Childbirth Effects of the 2004 Indian Ocean Tsunami

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Abstract

This paper evaluates the effect of in utero exposure to the 2004 Indian Ocean Tsunami on short-term childbirth outcomes in Indonesia. Exploiting variation in the timing of exposure, we find that the probability of a successful pregnancy drops by 5.9 percentage points (pp), while miscarriages increase by 5.5 pp for those exposed in the earliest stage of pregnancy. We find suggestive evidence that post-disaster health investments by households may have shielded later cohorts from harmful effects. 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

1 Introduction

Because of climate change, large-scale natural disasters are becoming recurrent and more severe[1]. These natural disasters pose an increasingly serious threat to health and educational opportunities. They can do so directly, through death, injuries, and mental trauma, as well as indirectly by destroying infrastructure supplying health care and educational services (Currie and Rossin-Slater 2013; Kousky 2016). Children in developing countries are particularly vulnerable to the potential threats of natural disasters. Children's long-run health and labor market outcomes can be irreversibly affected by absorbing the negative shocks that affect their mothers while pregnant (Almond and Currie 2011; Barker 1992; Black et al. 2019; Guantai and Kijima 2020; Ramirez and Haas 2021)[2]. Developing countries have difficulty insuring against possible costs and face more difficulties in recovering from the damages incurred by natural disasters, despite being just as likely to face them as developed countries (Kahn 2005; Strömberg 2007). Despite the sizable harms potentially imposed by natural disasters, little is known about the effects of in utero exposure to these events in a developing country context, where relevant data only recently became available.

In this study, we investigate how in utero exposure to natural disasters affects birth outcomes in a developing country. 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'). While the casualties and the negative effects on health and educational progress of various population groups are well-documented (Shaw 2015; Frankenberg et al. 2020b; Cas et al. 2014), the effect of the Tsunami on the birth outcomes and health of those who were unborn at the time are not yet fully understood. By studying various immediate birth outcomes, we aim to complement the existing 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.

We utilize the Tsunami as a natural experiment whose treatment varies across timing of exposure

[1]Indeed, records from the Emergency Events Database (EM-DAT) show that the number of natural disasters per year has risen by 10 times since 1960s. 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]Although much of the findings are from developed countries, 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.

and communities. The dataset we use extensively tracks the life trajectories of the individuals exposed to the Tsunami, which is rare in other publicly-available datasets. From this dataset, we use pregnancy records from individuals in the affected provinces, including the ones conceived before the disaster. We primarily employ an event-study regression that identifies the effects on birth outcomes across the timing of exposure and isolates the different birth outcomes between those exposed to the Tsunami and those unaffected by the Tsunami. We also provide complementary specifications where we incorporate regional variation in damage intensities using a difference-in-differences approach. Finally, we use other outcomes such as health expenditures and maternal health indicators to unearth mechanisms driving the treatment effect and provide suggestive evidence on what policymakers could do to minimize negative birth effects.

We find that there is a negative impact on birth outcomes due to the Tsunami, especially for those in the earliest stage of pregnancy at the time. Pregnancies in the first trimester when the Tsunami struck are 5.9 percentage points (pp) less likely to end in livebirths and 5.5 pp more likely to end in miscarriages compared to those that began in the last pre-treatment period[3]. The probability 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 fetuses conceived after the Tsunami or those in later stages of pregnancies. However, there are no consistent findings suggesting that the birth outcomes differ depending on the varying extent of damages across locations. The results are robust to a battery of tests including placebo tests, selective migration, and changes in total conception trends, among others. There is suggestive evidence from household health expenditure patterns that investments after the Tsunami could be offsetting the negative effects of the Tsunami. We also find that our results are not likely to be driven by maternal distress and socioeconomic statuses.

We contribute to three strands of literature. First, we aim to complement the literature on the human cost of natural disasters by utilizing indicators relevant and available for developing countries. Currie and Rossin-Slater (2013), Karbownik and Wray (2019), and Imberman et al. (2012) use indices exclusively available in developed countries to study the damaging effects of natural disasters[4]. Be-

[3]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.

[4]Currie and Rossin-Slater (2013) uses meconium aspiration syndrom to measure abnormal conditions at birth of those exposed to hurricanes in the US. Karbownik and Wray (2019) uses a battery of labor market outcomes to capture long-run effects of in-utero exposure to hurricanes. Karbownik and Wray (2019) documents the changes in student achievement due to changes in composition of students due to Hurricane Katrina using enrollment and student performance data found in administrative data.

cause of limited data availability, there are shortcomings in using these measures to capture costs of adverse events such as natural disasters in developing countries (Guantai and Kijima 2020). We use data on fetal deaths, which are obtainable and more relevant in developing countries due to the higher likelihood of such events (Institute of Medicine 2003; Weinhold 2009). With these indicators, we identify the damaging effects of natural disasters on developing countries that may not occur in developed countries.

We relate to the literature on causal mechanisms behind negative effects of in-utero exposure to adverse events. We analyze whether factors known to explain adverse outcomes arising from in-utero exposures, including maternal distress and socioeconomic factors, also applies to similar events in developing countries (Almond et al. 2009; Black et al. 2019; Currie and Rossin-Slater 2013; Ramirez and Haas 2021). With relevant panel data becoming more accessible in developing countries, researchers are beginning to unearth similar outcomes of in-utero exposure in developing countries, (de Oliveira et al. 2021; Guantai and Kijima 2020; Koppensteiner and Manacorda 2016; Rosales-Rueda and Triyana 2019; Torche 2011). However, not all works on developing countries are able to identify mechanisms behind the birth effects. We contribute to this literature by testing whether mechanisms that apply to developed countries can be extended to developing countries. We also provide suggestive mechanisms that are more relevant for developing countries.

Lastly, this paper contributes to the literature studying recovery efforts of individuals and communities in developing countries. Economists concur that individuals and communities in developing countries recover slower than those in developed countries due to lack of resources and institutional capacity (Kahn 2005; Kellenberg and Mobarak 2008). Gignoux and Menéndez (2016) and Lépine et al. (2021) highlight the role of external aid in rebuilding infrastructure and mitigating negative health effects. Deryugina et al. (2018) and Zhang (2018) point out that individual-level responses such as migration help mitigate further human costs of disasters. Bhalotra (2007) and Paxson and Schady (2005) explore the role of public and private expenditures on health in addressing mortality at times of crises. We note an inverse association between post-disaster household health expenditures and negative birth effects. We find this as suggestive evidence that policies facilitating households to spend on mitigation efforts can reduce costs of natural disasters.

The rest of the paper is organized as follows. We review the events and papers addressing the demographic impacts of the 2004 Indian Ocean Tsunami in Section 2. We introduce the dataset and the identification strategies in Sections 3 and 4. We present the empirical results in Section 5. We conduct various robustness tests on our results in Section 6. In Section 7, we discuss various mechanisms driving the treatment effect. We conclude this paper in Section 8.

2 Background: The Impact of the 2004 Indian Ocean Tsunami

The Indian Ocean Tsunami occurred on December 26th, 2004. It started with an undersea earthquake with magnitude of 9.1 striking the western coast of Aceh province in the northern tip of the Sumatra Island in Indonesia (Lay et al. 2005). The Tsunami hit two northern provinces on the Sumatra Island - the Aceh and North Sumatra. Waves reached the shorelines within 15 minutes and no functioning early warning system was in place. There were casualties in multiple countries, including Indonesia, Sri Lanka, and even countries in eastern coast of Africa (Shaw 2015). Globally, more than 200,000 people were killed and 1 million people were forcibly displaced (Shaw 2015). Indonesia suffered the most in terms of lives lost and damages accrued. More than 110,000 people lost their lives due to the Tsunami, many of whom were women and children (Frankenberg et al. 2011) [5].

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 is the less developed province out of the two which relies heavily on oil and gas industries and suffered from two decades of continued, low-intensity conflicts [6]. North Sumatra is the most populous province outside of Java, where the capital city of Jakarta is located. In 2004, the regional GDP for Aceh and North Sumatra were US$3.1 billion and US$5.5 billion respectively, according to the 2005 Indonesia Population Census.

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. 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 that mothers who lost at least one child or who resided in communities with higher mortality rates 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 wid-

[5] 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.

[6] 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)

owed due to the Tsunami. Lépine et al. (2021) finds that child mortality increased one year after the Tsunami but not permanently, crediting a coordinated international and government response.

We build on these studies in two ways. First, we find that the Tsunami affected a wider group of population than previously understood. Previous works focus on the health and demographic effect on those who were born before the Tsunami. We stress that the harmful effects even extend to those who were in utero at the time. Second, we highlight possible ways in which households can mitigate the negative effects of the disasters. In this regard, we provide suggestive hints that health-related expenditures at the household level can play an important role in preventing long-run damage to health and demographic outcomes.

3 Data

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

Our data on the 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[7]. The STAR dataset is a longitudinal survey of individuals and households residing in the Aceh and North Sumatra Provinces[8]. The first wave of the STAR project began on May 2005, five months after the Tsunami. Since then, annual follow-ups have been made for four more years, followed by a ten-year follow-up in 2015-2016. We utilize the first two waves for our analysis[9]. All respondents of the STAR survey lived in Aceh and North Sumatra when the survey began[10] [11].

For our analysis, we use data on educational attainment, marriage, and pregnancy history from the second wave. Using marriage history, we are able to match husbands, wives, and children. Moreover, the data contains a full pregnancy history of ever-married women aged 15 to 49 including details on the type of child birth outcomes (livebirths, stillbirths or miscarriages), gestation length, and birthweight. It also includes year and month of birth, allowing us to exploit variation across timing

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

[8]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.

[9]In terms of attrition, 94% of individuals interviewed in the first wave were followed up in the second wave (Frankenberg et al. 2020a).

[10]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.

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

of exposure in a high-frequency time unit and to define treated and control groups. In addition, these data include information that we can use to construct other controls that we include in the regression such as mother's age at birth, birth order, and years of education.

3.2 Sample restrictions

From the 19,346 pregnancies documented in the dataset from August 1978 to November 2006, we restrict our sample to observations in which both birth year and month are identified, the records of both parents are matched, and responses for parental education exist. We include pregnancies from single-parent families as well as two-parent families. That leaves us with 10,102 observations[12]. For our main specifications, we further restrict to pregnancies conceived in years 2003-05 so that we are comparing cohorts that are similar in other attributes to those in utero during the Tsunami[13]. 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. Panel A in Table 1 breaks down the observations included in our sample into each year-quarter starting from the first quarter of 2003 to the fourth quarter of 2005.

3.3 Outcome variables of interest

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[14]. 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. Panel B in Table 1 provides the summary statistics for the birth outcomes. In the sample, the average gestation length is 8.76 months and 7.5% of pregnancies ended prematurely. We aggregate the month of conception into quarters in our main specification. In this way, we analyze how the

[12] The sample exclusion was implemented in the order of missing birth year/month records, missing IDs of husbands, and missing education records. 4,916 cases had missing birth years/months, 1,281 did not specify identification of their husbands, and 3,047 did not respond to education-related questions. Around 95% of these omissions occurred for those giving birth outside of our timeframe of interest.

[13] Even without this process, we get 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.

[14] 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.

birth effects of the Tsunami is heterogenous with respect to different trimesters of pregnancy.

3.4 Other variables

Our control variables are from the data on pregnancy history and educational history. In the pregnancy history, there is information on mother's age at birth and the order of birth. 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[15]. Details about these statistics are provided in Panels C and D of Table 1, where Panel C provides mean and standard deviation of the control variables and Panel D breaks down educational experience of the parents by schooling levels.

4 Identification strategy

4.1 Determining treatment status

We first use pregnancy records to determine treatment assignment based on timing of exposure. The pregnancy records in our data contain 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. Then, 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[16].

This approach addresses two endogeneity concerns that arise from using actual gestation duration (Black et al. 2016; Currie and Rossin-Slater 2013; Matsumoto 2018). The first 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 is that the treatment itself may directly affect gestation length, which we later show is the case with exposure to the Tsunami.

[15] 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 similar to the specification with years of schooling included.

[16] 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).

Using this approach, we can also differentiate exposure by the stages of pregnancy. 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. This distinction is crucial as there are many works documenting the additional negative effects of in utero exposure to adverse events in the first trimester[17].

We also have variation in the extent of damage at the community level. The dataset's geographical unit, a "cluster", is a group of households with similar extent of damage due to the Tsunami, ecological attributes, distance to the coastline, and levels of urbanization[18]. Each cluster is categorized as either "lightly affected", "moderately affected", and "heavily affected" based on the extent of the damage incurred[19]. The assignment is predetermined in the dataset and is based on assessments by the community leaders, survey supervisors, and satellite imagery.

4.2 Event-study: Using differences in timing of exposure

The first regression specification uses an event study approach. We leverage the variation in timing of exposure to separately identify heterogenous effects of the Tsunami across different stages of pregnancy. We use the following equation.

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

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. Each outcome variables except gestation length is an indicator

[17]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)

[18]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.

[19]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.

variable. For these regressions, we run a linear probability model. We include cluster fixed effects ϕ_c that absorb any unobserved cluster-level attributes. The regression also includes X_{ict}, controlling for factors determining selection into fertility. We include years of schooling for both the mother and the father. We also control for the mother's age at birth, in levels and squared as well as controls for birth order. Standard errors are clustered at a cluster-level.

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. Based on this approach, the treatment starts from second quarter of 2004. $\mathbb{1}[t \geq 2004Q2]$ is an indicator that equals 1 if the year-quarter of conception t is on the second quarter of 2004 or after, thus the treatment group. Similarly, $\mathbb{1}[t \leq 2003Q4]$ indicates that the year-quarter of conception t is on the fourth quarter of 2003 or before, thus the control group. The coefficient for the first quarter of 2004, the last pre-treatment period, is set to zero following the standard normalization in event-studies (Schmidheiny and Siegloch 2020).

The coefficients of interest are the β_t's, calculated for each year-quarter since the second quarter of 2004. This coefficient picks up the effect of the Tsunami on birth outcomes for those conceived on year-quarter t relative to those conceived on the first quarter of 2004. By allowing β_t to vary across each period, we can capture the distinct average birth effects for those conceived in different periods. We hypothesize that those exposed in utero, especially in the earlier stages of pregnancy, would suffer the worst birth outcomes. Therefore, we expect livebirths to be less likely and gestation lengths to be shorter for this group ($\beta_t < 0$). As for miscarriages and preterm births, we anticipate these to be more likely ($\beta_t > 0$).

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. We conduct a balance test in Table 2. The balance test shows that there are statistically negligible differences in observables for the treated and the controlled at the 5% level.

4.3 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. We incorporate a differences-in-differences approach (hereafter DD-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 specification.

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

$\mathbb{1}[\text{damage}_c = k]$ 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 are the same 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. There are subtle differences in interpreting the β_t coefficients between Equations (1) and (2). In the former, the β_t coefficient 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 clusters*. 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 attributable to residing in more damaged areas at the time of the Tsunami. δ_{3t} and δ_{2t} represent additional effects for those conceived in heavily and moderately affected areas compared to lightly affected areas. We hypothesize that for a given t, the negative birth effect would be more dominant as the extent of damage becomes more serious. Thus, we expect that $\delta_{3t} < \delta_{2t} < 0$ for livebirth and gestation length outcomes and $\delta_{3t} > \delta_{2t} > 0$ for miscarriages and preterm births.

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 the 5% level with the balance table in Table 3.

5 Main Results

5.1 Event-study results

The regression results for Equation (1) are presented in columns (1) - (4) of Table 4, with coefficients and 95% confidence intervals graphically presented in Figure A1 of the online appendix. Since coefficients for those conceived in the first quarter of 2004 is set to zero, coefficients for other periods indicate the differences in average birth effect relative this period. 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) of Table 4. Those conceived from the second to 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 the 5% level. We report in Figure A1 that for all other periods, the effect is mostly indistinguishable from zero at conventional significance levels. 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) of Table 4. The results in this regression yield similar conclusions to the livebirth outcome in that the negative effects appear most evidently 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.

We compare our estimates to two studies which provide figures on that are closest to livebirth and miscarriage measures used in our research. Liu et al. (2015) finds that *cohort sizes* for those exposed to the 1999 Taiwan earthquake in utero during 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 the *fetal death rate*[20] of babies born to mothers who are 15-24 years old

[20]de Oliveira et al. (2021) defines fetal death rate as fetal deaths divided by the number of resident live births plus fetal

by 17 per 1,000 pregnancies (de Oliveira et al. 2021). Since the measures being used are not exactly the same, one should be cautious when directly comparing the estimates. Nevertheless, our results speak to the well-established finding that exposure to natural disasters, particularly at the earliest stage of pregnancy, is detrimental to successful pregnancies.

Results on the gestation length are displayed in column (3) of Table 4. The differences in 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 2004. 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.

Lastly, the results for preterm birth are in column (4) in Table 4. As with other results, those in the first trimester of pregnancy at the Tsunami are the most affected. The pregnancies that began in the fourth quarter of 2004 are 6.7 pp more likely than those conceived in the first quarter of 2004 to end prematurely. 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 Brazilian municipalities in the first trimester of pregnancy decreased gestation length by 0.006 weeks. 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 for those exposed in the first trimester of pregnancy[21].

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[22]. 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 (Mulder et al. 2002; Schulz 2010). Our results also share similarities with Guantai and Kijima (2020), Karbownik and Wray (2019), and Koppensteiner and Manacorda (2016) in that the negative effect is the most

deaths for the same unit multiplied by 1,000. It does not break down the fetal death to miscarriages and stillbirths.

[21] There are also some findings that report null effects of natural disasters on these outcomes. de Oliveira et al. (2021) and Currie and Rossin-Slater (2013) find no changes to preterm births and gestational age due to natural disasters.

[22] In the online appendix Figure A1, We also find limited effects on those conceived after the Tsunami. The birth outcome estimates for those conceived in the first quarter of 2005 is just slightly lower than the estimates for those conceived at the fourth quarter of 2004. Some of these estimates are significant at 10% levels. It is possible that these cases could have been affected by some afterquakes. However, besides the 8.6 afterquake on March 28th 2005 in Nias-Simeulue, these are not reported by the respondents.

pronounced on those exposed in the earliest stage of the pregnancy.

One possible threat to our estimates is measurement error from using quarterly or monthly data instead of more fine-grained weekly data to determine treatment assignment. We address this in Section 2.5 of the online appendix by bounding our treatment effects in a manner similar to the exercise suggested by Lee (2009). We show that most of the lower and upper bounds of the treatment effects obtained through this exercise are within the confidence intervals of our point estimates, making the estimates from the event-study specification credible.

5.2 DD-approach results

The results for the β_t coefficients in Equation (2) are reported in columns (5)-(8) of Table 4. Coefficients in 2004Q2 - 2004Q4 without interactions refer to the point estimates for β_t for the second through fourth quarter of 2004. The year-quarters indicators interacted with 'Medium' report the point estimates for δ_{kt} for the moderately affected areas, whereas those interacted with 'Heavy' report the same for the heavily affected clusters. Point estimates and the confidence intervals for the full duration of the sample are graphically displayed in Figure A2 of the online appendix.

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 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. [23]

For the results pertaining to the timing of exposure, we find that they show the same conclusion to the event-study results in regards to those in the first trimester at the Tsunami. From columns (5)-(8) of Table 4, we note that negative birth effects among those exposed to the Tsunami in utero exclusively appear in those in the first trimester of pregnancy.

However, we find no heterogeneity in birth effects across clusters with different extent of the Tsunami damage. As shown in columns (5)-(8) in Table 4 as well as the bottom panel of Figure A2, nearly all coefficients are statistically indistinguishable from zero at the 5% level. Even for those

[23] We also show that the results hold even if we assign those conceived after the Tsunami in the control group using a different regression approach. Detailed explanation of the procedure and the results are shown in Section 2.1 of the online appendix.

that are distinct from zero, the coefficients are not consistent with our hypotheses 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. However, these inverse relationships do not survive robustness tests for alternative regional variations, which will be shown in Section 6.3. Moreover, since we were unable to access geolocated data due to privacy restrictions, caution is advised in interpreting these results.

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 (Mulder et al. 2002; Schulz 2010). However, we find inconclusive heterogenous differences in treatment effect across different clusters.

6 Specification and robustness tests

6.1 Verifying timing effects: Placebo results

To ensure that the birth effect captured in our estimates are not confounded by any seasonal patterns, we conduct a placebo study on cohorts conceived at different time periods. 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 cohorts used in the placebo study. For our exercise, we select three "placebo samples" - those conceived on 2000-2002, 2001-2003, and 2002-2004. 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 5.

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 placebo regressions are presented in Figure 1, with the top and bottom panels representing event-study and DD-approach results respectively. Periods 0, 1, and 2 for each cohort correspond to those who on the third, second, and first trimester of pregnancy when the tsunami occurs for each cohort. The effects of the 'fake' tsunamis, represented by gray lines, are all statistically zero at the 5% level across all placebo samples. The blue lines, representing the effect on our main sample, indicate that the birth effect of the Tsunami is statistically significant exclusively for 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.

6.2 Testing for selection biases

The other potential concern against our estimation results is that the results may be endogenous if there are changes in migration and fertility patterns after the Tsunami. The identifying assumption required to verify the treatment effect by timing of exposure is that assignment to control and treatment groups is as good as random. While the unexpected nature of the Tsunami makes this assumption plausible for those conceived in 2003 and 2004, the same cannot be said for those conceived in 2005. Mothers beginning their pregnancy in 2005 can choose to migrate away or not give birth at all. This can lead to selective attrition and changes in the composition of mothers in our sample, which can threaten the validity of the random assignment assumption. We take a number of approaches to address this issue in this section.

6.2.1 Checking for selective migration

We check for changes in migration decisions of pregnant mothers after the Tsunami to address potential selective attrition problems. If mothers who went into labor in 2005 are more likely to migrate than those who became pregnant before the Tsunami, our sample could be subject to compositional changes that may affect the validity of our estimates. To identify changes in migration decisions, we use questions related to migration and residency status to identify migration histories of each individuals[24]. Based on the responses, we are able to identify whether the respondent is a temporary migrant or a permanent migrant. The categorization of our sample into non-migrants, temporary, and permanent migrants can be found in Table A5 of the online appendix.

For regression, we substitute the dependent variable in Equations (1) and (2) with either of the two indicator variables for whether the mother is a temporary or permanent migrant. The event-study results graphically presented in Part (a) of Figure 2, while the full set of results for the DD-approach and numerical results are found in Figures A5-A6 and Table A5. We find that the coefficients on all the event indicators are statistically indifferent from zero at the 5% level. Thus, we find that post-Tsunami cohorts are not more likely to migrate away compared to others. This alleviates concerns of selective attrition in terms of migration.

6.2.2 Verifying stable trend in total conception

We check for the changes in the fertility trend at each cluster of residence by regressing the total number of conceptions in a cluster on various controls, as in Black et al. (2016) and Koppensteiner and Manacorda (2016). The total number of conceptions is a summation of all livebirths, miscarriages, and stillbirths. We aggregate these for each cluster and quarter, which we refer to as blocks. We regress the log of total number of conceptions at each 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 and report the findings in Part (b) of Figure 2[25]. The coefficients on the year-quarter dummies are not statistically different from zero. These results suggest that there is no noticeable difference in the total number of conceptions across the sample period.

There are other noticeable takeaways from this exercise. One potential concern is that increase in unreported miscarriages may drive the conception numbers down and mechanically decrease livebirths[26]. However, the decrease in livebirths in our main results occurs despite no changes in total conceptions and potential problems caused by unreported miscarriages. Furthermore, these suggest that our estimate of the Tsunami impact on miscarriages would be underestimated, suggesting that

[24]In the survey, there is a question that asks whether the respondent have moved somewhere else for a period of two weeks or more since the Tsunami hit (migration question). There is also a question asking whether the interviewees are living in the same house as they did before the Tsunami (residency question). Those who said yes only to the first question are temporary migrants. Those who said yes to both are permanent migrants.

[25]Further details about the regression, alongside results on additional outcomes are found in the online appendix Section 2.3.2.

[26]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 miscarriage.

the true effect could be higher if all of these could be captured.

6.2.3 Additional exercises

We conduct two additional exercises to address the problems of selective attrition and compositional changes of our observations. First, we reduce the sample further down to those conceived in 2003 and 2004 only. Results reported in Figures A3-A4 and Table A2 show that the Tsunami effect is statistically significant only for those conceived in the fourth quarter of 2004, consistent with our main results. Additionally, we check for changes in the type of mothers going into pregnancy after the Tsunami to check whether our observation is subject to compositional changes due to the treatment. Results in Figures A7-A8 and Table A6 show that point estimates of coefficients of all event indicators are statistically zero, alleviating concerns of compositional change. Details of the procedures used are found in Sections 2.3.1 and 2.3.2 of the online appendix.

6.3 Alternative measures of cross-sectional variance

In this section, we use alternative measures of variation in the extent of damages due to the Tsunami across cross-sections. The aim of this exercise is to confirm the insignificant differences of our results across clusters with different degrees of damage using different measures of cross-sectional variation in the dataset. We conduct two exercises. First, we use a cluster-level variation of exposure to Tsunami-related destruction by using the share of households reported damaged by the Tsunami within each cluster of residence. Second, we utilize a household-level variation by using a dummy variable indicating whether the respondent's house was damaged due to the Tsunami. Further details on how these measures are created are documented in Section 2.4 of the online appendix.

We report our results in Figures A12-A13 and Tables A9 in the online appendix. As with our main results, the harmful effects of the Tsunami are concentrated on those conceived on the fourth quarter of 2004. We find no statistically significant differences across cross-sections at the 5% level, further complementing the lack of geographical heterogeneity of birth outcomes in the main result.

7 Discussion

7.1 Potential mechanisms for mitigated negative effects

In this section, we introduce suggestive mechanisms that cause the negative birth effects to be short-lived. Our main results show that the negative birth effects are only significant for those conceived on the fourth quarter of 2004, but no statistically significant effect of the Tsunami for those conceived in later quarters. We analyze two possible channels - change in household expenditures on health after the Tsunami and selective survival of fetuses.

7.1.1 Household health expenditures

We 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 evidence that complements a well-known finding that spending on health funded by various external sources leads to better birth and health outcomes (Jaba et al. 2014; Lépine et al. 2021; Paxson and Schady 2005). 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.

We report the result of our investigation on the left panel in Figure 3. For the log of health-related expenditure of each household, 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 the first quarter of 2004. In Table A14 of the online appendix, we provide numerical estimates for health expenditure and other categories related to prenatal care - amount spent, number of visits, and giving birth at own residence. We find that these do not change significantly for conceptions occurring in 2005.

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 is smaller.

7.1.2 Selective survival of fetus

Second, we test for selective survival of fetuses, a necessary condition for the harvesting effect (Luy et al. 2020), by comparing changes in birthweight of newborn babies before and after the Tsunami. We use the raw birthweight, log 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 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).

We report the results for the log of birthweight on the right panel in Figure 3, with point estimates reported in columns (5)-(8) in Table A14 of the online appendix. 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 hypothesis is mixed at best[27].

Similar to the analysis for health expenditures, one caveat is our small sample size. 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 pointing towards suggestive relations, as opposed to causal relations.

7.2 Other potential mechanisms

7.2.1 Maternal mental and physical health

One well-known mechanism behind negative birth effects of in utero exposure in developed countries is maternal stress (Koppensteiner and Manacorda 2016; Black et al. 2016). To check whether this channel is also applicable in developing countries, we first use self-reported responses of having symptoms of fear of death, fear of injury, sleeping disorders, anticipatory anxiety and aquaphobia. Second, we use parental deaths from the Tsunami to proxy the likelihood of suffering severe stress.

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

Columns (1)-(5) of Table A15 in the online appendix show no significant correlation between mental distress and pregnancies in different year-quarters, suggesting that maternal stress from the disaster is 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. However, the estimate is not statistically significant after adjusting for multiple hypotheses testing.

In addition to mental health, we look into how maternal physical health could explain the negative birth outcomes. We use a self-reported response on whether individuals feel that they are in the same or worse health conditions. We use a dummy variable indexing whether the mother used some type of outpatient care service one month before the survey to measure usage of hospital services post-Tsunami. Columns (6)-(8) in Table A15 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, suggesting a possible role for the physical health of mothers determining our main outcomes.

7.2.2 Socioeconomic status

Next, we determine whether birth outcomes differ depending on the socioeconomic status of mothers [28]. We use four indicators of socioeconomic status – employment, household assets, building material of houses, and transfers from aid organizations. The first three categories capture elements of household income and wealth. Each of these represent some form of household income and wealth. Summary statistics for the variables are found in Table A13 in the online appendix.

The results presented in Table A16 in the online appendix suggest that different socioeconomic status are unlikely to drive our main results. 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

More people 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 attainment for those

[28] Whether those with better socioeconomic statuses can cushion themselves from adverse events is not yet certain. 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. Similarly, Frankenberg et al. (2015) shows that wealthier households may protect themselves by using their resources to choose well-protected locations and households. However, Frankenberg et al (2011) finds that socioeconomic status has no relation with mortalities attributed to the 2004 Tsunami

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 to identify whether being exposed to the Tsunami at different stages of pregnancy leads to heterogeneous birth outcomes. 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 survival for the fetuses in the first trimester. In particular, the probability of miscarriage increases while that of live-birth decreases. 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. In addition, we find limited roles played by maternal distress and socioeconomic status at the time of exposure in determining our outcome.

Our results speak to the need of looking into the probability of fetal loss, a problem largely overlooked in developed countries. Although the effects of other natural disasters are likely to be subtler considering the drastic nature of the Tsunami, neglecting the short-run outcomes may lead to underestimating the dangers of fetal loss that are prevalent in developing countries (Institute of Medicine 2003). Thus, considering fetal survival probabilities is essential for understanding the birth effects of adverse events in developing countries. We also provide suggestive evidence that causal mechanisms that apply to developed countries may not directly explain similar in-utero exposure events in developing country context. Furthermore, our results suggest some actionable guidance on how governmental and non-governmental organizations can promote children's health by facilitating household investment in health. Further studies could focus on verifying possible alternative causal mechanisms behind the negative birth outcomes and how relaxing households' health spending constraints can improve birth outcomes and fetal health.

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A Tables and Figures

Table 1: Summary statistics, 2003-05 conception cohort

Panel A. ***Pregnancies per year-quarter***			
Quarter of conception	2003	2004	2005
First quarter	157	171	212
Second quarter	150	148	203
Third quarter	180	200	206
Fourth quarter	170	179	183
Panel B. ***Final results of each pregnancies***			
Birth outcome (dummy variable)		Frequency	Proportion
Livebirth		2,050	94.95%
Stillbirths		23	1.07%
Miscarriage		86	3.98%
Preterm birth		162	7.50%
Birth outcome (continuous variable)		Mean	Std.dev
Gestation		8.76	1.32
Panel C. ***Averages for the control variables***			
Category		Mean	Std.dev
Age at birth (Years)		27.60	5.81
Year of education, female (Years)		8.42	4.06
Year of education, male (Years)		8.76	3.94
Panel D. ***Educational level attained by gender***			
Highest education obtained		Female	Male
No education		91	24
Elementary school		786	765
Junior high school		499	546
Senior high school		538	586
Associate degree		147	60
Bachelor's degree		93	168
Masters/PhD		5	10

Note: Panel A collects the total pregnancies recorded in the data per year-quarter in the sample. Panel B includes summary statistics of the outcome variables. For gestation duration, average months and the standard deviation is shown. For other birth outcomes, count and share of such outcome relative to the total observation are displayed. The categorization of outcomes into stillbirth and miscarriage is based on the respondent's self-reported answer. Panel C includes means and standard deviations of covariates used in the main regressions. Panel D counts the number of female and male respondents who achieved each level of education.

Table 2: Balance table: Pre vs Post Tsunami

Variable	(1) Pre-Tsunami Mean(SE)	(2) Post-Tsunami Mean(SE)	T-test Difference (1)-(2)
Literate, wife	0.918	0.920	-0.002
	(0.014)	(0.015)	
Literate, husband	0.955	0.951	0.004
	(0.009)	(0.009)	
Enrollment, wife	0.953	0.961	-0.008
	(0.011)	(0.010)	
Enrollment, husband	0.988	0.989	-0.002
	(0.004)	(0.004)	
Yrs of schooling, wife	8.268	8.512	-0.244
	(0.263)	(0.271)	
Yrs of schooling, husband	8.708	8.800	-0.092
	(0.223)	(0.199)	
Rural	0.716	0.734	-0.018
	(0.049)	(0.047)	
Age at birth, wife	27.536	27.646	-0.110
	(0.213)	(0.169)	
Summary statistics			
Total number of clusters	107	108	
Total number of respondents	828	1331	

Note: The observations included in this table reflect all pregnancies that began in calendar years 2003-05. Pre-Tsunami column summarizes the observations for conceptions occurring before the first quarter of 2004 and not exposed to the Tsunami. Post-Tsunami column indicates observations conceived on the second quarter of 2004 or after. The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at variable cluster. 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: Light vs Medium vs Heavy damaged clusters

Variable	(1) Light Mean(SE)	(2) Medium Mean(SE)	(3) Heavy Mean(SE)	T-test Difference (1)-(2)	(1)-(3)	(2)-(3)
Literate, female	0.957	0.902	0.915	0.055*	0.042	-0.013
	(0.008)	(0.020)	(0.044)			
Literate, male	0.962	0.953	0.935	0.010	0.027	0.018
	(0.008)	(0.010)	(0.030)			
Enrollment, female	0.986	0.943	0.964	0.043*	0.022	-0.021
	(0.004)	(0.016)	(0.028)			
Enrollment, male	0.995	0.990	0.974	0.005	0.021	0.016
	(0.003)	(0.003)	(0.022)			
Yrs of schooling, female	8.652	8.137	9.137	0.515	-0.485	-1.000
	(0.329)	(0.379)	(0.657)			
Yrs of schooling, male	8.722	8.607	9.498	0.115	-0.776	-0.892
	(0.266)	(0.281)	(0.558)			
Rural	0.777	0.728	0.629	0.049	0.148	0.099
	(0.077)	(0.068)	(0.106)			
Age at birth, female	27.883	27.439	27.756	0.444	0.128	-0.317
	(0.325)	(0.182)	(0.331)			
Summary statistics						
Total number of clusters	31	53	24			
Total respondents	583	1,269	307			
Total households	396	870	219			
Respondents whose house were damaged	80	463	208			
Number of households damaged	42	220	127			
Share of households damaged	10.09%	21.31%	46.77%			

Note: The observations included in this table reflect all pregnancies that began in calendar years 2003-05. The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at variable cluster. 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. For the summary statistics in the bottom panel, the unit are in frequencies except for share of households damaged, which is in percentages.
* $p < .05$

Table 4: Birth effects of the Tsunami by timing of exposure

	Event study				Difference-in-differences			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conception period	Livebirth	Miscarriage	Duration	Preterm birth	Livebirth	Miscarriage	Duration	Preterm birth
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)
2004Q2 × Medium					-0.0247	0.00406	0.103	0.00454
					(0.0423)	(0.0437)	(0.261)	(0.0611)
2004Q2 × Heavy					0.0746†	-0.0572	-0.0103	-0.0583
					(0.0426)	(0.0398)	(0.632)	(0.0846)
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)
2004Q3 × Medium					-0.0187	0.0193	-0.0768	0.0669*
					(0.0263)	(0.0249)	(0.139)	(0.0309)
2004Q3 × Heavy					-0.0138	0.0283	-0.163	0.0522
					(0.0397)	(0.0363)	(0.193)	(0.0462)
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)
2004Q4 × Medium					0.0703	-0.0896†	0.570	-0.0661
					(0.0544)	(0.0514)	(0.348)	(0.0578)
2004Q4 × Heavy					0.155**	-0.145**	0.625	-0.0656
					(0.0538)	(0.0491)	(0.434)	(0.0860)
Obs.	2159	2159	2159	2159	2159	2159	2159	2159
No. of Clusters	108	108	108	108	108	108	108	108

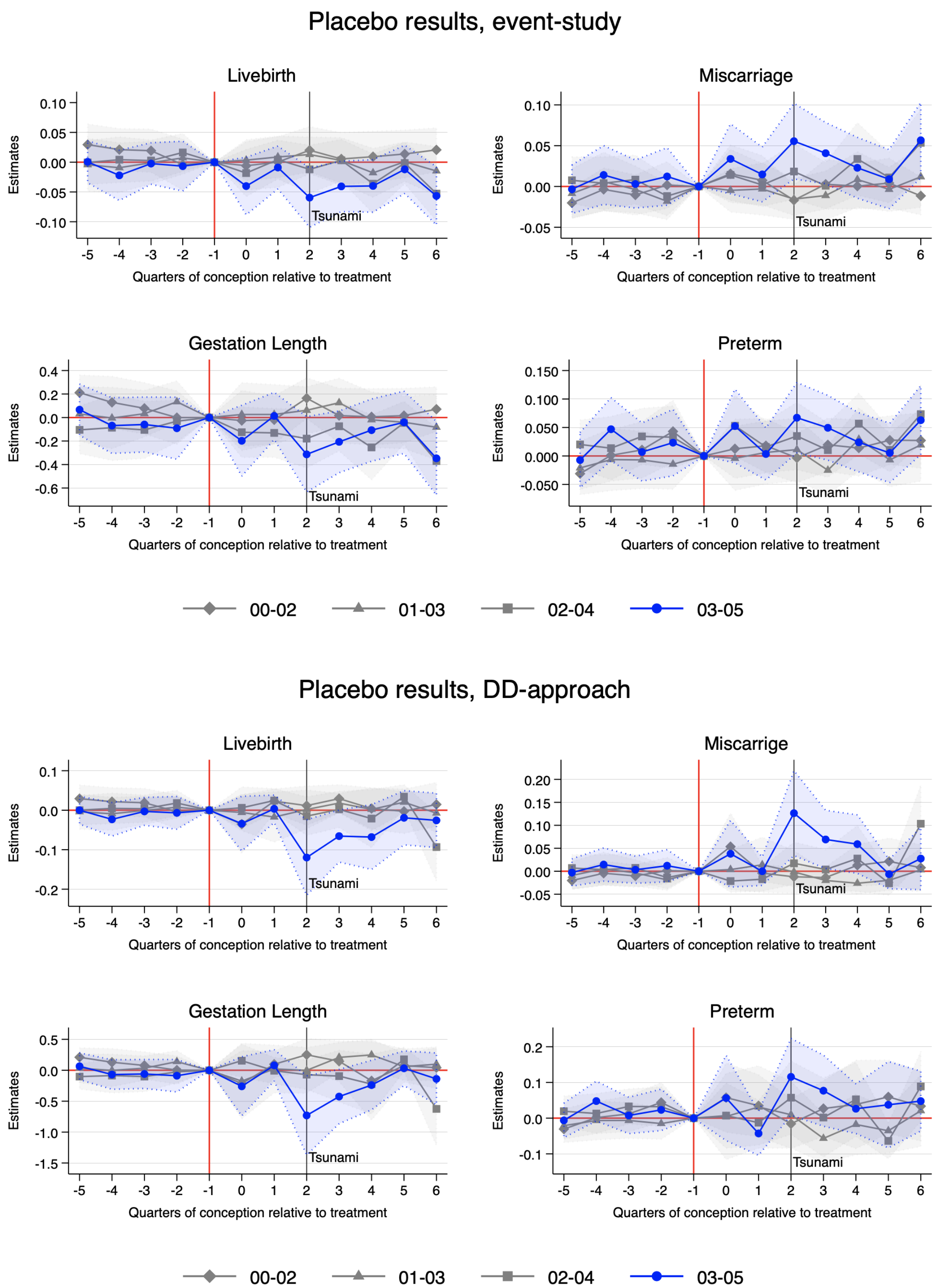
Note: Year-quarters in the first column indicate the period of conception. Coefficients for the year-quarter dummies are reported in the tables, with those for 2004Q1 normalized to 0. The regressions include time dummies for each quarters from 2003Q1 to 2005Q4. The coefficients reported for these dummies are either statistically zero or have estimates that are not robust (See Figures A1 and A2 in online appendix). For periods since 2004Q2, each time dummy is also interacted with damage indicators. These estimates are also reported to be statistically equivalent to zero (See Figure A2 in online appendix). 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.
† $p < .10$, * $p < .05$, ** $p < .01$

Table 5: 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
Number of observations	1,888	1,914	2,007	2,159

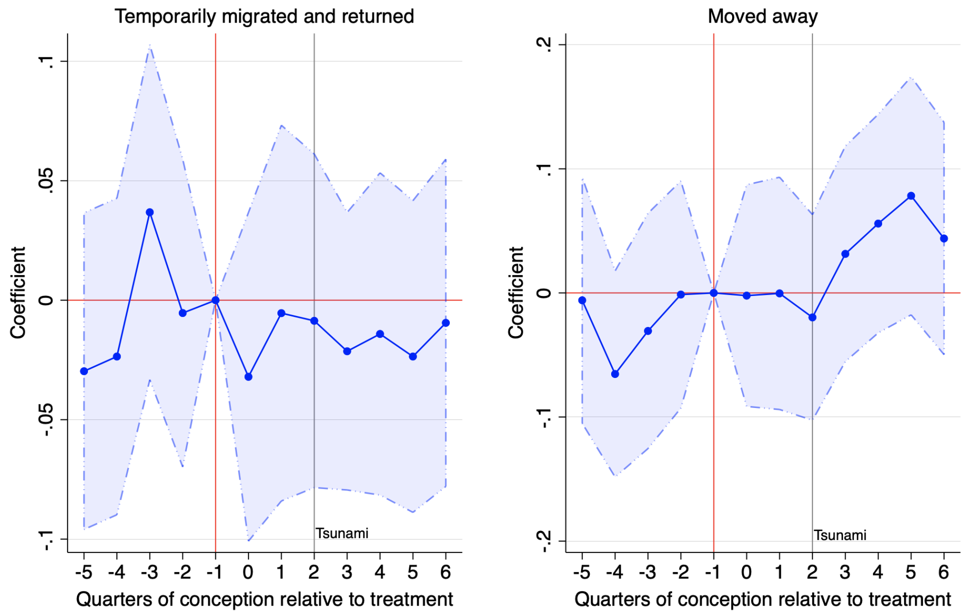
Note: The unit of years of schooling and mother's age variables are in years. As for other variables, the number indicates the proportion of respondents, ranging from 0 to 1, that responded yes to each category. The observations included in this table reflect all pregnancies that began in calendar years 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.

Figure 1: Placebo results

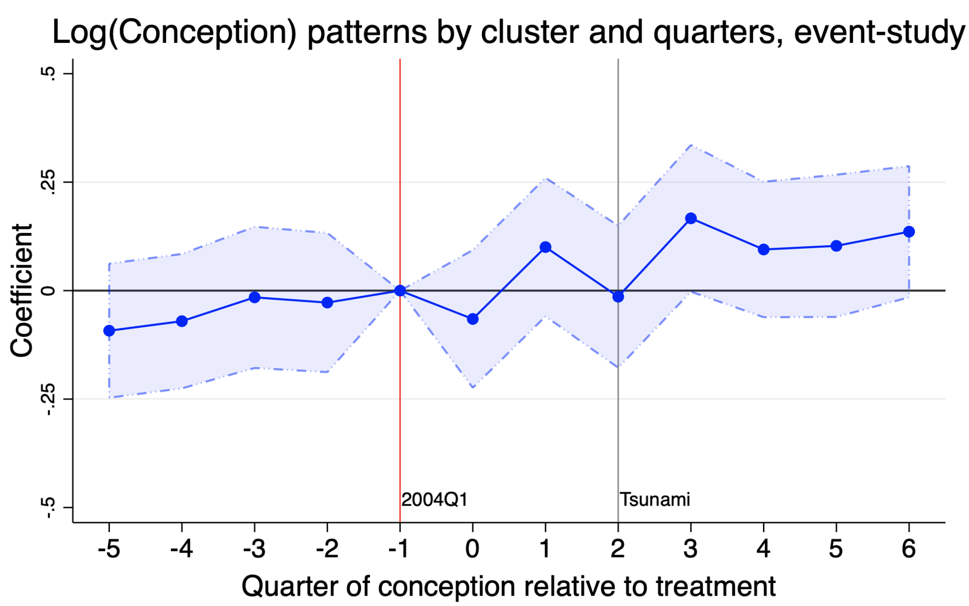


Note: Period 0 in the x-axes in both panels refers to the starting period of each treatment - 2001Q2, 2002Q2, 2003Q2, and 2004Q2 respectively. There are 108 clusters for all regressions and 1,888, 1,914, 2,007, and 2,159 observations in each regressions. Point estimates and 95% confidence interval are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

Figure 2: Migration and conceptions before and after the Tsunami, event-study



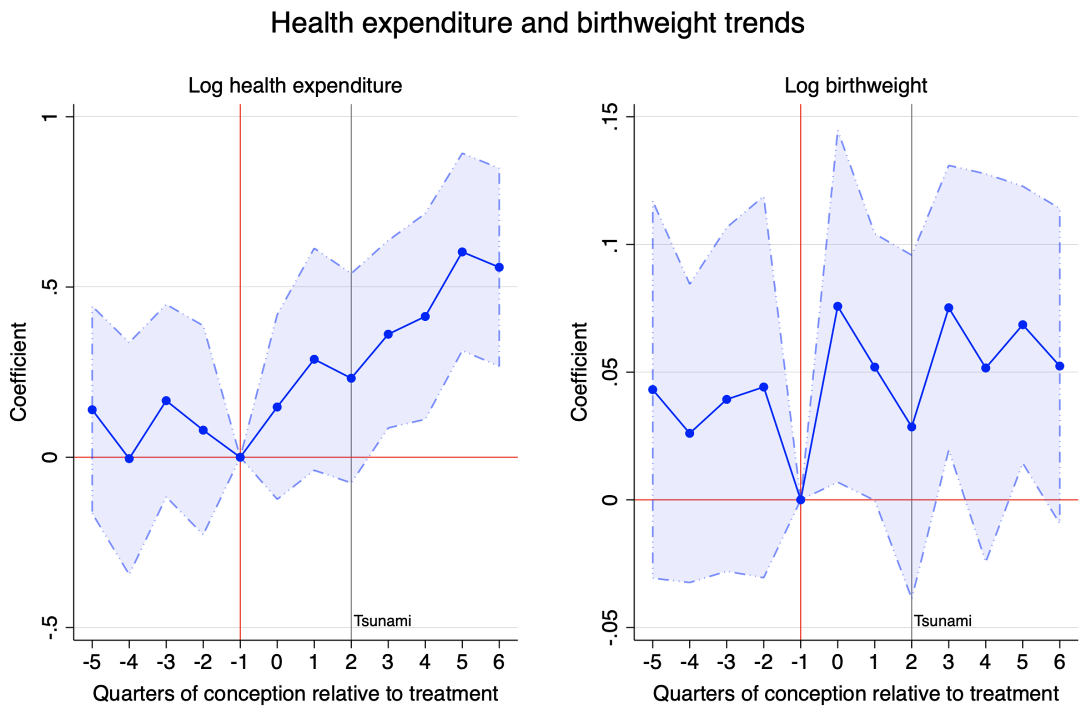
(a) Testing for migration patterns after the Tsunami, event-study



(b) 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 3: 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.