiety and personality disorders (mean=0.15)		Mood disorders (mean=0.04)		Schizophrenia (mean=0.01)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(exposed to severe typhoon)*(landfall county), β1	0.021**	0.024***	0.018*	0.019**	0.015**	0.017***	0.005**	0.004***
	(0.010)	(0.009)	(0.010)	(0.008)	(0.006)	(0.006)	(0.002)	(0.001)
(exposed to severe typhoon)*(non-landfall county), β2	0.005	0.005	0.005*	0.005*	0.004**	0.004**	-0.001*	-0.001*
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
P-value of H0: β1-β2=0	0.116	0.030	0.226	0.069	0.075	0.026	0.002	0.001
County-specific cohort trend	No	Yes	No	Yes	No	Yes	No	Yes
Panel B	Ever been prescribed							
	Any psychiatric drugs (mean=0.16)		Antidepressants (mean=0.05)		Anxiolytics (mean=0.15)		Antipsychotics (mean=0.04)	
(exposed to severe typhoon)*(landfall county), β1	0.016	0.018*	0.026***	0.026***	0.012	0.013	0.005	0.004
	(0.011)	(0.010)	(0.009)	(0.009)	(0.012)	(0.011)	(0.008)	(0.008)
(exposed to severe typhoon)*(non-landfall county), β2	0.008**	0.008**	0.004**	0.004**	0.007*	0.006*	-0.002	-0.002
	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)	(0.001)	(0.001)
P-value of H0: β1-β2=0	0.455	0.348	0.018	0.020	0.677	0.597	0.349	0.423
County-specific cohort trend	No	Yes	No	Yes	No	Yes	No	Yes

Note: Sample is as described in Table 3.1. This table presents the results of estimating 3.1 from linear probability models. Models also control for an indicator for male, year of birth FE, month of birth FE, and county FE. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are clustered at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 3.3: Impact of Intrauterine Exposure to Severe Typhoon on on Psychiatric-Related Health Care Utilization

	Number of psychiatric-related visits (mean=1.58)		Number of bed-days in psychiatry (mean=1.66)	
	Poisson FE	Negative Binomial FE	Poisson FE	Negative Binomial FE
	(1)	(2)	(3)	(4)
(exposed to severe typhoon)*(landfall county), β1	0.301***	0.303**	0.362	-0.030
	(0.101)	(0.128)	(0.508)	(0.845)
(exposed to severe typhoon)*(non-landfall county), β2	-0.059	-0.059	0.229	0.387
	(0.045)	(0.044)	(0.165)	(0.330)

Note: Sample is as described in Table 3.1. This table presents the results of estimating 3.1 from Poisson FE and Negative Binomial FE models. Models also control for an indicator for male, year of birth FE and month of birth FE, county FE, and county-specific cohort trend. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are clustered at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 3.4: Impact of Intrauterine Exposure to Severe Typhoon on Psychiatric-related Health Care Expenditures

Panel A	Outpatient Psychiatric-related Expenditures					
	total expenditures (mean=61.3 USD)			out-of-pocket expenditures (mean=4.9 USD)		
	Inverse hyperbolic sine transformation	Log (y+1) transformation	Tobit	Inverse hyperbolic sine transformation	Log (y+1) transformation	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)
(exposed to severe typhoon)*(landfall county), β1	0.123***	0.109***	106.605***	0.077**	0.068**	9.583**
	(0.040)	(0.034)	(34.367)	(0.034)	(0.030)	(3.853)
(exposed to severe typhoon)*(non-landfall county), β2	0.023	0.020	-0.521	0.017	0.014	0.822
	(0.016)	(0.014)	(13.963)	(0.011)	(0.010)	(1.077)
Panel B	Inpatient Psychiatric-related Expenditures					
	total expenditures (mean=68.9 USD)			out-of-pocket expenditures (mean=0.6 USD)		
	Inverse hyperbolic sine transformation	Log (y+1) transformation	Tobit	Inverse hyperbolic sine transformation	Log (y+1) transformation	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)
(exposed to severe typhoon)*(landfall county), β1	0.029	0.027	1,995.279	-0.008	-0.007	-152.205
	(0.024)	(0.022)	(2,393.989)	(0.009)	(0.008)	(130.873)
(exposed to severe typhoon)*(non-landfall county), β2	-0.007	-0.006	-1,056.977	-0.005**	-0.005**	-101.882**
	(0.004)	(0.004)	(867.147)	(0.002)	(0.002)	(45.791)

Note: Sample is as described in Table 3.1. This table presents the results of estimating 3.2 using OLS and Tobit models. Expenditures are inflation-adjusted and in 2011 USD. Column 1 and 3 apply inverse hyperbolic sine transformation to dependent variables ($log(y + (y^2 + 1)^{0.5})$). Columns 2 and 5 apply conventional log transformation to dependent variables (log(y+1)). Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are clustered at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 3.5: Robustness Checks I: Alternative Measures of Typhoon Severity

	Ever been diagnosed with any mental disorders				
	baseline	alternative definition of severe typhoons			
	deaths≥50	deaths≥20	deaths≥70	collapsed buildings≥2000	collapsed buildings≥4000
	(1)	(2)	(3)	(4)	(5)
(exposed to severe typhoon)*(landfall county), β1	0.024***	0.017**	0.026**	0.027***	0.030***
	(0.009)	(0.007)	(0.012)	(0.008)	(0.010)
(exposed to severe typhoon)*(non-landfall county), β2	0.005	0.002	0.004	0.005	0.004
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)
Number of severe typhoons	5	7	4	8	7

Note: Sample is as described in Table 3.1. This table presents the estimation results from linear probability models. Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Baseline definition of severe typhoon indicates more than 50 deaths. Columns 2-5 use alternative measures including 20 and 70 deaths, and 2000 and 4000 collapsed buildings. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are clustered at the the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 3.6: Robustness Checks II: Alternative Samples and Additional Controls

	Ever been diagnosed with any mental disorders								
	baseline	excluding north and central regions	excluding year of birth>=1966	excluding cohorts exposed to no-landfall severe typhoons	excluding counties with migration rate>=20%	farmers/ fishermen only	including region-year of birth FE	including temperature & rainfall	two-way clustering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(exposed to severe typhoon)*	0.024***	0.020**	0.041***	0.020**	0.026***	0.051**	0.022***	0.026***	0.024**
(landfall county), β1	(0.009)	(0.010)	(0.012)	(0.010)	(0.007)	(0.021)	(0.008)	(0.009)	(0.010)
(exposed to severe typhoon)*	0.005	-0.003	0.002	0.006	0.008	0.001	0.005	0.006	0.005
(non-landfall county), β2	(0.004)	(0.006)	(0.004)	-0.005	(0.006)	(0.008)	(0.003)	(0.004)	(0.004)
N	69,549	27,641	41,142	52,926	23,798	16,042	69,549	69,549	69,549

Note: Sample is as described in Table 3.1. This table presents the estimation results from linear probability models. Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Columns 1-8 cluster standard errors at the county level. Column 9 applies the Cameron et al. (2011) multiway clustering technique. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are showed in parentheses. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 3.7: Impact of Intrauterine Exposure to Severe Typhoon on Mental Illness By Gender

	Male		Female		P-values of H0: (2)=(4)
	mean	coefficient for (exposed)* (landfall), β1	mean	coefficient for (exposed)* (landfall), β1	
	(1)	(2)	(3)	(4)	(5)
Ever been diagnosed with any mental disorders	0.162	-0.012	0.247	0.063***	0.014
		(0.019)		(0.014)	
Among which					
Ever been diagnosed with anxiety disorders	0.110	0.006	0.181	0.033**	0.229
		(0.011)		(0.015)	
Ever been diagnosed with mood disorders	0.030	0.020**	0.048	0.013*	0.582
		(0.008)		(0.007)	
Ever been diagnosed with schizophrenia	0.013	0.006	0.010	0.001	0.597
		(0.006)		(0.005)	
Ever been prescribed any psychiatric drugs	0.127	-0.004	0.201	0.040***	0.014
		(0.016)		(0.008)	
Ever been prescribed antidepressants	0.042	0.019	0.061	0.033***	0.418
		(0.016)		(0.006)	
Number of psychiatric visits	1.481	0.251	1.685	0.364***	0.753
		(0.288)		(0.086)	
Psychiatric-related outpatient expenditures	62.708	-0.059	59.817	0.332***	0.030
		(0.098)		(0.082)	
N		34,811		34,738	

Note: Sample is as described in Table 3.1. Expenditures are inflation-adjusted, and in 2011 USD. Inverse hyperbolic sine transformation is applied to psychiatric-related outpatient expenditures. Models also control for year of birth FE, month of birth FE, county FE, county-specific cohort trend, and a set of interaction terms between non-landfall county and *in utero* exposure to severe typhoon (same as Table 3.2). Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are cluster at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 3.8: Impact of Intrauterine Exposure to Severe Typhoon on Physical Illness By Gender

	Male		Female		P-values of H0: (2)=(4)
	All		All		
	mean	coefficient	mean	coefficient	
	(1)	(2)	(3)	(4)	(5)
Ever been diagnosed with					
Coronary heart diseases	0.035	0.009	0.040	0.003	0.625
		(0.008)		(0.005)	
Hypertension	0.071	-0.003	0.054	0.004	0.866
		(0.018)		(0.021)	
Diabetes	0.046	-0.033**	0.056	0.003	0.147
		(0.016)		(0.010)	
Stroke	0.018	0.007**	0.024	-0.000	0.120
		(0.003)		(0.003)	
Cancer	0.014	0.006	0.026	0.014	0.708
		(0.025)		(0.011)	
N		34,811		34,738	

Note: Sample is as described in Table 3.1. This table presents the estimation results from linear probability models. Models also control for year of birth FE, month of birth FE, county FE, county-specific cohort trend, and a set of interaction terms between county intensity level and *in utero* exposure to severe typhoon (same as Table 3.2). Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are cluster at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table 3.9: Potential Mechanism for the Impact of Intrauterine Exposure to Severe Typhoon: Communicable Diseases

	Ever been diagnosed with any mental disorders		Ever been prescribed any psychiatric drugs		Number of psychiatric visits		Psychiatric-related outpatient expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(exposed to severe typhoon)*(landfall county), β1	0.041***	0.041***	0.033***	0.032***	0.329***	0.324***	0.200***	0.199***
	(0.012)	(0.012)	(0.009)	(0.009)	(0.072)	(0.073)	(0.056)	(0.057)
(exposed to severe typhoon)*(non-landfall county), β2	0.002	0.002	0.004	0.004	-0.044	-0.043	0.010	0.010
	(0.004)	(0.004)	(0.005)	(0.005)	(0.061)	(0.062)	(0.022)	(0.022)
communicable disease rate		0.019*		0.014		0.321***		0.148***
		(0.010)		(0.010)		(0.095)		(0.046)

Note: N=41,142. Data source: 5% Health Insurance Claim Records, 1998–2002. Analytical sample includes individuals who were born between 1959 and 1965 and were resided in rural townships. Expenditures are inflation-adjusted and in 2011 USD. Columns 1-4 present the results from linear probability models. Columns 5-6 show the results from Poisson regression models. Columns 7-8 present the results from OLS models with applying inverse hyperbolic transformation to psychiatric-related outpatient expenditures.. Models also control for year of birth FE, month of birth FE, county FE, county-specific cohort trend. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are cluster at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

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Appendix A

A.1 ICD-9 Diagnosis Codes

Any mental disorder: 290.xx-312.xx

Anxiety and personality disorders: 300.xx, 301.xx

Mood disorders: 296.xx, 300.4x, 311.xx

Schizophrenia: 295.xx

Coronary heart diseases: 410.xx-414.xx

Cancer: 140.xx-208.xx

Diabetes: 250.xx

Hypertension: 401.xx-405.xx

Stroke: 430.xx-438.xx

There are two systems that have been used by the physicians and clinics/hospitals in Taiwan to record the classification of diseases, specifically A-codes and ICD-9 codes. In my data set, most physicians and clinics/hospitals used A-codes before 1999. By 2000, almost all classifications were recorded using ICD-9 codes. I converted the relevant A-codes to

ICD9 codes for the analysis.

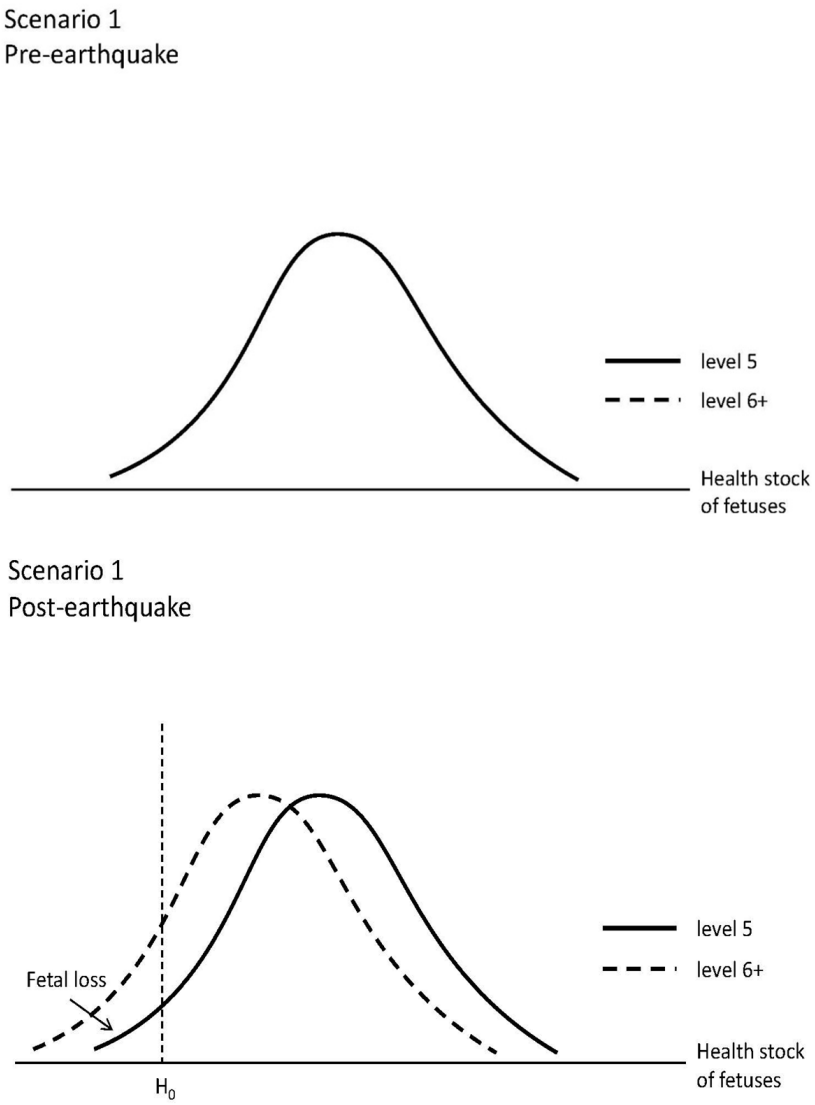
A.2 ATC Codes

Antidepressants: N06A, N06CA

Anxiolytics: N05B

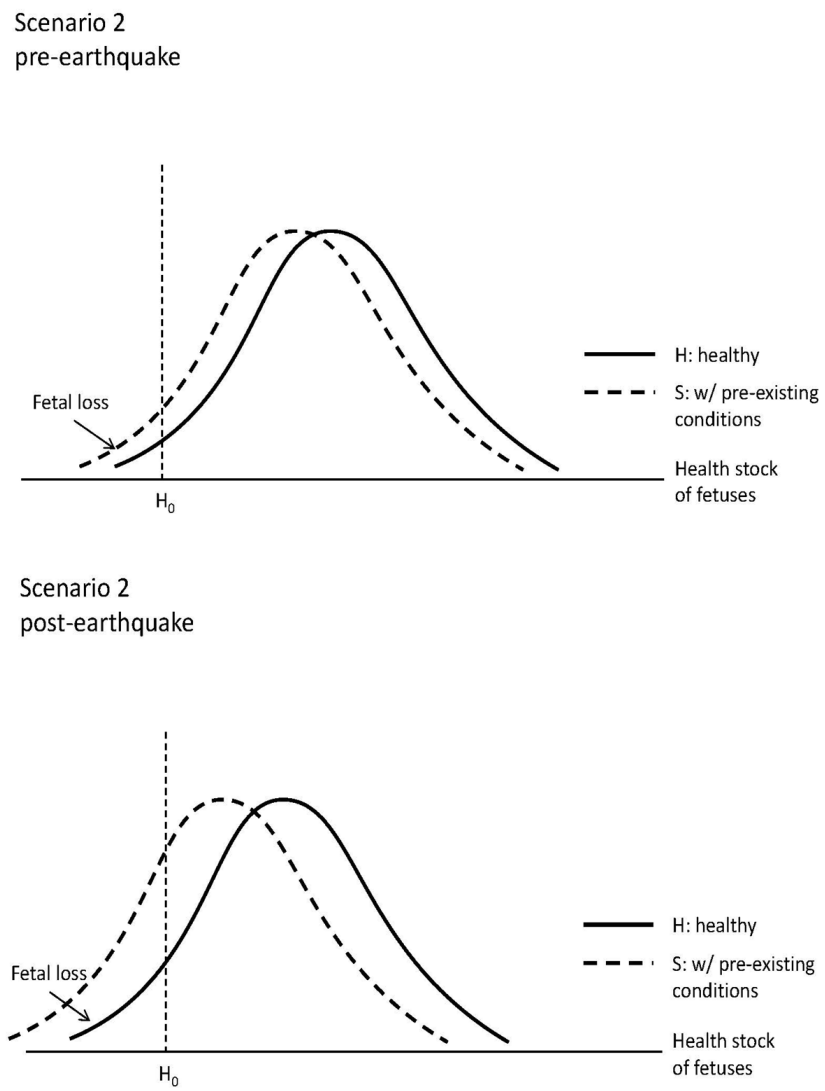
Antipsychotics: N05A

Figure A1: Pattern of Positive Selection I



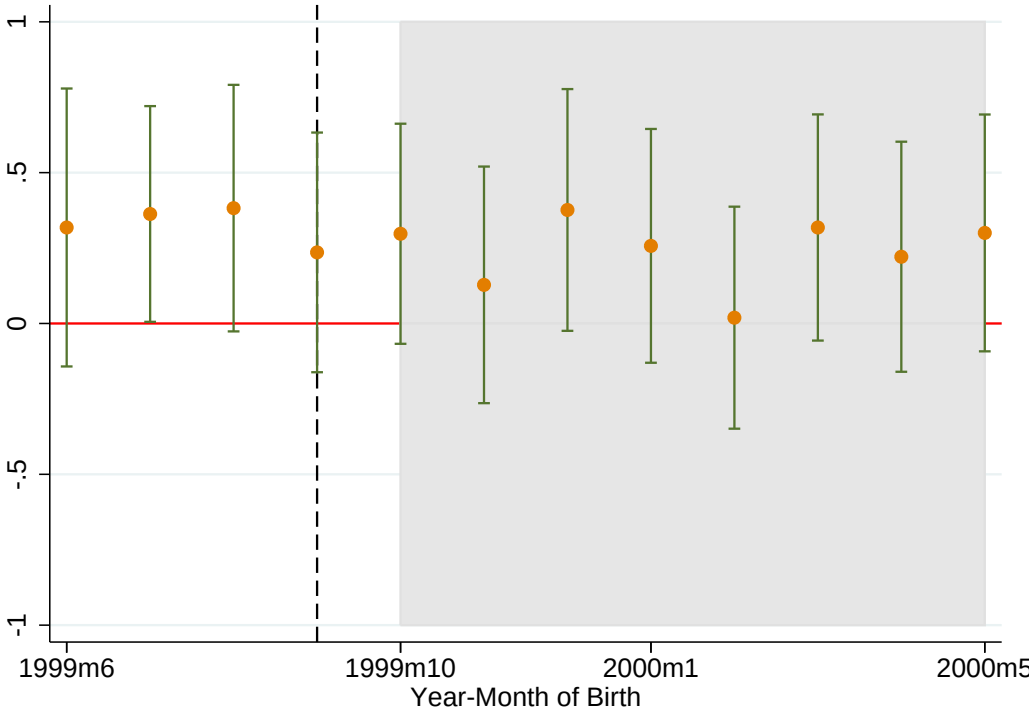
Suppose the health distribution between those residing in level 5 and level 6+ are the same. Those residing in Level 6+ experience greater negative shock, so the health distribution shift to further left compare to those residing in level 5 areas. Those below H0 are culled. The average survivors from level 6+ could be healthier than the average survivors from level 5 areas.

Figure A2: Pattern of Positive Selection I



Suppose the health distribution of those who have a healthy mother is to the right of those with mothers that have chronic illness. As a result of the earthquake, both groups experience negative shock and shift to the left (scenario 2). Those below H_0 are culled. The average survivors of those with unhealthy mothers could be healthier than the average survivors of those with healthy mothers.

Figure A3: Number of Prenatal Visits Conditional on Births



Note: The coefficients estimates presented the coefficients β_k from equation below. This figure suggest among those who were born in 1999m10 and 1999m11 residing in high intensity areas, they do not have fewer prenatal visits prior to the births.

$(NumberofPrenatalVisit_{iwt})= \sum_{k=1998M2}^{k=2001M12} I(YearMonth = k)_t + \sum_{k=1998M2}^{k=2001M12} \beta_k \; I(YearMonth = k)_t \; I(Intensity \geq 5)_w + Age_{it} + \delta_w + \epsilon_{iwt}$

Table A1: Impact of Intrauterine Exposure to Severe Typhoon on Mental Health: Comparing Linear Probability Model and Logit Model

Panel A	Ever been diagnosed with							
	Any mental disorders		Anxiety and personality disorders		Mood disorders		Schizophrenia	
	Baseline (1)	Logit (2)	Baseline (3)	Logit (4)	Baseline (5)	Logit (6)	Baseline (7)	Logit (8)
(exposed to severe typhoon)*(landfall county), β1	0.024***	0.022***	0.019**	0.018***	0.017***	0.015***	0.004***	0.002**
	(0.009)	(0.008)	(0.008)	(0.006)	(0.006)	(0.003)	(0.001)	(0.001)
(exposed to severe typhoon)*(non-landfallcounty), β2	0.005	0.005	0.005*	0.005*	0.004**	0.004**	-0.001*	-0.001*
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Panel B	Ever been prescribed							
	Any psychiatric drugs		Antidepressants		Anxiolytics		Antipsychotics	
	Baseline	Logit	Baseline	Logit	Baseline	Logit	Baseline	Logit
(exposed to severe typhoon)*(landfall county), β1	0.018*	0.016*	0.026***	0.023***	0.013	0.011	0.004	0.003
	(0.010)	(0.008)	(0.009)	(0.005)	(0.011)	(0.009)	(0.008)	(0.006)
(exposed to severe typhoon)*(non-landfallcounty), β2	0.008**	0.007**	0.004**	0.004**	0.006*	0.006*	-0.002	-0.002
	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)	(0.001)	(0.001)

Note: Sample is as described in Table 3.1. This table presents the estimation results from linear probability and logit regression models. Columns 2, 4, 6, and 8 present marginal effects evaluated at mean. Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are clustered at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table A2: Impact of Intrauterine Exposure to Severe Typhoon on Psychiatric Hospitalization

	Ever been admitted in psychiatric units (mean=0.007)			
	Baseline		Logit	
	(1)	(2)	(3)	(4)
(exposed to severe typhoon)*(landfall county), β1	0.004	0.003	0.002	0.001
	(0.003)	(0.002)	(0.002)	(0.001)
(exposed to severe typhoon)*(non-landfallcounty), β2	-0.001*	-0.001*	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)
County-specific cohort trend	No	Yes	No	Yes

Note: Sample is as described in Table 3.1. This table presents the estimation results from linear probability and logit regression models. Columns 3 and 4 present marginal effects evaluated at mean. Models also control for an indicator for male, year of birth FE, month of birth FE, and county FE. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are clustered at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table A3: Robustness Checks I: Alternative Measures of Typhoon Severity

	baseline	alternative definition of severe typhoons	
	deaths≥50	deaths≥20	collapsed buildings≥2000
	(1)	(2)	(3)
Panel A	Ever been prescribed psychiatric drugs		
(exposed to severe typhoon)*(landfall county), β1	0.018*	0.014*	0.020**
	(0.010)	(0.007)	(0.009)
(exposed to severe typhoon)*(non-landfall county), β2	0.008**	0.004	0.008**
	(0.004)	(0.003)	(0.003)
Panel B	Number of psychiatric-related visits		
(exposed to severe typhoon)*(landfall county), β1	0.301***	0.301***	0.309***
	(0.101)	(0.088)	(0.093)
(exposed to severe typhoon)*(non-landfall county), β2	-0.059	-0.091**	-0.069
	(0.045)	(0.044)	(0.043)
Panel C	Psychiatric-related outpatient expenditures		
(exposed to severe typhoon)*(landfall county), β1	0.123***	0.102***	0.128***
	(0.040)	(0.022)	(0.035)
(exposed to severe typhoon)*(non-landfall county), β2	0.023	0.009	0.013
	(0.016)	(0.018)	(0.013)
Number of severe typhoons	5	7	8

Note: Sample is as described in Table 3.1. Panel A presents the estimation results from linear probability models. Panel B represents the estimation results from Poisson FE models. Panel C presents the estimations results from OLS models with applying inverse hyperbolic transformation to psychiatric-related outpatient expenditures. Expenditures are inflation-adjusted and in 2011 USD. Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are clustered at the the county level. *** p¡0.01, ** p¡0.05, * p¡0.1

Table A4: Robustness Checks II: Impact of Intrauterine Exposure to Severe Typhoon on Psychiatric Drug Use

	Ever been prescribed any psychiatric drugs								
	baseline	exluding north & central regions	excluding year of birth>=1966	excluding cohorts exposed to non-landfall severe typhoons	excluding counties with migration rate>=20%	farmers/ fishermen only	including region-year of birth FE	including temperature & rainfall	two-way clustering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(exposed to severe typhoon)* (landfall county), β1	0.073**	0.064**	0.106***	0.057**	0.059***	0.151***	0.061***	0.075**	0.073**
	(0.030)	(0.026)	(0.009)	(0.028)	(0.022)	(0.045)	(0.016)	(0.038)	(0.029)
(exposed to severe typhoon)* (non-landfall county), β2	0.007	-0.008	0.004	0.008	0.017	0.001	0.007	0.009	0.007
	(0.009)	(0.013)	(0.012)	(0.010)	(0.014)	(0.019)	(0.008)	(0.008)	(0.010)
N	69,549	27,641	41,142	52,926	23,798	16042	69,549	69,549	69,549

Note: Sample is as described in Table 3.1. This table presents the estimation results from linear probability models. Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Columns 1-8 cluster standard errors at the county level. Column 9 applies the Cameron et al. (2011) multiway clustering technique. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are showed in parentheses. *** p¡0.01, ** p¡0.05, * p¡0.1

Table A5: Robustness Checks II: Impact of Intrauterine Exposure to Severe Typhoon on Psychiatric-related Health Care Utilization

	Number of psychiatric visits							
	baseline	exluding north and central regions	excluding year of birth>=1966	excluding cohorts exposed to non-landfall severe typhoons	excluding counties with migration rate>=20%	farmers/ fishermen	including region-year of birth FE	including temperature & rainfall
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(exposed to severe typhoon)* (landfall county), β1	0.301***	0.270***	0.329***	0.234***	0.240***	0.680***	0.325**	0.318***
	(0.101)	(0.090)	(0.072)	(0.091)	(0.087)	(0.122)	(0.139)	(0.116)
(exposed to severe typhoon)* (non-landfall county), β2	-0.059	-0.121**	-0.044	-0.065	-0.079	-0.144	-0.059	-0.038
	(0.045)	(0.058)	(0.061)	(0.062)	(0.062)	(0.110)	(0.043)	(0.047)
N	69,549	27,641	41,142	52,926	23,798	16042	69,549	69,549

Note: Sample is as described in Table 3.1. This table presents the estimation results from Poisson FE models. Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Columns 1-8 cluster standard errors at the county level. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are showed in parentheses. *** p¡0.01, ** p¡0.05, * p¡0.1

Table A6: Robustness Checks II: Impact of Intrauterine Exposure to Severe Typhoon on Psychiatric-related Health Care Expenditures

	Psychiatric-related outpatient expenditures								
	baseline	exluding north and central regions	excluding year of birth>=1966	excluding cohorts exposed to non-landfall severe typhoons	excluding counties with migration rate>=20%	farmers/ fishermen	including region-year of birth FE	including temperature & rainfall	two-way clustering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(exposed to severe typhoon)* (landfall county), β1	0.123***	0.101**	0.200***	0.089**	0.120***	0.254**	0.117***	0.130***	0.123***
	(0.040)	(0.043)	(0.056)	(0.039)	(0.035)	(0.116)	(0.031)	(0.043)	(0.046)
(exposed to severe typhoon)* (non-landfall county), β2	0.023	-0.012	0.010	0.028	0.053*	0.016	0.024	0.029*	0.023
	(0.016)	(0.025)	(0.022)	(0.020)	(0.030)	(0.038)	(0.016)	(0.017)	(0.019)
N	69,549	52,926	23,798	16042	69,549	69549	27,641	41,142	69,549

Note: Sample is as described in Table 3.1. This table presents the estimation results from OLS models. Inverse hyperbolic sine transformation is applied to psychiatric-related outpatient expenditures. Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Columns 1-8 cluster standard errors at the county level. Column 9 applies the Cameron et al. (2011) multiway clustering technique. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are showed in parentheses. *** p¡0.01, ** p¡0.05, * p¡0.1

Table A7: Sensitivity of Estimates to Specific Typhoon Incident

	Ever been diagnosed with any mental disorders (mean=0.20)					
	baseline	excluding 1st severe typhoon	excluding 2nd severe typhoon	excluding 3rd severe typhoon	excluding 4th severe typhoon	excluding 5th severe typhoon
	(1)	(2)	(3)	(4)	(5)	(6)
(exposed to severe typhoon)*(landfall county), β1	0.024***	0.031***	0.018***	0.022**	0.022*	0.026***
	(0.009)	(0.012)	(0.007)	(0.010)	(0.011)	(0.007)
(exposed to severe typhoon)*(non-landfall county), β2	0.005	0.005	0.005	0.005	0.005	0.005
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
P-value of H0: β1-β2=0	0.030	0.026	0.046	0.105	0.127	0.002
N	69,549	65,360	65,284	65,194	65,448	65,545

Note: Sample is as described in Table 3.1. This table presents the estimation results from linear probability models. Column 2 to 6 exclude individuals who were exposed to each severe typhoon incident while *in utero* one by one. Models also control for an indicator for male, year of birth FE, month of birth FE, county FE, and county-specific cohort trend. Exposure to severe typhoons is a dummy variable, which equals 1 if one was *in utero* during a severe typhoon. Severe typhoon is defined as a typhoon that caused 50 deaths. Landfall county equals to 1 if one resides in the landfall county for the given typhoon. Non-landfall county is a dummy variable indicating whether individual resides in the non-landfall county for a given typhoon. Omitted group is individuals who were not exposed to severe typhoons while *in utero*. Standard errors are clustered at the county level. *** p¡0.01, ** p¡0.05, * p¡0.1