Childbirth Effects of the 2004 Indian Ocean Tsunami *

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Abstract

This paper evaluates the effect of the in utero exposure to the 2004 Indian Ocean Tsunami on short-term childbirth outcomes in Indonesia. Exploiting variation in damage intensities across locations and the timing of exposure, we find that the probability of successful pregnancies drops by 5.9 pp, while miscarriages increase by 5.5 pp. However, this does not vary by intensity of exposure across locations. Our results suggest the importance of considering fetal loss in developing countries and highlight that facilitating household investment in health through various policies may mitigate negative birth effects in the aftermath of natural disasters.

Keywords: Natural disasters, 2004 Indian Ocean Tsunami, in-utero exposure, birth outcomes

JEL Codes: J13, O15, Q54

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1 Introduction

Because of climate change, large-scale natural disasters are becoming increasingly frequent and severe. Indeed, records from Emergency Events Database (EM-DAT), as shown in Figure 1, show that the number of natural disasters per year has risen by 10 times since 1960s [1]. Natural disasters pose an increasingly serious threat to health and educational opportunities, as well as direct damages to assets and properties. They can directly affect health through death, physical injuries, and traumatic mental distress (Currie and Rossin-Slater 2013). They also have indirect effects on health by destroying infrastructure supplying health care (Kousky 2016).

While these ramifications indiscriminately affect all individuals, children are especially vulnerable (Kousky 2016). They can be harmed by being victims themselves (Lépine et al. 2021) or through the negative shocks that affect their mothers while pregnant (Almond and Currie 2011; Almond et al. 2018). In addition, evidence from medical and economics literature notes that exposure to adverse events at early stages of pregnancy has irreversible long-term effects on long-run health and labor market outcomes (Almond and Currie 2011; Barker 1992; Black et al. 2019)[2]. Natural disasters also disproportionately affect developing countries as they are more vulnerable to the costs that follow, despite facing an equal likelihood of experiencing one compared to developed countries (Strömberg 2007)[3]. They also have difficulty insuring themselves and recovering from the damages relative to developed countries due to lack of resources and government accountability (Kahn 2005; Strömberg 2007). Despite this, very little is known about the effects of an adverse shock in utero in a developing country context where relevant data became recently available.

In this study, we investigate how prenatal exposure to natural disasters affects various indicators of child birth outcomes in developing countries. In particular, we focus on one of the most devastating natural disasters to strike developing countries in the recent memory - the 2004 Indian Ocean Tsunami (hereafter 'the Tsunami'). The casualties and the negative effects on health and educational progress of various population groups are well-documented (Shaw 2015; Lépine et al. 2021;

[1] EM-DAT database keeps track of the incidence of natural disasters if an event satisfies one of the following four criteria: 1) There are 10 or more people reported dead. 2) 100 or more people are reported to be affected. 3) The local government declares a state of emergency. 4) There is a call for international assistance.

[2] Pregnancies in the first trimester is considered critical as central nervous system is developed at this point (de Rooij et al. 2010). Some papers, such as Karbownik and Wray (2019) and Koppensteiner and Manacorda (2016), find empirical evidence that those affected by adverse events in the first trimester of pregnancy are the most vulnerable.

[3] Strömberg (2007) shows that geographic distribution of mortality risk from natural hazards is imbalanced, with Central African and Southeast Asian regions demonstrating the highest mortality risk deciles (Figure 3 in Strömberg (2007))

Frankenberg et al. 2020b; Cas et al. 2014). However, the effect of the Tsunami on the immediate birth outcomes and health of those who are unborn at the time are not fully understood yet. By studying various immediate birth outcomes, we aim to complement the research on the mortality effects of the Tsunami. Additionally, we contribute to the understanding of demographic effects of the Tsunami and examine suggestive evidence on how various channels amplify or mitigate the negative birth effects.

This paper aims to fill the gap in the understanding of the impacts of adverse events in utero by finding whether there are birth effects and mechanisms specific to developing countries. Many existing works find significant short-run and long-lasting impacts of prenatal exposure to adverse events in developed countries (Almond 2006; Black et al. 2016; Currie and Rossin-Slater 2013). However, there are ways in which the effect of exposure to such events in utero may differ for developing countries. For instance, while studies in developed countries look into health conditions at birth, fetal loss is much more prevalent in developing countries (Weinhold 2009; Institute of Medicine 2003)[4]. Additionally, due to lack of endowments of resources and institutions in these countries (Kahn 2005), the channels in which the adverse effects are aggravated or mitigated may differ.

We utilize the Tsunami as our natural experiment, whose treatment varies across timing of exposure and communities. The dataset we use consists of high-frequency pregnancy records from geographically homogenous regions, compared to other studies in Indonesia (Gignoux and Menéndez 2016; Lépine et al. 2021). Our strategy to identify causal treatment effect consists of the event-study and difference-in-difference approaches. Then, we also include outcomes related to various possible mechanisms to understand what drives the treatment effect. This would allow us to uncover further heterogeneities in treatment effects, as well as to provide suggestive evidence on what should be done by the policymakers to minimize negative birth effects.

We find that there indeed is a negative impact on birth outcomes due to the Tsunami, especially for those in the earliest stage of pregnancy at the time. In our preferred specification, we find that the probability of pregnancy ending in livebirth decreases by 5.9 pp and probability of miscarriage increase by 5.5 pp for those in the first trimester of pregnancy when the Tsunami struck[5]. The prob-

[4]While estimates differ depending on sources, most incidences of birth defects that lead to stillbirths or miscarriages occur in developing countries. According to Weinhold (2009), 85% birth defects that can cause miscarriages and stillbirths are reported in developing countries. Furthermore, Institute of Medicine (2003) states that out of approximately 4 million fetal deaths that occur annually, 98% of them are in developing countries.

[5]There are three possible outcomes for pregnancies in our data - livebirth, miscarriage, and stillbirth. As for stillbirth, we find that there is no statistically significant change at the 5% level.

ability of premature birth, defined as pregnancies shorter or equal to 8 months, increases by 6.7 pp for the same cohort. All these negative effects do not appear for those conceived after the Tsunami and those in later stages of pregnancies. However, the birth outcomes do not differ depending on the varying extent of damages across locations. The results are robust to using different treatment assignment strategies and do not appear on various placebo samples, confirming that general temporal trends are not driving our results. There is suggestive evidence from household health expenditure patterns that environmental investments after the Tsunami could be offsetting the negative effects of the Tsunami.

We contribute to three strands of literature. First, we add to the previous literature on the impact of adverse events on the health outcomes of utero infants. Such impacts and the causal mechanisms in the context of developed countries are well-understood with the availability of the detailed administrative data (Almond 2006; Almond et al. 2009; Black et al. 2016; Black et al. 2019; Currie and Rossin-Slater 2013; Karbownik and Wray 2019). With relevant panel data becoming more accessible in developing countries, researchers are beginning to unearth similar effects in developing countries, (Armand and Kim Taveras 2020; de Oliveira et al., Forthcoming; Koppensteiner and Manacorda 2016; Kunto and Mandemakers 2019; Rosales-Rueda and Triyana 2019; Torche 2011). However, not all works on developing countries are able to identify mechanisms behind the birth effects. We further contribute to this literature by providing comparison of birth effect estimates between developed and developing countries[6] and finding possible mechanism behind negative birth effects that could be specific to developing countries. In our estimation, we find roughly equal or larger birth effects of adverse events compared to the estimates found in developed countries.

We also aim to complement the literature on the human cost of natural disasters by finding aspects of damages that are specific to the developing countries. Strömberg (2007) provides a theoretical framework for understanding the extent of damage caused by natural disasters. Currie and Rossin-Slater (2013), Karbownik and Wray (2019), and Imberman et al. (2012), and Mahajan and Yang (2020) uses indices exclusively available in developed countries - exposure to meconium aspiration syndrome and labor market indices, for instance - to study the damaging effects of natural disasters. Lépine et al. (2021) also studies the 2004 Tsunami and finds a temporary increase in infant mortality which subsides afterwards. However, the paper is silent on the effect of the Tsunami on fetal death

[6] In this regard, we use studies such as de Oliveira et al. (Forthcoming), Koppensteiner and Manacorda (2016), and Torche (2011) and others to compare the results.

for those in utero at the time, which is far more likely to happen in developing countries (Institute of Medicine 2003; Weinhold 2009). Given that we have related variables - livebirths and miscarriages - we can make rigorous empirical statements on these outcomes. Furthermore, since developing countries lack endowments that helps cushion negative effects (Kahn 2005), our estimates are less likely to be confounded by institutional factors and infrastructure. These allow us to identify the damaging effects of natural disasters on developing countries that may not occur in developed countries.

Lastly, this paper contributes to the literature studying how individuals and communities in developing countries recover from natural disasters. In this literature, economists currently concur that individuals and communities in developing countries recover slower than compared to those in developed countries due to lack of resources and institutional capacity (Kahn 2005; Kellenberg and Mobarak 2008). Many studies have analyzed how various post-disaster responses contributed to the recovery from natural disasters in developing countries. Gignoux and Menéndez (2016) and Lépine et al. (2021) highlight the role of external aid in rebuilding infrastructure and mitigating negative health effects induced by disasters. Deryugina et al. (2018) and Zhang (2018) point out that individual-level responses such as migrating out of affected areas helped mitigate further human costs of disasters. Additionally, Bhalotra (2007) and Paxson and Schady (2005) explore the role of public and private expenditure on health in addressing mortality at times of crises. We highlight the inverse association between post-disaster household health expenditures and negative birth effects, suggesting that policies facilitating household investment can mitigate costs of natural disasters.

The rest of the paper is organized as follows. We review the events that unfolded in, and papers that address demographic impacts of the 2004 Indian Ocean Tsunami in Section 2. We introduce the dataset and the identification strategies in Section 3. Main identification strategies are discussed in Section 4. We present the empirical results in Section 5. We conduct various diagnostic tests on our empirical results in Section 6. In Section 7, we discuss various mechanisms that could amplify or mitigate the treatment effect. We conclude this paper in Section 8.

2 Background: The Impact of the 2004 Indian Ocean Tsunami

2.1 What happened: The 2004 Indian Ocean Tsunami in numbers

The Indian Ocean Tsunami occurred on December 26th, 2004. It started with an undersea earthquake with a registered magnitude of 9.1 striking the western coast of Aceh province, located in the northern tip of the island of Sumatra in Indonesia (Lay et al. 2005). The Tsunami hit two northern provinces on the Sumatra Island - the Aceh and North Sumatra provinces. The map in Figure 2 indicates the exact location of the epicenter and the surrounding areas. The series of massive tsunami waves arrived at the coastlines quickly and unexpectedly. The first rupture from the earthquake began at 7:58AM local time and the tsunami waves reached the Western coastline of Aceh province 15 minutes later (Lay et al. 2005; Shaw 2015). With the lack of an organized early tsunami warning system in the region, many residents were unaware of the impending tsunami (Shaw 2015)[7].

The human costs that ensued affected multiple countries, including Indonesia, Sri Lanka, and even countries in eastern coast of Africa (National Planning Development Agency and World Bank 2005; Shaw 2015). Globally, more than 200,000 people were killed and 1 million people were displaced across Indonesia, Sri Lanka, Thailand, and Somalia, among other countries (Shaw 2015). The Indonesian people suffered the most in terms of displacements and lives lost. In Indonesia alone, more than 110,000 people lost their lives and around 700,000 people were displaced due to the tsunami, many of whom were women and children (Frankenberg et al. 2011) [8]. In terms of damage, Indonesia alone suffered damages and losses worth US$4.45 billion (Masyrafah and Mckeon 2008).

In the aftermath, there was an influx of humanitarian response from international communities and the Indonesian central government (Lépine et al. 2021). In total, the international community pledged US$7.7 billion to the recovery efforts, with US$6.4 billion being allocated by the end of 2007 (Masyrafah and Mckeon 2008; Lépine et al. 2021). In particular, programs such as 'Building Back Better' aimed to reconstruct schools and provide material support to local hospitals and midwives to support health care for children (UNICEF 2005; Fan 2013). Out of the US$250 million projected to be spent between 2005 and 2007, about 33% of the funds had been allocated by October 2005[9].

[7]Subsequently, the Indian Ocean Tsunami Warning System was developed, providing countries in the Indian Ocean area with early warnings of the Tsunami (Shaw 2015).

[8]The reported casualties differ depending on the source and the timing of the report. For instance, Shaw (2015), which was written 10 years after the Tsunami, reports that 168,000 people were killed by the Tsunami in Indonesia alone.

[9]Additionally, the Tsunami ended the civil conflict in Aceh that lasted for almost 30 years with a peace agreement in August 2005 pledging additional government funding in return for ceasing the separatist movement (Lépine et al. 2021)

This study focuses on the Aceh and North Sumatra provinces. Some noteworthy features of the two provinces according to National Planning Development Agency and World Bank (2005) are as follows. Aceh relies heavily on oil and gas industries, which accounts for 43% of the regional GDP. This province suffered from two decades of continued, low-intensity conflicts. North Sumatra, on the other hand, is the most populous province outside of Java, where the capital city of Jakarta is located. In 2004, 5.53% of the Indonesian population resided in this province. It is also one of the largest economies outside of Java, with agriculture and manufacturing being the dominant sectors.

2.2 Previous studies on the demographic impact of the 2004 Indian Ocean Tsunami

There are a number of papers examining the demographic effects of the 2004 Tsunami using the data used in this paper and other related datasets. Among the papers exploring the mortality and fertility effects of the Tsunami, Frankenberg et al. (2011) finds that the probability of survival is positively correlated with biological factors such as strength, age, and gender as opposed to socioeconomic factors. Nobles et al. (2015) documents the change in fertility patterns after the 2004 Tsunami. Mothers who lost at least one child or who resided in communities where mortality rates are higher were more likely to bear additional children after the Tsunami. Frankenberg et al. (2020b), and Ho et al. (2017) propose evidence of increased adult mortality rate of males with poor psychological health and females who were widowed due to the Tsunami.

More closely related to this work is that by Lépine et al. (2021). They study the effect of the Tsunami on child mortality using Demographic and Health Survey (DHS) data and a synthetic control approach. They find a temporary increase one year after the Tsunami but no permanent increase in under-5 mortality. They credit the improvement of environmental factors made possible by coordinated post-disaster response from government and international organizations for the lack of negative long-run effects. It documents an increase in government's expenditure on infrastructure and improved antenatal care in the medium term. However, they are silent on the effect of the Tsunami on the children exposed to the event in utero and fetal death outcomes for these pregnancies. Moreover, it opens, but does not directly test the possibility that the 'harvesting' effect - whereby the Tsunami led to a death of the most vulnerable fetuses only while leaving others unaffected - is behind the temporary effect.

Another stream of papers concentrates on how the Tsunami affected the acquisition of various dimensions of human capital - educational attainment and mental health in particular. Cas et al. (2014) finds that children who lost at least one parent to the Tsunami were less likely to be enrolled in school compared to children with two living parents in the short-run. In the long run, they have shorter years of schooling and are less likely to be married. Frankenberg et al. (2008) and Frankenberg et al. (2012) show that various symptoms of post-traumatic mental distress is positively correlated with the extent of community-level destruction. They also demonstrate that some symptoms have persistent effects.

3 Identification strategy

In this section, we develop identification strategies that achieve two goals. The first is to find whether those exposed in utero are more susceptible to negative birth outcomes. In particular, fetuses exposed while in the first trimester of pregnancy are expected to be the most vulnerable (de Rooij et al. 2010; Schulz 2010). Therefore, we expect the negative effect to be most evident for those in the first trimester of pregnancy at the time of the Tsunami [10].

We also explore if there are heterogenous birth effects depending on the damage incurred at the cluster of residence. Specifically, we hypothesize that those residing in more damaged clusters would be subject to worse birth outcomes. Those clusters may experience more destruction to key infrastructure that is critical in minimizing damage and speeding up recovery (Kahn 2005).

To achieve both goals, we start by defining how the treatment status is categorized. We utilize two specifications in this study. The first regression is an event-study specification using only the different timing of exposure to the Tsunami as a treatment variation. The second regression is a differences-in-differences approach that uses both variation in timing and cluster of residence to determine treatment assignment.

3.1 Determining treatment status

There are two dimensions of treatment that we exploit in this research. The first dimension is based on the different timing of exposure to the Tsunami. The pregnancy records in our data contain

[10] There may be cases where in utero exposure to adverse events are most damaging for those in other stages of pregnancy. For instance, Roseboom et al. (2006) finds that children whose mothers were exposed to the Dutch famine of 1944-45 at the third trimester of pregnancy had higher blood pressure at adulthood. According to the same paper, the major driving force behind the result is the adverse intrauterine environment near the conclusion of pregnancies.

information on the birth date of a child and the gestation duration, in months, for each pregnancy. Using these two variables, we back out the month of conception. For instance, a child born in April 2005 after 10 months of gestation would be conceived in June 2004.

Once the date of the conception is deduced for all observations, we calculate the *expected* birth date by unilaterally calculating forward by nine months from the time of conception. If the Tsunami occurred between the point of conception and the *expected* birthdate, we consider that pregnancy to be exposed to the Tsunami in utero[11].

The rationale for using this approach is that the actual duration of gestation may be endogenous (Black et al. 2016; Currie and Rossin-Slater 2013; Matsumoto 2018). There are two potential sources of endogeneity. The first source is that the probability of being included in the treatment group mechanically rises with the gestation length, biasing downward the outcomes for gestation length and premature birth. The second source is that the treatment itself may directly affect gestation length, which we later show is the case with exposure to the Tsunami. Therefore, using information on the actual gestation period to determine Tsunami exposure can bias our estimates.

Using this approach, we can also differentiate exposure by the stages of pregnancy. As the infant's central nervous system is developed in the first trimester (de Rooij et al. 2010; Schulz 2010), in utero exposure can have heterogenous effects depending on the stages of the pregnancy. In fact, there is evidence attesting that exposure to adverse events in the first trimester of pregnancy is more devastating than other stages of pregnancy (Karbownik and Wray 2019; Koppensteiner and Manacorda 2016). Thus, exploring whether the treatment effect differs depending on the stage of pregnancy is essential for understanding potential long-run effects.

We use the conception date and the fact that the Tsunami occurred on December 26th, 2004, to determine whether the pregnancy was in the first, second, or third trimester when the Tsunami struck. Specifically, those conceived between April and June of 2004 would be in the third trimester of pregnancy when the Tsunami occurred, those conceived between July and September of 2004 would be in the second trimester, and those conceived between October and December of 2004 would be in the first trimester of pregnancy.

We also have variation in the extent of damage at a community level. In the dataset, the unit of communal observation is referred to as a "cluster", which is a group of households with similar

[11] This would not be different from using the *actual* birthdate if all pregnancies were 9 months long. However, only 85.7% of pregnancies in our sample are 9 months long, with 7.5% of pregnancies ending prematurely. This is an approach used in related works such as Black et al. (2016) and Currie and Rossin-Slater (2013).

extent of damage due to the Tsunami, ecological attributes, distance to the coastline, and levels of urbanization[12]. Each cluster is categorized as either "lightly affected", "moderately affected", and "heavily affected" based on the extent of the damage incurred[13]. All clusters in the dataset have been affected by the Tsunami. The assignment is predetermined in the dataset and is based on assessments by the community leaders, survey supervisors, and satellite imagery. Summary statistics and balance tables are provided in Tables 1 and 2.

3.2 Event-study: Using differences in timing of exposure

The first regression specification uses an event study approach, noting that all clusters have been affected by the Tsunami. This would allow us to capture the different birth effects of the Tsunami across different times of conception. Ultimately, we can separately identify heterogenous effects across different stages of pregnancy. Since we are assigning different treatment statuses across times of conception based on expected birthdays, the treatment starts from second quarter of 2004. For the event-study approach, we use the following equation.

$$y_{ict} = \alpha + \phi_c + \gamma X_{ict} + \sum_t \theta_t \mathbb{1}[t \leq 2003Q4] + \sum_t \beta_t \mathbb{1}[t \geq 2004Q2] + \epsilon_{ict} \tag{1}$$

In the above equation, i indexes each case of pregnancy, c indexes cluster of residence, and t indexes the year-quarter of conception. y_{ict} is the outcome variable of interest - livebirth, miscarriage, gestation length, or preterm birth. All outcome variables except gestation length is an indicator variable. For these regressions, we run a linear probability model. We include cluster fixed effects ϕ_c that absorb any unobserved cluster-level attributes. Standard error is clustered at a cluster-level.

The regression also includes the set of controls X_{ict}. This controls for factors determining selection into fertility and minimize omitted variable bias. We control for years of schooling for both the mother and the father. We also control for the mother's age at birth, in terms of raw levels and squared age. Dummy variables for the order of birth are also included. These are the standard set of controls included in related studies, even in the context of developing countries (Armand and Kim

[12] In the whole dataset, there are on average 92 households per cluster. In the reduced sample where we only keep pregnancies occurring in the 2003-05 calendar years, there are 17 households on average.

[13] Due to the limitations of the data, we are unable to match each cluster with its exact geographical location. This prevents us from using more detailed regional attributes such as elevation, zoning, and building areas. However, we do find that more homes are destroyed as the extent of the Tsunami damage in a cluster becomes more serious. We later show that our analysis is qualitatively identical whether we use the predetermined damage categorizations or treatment assignment based on house destruction.

Taveras 2020; Black et al. 2016).

The indicator variables $\mathbb{1}[t \geq 2004Q2]$ and $\mathbb{1}[t \leq 2003Q4]$ denote assignment to treated and control groups based on the expected birthday for a given pregnancy. $\mathbb{1}[t \geq 2004Q2]$ is an indicator that equals 1 if the year-quarter of conception t is on the second quarter of 2004 or after. This would be the treatment group. Similarly, $\mathbb{1}[t \leq 2003Q4]$ indicates whether the year-quarter of conception t is on the fourth quarter of 2003 or before, thus assigned to the control group. The coefficient for those conceived in the first quarter of 2004, who belong in the control group, is set to zero. This follows the standard normalization procedure used in event-studies (Freyaldenhoven et al. 2019; Schmidheiny and Siegloch 2020).

The coefficients of interest are the β_t's, calculated separately for each year-quarter beginning from the second quarter of 2004. This coefficient picks up the effect of the Tsunami on birth outcomes for those conceived on year-quarter t, on or after the second quarter of 2004. By allowing β_t to vary across each conception period, we can capture the distinct average birth effects for those conceived in different periods. Since we are testing whether there exist negative birth effects for those in utero during the Tsunami, we expect livebirths to be less likely and gestation lengths to be shorter. In terms of regression coefficients, we expect that $\beta_t < 0$. As for the regressions on miscarriages and preterm births, we anticipate them to be more likely and the β_t in these cases to be positive.

The required identifying assumption for Equation (1) is that selection into treatment and control is as good as random. Only then can we attribute the different birth effects across timing of exposure to the Tsunami. Since the Tsunami was unexpected and sudden, non-random selection between those conceived in years 2003 and 2004 would be unlikely. We do verify this in the balance table, displayed at Table 3.

3.3 Differences-in-differences: Using both timing and cross-sectional variation

In the second specification we exploit the variations across clusters in the extent of damage in addition to the variation in the timing of exposure. In doing so, we incorporate a differences-in-differences approach where we compare the birth outcomes of those conceived before and after the Tsunami across clusters with different levels of damage. We use the following equation, hereafter referred to as the DD-approach.

$$y_{ict} = \alpha + \phi_c + \gamma X_{ict} + \sum_t \theta_t \mathbb{1}[t \leq 2003Q4] + \sum_t \beta_t \mathbb{1}[t \geq 2004Q2]$$
$$+ \sum_{k=2}^{3} \sum_t \delta_{kt} \mathbb{1}[\text{damage}_c = k] \times \mathbb{1}[t \geq 2004Q2] + \epsilon_{ict} \quad (2)$$

$\mathbb{1}[\text{damage}_c = k]$ is an indicator for the extent of damage incurred in cluster c, capturing differences across communities with varying extents of damage. k can take one of three values: 1 for a lightly affected, 2 for a moderately affected, and 3 for a heavily affected cluster. In the regression, lightly affected clusters are omitted to avoid perfect multicollinearity. All other variables have the same meaning as in Equation (1).

There are two parameters of interest. First, β_t's represent the birth effect of the tsunami on those conceived in period t and whose mothers were in the lightly affected cluster. While this parameter still captures heterogeneities in the birth effect across different conception periods, there are subtle differences in interpreting the β_t coefficients between Equations (1) and (2). In the former, we do not use the variation across clusters with different levels of damage. As such, the β_t coefficient in Equation (1) represents an average change in y_{ict} against the immediate pre-treatment period *across all individuals*. In Equation (2), we distinguish those residing in different clusters using $\mathbb{1}[\text{damage}_c = k]$. Thus, β_t now represents the average change in y_{ict} for those residing in *lightly affected areas*. The total birth effect for those conceived in other clusters can be obtained by summing over β_t and relevant δ_{kt}. The expected sign of β_t is the same as in Equation (1).

Second, δ_{kt} compares those conceived in period t at a cluster with damage level k against those conceived in the same period but in the lightly affected cluster. It represents the additional birth effect of the Tsunami on top of the effect on those conceived in lightly affected clusters. We hypothesize that for a given t, the negative birth effect would be more dominant as the extent of damage becomes more serious. Specifically, for a given conception period t, we expect that $\delta_{3t} < \delta_{2t} < 0$ for livebirth and gestation length outcomes. In contrast, we expect that the probability of miscarriages and preterm births are the highest in the clusters with the heaviest damage, thus $0 < \delta_{2t} < \delta_{3t}$.

The additional identifying assumption for Equation (2) is the random assignment across clusters with different levels of damage. In other words, pregnancies in different clusters are similar in all other aspects except for the extent of the damage incurred by the Tsunami. Otherwise, the regional differences beyond the exposure to the Tsunami may confound our estimates. We verify that individuals across the three types of clusters are largely similar, except for educational enrollment and literacy of mothers, at a 5% level with the balance table in Table 2.

3.4 Other concerns for identification

To ensure that the birth effect captured in our estimates are not confounded by any seasonal patterns, we conduct a placebo study on a comparison cohort conceived at different time periods. In particular, we use three sets of placebo cohorts whose attributes are similar to our main analysis sample. However, these cohorts are unaffected by the Tsunami in reality. Our claim that the Tsunami contributed to a negative birth effect would be verified only if the results are significant exclusively on our main analysis sample and null in the placebo cohorts. We later show in Section 6.1 that this is the case.

The other potential concern against our estimation results is that the consistency and the unbiasedness of our results could be sacrificed if fertility patterns change after the Tsunami. For those conceived in 2005, their mothers were affected by the Tsunami before the beginning of their pregnancies. Therefore, there is a possibility that the knowledge of the Tsunami event has affected fertility decision. We take three approaches to address this issue in Section 6.2. First, we remove pregnancies whose conception occurred after the Tsunami and check whether the birth effects in the reduced sample are similar. Additionally, we check for structural changes in fertility patterns at a cluster level by regressing the total number of conceptions in a clusters on various controls. Last, we also test whether the type of mothers going into fertility change by regressing the covariates onto dummies for time period of conception and cluster of residence. We verify that our main results still stand after removing pregnancies conceived after the Tsunami and that there are no noticeable changes to total conception and type of mothers becoming pregnant post-Tsunami.

4 Data

4.1 Data source: Study of the Tsunami Aftermath and Recovery (STAR)

Our data on the 2004 Indian Ocean Tsunami comes from the Study of the Tsunami Aftermath and Recovery (STAR) project, which was initiated to study outcomes related to the tsunami and subsequent recovery[14]. The STAR dataset is a longitudinal survey of individuals and households residing in the Aceh and North Sumatra Provinces[15]. The first wave of the STAR project began on May 2005, five months after the Tsunami (STAR 1). Since then, annual follow-ups have been made for four more years (STAR 2- STAR 5), followed by a ten-year follow-up in 2015-2016 (STAR 6) [16]. We utilize the first two waves for our analysis. In particular, STAR 2 includes data on history of education, pregnancy and marriages of the respondents in the affected areas. STAR 1 provides information on pre-Tsunami asset, income and employment status. All respondents of the STAR survey lived in Aceh and North Sumatra when the survey began[17] [18].

For our main analysis, we use data on educational attainment, marriage history, and pregnancy history. Using marriage history, we are able to match husbands, wives, and children. Moreover, the data contains full pregnancy history of ever-married women aged 15 to 49. The pregnancy history includes details on the type of child birth outcomes (livebirths, stillbirths or miscarriages), gestation length, and birthweight. It also includes year of birth and month of birth, allowing us to exploit variation across timing of exposure in a high-frequency time unit and to define treated and control groups. In addition, these data include information with which we can use to construct other control variables that we include in the regression.

4.2 Outcome variables of interest

In our main results, we present four indicators of birth outcomes for our dependent variable - livebirth, miscarriage, gestation length, and preterm birth. Livebirth is a dummy variable equal to one if a fetus survives until the conclusion of the pregnancy and zero otherwise. Miscarriage occurs if a fetal death occurs early into the pregnancy[19]. We use the question on the duration of pregnancy to determine the gestation length. Preterm birth is a dummy variable that equals one if the pregnancy lasted 8 months or shorter. In the sample, the average gestation length is 8.76 months

[14] Frankenberg et al. (2020a) includes detailed explanation of the dataset and the relevant survey instruments. The dataset can be accessed at `https://stardata.org`

[15] Lépine et al. (2021) uses the Demographic Health Survey and Gignoux and Menéndez (2016) uses the Indonesian Family Life Survey, which contains fewer individuals affected by the Tsunami and more observation from other Provinces.

[16] At the point of writing, only the first two waves have been made public.

[17] The respondents were selected based on the baseline data for the Socioeconomic Survey (SUSENAS) conducted by Statistics Indonesia in the two provinces in February 2004 (Frankenberg et al. 2012). This provides a sample representative of the pre-Tsunami population.

[18] A household member who moved out after STAR1 was interviewed as a member of a 'split-off' household afterwards.

[19] In the dataset, the respondent self-reports whether the pregnancy resulted in a miscarriage. While 92.8% of the reported miscarriages occurred within 5 months of conception, the rest happened after 5 months.

and 7.5% of pregnancies ended prematurely. Since impediments that prevent livebirths and lead to miscarriages are more common in developing countries (Weinhold 2009; Institute of Medicine 2003), we can look into birth effects of in utero exposure to adverse events that are more relevant for low-income countries.

As for unit of time, we aggregate the month of conception into quarters in our main specification. As such, the unit of time in our preferred specification is in year-quarters. In this way, we analyze how the birth effects of the Tsunami is heterogenous with respect to different trimesters of pregnancy.

4.3 Other variables

In the data on pregnancy history, there is information on mother's age at birth and the order of birth. We include these variables into our regressions as our control variables. In the records for educational history, each respondent is asked about the highest level of schooling attended and the total number of years spent at that level of education. Combined with the fact that there are six years in the elementary level and three each at middle and high school levels, we are able to deduce the total years of schooling for each individual[20].

To analyze mechanisms driving the birth effects, we use migration histories, information pertaining to household finances, usage of outpatient health care service, and self-reported symptoms of mental and physical distress. Employment status and income, expenditures on health-related items (including prenatal care), receipt of transfers and household assets are contained in the dataset. It also includes information on whether the respondents used any outpatient care at all and if so, at which type of facilities - ranging from public/private hospitals to local village care centers. In addition, each respondent is asked to report whether she experiences various symptoms in a yes-or-no format. The symptoms queried include fear of death, feeling disturbed when reminded of the tsunami, anxiety about future life, and fear of water.

4.4 Sample restrictions

We have a record of pregnancies that date from August 1978 to November 2006. There are 19,346 pregnancies documented in the dataset. Within those observations, we only keep observations in

[20]We also run the regression by including the dummy variables for the highest level of schooling completed for both mothers and fathers. The results are qualitatively and quantitatively identical to the specification with years of schooling included.

which both birth year and month are identified and the records of both parents are matched. We include pregnancies from single-parent families as well as two-parent families. That leaves us with 10,102 observations. We also eliminate further observations so that we are comparing cohorts that are similar in other attributes to those in utero during the Tsunami[21]. Thus, our main cohort of interest is comprised of those conceived in the calendar years 2003-05. We end up with a main cohort of 2,159 observations. This sample selection also allows us to capture roughly equal proportions of those who were completely unaffected, those in utero, and those conceived after the Tsunami. Summary statistics and the balance test between the unaffected and affected population are found in Tables 3 and 4. In addition, Table 5 contains the number of pregnancies per year-quarter of the main sample period.

Based on the 2005 Indonesian Population Census, we find that the levels of educational attainment of our main sample and that of the general Indonesian population are similar. In 2005, the illiteracy rate for individuals aged 15 years or over was slightly above 3% nationwide, while for our sample the illiteracy rate of the 2003-2005 cohorts were around 8% for mothers and 5% for fathers. The enrollment rate for any primary education or above was around 97% across the nation, while similar numbers were found for our 2003-2005 cohorts in the two provinces (the enrollment rate is 96% and 99% for mothers and fathers respectively). The summary statistics of our cohort sample are displayed in Table 1. Economically, Aceh is less developed compared to North Sumatra and the rest of Indonesia. In Aceh, 23.6% of the population lives in urban areas, whereas the same for North Sumatra is 42.4%. For Indonesia as a whole, urban populations take up 49.9% of the entire population. North Sumatra has higher regional GDP than Aceh. In 2004, the regional GDP for the two provinces were US$3.1 billion and US$5.5 billion., whereas the GDP for Indonesia as a whole was US$256.9 billion.

[21]Even without this process, we get numerically and qualitatively similar results when we use our preferred identification strategy and the entire sample. However, the treated and control groups are statistically different in educational attainment and age at birth in this setup. Therefore, rigorous causal interpretation on this regression result is not applicable.

5 Main Results

5.1 Event-study results

The regression results for Equation (1) are presented in columns (1) - (4) of Table 6 and Figure 3. Coefficients for those conceived in the first quarter of 2004, the last pre-treatment period, is normalized to 0. Thus, the coefficients for those conceived in conception period t indicate the differences in average birth effect relative to those conceived in the first quarter of 2004. Since livebirth, miscarriage, and preterm birth outcome variables are all binary indicators, the coefficients for these outcomes are interpreted as differences in percentage points of that outcome being realized. For the gestation length outcome, which is a continuous variable in the unit of months, the coefficients should be interpreted as the differences in the months of gestation.

Livebirth results are presented in column (1) in Table 6 and the top left panel of Figure 3. The treated were conceived since the second quarter of 2004, and those considered from that period until the fourth quarter of 2004 were exposed to the Tsunami in utero. For those conceived in the fourth quarter of 2004, we find a statistically significant negative effect on livebirth. The probability of a successful birth outcome drops by 5.9 pp compared to those conceived on the first quarter of 2004. As for those conceived in the second and third quarter of 2004, the effect is not statistically different from zero at a 5% level. There is a statistically significant nonzero effect for those conceived in the fourth quarter of 2005 which does not survive other specification tests later on. For all other periods, the effect is not distinguishable from zero at a 5% level. Therefore, the negative birth effects of the Tsunami are concentrated on those in the first trimester of pregnancy during the Tsunami.

We present the results on miscarriages in column (2) in Table 6 and the top right panel of Figure 3. The results in this regression are qualitatively similar to the livebirth outcome in that the negative effects appears on those in the first trimester of pregnancy. Numerically, pregnancies commenced in the fourth quarter of 2004 are 5.5 pp more likely than those that began in the first quarter of the same year to end in a miscarriage. The difference in the same probability is statistically zero for those conceived in the second and third quarter of 2004. There is also a statistically significant birth effect for those conceived one quarter after the Tsunami.

We compare among estimates from our work and two studies which provide figures on livebirth and fetal losses. Liu et al. (2015) finds that livebirths among pregnancies exposed to the 1999 Taiwan earthquake in the first trimester decreases by 4.4 percent but finds no significant changes for those exposed in other stages of pregnancy. Also, in utero exposure to a hurricane in Brazil increased fetal death of babies born to mothers who are 15-24 years old by 17 per 1,000 pregnancies (de Oliveira et al., Forthcoming). Taken at a face value, our estimates indicate larger reduction in livebirths and increase in fetal losses.

Results on the gestation length are displayed in column (3) of Table 6 and the bottom left panel of Figure 3. The gestation length for those conceived in the fourth quarter of 2004 is about 0.31 months shorter than those conceived in the first quarter of the same year. However, the effect is significant at the 10% level (p-value: 0.055). The gestation length of others who were exposed to the Tsunami in utero is not statistically different from zero. We also find statistically significant results for those conceived in the fourth quarter of 2005, although this is not robust to specification choice.

Lastly, the regression results for preterm birth are found in column (4) in Table 6 and the bottom right panel of Figure 3. As with other results, those in the first trimester of pregnancy at the Tsunami are the most affected. The pregnancies that were conceived in the fourth quarter of 2004 are 6.7 pp more likely than those conceived in the first quarter of the same year to end prematurely. The statistically significant result in the fourth quarter of 2005 also do not survive many other robustness tests we conduct. The differences in the likelihood of a pregnancy ending prematurely are not statistically different from zero at a 5% level for the pregnancies that started on other year-quarters.

We can also find comparable estimates for gestation lengths and preterm births. Koppensteiner and Manacorda (2016) finds that a one standard deviation increase in exposure to local violence in small municipalities in Brazil in the first trimester of pregnancy decreased gestation length by 0.006 weeks (0.024 months) and increased probability of preterm birth by 1.5%. Black et al. (2016) finds that mental distress due to bereavement in utero decreases gestation length by 0.09 weeks (0.36 months) in Norway. Torche (2011) finds that due to the 2005 earthquake in Chile, the probability of preterm delivery increased by 2.6 pp and gestation length was reduced by 0.19 weeks (0.76 months) for those exposed in the first trimester of pregnancy. The results here suggest that the negative effects of the Tsunami on gestation length and preterm birth can be considered slightly larger or similar to other adverse events.

In summary, among those exposed to the Tsunami in utero, those in the first trimester of pregnancy are the worst affected. For those in later stages of pregnancy, the changes in the birth outcomes are not statistically different from zero. We also find no effects on those conceived after the Tsunami. The results are in line with the findings in the medical literature that negative events to mothers in the first trimester can affect birth outcomes (McLean et al. 1995; Mulder et al. 2002; Schulz 2010). Our results also share similarities with Karbownik and Wray (2019) and Koppensteiner and Manacorda (2016) in that the negative effect is the most pronounced on those exposed in the earliest stage of the pregnancy.

5.2 DD approach results

The results for the β_t coefficients in Equation (2) are reported in columns (5) through (8) of Table 6. The top half of Figure 4 reports the β_t coefficients from the same equation in graphs. The bottom half of the same figure displays the results on the δ_{kt} coefficients that indicate differences across clusters with different extent of the Tsunami damage. As with the event-study results, the β_t coefficients should be interpreted as differences in average effects relative to the first quarter of 2004. This coefficient alone captures the effect on those conceived at period t in lightly affected areas. For those conceived in other clusters, β_t as well as δ_{kt} from the same conception period is required. The δ_{kt} coefficients should be interpreted as a difference in the birth effects in clusters with damage level k against the lightly affected clusters for those conceived in the same period t. [22]

The bottom half of Figure 4 collects all the δ_{kt} coefficients for the entire set of outcome variables. Interestingly, we find no heterogeneity in birth effects across clusters with different extent of the Tsunami damage. As shown in the bottom panel of Figure 4, nearly all coefficients are statistically indistinguishable from 0 at the 5% level. Even for those that are distinct from zero, the coefficients are not consistent with our hypotheses. The hypotheses are that the drop in the livebirth probability would be the greatest for the heavily affected clusters and that miscarriages would rise the most in the same clusters. Our estimates show that δ_{3t} for those conceived on the fourth quarter of 2004 is positive for the livebirth outcome and negative for the miscarriage outcome. These imply that the heaviest affected clusters the least likely to see increases in livebirths and decreases in miscarriages. These results still stand even if we use finer level of cross-sectional treatment variation. We will go over this result in Section 6.3.

As for the results pertaining to the timing of exposure, we find that they are qualitatively identical to the event-study results. From columns (5) to (8) in Table 6 and the top panel of Figure 4, we note that negative birth effects among those exposed to the Tsunami in utero exclusively appear in those

[22] We also show that the results hold even if we assign those conceived after the Tsunami in the control group, as shown in Section 1.1 of the online appendix.

in the first trimester of pregnancy. Livebirth and gestation length fall by 12 pp and 0.73 months respectively, while miscarriage and preterm birth increase by 12.6 pp and 11.6 pp.

In short, it is evident that the Tsunami has affected the birth outcomes of those in the first trimester of pregnancy, and only those pregnancies. As in the event-study specifications, the findings are consistent with discoveries from the medical literature with regards to the significance of the first trimester of pregnancy (McLean et al. 1995; Mulder et al. 2002; Schulz 2010). However, we find no heterogenous differences in treatment effect across different clusters.

6 Specification and robustness tests

6.1 Verifying timing effects: Placebo results

To ensure that the effects captured in Section 5 are attributable to the Tsunami, we conduct a placebo exercise. Since the Tsunami is a one-time event, such effects should not be repeated on samples entirely comprised of conception cohorts that were not exposed to the Tsunami. For our exercise, we select three "placebo samples" - those conceived on 2000-2002, 2001-2003, and 2002-2004. We refer to the sample used in our main regression as the "main sample". These three sets of placebo samples do not differ with the main sample with regards to educational attributes, mother's age at birth, and urbanization, as shown in Table 7.

To run the placebo test, we use the same regressions used in Equations (1) and (2) on our placebo samples. Within each placebo sample, those conceived in the last 7 quarters will be subject to a fake tsunami treatment. As such, the placebo test shares the same time structure as our main regression. However, the estimated effects should be null for the placebo samples. If this is the case, we can attribute the birth effects obtained in the main results to the Tsunami. Otherwise, our estimates could be confounded by repeated seasonal patterns in birth outcomes not related to the Tsunami.

Results of the regression on the main sample and the placebo samples are presented in Figures 5, with the top and bottom panels representing event-study and DD-approach results respectively. The numbers in the horizontal axes in the graphs indicate the quarters since the beginning of the treatment. In particular, period 0, 1, and 2 correspond to those who on the third, second, and first trimester of pregnancy respectively when the tsunami occurs for each cohort. However, the birth effects of the tsunamis occurring on the placebo cohorts, represented in gray lines, should be null as these are only hypothetical events. Indeed, the effects of the 'fake' tsunamis are all statistically equivalent to zero at the 5% level across all three placebo samples. The blue lines representing the main sample results indicate that the birth effect of the Tsunami is statistically significant only for those conceived 2 quarters after the treatment - or those in the first trimester of pregnancy - in the main sample. In particular, the difference between the placebo estimates and the main estimates are the most evident for livebirth and miscarriage outcomes.

These results suggest the following; 1) the birth effects of the Tsunami we found on the main sample are not confounded by other time trends such as seasonal patterns. 2) The pre-trends are not evident, since the hypothetical tsunamis that were set up before the actual Tsunami occurred, show no significant effects. Lastly, 3) the differences in birth outcomes when compared against cohorts conceived on other years are most evident for livebirth and miscarriage outcomes[23].

6.2 Testing for possible changes to post-Tsunami fertility patterns

The identifying assumption required to verify the heterogenous treatment effect by timing of exposure is that assignment to control and treatment groups is as good as random. The unexpected and sudden nature of the Tsunami makes this assumption plausible for those conceived in 2003 and 2004. As for those conceived in 2005, this assumption is less plausible as mothers are aware of the Tsunami and can take this into account in their fertility decisions.

We address this in three ways. First, we reduce the sample further down to those conceived in 2003 and 2004 only. This would leave us with 8 quarters of data, where those conceived in the first five quarters are in the control group and those in the latter three are in the treatment group. We use the same identification strategy used in Equations (1) and (2), with the smaller number of leads to adjust for the sample period. Results reported in Section 1.2 of the online appendix show that the Tsunami effect is statistically significant only for those conceived in the fourth quarter of 2004, consistent with our main results.

Another way is to test for the changes in the fertility trend at the cluster level, as in Black et

[23] We have conducted back-of-the-envelope calculations on these outcomes in the following manner:

$$w_{(03-05)}\beta_{2,(03-05)} - (w_{(00-02)}\beta_{2,(00-02)} + w_{(01-03)}\beta_{2,(01-03)} + w_{(02-04)}\beta_{2,(02-04)})$$

w_s indicates weights put on each sample, determined by the number of observations belonging in that sample over the total number of samples. $\beta_{2,s}$ is the coefficient on the birth outcomes 2 quarters after the start of the treatment each sample s. In this manner, we find that livebirth probability and gestation lengths falls by 2 pp and 0.093 months respectively. The probability of miscarriages and preterm births rise by 1.8 pp and 0.7 pp each.

al. (2016) and Koppensteiner and Manacorda (2016). Here, we aggregate the total number of conceptions that occurred within the same period at a cluster level, where the total conceptions are composed of pregnancies ending in livebirth, miscarriage, and stillbirth. We denote each cluster-conception period cell as blocks. We regress the total number of conceptions at a given block onto cluster fixed effects, and dummies for each year-quarter of conception to check for structural changes in the total conceptions in the event-study specification. Thus, we regress the following equation.

$$c_{ict} = \alpha + \phi_c + \sum_t \theta_t \mathbb{1}[t \leq 2003Q4] + \sum_t \beta_t \mathbb{1}[t \geq 2004Q2] + \epsilon_{ict} \quad (4)$$

We use both the raw and log total conceptions in a block for the outcome c_{ict} . The time period dummies included are in a similar manner to Equation (1) in that the coefficient for the immediate pre-treatment period is set to zero. We also regress the DD-approach counterpart of this regression in Section 1.2 of the online appendix.

For the fertility trends within blocks, we report the findings visually in Figure 6. In the event-study specification, the coefficients on the year-quarter dummies are not statistically different from zero in most cases. These results suggest that there is no noticeable difference in the total number of conceptions across the sample period. The results are qualitatively identical even in the DD-approach, as reported in Section 1.2 of the online appendix.

This regression also allows us to conclude that the effect of uncounted miscarriages is minimal. Miscarriages can occur without mothers being aware of it (Linnakaari et al. 2019)[24]. Thus, one concern could be that increase in unreported miscarriages may drive the total conception numbers down and mechanically decrease livebirths. Since our definition of total conception also includes reported miscarriages, we can use this regression as an indirect test to see if uncounted miscarriages rise after the Tsunami. If there is an increase in uncounted miscarriages, this could potentially drive the total conception numbers downward. As our results show, there is no noticeable downturn in the number of total conceptions. More importantly, the existence uncounted miscarriages would suggest that our estimate of the Tsunami impact on miscarriages would be underestimated, suggesting that the true effect would only increase further if all of these could be captured. Similarly, the negative effect seen on livebirth outcomes could be strengthened if all uncounted miscarriages were observable.

[24] Linnakaari et al. (2019) states that while 8-15% of the clinically recognized pregnancies conclude in a miscarriage, an estimated 30% of all pregnancies end in miscarrage.

Lastly, we test for the changes in the type of mothers becoming pregnant after the treatment using a similar method. While the balance table introduced in Table 3 addresses the selection into treatment and control group at a *group level*, it is silent when it comes to picking up differences in sample selection across *each* year-quarter. To address this, we run regressions similar to Equation (6) with c_{ict} variables replaced by X_{ict} variables from the main regressions. The results are graphically presented in Figures 7, with point estimates reported in Table A4 of the online appendix. The estimated coefficients for those exposed to Tsunami in utero are statistically insignificant at the 5% level. The same holds for the estimates for those conceived after the Tsunami. These results suggest that there are no noticeable changes in the type of mothers becoming pregnant before and after the event. Therefore, changes in the type of pregnant mothers are unlikely to drive our main findings. In Section 1.2 of the online appendix, we also find similar results if we use a DD-approach.

6.3 Alternative measure of damage

In this section, we use alternative definitions of treatment assignment at a cross-sectional level. The cross-sectional variation of treatment in Section 5.2 categorizes different clusters into the same group if the extent of damage is considered identical. As such, the current measure does not capture the variation that can exist across different clusters within the same extent of damage. The following exercise captures these variations and complements the DD-approach results on Section 5.2.

To do this, we utilize the respondents' report on whether their houses were damaged by the Tsunami. In the survey, respondents could answer that their houses were either unaffected, damaged, or destroyed by the Tsunami. We aggregate those that reported that their houses were either damaged or destroyed into a single category. Table 1 shows, more houses are destroyed on average as damage extent becomes greater. As such, they can serve as a proxy for the indicators for the extent of damage used in earlier DD-approaches.

We then derive a new variable that indicates the intensity of the Tsunami damage for each cluster. We refer to this treatment intensity variable as 'house damage rate' and define it as

$$\text{House damage rate in cluster } c = \frac{\text{Total damaged houses in cluster } c}{\text{Total houses in cluster } c} \tag{6}$$

In the DD-approach regression (Equation (2)), this variable replaces $\mathbb{1}[\text{damage}_c = k]$. It can take any values from zero to one, making it a continuous variation of treatment intensity. The clusters with higher house damage rate are subject to more serious damages from the Tsunami. Similar to Equation (2), we hypothesize that those in clusters with higher house damaged rate are more susceptible to additional harmful birth effects of the Tsunami.

The results are in Figure 8, with estimates reported in Table A5 of the online appendix. As with the main results, the harmful birth effects of the Tsunami are concentrated on those conceived in the fourth quarter of 2004. The regression results show no statistically significant treatment effect differences across cross-sections at the 5% level, suggesting that the null treatment heterogeneities across cross-sections are robust to treatment assignment strategies for different communities[25].

7 Discussion

7.1 What explains negative birth effects?

One candidate mechanism behind our negative birth effects result is the maternal mental distress that follows the Tsunami. Many previous papers pinpoint the mental distress that pregnant mothers experience as the source of negative birth outcomes and subsequent demographic and human capital effects (Koppensteiner and Manacorda 2016; Black et al. 2016). These findings are also supported in the medical science literature that studies hormonal responses to maternal stress and the ensuing birth outcomes (McLean et al. 1995; Pike 2005; Latendresse 2009).

To check whether mothers who experience mental distress are more likely to have negative birth outcomes, we use two categories of mental distress. First, we use self-reported responses of having particular symptoms of mental distress from the survey respondents. The types of symptoms include fear of death, fear of injury, sleeping disorders, anticipatory anxiety, and aquaphobia. Second, we use the parental deaths from the Tsunami to proxy the likelihood of suffering severe stress. Columns (1) to (5) of Table 8 show no significant correlation between indicators of mental distress and pregnancies in different year-quarters, suggesting that they are unlikely to drive our results. One exception is the fear of death, which appears to be statistically significant for mothers in the first trimester of pregnancy during the Tsunami. However, after adjusting for the multiple hypotheses testing (Cameron and Trivedi 2005), the estimate is actually not statistically different from zero.

We also look into how the mothers' physical health could explain the negative birth outcomes. We

[25] In Section 1.3 of the online appendix, we introduce another approach where we use the indicator for the house being damaged due to the Tsunami to determine cross-sectional treatment. Results are qualitatively identical.

use a self-reported response on whether the individuals feel that they are in a same health condition or worse. In addition, we also use a dummy variable indexing whether the mother used some type of outpatient care service one month before the survey to check for usage of hospital services after the Tsunami. Columns (6) to (8) in Table 8 show that some of the mothers whose pregnancies occurred in our treatment period are more likely to report being in worse health and use outpatient care. These results suggest a possible role for the physical health of mothers determining our main outcomes.

7.2 What explains the lack of cross-sectional differences?

In this section, we test whether there are endogenous responses after the Tsunami that explain the lack of cross-sectional differences in the treatment effects. After natural disasters, certain responses alter the composition of residents and their characteristics (Torche 2011). For instance, individuals can selectively out-migrate from heavily affected areas in order to find a better living environment (Deryugina et al. 2018; Peri et al. 2020). Additionally, transfers from external sources are usually targeted to those living in more adversely affected areas to speed up recovery efforts in those regions (Gignoux and Menéndez 2016). These responses may mitigate further damages as well as compensate for the costs already accrued.

We first look at whether those who began pregnancy in heavily affected areas at the time of the Tsunami are more likely to out-migrate. This is motivated by the finding that the likelihood of out-migration increases with the extent of damage (Mahajan and Yang 2020)[26]. We also check whether the total amount of transfers received, those from the government, and those from non-governmental organizations increase for residents in clusters with heavier levels of damage beginning pregnancies around the Tsunami period. Relevant summary statistics on these variables are included in Table 9. Since we are interested in checking whether these factors explain the null differences across communities, we use Equation (2) with variables related to migration and transfer as outcome variables.

The results are in Table 10. We find that mothers whose pregnancies were exposed to the Tsunami in utero are not more likely to move out or receive more assistance of any type in 2005. Thus, we cannot conclude that migration and transfers are the factors offsetting the regional differences in the Tsunami effect [27].

[26] There are also evidence to the contrary. For instance, Spitzer et al. (2020) uses historical data on the Messina-Reggio Calabria Earthquake in 1908, but finds no evidence of increased emigration due to the incident.

[27] One caveat has to be made clear about the transfer result above. There are findings where the inflow of aid and transfers did contribute to the recovery in Indonesia (Lépine et al. 2021; Fan 2013; Gignoux and Menéndez 2016; Shaw

7.3 Why do negative birth effects die out?

In this section, we introduce suggestive signals as to why negative birth effects are short-lived. Our main results show that the negative birth effects are only significant for those conceived on the fourth quarter of 2004. For pregnancies conceived in the first quarter of 2005 or after, there is no statistically significant effect of the Tsunami. We analyze two possible channels - change in household expenditures on health after the Tsunami and selective survival of fetuses.

In a similar study, Lépine et al. (2021) analyzes the effect of the 2004 Indian Ocean Tsunami on child mortality for those born between 1990 and 2017. Lépine et al. (2021) uses synthetic control methods and finds that compared to the unaffected provinces, the under-5 mortality had increased for just one year after the Tsunami, without any medium- or long-term effects. Lépine et al. (2021) credits the increased infrastructure spending per capita by local governments and better provision of pregnancy delivery services for the decaying of the harmful effects of the Tsunami, made possible by coordinated international responses. It also opens up the possibility of a harvesting effect where the deaths of the most vulnerable children before the age of 1 lead to significant declines in mortality indicators in later time periods. This idea, however, is not directly tested on the paper.

We extend from Lépine et al. (2021) in the following ways. First, we can use the data on household expenditures to identify how household-level responses, as opposed to public-level investment, affected the post-Tsunami treatment effects. In doing so, we also provide an evidence that complements a well-known finding that spending on health at a private and public level leads to better birth and health outcomes (Bhalotra 2007; Jaba et al. 2014; Paxson and Schady 2005). Second, we can test for a suggestive sign of selective survival of fetuses, a necessary condition of harvesting effect (Luy et al. 2020), by comparing changes in birthweight of newborn babies before and after the Tsunami.

Empirically, we proceed as follows. We check for changes in household expenditures on health-related items and type of prenatal care services to pick up patterns in post-Tsunami investments. This would provide suggestive evidence on how post-Tsunami investments correlate to birth outcomes. To test for selective survival of fetuses, we use the raw birthweight, logged birthweight, and an indicator for very low birthweight ($\leq$ 1.5kg) and for low birthweight ($\leq$ 2.5kg) in our regressions. Should selective survival exist, there should be an increase in birthweight for the treatment cohort

2015). Our results do not contradict theirs in that most types of assistance covered in their studies started flowing in in the mid-2005. The sample period of our study precedes these events. As such, we are not making any statement related to the effectiveness of the total amount of aid that went into the affected regions.

through the underrepresentation of those with low birthweight. On the other hand, a statistically significant reduction of birthweight would suggest a scarring effect in which all fetuses were negatively affected by the Tsunami regardless of underlying health conditions (Bozzoli et al. 2009). Relevant summary statistics are included in Table 9.

We report our investigation of environmental factors and selective survival in Table 11 and Figure 9. For the log of health-related expenditure by the households, we find that there is a significant increase after the Tsunami. Household spending on health increased by more than 30% for mothers conceiving the baby after the Tsunami compared to those conceiving at 2004Q1. We also find, however, that categories related to prenatal care - amount spent and number of visits - do not change significantly. Some treated households are also less likely to give birth at their own home or that of their family members. As for signals of selective survival, we find that birthweight of the newborn rises for some, but not all, conception periods within our treatment period. Results for both very low birthweight and low birthweight indicators are qualitatively identical. There is a decrease in pregnancies counted as low birthweight in some treated periods, but coefficients are otherwise zero. Thus, evidence of the selective survival is mixed at best[28].

These results suggest that household-level investments may explain why no birth effect appears for those conceived after the Tsunami. Also, they hint that policy options that ease household investment decisions could contribute to minimizing harmful effects on fetal health and other demographic indicators. However, there are some caveats. We do not have the relevant variables for some of the observations in the main sample, which is why the sample size in these regressions are smaller. This is especially true for the birthweight regressions. We have confirmed that the exposure to the Tsunami affects livebirths and miscarriages. Thus, the treatment determines who is omitted from the birthweight regression, increasing the likelihood that our estimates may be subject to biases. Thus, the results here should be taken as suggestive relations, not as a causal interpretation.

7.4 Heterogeneity across different socioeconomic status

In this section, we determine whether birth outcomes differ depending on the socioeconomic status of mothers. Whether those with better socioeconomic statuses can cushion themselves from

[28]We include more pregnancies in the sample, dating back to those conceived in 1999, and run Equations (1). The results are qualitatively identical in that the significant rise in birthweight is only observed in some, but not all treatment cohorts.

adverse events is not yet certain[29]. Wealthier households may protect themselves by using their resources to choose well-protected locations and households (Frankenberg et al. 2015). However, Frankenberg et al. (2011) finds that when physiological and biological attributes are controlled for, the correlation between socioeconomic status factors and probability of survival falls [30].

In this section, we check if there are noticeable differences among cohorts in different stages of pregnancies and clusters in terms of various socioeconomic status indicators - employment, household assets, and building material of houses. This would allow us to identify whether there are heterogenous treatment effect across those with different socioeconomic attributes as well as to detect possible omitted variable biases in our main results. Summary statistics for the variables tested are found in Table 9. In the results presented, we use Equation (2) to capture potential differences of socioeconomic attributes across pregnancies that began in different time periods as well as clusters.

Results are presented in Table 12 and suggest that differences socioeconomic status are unlikely to drive the main results. The log of loss in value of housing due to the Tsunami has no statistically meaningful relation to the affected pregnancies. Whether either parent is unemployed does not have any relation to the treated groups. The same can be said with regards to the four indicators of housing material - living in the house with brick walls, wooden walls, iron roof, and dirt floor. The results are qualitatively similar to Frankenberg et al. (2011) in that socioeconomic status has no relation to the various measures of mortality.

8 Conclusion

Increasing numbers of populations in developing countries are being exposed to the human costs and economic damages from natural disasters. Pregnant mothers, whose health condition can be passed onto their children, are no exception. As opposed to the long-run health and educational attainments for those exposed to adverse events in utero, much less is known about how disasters affect short-run birth outcomes. In this paper, we use a dataset containing high-frequency pregnancy records and populations living in geographically homogenous communities to identify whether being exposed to the Tsunami at different stages of pregnancy leads to heterogeneous birth outcomes.

[29]For instance, Brown and Thomas (2018) finds that parents of those affected by the 1918 Spanish influenza pandemic while in utero have lower socioeconomic status compared to unexposed cohorts. However, Frankenberg et al. (2011) finds that socioeconomic status has no relation with mortalities attributed to the 2004 Tsunami.

[30]Frankenberg et al. (2011) uses physiological attributes such as stamina, running and swimming ability, and strength to explain that children, older adults, and females were less likely to survive than others.

We also investigate whether the effects are heterogenous across communities with different extent of damages attributable to the Tsunami.

We find that the exposure to the Tsunami negatively affects the probability of the fetuses in the first trimester surviving until the conclusion of pregnancy. In particular, probability of miscarriage increases while that of livebirth decreases at a statistically significant magnitude. However, those conceived after the Tsunami or in the later stages of pregnancy when the Tsunami struck are shielded from these negative effects. Additionally, we cannot conclude that there are meaningful community-level differences in the Tsunami effect. We also note that health expenditures by households for those conceived post-Tsunami are significantly larger, suggesting that household-level responses may have prevented damaging effects in the aftermath.

Our results provide suggestive evidence that if governmental and non-governmental organizations provide assistance that could facilitate household investment in health, it could minimize harms to children's health further. In addition, given the uniquely large extent of damage caused by the Tsunami, the effects of other natural disasters are likely to be subtler. Still, our results speak to the necessity of looking into the probability of fetal loss, a problem largely overlooked in developed countries. Given the prevalence of fetal loss in developing countries (Institute of Medicine 2003), there is a risk of underestimating the negative birth effects of various adverse events in developing countries by neglecting short-run birth outcomes and indicators of fetal loss. Thus, considering differences in fetal survival probabilities is essential for such studies based on developing countries.

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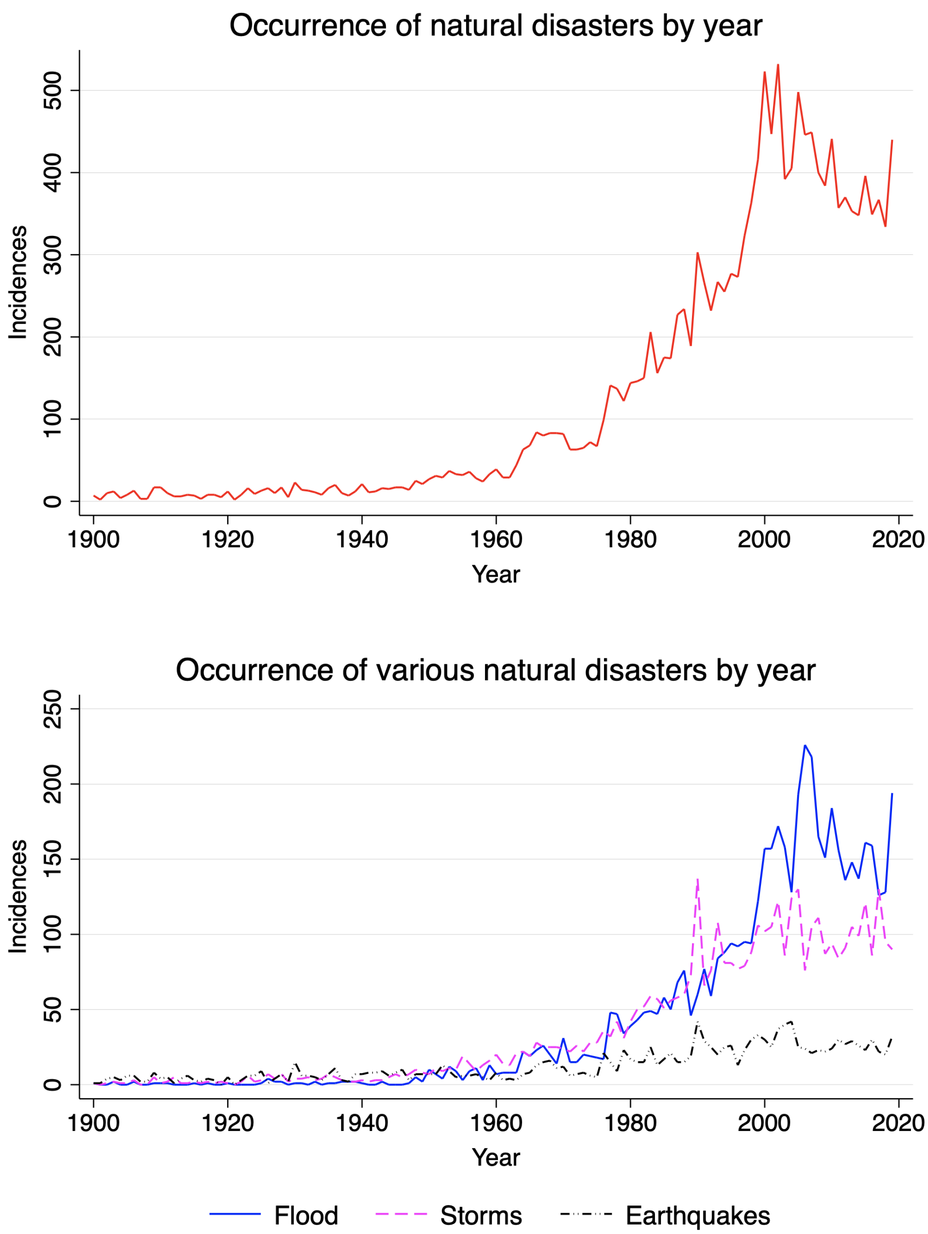
A Figures

Figure 1: Number of natural disasters by year since 1900



Source: EM-DAT database, maintained by the Center for Research of Epidemiology of Disasters in University of Louvain

Figure 2: Map of the affected areas

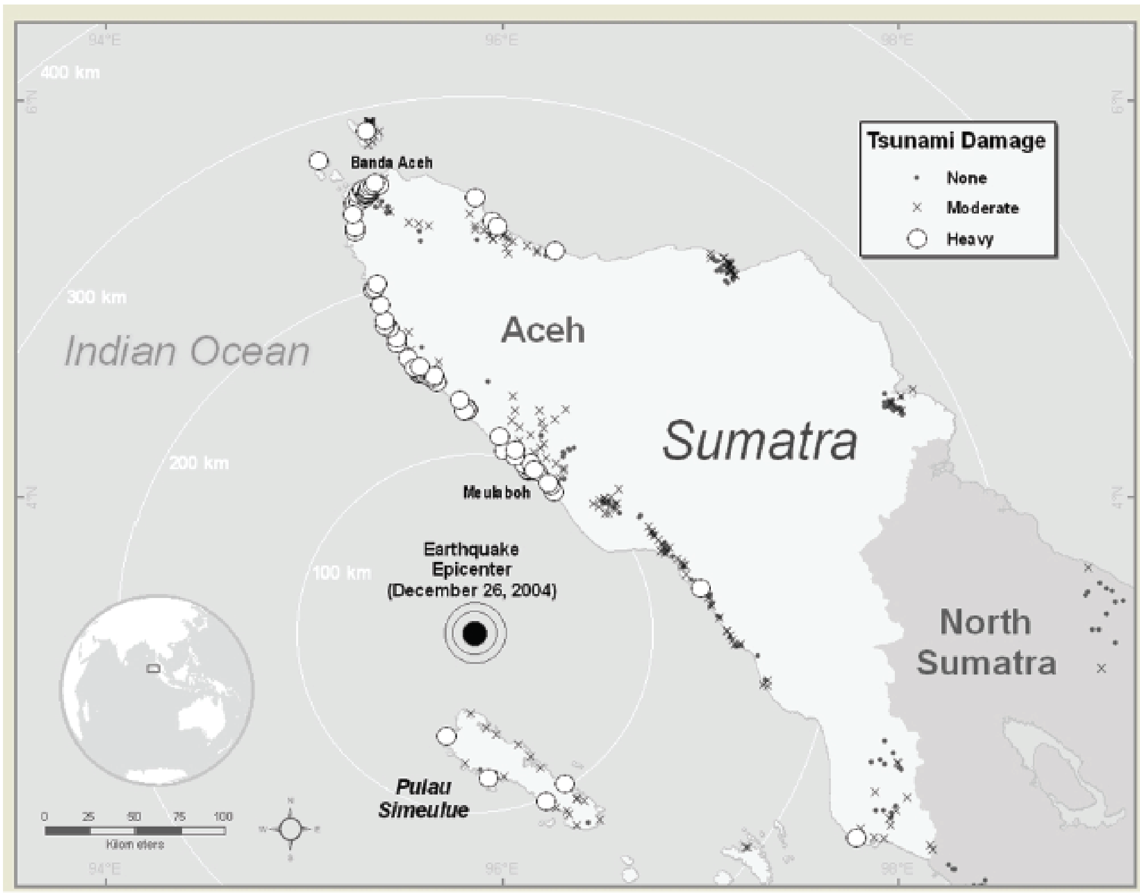
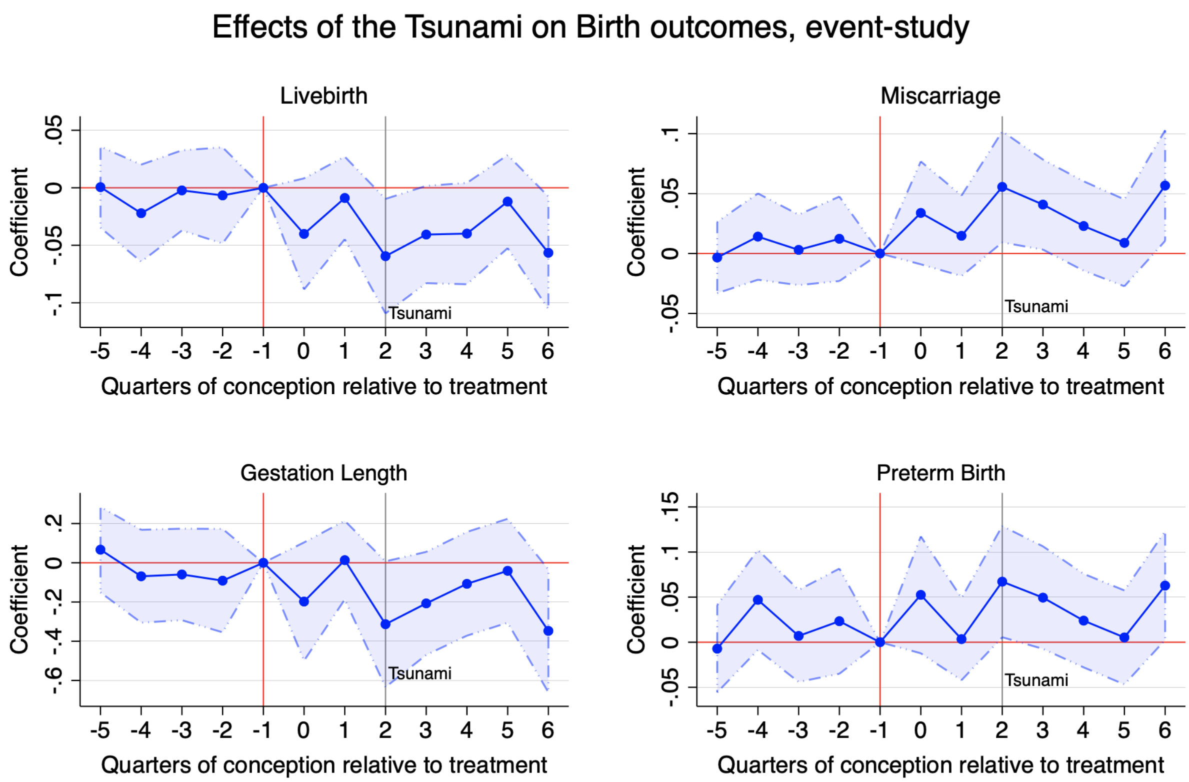


Figure 1. Map of Northern End of Sumatra Island, with Survey Clusters Coded by Degree of Damage

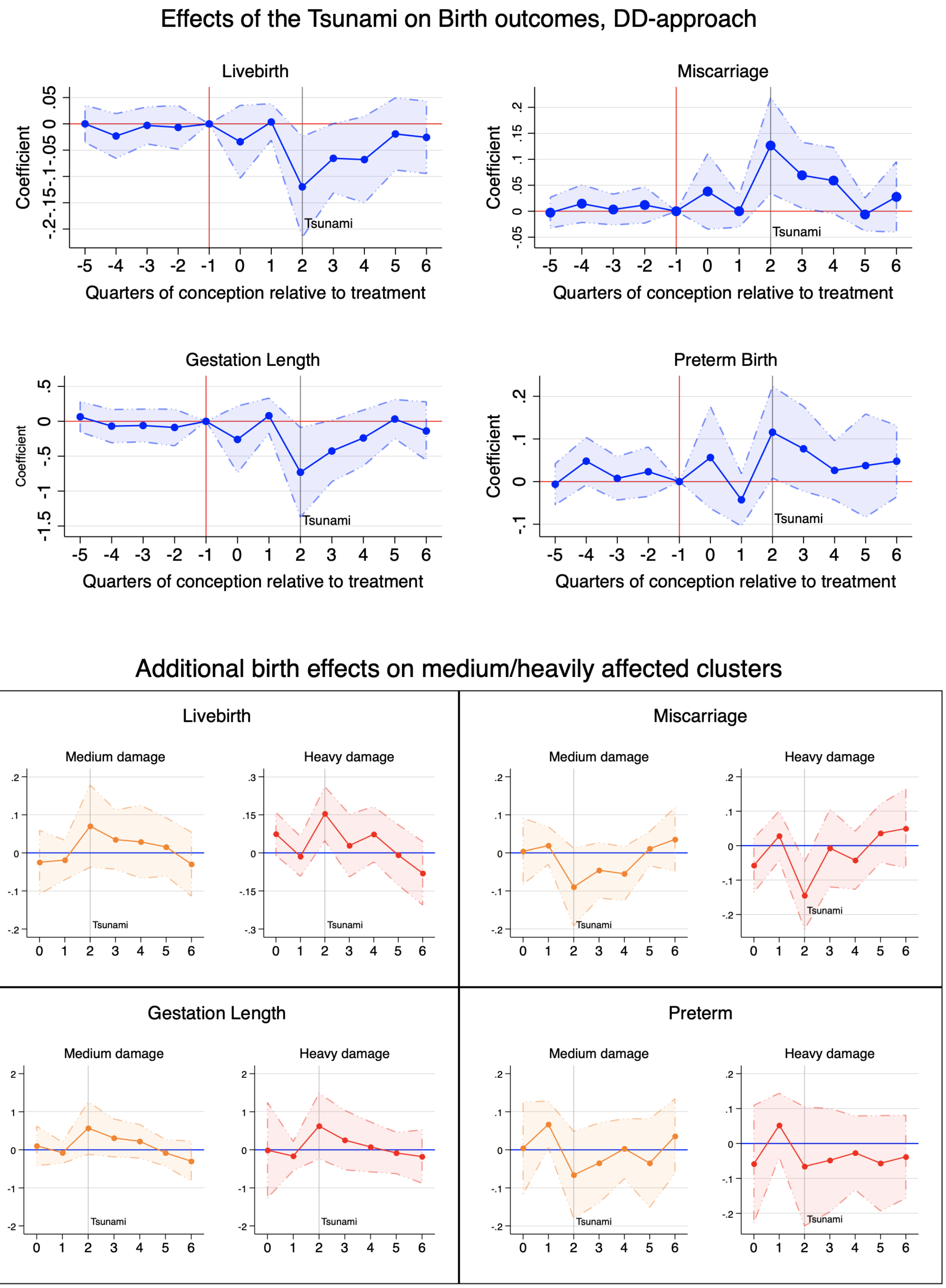
Source: Frankenberg, Elizabeth, Jenna Nobels, and Cecep Sumantri. 2012. "Community Destruction and Traumatic Stress in Post-Tsunami Indonesia." *Journal of Health and Social Behavior* 53 (4): 498–514

Figure 3: Birth effects of the Tsunami, event-study



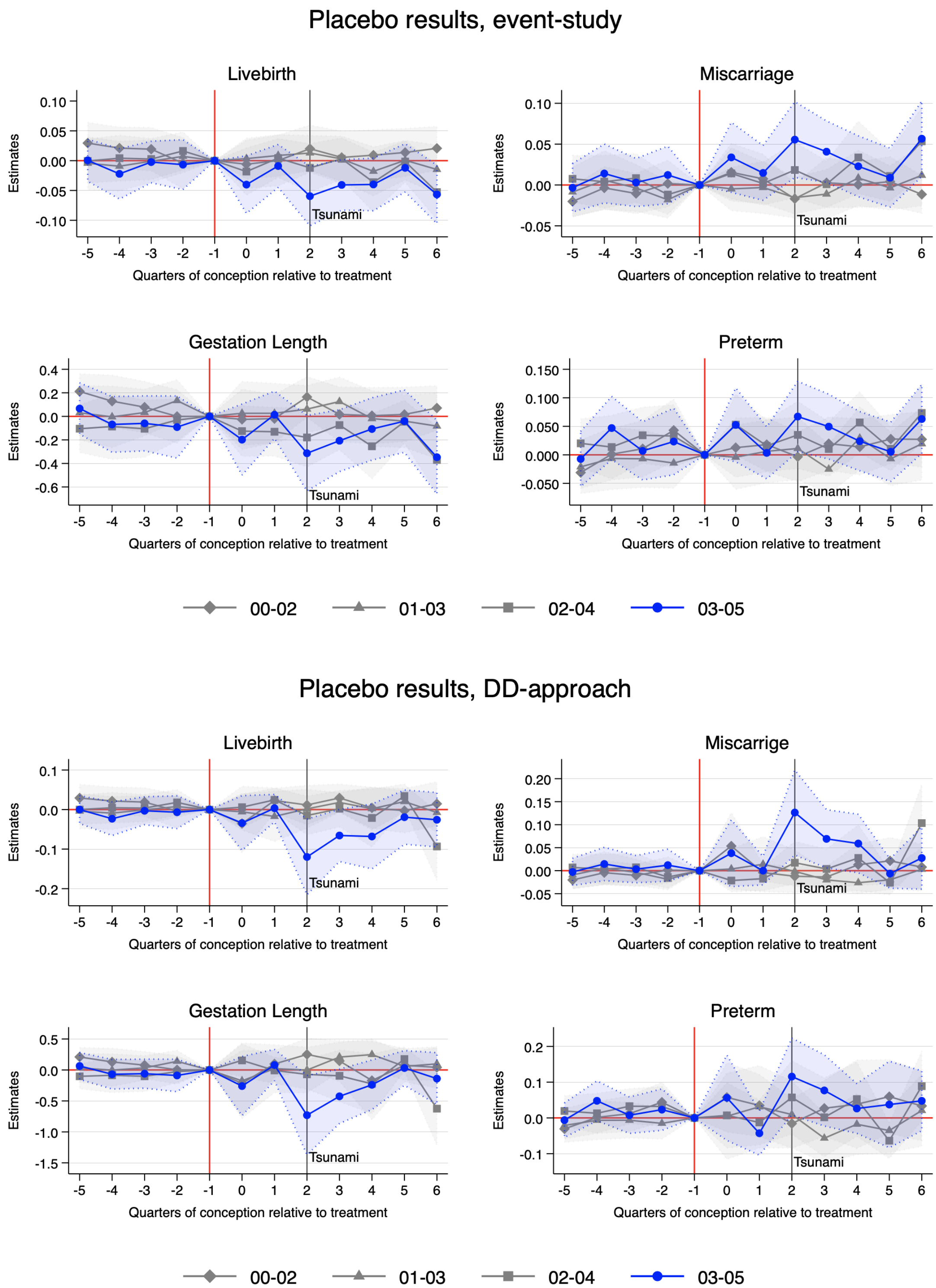
Note: Period 0 indicates those conceived in 2004Q2 and in the 3rd trimester of pregnancy at the point of the Tsunami, 95% confidence interval is included, where the circle markers indicate point estimates. Regression includes controls for mother's age at birth (level and squared), years of schooling (mother and father), birth order indicators, and cluster fixed effects. Standard errors are clustered at cluster-level. Observations: 2,159, Clusters: 108

Figure 4: Birth effects of the Tsunami, DD-approach



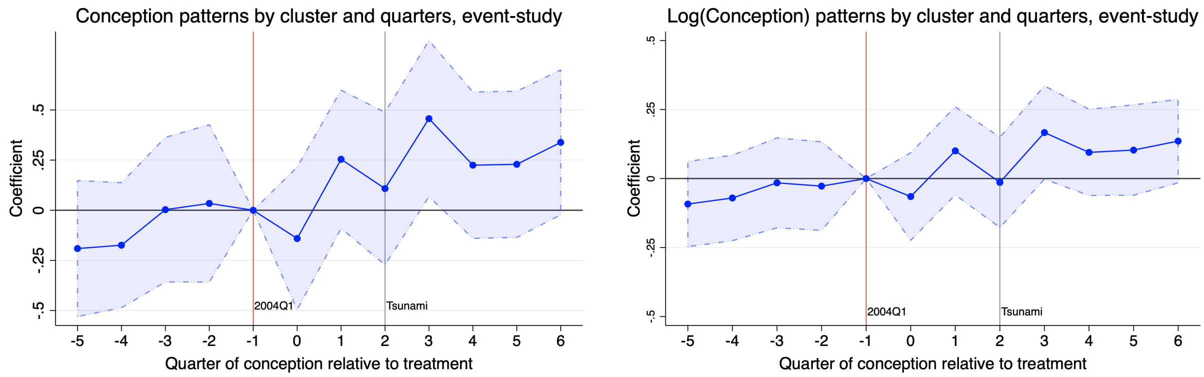
Note: Period 0 indicates those conceived in 2004Q2 and in the 3rd trimester of pregnancy at the point of the Tsunami, 95% confidence interval is included, where the blue, orange, and red circle markers indicate point estimates for β_t, δ_{2t}, and δ_{3t}, respectively. Regression includes controls for mother's age at birth (level and squared), years of schooling (mother and father), birth order indicators, cluster fixed effects as well. Standard errors are clustered at cluster-level. Observations: 2,159, Clusters: 108

Figure 5: Placebo results



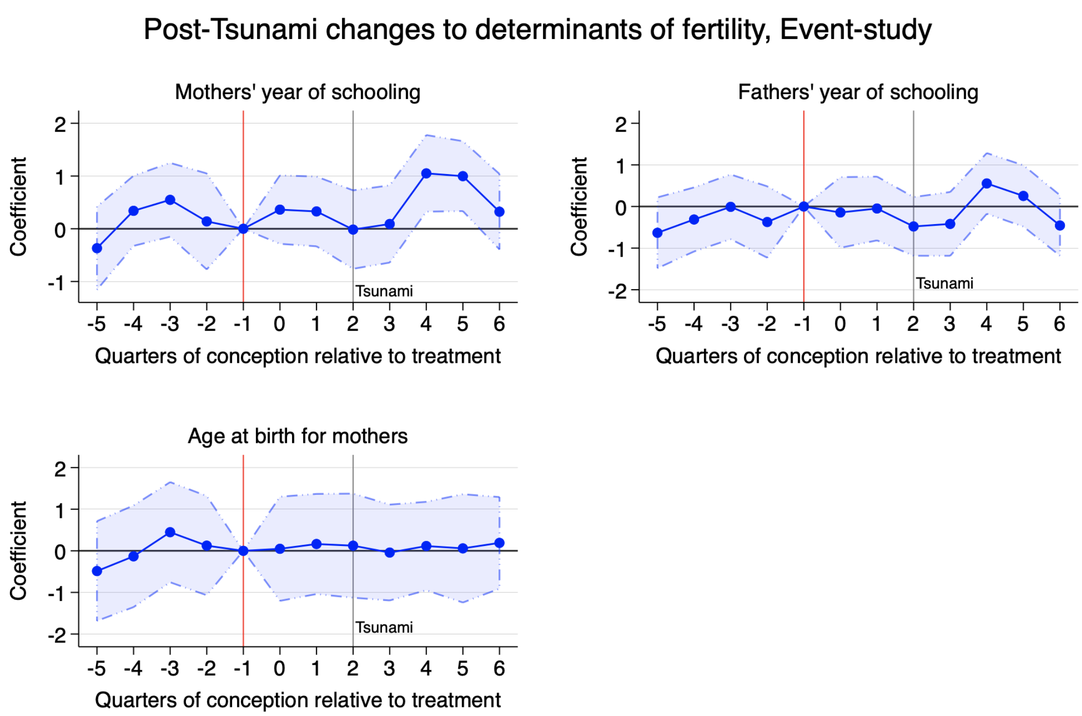
Note: Period 0 in the x-axes in both panels refers to the starting period of each treatment - 2001Q2, 2002Q2, 2003Q2, and 2004Q2 respectively. There are 108 clusters for all regressions and 1,888, 1,914, 2,007, and 2,159 observations in each regressions. Point estimates and 95% confidence interval are presented. The regressions use same control variables as in the main regressions in Section 5. Standard errors clustered at cluster-level.

Figure 6: Total conceptions before and after treatment, event-study



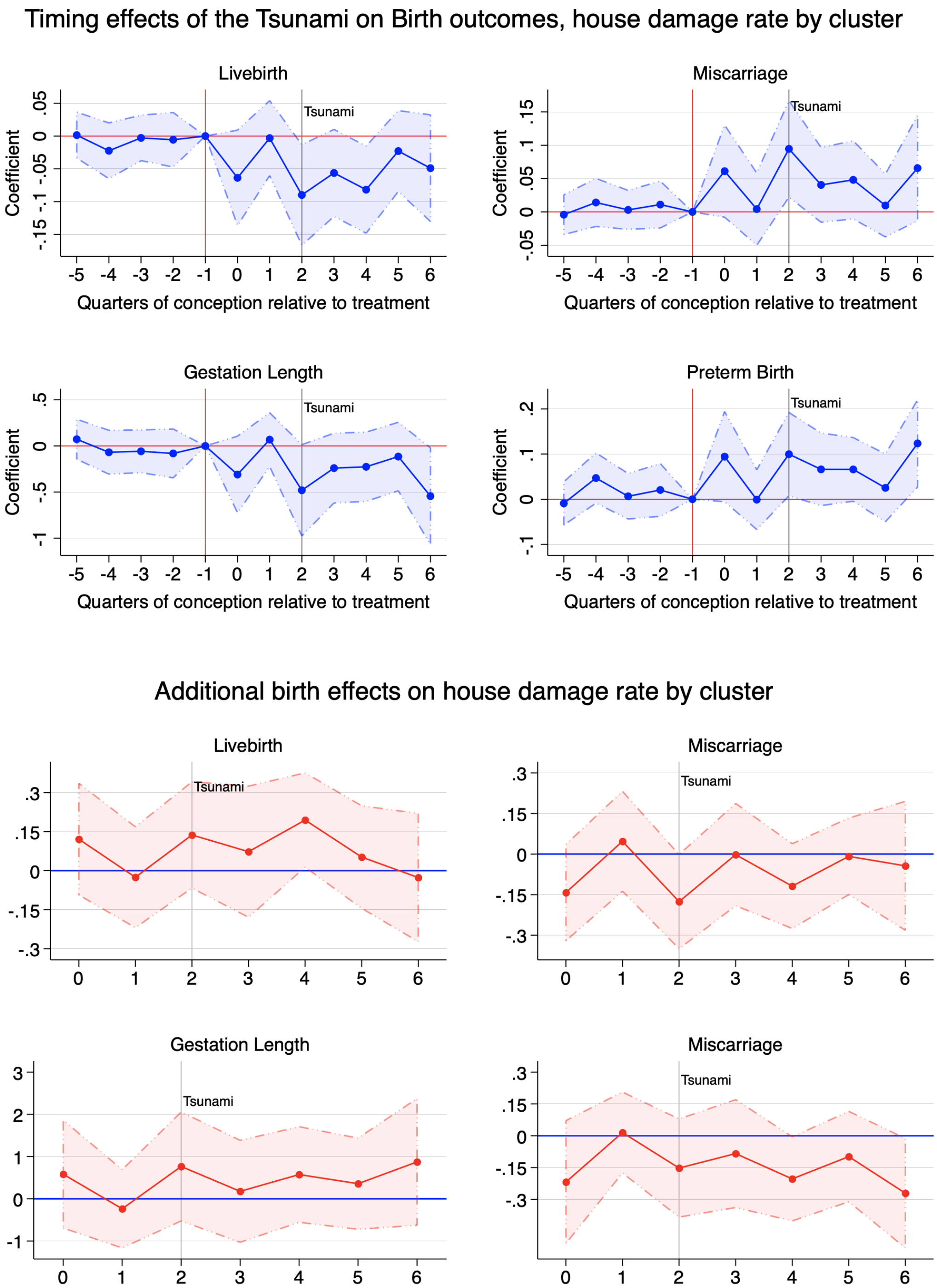
Note: Period 0 refers to the starting period of each treatment - 2004Q2. Point estimates and 95% confidence interval are presented. The regressions include controls for cluster fixed effects and year-quarter of conception dummies. Standard errors clustered at cluster-level. There are 1,002 observations and 108 clusters.

Figure 7: Testing for selection into fertility based on types, event-study



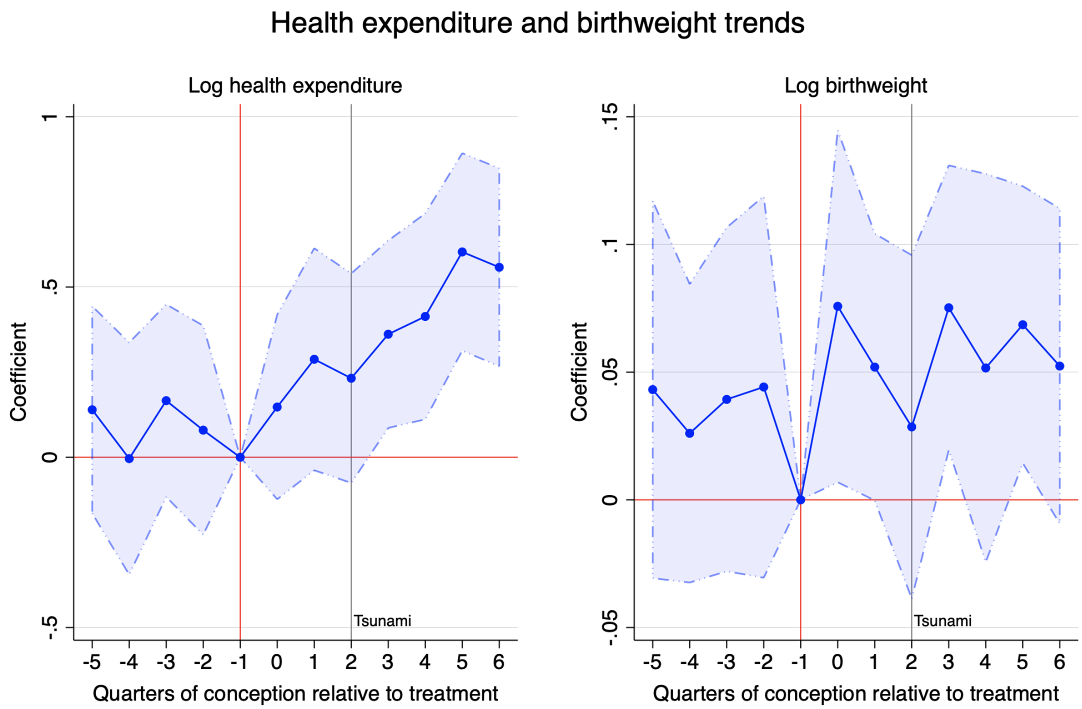
Note: Period 0 indicates those conceived on 2004Q2 and in the third trimester of pregnancy at the point of the Tsunami. 95% confidence interval is included, where the circle marker indicates point estimates. Regression includes controls for cluster fixed effects. Standard errors are clustered at cluster-level. Observations: 2,159, Clusters: 108

Figure 8: Birth effects of the Tsunami using damage rates across clusters



Note: Period 0 indicates those conceived on 2004Q2 and in the third trimester of pregnancy at the point of the Tsunami. 95% confidence interval is included, where the blue and red markers indicate point estimates for the time effect and the differential effect by the treatment intensity. Regressions include the same controls used in Section 5.2. Standard errors are clustered at cluster-level. Observations: 2,159, Clusters: 108

Figure 9: Household decisions on care vs selective survival of fetuses



Note: Period 0 indicates those conceived on 2004Q2 and in the third trimester of pregnancy at the point of the Tsunami. 95% confidence interval is included, where the circle markers indicate point estimates. Regressions include the same controls used in Section 5.1. Standard errors are clustered at cluster-level. There are 1,876 observations for health expenditures regression and 1,325 for birthweight regression. The number of clusters are 108 in both regressions.

B Tables

Table 1: Summary statistics, characteristics of clusters with different extent of damage

Category	Light	Medium	Heavy
Total number of clusters	31	53	24
Total respondents	583	1,269	307
Respondents whose house were damaged	80	463	208
Total households	396	870	219
Number of households damaged	42	220	127
House damage rate	0.1009	0.2131	0.4677

There are total of 2,159 respondents and 108 clusters in the sample, with 4 respondents not specifying whether their house was damaged or not. House damage rate is defined as the the proportion of the number of damaged households that are damaged within a given cluster and is used to characterize the treatment intensity on each cluster.

Table 2: Balance table: Light vs Medium vs Heavy damaged clusters

	(1) 0.Light		(2) 1.Medium		(3) 2.Heavy		T-test Difference		
Variable	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	(1)-(2)	(1)-(3)	(2)-(3)
Literate, wife	583	0.957	1269	0.902	307	0.915	0.055**	0.042	-0.013
	[31]	(0.008)	[53]	(0.020)	[24]	(0.044)			
Literate, husband	583	0.962	1269	0.953	307	0.935	0.010	0.027	0.018
	[31]	(0.008)	[53]	(0.010)	[24]	(0.030)			
Enrollment, wife	583	0.986	1269	0.943	307	0.964	0.043**	0.022	-0.021
	[31]	(0.004)	[53]	(0.016)	[24]	(0.028)			
Enrollment, husband	583	0.995	1269	0.990	307	0.974	0.005	0.021	0.016
	[31]	(0.003)	[53]	(0.003)	[24]	(0.022)			
Yrs of schooling, wife	583	8.652	1269	8.137	307	9.137	0.515	-0.485	-1.000
	[31]	(0.329)	[53]	(0.379)	[24]	(0.657)			
Yrs of schooling, husband	583	8.722	1269	8.607	307	9.498	0.115	-0.776	-0.892
	[31]	(0.266)	[53]	(0.281)	[24]	(0.558)			
Rural	583	0.777	1269	0.728	307	0.629	0.049	0.148	0.099
	[31]	(0.077)	[53]	(0.068)	[24]	(0.106)			
Age at birth, wife	583	27.883	1269	27.439	307	27.756	0.444	0.128	-0.317
	[31]	(0.325)	[53]	(0.182)	[24]	(0.331)			

Notes: The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at variable cluster. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. The observations included in this table reflect all pregnancies that began in calendar years 2003-05. Literate refers to the respondent being able to read and write Indonesian. Enrollment refers to receiving any level of schooling at an elementary school level or above. Year of schooling refers to the total number of years receiving education. Rural refers to the cluster of residence being classified as a rural area, as predetermined by the dataset. Age at birth is the wife's age at birth when giving birth.

Table 3: Balance table: Pre vs Post Tsunami

Variable	(1) Control N/[Clusters]	Mean/SE	(2) Treat N/[Clusters]	Mean/SE	T-test Difference (1)-(2)
Literate, wife	828	0.918	1331	0.920	-0.002
	[107]	(0.014)	[108]	(0.015)	
Literate, husband	828	0.955	1331	0.951	0.004
	[107]	(0.009)	[108]	(0.009)	
Enrollment, wife	828	0.953	1331	0.961	-0.008
	[107]	(0.011)	[108]	(0.010)	
Enrollment, husband	828	0.988	1331	0.989	-0.002
	[107]	(0.004)	[108]	(0.004)	
Yrs of schooling, wife	828	8.268	1331	8.512	-0.244
	[107]	(0.263)	[108]	(0.271)	
Yrs of schooling, husband	828	8.708	1331	8.800	-0.092
	[107]	(0.223)	[108]	(0.199)	
Rural	828	0.716	1331	0.734	-0.018
	[107]	(0.049)	[108]	(0.047)	
Age at birth, wife	828	27.536	1331	27.646	-0.110
	[107]	(0.213)	[108]	(0.169)	

Notes: The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at variable cluster. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. The observations included in this table reflect all pregnancies that began in calendar years 2003-05. Literate refers to the respondent being able to read and write Indonesian. Enrollment refers to receiving any level of schooling at an elementary school level or above. Year of schooling refers to the total number of years receiving education. Rural refers to the cluster of residence being classified as a rural area, as predetermined by the dataset. Age at birth is the wife's age at birth when giving birth.

Table 4: Summary statistics, 2003-05 conception cohort

Panel A. ***Averages for the key statistics***			
Category	Units	Average	Std.dev
Age at birth	Years	27.60	5.81
Year of education	Years	8.42	4.06
Year of education, husband	Years	8.76	5.80
Gestation length	Months	8.76	1.33
Panel B. ***Proportions of those belonging to each categories***			
Category	Total respondents	Respondents	Proportion(%)
Literate	2,159	1,984	91.89
Literate, husband	2,159	2,057	95.28
Enrollment	2,159	2,068	95.79
Enrollment, husband	2,159	2,135	98.89
Rural	2,159	1,570	72.72
Livebirth	2,159	2,050	94.95
Miscarriage	2,159	86	3.98
Preterm birth	2,159	162	7.50
Panel C. ***Breakdown of highest level of educational level attained, wife***			
Category	Total respondents	Respondents	Proportion(%)
Elementary school	2,068	786	37.62
Junior high school	2,068	499	24.13
Senior high school	2,068	538	26.01
Associate degree	2,068	147	7.11
Bachelor's degree	2,068	93	4.50
Masters/PhD	2,068	5	0.24
Panel D. ***Breakdown of level of educational level attained, husband***			
Category	Total respondents	Respondents	Proportion(%)
Elementary school	2,135	764	35.79
Junior high school	2,135	547	25.62
Senior high school	2,135	586	27.45
Associate degree	2,135	60	2.81
Bachelor's degree	2,135	168	7.87
Masters/PhD	2,135	10	0.47

Panel A collects averages and standard deviations of continuous variables used in the main regressions. The units of each statistics are specified in the second column. Panels B, C, and D collect the summary statistics of the binary variables used in the regressions. The proportion on the last column refers to the total share of respondents who answered "yes" to each of the variables. Those who did not receive any levels of education are not included in Panels C and D.

Table 5: Total pregnancies per year-quarter

Conception period	Treatment	Pregnancies
2003q1	Control	157
2003q2	Control	150
2003q3	Control	180
2003q4	Control	170
2004q1	Control	171
2004q2	Treated, exposed in utero	148
2004q3	Treated, exposed in utero	200
2004q4	Treated, exposed in utero	179
2005q1	Treated	212
2005q2	Treated	203
2005q3	Treated	206
2005q4	Treated	183
Total pregnancies in the main sample		2159

The first column indicates the year-quarter of conception. The first four digits indicate year of conception, while the last two characters indicate quarter of conception. The second column indicates the treatment status of conception in each year-quarter. The third column documents the total pregnancies recorded in each year-quarter.

Table 6: Birth effects of the Tsunami by timing of exposure

	Event-study				DD-approach			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conception period	Livebirth	Miscarriage	Duration	Preterm birth	Livebirth	Miscarriage	Duration	Preterm birth
2003q1	0.000563	-0.00335	0.0667	-0.00718	0.0000866	-0.00271	0.0646	-0.00618
	(0.0178)	(0.0150)	(0.110)	(0.0244)	(0.0178)	(0.0149)	(0.109)	(0.0243)
2003q2	-0.0221	0.0141	-0.0688	0.0470*	-0.0231	0.0146	-0.0694	0.0480*
	(0.0213)	(0.0181)	(0.119)	(0.0280)	(0.0214)	(0.0182)	(0.120)	(0.0281)
2003q3	-0.00227	0.00302	-0.0595	0.00696	-0.00296	0.00326	-0.0599	0.00756
	(0.0176)	(0.0148)	(0.118)	(0.0257)	(0.0178)	(0.0149)	(0.118)	(0.0256)
2003q4	-0.00662	0.0123	-0.0910	0.0233	-0.00668	0.0121	-0.0884	0.0233
	(0.0211)	(0.0178)	(0.133)	(0.0294)	(0.0209)	(0.0176)	(0.132)	(0.0293)
2004q1	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2004q2	-0.0401	0.0339	-0.198	0.0525	-0.0341	0.0381	-0.260	0.0565
	(0.0243)	(0.0216)	(0.152)	(0.0327)	(0.0349)	(0.0366)	(0.241)	(0.0602)
2004q3	-0.00888	0.0148	0.0135	0.00345	0.00358	0.0000511	0.0800	-0.0428
	(0.0181)	(0.0169)	(0.101)	(0.0230)	(0.0175)	(0.0156)	(0.127)	(0.0310)
2004q4	-0.0595**	0.0556**	-0.313*	0.0672**	-0.120**	0.126***	-0.730**	0.116**
	(0.0251)	(0.0234)	(0.162)	(0.0311)	(0.0482)	(0.0466)	(0.324)	(0.0543)
2005q1	-0.0406*	0.0408**	-0.207	0.0495*	-0.0655*	0.0692**	-0.425*	0.0769
	(0.0213)	(0.0189)	(0.132)	(0.0287)	(0.0332)	(0.0320)	(0.221)	(0.0503)
2005q2	-0.0398*	0.0229	-0.107	0.0239	-0.0680	0.0591*	-0.239	0.0264
	(0.0222)	(0.0188)	(0.133)	(0.0262)	(0.0414)	(0.0321)	(0.202)	(0.0350)
2005q3	-0.0120	0.00889	-0.0408	0.00533	-0.0192	-0.00631	0.0325	0.0376
	(0.0204)	(0.0183)	(0.134)	(0.0263)	(0.0346)	(0.0160)	(0.141)	(0.0610)
2005q4	-0.0565**	0.0567**	-0.347**	0.0628**	-0.0257	0.0276	-0.139	0.0478
	(0.0247)	(0.0233)	(0.158)	(0.0304)	(0.0347)	(0.0342)	(0.211)	(0.0422)
Obs.	2159	2159	2159	2159	2159	2159	2159	2159
No. of Clusters	108	108	108	108	108	108	108	108
Damage × Post	No	No	No	No	Yes	Yes	Yes	Yes

* $p < .10$, ** $p < .05$, *** $p < .01$

Year-quarters in the first column indicate the period of conception. Coefficients for the year-quarter dummies are reported in the tables, with those for 2004Q1 normalized to 0. There are controls for age of mother (level and squared) at birth, year of schooling of both the mother and the father, indicators for birth order, cluster fixed effects. Standard errors are in the parentheses and are clustered at the cluster zone level.

Table 7: Summary statistics for main and placebo cohorts

	2000-02	2001-03	2002-04	2003-05
Literacy(mother)	.92	.917	.917	.919
Literacy(father)	.945	.953	.956	.953
Enrollment(mother)	.956	.955	.951	.958
Enrollment(father)	.982	.985	.986	.989
Years of schooling (mother)	8.29	8.33	8.28	8.42
Years of schooling (father)	8.63	8.66	8.67	8.76
Mother's age at birth	26.9	27.1	27.4	27.6
Rural	.712	.706	.719	.727
Observation	1,888	1,914	2,007	2,159

Note: The unit of years of schooling and mother's age variables are in years. As for other variables, the number indicates the proportion of respondents, ranging from 0 to 1, that responded yes to each category. The observations included in this table reflect all pregnancies that began in calendar years specified in the top of each column. Literate refers to the respondent being able to read and write Indonesian. Enrollment refers to receiving any level of schooling at an elementary school level or above. Year of schooling refers to the total number of years receiving education. Rural refers to the cluster of residence being classified as a rural area, as predetermined by the dataset. Age at birth is the wife's age at birth when giving birth.

Table 8: Regression on maternal health indicators

	(1) Fear of death	(2) Fear of injury	(3) Disturbed	(4) Anxiety	(5) Parent death	(6) same health	(7) worse health	(8) outpatient care
2003q1	-0.0364	-0.0873*	-0.0199	0.0658	-0.0116	-0.0612*	0.0284	0.0510
	(0.0557)	(0.0494)	(0.0560)	(0.0463)	(0.0183)	(0.0341)	(0.0286)	(0.0376)
2003q2	0.0512	-0.0168	-0.0912*	0.0339	-0.0164	0.0136	0.00324	0.0996**
	(0.0468)	(0.0458)	(0.0511)	(0.0423)	(0.0181)	(0.0217)	(0.0210)	(0.0429)
2003q3	0.114***	0.0573*	-0.0375	0.0809*	0.0107	-0.0623*	0.0351	0.0442
	(0.0414)	(0.0341)	(0.0527)	(0.0464)	(0.0226)	(0.0334)	(0.0266)	(0.0429)
2003q4	0.0194	-0.0421	-0.0398	0.00945	-0.0149	-0.0732**	0.0596*	0.0687*
	(0.0466)	(0.0379)	(0.0612)	(0.0565)	(0.0188)	(0.0349)	(0.0329)	(0.0404)
2004q1	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2004q2	-0.00843	-0.0514	-0.0123	0.0754	-0.00736	-0.0811*	0.0443	0.0651
	(0.0442)	(0.0452)	(0.0620)	(0.0520)	(0.0189)	(0.0431)	(0.0332)	(0.0394)
2004q3	0.0571	-0.00380	-0.0420	0.0504	0.0166	-0.0688**	0.0772***	0.0257
	(0.0457)	(0.0483)	(0.0540)	(0.0423)	(0.0209)	(0.0299)	(0.0275)	(0.0366)
2004q4	0.0859**	0.0143	-0.0315	0.0564	-0.0248	-0.0654**	0.0631**	0.0928**
	(0.0425)	(0.0460)	(0.0508)	(0.0463)	(0.0158)	(0.0307)	(0.0263)	(0.0437)
2005q1	0.0432	-0.0126	-0.0839	0.0368	-0.0306**	-0.0508	0.0328	0.0306
	(0.0425)	(0.0402)	(0.0514)	(0.0401)	(0.0148)	(0.0317)	(0.0236)	(0.0406)
2005q2	0.0373	0.00921	-0.0757	0.0576	0.00356	-0.0609*	0.0375	0.0331
	(0.0461)	(0.0395)	(0.0468)	(0.0424)	(0.0196)	(0.0357)	(0.0288)	(0.0359)
2005q3	0.0687*	0.0739**	-0.115**	0.0810*	-0.0148	-0.121***	0.0967***	0.107***
	(0.0391)	(0.0355)	(0.0510)	(0.0455)	(0.0189)	(0.0360)	(0.0328)	(0.0370)
2005q4	0.105**	0.0208	-0.0365	-0.0372	-0.0211	-0.0656**	0.0466	0.0792*
	(0.0476)	(0.0398)	(0.0510)	(0.0382)	(0.0173)	(0.0325)	(0.0322)	(0.0411)
Obs.	2159	2159	2159	2159	2159	2159	2159	2159
No. of Clusters	108	108	108	108	108	108	108	108

* $p < .10$, ** $p < .05$, *** $p < .01$

The following outcomes are used: Fear of death, fear of injury, being disturbed, anxiety about future, death of a parent, same health condition, worse health condition, and outpatient services usage one month before. Year-quarters in the first column indicate the period of conception. Coefficients for the year-quarter dummies are reported in the tables. Coefficients for the pre-treatment year-quarters are included in the regression but omitted from the above table. There are controls for age of mother (level and squared) at birth, year of schooling of both the mother and the father, indicators for birth order, cluster fixed effects. Standard errors are in the parentheses and are clustered at the cluster zone level.

Table 9: Summary statistics, Mechanism variables

Panel A. ***Averages for key statistics***			
Category	Unit	Average	Std.dev
Health spending	Indonesian Rupiah	457,269	1,173,517
Prenatal care spending	Indonesian Rupiah	172,460	346,252
Prenatal care visits	Number	6.37	3.56
Birthweight	Kg	3.2	.693
All transfers	Indonesian Rupiah	1,072,472	3,820,265
Gov't transfers	Indonesian Rupiah	466,174	1,897,235
NGO transfers	Indonesian Rupiah	477,793	2,966,257
Loss in housing value	Indonesian Rupiah	2,793,957	12,902,459
Panel B. ***Proportions of those belonging to each categories***			
Category	Total respondents	Respondents	Proportion(%)
Birth at own or family's house	2,055	1333	64.86
Disturbing memories	2,159	1072	49.65
Anxiety about future	2,159	549	25.43
Fear of death	2,159	1579	73.14
Fear of injury	2,159	1749	81.01
Parent death	2,159	82	3.80
Same health	2,159	1822	84.39
Worse health	2,159	237	10.98
Outpatient care	2,159	443	20.52
Moved out	2,159	839	38.86
Wife unemployed	1,065	242	22.72
Husband unemployed	2,090	711	34.02
Brick wall	2,103	801	38.09
Wooden wall	2,103	1273	60.53
Iron roof	2,103	1532	72.85
Dirt floor	2,102	153	7.28

Panel A collects averages and standard deviations of continuous outcome variables used in mechanism tests. The units of each statistics are specified in the second column. Panel B collects the summary statistics of the binary variables used in the mechanism tests. The proportion on the last column refers to the total share of respondents who answered "yes" to each of the variables.

Table 10: Migration status, aid, and effects of the Tsunami

	(1)	(2)	(3)	(4)
	Mover	log(all aid)	log(govt aid)	log(ngo aid)
2004q2	-0.0686	-0.212	-0.479	2.717***
	(0.0635)	(0.310)	(0.321)	(0.405)
2004q2 × 1.Medium	0.0811	-0.192	0.128	-2.643***
	(0.0763)	(0.361)	(0.353)	(0.456)
2004q2 × 2.Heavy	-0.114	0.174	0.0980	-1.318***
	(0.164)	(0.455)	(0.398)	(0.467)
2004q3	-0.0676	-0.181	-0.398	0.160
	(0.0566)	(0.300)	(0.247)	(0.510)
2004q3 × 1.Medium	0.110	-0.0701	0.0585	0.198
	(0.0684)	(0.327)	(0.288)	(0.518)
2004q3 × 2.Heavy	-0.0169	0.494	0.279	0.391
	(0.0779)	(0.435)	(0.418)	(0.558)
2004q4	0.0115	-0.197	-0.256	0.563
	(0.0735)	(0.249)	(0.245)	(0.455)
2004q4 × 1.Medium	-0.0241	0.132	0.0142	-0.365
	(0.0778)	(0.289)	(0.271)	(0.394)
2004q4 × 2.Heavy	-0.206	0.409	0.0212	-0.0657
	(0.124)	(0.338)	(0.335)	(0.391)
Obs.	2159	1508	1341	470
No. of Clusters	108	108	107	79

* $p < .10$, ** $p < .05$, *** $p < .01$

The first column is the regression result with the indicator variable for those who moved out since the Tsunami. The last three columns represent results from the regression with the log of all aid, log of government aid, and log of aid from NGO as outcomes. The coefficients for the conception period dummy for the second-fourth quarters of 2004 and those interacted with two levels of damage indicators are reported in this table. All other periods have been omitted for the presentation. The time periods in the first column indicates the period of conception. Standard errors are in the parentheses and are clustered at the cluster-level. There are controls for age of mother (level and squared) at birth, year of schooling of both the mother and the father, and indicator for birth order. The regressions include fixed effects for cluster zones as well as interaction between degrees of damage and treated period year quarters.

Table 11: Regression on household choices on care and selective birth

	(1) log(hlth spend)	(2) log(care spend)	(3) log(visits)	(4) own/family house	(5) Birthweight	(6) log(birthweight)	(7) VLBW	(8) LBW
2003q1	0.140	-0.0228	0.0303	0.0239	0.125	0.0432	-0.0104	-0.0516
	(0.153)	(0.127)	(0.0544)	(0.0474)	(0.106)	(0.0373)	(0.0240)	(0.0571)
2003q2	-0.00393	0.0802	0.0488	-0.0737	0.0740	0.0260	-0.00446	-0.0362
	(0.172)	(0.136)	(0.0655)	(0.0497)	(0.0786)	(0.0295)	(0.0209)	(0.0487)
2003q3	0.166	-0.135	0.00153	-0.0140	0.109	0.0393	-0.0235	-0.0449
	(0.143)	(0.129)	(0.0582)	(0.0533)	(0.0922)	(0.0340)	(0.0187)	(0.0558)
2003q4	0.0796	0.0224	0.0447	-0.0923*	0.117	0.0442	-0.0225	-0.0693
	(0.155)	(0.128)	(0.0554)	(0.0479)	(0.103)	(0.0377)	(0.0211)	(0.0585)
2004q1	0	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
2004q2	0.148	0.00284	-0.0833	-0.138**	0.224**	0.0758**	-0.0272	-0.0535
	(0.137)	(0.123)	(0.0686)	(0.0567)	(0.0991)	(0.0348)	(0.0169)	(0.0591)
2004q3	0.288*	0.163	0.0381	-0.0699	0.125	0.0520*	-0.0238	-0.121***
	(0.164)	(0.135)	(0.0531)	(0.0450)	(0.0757)	(0.0263)	(0.0157)	(0.0387)
2004q4	0.232	0.0674	-0.0603	-0.0766*	0.0825	0.0286	-0.00567	-0.0459
	(0.155)	(0.144)	(0.0715)	(0.0426)	(0.0922)	(0.0339)	(0.0205)	(0.0552)
2005q1	0.361**	0.121	-0.0263	-0.130**	0.237***	0.0752***	-0.0151	-0.0972*
	(0.139)	(0.123)	(0.0663)	(0.0497)	(0.0812)	(0.0281)	(0.0176)	(0.0522)
2005q2	0.414***	0.168	0.00556	-0.0653	0.168	0.0516	0.00276	-0.0460
	(0.152)	(0.137)	(0.0617)	(0.0475)	(0.107)	(0.0383)	(0.0245)	(0.0534)
2005q3	0.603***	0.0635	-0.0129	-0.0387	0.177**	0.0686**	-0.0304*	-0.111**
	(0.146)	(0.130)	(0.0523)	(0.0414)	(0.0732)	(0.0274)	(0.0178)	(0.0474)
2005q4	0.558***	0.00854	-0.0952	-0.131**	0.155*	0.0524*	-0.0138	-0.0480
	(0.147)	(0.115)	(0.0601)	(0.0539)	(0.0889)	(0.0311)	(0.0196)	(0.0549)
Obs.	1876	1493	1956	2055	1325	1325	1325	1325
No. of Clusters	107	108	108	108	107	107	107	107

* $p < .10$, ** $p < .05$, *** $p < .01$

Each column represents the regression results with log of household health expenditures, log of amount spent on prenatal care, log of number of visits to prenatal care centers, giving birth at own or family member's house, birthweight, log of birthweight, very low birthweight, and lowbirthweight as outcome variables. Year-quarters in the first column indicate the period of conception. Coefficients for the year-quarter dummies are reported in the tables, with those for 2004Q1 normalized to 0. There are controls for age of mother (level and squared) at birth, year of schooling of both the mother and the father, indicators for birth order, cluster fixed effects. Standard errors are in the parentheses and are clustered at the cluster zone level.

Table 12: Socioeconomic status and effects of the Tsunami

	(1) log(house loss)	(2) Wife unemp	(3) husb. unemp	(4) Brick wall	(5) Wood wall	(6) Iron roof	(7) Dirt floor
2004q2	-0.466	0.0556	-0.0201	0.0248	-0.0295	0.114^{*}	0.00262
	(0.358)	(0.101)	(0.0573)	(0.0746)	(0.0761)	(0.0588)	(0.0447)
2004q2 × 1.Medium	-0.0178	-0.102	0.0104	0.0828	-0.0966	-0.0321	0.0209
	(0.518)	(0.116)	(0.0807)	(0.0848)	(0.0863)	(0.0598)	(0.0573)
2004q2 × 2.Heavy	0.499	-0.0895	0.161	-0.0615	0.109	0.132	0.0187
	(0.495)	(0.113)	(0.129)	(0.121)	(0.130)	(0.137)	(0.0940)
2004q3	0.669^{**}	-0.0521	-0.0573	-0.0181	0.0330	0.0283	0.0555
	(0.310)	(0.121)	(0.0520)	(0.0664)	(0.0665)	(0.0779)	(0.0535)
2004q3 × 1.Medium	-0.638^{**}	0.00427	0.0735	0.104	-0.0978	0.0000734	-0.0630
	(0.307)	(0.122)	(0.0652)	(0.0793)	(0.0803)	(0.0776)	(0.0591)
2004q3 × 2.Heavy	-0.652^{*}	-0.00356	-0.0424	0.149	-0.143	0.0894	-0.157^{***}
	(0.358)	(0.162)	(0.120)	(0.105)	(0.102)	(0.128)	(0.0584)
2004q4	0.912^{*}	-0.0300	-0.0853^{*}	-0.00999	0.0108	-0.0366	0.0103
	(0.461)	(0.106)	(0.0499)	(0.0735)	(0.0702)	(0.0699)	(0.0399)
2004q4 × 1.Medium	-0.595	-0.0154	0.126^{**}	0.0382	-0.0260	0.118^{*}	-0.0438
	(0.452)	(0.129)	(0.0632)	(0.0804)	(0.0779)	(0.0697)	(0.0415)
2004q4 × 2.Heavy	-0.624	0.0949	0.209	0.263^{*}	-0.236	0.100	-0.0571
	(0.520)	(0.257)	(0.138)	(0.153)	(0.151)	(0.118)	(0.0745)
Obs.	446	1065	2090	2103	2103	2103	2102
No. of Clusters	72	107	108	108	108	108	108

* $p < .10$, ** $p < .05$, *** $p < .01$

The first column is the regression result with loss of housing value due to the Tsunami as outcome variable. Columns (2) and (3) regress the post-Tsunami unemployment for mother and father. The outcome variable for last four columns are housing material.The coefficients for the conception period dummy for the second-fourth quarters of 2004 and those interacted with two levels of damage indicators are reported in this table. All other periods have been omitted for the presentation. The time periods in the first column indicates the period of conception. Standard errors are in the parentheses and are clustered at the cluster-level. There are controls for age of mother (level and squared) at birth, year of schooling of both the mother and the father, and indicator for birth order. The regressions include fixed effects for cluster zones as well as interaction between degrees of damage and treated period year quarters.