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ABSTRACT

Shaken, Not Stunted? Global Evidence on Natural Disasters, Child Growth and Recovery*

A substantial share of the world's children reside in disaster-prone areas and suffer from stunted growth. Child growth in the first 1000 days of life can falter depending on health endowments and investments. We investigate growth faltering and catch-up in children exposed to comparable earthquakes in utero. Our analysis leverages within cluster or mother variation, controls for temporal trends, and utilizes a global sample of localized data spanning several decades. On average, we document modest adverse effects on children's height that are more pronounced when earthquakes are more unexpected and higher in magnitude. These average effects, however, conceal negative short-term effects and posterior recovery mechanisms via parental health investments, economic recouping, and foreign aid, which facilitate subsequent catch-up growth of children. We discuss our findings and contributions within the literature on child health and disasters, which has largely been confined to single-country studies.

JEL Classification: I15, I18, J13, O15, Q54

Keywords: child health, natural disasters, global evidence, local data, aid

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1 Introduction

Globally, a quarter of children reside in countries touched by natural disasters and conflict.[1] A fifth under the age of five are stunted.[2] Stunting in early life has serious repercussions on economic productivity, health, and well-being over the lifecycle (Hoddinott et al., 2013). When disasters such as earthquakes (EQ) strike, incomes are lost, access to health services and food may be difficult, and families are under stress. All of the latter may cause a child's health to deteriorate. Countries, local communities, and households are, however, remarkably resilient and may rebound. After disasters recovery efforts often start quickly, and as a result, adverse macro- and microeconomic impacts may fade over time or even retract (Cavallo et al., 2013; Gignoux and Menéndez, 2016). The key question then becomes whether this recovery also helps exposed children with growth catch-up, and if so, how quickly and via which mechanisms.

To the best of our knowledge, this is the first paper using global microdata spanning several decades to analyze how in-utero exposure to earthquakes, specifically, impacts child growth. We focus on in-utero exposure to earthquakes and within mother variation as it would allow us to study a causal health endowment shock followed by a rich set of potential compensatory investments. Recent single-country studies in economics have shown that in-utero exposure to disasters including earthquakes impacts birth weights and gestational age (e.g., de Oliveira et al., 2023; Menclova and Stillman, 2020). When pregnant women are exposed to stress their unborn children may experience stunted growth in-utero regardless of physical damage (Menclova and Stillman, 2020; see also earlier work by Currie and Rossin-Slater, 2013a on stress). Similarly, Lafortune et al. (2021) review the literature showing that disaster-related maternal stress *"...could prepare the child to live in a stressful environment that is potentially no longer representative of the actual postnatal environment."* (p.2), implying the negative effects of such stress. There is evidence that child growth and mortality are impacted by large disasters (e.g., Ogasawara, 2022; Dhamija and Sen, 2022; Lépine et al., 2021). However, there is also evidence that some children may recover after disasters (Frankenberg et al., 2017; Ogasawara, 2022).

We analyze 60 countries that have had at least one earthquake from 1971 to 2021 and that feature at least one georeferenced Demographic and Health Survey (DHS). DHS are comparable across countries and interview mothers of reproductive ages on their complete birth history as well as measure anthropometrics of children under the age of five. The DHS employs a two-stage sampling framework relying (if available) on a recent census. Stratification is usually implemented by geographic area. Clusters (also known as primary sampling units or enumeration areas) are picked per strata, and households in these clusters are then picked from a listing. The DHS normally fixes the number of households to 25-30 per cluster.[3]

[1] UNICEF estimated in 2016 that "535 million children – nearly one in four – live in countries affected by conflict or disaster, often without access to medical care, quality education, proper nutrition and protection", available at *https://www.unicef.org/press-releases/nearly-quarter-worlds-children-live-conflict-or-disaster-stricken-countries* [Accessed 28 May 2023].

[2] Stunted children have body heights too short for their age compared to the WHO's growth standard. See 2020 report by the United Nations Children's Fund, World Health Organization, International Bank for Reconstruction and Development/The World Bank. Levels and trends in child malnutrition: Key Findings of the 2020 Edition of the Joint Child Malnutrition Estimates – UNICEF regions. [Accessed 18 May 2023].

[3] For more details see Guide to DHS Statistics: Analyzing DHS Data, available at: https://dhsprogram.com/data/Guide-to-DHS-Statistics/Analyzing˙DHS˙Data.htm Last Accessed [23 August 2024]).

We record earthquake events within various radii around DHS survey clusters during the in-utero period based on data from the US Geological Survey for Earthquakes. Our baseline height-for-age z-score models include close to a million children. We model child growth faltering and catch-up with parametric specifications and local polynomials smooths. We exploit (within) cluster or mother variation in in-utero exposure to earthquakes and employ a rich set of temporal fixed effects to account for country-level seasonality, secular trend differentials in child health among countries, and birth year shocks. Moreover, note that the bias coming from the non-random geographic distribution of earthquakes—only exogenous in timing—is controlled by the cluster and mother fixed effect strategy as it captures the baseline risk of experiencing them while exploiting variation in the timing of events. We also include the usual demographic covariates in the literature: gender, age (in months), and birth order. We complement these data with (i) household information on health investments from the DHS; (ii) nighttime lights from the National Oceanographic and Atmospheric Administration to proxy economic activity; and (iii) georeferenced data on World Bank aid projects.

We find that earthquake exposure in utero has a negative and significant albeit modest average impact on subsequent child growth as proxied by height-for-age z-scores (HAZ). When we explicitly model child growth dynamics (as measured by HAZ-age profiles) following in-utero shocks, we find relatively larger negative effects in the first months of life, but recovery after a little over a year. This recovery pattern is matched by mechanisms as predicted by theory: (i) there is an increase in compensatory health investments in early childhood in the form of more complete vaccinations, (ii) while nighttime lights around survey clusters—a measure of local economic activity—are reduced in the year following an earthquake, they rebound quickly, and (iii) World Bank's aid disbursements in the vicinity spike in the months after the earthquake.

We nuance our findings with several robustness checks and extensions. Investment mechanisms (especially vaccinations and aid) do have a direct effect on stunting. Mean effects do not vary much by gender. There is some suggestive, qualitative evidence that effects seem to increase when exposed in the first trimester in utero. There is no evidence of spurious time trends as tested by temporal placebo tests. We find similar results when accounting for population movements by restricting the sample to mothers who have resided in the location for several years. Likewise, we find that fertility and mortality-related effects are not driving our main findings nor concerns related to selection into mother fixed effect samples (Spears et al., 2022; see also Miller et al., 2023). In addition, alternative anthropometric indicators such as height-for-age differences (HAD) produce similar results, countering reservations about using cross-sectional data to carve out growth dynamics (Leroy et al., 2015). We also find some very suggestive but imprecise evidence of impacts on birthweight and gestational age (partly due to data limitations). Evidence on mechanisms suggests that even relatively low-magnitude earthquakes can result in stress-induced growth stunting (Menclova and Stillman, 2020). We find that effects are more pronounced when earthquake magnitudes are higher and when events are, historically speaking, *more unexpected*, suggesting infrastructure-induced effects on stunting, too.

We make five contributions to the literature: *First*, we provide global evidence on natural disasters (earthquake exposure in utero) and child growth, improving external validity of existing case evidence.[4] One previous multi-country study on child growth was limited to dust exposure in the Western African region (12 countries in Adhvaryu et al. (2024a)) and community/household level compensatory investments. Our analysis offers a more comprehensive perspective by examining data from 55 countries across Asia, Africa, and Latin America and the Caribbean along with international aid and proxies of local economic development. This significantly expanded dataset allows us to generalize our findings beyond a regional context, demonstrating the broader applicability of our results to most developing regions worldwide. *Second*, we explicitly test economic models of child health production exploiting plausibly exogenous variation in the timing of the shock as earthquakes cannot be predicted. As can be seen in Figure 1, Panel A, height-for-age z-scores of children in developing countries typically "falter" in the first two years of life (Victora et al., 2010). This growth faltering can be theorized via health production as a function of health endowments and investments (see theory and evidence in Aiyar and Cummins, 2021; as well as earlier empirical work by Rieger and Trommlerová, 2016). Figure 2 shows this theoretical framework in a nutshell. There, HAZ is plotted against child age in months. We theorize that in-utero earthquakes would negatively impact health endowments H_0, leading to a downward shift in the HAZ-age curve, while successful compensatory and age-specific investments I_a would stimulate catch-up, leading to slope changes. *Third*, our stringent fixed effect model allows us to test for such compensatory mechanisms using comparable data. At the household level, we examine the impacts on vaccinations at birth and later life, as well as pre-natal checks and facility delivery. At the community level, using further georeferenced information we proxy economic dynamics using nighttime lights and World Bank's aid inflows. *Fourth* and related, we provide causal evidence on some of the direct impacts on stunting of these investments, more specifically, on the effects of aid (exploiting variation in donor financing constraints) on HAZ.[5] *Fifth*, we provide evidence of both psychological and physical channels of damage on children's growth, aligning previous evidence that normally highlights just one of the two.

Our global evidence adds to a host of case studies across the world looking at child health or mortality (e.g., Frankenberg et al., 2017; Gaire et al., 2016; Lépine et al., 2021; Datar et al., 2013; Islam et al., 2020; Thamarapani, 2021; Dhamija and Sen, 2022; Pun et al., 2022; Ogasawara, 2022; Andrabi et al., 2023; Bustelo et al., 2012; Adhvaryu et al., 2024b; Gunnsteinsson et al., 2022), fertility (Nandi et al., 2018; Nobles et al., 2015), long run health (Noghanibehambari, 2022; Dodlova et al., 2023), mental health (Bertinelli et al., 2023), and broader human capital (Dodlova et al., 2023; Paudel and Ryu, 2018; Berthelon et al., 2021; Tian et al., 2022; Par et al., 2022; Caruso and Miller, 2015). We also add to the very limited cross-country evidence on disasters and child mortality (Ponnusamy, 2022), human capital (Caruso, 2017), and child poverty (Daoud et al., 2016). We also complement emerging case studies on the role of local aid in child health recovery (notably, Dodlova et al., 2023 on World Bank aid and child health around Haiti's 2010 earthquake). Our more generalizable findings can also be consistently compared

[4] See Table B.14 for an earthquake-specific literature summary on child health. Previous case studies cannot be easily compared due to widely different contexts and magnitudes of disaster events, as well as dissimilar empirical approaches regarding mechanisms. We also do not select specific cases to produce estimates, but cast a wider net to estimate more externally valid effects.

[5] Some previous studies have looked at aid but just by including it as endogenous covariate. Compare Table B.14.

to other in-utero shocks in pooled cross-country studies using micro data: notably, Adhvaryu et al. (2024a) use the same data source (DHS) for a set of 12 African countries affected by sand dust pollution. The authors document that in-utero exposure (rather than exposure later in life) may lead to higher risks of infant and child death. Despite compensatory household-related investment patterns quite similar to those found in our paper, children who survive are not able to fully recover in terms of health.

Our research shows the potential for compensatory mechanisms to mitigate the initial negative effects of in-utero earthquake exposure on child growth. This finding has far-reaching policy implications. Investment in early childhood development, particularly in vulnerable regions, may be effective. Both demand-side and supply-side interventions are effective in promoting catch-up for those exposed. One of our limitations is that, of course, we cannot say much about the potential implications of disasters in high-income regions, given that our health (DHS) sample stems from low and middle-income regions.

This paper is organized as follows: section 2 introduces the empirical strategy and describes the data. Section 3 presents the baseline results on the impact of earthquakes on child height along with a host of robustness checks. Section 4 tests the theoretical model of growth faltering, while section 5 provides evidence of its corresponding underlying mechanisms. Section 6 discusses our findings, and concludes.

2 Empirical strategy and data

We model the impact of in-utero exposure to earthquakes on child growth under the age of five years. To start, we consider child i, born in month m and year t to mother h, living in survey cluster v, country c and surveyed in year s and estimate the following model:

$$HAZ_{imthvc,s} = \beta_1 EQ_{imthvc,s} + X'_{imthvc,s}\beta_2 + \gamma_{hvc,s} + \theta_{c,s} + \delta_t + \mu_{mc} + \epsilon_{imthvc,s} \tag{1}$$

where HAZ is our main measure of child growth (height-for-age z-score), EQ denotes in-utero earthquake exposure (in the nine months before birth) within 111 km around the survey cluster.[6] *X'* is a matrix of child characteristics (child age in months, birth order, gender), and γ are (cluster or) mother fixed effects (FEs).[7] We also include a rich set of temporal FEs similar to the mother FEs specification by Adhvaryu et al. (2024a)—see their Table 9, column 5.[8] θ captures overall improvements in child health per country via country-specific linear survey year trends, δ accounts for birth-year FEs, and μ adjusts for seasonality via country-birth month FEs. In extensions, we also control for early life exposure (as multiple exposure is possible).

[6]The choice of a 111 km radius for the exposure clusters is informed by several factors. This distance threshold is commonly used in previous spatial research, ensuring consistency with existing literature (see among others de Oliveira et al. (2023); Sinding Bentzen (2019); Currie and Rossin-Slater (2013b)). Additionally, sensitivity analyses (columns 3 and 4 Table B.10) indicate that the results are not significantly affected by varying the radius, suggesting that the chosen distance is robust. Furthermore, the 111 km (approximately 1 degree at the equator) radius cover (around 38,000 km2) aligns with the average area covered by provinces/states (around 37,000 km2), providing a geographically and politically relevant measure of exposure.

[7]Recent work has warned that using mother fixed effects using DHS or household data may suffer from endogenous sample selection or few switchers (Spears et al., 2022; Miller et al., 2023). We show both cluster and mother fixed effect estimations but also present switching rates and effects by the number of children.

[8]We report below that estimates are robust to adding cluster-specific survey year trends.

Standard errors are clustered at the level of the primary sampling unit (or DHS cluster) v.

Health-related and child variables to estimate equation (1) stem from the Demographic and Health Surveys (DHS) (ICF, 2017).[9] We primarily rely on the birth recode files to obtain information on all births per interviewed woman of reproductive age. We calculate standardized anthropometric measurements based on the WHO Growth Standard.[10]

Our primary dependent variable is the height-for-age z-score (HAZ) for children under the age of five, although we also consider related indicators including child stunting, as well as standardized weight-for-age and height-for-age, and a low birthweight indicator. Additionally, we employ height-for-age differences (HAD) since catch-up patterns observed in cross-sectional HAZ data may mechanically stem from dividing height differences by WHO's age-specific standard deviations that increase with age (Leroy et al., 2015). In extensions, we examine fertility and child mortality outcomes.

We downloaded all available surveys from 1981 until 2021 that include GPS coordinates for primary sampling units viz. DHS clusters (2 to 10 km of spatial noise to guarantee anonymity). We build buffers of various radii around these clusters (111 km, 55 km, 25 km, 11 km) and capture earthquake events in utero of moment magnitude 4 or higher as recorded in the US Geological Survey for Earthquakes. Magnitude 4 events are noteworthy and are acknowledged by individuals, but there is typically no large-scale damage.[11] Our initial data frame includes 60 countries that have had at least one earthquake from 1971 to 2021 and at least one georeferenced DHS. Figure 3 shows DHS cluster locations and global earthquake events over the period.

We end up with 55 countries (DHS survey years 1999-2021) that enter the most basic anthropometric regression (see sample sizes by country and earthquake exposure in Appendix Table B.1, used in the unconditional OLS model in Table 1, column 1).[12] Our main analysis sample includes up to 967,679[13] children with HAZ and co-variate information and we find no economically important selection into the sample as a function of earthquakes.[14]

Table B.2 shows descriptive statistics. The mean height-for-age z-score is -1.327 standard deviations, suggesting that the average child is well below the WHO growth standard. Children exposed to an earthquake in utero of magnitude 4 or higher within a 111 km radius have slightly lower z-scores (-1.297) than unexposed ones (-1.334). About a third of children are stunted, which is much higher than the global average of about a fifth.[15] 19.3% of children for which we have HAZ information have been exposed.[16]

[9] DHS data are available at: *https://dhsprogram.com/data/* [Accessed 26 May 2023].

[10] We use *zscore06* in Stata (Leroy, 2011).

[11] Earthquake data can be explored here: *https://www.usgs.gov/programs/earthquake−hazards* [Accessed 26 May 2023].

[12] Note that some in this list feature no or little within-country variation in exposure.

[13] Compare sample size in regression in Table 1, column 1

[14] There are 1,653,870 children under the age of five in the initial data frame. We have complete age/gender records with at least one measured anthropometric record (HAZ, WAZ, or WHZ) for 988,692 children. We find that 59.1% of unexposed and 62.9% of children exposed to an earthquake in utero within 111km are included in our analysis sample. This modest unconditional difference of 3.8pp (p-value=0.00) falls to a mere 0.09pp (p-value=0.41) once cluster fixed effects are controlled for. Selection into our sample as a function of earthquake exposure in utero is unlikely to drive our fixed effect results.

[15] For 2022, UNICEF reports that 22.3% of under-five-year-olds around the world were stunted, see report here: *https://data.unicef.org/topic/nutrition/malnutrition/* [Accessed 30 December 2023].

[16] Note the exposure rates by age groups, namely: 17.8% (age 0), 19.1% (age 1), 20.7% (age 2), 19.9% (age 3), 19.2% (age 4). Exposure rates are thus qualitatively similar across age groups (about 20% as expected) and

To investigate investment mechanisms linked to our baseline child health models, we rely on child-specific information on vaccinations, antenatal care, and place of delivery—all available with gaps/missings in the DHS (see complete list of variables in Table B.2). To investigate local aid and economic mechanisms during and after earthquake years, we use two sources: first, AidData.org allows us to capture the number of World Bank projects and their amounts within an 11km radius at the cluster-year level from 1995 to 2014 (AidData, 2017).[17] Second, we use nighttime lights from the National Oceanographic and Atmospheric Administration to proxy economic activity from 1992-2020 (extension from 2013 made by Li et al., 2020), again within an 11km radius from the DHS cluster.

3 Results

Table 1 quantifies the average impact of in-utero exposure to earthquakes within 111km on height-for-age z-scores (HAZ). Standard errors are clustered at the primary sampling unit (viz. DHS cluster).[18] Column 1 presents a small, positive point estimate when only controlling for a child's age in months, gender, and birth order. Column 2 includes country fixed effects and column 3 further adds temporal trends; both models point to a small positive correlation. These specifications do not take into account that earthquakes are plausibly exogenous (in timing) to households but not geographically random. The positive sign reverses once we account for local unobservables. When including cluster fixed effects (column 4), the coefficient associated with in-utero exposure falls to -0.030 standard deviations. The coefficient grows in absolute magnitude once mother fixed effects are included (column 6): Earthquake exposure reduces HAZ by 0.055 standard deviations, which amounts to a very modest but significant 4% reduction relative to the sample mean in the control group (-0.055/1.334). [19] The effect translates to 3.2% (-0.055/1.721) of the unexposed group standard deviation in HAZ, which is equivalent to a very small Cohen's D.[20]

distributional shifts would unlikely bias our findings.

[17]We only use aid projects with exact location georeference.

[18]Near identical standard errors are obtained when clustering at the mother and/or cluster level (results included in replication folder).

[19]Note that qualitatively similar results are obtained when we use the DHS-supplied z-scores using the new and old WHO standards (results in replication folder). Also, while the DHS tends to survey a similar number of households per primary sampling unit, one may wonder if results are driven by cluster sizes. In our analysis sample, the average cluster size (in our case, number of children per mother) is 1.41 (median = 1;SD = 0.59; min = 1;max = 9). When we drop cluster sizes with fewer than 2 observations—below 0.75 quantile—the point estimate associated with the exposure dummy is -0.055 (p-value = 0.000). Our results are thus relatively robust to cluster size. Similarly, the point estimate remains comparable when adding cluster-specific time trends (-0.055, se=0.013).

[20]One might be concerned that differential survey response rates might drive some of the results. These could occur at the household, woman and child level. Unfortunately, we cannot document response rates by exposure status at the cluster level. Do note, however, that response rates to the DHS survey tend to be very high and thus are unlikely to pose a major source of bias. For instance, in a sample of 17 countries, 13 countries saw household response rates of over 97% (available at https://dhsprogram.com/pubs/pdf/AS12/AS12.pdf [Last Accessed 13 October 2024]). Likewise, another report indicated average response rates of 97.5% for households and 95% for women across surveys (available at https://unstats.un.org/unsd/hhsurveys/pdf/Chapter˙22.pdf). [Last Accessed 13 October 2024].

3.1 Basic robustness checks

Before exploring effect heterogeneity in terms of exposure intensity and age, we present a series of extensions relating to gender, temporal placebo tests, maternal residency length, fertility and mortality-related effects, biased mother fixed effect estimations, and alternative anthropometric indicators (all presented in Online Appendix B):

(i) Table B.3 shows economically similar estimation patterns and effects among male and female children. Point estimates among females stemming from the cluster and mother fixed effect models are economically largest in absolute magnitude but remain modest. (ii) Temporal placebo tests in Table B.4 show no systematic patterns or pre-trends in the years leading up to the in-utero period. Our small and *negative* baseline effect clearly stands out from the data, in particular in the mother fixed effect estimation in column 2. The same holds true for extended placebo tests on HAZ, and on other relevant health outcomes visualized in Figure A.3.[21](iii) Earthquakes may lead to (endogenous) population movement and sorting. We thus restrict the sample to mothers who have resided in the survey location for up to five years of childhood plus two years prior to birth to also cover conception and pregnancy. Table B.5 shows that results remain significant and similarly modest in magnitude. (iv) We also examine fertility and mortality-related outcomes using each mother's entire birth record—both of which may respond to earthquake events.[22] Table B.6 points to a small increase in the likelihood of a younger sibling (0.8 pp in column 2 with mother fixed effects over a control group base of 27%), which in turn may partly explain the adverse HAZ effects as this would increase the costs of providing quality nurturing.[23] In parallel, there is no effect on child mortality due to exposure in utero, which implies that selective mortality unlikely leads to the modest adverse effects on HAZ. (v) The DHS only measures children under the age of five, which—depending on the research question—may impact the external and internal validity (of within-mother variation). While the mother fixed effect estimation has the advantage of controlling for maternal unobservables, it relies on few "switchers" as defined by Miller et al. (2023): we find that 4.39% of mothers (or 6.03% of analysis sample observations) display "within" variance in treatment status. As recommended by the authors, we first assess the covariate balance by switcher status. As expected and in line with the nature of the DHS survey sampling under 5s only, mothers with switching status have 0.64 (p-value=0.000) more under 5s. In a second step, we test for heterogeneity of treatment effects across the number of under 5s. Table B.7 shows small negative point estimates across mothers with 1, 2 or 3+ under 5s. These effects are qualitatively similar to our main one in Table

[21]See Figure A.3 for an extended placebo test on HAZ and on other relevant health outcomes. As seen, the results point in the same direction as in Table B.4, and in parallel, how the exposure in utero is more relevant than the exposure in other potentially plausible periods (first years of life). Note that pre-utero effects in these graphs can be interpreted as temporal placebo tests. Relatedly, we also tested the role of in-utero exposure on cluster's, time-varying traits like mean age (in months) and sex composition, finding as expected, not statistically significant placebo effects. Details of the latter tests are available upon request and included in the replication folder.

[22]See evidence in Nandi et al. (2018); Nobles et al. (2015)

[23]Stress during pregnancy may also lead to terminated pregnancies. In an unreported test, we examine how a mother's average earthquake exposure (across all birth records) impacts the likelihood of reporting ever having a terminated pregnancy, controlling for the number of kids. One caveat is that there are comparability issues across surveys (see discussion of v228 here: *https://www.idhsdata.org/*). That said, 17.4% of control mothers report a terminated pregnancy. We find that (mean) in-utero exposure if at all decreases the likelihood of a terminated pregnancy by just 0.35pp.

1. We fail to reject equal impacts across mothers with 1 vs. 2 or 3 under 5s at conventional levels in cluster fixed effect models. Also in mother fixed effect models point estimates between having 2 and 3+ under 5s are statistically/economically similar. In sum, while selection into the mother fixed effect sample might be driven mainly by the number of children (as shown by Spears et al. (2022)), we do not find substantial impact heterogeneity along this dimension. In addition, cluster and mother fixed effect estimation yield comparable results on average and in the age-interacted models. (vi) Finally, we find consistent patterns using alternative anthropometric indicators employing the same cluster or mother fixed effect specifications: Appendix Table B.8, columns 1 and 2 report that the probability of child stunting increases by up to 1 pp which amounts to a small increase of 3% over the control group mean (0.010/0.339). Rather than standardizing the height deficit by the standard deviation in the reference population and for easier interpretation, columns 3 and 4 show a very small albeit significant adverse effect on the difference in cm between the child's height and the median height in the WHO growth standard (HAD) (Leroy et al., 2015). Similarly negative but modest impacts emerge for weight-for-age z-scores (WAZ) in columns 5 and 6—a composite indicator of both health capital and more immediate nutritional inputs. When we look at weight given height in the form of weight-for-age z-scores (WHZ) in columns 7 and 8, then the adverse impact vanishes entirely, conveying that earthquakes impact the accumulation of health capital as proxied by height. Columns 9 and 10 show no significant effect on low birthweight likelihood. Here we suffer from a much-reduced sample size and known reliability/availability issues with DHS information on birthweights (i.e., missings, accuracy; see similar patterns in Adhvaryu et al., 2024a). However, we explore this further by restricting the sample to children with a health card (rather than maternal recall) differentiating by trimester of exposure, and also considering birthweight in grams. Table B.9 gives some suggestive evidence that the impacts might be negative for birthweight (in gr.). The coefficients are however not statistically significant and sample sizes are much reduced, thus they should be taken with a grain of salt.

3.2 Alternative geographical and magnitude indicators of exposure

Table B.10 explores alternative indicators of exposure in utero. Columns 1 and 2 restrict the sample to *unexpected* earthquake events, namely to locations with less than one event between 1971 and 2021. A caveat is that our sample size is much reduced. Still, as one would expect if preparedness and experience matter, absolute magnitudes are about three to four times larger (cluster FEs, 0.119 vs. 0.030; mother FEs 0.194 vs. 0.055). Columns 3 and 4 zoom in on proximity by splitting the exposure variable into different radii. This can be thought of as a stepwise function. The excluded category comprises children who were not exposed to an earthquake within 111 km in utero. There is no significant evidence of a gradient,i.e., we do not find statistical differences among the four exposure coefficients at conventional levels. In the remaining specifications we will continue to work with the largest and most inclusive radius also given that previous disaster studies have worked with equally large radii (see for instance de Oliveira et al., 2023, p.833).

We can further gauge if effects are channeled via intensity/damage rather than just stress experienced during lighter quakes. In the case of an earthquake in New Zealand, featuring infrastructure and health services equipped to cope with earthquakes, Menclova and Stillman (2020) find no effect heterogeneity by extent of physical damage and argue that *"stress caused by the earthquakes, rather than reduced infrastructure or direct impacts on individuals, was the main channel leading to negative effects on children."* (p.1707) Columns 5 to 8 show that intensity exposure in utero is negatively and significantly correlated with HAZ. We find that adverse effects are larger for more important earthquakes when exploiting a stepwise function in columns 9 and 10 (see also Table B.11).[24] A caveat here is that very few children fall into the highest exposure categories so gradients and demanding fixed effect estimations have to be taken with a grain of salt.[25] That said, there is consistent evidence (between cluster and mother fixed effect models) that earthquake events in the 7 magnitude range decrease HAZ by a sizeable 0.19 standard deviations, which amounts to about 14% relative to the control group mean (0.19/1.334). Such magnitude earthquakes are quite rare and are considered very destructive. These larger effect sizes could thus be interpreted in that stress is not the only underlying mechanism in low/middle income settings.[26]

3.3 Exposure at other ages and across trimesters

Thus far we have focused on exposure in utero because our interest lies in carving out recovery potential in the first five years of life. Naturally, earthquake exposure in early life may *independently* impact HAZ, and also leave some room for recovery. Children may be impacted at several points in time: the majority of children exposed in utero also experienced earthquakes in the early stage of life—hinting at so-called earthquake swarms. Table 2 shows that the small but significant effect associated with exposure in utero remains robust when we include several early-life exposure controls (dummies for exposure at ages zero to five).

The final columns of the same table further break down exposure effects by allowing them to vary by trimester in utero. Here, the trimester effects are statistically equivalent at conventional levels. However, there is qualitative evidence that effects are larger if exposure happens in the first trimester. These early life but also trimester patterns are relevant for a potential direct channel leading to relatively low height in early life. Earthquakes or stress exposure may lower gestational age. Thus, the earlier the exposure in utero, the more likely gestational age, and consequently, height are reduced. Columns 5 to 8 of Table B.13 point qualitatively in this direction.[27]

[24]Note that *"[b]ecause of the logarithmic basis of the scale, each whole number increase in magnitude represents a tenfold increase in measured amplitude as measured on a seismogram."* See USGS website, available at *https://www.usgs.gov/programs/earthquake-hazards/earthquake-magnitude-energy-release-and-shaking-intensity* [Accessed 9 February 2024].

[25]Exposure rates by magnitudes in the full sample (N=988,692) are 19.3% (Modest/Considerable), 0.7% (Considerable), 0.1% (Considerable/Catastrophic).

[26]Note we reject the equality of all significant exposure coefficients of columns 9 and 10.

[27]There are a couple of caveats to keep in mind for Table B.13. One, the sample available for these computations is much smaller than the one used in our main tests. Second, the patterns pointing at the first trimester negatively and significantly impacting gestational age are only evident for a subsample of unexpected earthquake events. Therefore, while there is some suggestive evidence pointing at the role of first trimesters on negative height outcomes, this should be taken with a grain of salt.

4 Child growth faltering and catch-up

Until now we have quantified the average impact of in-utero exposure to earthquakes, which may conceal temporal dynamics and investment responses. In this section we test a standard theoretical framework by Aiyar and Cummins (2021) as summarized in Figure 2. Health production H is a function of endowments H_o and age-specific investments I_a and can be written as $H = f(H_o, I_a)$. We predict that an initial endowment shock due to an earthquake leads to a downward shift (intercept change) in the HAZ-age profile. Similarly, a positive slope change of the HAZ-age curve after the shock would point to compensatory parental or local investments. To test our model we interact the earthquake variable with age in months, as well as a structural break dummy at 24 months.[28] This approach is inspired by recent papers modeling the HAZ-age curve (Rieger and Trommlerová, 2016; Aiyar and Cummins, 2021), but with an added exogenous in-utero shock.

Table 3 summarizes the results with cluster fixed effects (column 1) and controls for early life exposure to earthquakes (column 2), as well as mother fixed effects (column 3). We focus on the first 24 months of life. All specifications include early-life exposure controls as above, but results are unaffected by this inclusion. In line with the theoretical model, estimates show an initial drop in height-for-age that vanishes with age—see negative main impact and the positive slope associated with the earthquake x age in months interactions. Estimates of our preferred mother fixed effect model in column 3 imply that at age zero, earthquakes cause a sizeable 0.208 standard deviation or 15.6% reduction in HAZ (-0.208/1.334).[29] Before the age of two, exposed children catch up 0.013 standard deviations per month. By 16 months (0.208/0.013), they have caught up to unexposed children.

For robustness, we also estimate a completely flexible specification in Appendix Figure A.1. First, we purge cluster and mother fixed effects and the set of temporal trends from our dependent variable (HAZ) in graphs a) and b). Then we use the resulting residuals in a local polynomial smooth akin to Figure 1.[30] The graphs paint a similar picture as the previous parametric specification of Table 3: earthquake exposure leads to an initial downward shift (intercept change) and subsequent catch-up (slope change) compared to unexposed children of the same cluster or mother.

To further elucidate the *net impact* of exposure and age dynamics, we also compute effects by *year of age*, interacting exposure with year of age dummies (unreported analysis but included in replication folder): In the first year of life, the exposure effect is -0.109. In the second year of life the effect is -0.011 (p-value=0.334) and it turns positive in life years three (0.077,

[28] Previous work has estimated and used structural breaks by region (Rieger and Trommlerová, 2016) suggesting that breaks in months average between 18 in Sub-Saharan Africa, 21 in Middle-East North Africa and South Asia regions, 22 in East Asia Pacific, 23 in Latin America, and 35 in Europe Central Asia. For simplicity and given that we work with a smaller set of (earthquake-exposed) countries, we opted for the average of these reported estimates at about 24 months. However, we also ran a robustness check where we used an 18-month break instead, which implies that by the age of 16 months, the initial negative effects are overcome (compared to 16.6 months when using the 24-month break model). Our results seem relatively robust to the choice of break-point.

[29] This translates to a small Cohen's D of around 12% (-0.208/1.721).

[30] Note that Figure 1b shows that the HAZ of the exposed children outperforms those of unexposed ones from around 20 months. However, this graph does not account for any unobservables and is meant to illustrate the shape of the curves rather than causal magnitudes.

p-value=0.000) and four (0.017, p-value=0.095). We find that exposure in the fifth year of age settles at -0.014. However, such a "net" effect is economically small and insignificant at conventional levels (p-value=0.169).[31]

One recently voiced concern with estimating HAZ-age profiles on cross-sectional data is that height differences are standardized by the WHO growth standard's age-specific standard deviation. The standard deviation increases in age so that any catch-up patterns may partly stem from an increase in the denominator, rather than a shrinking height difference in the nominator (see details in Leroy et al. (2015)). Appendix Table B.12 focuses on the nominator in the form of height-for-age differences (HAD) in models corresponding to columns 1 and 2 of Table 3.[32] As before, there is a negative effect (-0.466cm) at birth that diminishes by 0.043cm every month before the age of two. By about 10.8 months the gap is closed (0.466/0.043). In other words, catch-up patterns are similar but slightly quicker when using HAD as an indicator.[33]

Overall, we would argue that age-patterns are consistent with a theoretical model where a pronounced initial HAZ shock leads to large compensatory investments in the short term, resulting in catch-up and reduced investments thereafter (relative to the control group). We also find evidence that the magnitude of investments and causal variation in aid explain catch-up growth.

To place these findings in the literature, we can compare our catch-up findings to experiments in the literature where children are moved from an unfavorable environment (via adoption) to a much more favorable one. Leroy et al. (2020) summarize 11 such studies featuring 13 estimates of linear growth catch-up: 9 of 13 estimates point to substantial catch-up in HAD. Complete catch-up in HAD was seen in 2 cases; when children moved from Romania (Eastern Europe) to the US, HAD in cm went from -2.5(-3.3) to 3(3.3). The authors also argue "that the potential to catch up might be larger when growth retardation is more severe." (p.1035) A drawback of these studies are small sample sizes (ranging from 33-128) and the possibility of positive or negative selection into adoption and no counterfactual design. Our study is different in nature in that we are looking at a one-off temporary shock in low- and middle-income countries settings where stunting is the norm followed by subsequent investments. We also exploit counterfactual models.

In sum, the average negative effect on child growth appears modest, but this disguises a dynamic process: larger initial impacts are followed by the activation of recovery mechanisms, leading to a catch-up effect.

[31] Also the non-parametric and our own parametric fixed effect models point to a small negative net effect at the end of life year 5. For example, Table 3, column 1 implies an effect of -.0258961 (p-value=0.028) at age in months 59. In the case of HAD (see subsequent discussions on this indicator), the net effect is small and insignificant (0.026, p-value=0.574).

[32] We calculated height-for-age differences (HAD) using tables from $https://www.who.int/tools/child-growth-standards/standards/length-height-for-age$ [Accessed 20 December 2023].

[33] For completeness and so that a reader can get a sense of the HAD-age profile, we show the unconditional polynomial smooth in Appendix Figure A.2

5 Recovery Mechanisms

We explore two sets of mechanisms, at the household and local levels. *First*, Table 4 shows health investments inspired by Adhvaryu et al. (2024a). Exposed children are relatively more likely to have complete polio, DPT, and measles vaccinations.[34] For instance, in mother fixed effect estimations, polio and DPT vaccinations (usually requiring 3 doses at 6, 10, and 14 weeks of age) are 1.4pp and 1.6pp higher in exposed children (compare columns 2 and 4). Likewise, the likelihood of measles vaccination (usually administered at 9 months or later) increases by 2.3pp following the in-utero shock (column 6). Consistent with these effects, the total number of shots also increases by 0.092 shots over a base of 5.1 shots in control (column 8). These effects are significant even after adjusting for multiple hypothesis testing.[35]

A different picture emerges when considering pre- or at-birth investments: exposed children are equally likely to have at-birth polio or BCG vaccinations (columns 9 to 12), a prenatal check, or a facility delivery compared to control children (columns 13 to 16). At first glance, one would argue there is little evidence showing that the endowment shock induced by earthquake exposure in utero leads to immediate compensatory investment. Rather the compensatory investments seem to materialize as children age and parents discover their child's health status. While these effects are very modest in magnitude, they line up with HAZ-age patterns and hint at broader parental (or also supply-driven) compensation after birth. In extensions, we also find plausible evidence that investment effects vary across earthquake magnitudes. In Table B.11 we show that for earthquakes causing more than considerable damage (7+ magnitude) the impact on investments is qualitatively different, i.e., mostly being negative for at-, and post-birth investments.

Second, we consider the impacts of earthquakes on cluster-level economic and aid dynamics. As we mentioned before, to identify potential broader compensation mechanisms at play we would need to test the role of a richer set of economic investment measures. To this end, we collapse our data to the cluster-(birth)year level and follow nighttime light dynamics (inverse hyperbolic sine transformed). Previous literature has shown that earthquakes have differential short and long-term effects (see recent discussions by Skoufias et al., 2021 on using nighttime lights at different scales and from different sources and frequencies to quantify the effects of disasters, as well as earlier work by Skoufias et al., 2017). Both economic damage and increased human activity—due to humanitarian activities and reconstruction—may coincide in the year of a disaster. Nighttime light measurements in themselves will not allow differentiating these opposing phenomena. To at least partially capture net dynamics, we estimate an annual (difference-in-differences) event study using the approach by Callaway and Sant'Anna (2021).[36] Figure 4 shows that three years prior and including the year of earthquakes treated and control units have similar nighttime light trajectories. The null effect in the year of the event could stem

[34] See information on vaccination variables by the DHS program: *https://dhsprogram.com/data/Guide-to-DHS-Statistics/Vaccination.htm* [Accessed 26 May 2023].

[35] We calculate q-values using the procedure outlined in Andersen et al. (2008) and using the author's associated do-file. The q-values corresponding to the 8 p-values in the mother fixed effect models (even columns) are 0.001, 0.001, 0.001, 0.001, 0.914, 0.914, 0.445 and 0.563.

[36] We do not use monthly or daily nightlight data for this analysis as it is more volatile (see discussions around VIIRS data in Skoufias et al., 2021 and Tveit et al., 2022). Also, the computational demands would be massive for noise adjustments and multiple period difference-in-difference modeling at a global scale.

from the opposing phenomena of damage and relief/reconstruction. In year 1 after the event, we find a negative effect on nighttime lights, possibly due to the end of relief efforts, with a subsequent upward boom and return by year 5.[37] The patterns point to relatively fast economic recovery, adding to previous macroeconomic and microeconomic studies (Cavallo et al., 2013; Gignoux and Menéndez, 2016), which in turn suggestively matches growth patterns we find among children.

To assess local aid dynamics—as captured by high-frequency, monthly World Bank disbursements—we first purge cluster and month-country fixed effects, and then plot residuals by exposure group in Figure 5. In the twelve months prior to the earthquake events all groups receive similarly low funding amounts. Following the earthquake event, we see important spikes in the most exposed groups (11km, 27km, 55km) that happen within the first 12 months and up to month 48. We do not detect similar spikes for the least exposed group (111km).[38] These patterns suggest that children living close to exposed areas may benefit from aid-related compensatory investments (Dodlova et al., 2023).[39] Unfortunately, we do not have global-scale information on disbursements specifically from humanitarian and state actors that might help to refine this analysis, in particular within the first weeks of the disaster.

Having said this, these associations between investments and earthquakes do not necessarily show a causal connection between such investing and our main variables of stunting. Thus, we start by exploring the impact of household investments as proxied by the number of vaccination shots after birth on HAZ. Graphs a) (unconditional) and b) (purged cluster and temporal fixed effects) of Figure 6 plot the HAZ-age profile for children exposed to earthquakes. The dashed-line fit includes exposed children with seven vaccination shots after birth (the maximum number in the sample), while the solid fit includes exposed children with less than four (below the 25 percentile in vaccination numbers). An interesting qualitative pattern emerges in this high vs. low investment comparison. High investment is positively linked with better HAZ outcomes across ages and also improved catch-up, both in the unconditional and conditional models.

Similarly, to address potential concerns about the absence of exogenous variation in the economic compensatory responses explored above, and following previous literature (Cruzatti et al., 2023), we use an instrument for aid flows to study their direct effects on children's health.[40] We use plausibly exogenous variation in aid flows to a cluster by interacting the historical probability of a cluster receiving aid with yearly global financing constraints.[41] Figure 7 shows a causal effect of aid on HAZ. We find that the effect of aid materializes significantly after 1 and 2 years and becomes insignificant thereafter. This is in line with Figure 5 which

[37] In case analysis of Nepal's 2015 earthquake, Tveit et al. (2022) find similar patterns in monthly fixed effect models with immediate negative effect, followed by positive effects and subsequent "*tapering off*" (p.2).

[38] We attribute the spikes to the volatility of the time series as World Bank funding is not continuous and can be thought of as discrete events.

[39] Dodlova et al. (2023) examine the nutrition and schooling effects of Haiti's dramatic earthquake in 2010 using several rounds of DHS and georeferenced World Bank aid. They find a significant and positive correlation between HAZ and aid (see their Table 2, columns 1 and 2, p.15).

[40] Cruzatti et al. (2023) show a causal relationship between aid and children's health (mostly proxied by infant mortality) in a comparable data and design setting.

[41] We compute the historical probability of receiving aid by dividing the years the cluster received aid over the total years of the World Bank's time sample (20, 1995-2014). The "yearly global financing constraints" are proxied by capturing information on World Bank's IBRD's equity-to-loan ratio and IDA's funding position specified in their annual statements.

shows that most of the financing in the EQ nearest regions take place between the first and second year after the EQ, and more importantly, matches initial catch-up patterns in HAZ in the *by year of age* regressions detailed in Section 4. Exposed kids perform worst in the first year of life and in the long term catch-up growth leads to an insignificant net effect on HAZ.

6 Discussion and Conclusion

We study the local impact of earthquakes on the growth process of children under five years of age on a global scale. We observe that earthquakes, while stressful and sometimes disastrous for local populations and children in the short term, also lead to compensatory dynamics. This echoes previous macroeconomic (notably Cavallo et al., 2013) and microeconomic findings (Gignoux and Menéndez, 2016): natural disasters cause short-term economic harm, but also trigger subsequent recovery mechanisms that allow for catch-up.[42]

Building on efforts to model child growth dynamics using cross-sectional surveys (Rieger and Trommlerová, 2016; Aiyar and Cummins, 2021), we find that after initial undergrowth in the first year of life, children can catch up. Previous literature has neither explored growth dynamics using this rigorous HAZ-age profile approach nor using within-mother variation in a large, global sample. These patterns also hold when using height-for-age differences (HAD) which are less sensitive to spurious catch-up patterns when using cross-sectional survey data (Leroy et al., 2015). Similar patterns have been found in some cases, for instance, for the very catastrophic 2004 Aceh Tsunami (Frankenberg et al., 2017): children exposed in utero have a lower HAZ of about a third of a standard deviation at age 2. This magnitude is much larger than our findings even directly after birth, which may be explained by the fact that our paper also includes smaller disasters in the exposure treatment. We did find much larger mean effects when exploiting rare, high-magnitude earthquakes. That said, Frankenberg et al. (2017) report that at "*age 5 years, these deficits are eliminated for all children who were in utero at the time of the tsunami except for one group of children. Mothers of 10% of children reported very high levels of post-traumatic stress reactivity.*" (p.21).[43]

We find signals of plausible recovery mechanisms at the household and local level: exposed children are more likely to be vaccinated in childhood. Needless to say, our findings have to be interpreted with caution. Foremost, we solely looked at anthropometric dynamics in the first years of life. The DHS does not systematically feature high-quality birth outcomes (length for gestational age, Apgar scores, etc.). In addition, it would be interesting to also look at exposure in later periods as well as long-term effects; there is a second window of intense growth during puberty. Earthquakes and resulting health impacts may have more lasting effects on human capital or cognitive outcomes as well. Caruso (2017), for example, estimates impacts on human

[42] See related discussion of Cavallo et al (2013) and follow-up evidence by Aksoy and coauthors on the Vox-EU blog, available at *https://cepr.org/voxeu/columns/impact-2023-earthquakes-turkiyes-economy-first-estimates* [27 May 2023].

[43] In another report, the same research consortium writes: "*...three years later, these children have caught up to or surpassed height-for-age of their older counterparts suggesting that the post-tsunami reconstruction will have long-term impacts on health and well-being.*" See *The Study of the Tsunami Aftermath and Recovery (STAR): Resilience and Recovery in Sumatra after the 2004 Indian Ocean Tsunami available at: https://ipl.econ.duke.edu/dthomas/STAR/Overview.pdf [Accessed 8 February 2024].*

capital that last across generations looking at earthquakes in Latin America (for similar evidence from Peru see also Caruso and Miller, 2015).

Our paper also speaks to recent case evidence on local World Bank aid and child health following disasters: Dodlova et al. (2023) report that aid is significantly and positively correlated with HAZ. That said, in their work, the negative point estimate associated with earthquake exposure remains even after World Bank aid is included as a control variable. The authors determine that aid was only partially helpful. Rather than controlling for endogenous aid, we preferred to model aid as a dependent variable in a mechanism regression, qualitatively map patterns into child growth dynamics, and unveil the causal link between aid and stunting—leveraging an interacted instrument of World Bank's liquidity and sub-national probabilities of receiving such aid. We present global evidence that World Bank aid (as a proxy of broader aid) is one plausible mechanism of recovery.

One of the advantages of our global approach is that it allows us to compare mechanisms to other studies looking at different shocks but similar dependent variables: Adhvaryu et al. (2024a) study dust exposure in utero using a set of pooled DHS from 12 African countries. They document very similar compensatory investment dynamics, yet unlike in our study, their headline finding is that "*[d]espite these efforts, surviving children still exhibit negative health impacts.*" (Adhvaryu et al., 2024a, p.1). One potential explanation for this final, divergent result is that we also find evidence of local economic mechanisms following the destruction caused by earthquakes. Dust pollution on the other hand is a less materially destructive and more invisible health upset that might not get such an economic support.

We add external and causal validity to a handful of case studies on EQs and child anthropometrics that have used dissimilar methods and tests of mechanisms and that have yielded mixed findings (see Table B.14).[44] Overall, while we find that earthquake exposure in utero adversely impacts the growth of children, we also show that compensatory mechanisms can lead to catch-up. Children are shaken, not permanently stunted.

[44] Note that three of the countries in our data are case studies previously explored in the papers presented in Table B.14: Colombia, Haiti, and Pakistan.

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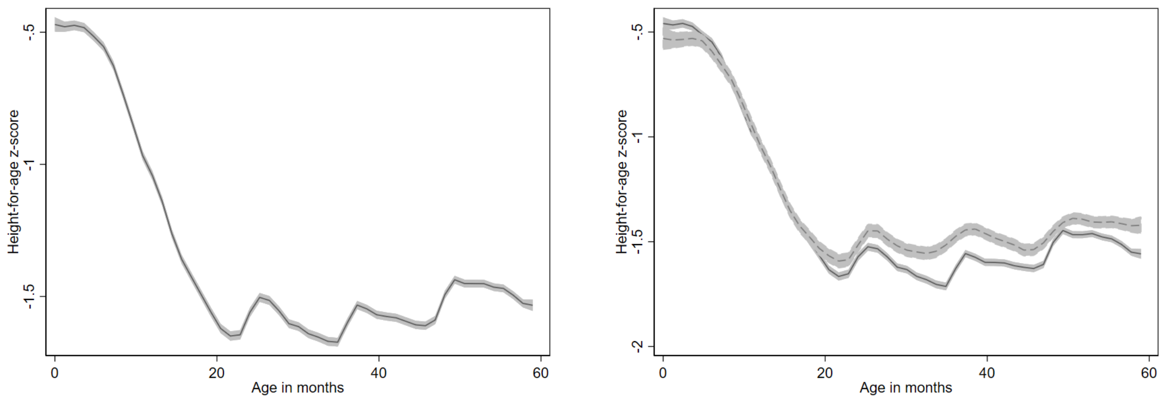
Figure 1 – HAZ-Age profiles proxying child growth faltering (children under 5 years of age)

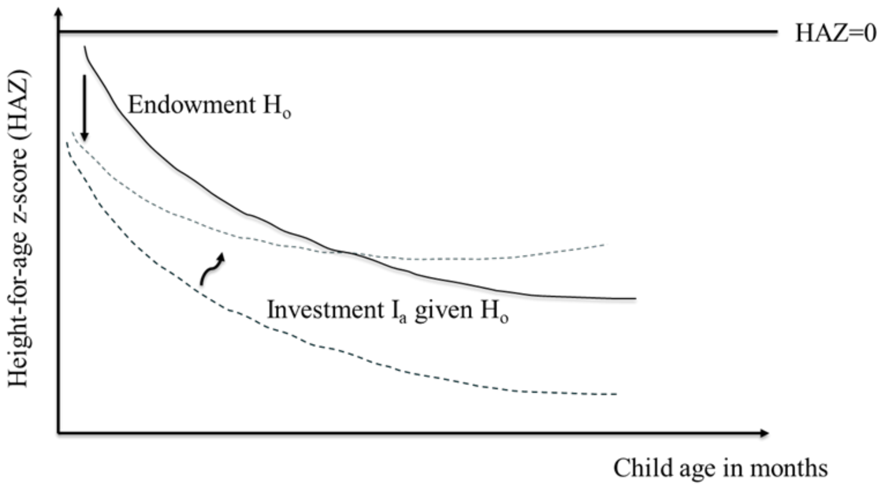
(a) Overall

(b) By earthquake exposure (solid line is control, dashed line was exposed to earthquake in utero, within 111km)



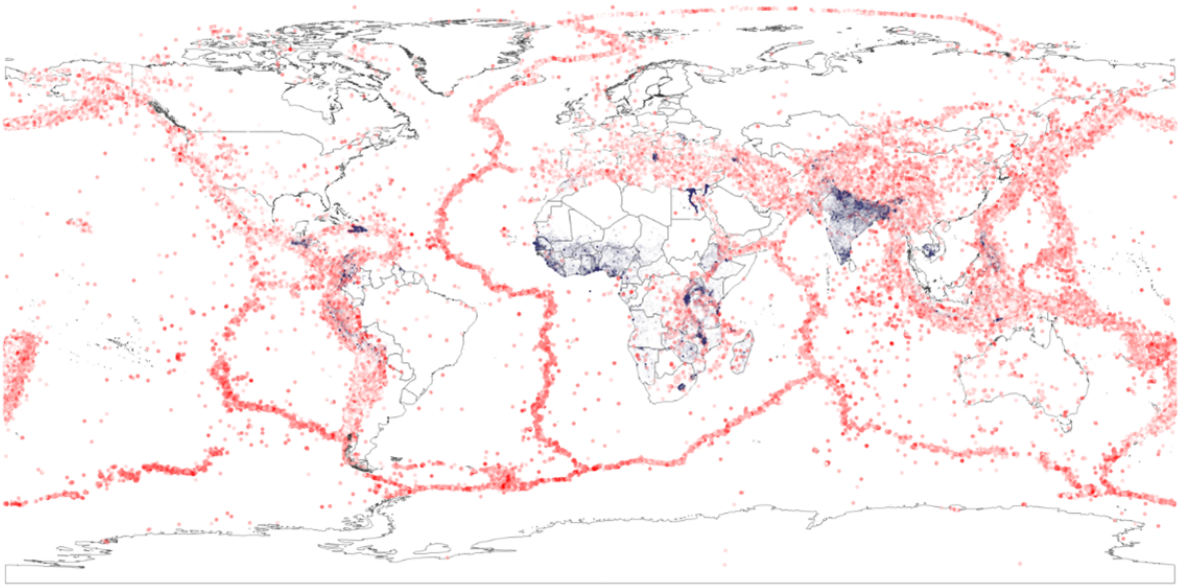
Notes: Local polynomial smooth. Own calculations and graphs using analysis sample of Demographic and Health Surveys. No covariates.

Figure 2 – Endowment shocks, investments, and child growth faltering and catch-up



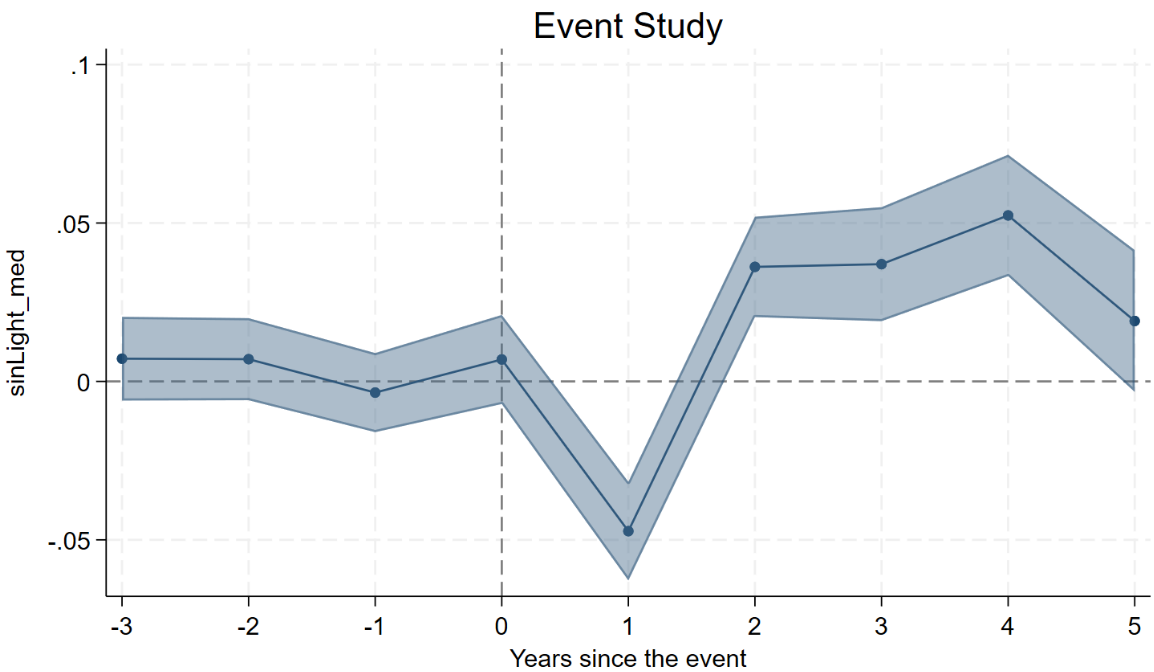
Notes: Inspired by the theoretical model in Aiyar and Cummins (2021) where height-for-age H is a function of health endowment Ho and age-specific investments I_a: $H = g(Ho, I)$.

Figure 3 – Earthquake events and Demographic and Health Surveys



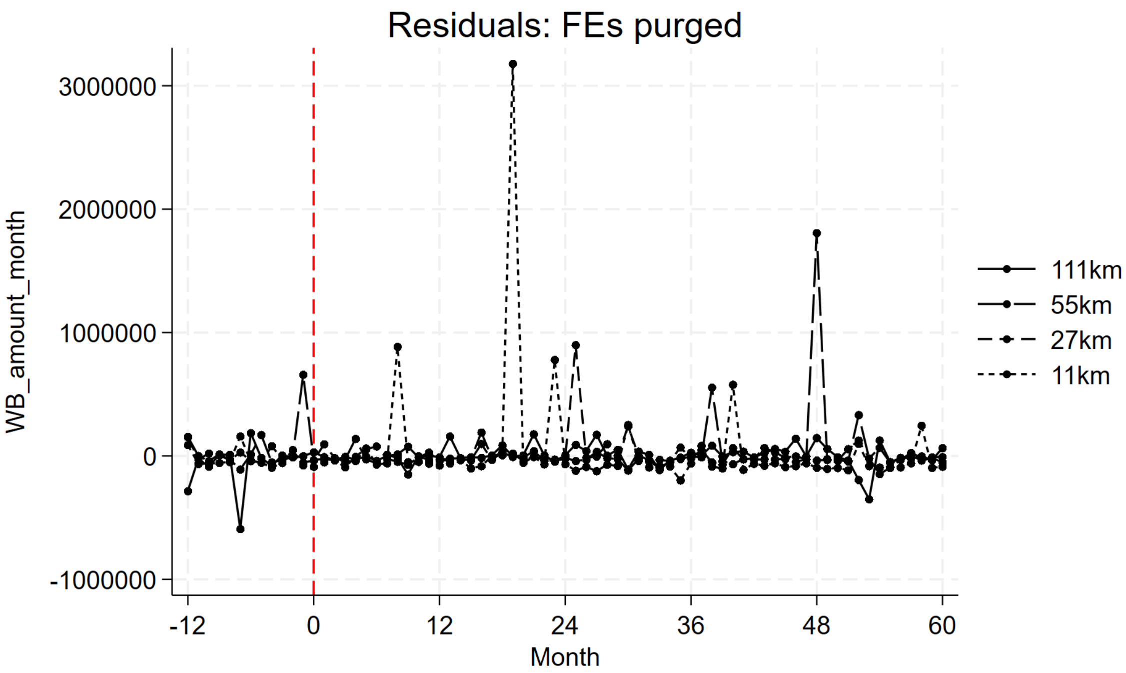
Notes: Earthquake epicenters and Demographic and Health Survey (DHS) clusters included in the initial sample. Clusters in dark blue, earthquakes in red—layered by magnitude, the redder the more intense.

Figure 4 – Nighttime lights (IHS)



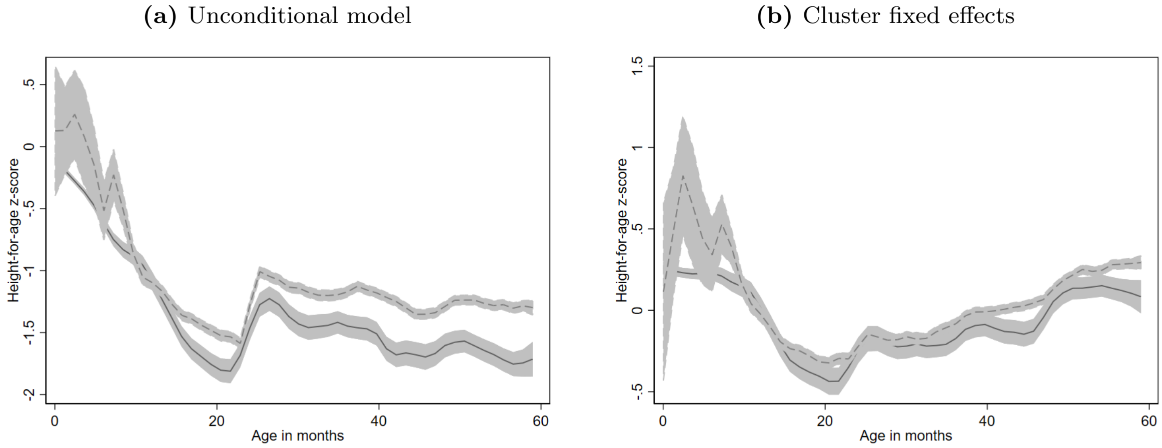
Notes: Evolution of nighttime lights transformed via inverse hyperbolic sine function (IHS) following event-study approach in (Callaway and Sant'Anna, 2021).

Figure 5 – World Bank, amount of aid



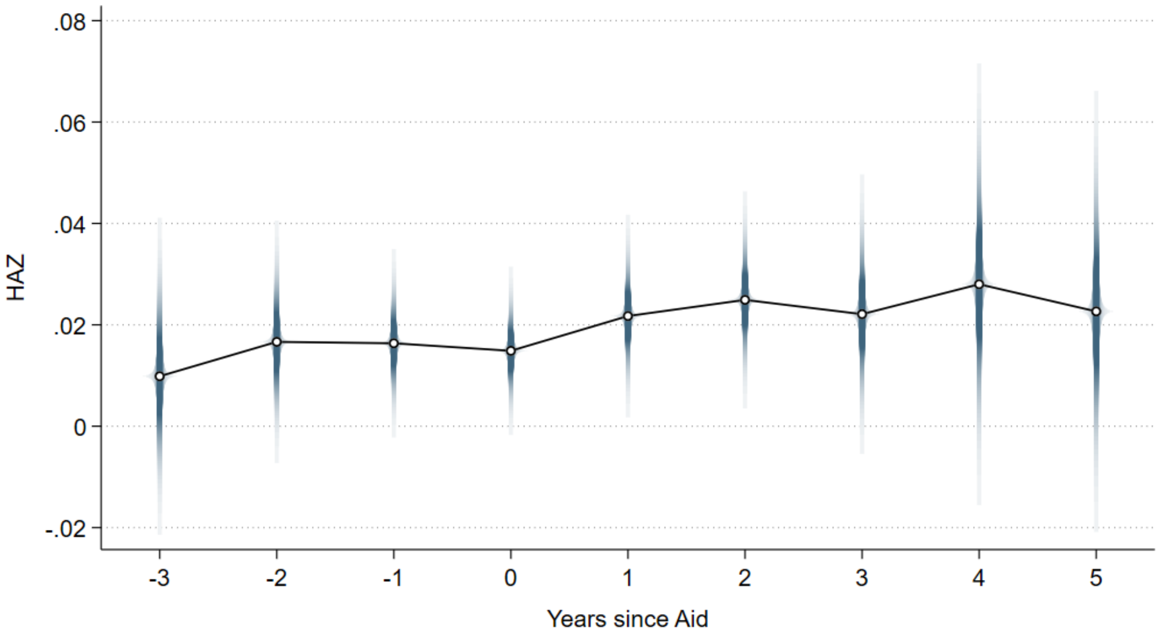
Notes: Graph of residuals after purging of cluster and month-country fixed effects (FEs).

Figure 6 – HAZ-Age profiles by magnitude of investment as proxied by the total number of vaccination shots after birth (solid line is number of shots<4, dashed line is more than 6 shots)



Notes: Local polynomial smooth. Own calculations and graphs using analysis sample of Demographic and Health Surveys. No covariates.

Figure 7 – Aid and HAZ



Notes: Inspired by the model in Cruzatti et al. (2023) where aid is instrumented by the interaction of donor's liquidity and historical probability of receiving aid in the region.

Table 1 – Impacts on height-for-age z-scores

	(1)	(2)	(3)	(4)	(5)	(6)
EQ in utero, within 111km	0.032***	0.044***	0.049***	-0.030***	-0.046***	-0.055***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.011)	(0.012)
Age in months	-0.017***	-0.017***	-0.024***	-0.027***	-0.029***	-0.057***
	(0.000)	(0.000)	(0.001)	(0.002)	(0.003)	(0.007)
Male	-0.118***	-0.120***	-0.121***	-0.126***	-0.127***	-0.129***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)
Birth order	-0.042***	-0.042***	-0.040***	-0.010***	-0.016***	-0.446***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.011)
Observations	967,679	967,679	967,679	963,746	454,374	454,374
Adjusted R-squared	0.032	0.064	0.074	0.182	0.207	0.349
Country FEs		X	X			
Mother FEs						X
Temporal FEs			X	X	X	X
Cluster FEs				X	X	

Notes: Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1. Column 5 uses mother sample of column 6.

Table 2 – Controlling for early life exposure and impacts on height-for-age z-scores

	(1)	(2)	(3)	(4)
EQ in utero, within 111km	-0.033***	-0.060***		
	(0.007)	(0.012)		
EQ in utero Trimester 1, within 111km			-0.025***	-0.042***
			(0.007)	(0.013)
EQ in utero Trimester 2, within 111km			-0.009	-0.019
			(0.007)	(0.013)
EQ in utero Trimester 3, within 111km			-0.013*	-0.015
			(0.007)	(0.013)
Observations	963,746	454,374	963,746	454,374
Adjusted R-squared	0.182	0.349	0.182	0.349
Early-life EQ exposure controls	X	X	X	X
Cluster FEs	X		X	
Temporal FEs	X	X	X	X
Mother FEs		X		X

Notes: Child age in months, gender, and birthorder are included as covariates, but not shown. Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table 3 – Growth faltering in height-for-age z-scores

	(1)	(2)	(3)
Age in months	-0.078***	-0.079***	-0.119***
	(0.002)	(0.002)	(0.007)
Age>24 months	-1.565***	-1.567***	-1.901***
	(0.013)	(0.013)	(0.019)
Age x Age>24 months	0.074***	0.074***	0.091***
	(0.001)	(0.001)	(0.001)
EQ in utero, within 111km	-0.166***	-0.187***	-0.208***
	(0.017)	(0.018)	(0.027)
EQ in utero, within 111km x Age	0.010***	0.010***	0.013***
	(0.001)	(0.001)	(0.002)
EQ in utero, within 111km x Age>24 months	0.325***	0.328***	0.336***
	(0.028)	(0.028)	(0.043)
EQ in utero, within 111km x Age x Age>24 months	-0.013***	-0.013***	-0.016***
	(0.001)	(0.001)	(0.002)
Observations	963,746	963,746	454,374
Adjusted R-squared	0.206	0.206	0.397
Early-life EQ exposure controls		X	X
Cluster FEs	X	X	
Temporal FEs	X	X	X
Mother FEs			X

Notes: Child gender and birthorder are included as covariates, but not shown. Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table 4 – Investment mechanisms following an in-utero shock

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Polio		DPT		Measles		Shots after birth		Polio at birth		BCG at birth		Prenatal check		Facility delivery	
EQ in utero, within 111km	0.008***	0.014***	0.010***	0.016***	0.014***	0.023***	0.053***	0.092***	-0.000	-0.001	-0.001	0.000	0.001	0.007	-0.003	-0.002
	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.004)	(0.011)	(0.017)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.006)	(0.002)	(0.002)
Observations	874,134	371,719	870,998	370,289	868,929	368,524	876,052	373,085	779,619	335,408	874,822	372,142	728,162	35,439	975,164	460,239
Adjusted R-squared	0.252	0.436	0.342	0.511	0.374	0.489	0.384	0.562	0.407	0.596	0.362	0.598	0.410	0.815	0.480	0.707
Cluster FEs	X		X		X		X		X		X		X		X	
Temporal FEs	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Mother FEs		X		X		X		X		X		X		X		X

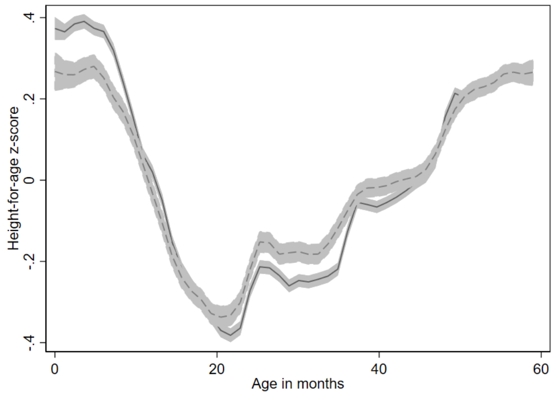
Notes: Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Online Appendix

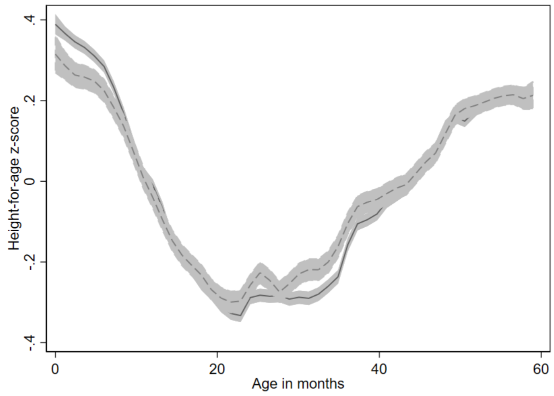
A Appendix Figures

Figure A.1 – HAZ-Age profiles proxying child growth faltering (children under 5 years of age), by earthquake exposure (solid line is control, dashed line was exposed to the earthquake in utero, within 111km)

(a) Cluster Fixed Effects

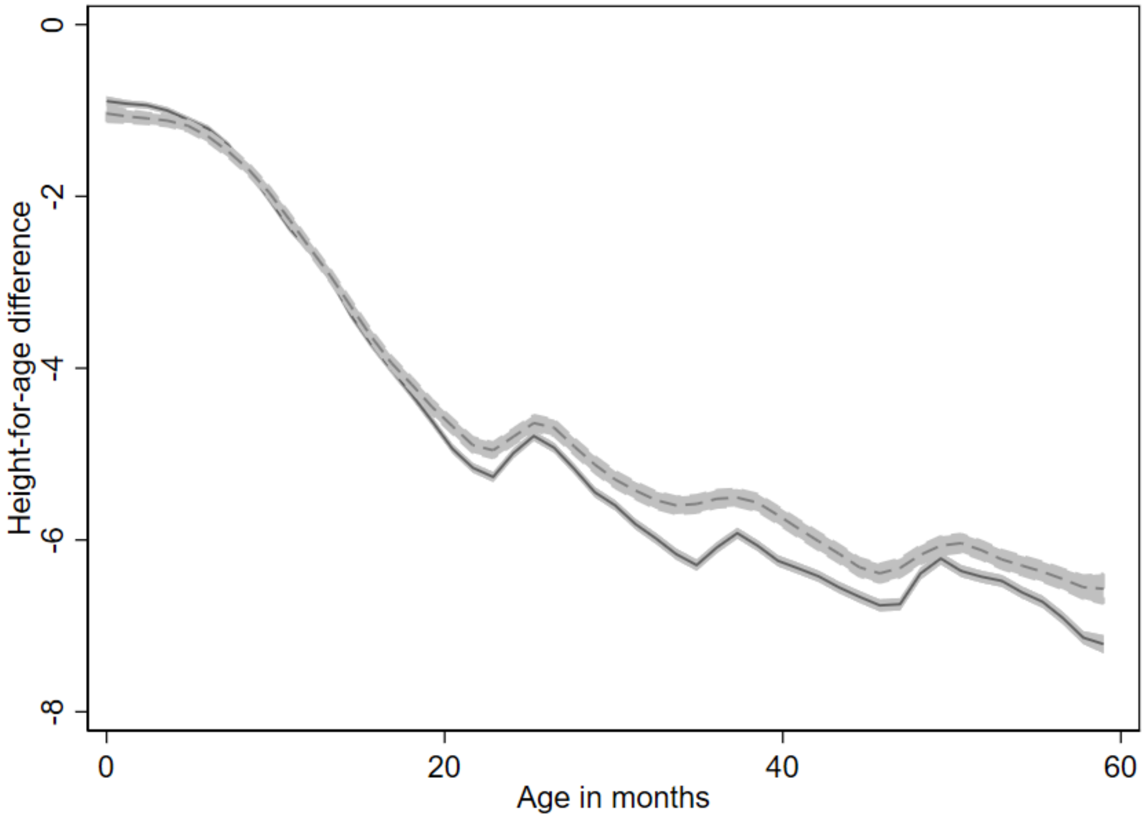


(b) Mother Fixed Effects



Notes: Local polynomial smooth. Cluster (Mother) fixed effects were purged in Panel a(b). Own calculations and graphs using analysis sample of Demographic and Health Surveys.

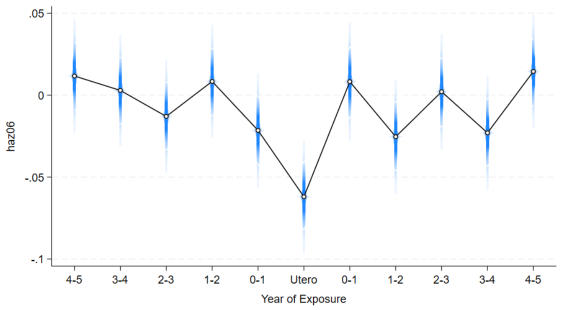
Figure A.2 – HAD-Age profiles proxying child growth faltering (children under 5 years of age), by earthquake exposure (solid line is control, dashed line was exposed to earthquake in utero, within 111km)



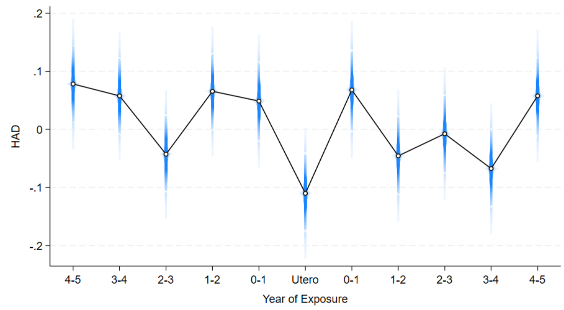
Notes: Local polynomial smooth. Own calculations and graphs using analysis sample of Demographic and Health Surveys. No covariates.

Figure A.3 – Other placebo tests

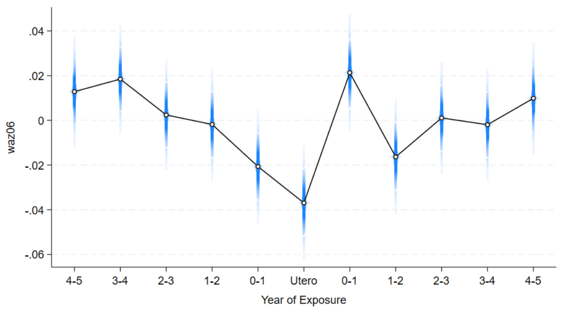
(a) HAZ



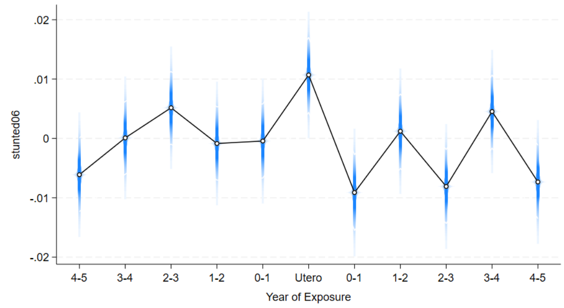
(b) HAD



(c) WAZ



(d) Stunted probability



Notes: Graph details the timing of earthquake exposure and their effects on the different variables exposed in a) to d). Lines refer to 90% C.I., blue shades up to 99% C.I.

B Appendix Tables

Table B.1 – Analysis observations across countries by earthquake exposure in utero, within 111km

Country	Control Obs.	Exposed Obs.	Total
Albania	119	3,777	3,896
Angola	6,194	94	6,288
Armenia	1,988	942	2,930
Bangladesh	16,823	11,633	28,456
Benin	20,866	767	21,633
Bolivia	4,809	2,888	7,697
Burkina Faso	11,687	0	11,687
Burundi	5,405	3,790	9,195
Cambodia	13,258	0	13,258
Cameroon	11,924	22	11,946
Chad	7,903	0	7,903
Colombia	7,909	7,802	15,711
Comoros	2,111	319	2,430
Congo Democratic Republic	9,987	1,144	11,131
Cote d'Ivoire	3,008	0	3,008
Dominican Republic	1,761	11,972	13,733
Egypt	44,801	5,639	50,440
Ethiopia	29,913	2,657	32,570
Gabon	3,309	0	3,309
Gambia	3,764	0	3,764
Ghana	7,649	0	7,649
Guatemala	591	11,067	11,658
Guinea	7,674	120	7,794
Guyana	1,511	23	1,534
Haiti	9,212	8,234	17,446
Honduras	2,599	7,305	9,904
India	190,771	34,343	225,114
Jordan	11,472	3,960	15,432
Kenya	21,704	1,458	23,162
Kyrgyz Republic	1,150	2,880	4,030
Lesotho	4,120	154	4,274
Liberia	9,790	0	9,790
Madagascar	4,984	84	5,068
Malawi	21,748	5,238	26,986
Mali	28,036	49	28,085
Mauritania	7,368	33	7,401
Moldova	1,282	31	1,313
Morocco	4,617	631	5,248
Mozambique	8,370	978	9,348
Namibia	8,205	93	8,298
Niger	2,710	0	2,710
Nigeria	58,731	145	58,876
Pakistan	1,761	2,326	4,087
Peru	5,819	19,376	25,195
Rwanda	9,268	5,476	14,744
Senegal	58,989	99	59,088
Sierra Leone	10,337	0	10,337
Swaziland	2,016	0	2,016
Tajikistan	1,448	8,915	10,363
Tanzania	15,477	3,553	19,030
Timor-Leste	49	13,537	13,586
Togo	3,141	30	3,171
Uganda	11,296	1,749	13,045
Zambia	22,787	2,308	25,095
Zimbabwe	15,442	375	15,817
Total	779,663	188,016	967,679

Notes: List of countries and observations by earthquake exposure corresponding to the unconditional OLS model in Table 1, column 1.

Table B.2 – Descriptive statistics by earthquake exposure in utero, within 111km

	Control	Exposed	Overall	P-value (diff.)	N
Height-for-age z-score (HAZ)	-1.334	-1.297	-1.327	0.000	967,679
Weight-for-age z-score (WAZ)	-1.032	-0.788	-0.984	0.000	980,893
Weight-for-height z-score (WHZ)	-0.344	-0.042	-0.285	0.000	955,550
Stunted	0.339	0.322	0.335	0.000	967,679
Low birthweight	0.130	0.108	0.125	0.000	578,858
Age in months	28.250	28.911	28.378	0.000	988,692
Male	0.510	0.511	0.510	0.625	988,692
Exposure variables					
EQ in utero, within 111km	0.000	1.000	0.193	0.000	988,692
EQ in utero, within 55km	0.000	0.461	0.089	0.000	988,692
EQ in utero, within 27km	0.000	0.168	0.032	0.000	988,692
EQ in utero, within 11km	0.000	0.036	0.007	0.000	988,692
EQ in utero, mean intensity within 111km	0.000	4.615	0.891	0.000	988,692
EQ in utero, maximum intensity within 111km	0.000	4.750	0.917	0.000	988,692
EQ in utero Trimester 1, within 111km	0.000	0.617	0.119	0.000	988,692
EQ in utero Trimester 2, within 111km	0.000	0.623	0.120	0.000	988,692
EQ in utero Trimester 3, within 111km	0.000	0.628	0.121	0.000	988,692
Child level secondary variables					
Child has younger sibling	0.267	0.236	0.261	0.000	976,363
Polio	0.641	0.696	0.652	0.000	879,406
DPT	0.666	0.733	0.679	0.000	876,082
Measles	0.619	0.685	0.632	0.000	874,318
Shots after birth	5.109	5.453	5.177	0.000	881,294
Polio at birth	0.628	0.559	0.616	0.000	784,283
BCG at birth	0.853	0.907	0.864	0.000	880,067
Prenatal check	0.796	0.796	0.796	0.853	732,870
Facility delivery	0.652	0.675	0.656	0.000	979,106

Notes: Sample (maximum 988,692 observations) is restricted to children under the age of 5 with age information and at least one anthropometric outcome record. Observations vary due to incomplete information and are processed using list-wise deletion.

Table B.3 – Gender heterogeneity in impacts on height-for-age z-scores

	(1)	(2)	(3)	(4)
	Male		Female	
EQ in utero, within 111km	-0.024**	-0.064***	-0.034***	-0.082***
	(0.011)	(0.023)	(0.011)	(0.022)
Observations	481,057	122,982	462,318	124,280
Adjusted R-squared	0.171	0.359	0.194	0.374
Cluster FEs	X		X	
Temporal FEs	X	X	X	X
Mother FEs		X		X

Notes: Child age in months and birthorder are included as covariates, but not shown. Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.4 – Placebo tests and impacts on height-for-age z-scores

	(1)	(2)
Placebo effects		
EQ 4-5 years before in utero, within 111km	0.012	0.013
	(0.008)	(0.012)
EQ 3-4 years before in utero, within 111km	0.006	0.004
	(0.008)	(0.012)
EQ 2-3 years before in utero, within 111km	-0.015*	-0.011
	(0.008)	(0.012)
EQ 1-2 years before in utero, within 111km	0.018**	0.011
	(0.008)	(0.012)
EQ 0-1 years before in utero, within 111km	-0.009	-0.017
	(0.008)	(0.012)
EQ in utero, within 111km	-0.030***	-0.055***
	(0.007)	(0.012)
Observations	963,746	454,374
Adjusted R-squared	0.182	0.349
Cluster FEs	X	
Temporal FEs	X	X
Mother FEs		X

Notes: Child controls included. Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.5 – Impacts on height-for-age z-scores, longer-term residents

	(1)	(2)
EQ in utero, within 111km	-0.046***	-0.078***
	(0.011)	(0.017)
Age in months	-0.029***	-0.142***
	(0.003)	(0.015)
Male	-0.121***	-0.123***
	(0.005)	(0.009)
Birth order	-0.012***	-0.650***
	(0.001)	(0.015)
Observations	422,080	198,268
Adjusted R-squared	0.180	0.341
Cluster FEs	X	
Temporal FEs	X	X
Mother FEs		X

Notes: Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.6 – Impacts on fertility and mortality

	(1)	(2)	(3)	(4)
	Younger sibling	Younger sibling	Child is dead	Child is dead
EQ in utero, within 111km	0.002	0.008***	-0.000	-0.001
	(0.002)	(0.002)	(0.000)	(0.001)
Age in months	0.011***	0.018***		
	(0.001)	(0.002)		
Male	-0.021***	-0.002**	0.011***	0.013***
	(0.001)	(0.001)	(0.000)	(0.000)
Birth order			0.002***	-0.022***
			(0.000)	(0.000)
Observations	972,577	458,319	6,908,496	6,489,246
Adjusted R-squared	0.296	0.703	0.063	0.077
Cluster FEs	X		X	
Temporal FEs	X	X	X	X
Mother FEs		X		X

Notes: Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.7 – Number of under 5s per mother

	(1) HAZ	(2) HAZ
EQ in utero: 1 child under 5	-0.018*	
	(0.010)	
EQ in utero: 2 children under 5	-0.042***	-0.052***
	(0.011)	(0.013)
EQ in utero: 3+ children under 5	-0.055*	-0.065**
	(0.029)	(0.029)
Observations	952,736	454,374
Adjusted R-squared	0.200	0.339
Cluster FEs	X	
Temporal FEs	X	X
Mother FEs		X

Table B.8 – Impacts on alternative anthropometric indicators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Stunted		HAD		WAZ		WHZ		Low birthweight	
EQ in utero, within 111km	0.005**	0.010***	-0.053**	-0.099***	-0.023***	-0.034***	-0.007	-0.007	-0.001	-0.000
	(0.002)	(0.004)	(0.024)	(0.038)	(0.005)	(0.009)	(0.006)	(0.010)	(0.002)	(0.003)
Age in months	0.005***	0.011***	-0.139***	-0.232***	-0.023***	-0.040***	-0.008***	-0.010**	0.000	0.001
	(0.001)	(0.002)	(0.007)	(0.023)	(0.002)	(0.006)	(0.002)	(0.004)	(0.000)	(0.002)
Male	0.036***	0.036***	-0.164***	-0.179***	-0.081***	-0.081***	-0.024***	-0.026***	-0.025***	-0.026***
	(0.001)	(0.002)	(0.011)	(0.019)	(0.003)	(0.004)	(0.003)	(0.005)	(0.001)	(0.002)
Birth order	0.004***	0.101***	-0.038***	-1.546***	-0.012***	-0.315***	-0.010***	-0.058***	-0.004***	0.006**
	(0.000)	(0.003)	(0.003)	(0.037)	(0.001)	(0.009)	(0.001)	(0.007)	(0.000)	(0.003)
Observations	963,746	454,374	963,746	454,374	977,097	466,972	951,521	446,877	570,110	224,364
Adjusted R-squared	0.132	0.275	0.257	0.423	0.239	0.425	0.204	0.341	0.064	0.293
Cluster FEs	X		X		X		X		X	
Temporal FEs	X	X	X	X	X	X	X	X	X	X
Mother FEs		X		X		X		X		X

Notes: HAD is height-for-age difference, WAZ is weight-for-age z-score, WHZ is weight-for-age z-scores. Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.9 – Exploring effects on birthweight (sample of children with birthcards)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Low birthweight				Birthweight (in gr.)	
EQ in utero, within 111km	-0.002	-0.001			-8.487	-17.207		
	(0.003)	(0.007)			(5.713)	(10.480)		
EQ in utero Trimester 1, within 111km			-0.002	0.012			-0.413	-15.871
			(0.004)	(0.008)			(6.166)	(12.449)
EQ in utero Trimester 2, within 111km			0.001	-0.008			-7.034	-3.456
			(0.004)	(0.008)			(6.225)	(12.791)
EQ in utero Trimester 3, within 111km			-0.003	-0.000			2.787	3.591
			(0.004)	(0.008)			(6.184)	(12.704)
Observations	259,348	89,068	259,348	89,068	259,348	89,068	259,348	89,068
Adjusted R-squared	0.076	0.301	0.076	0.301	0.198	0.475	0.198	0.475
Cluster FEs	X		X		X		X	
Temporal FEs	X	X	X	X	X	X	X	X
Mother FEs		X		X		X		X

Notes: Child age in months, gender and birthorder are included as covariates, but not shown. Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.10 – Alternative exposure indicators and impacts on height-for-age z-scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unexpected earthquake in utero,	-0.119***	-0.194***								
within 111km	(0.042)	(0.062)								
Earthquake in utero, within 111km			-0.024***	-0.047***						
			(0.008)	(0.013)						
Earthquake in utero, 111x55km			-0.013	-0.024						
			(0.010)	(0.017)						
Earthquake in utero, 111x27km			-0.052	-0.083						
			(0.014)	(0.024)						
Earthquake in utero, 111x11km			-0.058	-0.090						
			(0.023)	(0.041)						
Earthquake in utero,					-0.006***	-0.012***				
mean intensity within 111km					(0.002)	(0.003)				
Earthquake in utero,							-0.007***	-0.012***		
maximum intensity within 111km							(0.002)	(0.003)		
Earthquake in utero,									-0.028***	-0.054***
Modest/Considerable Damage (4-6)									(0.007)	(0.012)
Earthquake in utero,									-0.093***	-0.059
Considerable Damage (6-7)									(0.023)	(0.038)
Earthquake in utero,									-0.187***	-0.188**
Considerable/ Catastrophic Damage (7+)									(0.044)	(0.078)
Observations	101,704	47,747	963,746	454,374	963,746	454,374	963,746	454,374	963,746	454,374
Adjusted R-squared	0.169	0.348	0.182	0.349	0.182	0.349	0.182	0.349	0.182	0.349
Cluster FEs	X		X		X		X		X	
Temporal FEs	X	X	X	X	X	X	X	X	X	X
Mother FEs		X		X		X		X		X

Standard errors are clustered at the DHS cluster-level.
*** p<0.01, ** p<0.05, * p<0.1

Table B.11 – Investments per EQ magnitude

	(1) Polio	(2) DPT	(3) Measles	(4) Shots after birth	(5) Polio at birth	(6) BCG at birth	(7) Prenatal check	(8) Facility delivery
Modest/Considerable Damage (4-6)	0.014***	0.016***	0.024***	0.090***	-0.001	0.000	0.007	-0.002
	(0.004)	(0.003)	(0.004)	(0.017)	(0.003)	(0.002)	(0.006)	(0.002)
Considerable Damage (6-7)	0.024**	0.027**	-0.010	0.150**	0.035***	0.026***	-0.316	0.006
	(0.012)	(0.011)	(0.012)	(0.060)	(0.011)	(0.006)	(0.218)	(0.007)
Considerable/ Catastrophic Damage (7+)	-0.080***	-0.054***	-0.060**	-0.249**	-0.083***	-0.007	0.011	-0.034**
	(0.022)	(0.021)	(0.024)	(0.114)	(0.019)	(0.016)	(0.015)	(0.014)
Observations	371,719	370,289	368,524	373,085	335,408	372,142	35,439	460,239
Adjusted R-squared	0.436	0.511	0.489	0.562	0.596	0.598	0.815	0.707
Mother FEs	X	X	X	X	X	X	X	X
Temporal FEs	X	X	X	X	X	X	X	X

Notes: Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.12 – Growth faltering in height-for-age differences (HAD)

	(1) HAD	(2) HAD
Age in months	-0.263***	-0.388***
	(0.008)	(0.022)
Age>24 months	-4.064***	-5.341***
	(0.043)	(0.061)
Age x Age>24 months	0.184***	0.239***
	(0.002)	(0.003)
EQ in utero, within 111km	-0.484***	-0.466***
	(0.046)	(0.079)
EQ in utero, within 111km x Age in months	0.029***	0.043***
	(0.003)	(0.005)
EQ in utero, within 111km x Age>24 months	0.851***	1.040***
	(0.090)	(0.137)
EQ in utero, within 111km x Age x Age>24 months	-0.034***	-0.055***
	(0.004)	(0.006)
Observations	963,746	454,374
Adjusted R-squared	0.270	0.453
Cluster FEs	X	
Temporal FEs	X	X
Mother FEs		X

Notes: Child gender and birthorder are included as covariates, but not shown. Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.13 – Impact on gestational age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Baseline EQs				Unexpected EQs		
Earthquake in utero, within 111km	0.006	-0.005			-0.031	-0.032		
	(0.005)	(0.007)			(0.030)	(0.030)		
Earthquake in utero Trimester 1, within 111km			0.003	0.009			-0.097*	-0.100*
			(0.005)	(0.007)			(0.051)	(0.055)
Earthquake in utero Trimester 2, within 111km			0.010**	-0.005			0.053	0.021
			(0.005)	(0.007)			(0.046)	(0.055)
Earthquake in utero Trimester 3, within 111km			-0.001	-0.003			-0.048	-0.027
			(0.005)	(0.007)			(0.034)	(0.047)
Observations	131,044	67,343	131,044	67,343	14,657	7,158	14,657	7,158
Adjusted R-squared	0.088	0.424	0.088	0.424	0.083	0.463	0.083	0.463
Cluster FEs	X		X		X		X	
Temporal FEs	X	X	X	X	X	X	X	X
Mother FEs		X		X		X		X

Notes: Standard errors are clustered at the DHS cluster-level. *** p<0.01, ** p<0.05, * p<0.1

Table B.14 – Previous earthquake evidence on child growth

Study	Country	Earthquake year	Magnitude	GDP pc	Stunting rate (%)	In-utero model	Impact on height	Catch-up	Recovery mechanism
Ogasawara (2022)	Japan	1923	7.9			Yes	Height in cm, ages 6-8: ↓0.15cm or 0.03 SDs (boys); ↓ 0.28 to 0.59cm or 0.06 to 0.14 SDs (girls)	Partial; gender het.	Relief expenses as covariate
Dodlova et al. (2023)	Haiti	2010	7	8,944	25.2	Yes	HAZ: ↓ 0.09 or 0.06 SDs with 1 SD increase in shaking intensity (average affect across ages); no additional effect of in-utero exposure	Partial	World Bank aid included as covariate
Pun et al. (2022)	Nepal	2015	7.8	15,998	36	No	HAZ: (boys); ↓ 0.76 (girls)	N/A	No
Andrabi et al. (2023)	Pakistan	2005	>7	13,785	44.8	Yes	HAZ: ↓ 1.08 compared to children 30km from the faultline	No	Aid received included as covariate
Bustelo et al. (2012)	Colombia	1999	6.2	29,991	17.4	No	HAZ: ↓ 0.182 or ↓ 0.20% after 1 year; after 6 years (effect across ages)	Full Full	No No

Notes: June 11 2024, we systematically searched for articles on IDEAS-REPEC (earthquake AND stunting OR height-for-age OR child growth) and PubMed (earthquake AND stunting OR height-for-age OR child growth NOT posttraumatic growth NOT mental health) published over the period 2000-2024 and written in English. After abstract scans, we retained 7(30) articles on IDEAS-REPEC(PubMed). We then read the paper closely retaining those with an explicit identification strategy (difference-in-difference) to test for the impact of earthquakes on child height-for-age or child stunting. Earthquake magnitude as reported in paper. GDP pc (constant 2021 PPP) in year of earthquake from World Bank, World Development Indicators database, available at: https://data.worldbank.org/indicator/SL.GDP.PCAP.EM.KD [Accessed June 12 2024]. Stunting rate near year of earthquake comes from the UNICEF, WHO, World Bank: Joint child Malnutrition Estimates (JME), available at: https://data.worldbank.org/indicator/SH.STA.STNT.ME.ZS [Accessed June 12 2024]