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 birth outcomes in Indonesia. Exploiting variation in the timing of exposure, we find that the probability of a successful pregnancy drops by 5.4 percentage points (pp), while miscarriages increase by 5 pp for those exposed in the first trimester of pregnancy. Gestation length shortens by 0.28 months and the likelihood of preterm birth rises by 5.4 pp. We find suggestive evidence that post-disaster health investments by households may have shielded later cohorts from harmful effects, while measures of maternal health and socioeconomic status have limited explanatory power. Our results suggest the importance of considering fetal loss as a first-order cost in natural disasters and highlight that facilitating household investment in health through various policies may mitigate negative birth effects in the aftermath.

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

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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 the timing of in utero exposure to natural disasters determines the outcome of pregnancies. In particular, we focus on one of the most devastating natural disasters to strike developing countries in 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, Sumantri, et al. 2020; Cas et al. 2014), the effect of the Tsunami on birth outcomes and health of those who were unborn at the time is not yet fully understood. We focus on fetal loss, an outcome rarely measured in disaster research despite being a first-order cost where it occurs. We further ask which channels account for the effect, and whether they resemble those documented in other settings.

We utilize the Tsunami as a natural experiment whose treatment varies across timing of exposure. The dataset we use extensively tracks the life trajectories of the individuals exposed to the Tsunami,

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 the 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 are considered critical because the central nervous system develops during this period (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.

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 leverage variation across timing of exposure, comparing those conceived before the Tsunami and those conceived afterwards, and isolate the different birth outcomes between those exposed to the Tsunami and those unaffected by the Tsunami. We conduct a series of robustness checks to confirm that the effects of the Tsunami are identified by timing of exposure. 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 5.4 percentage points (pp) less likely to end in livebirths and 5.0 pp more likely to end in miscarriages compared to those conceived before the second quarter of 2004.[3] Gestation length is shorted by about 0.28 months. The probability of premature birth, defined as pregnancies shorter or equal to 8 months, increases by 5.4 pp for the same cohort. All these negative effects are attenuated for fetuses conceived after the Tsunami or those in later stages of pregnancies.

To verify our findings, we conduct a placebo test, test for selection biases, and address potential concerns raised by measurement quality. The placebo test results indicate that our findings are distinctly attributable to the Tsunami rather than a seasonal pattern. We find limited evidence of selective migration, changes in the trend in the number of total conceptions, and shifts in the compositions of mothers going into pregnancies. Because survivors of the Tsunami were positively selected on health (Frankenberg, Sumantri, et al. 2020), survival selection works against finding the effects we document. We report that our conclusions are robust to within-mother comparisons and to reweighting toward high-mortality communities. Our findings also hold when using observations with clearly defined time units in the survey.

In addition, we also examine differential birth effects across communities with varying intensity of damage following the Tsunami. We find that there is no consistent pattern across different clusters depending on the measure of intensity used in the regression. Furthermore, we find that parallel trends across communities with different damage intensity are not reliably satisfied across all specifi-

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.

cations. Moreover, there is a post-Tsunami differential in aid flows that may mitigate negative effects on the most severely affected communities, thus confounding the comparison further. Therefore, we treat the regressions involving cross-sectional variation by intensity of exposure as descriptive.

Lastly, we explore empirical evidence that suggests channels that explain our findings. 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 mental and physical health, and socioeconomic status. We also find no evidence that selective fetal survival explains our results, as birthweight is unchanged for the cohort bearing the birth effect.

We contribute to three strands of literature. First, we aim to complement the literature on the human cost of natural disasters by quantifying an outcome largely under-utilized in disaster research. Existing studies have focused on later-life outcomes such as labor market outcomes, educational attainment, and adult mortality (Currie and Rossin-Slater 2013; Karbownik and Wray 2019; Imberman et al. 2012).[4] This is also the case for works on the 2004 Tsunami (Cas et al. 2014; Frankenberg et al. 2011; Frankenberg et al. 2012; Frankenberg, Sumantri, et al. 2020). In-utero outcomes such as fetal death are first-order human costs in settings that are less studied due to limited data availability (Institute of Medicine 2003; Weinhold 2009). By identifying effects on miscarriage and livebirth by trimester of exposure, we measure a cost that is central to disaster impacts in developing countries yet absent from the existing evidence.

Second, we relate to the literature on the causal mechanisms behind negative effects of in-utero exposure to adverse events. Studies in developed countries emphasize maternal stress as the operative channel (Almond et al. 2009; Black et al. 2019; Currie and Rossin-Slater 2013; Hajdu 2024; Ramirez and Haas 2021; Triyana and Xia 2023). Testing these channels in the context of the 2004 Tsunami, we find that the mechanisms differ from those documented in wealthier settings. Measures of maternal stress, physical health and socioeconomic status have limited explanatory power. We further provide suggestive evidence of a behavioral channel where differential health investment across cohorts further shapes which cohorts bear the effect. In doing so, we find operating chan-

4. Currie and Rossin-Slater (2013) uses meconium aspiration syndrome 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. Imberman et al. (2012) 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.

nels distinct from previous works on developing-country settings (Oliveira et al. 2021; Guantai and Kijima 2020; Koppensteiner and Manacorda 2016; Rosales-Rueda and Triyana 2019; Torche 2011).

Lastly, this paper contributes to the literature studying disaster recovery efforts 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 document that the birth effect is concentrated among cohorts that received lower compensating health expenditure. Health spending is significantly lower for the exposed cohort than for cohorts conceived during the post-disaster recovery. In doing so, we find suggestive support that policies enabling household health expenditure can reduce the human costs of 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 a 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 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, Sumantri, et al. (2020), 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. 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 the 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.

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)

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 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

7. Frankenberg, Katz, et al. (2020) 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, Katz, et al. 2020).

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.

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

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 birth effects of the Tsunami are heterogeneous 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

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.

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.

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* birth date, 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.

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]

16. This would not be different from using the *actual* birth date 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).

17. As the infant's central nervous system is developed in the first trimester (Rooij et al. 2010; Schulz 2010), in utero exposure can have heterogeneous 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)

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 Main specification: Leveraging timing of exposure

The first regression leverages the variation in timing of exposure to separately identify heterogeneous effects of the Tsunami across different stages of pregnancy. In doing so, we isolate the effect of in utero exposure to the Tsunami exposure on birth outcomes from exposure at pre-Tsunami and post-Tsunami periods. We use the following equation.

$$y_{ict} = \alpha + \phi_c + \gamma X_{ict} + \sum_{\tau} \beta_\tau \text{InUtero}^{\tau}_{ict} + \pi \text{PostDec2004}_{ict} + \epsilon_{ict} \quad (\tau \in [2004Q2, 2004Q3, 2004Q4]) \tag{1}$$

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. Each outcome variable 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. 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 the cluster-level.

The indicator variable $\text{InUtero}^{\tau}_{ict}$ denotes exposure to the Tsunami in utero. This indicator is 1 for all conceptions starting at the second, third, and fourth quarter of 2004. These conceptions would be

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.

at third, second, and first trimester, respectively. PostDec2004$_{ict}$ indicates pregnancies beginning after the Tsunami. We separate this group because conceptions occurring after the Tsunami cannot reflect in-utero exposure, yet may capture distinct post-disaster patterns arising from behavioral responses or altered conditions during recovery. Pregnancies starting on or before first quarter of 2004 are the omitted group and serve as the control group.

The coefficients of interest are the three β_τ's, calculated for second, third, and fourth quarter of 2004. Each is measured relative to the pooled pre-Tsunami control. By allowing β_τ 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 first trimester 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 ($\beta_{2004\text{Q4}} < 0$). As for miscarriages and preterm births, we anticipate these to be more likely ($\beta_{2004\text{Q4}} > 0$).

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. Furthermore, we require that the outcome variables across treated and control groups are on parallel trends. We further assess whether pre-Tsunami cohorts show differential trends using the event-study specification below, which provides the pre-trend diagnostics for this design

4.3 Complementary specifications

4.3.1 Event-studies: Flexible timing and pre-trend diagnostics

To estimate the period-by-period effects more flexibly and diagnose pre-trend characteristics, we employ an event-study style regression of the following form:

$$y_{ict} = \alpha + \phi_c + \gamma X_{ict} + \sum_t \theta_t \mathbb{1}[t \leq 2003Q4] + \sum_t \beta_t \mathbb{1}[t \geq 2004Q2] + \epsilon_{ict} \tag{2}$$

The indicator variables $\mathbb{1}[t \geq 2004Q2]$ and $\mathbb{1}[t \leq 2003Q4]$ denote timing of pregnancies. $\mathbb{1}[t \geq 2004Q2]$ is an indicator that equals 1 if the year-quarter of conception t is on the second quarter of 2004 or after. Similarly, $\mathbb{1}[t \leq 2003Q4]$ indicates that the year-quarter of conception t is on the fourth quarter of 2003 or before. 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 and θ_t's. These coefficients identify the effects of the Tsunami exposure relative to the first quarter of 2004. β_t estimates are used to isolate period-by-period effects of the Tsunami for conception cohorts after the first quarter of 2004 and are employed in placebo study in the next section. θ_t's estimate pre-Tsunami period effects.

The required assumption for identification of Equation (2) is largely similar to that of Equation (1). On top of the as-good-as-random selection into pre- and post-periods, we formally test validity of parallel trends assumption with Roth (2022). Our baseline test uses joint significance of the pre-Tsunami time dummies. Recent works, such as Bilinski and Hatfield (2026) and Freyaldenhoven et al. (2019), warn that such tests may be under-powered to detect violation of parallel trends assumption. To formally test the statistical power and compare with the counterfactual outcome had such violation is not detected, we apply the test suggested by Roth (2022).

4.3.2 Differences-in-differences: Cross-sectional variation in damage intensity

We also leverage variation across clusters in the extent of damage by interacting indicators for medium and heavy cluster-level damage with the exposure indicators of Equation (1), so that lightly-affected clusters serve as the benchmark:

$$\begin{aligned} y_{ict} = {} & \alpha + \phi_c + \gamma X_{ict} + \sum_{\tau} \beta_\tau \text{InUtero}^{\tau}_{ict} + \pi \, \text{PostDec2004}_{ict} \\ & + \sum_{k=2}^{3} \left(\sum_{\tau} \delta_{k\tau} \text{InUtero}^{\tau}_{ict} + \delta^{post}_{k} \, \text{PostDec2004}_{ict} \right) \mathbb{1}[\text{damage}_c = k] + \epsilon_{ict} \end{aligned} \tag{3}$$

The damage main effect is absorbed by ϕ_c. The timing coefficients β_τ retain their earlier interpretation but now apply to lightly-affected clusters, while $\delta_{k\tau}$ captures the additional birth effect of residing in damage stratum k. In Section 6.3.3 we also estimate a version interacting damage with every conception quarter, which allows the pre-Tsunami interactions to be estimated, and apply alternative binnings and a spline in the continuous damage share.

Identification here requires that clusters of differing damage would have followed parallel trajectories absent the Tsunami, and comparing effects across damage levels additionally requires ruling out selection into intensity, which parallel trends alone does not deliver (Callaway et al. 2024). Neither holds reliably. Clusters are balanced on observable levels (Table 3), but balance in levels does not ensure parallel trends, and the pre-trend tests in Section 6.3.3 are sensitive to how intensity is parameterized. The interactions show no consistent dose-response, and the comparison may be confounded by differential post-disaster dynamics, since more heavily damaged areas received significantly more aid (Section 6.3.4). We therefore treat these results as descriptive and base our estimates on the timing design of Equation (1).

5 Main Results

5.1 Timing of exposure matters

The regression results for Equation (1) are presented in columns (1) - (4) of Table 4. 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 5.4 pp compared to those conceived before the second 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. There is a smaller decline in these effects for those conceived after the Tsunami. Therefore, the negative birth effects of the Tsunami are concentrated on those in the first trimester of pregnancy during the Tsunami. The significance of our findings is robust to Romano-Wolf corrections for multiple hypotheses testing.

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 pp more likely than those that began before the second quarter of 2004 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. The size of the effects of the post-Tsunami conceptions attenuate by half compared to the fourth quarter of 2004. These findings are robust to Romano-Wolf adjustments for multiple hypotheses tests.

We compare our estimates to two studies which provide figures 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 by 17 per 1,000 pregnancies (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 are about 0.28 months shorter than those conceived before the second quarter of 2004. The gestation length of others who were exposed to the Tsunami in utero is not statistically different from zero. As with other outcomes, the effect sizes for the post-Tsunami cohorts attenuate by half. This finding remains significant after Romano-Wolf corrections.

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 5.4 pp more likely than those conceived before the second 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. Findings remain robust even after adjusting p-values with Romano-Wolf correction.

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]

20. Oliveira et al. (2021) defines fetal death rate as fetal deaths divided by the number of resident livebirths plus fetal 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. Oliveira et al. (2021) and

We conduct further pre-trend diagnostics for the timing design using Equation (2), reported in Table A1 and Figure A1. Birth effects on the first-trimester cohorts still remain. The pre-Tsunami coefficients are jointly insignificant for all four outcomes (p = 0.32 to 0.82). Based on methods suggested by Roth (2022), our tests are powered to detect violations. A linear pre-trend detectable at 80% power has a slope of 0.010 to 0.016 per quarter for the binary outcomes and 0.072 months for gestation length. A pre-trend large enough to generate our estimates would be 1.4 - 1.9 times larger, and would be detected with probability between 0.97 and 0.999. The observed pre-Tsunami coefficients are also far more consistent with parallel trends, with likelihood ratios below 0.005 for all outcomes. Figure A1 further shows that the hypothesized pre-trend lies outside the confidence intervals of the two to three earliest pre-Tsunami cohorts. Placebo tests assigning the Tsunami to earlier cohorts show no spurious effects (Figure 1); we discuss these diagnostics further in Section 6.1.

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. The negative effects somewhat remain, albeit smaller, for 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 (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 pronounced on those exposed in the earliest stage of the pregnancy.

5.2 Robustness to alternate treatment and control assignments

In this section, we examine the robustness of our finding to alternate choices of specifications. First, we allow the estimates of the effects of the Tsunami for cohorts conceived on the first quarter of 2005 and afterward to vary. In this way, we can examine not only if our findings of the negative effects on those in the first trimester of pregnancy is robust, but also which cohort among the post-December 2004 cohorts drives the post-Tsunami cohort effects. Results are reported in Table A2 in the appendix. The estimates are largely robust, and the post-Tsunami cohort effect is driven by those conceived in the fourth quarter of 2005. However, as the survey was carried out in 2006, the 2005Q4 estimates reflect right-censoring: some pregnancies were still ongoing at the time of interview. Because these

Currie and Rossin-Slater (2013) find no changes to preterm births and gestational age due to natural disasters.

outcomes are not fully observed, we caution against interpreting this coefficient as a birth effect. Excluding these incompletely observed pregnancies leaves the first-trimester estimate unchanged.

We further examine whether the effect reflects pre-existing area-specific trajectories. Interacting the covariates with a linear conception-time trend, the first-trimester effect remains significant for all four outcomes (Table A3). The results also hold for livebirth, miscarriage, and preterm birth when allowing each cluster its own linear trend, with the gestation-length effect attenuating somewhat as the additional cluster-specific slopes reduce precision (Table A4). These results indicate our estimates are not driven by differential area-level trajectories.

Then, we reduce the sample further down to those conceived in 2003 and 2004 only. In doing so, the treatment group is comprised of cohorts exposed to the Tsunami in-utero, addressing the concerns that those who conceived after the Tsunami may contaminate this group. Results reported in Table A5 show that the Tsunami effect is statistically significant only for those conceived in the fourth quarter of 2004, consistent with our main results. Furthermore, the estimates are largely similar between our main findings in Table 4.

Furthermore, we use alternate specifications that compare pregnancies whose expected gestation period overlaps with the Tsunami against all other periods. In doing so we also extend the sample period to confirm robustness to sample selection. Further explanations about the specification and the results are reported in Section B of the online appendix and Table A6. In all estimations, those in the first trimester of pregnancy at the time of the Tsunami are the worst affected. This confirms our finding in this section thus far.

6 Specification and robustness tests

6.1 Verifying timing effects: Placebo results

To ensure that the birth effect captured in our estimates is 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 (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 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 is 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 to address potential selective attrition. Migration is a common response to disasters, and if mothers pregnant at the time of the Tsunami relocated differentially, the pregnancies we observe in the exposed cohorts would be a selected subset of those that occurred, so that the estimated birth effects could reflect who remained in the sample rather than exposure itself. To identify changes in migration patterns, we use questions related to migration and residency status to construct migration histories for each individual.[22] Based on the responses, we construct four indicators: any migration experience, temporary migration, permanent migration, and residing in the same house as before the Tsunami. The categorization of our sample is reported in Table A7 of the online appendix.

We substitute the dependent variable in Equations (1) and (2) with each of these indicators. Results based on Equation (1) are presented in Table 6, with the event-study estimates reported as complementary evidence in Figure A3 of the appendix. Cohorts exposed to the Tsunami in utero are no more likely to have migrated than those conceived before the second quarter of 2004, on any of the four measures. Thus, selective migration does not affect the birth effects among the in utero cohorts. Because migration is recorded after these pregnancies concluded, it could bias our estimates only through selection into the sample, and these cohorts differ neither in migration nor in observable characteristics (Section 6.2.3).

Mothers conceiving after the Tsunami are more likely to have moved away and correspondingly less likely to remain in their pre-Tsunami residence. The event-study estimates display the same pattern for these cohorts, though each quarter is estimated imprecisely relative to the pooled indicator. This reflects the post-Tsunami dynamic rather than a threat to identification. As the results indicate that post-Tsunami cohorts faced different circumstances, this further justifies estimating this group separately in Equation (1).

22. In the survey, there is a question that asks whether the respondent has 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 report having moved but are living in the same house as before are temporary migrants. Those who report having moved and are no longer in their pre-Tsunami house are permanent migrants.

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 Figure 2. As the intent of this exercise is to examine changes in the overall trend of conceptions across time, we utilize the event-study specification in Equation (2). Further details about the regression are found in the online appendix. Across all specifications, 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.[23] 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 the true effect could be higher if all of these could be captured.

6.2.3 Testing for compositional changes of mothers

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. Specifically, we examine changes in the educational attainment of mothers and fathers as well as age of mother giving birth. If there are changes in these attributes, the effects of the Tsunami on outcomes may be confounded by these factors. Thus, it is necessary to rule these changes out. Results in Table A8 show that point estimates of coefficients of all indicators for the in utero-exposed cohorts are statistically zero, alleviating concerns of compositional change. Details of the procedures used are found in Sections A.3.1 of the online appendix.

23. 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.

6.2.4 Addressing selective survival of mothers

A further concern is that the STAR sample consists of mothers who survived the Tsunami. Because mortality was substantial in the affected communities, the mothers we observe are not a random draw from the pre-Tsunami population. Frankenberg, Sumantri, et al. (2020) document that survivors who directly experienced the Tsunami exhibited lower subsequent mortality than those who did not, indicating positive survival selection. If surviving mothers were positively selected on health and resources, the cohorts we observe would be less prone to adverse birth outcomes than the population from which they are drawn, so that survival selection would work against finding the effects we document. We therefore ask whether our findings hold when we use within-mother variation, and whether they are driven by the communities that lost the fewest mothers.

We probe this in two ways, neither of which is decisive but both of which leave our conclusions intact. First, a mother fixed-effects specification, comparing pregnancies within the same woman, yields a larger effect on all birth outcomes than our baseline (Tables A9-A10). However, we do not rely on these estimates for the following reasons. Identification rests on 79 of 1,915 mothers who contribute pregnancies both inside and outside the exposure window and on only 34 of the 179 pregnancies conceived in 2004Q4. Also, roughly half of the unexposed pregnancies these mothers contribute were themselves conceived after the Tsunami. Second, we reweight observations toward communities that lost more mothers, using the cluster-level share of respondents reporting that their biological mother died in the Tsunami.[24] All four first-trimester effects remain statistically significant at the five percent level, with point estimates similar to the baseline (Table A11). While we do not claim to have corrected for survivorship bias, the first-trimester effect persists under both within-mother comparison and reweighting toward high-mortality communities.

6.3 Cross-sectional variation in damage intensity

We also examine whether the birth effect varies with differential intensity of damage from the Tsunami across clusters. To do so, we use Equation (3) introduced in Section 4.3, starting with intensity measures provided by the dataset. Furthermore, we complement this specification with alternative measures that capture the differential cross-sectional damage following the Tsunami. Thus,

24. The share ranges from zero to 0.72 across clusters, with a median of 0.03, and is estimated from at least sixteen respondents in every cluster. Because it is reported by surviving children, it understates mortality where entire families were lost.

these specifications describe the variation in birth outcomes by exposure timing and the extent of exposure to the Tsunami.

We emphasize at the outset that we treat this cross-sectional dimension as descriptive rather than causal. As we show below, the damage comparison exhibits no reliable dose-response, is sensitive to how intensity is parameterized, and is confounded by differential post-disaster dynamics. Our identification of the in utero effect therefore rests on the timing design of Section 5. We report the damage results to characterize—rather than to causally estimate—cross-sectional heterogeneity.

6.3.1 Birth effects by damage intensity

Table A12 reports the findings from the specification that leverages timing and cross-sectional variation using the indicators of damage provided by the dataset. The timing pattern from Section 5 is unchanged. In particular, pregnancies in the first trimester of pregnancy experience the largest birth effects regardless of the extent of damage.

However, we find no consistent dose-response across damage levels. Where the damage interactions are significant, they run counter to the hypothesized gradient. The first-trimester interaction with heavily-damaged clusters implies a higher live-birth probability $(+0.15)$ and lower miscarriage (-0.14) in more heavily affected areas, which is the opposite of a harmful dose-response. We show below that this pattern does not survive alternative damage measures and is consistent with confounding. Specifically, more heavily damaged areas received substantially more post-disaster aid, which could improve outcomes independently of exposure intensity.

6.3.2 Results from alternative damage measures

We confirm the absence of a reliable dose-response using alternative measures of cross-sectional damage. We use the cluster-level share of households damaged, both in terciles and via a restricted cubic spline in the continuous share, and a household-level indicator for whether the respondent's own house was damaged. These measures are based on self-reported responses on whether their houses were damaged due to the Tsunami. Details and summary statistics are in Table A13.

We report these results in Tables A14 - A16 in the online appendix. Under a tercile parameterization, no damage interaction is statistically significant for any birth outcome. The household-level results are likewise null. The first-trimester effect remains, but its interactions with the damage indicator are insignificant. A spline in the continuous damage share yields no significant non-linear gradient at the 5% level. Where point estimates differ across damage levels, they run in the perverse direction, with heavily affected areas showing weaker rather than stronger effects. These differences are not statistically significant. Notably, the wrong-signed interaction from the damage-category specification does not appear under the tercile or household-level measures. These findings indicate that the cross-sectional variation is consistent with cluster-level confounding rather than a genuine effect of damage intensity.

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6.3.3 Assessing the parallel-trends assumption

Interpreting the damage interactions causally requires more than the parallel-trends assumption underlying our timing design. Comparing effects across damage levels additionally requires ruling out selection into intensity (Callaway et al. 2024). We assess the parallel-trends by jointly testing the pre-Tsunami damage interactions (Appendix Tables A17-A20).

We find that whether the assumption holds depends on how intensity is measured. The pre-Tsunami interactions are jointly insignificant under the tercile measure ($p = 0.251$ to 0.882; Table A18) and the damage-specific linear trends are likewise jointly insignificant in Table A19. Under a household-level damage measure, however, the pre-Tsunami interactions are jointly significant for livebirth ($p = 0.036$) and marginally so for miscarriage and preterm birth (Table A20). Thus, the assumption is not reliably satisfied. This contrasts with the timing design, where pre-Tsunami coefficients are jointly insignificant across all outcomes and the test is well-powered against violations large enough to matter (Section 5.1). We accordingly do not interpret the cross-sectional comparison as causal.

6.3.4 Post-disaster dynamics across damage levels

Areas of differing damage may have followed different post-Tsunami trajectories in recovery, aid, or migration that confound a cross-sectional comparison. To this end, we examine the amount of aid, migration patterns, and unemployment status, as these channels are likely to confound cross-sectional comparison. We find that more heavily damaged areas received significantly more aid across the sample period (Appendix Figure A5). This additional support could compensate for additional damage and explain the reversal of magnitudes across clusters. On the other hand, migration patterns and female unemployment show no differential by damage (Appendix Figure A6 and A7). Because these differences operate across communities, they are absorbed by our cluster fixed effects and differenced out by the within-cluster timing design. They therefore render the cross-sectional damage comparison unreliable but leave the timing estimates unaffected.

6.4 Robustness to measurement-quality issues

Treatment assignment may be measured with error. Gestation duration is reported in weeks for only 4.86% of pregnancies and in months otherwise, so exposure timing is recovered only to monthly precision for most observations, and pregnancies near a trimester boundary may be assigned to the wrong group. We address this in two ways (Appendix Section A.5). First, estimating Equation (1) on the month-recorded observations alone yields first-trimester coefficients close to our main estimates, all significant at the 5% level except gestation length at 10% (Table A22). Second, we trim the observations least well explained by the model, in a share equal to that recorded in weeks, to gauge the effect of misassignment. The sign of every effect is preserved, though the attenuating bound falls below significance for three of four outcomes (Table A23 and Figure A8). Since trimming discards much of the identifying variation in relatively rare outcomes, this bounds how far the estimates could move rather than telling against them.

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 first analyze whether change in household expenditures on health after the Tsunami and selective survival of fetuses may explain our findings. We then conduct additional tests to rule out other potential channels such as maternal health and socioeconomic status.

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. Summary statistics are in Panel A of Table A24 in the online appendix.

We report the result of our investigation in columns (1) - (4) in Table 7. 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 40% for mothers conceiving the baby after the Tsunami compared to those conceiving prior to the second quarter of 2004. Health spending for the first-trimester cohort is also significantly lower than for cohorts conceived after the Tsunami (p = 0.030). Births are also less likely to occur at the mother's own or a family member's home, though this appears for the third-trimester cohort, which shows no birth effect, so we do not read it as part of the mechanism. Other measures - amount spent on prenatal care and visits to the clinic, are not statistically significant at 5% level.

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 sustained harmful effects on fetal health and other demographic indicators. However, there are some caveats. As not all respondents have responded to the relevant questions, the sample size is smaller for some outcomes. Therefore, the results may not fully represent the whole sample.

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 in columns (5)-(8) of Table 7. We find that birthweight of the newborn does not rise at the 5% level in second to fourth quarter of 2004. While birthweight for newborn babies conceived post-Tsunami rises, there are no statistically significant changes to whether they are born with low or very low birth weight. For the first-trimester cohort, which bears the birth effect, none of the four birthweight measures report a statistically significant estimate. Selective survival would require the surviving births in this cohort to be positively selected on birthweight, and we find no such shift. The significant reductions in low and very low birthweight appear instead in the second- and third-trimester cohorts, for which we estimate no effect on livebirth or miscarriage, so they cannot reflect culling of the affected cohort. Harvesting therefore does not appear to explain our first-trimester estimates. Nor does birthweight fall, so the estimates are equally inconsistent with scarring. Thus, the first-trimester effect operates on whether a pregnancy survives rather than on the weight of those that do.

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, and anticipatory anxiety. Second, we use parental deaths from the Tsunami to proxy the likelihood of suffering severe stress. Summary statistics are found in Table A24 in the online appendix. 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 (1)-(5) of Table A25 in the online appendix show no significant differences in reported mental distress across conception cohorts at the 5% level, suggesting that maternal stress from the disaster is unlikely to drive our results. Columns (6) - (8) in the same table show that those in the first trimester of the pregnancy do not display statistically significant changes in their perceived health or usage of outpatient services. Mothers conceiving after the Tsunami are less likely to report unchanged health, but the share of respondents reporting worse health is only significant at the 10% level. Only those in the second trimester of pregnancy are more likely to report deteriorated health at the 5% level. However, as this cohort shows no birth effects, physical health is less likely to explain our findings. Therefore, maternal physical and mental health have limited explanatory power.

7.2.2 Socioeconomic status

Next, we determine whether birth outcomes differ depending on the socioeconomic status of mothers.[25] 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. Summary statistics for the variables are found in Panel B of Table A24 in the online appendix.

The results presented in Table A26 in the online appendix suggest that differences in socioeconomic status are unlikely to drive our main results. For those whose pregnancies were affected by the Tsunami in utero, there are no statistically significant differences in all but one socioeconomic status indicator. The significant indicator - iron roof for those at the third trimester of pregnancy - appears for a cohort with no birth effect. This is consistent with Frankenberg et al. (2011), who finds no relationship between socioeconomic status and Tsunami mortality. It should be noted that as some outcomes are estimated on a substantially smaller number of observations, these estimates may not be representative of the whole sample in our study.

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

25. 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

children, are no exception. As opposed to the later-life outcomes 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 to identify whether being exposed to the Tsunami at different stages of pregnancy leads to negative birth outcomes, drawing on variation in the timing of in utero exposure within communities.

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 livebirth decreases. Gestation length is shortened and preterm birth becomes more likely for the same cohort. There is suggestive evidence that those conceived after the Tsunami are shielded from negative effects, which we attribute tentatively to household investment in health. We find limited roles played by maternal health and socioeconomic status at the time of exposure in determining our outcomes.

Our results provide a way to measure a cost from natural disasters that is crucial yet largely absent from existing research. We highlight that in utero outcomes are a first-order human cost of disasters in developing countries, not captured by studies on later-life outcomes (Currie and Rossin-Slater 2013; Karbownik and Wray 2019; Institute of Medicine 2003). We also provide distinct mechanisms that may mitigate the negative effects from lasting over sustained periods. Channels documented in other contexts — maternal health and socioeconomic status — have limited explanatory power in our context. Rather, we find that compensatory responses in health investment after the disaster may limit sustained negative effects. Our results suggest a need for actionable guidance on how governmental and non-governmental organizations can promote children's health by facilitating household investment in health.

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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

	(1)	(2)	(3)		T-test	
	Before	In utero	After		Difference	
Variable	Mean(SE)	Mean(SE)	Mean(SE)	(1)-(2)	(1)-(3)	(2)-(3)
Literate	0.918	0.918	0.920	-0.001	-0.003	-0.002
	(0.014)	(0.018)	(0.016)			
Literate, husband	0.955	0.964	0.943	-0.009	0.013	0.021*
(0.009)	(0.009)	(0.011)				
Enrollment	0.953	0.953	0.966	0.000	-0.014**	-0.014
	(0.011)	(0.015)	(0.010)			
Enrollment, husband	0.988	0.991	0.989	-0.003	-0.001	0.002
	(0.004)	(0.007)	(0.004)			
Yrs of schooling	8.268	8.364	8.608	-0.096	-0.340*	-0.244
	(0.263)	(0.317)	(0.280)			
Yrs of schooling, husband	8.708	8.738	8.841	-0.030	-0.133	-0.103
	(0.223)	(0.245)	(0.213)			
Rural	0.716	0.725	0.740	-0.009	-0.024	-0.015
	(0.049)	(0.051)	(0.046)			
Age at birth	27.536	27.837	27.521	-0.301	0.015	0.316
	(0.213)	(0.282)	(0.197)			
Summary statistics						
Total number of clusters	107	104	107			
Total number of respondents	828	527	804			

Note: The observations included in this table reflect all pregnancies that began in calendar years 2003-05. Before column summarizes the observations for conceptions occurring before the first quarter of 2004 and not exposed to the Tsunami. In-utero indicates pregnancies exposed to the Tsunami - those conceived between second to fourth quarter of 2004. After column indicates observations conceived after the Tsunami. 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

Conception period	(1) Livebirth	(2) Miscarriage	(3) Duration	(4) Preterm birth
2004Q2 (3rd trimester)	-0.0343*	0.0288	-0.167	0.0390
	(0.0204)	(0.0194)	(0.124)	(0.0247)
2004Q3 (2nd trimester)	-0.00320	0.00979	0.0434	-0.00988
	(0.0149)	(0.0145)	(0.0745)	(0.0155)
2004Q4 (1st trimester)	-0.0536**	0.0504**	-0.282**	0.0537**
	(0.0216)	(0.0203)	(0.134)	(0.0243)
	[0.016]	[0.016]	[0.016]	[0.016]
Post-Dec 2004	-0.0310**	0.0267**	-0.140**	0.0213
	(0.0121)	(0.0107)	(0.0693)	(0.0134)
	[0.002]	[0.002]	[0.007]	[0.034]
Obs.	2159	2159	2159	2159
No. of Clusters	108	108	108	108

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

Each column regresses the birth outcome on indicators for in-utero exposure by trimester (conception in 2004Q2 to Q4). The omitted category is the pooled pre-tsunami control (conceptions 2003Q1 to 2004Q1). Post-tsunami conceptions (2005Q1 to Q4) are absorbed by a separate indicator (not shown). Mother's age at birth (level and square), years of schooling of mother and father, birth-order indicators, and cluster fixed effects are included in the covariates. Romano-Wolf p-value for correcting multiple hypotheses tests are reported for 2004Q4 conceptions and Post-Dec 2004 variables in square brackets. Standard errors are clustered at the cluster level and reported in parentheses.

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.

Table 6: Selective migration, event-study specification

	(1) Mover	(2) Temporary	(3) Long-run	(4) Same home
2004Q2 (3rd trimester)	-0.0111	-0.0290	0.0179	-0.0179
	(0.0415)	(0.0301)	(0.0367)	(0.0367)
2004Q3 (2nd trimester)	0.0177	-0.00194	0.0196	-0.0196
	(0.0338)	(0.0296)	(0.0312)	(0.0312)
2004Q4 (1st trimester)	-0.00495	-0.00539	0.000439	-0.000439
	(0.0324)	(0.0248)	(0.0292)	(0.0292)
Post-Dec 2004	0.0578***	-0.0147	0.0725***	-0.0725***
	(0.0189)	(0.0155)	(0.0201)	(0.0201)
Obs.	2159	2159	2159	2159
No. of Clusters	108	108	108	108

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

Each column represents the regression results with indicators for the migration status, temporary migration, permanent migration, and living in the same house as in the pre-Tsunami periods outcome variables across cohorts exposed to the Tsunami (2004Q2-Q4). The omitted category is the pooled pre-tsunami control (conceptions 2003Q1 to 2004Q1). Post-tsunami conceptions (2005Q1 to Q4) are captured with a single indicator. Mother's age at birth (level and square), years of schooling of mother and father, birth-order indicators, and cluster fixed effects are included in the covariates. Standard errors are clustered at the cluster level and reported in parentheses.

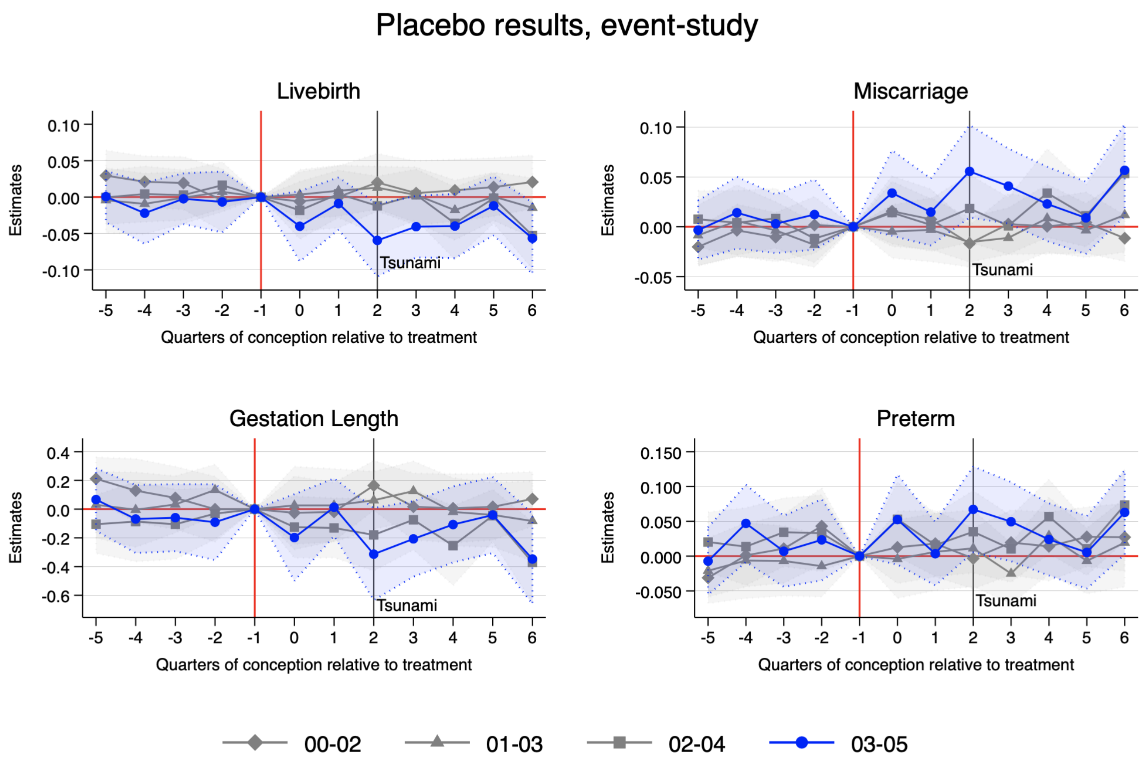
Table 7: Regression on household choices on care and selective birth

	(1) log(hlth spend)	(2) log(care spend)	(3) log(visits)	(4) own/family house	(5) Birthweight	(6) logweight	(7) VLBW	(8) LBW
2004Q2 (3rd trimester)	0.0695	0.0146	-0.108*	-0.108**	0.144*	0.0469*	-0.0153**	-0.0156
	(0.115)	(0.102)	(0.0611)	(0.0438)	(0.0833)	(0.0272)	(0.00687)	(0.0499)
2004Q3 (2nd trimester)	0.210*	0.175*	0.0139	-0.0401	0.0434	0.0229	-0.0122**	-0.0833***
	(0.125)	(0.103)	(0.0461)	(0.0343)	(0.0592)	(0.0187)	(0.00561)	(0.0316)
2004Q4 (1st trimester)	0.155	0.0796	-0.0847	-0.0466	-0.000348	-0.00112	0.00640	-0.00716
	(0.113)	(0.115)	(0.0570)	(0.0290)	(0.0750)	(0.0264)	(0.0139)	(0.0463)
post_dec2004	0.403***	0.105	-0.0546*	-0.0596***	0.104**	0.0332**	-0.00226	-0.0390
	(0.0666)	(0.0646)	(0.0280)	(0.0227)	(0.0447)	(0.0155)	(0.00786)	(0.0258)
Obs.	1876	1493	1956	2055	1325	1325	1325	1325
No. of Clusters	107	108	108	108	107	107	107	107

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

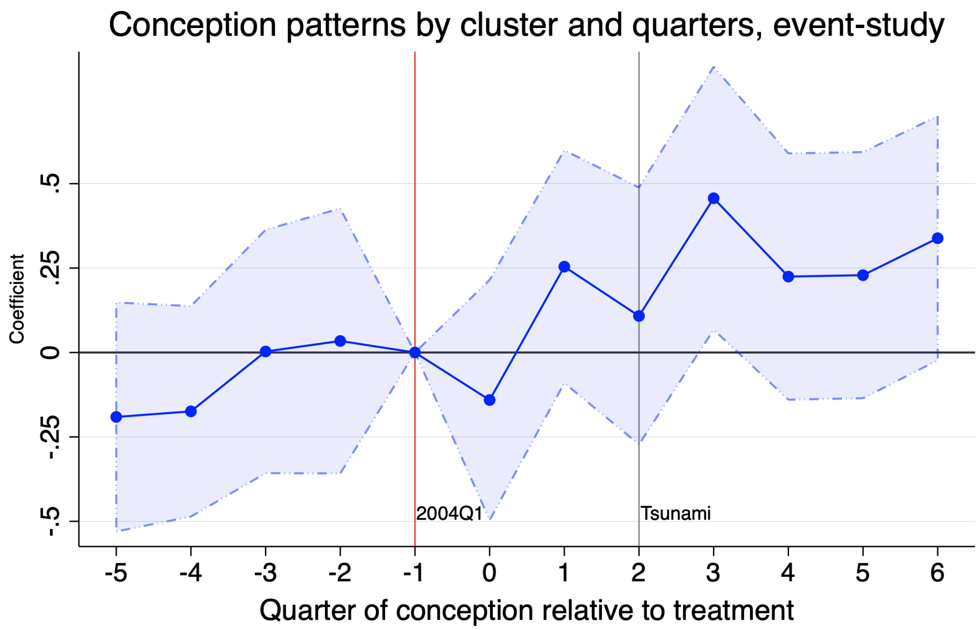
Each column represents the regression results with log of household health expenditures, log of amount spent on prenatal care, log of number of visits to prenatal care centers, giving birth at own or family member's house, birthweight, log of birthweight, very low birthweight, and lowbirthweight as outcome variables across cohorts exposed to the Tsunami (2004Q2-Q4). The omitted category is the pooled pre-tsunami control (conceptions 2003Q1 to 2004Q1). Post-tsunami conceptions (2005Q1 to Q4) are captured with a single indicator. Mother's age at birth (level and square), years of schooling of mother and father, birth-order indicators, and cluster fixed effects are included in the covariates. Standard errors are clustered at the cluster level and reported in parentheses.

Figure 1: Placebo results

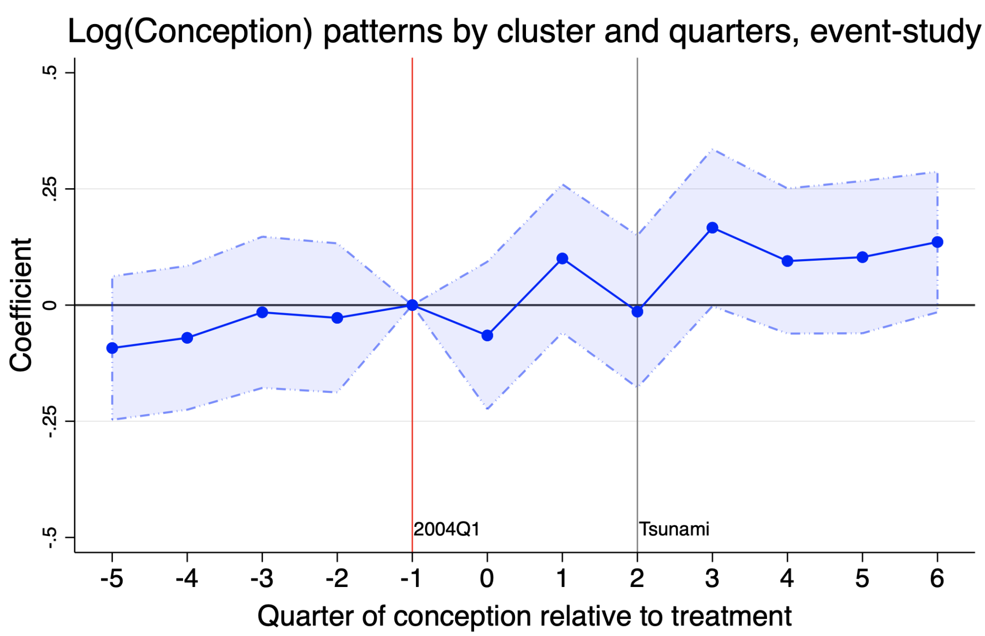


Note: Period 0 in the x-axes in both panels refers to the starting period of each treatment - 2001Q2, 2002Q2, 2003Q2, and 2004Q2 respectively. There are 108 clusters for all regressions and 1,888, 1,914, 2,007, and 2,159 observations in each regression. Point estimates and 95% confidence interval are presented. The regressions use the 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) Total conceptions before and after treatment, event-study



(b) Log of 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.