RISE
RESEARCH ON IMPROVING SYSTEMS OF EDUCATION

RISE Working Paper 20/039
May 2020

Human Capital Accumulation and Disasters: Evidence from the Pakistan Earthquake of 2005

Tahir Andrabi, Benjamin Daniels, and Jishnu Das

Abstract

We trace the effects of a devastating earthquake that occurred in Northern Pakistan in 2005. Using a new dataset from a survey conducted four years after the earthquake, we first show that the distance of the household from the fault line was not correlated with pre-existing household characteristics, while it was strongly predictive of earthquake-related damage and mortality. Through emergency relief aid, households living close to the fault line reported receiving substantial cash compensation that amounted to as much as 150 percent of their annual household consumption expenditure.

Four years after the earthquake, there were no differences in public infrastructure, household or adult outcomes between areas close to and far from the fault line. However, children in their critical first thousand days at the time of the earthquake accumulated large height deficits, with the youngest the most affected. Children aged 3 through 15 at the time of the earthquake did not suffer growth shortfalls, but scored significantly worse on academic tests if they lived close to the fault line. Finally, children whose mothers completed primary education were fully protected against the emergence of a test score gap. We estimate that if these deficits continue to adult life, the affected children could stand to lose 15 percent of their lifetime earnings. Even when disasters are heavily compensated, human capital accumulation can be critically interrupted, with greater losses for already disadvantaged populations.

UKaid from the British people
Australian Aid
Bill & Melinda Gates foundation
Oxford Policy Management
Blavatnik School of Government
University of Oxford

Human Capital Accumulation and Disasters: Evidence from the Pakistan Earthquake of 2005

Tahir Andrabi
Pomona College

Benjamin Daniels
Georgetown University

Jishnu Das
Georgetown University

Correspondence: 37th and O Streets NW, Washington, DC 20057; jishnu.das@georgetown.edu

Acknowledgements:

We thank John Wall, Raja Rehan Arshad, and especially Tara Vishwanath and Ali Cheema for initiating the earthquake research and National Engineering Services Pakistan for geological mapping information. We thank seminar participants at The World Bank, PacDev, Duke University, UCSD, LSHTM, UMASS-Boston, UC-Davis and Notre Dame and we especially thank Harold Alderman and Duncan Thomas for their comments.

This is one of a series of working papers from "RISE"—the large-scale education systems research programme supported by funding from the United Kingdom's Department for International Development (DFID), the Australian Government's Department of Foreign Affairs and Trade (DFAT), and the Bill and Melinda Gates Foundation. The Programme is managed and implemented through a partnership between Oxford Policy Management and the Blavatnik School of Government at the University of Oxford.

Please cite this paper as:
Andrabi, T., Daniels, B., Das, J. 2020. Human Capital Accumulation and Disasters: Evidence from the Pakistan Earthquake of 2005. RISE Working Paper Series. 20/039. https://doi.org/10.35489/BSG-RISE-WP_2020/039.

1. Introduction

A burgeoning literature establishes that adverse shocks during childhood, especially in the first 1,000 days of life, can lead to worse schooling outcomes as well as poorer physical and mental health in later life, ultimately translating into a loss of productivity and earnings.[1] Amid a larger debate on inequality and repeated shocks, the link between shocks in the early years and later life outcomes is rapidly gaining traction, and we expect that the closure of societies and economies due to the 2020 coronavirus pandemic will add further urgency to this discussion. (Narayan et al. 2018, Piketty 2015, Weil 2015)

Our aim here is to examine what happens to people in the medium term after a large-scale disaster, *when it is accompanied with substantial compensation*. This contrasts, for instance, with studies of adult populations examining the impact of severe shocks in childhood at a time when households were unlikely to be compensated, especially in low-income countries. We establish (a) the extent to which human capital accumulation may be interrupted even when there is substantial compensation and (b) the extent to which parental attributes may help mitigate these shocks.[2] Although not a primary motivation, these results may also help policymakers anticipate what will happen to children following the coronavirus pandemic of 2020, where there is, again, a severe shock followed by varying degrees of social protection and cash compensation for affected households.

We study the 2005 Kashmir earthquake in Pakistan, using unusually rich survey data that we collected four years later in 2009. (Andrabi, Daniels, and Das 2020) The Kashmir earthquake was one of the most physically destructive disasters in recorded history, and its 7.6 magnitude on

---

[1] See Pörtner (2010) and Baez et al. (2007, 2010) for a synthesis of the literature on shocks and human capital. Multiple studies on disasters, crop failures and droughts all show significant effects on height-for-age among young children with consequences persisting to adulthood: Banerjee et al. (2010), Bozzoli et al. (2009), Case et al. (2002), Christian and Dillon (2018), Datar, Liu, Linnemayr, and Stecher (2011), Fernald et al. (2009), Akresh, Verwimp, and Bundervoet (2011), Alderman, Hoddinott, and Kinsey (2006), Hoddinott and Kinsey (2001), Hoddinott et al (2011), Alderman et al. (1996), Alderman, Hoogeveen, and Rossi (2009), Akresh, Lucchetti, and Thirumurthy (2012), Rosales (2014), Weldeegzie (2017), Wierzba et al. (2001), and Maccini and Yang (2009). The most likely channel for adverse labor market outcomes later in life is through associated cognitive underdevelopment, especially early in life (Handa and Peterman 2007 and Black et al. 2013) with strong correlation between height and test scores in both the U.S. and Philipinnes (Case and Paxson 2010; Glewwe and King 2001; and Glewwe, Jacoby, and King 2001). One calibration of height on later wages is from Ethiopia, where similar height losses after a famine led to predicted annual income losses of 3-8% (Dercon and Porter 2010).

[2] Results from Hurricane Katrina in New Orleans (Deryugina et al. 2018), the Kobe earthquake in Japan (Sawada and Shimizutani 2008) and the Tsunami in Aceh (Frankenberg et al. 2013; Cas et al. 2014), all show that within 4 years of the disaster, household outcomes are “back to normal”; in all these disasters, there was substantial compensation to households after the disaster. While details differ, normality here is defined with respect to a population group that was arguably identical to the affected groups prior to the disaster.

the Richter scale makes it equivalent in force to the 1906 San Francisco earthquake. Eighty percent of homes in the immediate vicinity of the activated fault line were destroyed, as was a great deal of critical public infrastructure, including schools. The earthquake resulted in 73,000–79,000 deaths and 69,000–128,000 injuries. (Encyclopedia Britannica, 2018; Department for International Development and DFID Pakistan, 2018) In our data, one in five households in the affected area lost a family member during the earthquake. However, the loss to households was compensated: in our data, we observe that affected families within 20 kilometers of the fault line received 150% of their annual consumption expenditure in cash aid within 2 years of the disaster.

To estimate the impact of the earthquake on human capital accumulation, we utilize the earthquake as a natural experiment by relying on the fact that each household's distance to the fault line was conditionally independent of its observable characteristics. That is, households were essentially randomly distributed throughout the affected area, *prior* to the earthquake shock. In support of this conclusion, following Andrabi and Das (2017), we show that in our data, pre-determined household and village characteristics are uncorrelated with distance to the fault line. In addition, the affected region is crisscrossed with multiple other fault lines which could just as plausibly have been activated. Existing geological models cannot predict earthquake timing or location and to rule out endogenous household location decisions based on hypothetical fault exposure risk, we can control for the distance to the *nearest* fault line in our specifications. We can also conduct placebo tests by simulating the activation of the 50 other faults present in the region as all households live within 11.5km of some fault and half live within 2.5km. The observed combination of coefficients is at the 98th percentile of the joint distribution.

We then show three additional results to establish the causality of our findings. First, we present evidence that individual migration was not correlated with distance to the fault line at a magnitude sufficient to affect our estimates, alleviating concerns about selection due to relocation. Second, we confirm with a bounding exercise that our results are robust to reasonable assumptions about selective non-responsiveness due to migration, mortality, and unavailability. Third, we document that external aid intensity was strongly correlated with proximity to the fault line without spillover effects of aid to unaffected areas.

Therefore, by comparing households and individuals living farther from the activated fault line to those living closer to it, we estimate the causal effects of the earthquake on a host of household, adult and child outcomes. We consider the full effect as a "joint treatment" of disaster *and* relief aid compared to a control group that received neither. As compensation for housing reconstruction took well over a year to implement, it is best to think of the earthquake as a negative shock that put households under severe deprivation for up to a year or more followed by compensation that allowed them to "build back" their assets.

We first document that, four years after the earthquake, households nearest the fault line are at least as well off as those farther away in terms of wealth, consumption and infrastructure, and were significantly more likely to be living in a permanent masonry residence. Household consumption, household wealth and adult weight (a marker of short-term environmental stress) were at or above parity with less-affected areas. There were also no differences in access to public infrastructure as measured by geographic distance. This "back to normalcy" result closely mirrors findings from the U.S., Japan, and Indonesia and may be linked to the recovery effort led by the government and international aid organizations. (Deryugina et al. 2018, Sawada and Shimizutani 2008, Frankenberg et al. 2013)

However, we continue to observe large shortfalls in physical and cognitive development of children. Using height as an indicator for cumulative childhood shortfalls experienced during the recovery period, we find that children *in utero* at the time of the earthquake located 10km from the activated fault line were 4cm (1 standard deviation) shorter than their peers located 40km away.[3] These effects are attributable to the shocks that occurred around the time of the earthquake, as there is no lag in weight, a measure of current nutrition, for children at any age. The negative effect on stature emerges only for children in the first thousand days of development at the time of the earthquake, with the youngest the hardest hit. That effect on child growth is comparable to results from some of the worst emergencies to have hit Sub-Saharan Africa, such as the 1990-91 civil war in Rwanda, the long-lasting Burundi civil war, and the

[3] Physical growth during early childhood is a reliable indicator of a child's overall developmental progress. Short-term hunger or long-term nutritional deficiency can permanently stunt physical and cognitive development in young children. (Behrman and Hoddinott 2005) Measured as standard deviations (SD), height-for-age z-scores (HAZ) measure a child's position in a normalized reference distribution of healthy children from the United States. (Kuczmarski 2000) See Shrimpton et al. (2001) for a discussion of age-based trends in childhood anthropometrics under varying conditions of deprivation and UNCF (2019) for review of the current situation.

1982-84 drought in Zimbabwe. (Akresh, Verwimp, and Bundervoet 2011; Bundervoet, Verwimp, and Akresh 2009; Alderman, Hoddinott, and Kinsey 2006)

Turning to education, the signs are initially more encouraging. We find no evidence of a decline in current school enrollment in earthquake-affected areas. Within 20km of the earthquake, it led to school closures lasting 14 weeks on average, so these data suggest that children being out of school for a substantial period of time did not lead to higher dropout rates in this region. Neither were there any differential gender effects on enrollment. This is a remarkable result given a large literature on gender bias in education in Pakistan, and the fact that two of the four districts in our survey are in Khyber Pakhtunwa where gender bias can be quite severe. Although there is a difference in enrollment rates for girls and boys, there is no evidence that being out of school exacerbated these differences further. Third, we do not find any difference in grade attainment relative to age. Thus, children remained in the same grade and were promoted at the same rate as their counterparts in unaffected regions.

Nevertheless, independently measured test scores of children living within 10km from the fault reported test scores were 0.24 standard deviations lower than for children living 40km away. This difference does not vary by age, and the gap is equivalent to about 1.5 school grades: the average 15-year old has completed 5.6 grades and children linearly gain about 0.17 standard deviations in performance per grade level on our test. We therefore have evidence across the entire age range that persistent developmental deficits can arise in young children due to a large, albeit "temporary" shock.

The earthquake also opened up inequalities *within* the areas that were affected: In OLS specifications, school-age children whose mothers had completed primary school were largely protected from the earthquake's negative effects on test scores. Since children whose mothers were educated *already* enjoyed an academic advantage of 0.19 standard deviations, the shock served to substantially worsen inequality in test scores between these two groups of children. We do not find a mitigating effect of maternal education for the height impacts across the sample.

This result echoes Figlio (2014), who also finds that parental socioeconomic status does not mitigate in-utero biological shocks.[4]

We assess the causality of these mitigation results by using the availability of a girls' schooling option in the birth village of the mother at the appropriate age as an instrument for maternal education.[5] We show that the first-stage of our instrument is strong, with an F-statistic of 18.5, and the availability of the school increases mother's schooling by 1.2 years. (Stock and Yogo 2002) We also demonstrate the robustness of our instrument using two placebo tests: The availability of a boy's school has no effect on mother's schooling, and the effect of a school that arrived *after* the mother turned 8 also has no effect on her schooling. We again find a similar mitigation result for education, but not for height. Further, when we include birth-village fixed effects, thus allowing all variation to emerge only from the year that the mother was born relative to the opening of a school in her village, compared to other mothers from the same birth village, the coefficients are very similar although precision is lower.[6]

Taken as a whole, the effects of the earthquake on human capital accumulation for children in the population were substantial. A full census of households in our sample villages shows that 53% of households living within 20km of the fault line had a child in utero or below the age of three at the time of the earthquake who could have been affected by the growth lag. Further, uneducated mothers comprised 65% of our sample with 84% of all school-age children, and these children were therefore liable to fall even further behind in their test scores relative to the 16% of children whose mothers had some education. Using estimates from the literature in Pakistan on the association between wages, height and schooling shows that, if these deficits continue to adulthood, children in these age groups may face lifetime earning losses of 15% or more.

---

[4] Figlio et al. (2014) use data from twins in Florida to show that low birth weight affects subsequent cognitive development, lending credence to our finding that parental socioeconomic status does not mitigate against in utero shocks. Various studies (Almond and Mazumber 2013, Almond and Currie 2011, Eccleston 2011) provide more detail on shocks to children *in utero*.

[5] This strategy is motivated by Currie and Morretti (2003), who use proximity to educational opportunities to instrument for the mother's schooling decision and a similar strategy in Andrabi, Das, and Khwaja (2012), who demonstrate the validity of this instrument in the case of Pakistan. To construct this variable, we asked every mother for her birth village, and matched the village back to data on schooling availability. Since all levels of schooling were sex-segregated when these mothers were of school going age, what matters is the availability of a girls' rather than boy's school.

[6] Young (2019) has also raised a concern that, in many studies, IV estimates are not stable to the exclusion of even one or two clusters. We therefore re-estimate our IV specification by dropping each of 124 villages from the sample one at a time, and we find that, while the three weakest leave-one-out estimates are not statistically significant due to wider confidence intervals, their coefficients are similarly sized to our full-sample specification.

The paper extends the existing literature in three directions. First, our extensive data allow us to simultaneously study multiple outcome variables; as such, we are able to demonstrate continued adverse effects for children even as adult and household outcomes fully recover. Previous studies have not examined such a wide range of outcomes; even studies that look at the educational effects of disasters on children typically focus on schooling attainment as test score data are rare.[7]

Second, in terms of identification, the unpredictability of earthquakes (especially in this area, where fault lines are numerous and are not visible) also satisfies several unusual requirements that may not be fulfilled with other disasters. Our estimates are unlikely to be biased by mortality selection or by selection into who was more affected. We do not find any correlation between pre-existing characteristics and earthquake-related mortality, aside from slight excess vulnerability in the very young and very old, and even in the villages that were hardest hit, mortality never exceeds 5%.[8] The unpredictability of earthquakes also alleviates concerns about selection into exposure; hurricanes, for instance, come with significant advance warning such that the final effects also depend on the degree of responsiveness in the population and its correlation with household and individual characteristics.

Further, the fact that some villages are exposed to the earthquake shock and others are not allows us to examine the causal impact of the earthquake *across the entire age-range*, instead of using estimators that require us to compare children in the critical period to those who are older. For stature effects, we are still able to confirm validity of cohort comparisons—we do not find evidence of any physical effects among children who were older than 3 years at the time of the earthquake. However, in the case of test scores, children suffer at all ages in a similar way and here such cohort comparisons are no longer valid.

Finally, we make progress towards showing that maternal education mitigates the average impact of such a shock on cognitive development. By combining the exogenous exposure to the shock and the variation in schooling options in the birth village of the mother, we demonstrate that the

---

[7] Frankenberg et al. (2013) show enrollment recovery after the Tsunami but reduced aspirations among children. Like other studies, they examine enrollment, but not learning outcomes.

[8] For the 2004 Tsunami, Frankenberg et al. (2013) demonstrate that mortality and destruction were highly correlated with household and respondent characteristics due to pre-existing residential patterns and the ability to survive the Tsunami conditional on residence. This is worrisome for estimates of the impact, as mortality rates were as high as 30% in areas closest to the sea.

(in)ability of households to mitigate shocks plays a key role in the evolution of inequality for these regions. Our IV specification suggests that this mitigation result may reflect the causal impact of mother's education. However, we fully acknowledge that our sample sizes are too small to argue definitively for such a causal mitigation channel.[9]

Our findings also leave open several questions. We do not have panel data on household investments or child outcomes. Therefore we have cannot determine whether the impacts we observe among children reflect purely biological factors or household investments or an interaction of the two. Household investments and childhood biology inevitably interact and we do not know the timing and lag-structure of these investments interactions (Bharadwaj 2018).[10] We also do not know whether children will recover from these shocks in the future through "catch-up" growth. In contexts where aid flows are small or stop after a short while (as in Pakistan), the precise conditions under which children can recover from such nutritional deficiencies are unclear and complete recovery seems unlikely, especially for those who suffer shocks in the critical period.[11] Finally, our results do not imply that cash compensation is *never* enough; we observe a single draw of how cash can be distributed (which we describe below) and we do not have evidence for the impact of other schemes that differ in timing and amounts. (Paxson and Schady 2010)

The remainder of the paper is as follows. Section II describes the dataset and the survey process and places the research in the context of existing literature. Section III presents our identification strategy and exogeneity evidence for the earthquake shock. Section IV presents our results.

---

[9] Our estimates are less precise when we include village-of-birth fixed-effects and when we look at the effects on children in the critical ages, where the height effects are most pronounced.

[10] Further, these results on the lack of mitigation for child height do not imply that shocks to child height cannot be mitigated *at all*. Bharadwaj (2013) has shown how medical interventions immediately after birth can affect cognitive achievement. We present speculative results showing that the effect of the shock on child height was lower in households with lower maternal stress and those living at higher elevations. These are both variables collected after the earthquake (elevation was calculated from NASA SRTM [Ustun 2006] using the GPS measured in the field) and do not necessarily support a causal interpretation.

[11] In Zimbabwe a one-point decline in childhood HAZ led to a permanent growth loss of 0.4 standard deviations with 60% catch-up (Alderman, Hoddinott, and Kinsey 2006). Handa and Peterman's (2016) cross-country study observed catch-up growth over a 3-to-5-year period beginning from age 0-7 that ranged between 60% (in China and Nicaragua) to 80% (in South Africa). They do not, however, identify any consistent conditions that enable catch-up growth. Berkman et al. (2002) indicate that catch-up growth in Peru was substantially less complete for children stunted between the ages of 6-17 months. For the Tsunami, Frankenberg et al. (2013) have shown that even as children's heights declined in the aftermath of the disaster, significant aid flows allowed them to catch-up with their peers in later years. We have not been able to locate studies that examine whether physical catch-up is accompanied by recovery in test scores as well.

Section V concludes with a discussion of the consequences of these results for disaster relief and for the intergenerational transmission of inequalities.

II. The Pakistan Earthquake of 2005 and Data Description

The earthquake on October 8th, 2005 in Northern Pakistan left an estimated 73,000–79,000 dead, 69,000–128,000 seriously injured and over 2.8 million homeless. Immediately following the earthquake, organizations and individuals provided financial support as well as on-the-ground logistic and technical assistance, ranging from specialized services in medicine, excavation and evacuation to emergency shelters and food. Most operations were conducted by the Pakistan army, with critical support from international agencies.[12] In this phase, there was also an immediate injection of liquidity to affected households, who all received Rs.25,000 as well as additional compensation for injury and death.

Within one month of the earthquake, the government had set up the Earthquake Reconstruction and Rehabilitation Agency (ERRA), which coordinated efforts of international agencies and the army in the reconstruction of public infrastructure and administration of programs for households in the region. These programs included a cash grant of Rs.24,000 over 4 tranches for certain eligible households as well as compensation of Rs.175,000 for housing reconstruction. Although most households received Rs.25,000 of the full housing grant as well as injury and death compensation within a month of the earthquake, by end November it was clear that reconstruction funds would take a while to setup and distribute. As a result, the government distributed tin sheeting that households could use to construct temporary shelters (or as temporary roofing). Photos taken in December 2005 (Appendix Figure A1) show the kinds of structures that families lived in during the first winter after the earthquake.

By spring, basic assessments of damage had been conducted and further compensation was given for construction, along with training on earthquake-resistant housing. The full compensation was disbursed over the next 3 years as houses were slowly built up and funds were sequentially released on inspection of the plinth, structure and roofing. Considering the above timing, we

[12]U.S. aircraft alone flew more than 4,000 sorties, delivering over 11,000 tons of relief supplies, U.S. medical units treated 32,000 patients and crews cleared more than 50,000 metric tons of debris. (USAID 2006)

view the earthquake as a negative shock that put households under severe deprivation for up to a year or more followed by a compensation stream that allowed them to "build back" their assets.

Our data were collected from 2009-10 as the aid program wound down and most reconstruction had been completed. From the four districts most affected by the earthquake, we randomly selected 126 rural villages from the most recent 1998 census of villages for the study. The selection zone ranged up to 80km from the activated Balakot-Bagh Fault in the two affected provinces, with the average household located 17.5km from the activated fault line and 36.4km from the epicenter. We first completed a census of all 28,297 households (154,986 individuals) that captured GPS coordinates, a household roster, information on deceased household members, a listing of aid groups that assisted the household, and official cash grant programs the household participated in. For a randomly-selected 20% subsample, which covered 6,455 households, an extended census form was filled out that also included data on children's education, home destruction, public infrastructure access, and a depression and PTSD screening questionnaire. We refer to these two different modules as the "short" and the "extended" census; Andrabi and Das (2017) discuss the sampling of villages and demonstrate the validity of the randomization for the extended census.

The GPS coordinates recorded for every household were used to calculate each household's distance from the activated Balakot-Bagh Fault and the earthquake epicenter using United States Geological Survey (USGS) data on the exact path of the fault.[13] Further geographical data allowed us to calculate the proximity of each household to each potentially active fault line in the region. As a hilly region, local slope (averaged at the Union Council level, which includes several nearby villages) could also affect the intensity and destruction caused by an earthquake.[14] In all specifications, we include the average slope in the Union Council as a geographical control in order to prevent bias arising from self-selection into hillier regions.

A detailed multi-module survey was later administered to a randomly selected 10% subset of the census households, producing extended records for 2,456 households covering 15,036 individuals. Surveyors gathered information including school enrollment status before and after

[13] We use the Haversine formula to compute distance, which appropriately adjusts for the earth's curvature.
[14] This measure ranged from 4.9 degrees to 33.1 degrees, with a mean of 21.1 degrees across the 98 Union Councils in our sample.

the earthquake, household assets and consumption before and after the earthquake, earthquake-related mortality, and household members' highest education level. Recall questions were used to gather pre- and post-earthquake data on multiple topics. The combined records yielded a total of 152,435 living and 4,340 deceased individuals.

Figure 1 shows the location of all households covered by the detailed survey and all the fault lines in the area, with the activated fault line and earthquake epicenter highlighted. Figure 2 illustrates the distribution of households in the detailed survey with respect to the activated fault along with a quantile plot illustrating the 5th, 25th, 50th, 75th, and 95th percentiles of distance. The distance to the activated fault line ranges from zero to 75km with a mean of 19km and a median of 13km.

Child Development Outcomes: Of the 15,306 surveyed individuals, 4,475 were aged 3-15 at the time of the data collection exercise, meaning they were in utero or aged up to 11 at the time the earthquake struck. This group was therefore eligible for the collection of additional developmental data in the form of height and weight measurements. [15] Height and weight measurements were normalized for children aged 3 and older following WHO growth tables. This produced data for 4,097 children of whom 4,002 are matched with maternal data for use in analysis (various children were out of the home temporarily or had moved). Measurement coverage among eligible children is therefore 89% and is detailed in Table A1a. The mean measured height for these children was 117.5cm and the mean measured weight was 25.6kg (Table A1b). This group is gender-balanced, and, as shown in A1c, the tested group and the measured group are representative samples of the eligible groups. For all children, mothers are 37 years old on average; 86% of children were enrolled in school, with 30% of those enrolled in private school; and 57% have a father who completed primary school.

For children aged 5 and up, school enrollment information was recorded; children aged 7-15 were administered tests on English, Urdu, and Mathematics at home, regardless of their enrollment status.[16] In regressions using test scores, we utilize a sample of 1,875 school-age children with data on maternal education from 1,081 households. Test coverage among eligible

[15] Children below three years of age were not measured as it was difficult to get their heights accurately; laying them flat to measure them was considered a cultural taboo as it was done only in funerals.
[16] Testing children younger than seven was too expensive as it required substantial oral examinations which could not be funded through our survey.

children with maternal data is 81% (Table A1a) with representative sample t-tests reported in Table A1c and A1d. We only observe differences between the tested and the eligible children in school enrollment rates in tested children: children who were tested were 3-4 percentage points more likely to be enrolled in school than the population as a whole. We do not observe differences in maternal education between children who were tested and the whole group who were eligible.

We calculated test scores as a normalized distribution within each subject using item response theory (IRT), then averaged across subjects for each child. Figure A2 illustrates correct response accuracy rates for several demonstrative test questions across the tested age range. Each subject receives equal weight in analysis, and the results are broadly similar when repeated within each subject (Table A2c).

Despite low average levels of female education in rural Pakistan, even small variation in maternal education is known to exhibit a strong correlation with child developmental outcomes (Andrabi, Das, and Khwaja 2012); as is maternal health (Bhalotra and Rawlings 2011). The average amount of schooling for mothers of tested children is 1.7 years, but it is 7.3 years when conditioned on having received any education; it further increases to 8.2 years, conditional on attending primary school or higher compared to the non-primary-educated group, which has an average of just 0.57 years of education. Thus most of the variation in mother's education comes from a simple binary indicator of primary education for mothers.

To examine the causal impact of maternal education on child achievement, we will use maternal access to a school in her village of birth at age eight, the typical latest primary school enrollment age, as an instrument for actual education. The village of birth was recorded during the household survey and is then matched to school availability according to our census of villages, the national schooling census conducted by the Government of Pakistan, and the Educational Management Information System collected by the National Education Census 2005. This allows us to obtain the year of establishment of girls' schools in all villages in Pakistan. We match 92% of the mothers of tested children with complete historical data, for instrumental-variables coverage of 92% of tested children with mothers.

III. Econometric Approach and Identification of the Earthquake Effect

We use the fact that earthquakes are unpredictable and that the region where the earthquake struck has multiple 'at-risk' fault lines to assert that the shock experienced by the population was exogenous. We begin by presenting evidence that in the absence of the earthquake, those living closer to the fault line would be no different than those living further away with respect to post-earthquake outcomes. After substantiating this claim, we then compare post-earthquake outcomes across the shock spectrum to provide an estimate of the effect of the earthquake on childhood developmental achievement.

Our econometric specification exploits variation in household distance to the activated fault line as the exogenous measure of the strength of the earthquake shock, conditioning on district fixed-effects, the distance to the epicenter and the hilliness of the region.[17] The general form of the regression specification is then:

$$Y_i = \alpha + \beta * DistanceToFaultline_i + \gamma * X_i + \delta * District_i + \varepsilon_i$$

Where Y_i is our dependent variable (household or child level), *DistanceToFaultline* is the continuous proximity variable, and X_i represents the vector of geographical controls discussed earlier, as well as other household or individual-level controls depending on the regression. Standard errors are clustered at the village level.

Assessing Exogeneity

There are several pieces of evidence that support our claim of conditional exogeneity. Uniquely, earthquakes are disasters with zero lead time in forecasting, and this earthquake struck after a long period of geological calm in the region. Between 1935 and 2005 there were no earthquakes above magnitude 7.0 in Pakistan and all earthquakes above this magnitude struck the southwestern province of Balochistan between 1883 and 1995.[18] Additionally, as Figure 1 illustrates, there are multiple potentially active faults in the region affected by the 2005 earthquake, and most of the households in our survey live close to some other fault line that was equally likely to be activated. Thus, it is reasonable to assume that populations were randomly

[17] The geological literature highlights the importance of the activated fault line: "*Generally speaking, [distance to epicenter and hypocenter] are poor measures of distance for earthquakes with large rupture areas. [Commonly used is] the closest horizontal distance to the vertical projection of the rupture plane.*" (Scawthorn and Chen 2002)

[18] There was a smaller earthquake (6.2 on the Richter scale) that struck Hunza, Hazara, and Swat districts in North-West Frontier Province in 1974, but these previously affected districts were mostly unaffected by the 2005 earthquake.

distributed in terms of their pre-earthquake attributes with respect to the activated Balakot-Bagh Fault. We control for the distance to the nearest fault line in all regressions to remove effects of differential sorting by exposure to fault risk.

Consistent with our claim of conditional exogeneity, Table 2a shows that distance to the fault line is not systematically correlated with pre-earthquake village-level population, education, or infrastructure drawn from the population census.[19] We observe a very slightly older and taller population farther from the earthquake, potentially due to the earthquake mortality in the young; this difference is visible in the large difference in average age of death that we observe between the populations (much younger deaths occur in the affected area); we later calculate bounds on our estimates to account for potential mortality selection. We also report further correlations using data from our household survey as well as retrospective and current location data on village facilities. We find no correlation between distance to the fault line and adult education, water supply, or residence in a permanent structure before the earthquake. Neither do we find any correlation between distance to the fault line and the recalled travel time between the household and the closest private school, public school, water pump, medical facility, or market, although some have slight differences in linear distance based on our reconstructed maps.

We do find that households who lived farther from the fault line were less likely to report that they had electricity before the earthquake and that they had slightly lower asset and infrastructure levels. These correlations primarily stem from two remote villages that are more than 50km from the fault line, in an extremely mountainous part of the province. Among the remaining 124 villages, only the coefficient for health clinics remains significant (p=0.07), while the rest are statistically insignificant at conventional levels. Further, regressing the distance to fault line on all characteristics to test for joint significance yields an F-statistic of 0.97 and a corresponding p-value of 0.5. Taken together, both village and household data strongly suggest that pre-existing observed (and unobserved) characteristics were not correlated with distance to the fault line.

Despite the exogeneity of pre-earthquake characteristics to the distance to the fault line, concerns may remain in terms of (a) alternate specifications of intensity; (b) post-earthquake migration

[19] The census provides us with population variables (total and female) as well as education (village adult literacy rate and fraction of women with a secondary education) and three housing infrastructure variables: the fraction of houses with electricity, the fraction with indoor water, and a variable reflecting the type of construction. Using principal components methods, we create a village infrastructure index that combines these three infrastructure variables.

and selective mortality and; (c) aid spillovers. We discuss each in turn.

Alternate Specifications: Some studies have used alternate measures of earthquake intensity, such as the distance to the epicenter or the Mercalli intensity, which captures the actual extent of shaking at each point. Andrabi and Das (2017) discuss why these measures are not consistent with the geology of this earthquake and/or the conditional exogeneity requirement and in their Supplemental Appendix they demonstrate that these alternate measures are, in fact, correlated with pre-earthquake characteristics in their sample. They note:

> *An alternate measure of exposure is the Mercalli intensity or USGS "ShakeMap" cartography for the affected region. Our preference for the simpler distance measure is due to the exogeneity requirement: the localized ground shaking that results from an earthquake is a complex combination of the distance from the fault line, the specific geology of the fault (in this earthquake, villages on the "hanging wall" side, which were on the plane that actually moved suffered greater damage) and the characteristics of the local soil and physical characteristics that may be correlated to socioeconomic characteristics. For instance, moist soils such as clay lose their cohesion following an earthquake and can lead to additional damage as they become liquid. However, soil type is also directly correlated to agricultural yield and building suitability. We therefore sacrifice precision in the measurement of earthquake intensity in favor of the exogeneity that the distance to the fault line grants us, and which we verify in the data. Although using the Mercalli intensity instead does not affect our results, the Mercalli intensity is correlated with pre-earthquake housing characteristics and population size, making it a poor candidate for exogenous variation in the earthquake shock.*

Earthquake-Induced Migration and Mortality: Large population movements can be a response to disasters (McIntosh 2008, Deryugina et al. 2018) and lead to a selected sample as we do not have pre-earthquake characteristics for movers. To assess mobility-induced selection, we listed all persons who had lived in the household both before and after the earthquake in our survey modules, so that we could track both "out migration" and "in migration". Of the 5,112 living adults we listed as living close to the fault line in this inclusive method, 192 (3.8%) had moved out and 167 (3.3%) had moved in after the earthquake. The numbers and percentages are remarkably similar for those who lived far from the earthquake: Of 3,040 individuals listed, 65 had moved in (2.1%) and 95 (3.1%) had moved out, with comparable results for children[20]. We do not find any evidence of any differential migration of adult members after the earthquake by distance to fault line; and we find a significant but small difference in child in-migration (Table A2a).[21] Similarly, overall mortality was too low to induce severe selection bias under all but worst-case assumptions. At its highest, childhood mortality was 5%, which could not bias childhood development results unless the most vulnerable were also the tallest and smartest to a large degree. This is unlikely, as the slight excess mortality we observe is in poorer households; however, in Section IV, we compute bounds on selective attrition using mortality, migration, and incomplete surveys to demonstrate the robustness of our results.

Aid Spillovers: A final concern is the presence of aid spillovers, which has been demonstrated in the case of Aceh after the Tsunami. With aid spillovers, differences between affected and unaffected populations could arise from the aid delivered to groups unaffected by the disaster. We will present aid receipts by distance below to show that more than 30km from the fault-line, aid was close to zero. As a result, we believe that aid funds were well-targeted to the disaster region, completing our "joint treatment".

IV. Results

In this section, we discuss the nature of the "joint treatment" as a disaster followed by aid and then evaluate the impact of the earthquake on (a) household and adult outcomes and; (b) children's human capital acquisition. We then present our instrumental variables strategy for maternal education and the mediating role that it plays in protecting children from the shock.

IV.1. *Defining the shock: Destruction and Aid*

The vital role of distance to the fault line on the effects of the earthquake is shown in Figure 3, which plots the destruction of homes and public facilities as well as the percentage of people

[20] Of 4,475 children, 66 (2.3%) had moved in near the fault versus 69 (4.5%) far, and 25 (0.9%) had moved out near the fault versus 19 (1.2%) far.

[21] In our pilot for the survey, we also tried to assess whether entire households had left the village, but found very few examples; even in households where most members had left, at least one member remained behind to keep their property secure. While we do not have direct evidence, we believe that strong cultural and institutional features of the environment worked against household migration out of the area. Most people own their land, but have weak property rights against their own extended family. Households end up with strong ties to their land. Anecdotally, and in conversation with relief and rehabilitation personnel, very few people went to "tent cities" set up as temporary shelters as substantial sums of housing reconstruction aid money distributed over several years required the presence of the surviving household head in the earthquake area until the time of the survey.

who died during and after the earthquake against the distance to the fault line. Overall, 57% of households reported the destruction of their home, with this fraction decreasing from 73% in the immediate vicinity of the fault line to 26% once we cross 20km. These geographical concentrated effects are also evident in mortality and the destruction of public facilities; there is always a sharp drop-off after 20km and a full levelling off at close to (but not actually at) zero. Mortality rates, even right next to the fault line, never exceeded 5% and dropped off sharply to below 1% within 15km.

For individuals who died between the time of the earthquake and our survey, we collected information on their sex and age at time of death. We also separated deaths as those that occurred at the time of the earthquake (or very soon after) and those that occurred later on. To the extent that recall on the timing of death is reliable (we do not have data on exact cause of death), we find that 40% of those who were reported as having died "during the earthquake" were either very young (5 or under) or ages 65 and up, with a strong correlation with distance from the activated fault line. Mortality rates could also have been elevated in months following the earthquake. We therefore also collected data on additional deaths *after* the earthquake and find no evidence of excess mortality near the fault line. Both results in regression (Appendix Table A2b) are consistent with the visual summary in Figure 3. We observe no correlation with pre-quake wealth or any interaction between wealth and distance that would cause confounding in our results, and we show later that overall mortality was sufficiently low that even bounds allowing for strong differential selection do not substantially alter our results.

The second part of our "joint treatment" was the receipt of aid to households from public funds.[22] (Clarke and Dercon 2016) This aid was delivered through three programs. As discussed previously, families could have received a cash transfer (Rs.49,000), compensation for injuries (Rs.25,000-50,000) and death (Rs.100,000) and compensation for housing (Rs.150,000), conditional on the construction of earthquake-resistant structures. Figure 5 shows the total amount of aid received from these sources, plotted against the distance to the fault line. The immediate injection of liquidity averaged Rs.42,800 which is 43% of annual per-capita expenditures among households less than 20km from the fault line (with the average household

[22] We are aware of the possibility that funds could have also arrived from private sources, including family and friends, as has been shown in other contexts; it is also possible that public and private funds were substitutes.

outside that range receiving less than a quarter of that amount). By the time we surveyed households, cumulative aid receipts from the government averaged Rs.175,000 in the villages closest to the fault line, which exceeded 150% of the annual per capita expenditures among households more than 20km from the fault line. The majority of this was in housing compensation, which 86% of households within 20km from the fault line reported receiving. The non-parametric specification shows clearly that the pattern of receipts exactly mirrored the non-linearity observed for house destruction and mortality—it decreases slowly till 20km from the fault line, declines sharply between 20 and 30km from the fault line, and then tapers off around (but not quite at) zero.

This is the "exogenous" part of our joint treatment, the variation in aid arising from distance to the fault line is not correlated with pre-existing household and individual characteristics. We also investigated variation in aid receipts by households and, although 45% of total variation in aid receipts is within village, we find little evidence of differential aid by pre-existing household and individual characteristics and only a small and negligible correlation between the mother's or father's (or other adult's) education and the receipt of aid as well as the amount received (Table A2b).[23]

IV.2 *Household and adult outcomes*

Table 2b shows differences in household and adult outcomes by distance to the fault line, following our regression specification in Equation 1. The coefficient on distance-to-fault line is negative for the asset index as well as for in-home electricity, suggesting that, if anything, near-quake households are slightly wealthier than those farther away. The quality of housing stock is also significantly better in affected areas, with more households reporting a permanent dwelling with electricity and water in the home. Across the shock spectrum there is no difference in per capita expenditures based on a detailed household consumption survey.[24] In addition, Panel B

[23] There is considerable variation in the receipt of the cash grant which we investigated using administrative records and pinned down to differences in the number of children reported by households in eligibility surveys (eligible households were those with more than 3 children) and in our household survey. Interestingly, these differences were as likely to lead to exclusion as inclusion errors in receipts.

[24] It is surprising that there is no difference in PCE by distance to the fault line, especially since some families close to the fault line experienced the deaths of prime-age working members. The main reason for this is that mortality never exceeds 5%, and half of this is among children below the age of 15. Indeed, when we look at households close to the fault-line where a prime-age male (age 20-60) died during the earthquake, we do find that PCE was 10% lower compared to those without such losses.

report null results for access to all types of infrastructure, including distance to schools and health clinics.

Panel C examines adult heights and weights. Adult heights are of special interest in the age range from 18 to 24, as previous work by Deaton (2008) has shown that in South Asia, adverse conditions during childhood can delay the attainment of full adult height to the early 20's. Adult weight is of independent interest as it reflects nutritional conditions and morbidity in the period immediately preceding our survey. We find no indication that adults close to the fault line are systematically shorter or less healthy than those farther away. Thus, we observe a recovery that has made the affected households indistinguishable from those living further away, if not better off in some aspects. We cannot claim that this is *due* to the aid flows included in the net earthquake effect, but at least for the housing component, it is likely that this aid was important.

IV.3. Children's human capital acquisition

There are two questions we are interested in: Did the earthquake, despite our evidence of significant compensation and our evidence of no lasting effects among adults still impact human capital accumulation among children and, were these effects age-dependent. We first investigate these relationships non-parametrically, focusing both on variation by distance to the fault line and by variation in age. Figures 6a and 6b show, non-parametrically, the difference in child anthropometric outcomes by age for children located near the earthquake and for children far from it, split at the 20km mark for illustration of an average effect (recall that 20km is the point at which earthquake destruction tapers off). Several patterns are noteworthy.

First, there is no evidence of current nutritional deficits measured through weight-for-age with distance from the fault line. In the top panel, we have shown weight-for-age relative to an equivalent-aged U.S. population and although we observe a consistent worsening of weight-for-age in both groups, we do not observe differential effects for those closer to or farther from the fault line at any age. In the bottom panel we show a similar non-parametric relationship, but this time mapped continuously against distance to the fault line for children who were in-utero, newborn to 2 and 3 or older; these are the age-groups pertinent for our height results. We have demeaned all weights by subtracting the mean weight among those who were more than 20km from the fault line. Older children here look slightly heavier near the fault, but there is no clear difference in slopes; the largest possible magnitudes of difference are small; and confidence intervals overlap at almost all distances, suggesting that children's weight was not significantly affected by their age at the time of exposure to the earthquake.

By contrast, there are large and consistent differences in stature for children who were in below the age of 3 at the time of the earthquake compared to older children. Figure 6b follows the same procedure, first highlighting height differences by age in the top panel (split by far from and close to the earthquake) and then looking more closely at variation by distance to the fault line in the bottom panel. Relative to the U.S. reference height-for-age follows a complex pattern, first by narrowing the gap with the U.S. and then diverging till age 11 (7 at the time of the earthquake). Unlike weight, there is a clear and large difference by distance to the fault line for those who were in-utero or 0-2 at the time of the earthquake, and this gap then diminishes smoothly, with statistical significance disappearing around age 3. The bottom panel highlights this pattern and in addition shows the same marked non-linearity we found with damage, destruction and mortality—children suffered significant and similar deficits till they were 20km away from the fault line and after this, the disadvantage decrease rapidly and by 25-30km, it has disappeared. This trend fits with the CDC (2000) observations of growth trends in young children - growth rates are fastest at birth and slow monotonously throughout childhood, meaning that disruptions at earlier ages interrupt the periods of most rapid growth, and later aged children are unlikely to exhibit large growth shortfalls.[25]

Turning to education (Figures 7a and 7b), we find no differences in enrollment by distance to the fault line across the age spectrum; regressions below confirm that this is the case for both girls and boys and extends to grade attainment in this population. By contrast, there are large and consistent test score differences across the age spectrum with those farther from the fault line reporting higher test scores that are equivalent to two additional years of schooling at every age. The bottom panel again shows (this time separated by gender) that the test score deficits follow a similar non-linear pattern.

Table 3 presents the regression equivalent to these figures; note that we do not have power to detect the non-linearity discussed previously, and therefore focus on linear specifications only.

[25] For all children, standardized height-for-age trends downwards till age 7. The downward drift is noted in all growth charts from South Asia and shows cumulative stresses from high morbidity during infancy, but usually stabilizes at an earlier age. In our case, the downward trend halts at age 10-12, and rises after that, indicating catch-up growth in the population during the adolescent years.

Children who were in utero at the time of the earthquake are 0.036 standard deviations shorter per kilometer from the fault line, which translates to 1 full standard deviation over a 30km interval (Column 2). The impact on those aged 0-2 at the time of the quake is half that for those in utero (0.015SD/km) and significant at the 10% level (Column 2). Children over the age of 3 at the time of the earthquake, however, show no height loss at all. Neither do we find any adverse effect on weight-for-age in any age group (Column 1).

In terms of education effects, Table 3 first confirms that there are no impacts on enrollment or grade attainment (Columns 3 and 4). The test scores deficits evident in the figures amount to 0.008 standard deviations per kilometer or 0.24 standard deviations over a 30km range (Column 5). Column 6 then looks at the role of school closures in mediating the test score losses. When we include the length of school disruptions as a right-hand-side variable, the main effect is slightly weaker a formal mediation analysis as in Hicks and Tingley (2012) estimates that school disruption accounts for 5.7% (95% CI: 4.1–9.8%) of the total distance-to-fault effect.[26] This effect of school disruption is itself approximately equivalent to pro-rated years of learning, so that 8 weeks of disruption lead to losses that are identical to 20% of the yearly gains we see in the control group (assuming 40 weeks of school a year). Two final specifications include a full set of distance-to-fault interactions with gender and with age, and we find no heterogeneity in the test score gap across either age or gender (Columns 7 and 8).

IV.4. Protective Mothers

Next, we examine the effect of educated mothers on their children's test scores and heights. Using nonparametric local polynomial estimates, Figure 8a shows that, among children whose mothers did not complete primary school education, the pattern of test score losses closely mirrors the pattern of destruction. We again see that scores are substantially lower than average and flat across the 15km band closest to the activated fault line. They then increase gradually between 15–25km from the fault, and level off across the rest of the study area. As with our other results, this nonlinear progression closely mirrors the geographic pattern of the disaster impacts and aid receipts that we documented earlier. However, the gradient is completely absent among children of mothers who completed primary school: their scores are flat across the entire

[26] Note that we can only use the enrolled-during-earthquake subsample here so the sample size is also smaller.

proximity distribution, and therefore substantially higher than the scores of other children in the closest 15km to the activated fault. Figure 8b produces the same comparison for the heights of children under 3 at the time of the earthquake; there is no similar gap or flattening among those with educated mothers.

Table 4a confirms the significance of these findings. In these regressions, we continue to restrict the tested sample to children above the age of 5 at the time of the earthquake, which is the minimum age for starting school; and we restrict the height sample to those in utero or aged 0-2 (the affected group). We use an interaction specification with maternal education and distance to the fault line for both test scores and height, as shown in Columns 2 and 4. The estimates show that there is a strong level effect of maternal education on test scores (0.3 SD). In addition, there is a large mitigation effect for *test scores* by distance in the sample, which amounts to 77% of the fault line coefficient. In contrast, when we examine the link between maternal education and child height, we find neither a level nor a mitigation effect.

Instrumental Variables Strategy

Our first investigation produced OLS regression results including a large maternal primary school education effect as an explanatory variable in the basic regression. We consider maternal education as a conditional coping mechanism and provide instrumental variables estimates to remove the effect of correlated unobservable characteristics of the mother such as ability and effort and focus only on the causal effect of the mother's education.

To identify the variation in maternal education exogenous to the unobserved abilities of mothers, we follow an established literature first proposed by Card (1999) that uses maternal access to a school during the enrollment decision (in her birth village at the time of her enrollment decision in our case) as an instrument for educational attainment. (Burde and Linden 2013) The exclusion restriction requires that the presence of a school affects the outcome variables only through mother's education and not through other mechanisms such as changing social norms. The main source of identifying variation, as in previous studies, is the exposure to a girls' school for a mother during her childhood enrollment window. Andrabi, Das, and Khwaja (2012, 2013) first used this instrument in a different geographical setting in rural Pakistan and provide further details of this strategy. As a matter of policy, the Pakistani public schooling system is segregated by gender at all educational levels, so that mother's education is sensitive to the availability of girls' schooling in the village. Girls' school construction was ramped up during the sixth five-year plan in the early 1980s as a part of the Social Action Programs. Nevertheless, they are less prevalent and of a later vintage than boys' schooling, allowing us to exploit variation over time in schooling opportunities.

This set of IV regressions has as its first stages:

$$maternaleducation_i = \alpha + \beta_1 * girlsschool_i + \beta_2 * \lambda_i + \beta_3 * \gamma_i + \eta_i$$

$$interaction_i = \alpha + \beta_1 * DistanceToFaultline * girlsschool_i + \beta_2 * \lambda_i + \beta_3 * \gamma_i + \eta_i$$

The first regression is actual maternal education on availability of a girls' primary school in the mother's birth village at age eight plus a vector of controls; the second is an instrumental-variable specification for the interaction term. The $girlsschool_i$ dummy is an indicator variable that takes the value 1 if the mother had a girls' school in her birth village before age 9. The Government of Pakistan guidelines use the age of six as the normal school starting age, but school availability at age eight is in practice a more reasonable indicator given the widespread practice of delayed enrolment. A cutoff age higher than that is probably inaccurate since the enrollment window for girls in rural Pakistan is quite small. Our estimation results are robust to small variations in the specific cutoff, although standard errors vary.

After the primary effect of interest, γ represents the same vector of controls used in the earlier OLS regressions. The institutional environment and the policy details of school construction help guard against potential violations of the exclusion restriction, suggesting specific conditioning variables for inclusion in the λ_i control vector. One immediate issue with the expansion in school construction over the last three decades is that younger mothers will have greater exposure to schools at the time of their enrollment decision. Since other changes in the environment affecting enrollment are also time-varying, the first component of the λ_i vector includes controls for maternal age with a full set of age dummies—one for each maternal birth year.

Second, schools may have been constructed in selected villages and unobserved characteristics of these villages could be correlated both with maternal education and current child outcomes. To partially account for this selection, the second component of our λ_i vector is a full set of tehsil dummies, where a tehsil is an area roughly equivalent in size to a US county, one administrative level below the district. This raises the concern that unobserved characteristics of *villages* that received schools were correlated both to maternal education and to child outcomes today, or that school exposure in and of itself has a direct impact on child outcomes independent of maternal education.

In our main specifications, we attempt to account for this unobserved variation by taking cognizance of the official Government policy outlined in various program documents. In these documents, village population was used as the main criterion for school construction. According to the Manual of Development Projects of the Planning Commission of the Government of Pakistan, "Primary schools will be established in those areas where population of school age (boys and girls) is at least 80, the total population catchment area is at least 1,000 and that a middle/primary school does not exist within a radius of 1.5 km of the school." Therefore, the third component of the λ_i vector is the (log) birth village population. To the extent that this picks up salient dimensions of the unobserved heterogeneity in village characteristics, it should strengthen the case for the validity of the exclusion restriction. In the robustness section, we also provide additional results from an exacting specification that includes birth-village fixed effects. Since this specification requires multiple women to be born in the same village before and after the provision of a school, the precision of these estimates is lower, but reassuringly the results are qualitatively the same.

First stage regressions, robustness checks, and exclusion restriction tests are included in Table A3a. We find that having a school present at the time of the mother's enrollment decision increase her likelihood of completing primary school by 12.5%. If, even after controlling for village population, school construction was correlated with unobserved birth-village characteristics that were then transmitted to the mothers or children, our estimates will be biased. We test this condition by first restricting the sample to mothers that received a school at some point, then by adding the full set of current geographical controls. Neither specification changes the strength of the instrument. We also find that the presence of a boy's school at the same eligibility age has an extremely small and insignificant effect, and the construction of a girl's school after the enrollment age had passed also has little effect.

As a second stage, we then regress:

$$Y_i = \alpha + \beta_1 * DistancetoFaultline + \beta_2 * maternaleducation_i + \beta_3 * interaction_i + \beta_4 * \lambda_i + \beta_5 * \gamma_i + \varepsilon_i$$

Here, *maternaleducation*$_i$ and *interaction*$_i$ are the predicted values from the first stage regressions. We again report regression results with an interaction term between maternal education and the distance to the fault line, allowing us to further investigate the hypothesis that educated mothers were able to mitigate the impacts of the earthquake directly rather than simply producing an unconditional performance effect in their children.

The IV regression results reported in Table 4b are similar to those reported in the OLS estimation. The height estimates are still small and insignificant. They also make a stronger case that the protective effect of maternal education observed in the test score regression are causal and not driven by other characteristics which also increase the probability of a woman becoming educated, such as greater ability or effort. The results are substantially larger in magnitude, as is common in IV regression, and remain statistically significant. In a shock-ridden environment, such an advantage can make a crucial difference at a strong enough magnitude. In this case, the interaction effect of maternal education with distance from the fault line on test scores points to complete mitigation. Given that children of uneducated mothers were lower on the ladder in the first place, this result highlights the increasing divergence in learning outcomes in the affected area.

IV.4. Benchmarking the Effects

These effects are at the upper end of the range found in the literature to date. In Appendix Table A4 we have collated a list of effect sizes from multiple studies for comparison, and find that only the most extreme recorded events, the 1990 Rwandan civil war's effects on infant boys and the 2009 Mongolian Dzud winter, suggest long-term height effects of a similar size to those we estimate on the in-utero or newborn cohort. (Groppo 2016)

We can also ask how these childhood disadvantages will translate into productivity in adulthood. We do not know, but we can make some educated guesses under two (brave) assumptions: (A): that the disadvantages we see in our sample continue to adulthood in relative terms (so a child who is at a given height percentile in childhood will remain there in adulthood), and (B): that estimates on the relationship between wages, schooling and height from Pakistan are relevant to this sample. We can then use estimates from the literature, which suggest a 10% return to each year of schooling (Montenegro and Patrinos 2014) and 3% for every centimeter in height

(Bossaive and others 2017) to calibrate the wage equivalent of human capital losses among the children in our sample.

If the loss of test scores has the same effect as an equivalent loss in schooling attainment, children between the ages of 3 and 15 at the time of the earthquake will face losses similar to 1.5 fewer years of schooling and will therefore earn 15% less every year of their adult lives. In addition, children who were in-utero or under the age of 3 will earn 6% less per year. Based on a full census that we conducted in the 125 villages in our study, we estimate that at the peak, the affected cohort will constitute nearly 35% of the labor force between the ages of 18 and 60 (when the youngest among them is 18). At that peak, total earnings in each village will be a full 5% lower due to the earthquake in every year as this affected cohort progresses through their productive lives. This itself is an under-estimate of the true effects as those who were very young will likely have to endure worse health outcomes all their lives. (Strauss and Thomas 1998)

A second question we can ask is whether the effects will be anything like this with the current 2020 Covid-19 pandemic where schools may remain shut for up to 6 months in some countries (counting summer vacation). Again, we cannot tell, but one insight comes from looking at school closures during the earthquake. As we have shown earlier children who suffered a longer length of closures reported proportionally lower test-scores – an additional month out of school suggested a further decline in scores of 0.016 SD or 10% of a (ten-month) school year. Nevertheless, school closures account for at most 10% of the earthquake effect, suggesting that test score losses must have continued even after children rejoined schools. Therefore, we should expect substantial losses in test scores with the current school closures, but it may be the case that test-scores at the point that children return to school are only a harbinger of further losses to come. We discuss in the conclusion why this may be so.

IV.5. Threats to Identification

There remain several potential threats to the causal identification of our results due to selective responses or outliers. We assess four major potential sources of contamination in turn: (1) self-selection of households into risk exposure by fault proximity, (2) selective missingness due to mortality, migration, or unavailability, and (3) IV sensitivity to outliers and (4) potential sources of bias due to endogenous placement of girls' schools using birth-village fixed effects.

First, we assess potential selection of risk profiles into proximity to the activated fault line. In addition to the ex-ante case against selection due to pervasive, invisible, potentially active faults throughout the region and the lack of seismic activity in recent history, we included in every specification a variable for proximity to the *nearest* fault to control for potential selection into risk exposure. No household is more than 11.3km from some fault, and 50% live within 2.5km of some fault. To investigate further whether selective location decisions regarding fault line proximity could produce our results, we conducted a placebo test by performing identical test scores and education distance-to-fault regressions with respect to each fault line in our data (controlling for the true location of the activated fault).

For our placebo test, we treat 50 other possible faults as though they were the location of the shock, testing the distribution of these effect sizes under the null hypothesis of "no earthquake". Appendix Figure A2 illustrates the joint distribution of these coefficients, with our results plotted for reference, as well as the 95th percentile boundary of the estimated joint distribution. While there is a wide distribution of test score coefficients, they mostly occur with respect to much smaller fault lines; and large positive height coefficients appear *in combination* with large positive test score coefficients for only one specification other than the activated Balakot-Bagh fault. Taking these results together, it is unlikely that endogenous residential choices are responsible for the patterns we observe in the data: the observed activated fault lies at the 98th percentile ($p<0.02$) relative to the joint distribution of placebo effects of both learning and anthropomorphic impacts we obtain from the regressions on distance to the non-activated faults in the region. (Alexandersson 1998, Alexandersson 2004, McCartin 2003)

Second, we simulate unfavorable assumptions about selective missingness in our survey sample to investigate the robustness of our primary shock outcomes. Our sampling methodology has various sources of potentially selective missingness in both anthropometric and educational outcomes; namely, mortality, migration, and unavailability at survey time. We demonstrate that all three sources are individually small, with overall completion rates above 80% for all measures. To assess the extent to which selective missingness could compromise our results, we now utilize our complete roster of all potentially eligible non-responders to compute bounds on our primary effects using the method detailed in Lee (2009). Using our binary indicator of distance, this bounding method estimates 2.1% excess responsiveness with 442 non-responsive children of 2,317 potential respondents, and the lower bound on the shock effect between near and far school-age children of -0.13 SD with p=0.014, compared to an unadjusted estimate of -0.17 SD. For heights among children in utero or age 0-2, missingness appears more selective; 4.5% excess observations are trimmed and the worst-case assumptions lead to a point estimate of -0.33 SD with p=0.134, compared to an unadjusted estimate of -0.70 SD. Table A2d reports these results.

Third, we assess the sensitivity of our IV estimates for maternal mitigation to outliers. Young (2019) demonstrates that "*IV estimates are more often found to be falsely significant and more sensitive to outliers than OLS*", showing that in many IV regressions, point estimates swing dramatically and significance collapses with the exclusion of just one or two key high-leverage clusters. Since our IV regression estimates demonstrate the high variance typical of such specifications, we investigate robustness against this sensitivity to outliers. We re-run the maternal-education interaction IV regression, systematically excluding each one of our 124 clusters (villages) from the full IV regression (one of the 125 survey villages had no tested children with maternal information). Figure A5 demonstrates that our results are robust to this procedure, plotting the distribution of the 124 mitigation coefficients obtained this way. Only three clusters result in estimates whose 95% confidence interval includes zero, but these results are mainly due to increased variance than an attenuated coefficient: these villages include 6 (8), 8 (20), and 11 (21) mothers (children), respectively.

Finally, the use of Tehsil fixed effects could still miss some of the endogenous variation in placement of schools. Our final robustness check, shown in Table A3c, now includes birth-village fixed effects and replicates the specifications from Table 4b. It is worth highlighting that the variation in the data now comes from a smaller sample: 100 of 229 birth villages have only one mother; and 179 in total have no variation in the availability of the school at the mother's enrollment age, leaving 50 (birth) villages with 404 mothers of 690 children to supply the identifying variation. The reduced-form and IV specification show similar results in terms of estimated coefficients, although the precision falls with the reduction in effective sample size.

V. Channels

Our results on the impact of the earthquake on human capital acquisition among children as well as the ability of educated mothers to mitigate test score losses are of a reduced-form nature, in that the mechanism through which mother's education works is not uncovered. Disentangling and directly measuring the impact of the shock and of maternal education on "*the production function as well as the production process*" (Behrman 1997) requires more data and precise information on the interaction between shocks, child age, household inputs, and developmental lags. (Das et al 2013, Malamud et al 2016) One channel we were particularly interested in was whether the ability of educated mothers to protect their children from test score losses reflects their ability to switch schools after the earthquake.

As a further investigation, in Table A3d we restrict our test scores sample to villages which had only one schooling option for children, to rule out school switching as a mechanism through which maternal education had an effect on child learning. These regressions suggest that the maternal education mitigation effect is even stronger in this sample than in the overall sample, although the sample size is much smaller and the IV has a very weak first stage in this restricted sample. Taken together, these two results point in the direction of educated mothers potentially being somewhat better able to handle school disruptions and compensate for a decline in the availability or quality of schooling inputs, rather than (for example) having the knowledge or resources to switch children into better or less affected schools after the earthquake.

In Table A3e, we investigate further potential mitigating factors for both test scores and height results. We investigate three potential mitigating factors—maternal stress, household elevation, and household assets. Currie and Rossin-Slater (2013) and Lauderdale (2006) have demonstrated the nuanced role of maternal stress on child outcomes in the United States. Both studies are able to use exogenous events (hurricanes and the September 11th attacks) and populations that were arguably unaffected except through higher stress levels to causally identify a link between maternal stress and child outcomes. In our case, mothers were affected in multiple ways and the lack of any baseline data makes it harder to draw firm conclusions. However, we completed a mental health questionnaire with mothers in our endline that focused on depression and anxiety using the GSQ 12-item inventory.

Columns 1 through 3 show non-significant and non-causal mitigation results for the test scores results for all three potential mitigating factors. Columns 4 though 6 show significant (but non-causal) mitigation for maternal mental health and household elevation, and a reversed coefficient for the assets specification. What is of interest here is that, much as in our regressions for maternal education, the factors that mitigate against height losses are very different from those that mitigate against test scores, suggesting a greater possibility for biological channels in the case of the former. For all these potential interactions, however, we lack sufficient evidence to make any claims about the (non)existence or causality of the effects, and we believe they are all of interest for future investigation.

VI. Conclusion

Our results confirm the overwhelming conclusion from a large number of studies that early childhood deprivation can lead to significant interruptions in the accumulation of human capital. We have extended those results by (a) showing that they continue to hold even when households receive significant compensation and adult outcomes recover to parity; (b) that height effects are concentrated among children who were in the critical first 1,000 days of life at the time of the shock, but that test score effects are large across the age spectrum and; (c) that educated mothers are able to mitigate learning losses, but not height losses. We conclude that even a disaster followed by compensation has the potential to permanently scar children and increase inequalities both across regions and within the areas that were subject to the shock. In conclusion, we make three final points.

First, we have argued that school closures alone cannot have accounted for the loss in test scores, so that children in the earthquake affected regions *learnt less* every year after returning to school. We do not know why this is the case. One intriguing possibility is that every child had to be promoted in the new school year, and if teachers taught to the curriculum in the new grade, they could have fallen farther behind. An influential literature suggests that teaching at a higher level compared to where children reduces how much children learn, and this is a potential channel here. (Banerjee et al. 2016)

Second, detecting these types of losses can take time. If we had tested the children as soon as schools reopened, we would likely not have found the kinds of deficits that we do. This implies that following a disaster, the immediate effects on children may be a *lower-bound* for what they will experience in the subsequent years. Thus, fully understanding the nature of deficits after a shock requires either a longer-term follow-up.

Third, our study does not imply that the losses cannot be mitigated. Gunnsteinsson et al. (2014) have shown that children who were part of a Vitamin A trial when a typhoon struck in Bangladesh were fully protected in terms of their height losses. Similarly, in Aceh after the Tsunami, so much aid came in that children who lived close to the ocean (and were therefore the worst affected) were able to fully catch up (and even outgrow) children who lived farther away. Finally, maternal mental health appears to play a role in cushioning against height effects in very young children. Again, this result is not causal, but it points to the attraction of programs that provide mental health counselling to mothers with young children; Baranov et al. (2017) have documented the positive effects of one such program in Pakistan.

Finally, these results are from a specific disaster in a specific geographic, economic and social context, but the results are in line with multiple estimates from around the world. This large setback to human capital accumulation may affect the long-term prospects and evolution of inequality if the region, making the case for special attention to child health and schooling after a disaster.

References

Akresh, Richard, Leonardo Lucchetti, and Harsha Thirumurthy. "Wars and child health: Evidence from the Eritrean–Ethiopian conflict." *Journal of Development Economics* 99, no. 2 (2012): 330-340.

Akresh, Richard, Philip Verwimp, and Tom Bundervoet. "Civil war, crop failure, and child stunting in Rwanda." *Economic Development and Cultural Change* 59.4 (2011): 777-810.

Alderman, Harold, Hans Hoogeveen, and Mariacristina Rossi. "Preschool nutrition and subsequent schooling attainment: longitudinal evidence from Tanzania." *Economic Development and Cultural Change* 57, no. 2 (2009): 239-260.

Alderman, Harold, Jere R. Behrman, David R. Ross, and Richard Sabot. "The returns to endogenous human capital in Pakistan's rural wage labour market." *Oxford Bulletin of Economics and Statistics* 58, no. 1 (1996): 29-55.

Alderman, Harold, John Hoddinott, and Bill Kinsey. "Long term consequences of early childhood malnutrition." *Oxford Economic Papers* 58.3 (2006): 450-474.

Alexandersson, A. 1998. "gr32: Confidence ellipses." *Stata Technical Bulletin* 46: 10-13. In *Stata Technical Bulletin Reprints*, vol. 8, 54-57. College Station, TX: Stata Press.

Alexandersson, A. 2004. "Graphing confidence ellipses: An update of ellip for Stata 8." *Stata Journal* 4(3): 242-256.

Almond, Douglas, and Bhashkar Mazumder. "Fetal origins and parental responses." Annu. Rev. Econ. 5, no. 1 (2013): 37-56.

Almond, Douglas, and Janet Currie. "Killing me softly: The fetal origins hypothesis." *Journal of Economic Perspectives* 25, no. 3 (2011): 153-72.

Andrabi, Tahir, Benjamin Daniels and Jishnu Das. 2020. "Human Capital Accumulation and Disasters: Evidence from the Pakistan Earthquake of 2005." OSF. http://doi.org/10.17605/OSF.IO/3QG98

Andrabi, Tahir, and Jishnu Das. "In aid we trust: Hearts and minds and the Pakistan earthquake of 2005." *Review of Economics and Statistics* 99, no. 3 (2017): 371-386.

Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja. "Students today, teachers tomorrow: Identifying constraints on the provision of education." *Journal of Public Economics* (2013).

Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja. "What did you do all day? Maternal education and child outcomes." *Journal of Human Resources* 47, no. 4 (2012): 873-912.

Baez, Javier Eduardo, and Indhira Vanessa Santos. "Children's vulnerability to weather shocks: A natural disaster as a natural experiment." Social Science Research Network, New York (2007).

Baez, Javier, Alejandro de la Fuente and Indhira Vanessa Santos. “Do Natural Disasters Affect Human Capital? An Assessment Based on Existing Empirical Evidence.” IZA Discussion Paper No. 5164. Available at SSRN: https://ssrn.com/abstract=1672172

Banerjee, Abhijit, Esther Duflo, Gilles Postel-Vinay, and Tim Watts. "Long-run health impacts of income shocks: Wine and phylloxera in nineteenth-century France." *The Review of Economics and Statistics* 92, no. 4 (2010): 714-728.

Banerjee, Abhijit, Rukmini Banerji, James Berry, Esther Duflo, Harini Kannan, Shobhini Mukherji, Marc Shotland, and Michael Walton. “Mainstreaming an effective intervention: Evidence from randomized evaluations of ‘Teaching at the Right Level’ in India.” No. w22746. National Bureau of Economic Research, 2016.

Baranov, Victoria, Sonia R. Bhalotra, Pietro Biroli, and Joanna Maselko. “Maternal Depression, Women’s Empowerment, and Parental Investment: Evidence from a Large Randomized Control Trial.” No. 11187. IZA Discussion Papers, 2017.

Behrman, Jere R. “Mother's schooling and child education: A survey.” Philadelphia: University of Pennsylvania, 1997.

Behrman, Jere R., and John Hoddinott. "Programme evaluation with unobserved heterogeneity and selective implementation: The Mexican PROGRESA impact on child nutrition." *Oxford Bulletin of Economics and Statistics* 67.4 (2005): 547-569.

Berkman, Douglas S., Andres G. Lescano, Robert H. Gilman, Sonia L. Lopez, and Maureen M. Black. "Effects of stunting, diarrhoeal disease, and parasitic infection during infancy on cognition in late childhood: a follow-up study." *The Lancet* 359, no. 9306 (2002): 564-571.

Bhalotra, Sonia, and Samantha B. Rawlings. "Intergenerational persistence in health in developing countries: The penalty of gender inequality?." *Journal of Public Economics* 95, no. 3 (2011): 286-299.

Bharadwaj, Prashant, Juan Pedro Eberhard, and Christopher A. Neilson. "Health at birth, parental investments, and academic outcomes." *Journal of Labor Economics* 36, no. 2 (2018): 349-394.

Bharadwaj, Prashant, Katrine Vellesen Løken, and Christopher Neilson. "Early life health interventions and academic achievement." *American Economic Review* 103, no. 5 (2013): 1862-91.

Black, Sandra E., Aline Bütikofer, Paul J. Devereux, and Kjell G. Salvanes. *This Is Only a Test? Long-Run Impacts of Prenatal Exposure to Radioactive Fallout*. No. w18987. National Bureau of Economic Research, 2013.

Bossavie, Laurent, Harold Alderman, John Giles, and Cem Mete. "The effect of height on earnings: Is stature just a proxy for cognitive and non-cognitive skills?" (2017).

Bozzoli, Carlos, Angus Deaton, and Climent Quintana-Domeque. "Adult height and childhood disease." *Demography* 46, no. 4 (2009): 647-669.

Bundervoet, Tom, Philip Verwimp, and Richard Akresh. "Health and civil war in rural Burundi." *Journal of Human Resources* 44, no. 2 (2009): 536-563.

Burde, Dana, and Leigh L. Linden. "Bringing Education to Afghan Girls: A Randomized Controlled Trial of Village-Based Schools." *American Economic Journal: Applied Economics* 2013, 5(3): 27–40

Card, David. "The causal effect of education on earnings." *Handbook of Labor Economics* (1999): 1801-1863.

Cas, Ava Gail, Elizabeth Frankenberg, Wayan Suriastini, and Duncan Thomas. "The impact of parental death on child well-being: evidence from the Indian Ocean tsunami." *Demography* 51, no. 2 (2014): 437-457.

Case, Anne, and Christina Paxson. "Causes and consequences of early-life health." *Demography* 47.1 (2010): S65-S85.

Case, Anne, Darren Lubotsky, and Christina Paxson. "Economic status and health in childhood: The origins of the gradient." *American Economic Review* 92, no. 5 (2002): 1308-1334.

Christian, Paul, and Brian Dillon. "Growing and Learning when Consumption is Seasonal: Long-term Evidence from Tanzania." *Demography* 55, no. 3 (2018): 1091-1118.

Clarke, Daniel J., and Stefan Dercon. *Dull Disasters? How planning ahead will make a difference*. Oxford University Press, 2016.

Currie, Janet, and Enrico Moretti. "Mother's education and the intergenerational transmission of human capital: Evidence from college openings." *The Quarterly Journal of Economics* 118.4 (2003): 1495-1532.

Currie, Janet, and Maya Rossin-Slater. "Weathering the storm: Hurricanes and birth outcomes." *Journal of Health Economics* 32, no. 3 (2013): 487-503.

Das, Jishnu, Stefan Dercon, James Habyarimana, Pramila Krishnan, Karthik Muralidharan, and Venkatesh Sundararaman. 2013. "School Inputs, Household Substitution, and Test Scores." *American Economic Journal: Applied Economics*, 5 (2): 29-57.

Datar, Ashlesha, Jenny Liu, Sebastian Linnemayr, and Chad Stecher. "The impact of natural disasters on child health and investments in rural India." *Social Science & Medicine* 76 (2013): 83-91.

Deaton, Angus. "Height, health, and inequality: the distribution of adult heights in India." *American Economic Review* 98, no. 2 (2008): 468-74.

Dercon, Stefan, and Catherine Porter. "Live aid revisited: long-term impacts of the 1984 Ethiopian famine on children." *Journal of the European Economic Association* 12, no. 4 (2014): 927-948.

Deryugina, Tatyana, Laura Kawano, and Steven Levitt. "The economic impact of Hurricane Katrina on its victims: evidence from individual tax returns." *American Economic Journal: Applied Economics* 10, no. 2 (2018): 202-33.

Department for International Development and DFID Pakistan. (2018). Pakistan earthquake: Seven years on. [online] Available at: https://www.gov.uk/government/case-studies/pakistan-earthquake-seven-years-on [Accessed 29 Jun. 2018].

Ecclesston, Melissa. "In Utero Exposure to Maternal Stress: Effects of 9/11 on Birth and Early Schooling Outcomes in New York City." Job market paper: Harvard University (2011).

Encyclopedia Britannica. (2018). *Kashmir earthquake of 2005*. [online] Available at: https://www.britannica.com/event/Kashmir-earthquake-of-2005 [Accessed 29 Jun. 2018].

Fernald, Lia CH, Paul J. Gertler, and Lynnette M. Neufeld. "10-year effect of Oportunidades, Mexico's conditional cash transfer programme, on child growth, cognition, language, and behaviour: a longitudinal follow-up study." *The Lancet* 374.9706 (2009): 1997-2005.

Figlio, David, Jonathan Guryan, Krzysztof Karbownik, and Jeffrey Roth. "The effects of poor neonatal health on children's cognitive development." *American Economic Review* 104, no. 12 (2014): 3921-55.

Frankenberg, Elizabeth, Bondan Sikoki, Cecep Sumantri, Wayan Suriastini, and Duncan Thomas. "Education, vulnerability, and resilience after a natural disaster." *Ecology and Society: A journal of integrative science for resilience and sustainability* 18, no. 2 (2013): 16.

Frankenberg, Elizabeth, Thomas Gillespie, Samuel Preston, Bondan Sikoki, and Duncan Thomas. "Mortality, the family and the Indian Ocean tsunami." *The Economic Journal* 121, no. 554 (2011): F162-F182.

Glewwe, Paul, and Elizabeth M. King. "The impact of early childhood nutritional status on cognitive development: Does the timing of malnutrition matter?." *The World Bank Economic Review* 15, no. 1 (2001): 81-113.

Glewwe, Paul, Hanan G. Jacoby, and Elizabeth M. King. "Early childhood nutrition and academic achievement: a longitudinal analysis." *Journal of Public Economics* 81.3 (2001): 345-368.

Groppo, Valeria, and Kati Kraehnert. "Extreme weather events and child height: evidence from Mongolia." *World Development* 86 (2016): 59-78.

Gunnsteinsson, S., A. Adhvaryu, P. Christian, A. Labrique, J. Sugimoto, A. A. Shamim, and K. P. West Jr. "Vitamin A and Resilience to Early Life Shocks." Working Paper, 2014.

Handa, Sudhanshu, and Amber Peterman. "Child Health and School Enrollment A Replication." *Journal of Human Resources* 42.4 (2007): 863-880.

Handa, Sudhanshu, and Amber Peterman. "Is There Catch-Up Growth? Evidence from Three Continents." *Oxford Bulletin of Economics and Statistics* 78, no. 4 (2016): 470-500.

Hicks, Raymond, and Dustin Tingley. "Causal mediation analysis." *The Stata Journal* 11, no. 4 (2011): 605-619.

Hoddinott, John, and Bill Kinsey. "Child growth in the time of drought." *Oxford Bulletin of Economics and Statistics* 63.4 (2001): 409-436.

Hoddinott, John, John Maluccio, Jere R. Behrman, Reynaldo Martorell, Paul Melgar, Agnes R. Quisumbing, Manuel Ramirez-Zea, Aryeh D. Stein, and Kathryn M. Yount. "The consequences of early childhood growth failure over the life course." *Washington, DC: International Food Policy Research Institute Discussion Paper* 1073 (2011).

Kaneda, Heitaro, Takashi Nakata, Hiroyuki Tsutsumi, Hisao Kondo, Nobuhiko Sugito, Yasuo Awata, Sardar S. Akhtar, et al., ‘‘Surface Rupture of the 2005 Kashmir, Pakistan, Earthquake and Its Active Tectonic Implications,’’ *Bulletin of the Seismological Society of America* 98 (2008), 521–557.

Kuczmarski, Robert J. "CDC growth charts: United States." Advance Data 314 (2000): 1.

Lauderdale, Diane S. "Birth outcomes for Arabic-named women in California before and after September 11." *Demography* 43, no. 1 (2006): 185-201.

Maccini, S. Yang. 2009. "Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall." *American Economic Review* 99.3: 1006-26.

Malamud, Ofer, Cristian Pop-Eleches, and Miguel Urquiola. “Interactions between family and school environments: Evidence on dynamic complementarities?” No. w22112. National Bureau of Economic Research, 2016.

McCartin, B. 2003. “A Geometric Characterization of Linear Regression.” *Statistics: A Journal of Theoretical and Applied Statistics* 37(2): 101-117.

McIntosh, Molly Fifer. "Measuring the labor market impacts of Hurricane Katrina migration: Evidence from Houston, Texas." *American Economic Review* 98, no. 2 (2008): 54-57.

Montenegro, Claudio E., and Harry Anthony Patrinos. *Comparable estimates of returns to schooling around the world.* The World Bank (2014).

Narayan, Ambar; Van der Weide, Roy; Cojocaru, Alexandru; Lakner, Christoph; Redaelli, Silvia; Mahler, Daniel Gerszon; Ramasubbaiah, Rakesh Gupta N.; Thewissen, Stefan. 2018. *Fair Progress? : Economic Mobility Across Generations Around the World.* Equity and Development. Washington, DC: World Bank. https://openknowledge.worldbank.org/handle/10986/

Paxson, Christina, and Norbert Schady. "Does money matter? The effects of cash transfers on child development in rural Ecuador." *Economic Development and Cultural Change* 59.1 (2010): 187-229.

Pörtner, Claus. "Natural Hazards and Child Health." Available at SSRN 1599432 (2010).

Piketty, Thomas. "About capital in the twenty-first century." *American Economic Review* 105, no. 5 (2015): 48-53.

Rosales, Maria. "Impact of early life shocks on human capital formation: El Niño floods in Ecuador." (2014).

Sawada, Yasuyuki, and Satoshi Shimizutani. "How Do People Cope with Natural Disasters? Evidence from the Great Hanshin-Awaji (Kobe) Earthquake in 1995." *Journal of Money, Credit and Banking* 40.2-3 (2008): 463-488.

Scawthorn, Charles, and Wai-Fah Chen, eds. 2002. *Earthquake Engineering Handbook*. Boca Raton: CRC Press.

Shrimpton, Roger, Cesar G. Victora, Mercedes de Onis, Rosângela Costa Lima, Monika Blössner, and Graeme Clugston. "Worldwide timing of growth faltering: implications for nutritional interventions." *Pediatrics* 107, no. 5 (2001): e75-e75.

Strauss, John, and Duncan Thomas. "Health, nutrition, and economic development." *Journal of Economic Literature* 36.2 (1998): 766-817.

Stock, James H., and Motohiro Yogo. "Testing for weak instruments in linear IV regression." No. t0284. National Bureau of Economic Research, 2002.

The United Nations Children's Fund. *The State of the World's Children 2019: Nutrition*

Ustun, Aydin, I. Oztug Bildirici, H. Zahit Selvi, and R. Alpay Abbak. "An evaluation of SRTM3 DTM data: Validity, problems and solutions." *The first international conference on cartography and GIS.* 2006.

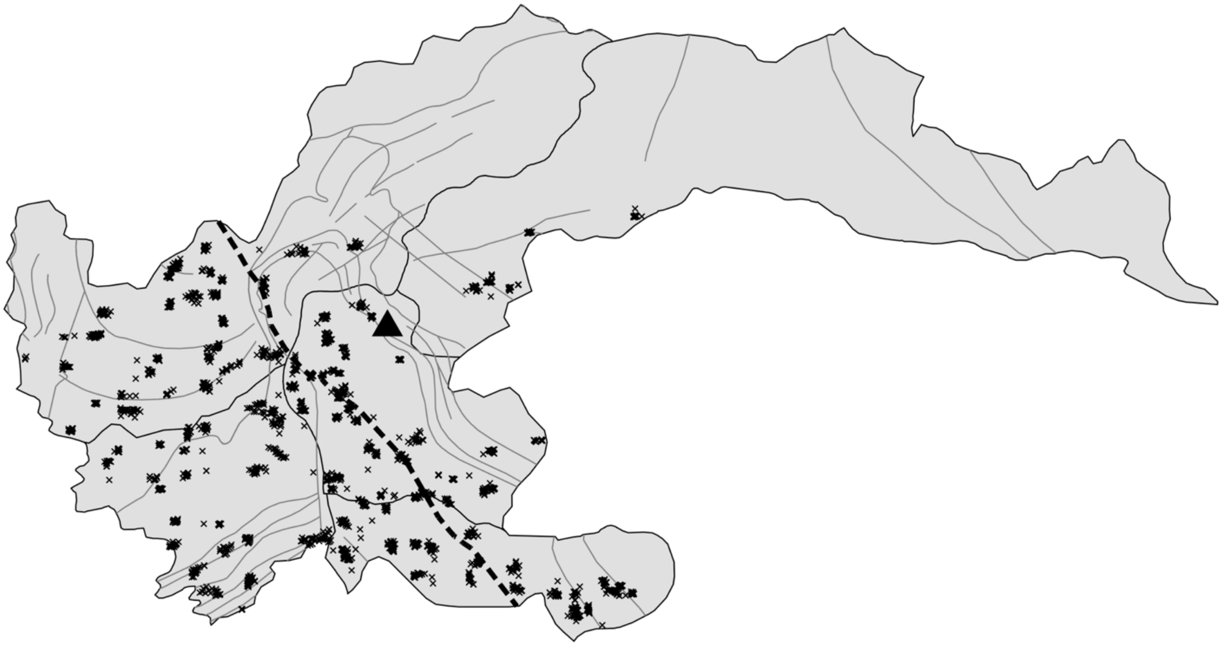
Weil, David. "Capital and Wealth in the 21st Century*", NBER Working Paper no. 20919,* (2015)

Weldeegzie, Samuel G. "Growing-up Unfortunate: War and Human Capital in Ethiopia." *World Development* 96 (2017): 474-489.

Wierzba, Thomas F., Remon Abu El-Yazeed, Stephen J. Savarino, Ali S. Mourad, Malla Rao, Manal Baddour, A. Nour El-Deen, Abdullah B. Naficy, and John D. Clemens. "The interrelationship of malnutrition and diarrhea in a periurban area outside Alexandria, Egypt." *Journal of Pediatric Gastroenterology and Nutrition* 32, no. 2 (2001): 189-196.

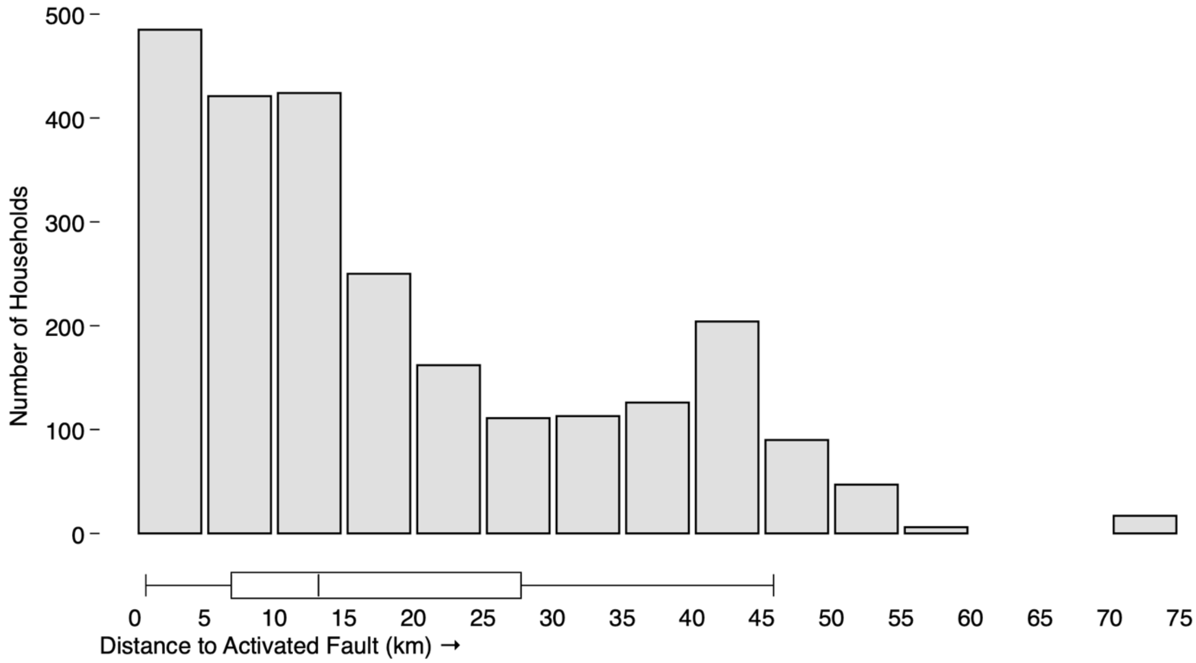
Young, Alwyn. "Consistency without inference: Instrumental variables in practical application." (2019).

Figure 1: Map of study area, surveyed households, activated fault and epicenter, and non-activated faults



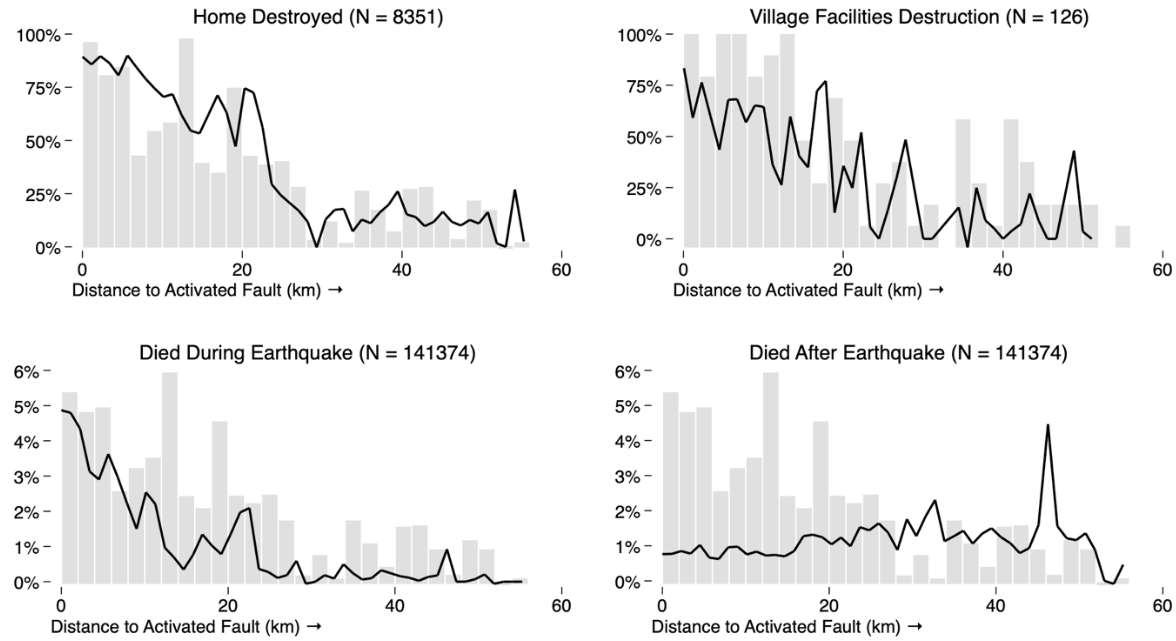
Notes: This map illustrates the location of all 2,456 households that completed the detailed household survey (X's), the location of the activated Balakot-Bagh Fault (thick dashed line), and the earthquake epicenter (black triangle). Current district boundaries are shown as thin solid black lines (Neelum District was part of Muzaffarabad District until 2005). Fault lines which were not activated in the earthquake are shown as thin solid gray lines.

Figure 2. Distance distribution of survey households to the activated fault line



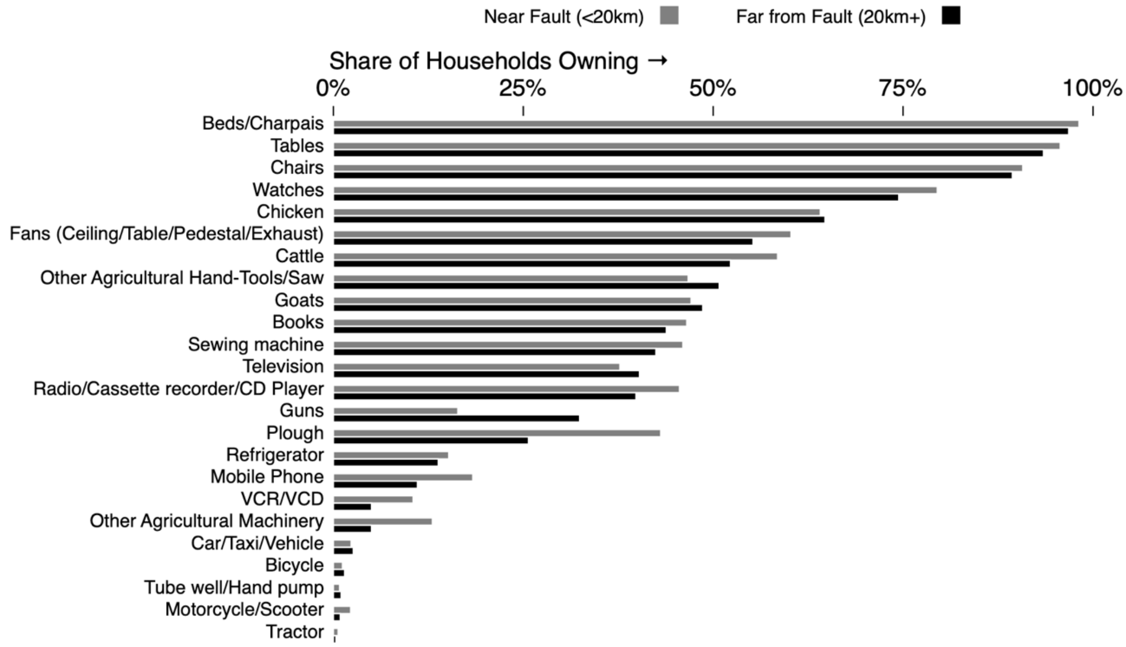
Notes: This figure illustrates the distance distribution of the 2,456 households from the detailed survey to the activated fault (histogram), as the number of households in each 5km bin as well as the 5th, 25th, 50th, 75th, and 95th percentiles of the distribution (box plot).

Figure 3. Immediate and extended earthquake deaths and destruction



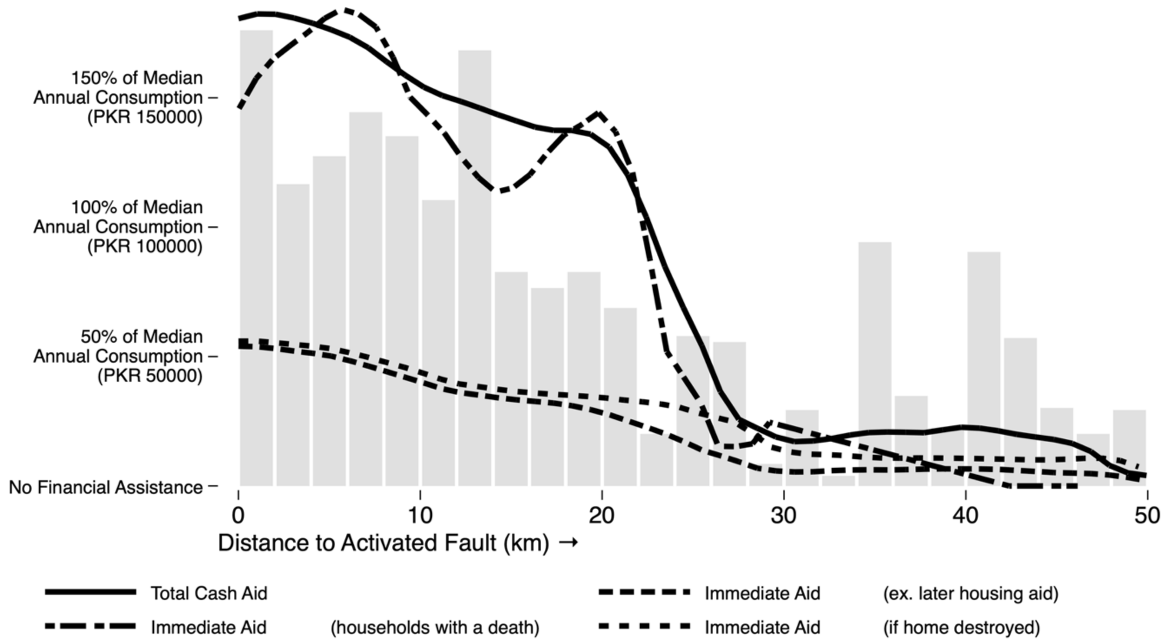
Notes: These plots illustrate the proportion of homes reported destroyed in both long census and detailed survey measures; the proportion of public infrastructure noted destroyed in village survey; and the proportion of census records reported deceased during and/or after the earthquake, as a non-parametric function of distance to the activated fault. Histograms show relative density as the number of observation units (households, villages, or individuals) in each 2km bin.

Figure 4. Pre-earthquake assets comparison



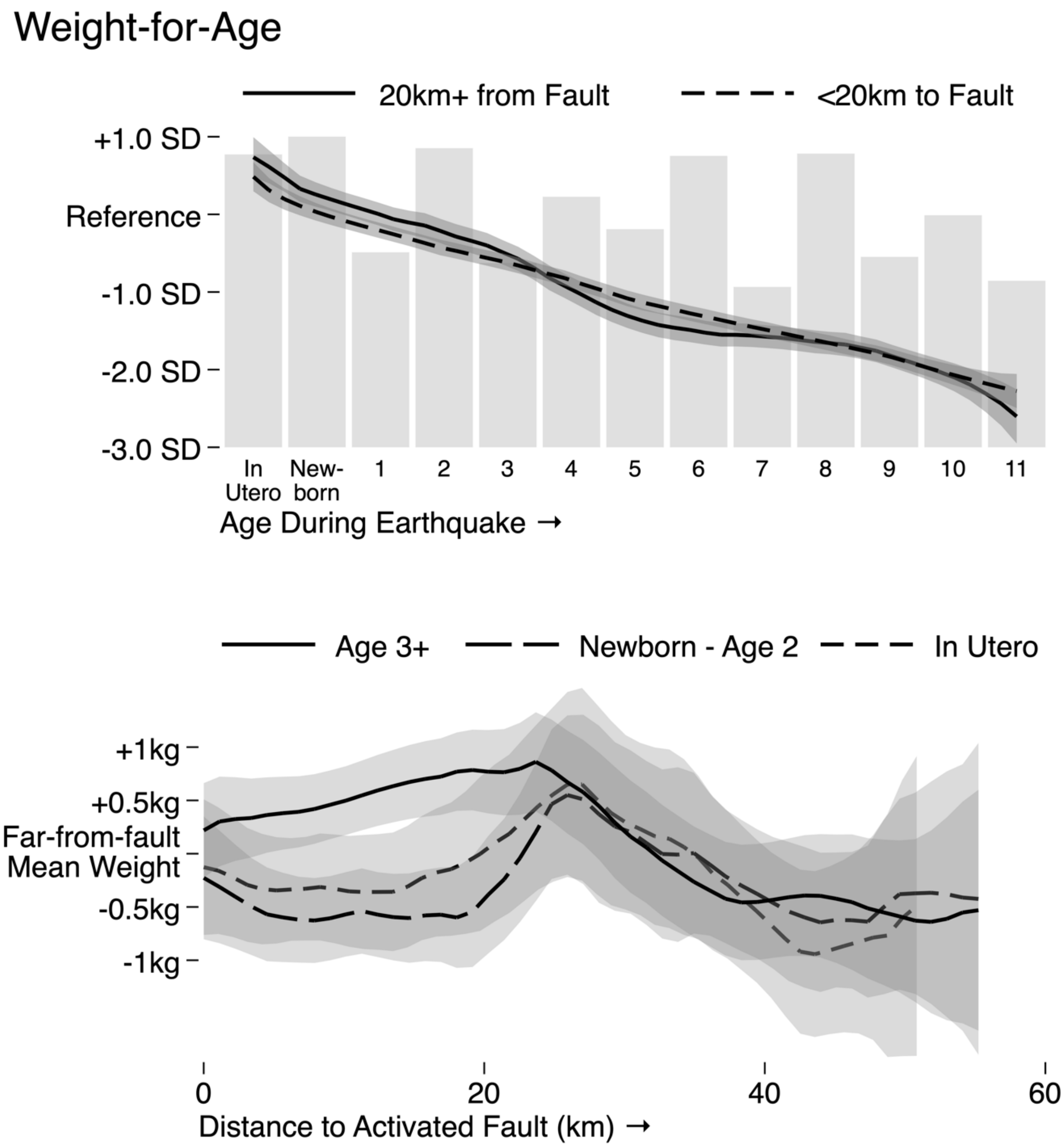
Notes: This figure tabulates the proportion of households who self-reported ownership rates (prior to the earthquake) of the assets in our wealth index are compared between near-fault (<20km) and far-from-fault (20km+) households.

Figure 5. Self-reported receipts of cash aid after the earthquake



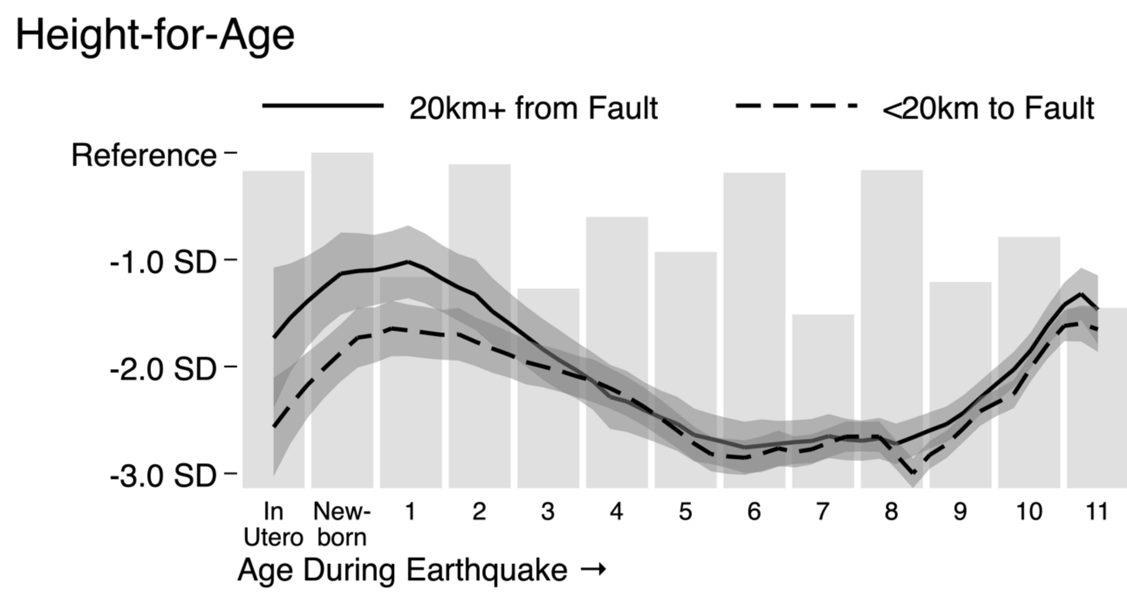
Notes: This figure illustrates self-reported aid received by households as a function of distance to the activated fault line, split into total aid (full recovery period) and immediate aid (non-rebuilding aid) for all households, households with a death, and households that reported home destruction. The histogram shows relative density as the number of households in each 2km bin.

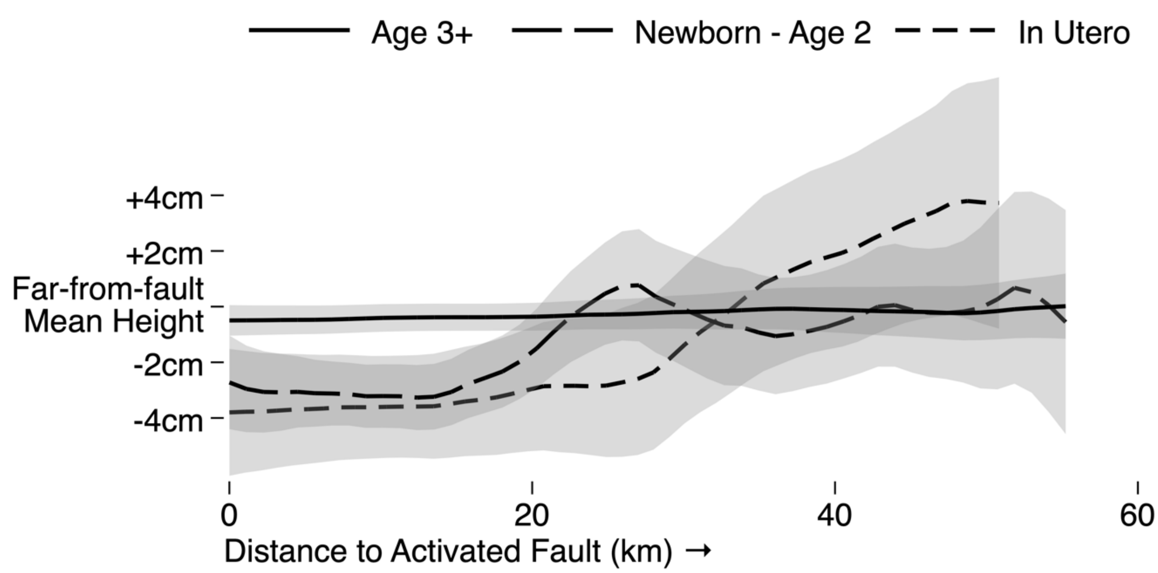
Figure 6a. Weight outcomes for children



Notes: As a function of age at the time of the earthquake and distance to the activated fault, these graphs compare the current weight-for-age (in both z-scores and kg) of children covered in the detailed survey between near-fault and far-from fault groups using nonparametric specifications. Test score results are presented as normalized IRT scores within the observed population with mean zero and standard deviation 1. The histogram shows relative density as the number of children at each age. Shaded areas show 95% confidence intervals.

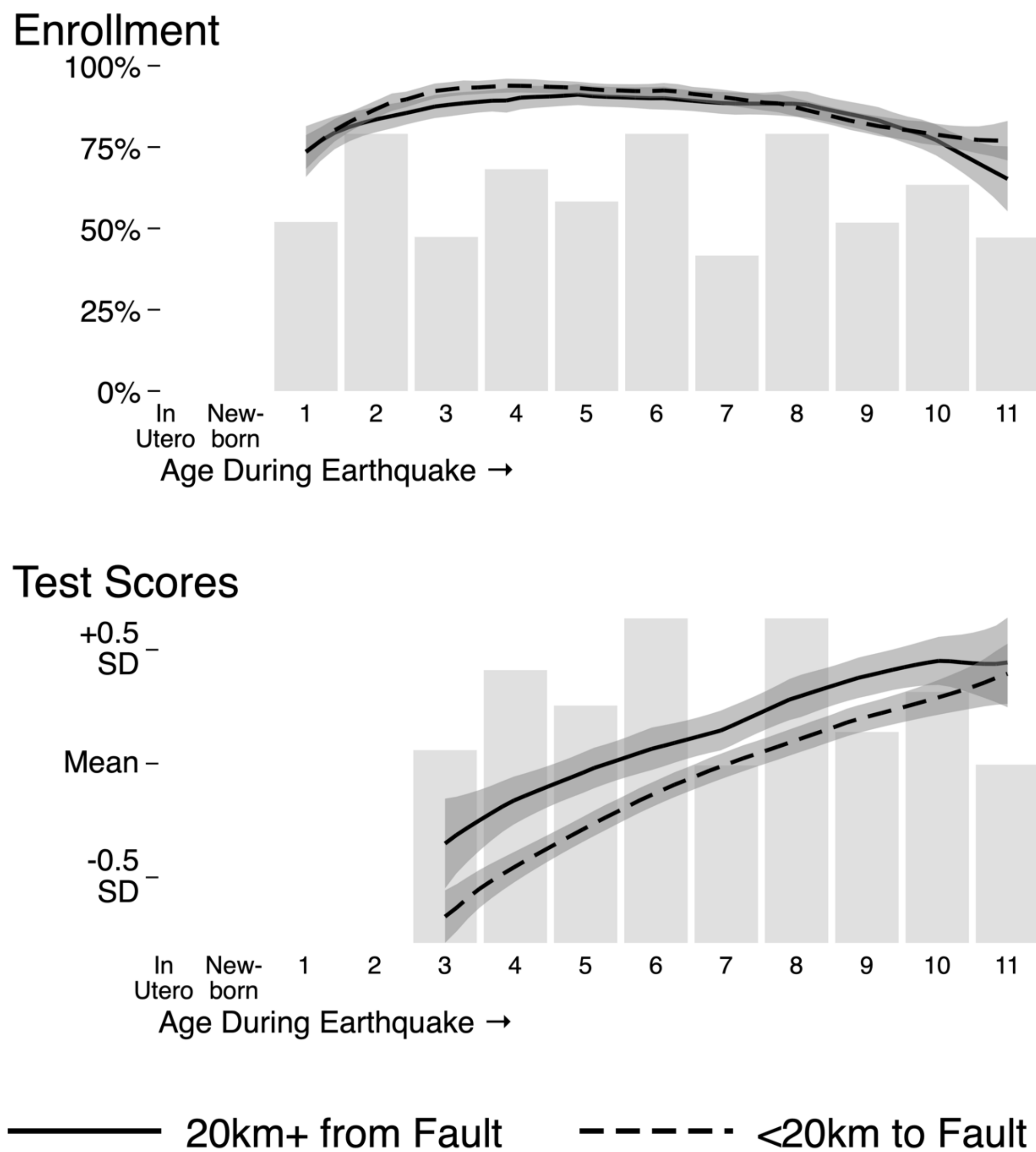
Figure 6b. Height outcomes





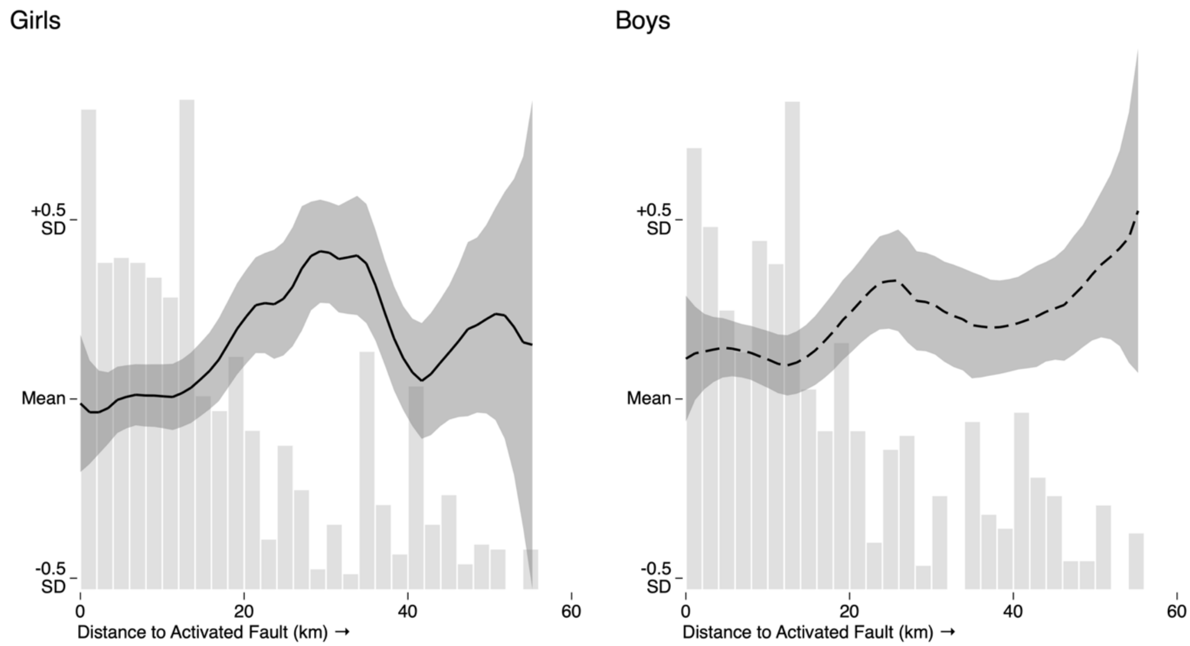
Notes: As a function of age at the time of the earthquake and distance to the activated fault, these graphs compare the current height-for-age (in both z-scores and cm) of children covered in the detailed survey between near-fault and far-from fault groups using nonparametric specifications. Test score results are presented as normalized IRT scores within the observed population with mean zero and standard deviation 1. The histogram shows relative density as the number of children at each age. Shaded areas show 95% confidence intervals.

Figure 7a. Education outcomes



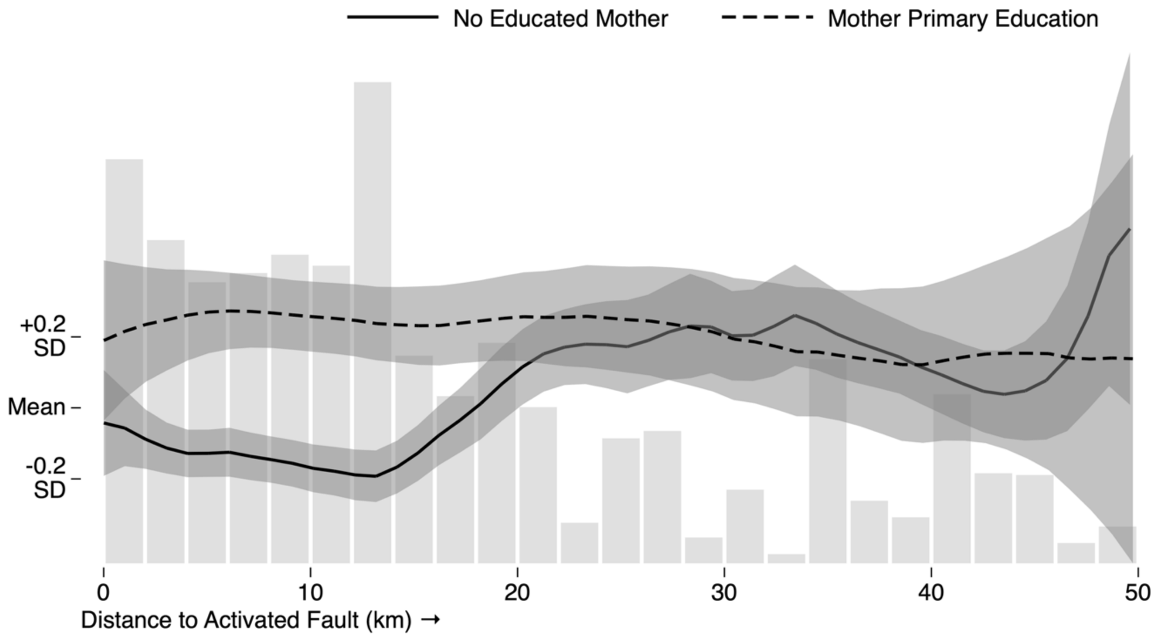
Notes: As a function of age at the time of the earthquake, these graphs compare the current school enrollment and academic performance of children covered in the detailed survey between near-fault and far-from fault groups using nonparametric specifications. Test score results are presented as normalized IRT scores within the observed population with mean zero and standard deviation 1. The histogram shows relative density as the number of children at each age. Shaded areas show 95% confidence intervals.

Figure 7b. Test performance by gender and distance to activated fault



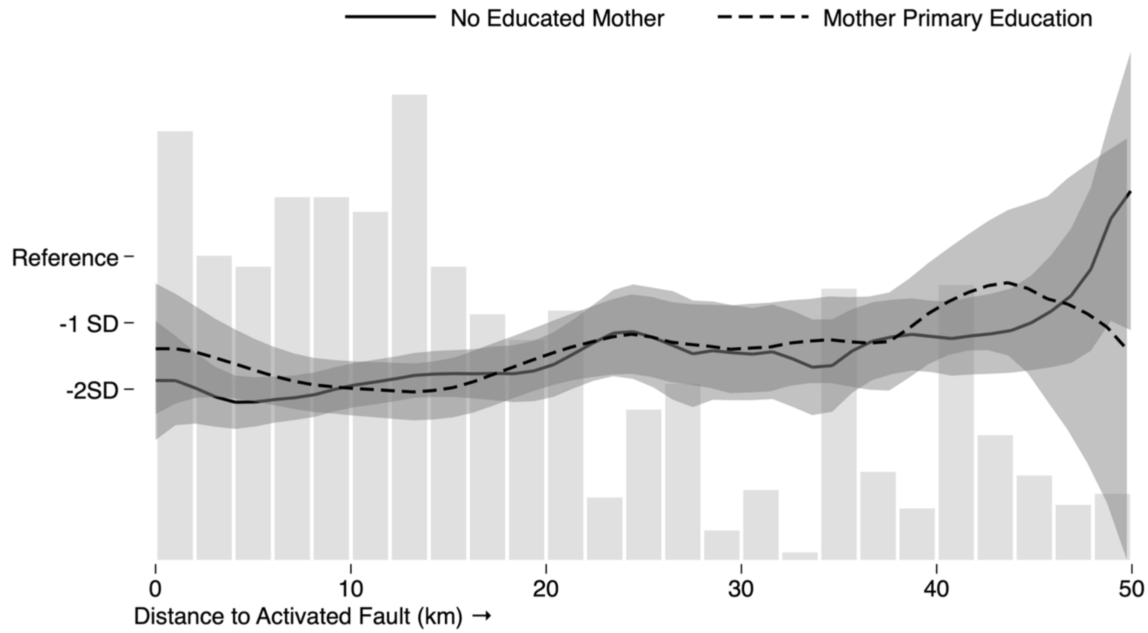
Notes: These graphs illustrate the test scores of boys and girls separately by distance to the activated fault using nonparametric local polynomial estimation. Test score results are presented as normalized IRT scores within the observed population with mean zero and standard deviation 1. The histogram shows relative density as the number of children in each 2km bin. Shaded areas show 95% confidence intervals.

Figure 8a. Test scores and maternal education



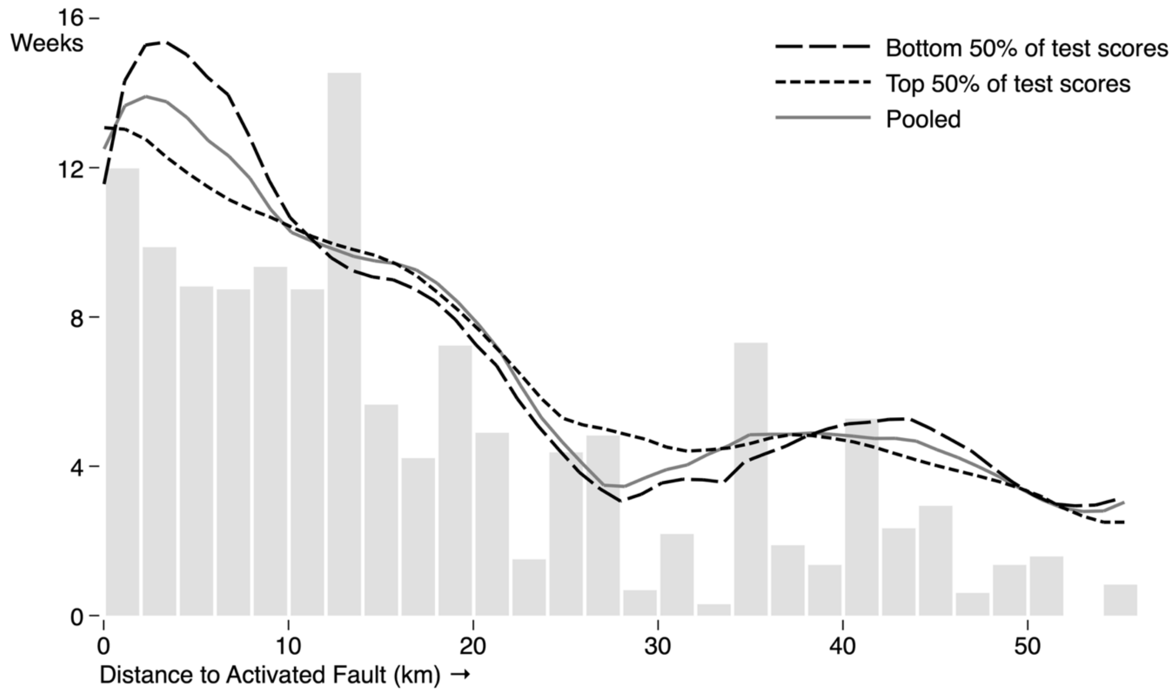
Notes: This graph illustrates the test scores of children whose mothers have or have not completed primary education, separately, by distance to the activated fault using nonparametric local polynomial estimation. Test score results are presented as normalized IRT scores within the observed population with mean zero and standard deviation 1. The histogram shows relative density as the number of children in each 2km bin. Shaded areas show 95% confidence intervals.

Figure 8b. Height and maternal education



Notes: For children under the age of 3 during the earthquake, this graph illustrates the height-for-age Z-scores of children whose mothers have or have not completed primary education, separately, by distance to the activated fault using nonparametric local polynomial estimation. The histogram shows relative density as the number of children in each 2km bin. Shaded areas show 95% confidence intervals.

Figure 9. Time out of school after the earthquake



Notes: As a nonparametric function of distance to the activated fault line, this graph illustrates the varying average time out of school taken by children who later ended up in the top and bottom half of the test score distribution. The histogram shows relative density as the number of households in each 2km bin.

Table 1. Descriptive Statistics

	(1) Mean	(2) SD	(3) 25th	(4) Median	(5) 75th	(6) N	(7) Source of Data
Household Geography							
Distance to Fault (km)	17.5	14.1	5.6	13.6	24.3	28,297	Standard Census
Distance to Epicenter (km)	36.4	17.5	25.1	35.2	48.0	28,297	Standard Census
Closest Faultline (km)	2.8	2.5	0.8	2.0	4.1	28,297	Standard Census
Mean Slope of Union Council (degrees)	21.1	6.7	16.9	22.2	26.1	98	GIS - Union Council Level
District - Abbottabad	20.6%					2,456	Household Survey
District - Bagh	17.5%					2,456	Household Survey
District - Mansehra	27.6%					2,456	Household Survey
District - Muzaffarabad	34.2%					2,456	Household Survey
Household Death, Destruction, and Aid							
Death in Household During Earthquake	6.1%					28,297	Standard Census
Home Damaged or Destroyed	91.1%					8,350	Extended Census and Survey
Home Destroyed	57.2%					8,351	Extended Census and Survey
Received any form of aid	66.8%					2,456	Household Survey
Received any cash aid	46.7%					2,456	Household Survey
Cash Aid Amount (PKR)	116,182	102,982	0	125,000	175,000	2,456	Household Survey
Household Socioeconomic Characteristics							
Household Size	6.1	2.7	4.0	6.0	8.0	2,455	Household Survey
Total Annual Food Expenditure (PKR)	83,208	88,161	37,500	62,280	98,805	2,456	Household Survey
Total Annual Nonfood Expenditure (PKR)	84,207	109,511	26,787	46,183	93,035	2,456	Household Survey
Pre-Earthquake Asset Index	0.00	1.00	-0.55	-0.09	0.57	2,456	Household Survey
Number of children under age 6 during earthquake	1.0	1.1	0.0	1.0	2.0	2,456	Household Survey
Female head of household	10.0%					2,456	Household Survey
Individual Characteristics							
Male	52%					152,435	Standard Census and Survey
Age	24.0	18.4	10.0	20.0	35.0	152,435	Standard Census and Survey
In Utero to Age 11 During Earthquake	33%					152,435	Standard Census and Survey
Children In Utero - Age 11 During Earthquake							
In Utero	9.0%					4,665	Household Survey
Age 0-2	25.7%					4,665	Household Survey
Age 3+	65.3%					4,665	Household Survey
Child's Height (cm)	117.5	22.3	101.0	119.0	132.0	4,096	Household Survey
Child's Weight (kg)	25.6	9.3	18.0	24.0	31.0	4,097	Household Survey
School Enrollment During Survey (Age 1+ during Earthquake)	86.1%					3,589	Household Survey
Private School Enrollment Rate During Survey	21.7%					3,089	Household Survey
Parents of Children In Utero - Age 11 During Earthquake							
Father Completed Primary School	57.3%					4,379	Household Survey
Mother Completed Primary School	22.2%					4,387	Household Survey
Mother's Age	37.425	8.4	31.0	37.0	42.0	4,387	Household Survey
Mother's Height (cm)	157.238	7.8	152.0	157.0	162.0	4,239	Household Survey
Mother's School Access Instrument	0.464	0.5	0.0	0.0	1.0	4,155	Household Survey
Father's Age	43.182	10.0	37.0	42.0	49.0	4,379	Household Survey
Father's Height (cm)	168.579	6.9	165.0	170.0	173.0	3,876	Household Survey

Table 2a. Distance to Faultline and Pre-Earthquake Characteristic Exogeneity

	Distance to Faultline Coefficient	N	R2	Mean
Villages (1998 Village Census)				
Total Population	-18.377	126	0.186	3,376.174
	19.625			
Male Population	-9.412	126	0.182	1,685.186
	10.037			
Female Population	-8.965	126	0.189	1,690.988
	9.623			
Muslim Population	-18.273	126	0.186	3,365.388
	19.543			
Literacy Rate	-0.000	125	0.401	0.456
	0.001			
Proportion with Primary Education	-0.002*	126	0.354	0.387
	0.001			
Proportion Females with Secondary Education	-0.000	126	0.143	0.026
	0.000			
Average Household Size	-0.024**	126	0.252	6.970
	0.011			
Number of Permanent Houses	-0.755	120	0.200	223.304
	1.259			
Number of Houses with Electricity	-2.324	112	0.130	290.126
	2.028			
Number of Houses With Potable Water	-1.269	100	0.167	100.083
	0.971			
Village Infrastructure Index	-0.013	126	0.154	0.421
	0.009			
Adults 18+ During Survey (2009 Household Census and Survey)				
Male Height (cm)	0.020	2,735	0.020	167.512
	0.027			
Female Height (cm)	0.046**	2,834	0.007	157.164
	0.023			
Male Age (Living)	0.008	36,755	0.001	36.554
	0.010			
Female Age (Living)	0.026**	33,273	0.002	35.052
	0.010			
Males Completed Primary School (Living)	-0.000	44,495	0.025	0.636
	0.001			
Females Completed Primary School (Living)	-0.002	40,474	0.024	0.315
	0.001			
Male Age (Deceased)	0.268***	1,459	0.066	56.883
	0.079			
Female Age (Deceased)	0.151*	950	0.115	45.609
	0.088			
Males Completed Primary School (Deceased)	0.000	75	0.079	0.280
	0.005			
Females Completed Primary School (Deceased)	-0.004	71	0.074	0.239
	0.004			
Male Age (All)	0.018*	38,214	0.001	56.883
	0.010			
Female Age (All)	0.024**	34,223	0.002	45.609
	0.009			
Males Completed Primary School (All)	-0.000	44,570	0.025	0.280
	0.001			
Females Completed Primary School (All)	-0.002	40,545	0.026	0.239
	0.001			
Households (2009 Household Survey)				
Recall				
Electricity in House	-0.009***	2,456	0.108	0.851
	0.002			
Water In House	-0.003	2,456	0.042	0.452
	0.002			
Permanent House	-0.003	2,456	0.103	0.375
	0.002			
Distance to Closest Market (min)	0.237	2,452	0.089	54.508
	0.336			
Distance to Closest Water Source (min)	0.056	2,456	0.030	9.564
	0.051			
Distance to Closest Medical Facility (min)	-0.086	2,444	0.069	57.421
	0.290			
Distance to Closest Private School (min)	-0.112	2,372	0.039	43.907
	0.251			
Distance to Closest Government School (min)	0.022	2,454	0.035	20.762
	0.085			
Measured				
Distance to Closest Water Source (km)	0.052	2,456	0.215	3.003
	0.035			
Distance to Closest Health Clinic (km)	0.122***	2,456	0.344	5.285
	0.043			
Distance to Closest Private School (km)	0.102**	2,456	0.255	3.318
	0.045			
Distance to Closest Boys School (km)	0.090*	2,456	0.251	1.142
	0.050			
Distance to Closest Girls School (km)	0.009	2,456	0.047	1.271
	0.023			

Notes: This table reports the results from a regression specification on pre-earthquake characteristics by distance to the activated fault line. The coefficient on distance to the fault is reported, along with the number of observations, the r-squared, and the overall mean of the variable. The regression includes controls for distance to the earthquake epicenter, local slope, and district fixed effects. Measured distance to water is replaced by zero when recall survey notes that water was available in the house.

Table 2b. Post-Earthquake Recovery at Time of Survey

	(1) Distance to Faultline (km) Coefficient	(2) N	(3) R2	(4) Mean
PANEL A: Household Socioeconomic Characteristics				
Asset Index (PCA) (Post-Quake)	-0.004	2,456	0.122	0.002
	0.004			
Household Infrastructure Index	-0.024***	2,456	0.168	0.000
	0.006			
Permanent House (Post-Quake)	-0.005**	2,456	0.089	0.635
	0.002			
Electricity	-0.008***	2,456	0.142	0.904
	0.002			
Water In House (Post-Quake)	-0.005*	2,456	0.057	0.498
	0.003			
Log Consumption per Capita	0.003	2,456	0.072	10.04
	0.003			
PANEL B: Access to Public Infrastructure				
Log Dist to Gov't School (min)	-0.004	2,454	0.039	2.78
	0.003			
Log Dist to Market (min)	0.004	2,452	0.119	3.62
	0.006			
Log Dist to Distr Office (min)	-0.005	2,449	0.240	4.83
	0.005			
Log Dist to Medical (min)	-0.003	2,444	0.048	3.79
	0.005			
Log Dist to Private School (min)	-0.006	2,369	0.037	3.40
	0.006			
PANEL C: Adult Health				
Adult Height	0.022	6,907	0.007	145.32
	0.022			
Adult Weight	0.017	6,907	0.012	45.59
	0.020			
Adult Height (18-24)	-0.011	1,717	0.012	130.25
	0.035			
Adult Weight (18-24)	0.016	1,717	0.011	34.12
	0.028			

Notes: This table reports the results from a regression specification on post-earthquake characteristics by distance to the activated fault line. The coefficient on distance to the fault is reported, along with the number of observations, the r-squared, and the overall mean of the variable. The regression includes controls for distance to the earthquake epicenter, local slope, distance to the nearest fault line, and district fixed effects. Measured distance to water is replaced by zero when recall survey notes that water was available in the house.

Table 3. Child Human Capital Acquisition After the Earthquake

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Weight (Z-score)	Height (Z-score)	School Enrollment	Grade Attainment	Test Scores (IRT)	Test Scores + Disruption	Test Scores + Gender	Test Score + Age
Distance from Faultline (km)	-0.007*	0.002	-0.000	0.00	0.008**	0.006*	0.007	0.012**
	(0.004)	*(0.005)*	*(0.001)*	*(0.01)*	*(0.004)*	*(0.003)*	*0.005*	*(0.005)*
Weeks out of School After Earthquake						-0.004*		
						(0.002)		
In Utero * Distance from Faultline (km)	0.003	0.036**						
	(0.006)	*(0.017)*						
Age 0-2 * Distance from Faultline (km)	0.005	0.015*						
	(0.005)	*(0.009)*						
Male	-0.041	0.037	0.077***	0.12	0.068	0.00	0.040	0.066
	(0.048)	*(0.081)*	*(0.016)*	*(0.11)*	*(0.043)*	*(0.04)*	*0.074*	*(0.044)*
(log) Consumption per Capita	-0.001	0.084	0.026**	0.25**	0.141***	0.11***	0.141***	0.139**
	(0.045)	*(0.082)*	*(0.011)*	*(0.10)*	*(0.045)*	*(0.04)*	*(0.044)*	*(0.045)*
Distance from Faultline (km) * Male							0.002	
							(0.004)	
Distance from Faultline (km) * Age 6								-0.004
								(0.004)
Distance from Faultline (km) * Age 7								-0.002
								(0.005)
Distance from Faultline (km) * Age 8								-0.007
								(0.005)
Distance from Faultline (km) * Age 9								0.005
								(0.004)
Distance from Faultline (km) * Age 10								-0.008*
								(0.004)
Distance from Faultline (km) * Age 11								-0.008
								(0.006)
Dependent Variable Mean	*-0.944*	*-2.155*	*0.903*	*4.17*	*0.131*	*0.229*	*0.131*	*0.131*
Geographic Controls	X	X	X	X	X	X	X	X
Individual and SES Controls	X	X	X	X	X	X	X	X
Age Dummies	X	X	X	X	X	X	X	X
Regression R2	0.247	0.077	0.074	0.338	0.099	0.110	0.100	0.104
Number of Observations	4,002	4,001	1,874	1,874	1,874	1,547	1,874	1,874

Notes: This table reports regression results for effects of the earthquake on early childhood development during the follow-up survey four years later, as measured by the coefficient of current outcomes on distance to the activated fault. The dependent variables are indicated in column names. The regressions include controls for distance to the earthquake epicenter, local slope, distance to the nearest fault line, and district fixed effects, as well as indicator variables for the age of the child. Significance levels are indicated by stars as follows: *** p<0.01, ** p<0.05, * p<0.1

Table 4a. Maternal Education Effects

	(1)	(2)	(3)	(4)
	Test Scores		Height-for-age: In Utero and Age 0-2	
	Maternal Education	Maternal Education Interaction	Maternal Education	Maternal Education Interaction
Distance from Faultline (km)	0.008**	0.009**	0.017	0.016
	(0.004)	*(0.004)*	*(0.012)*	*(0.013)*
Mother Completed Primary School	0.274***	0.398***	0.088	0.030
	(0.051)	*(0.079)*	*(0.230)*	*(0.338)*
Mother's Education * Distance		-0.007**		0.003
		(0.004)		*(0.017)*
Male	0.067	0.067	-0.147	-0.148
	(0.043)	*(0.042)*	*(0.167)*	*(0.167)*
Log Consumption per Capita	0.121***	0.121***	0.063	0.065
	(0.042)	*(0.042)*	*(0.171)*	*(0.171)*
Dependent Variable Mean	*0.13*	*0.13*	*-1.68*	*-1.68*
Geographic Controls	X	X	X	X
Individual and SES Controls	X	X	X	X
Age Dummies	X	X	X	X
Regression R2	0.113	0.114	0.030	0.030
Number of Observations	1,874	1,874	1,423	1,423

Notes: This table reports regression results for effects of the earthquake on early childhood development during the follow-up survey four years later, as measured by the coefficient of current outcomes on distance to the activated fault. These regressions specifically examine the potential for mitigation by maternal education, and include the level effect and the fault-distance interaction term. The dependent variables are indicated in column names. The regressions include controls for distance to the earthquake epicenter, local slope, distance to the nearest fault line, and district fixed effects, as well as indicator variables for the age of the child. Significance levels are indicated by stars as follows: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4b. Maternal Education Effects (Instrumental Variables)

	(1)	(2)	(3)	(4)
	Test Scores		Height-for-age: In Utero and Age 0-2	
	IV Maternal Education	IV Maternal Education Interaction	IV Maternal Education	IV Maternal Education Interaction
Distance from Faultline (km)	0.019***	0.032***	0.007	-0.014
	(0.006)	(0.009)	(0.023)	(0.038)
Mother Completed Primary School	1.594***	4.842**	-3.352*	-4.985
	(0.556)	(1.902)	(1.840)	(3.600)
Mother's Education * Distance		-0.142**		0.083
		(0.069)		(0.126)
Male	0.055	0.035	-0.212	-0.234
	(0.047)	(0.059)	(0.172)	(0.179)
Log Consumption per Capita	-0.029	-0.092	0.439	0.471
	(0.068)	(0.110)	(0.293)	(0.310)
Dependent Variable Mean	*0.135*	*0.135*	*-1.657*	*-1.657*
Geographic Controls	X	X	X	X
Individual and SES Controls	X	X	X	X
Age Dummies	X	X	X	X
Cragg-Donald F-statistic	40.123	9.466	40.660	13.254
Number of Observations	1,716	1,716	1,275	1,275

Notes: This table reports regression results for effects of the earthquake on early childhood development during the follow-up survey four years later, as measured by the coefficient of current outcomes on distance to the activated fault. These regressions specifically examine the potential for mitigation by maternal education using an IV specification, and include the level effect and the fault-distance interaction term, instrumented by the availability of a girls' school in the mother's birth village at enrollment age. The dependent variables are indicated in column names. The regressions include controls for distance to the earthquake epicenter, local slope, distance to the nearest fault line, and district fixed effects, as well as indicator variables for the age of the child. Significance levels are indicated by stars as follows: *** p<0.01, ** p<0.05, * p<0.1

Supplementary Materials

Human Capital Accumulation and Disasters:
Evidence from the Pakistan Earthquake of 2005

Tahir Andrabi[1], Benjamin Daniels[2], and Jishnu Das[2*]
[1]Pomona College, [2]Georgetown University
* Correspondence: 37th and O Streets NW, Washington, DC 20057; or
jishnu.das@georgetown.edu

A1. Description of Sample, Survey Implementation and Human Capital Measures

Our sampling strategy is fully reported in Andrabi and Das (2016).

> *The epicenter of the earthquake was near the city of Muzaffarabad, on the Pakistani side of the Kashmir region approximately 60 miles northeast of Islamabad, the capital. The data in this study come from the four worst affected districts: Bagh and Muzaffarabad (later split into two districts, Neelum and Muzaffarabad) in Kashmir Province, and Mansehra and Abbotabad in the North- Western Frontier Province (renamed Khyber Pakhtunkhwa in 2011). These districts are in two provinces—the North-West Frontier Province (NWFP), now called Khyber Pakhtunwa or KP and Azad Jammu & Kashmir or AJK. The latter is along the "Line of Control" with India. While there was a census of villages in NWFP in 1998 and a similar village listing in AJK, the latter was never publicly released given the disputed international status of AJK. Working together with administrations in both provinces, we managed over a year to compile a clean full list of 1656 "villages" that were theoretically in the census lists. Note though that in the case of NWFP there were 12 years since the last census update and in AJK the original lists had not been physically verified. From this list of 1656 villages in the 4 districts, we took an equal probability sample of 150 villages. We chose an equal probability rather than a PPS sample because the first stage of our work involved a full census of households. At the time we chose our sample, we did not know the physical locations of these villages–particularly in AJK–where geographical information was very sparse and even the local administration was not sure of village locations due to the mountainous terrain.*

> *We first completed a census of all households in the sampled villages. The census allowed us to estimate mortality (a low probability event), and receipt of aid (short-form) and home destruction long-form census, supplemented with the household survey to augment the sample). The randomization for which household received long and short-form censuses was done in the field as multiple census visits were not practical. Appendix Table A1 shows the comparison of key variables between the short and long-form census and validates the randomization. We present standard t-tests of the differences in means. Because the sample sizes are large, some very small differences are also statistically*

significant. We therefore also present the normed mean differences as in Imbens and Woolridge (2008) to document the size of the difference with reference to the underlying standard deviation, where we can verify that none of the differences exceed the 0.2sd threshold suggested by Imbens and Woolridge (2008).

As part of the detailed household survey, which covered a randomly selected 10% subsample of our village census households, we administered in-home academic tests covering English, mathematics, and Urdu (the vernacular language) to all children aged 3-15 at the time of the data collection exercise, meaning they were in utero or aged up to 11 at the time the earthquake struck. Each section was scored across all children using item response theory (IRT), and the overall score for each child is the mean of the three tests. Appendix Figure A1 gives some examples of the questions posed in this test, along with the response rates by age of the child. Table A1a reports testing completion among the eligible group recorded in the household roster listings along with reasons for non-completion, and Table A1c demonstrates that the tested group is a representative subsample of the eligible group.

Children aged 3 and up were eligible for height and weight measurements. We report measurement completion for children aged 3-15 (those in utero or aged up to 11 at the time of the earthquake) in table A1b, along with average measurement statistics. To remove skewness and adjust for the variations in anthropometric measures that depend on age, all heights and weights are standardized to Z-scores using the 2000 US CDC Growth Reference, trimmed at +/-4 standard deviations.[1] Table A1d confirms that the measured group is a representative sub-sample of the eligible group.

A2. Threats to Exogeneity: Aid Spillovers, Migration, and Mortality

Although the earthquake shock itself was exogenous to pre-earthquake characteristics, several further conditions must be satisfied for our survey measures, taken four years later, to be considered causal results from the "joint treatment" of earthquake and subsequent aid. First, aid delivery must be relatively constrained to the earthquake-affected area: otherwise, nearby areas unaffected by the earthquake but receiving aid money would not be an appropriate counterfactual

[1] Code description available at:
http://www.biostat.jhsph.edu/courses/bio624/misc/STATA%20article%20on%20anthropometry%20measures.pdf

to the exposed region. We demonstrate nonparametrically in Figure 5 that aid receipts drop off dramatically beginning around 20km from the activated Faultline, and we confirm in Table A2b that the relationship between aid delivery and fault distance is strong. Additionally, we observe that key socioeconomic characteristics such as household education and pre-earthquake wealth are uncorrelated with the amount of cash aid received as a result.

Next, we address the questions of selective migration and mortality. In our survey we specifically enumerated members of the household who lived there before the earthquake but not currently and vice-versa. Using this information, we regress the likelihood that a household member died or is a migrant on distance to the fault line in Table A2a. We find no difference among adults or among child out-migration; however, we find a significantly higher likelihood that a child is a new arrival further from the earthquake. It is a relatively small effect (2.4 percentage points in our comparison of 10km to 40km), and given that the overall rate of migration is around 3% for all groups we believe large migration flows are not a critical threat to the comparability of our measures. Similarly, as we demonstrated relatively low mortality even in the most exposed areas of the earthquake, we do not believe sample selection in general drives our results.

Nevertheless, we compute bounds for unfavorable selection assumptions for all possible forms of migration, mortality, and survey non-responsiveness in Table A2d. To assess the extent to which selective missingness could compromise our results, we now utilize our complete roster of all potentially eligible non-responders to compute bounds on our primary effects using the method detailed in Lee (2009). Using our binary indicator of distance, this bounding method estimates 2.1% excess responsiveness with 442 non-responsive children of 2,317 potential respondents, and the lower bound on the shock effect between near and far school-age children of -0.13 SD with p=0.014, compared to an unadjusted estimate of -0.17 SD. For heights among children in utero or age 0-2, missingness appears more selective; 4.5% excess observations are trimmed and the worst-case assumptions lead to a point estimate of -0.33 SD with p=0.134, compared to an unadjusted estimate of -0.70 SD.

Finally, we conduct a placebo test for the size of these effects in our sample population under strong null hypotheses. To investigate whether selective location decisions regarding fault line proximity could produce the adverse developmental results that we estimate due to the earthquake, we conducted a placebo test by simulating the test scores and education earthquake outcomes with respect to each fault line in our data. We run all 50 possible regressions in the true data under the null of "no earthquake" and evaluate the distribution of coefficients we obtain. Appendix Figure A2 illustrates the joint distribution of these coefficients, with the coefficients obtained in our regressions on the activated fault plotted for reference. While there is a wide distribution of test score coefficients, they mostly occur with respect to much smaller fault lines; and large positive height coefficients appear *in combination* with large positive test score coefficients in only one (smaller) fault other than the activated Balakot-Bagh fault. The observed combination of coefficients is at the 98th percentile of the joint distribution. Appendix Figure A3, for reference, compares the magnitude of our height effect on a range of effects from the literature.

A3. Maternal Education instrument and channels

Table A3a illustrates the first stage of our IV regression and uses alternative specifications and placebo tests to check for robustness. The first column reports the specification directly, showing that the availability of a girls' school in the mother's birth village increased her likelihood of completing primary school by 12.5 percentage points. Restricting that sample to those mothers who ever received a school or including geographical controls, as we do in Column 2 and 3, does not affect this result. Using the presence of a boys' school as a placebo, we find a null effect for an identical regression (Column 4); similarly, including indicators for the availability of girls' schools at slightly later ages shows no effect on the likelihood of maternal education (Column 5). Column 6 shows the (much weaker) first stage for the specification including maternal birth village fixed effects.

Figure A4 visualizes the results of our mitigation OLS regression for reference. Figure A4 illustrates the results of a robustness check in which we systematically exclude each cluster (village) from the mitigation IV regression on test scores; even the most extreme result remains statistically significant at conventional levels. There, we re-run the maternal-education interaction IV regression, systematically excluding each one of our 124 clusters (villages) from the full IV regression. Our results are robust to this procedure, plotting the distribution of the 124 mitigation coefficients obtained this way. The iteration closest to the null result, which drops a village containing 8 mothers of 12 children, gives a coefficient of -0.099 with a p-value of 0.005.

Excluding the two clusters closest to the null (two villages with 19 mothers of 33 children) gives a coefficient of -0.083 with a p-value of 0.010.

Table A3b examines differences between children of educated mothers and children whose mothers did not complete primary school. In many characteristics, OLS differences are significant: the children are more likely to have completed primary school, have a similarly educated father; be enrolled in school and in private school; and they are richer and closer to public infrastructure. Only the father's education remains significant in an IV specification; however, as in our other IV results, the precision of all the estimates is low.

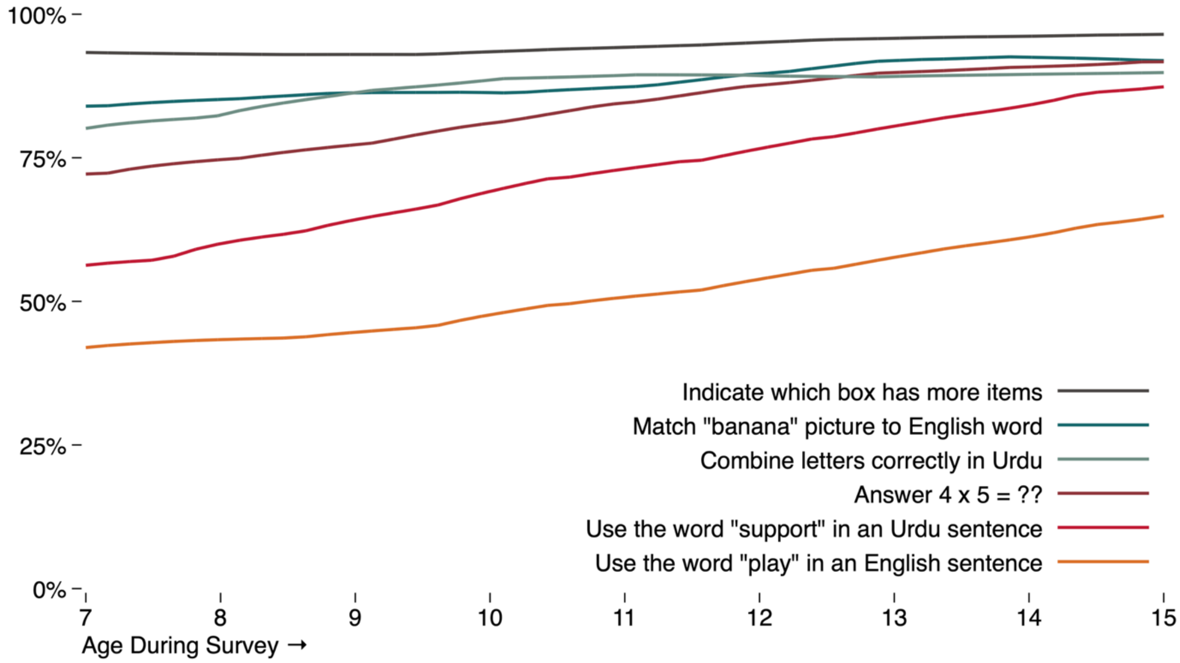
Table A3c presents reduced-form and IV estimates of the effect of maternal access to education and of actual schooling, including a full set of dummy controls for the maternal village of birth. We present results with and without clustering for the current village. Table A3d restricts the maternal mitigation regressions to the sample of children for which there is only one school available in the village, to investigate school-switching as a channel for educated mothers to have an effect; in this sample (where no other school could be chosen), the estimated effect of maternal education is even larger than in the full sample. However, the small sample size substantially increases the variance of the IV estimator and this result is no longer significant.

Table A3e investigates alternative potential mitigating characteristics for both height and test scores. Columns 1 through 3 show non-significant and non-causal mitigation results for the test scores results for all three potential mitigating factors. Columns 4 though 6 show significant but non-causal mitigation for maternal mental health and household elevation, and a reversed coefficient for the assets specification. For all these potential interactions, we lack sufficient evidence to make any claims about the (non)existence or causality of the effects, and we believe they are all of interest for future investigation.

Figure A1: Makeshift shelters after the Pakistan Earthquake

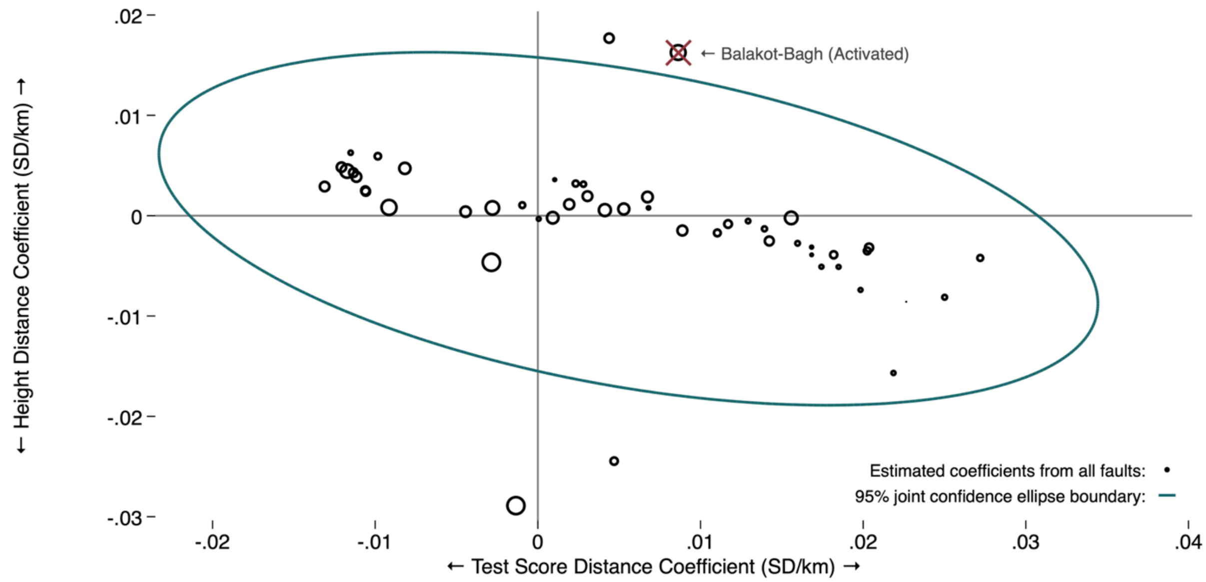
Notes: Photos by Das, taken in December 2005 in the Neelum Valley. Shelters after the earthquake reflect differences across households, ranging from a small lean-to to more extensive tents. Note that even when houses were not fully destroyed, people chose not to live in them because of the large number of after-shocks that continued.

Figure A2: Demonstrative test questions from educational attainment



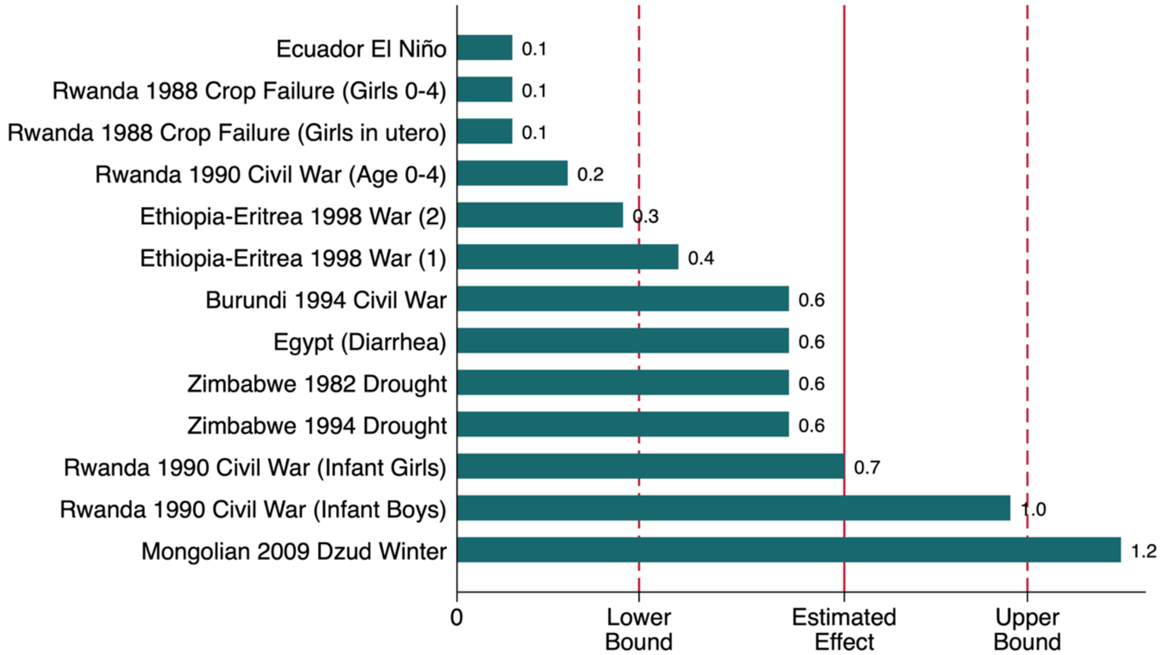
Notes: This figure illustrates results for some demonstrative questions from the knowledge exam in English, math, and Urdu administered to children in the detailed survey sample from ages 7-15 at the time of the survey. Ages at the time of the earthquake are 4 years younger.

Figure A3. Placebo fault lines simulation for primary earthquake effects



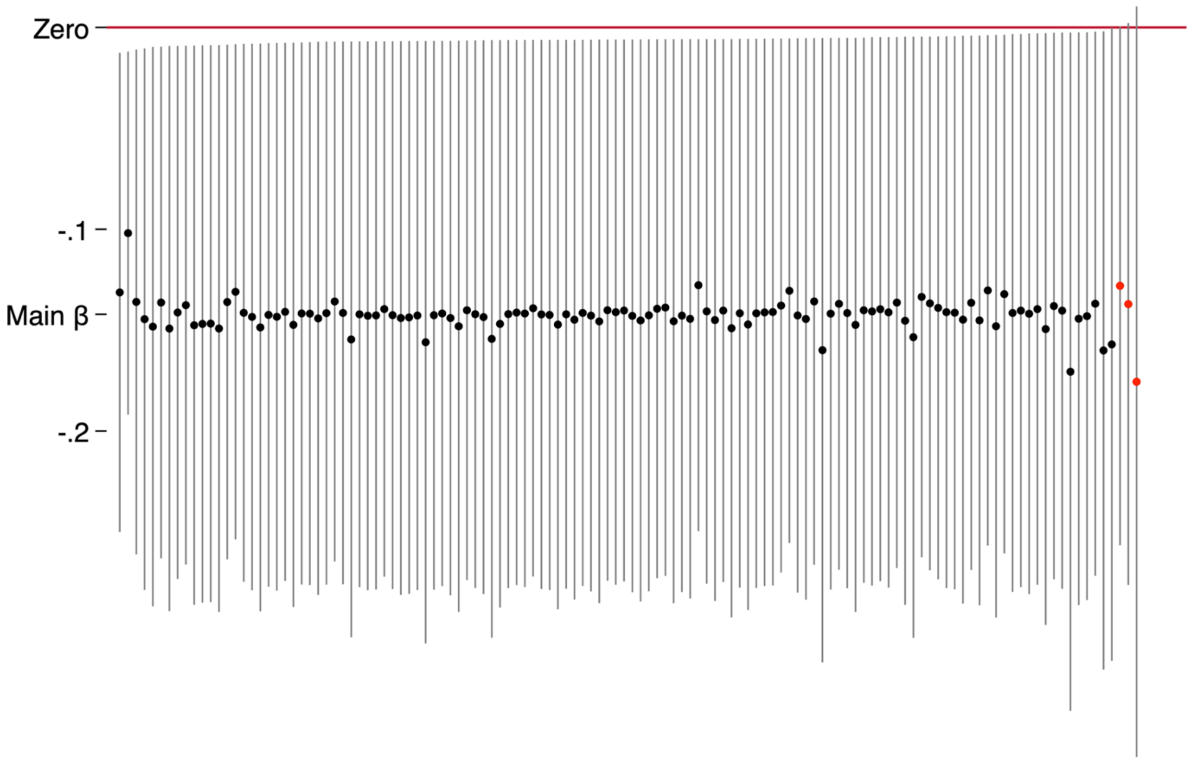
Notes: This figure illustrates the joint distribution of placebo distance coefficients for non-activated faults, for both test scores and for child heights (in utero – age 2). The estimated counterfactuals are obtained by calculating the distance from each household to the other 50 non-activated fault segments in the study area, then running the primary effects regression with that distance for each such fault included. The size of the marker indicates the relative size (length in km) of the fault; the Balakot-Bagh Fault marker (highlighted with X) represents the activated fault and the corresponding effect estimates from our main specifications. The ellipse indicates the 95% confidence area for the joint distribution of effect estimates from the non-activated faults.

Figure A4. Height-for-age Z-score effect sizes from relevant literature



Notes: This figure illustrates the height-for-age Z-score impacts estimated in other studies of disaster impacts on the very young. For reference, it indicates the estimated effect in our pooled results for children *in utero* to age 2 at the time of the earthquake and the corresponding bounds on that effect size accounting for selection.

Figure A5. Instrumental variables leave-one-out regression results



Notes: For each cluster (village) in our test scores maternal education mitigation IV specification, we repeat the estimation excluding that village. This figure plots the distribution of mitigation coefficients we obtain from this set of regressions. The dots represent the individual point estimates; the lines illustrate the corresponding 95% confidence intervals. Estimates are ordered by the magnitude of the upper bound of the confidence interval; those whose confidence intervals include zero are marked in red.

Table A1a. Test completion for eligible children, by age at time of survey

	Eilgible for Testing	Tested and Matched to Maternal Data	Tested but Missing Mother	Temporarily Away	No Longer in Household	Disabled	Working	No Reason Given
Age 7	258	212	7	16	2	2	0	19
Age 8	369	300	9	33	2	4	0	21
Age 9	315	261	3	24	6	3	1	17
Age 10	434	357	12	36	9	5	0	15
Age 11	223	195	4	11	1	0	0	12
Age 12	438	357	13	36	8	5	3	16
Age 13	281	232	6	27	4	2	1	9
Age 14	351	276	9	34	7	2	1	22
Age 15	258	196	3	28	4	1	3	23
Total	2,927	2,386	66	245	43	24	9	154

Notes: Ages at time of earthquake are four years younger.

Table A1b. Measurement completion for eligible children, by age at time of survey

	Eligible for Measurement	Measured and Matched to Maternal Data	Measured but Missing Mother	Mean Height (cm)	Mean Height-for-Age (Z-score)	Mean Weight (kg)	Mean Weight-for-Age (Z-score)
Age 3	404	380	2	87.28	-2.24	15.62	0.57
Age 4	440	403	10	95.84	-1.47	16.86	0.04
Age 5	278	253	5	103.14	-1.20	17.62	-0.62
Age 6	425	388	10	107.39	-1.63	21.16	-0.11
Age 7	258	239	7	111.24	-2.07	22.32	-0.54
Age 8	369	325	9	115.98	-2.19	23.69	-0.89
Age 9	315	283	3	118.41	-2.57	24.92	-1.26
Age 10	434	378	12	118.57	-3.17	26.61	-1.53
Age 11	223	208	4	128.67	-2.18	30.55	-1.27
Age 12	438	381	13	130.45	-2.68	32.13	-1.63
Age 13	281	247	6	130.72	-3.51	34.59	-1.87
Age 14	351	301	9	151.63	-1.41	37.50	-1.97
Age 15	258	216	5	153.75	-1.61	39.34	-2.40
Total	4,474	4,002	95	117.46	-2.16	25.63	-0.95

Notes: Ages at time of earthquake are four years younger.

Table A1c. Comparison of tested children to eligible children

	Full Sample	Tested	T-Test Difference
Male	0.52	0.51	-0.00
	0.01	*0.01*	*0.01*
Age	10.95	10.92	-0.04
	0.05	*0.05*	*0.07*
Height (cm)	128.12	128.23	0.11
	0.35	*0.36*	*0.50*
Weight (kg)	29.88	29.89	0.01
	0.17	*0.17*	*0.24*
Household Asset Index	0.01	0.05	0.04
	0.02	*0.02*	*0.03*
Completed Primary School	0.33	0.34	0.01
	0.01	*0.01*	*0.01*
Father Completed Primary School	0.54	0.56	0.02
	0.01	*0.01*	*0.01*
Mother Completed Primary School	0.18	0.18	0.01
	0.01	*0.01*	*0.01*
Mother's Age	39.48	39.45	-0.03
	0.15	*0.16*	*0.22*
Enrolled (Earthquake)	0.87	0.90	0.03***
	0.01	*0.01*	*0.01*
Enrolled (Survey)	0.90	0.94	0.04***
	0.01	*0.01*	*0.01*
Private School (Earthquake)	0.19	0.20	0.00
	0.01	*0.01*	*0.01*
Private School (Survey)	0.22	0.22	0.00
	0.01	*0.01*	*0.01*
Number of Observations	2,844	2,386	.

Notes: Differences are controlled for distance to the earthquake epicenter, local slope, district fixed effects, gender, household assets, and individual age indicators.

Table A1d. Comparison of measured children to eligible children

	Full Sample Mean	Measured Mean	T-Test Difference
Male	0.51	0.51	-0.01
	0.01	*0.01*	*0.01*
Age	8.70	8.61	-0.09
	0.06	*0.06*	*0.08*
Household Asset Index	0.01	0.02	0.01
	0.01	*0.02*	*0.02*
Completed Primary School	0.21	0.21	-0.00
	0.01	*0.01*	*0.01*
Father Completed Primary School	0.57	0.58	0.00
	0.01	*0.01*	*0.01*
Mother Completed Primary School	0.22	0.23	0.00
	0.01	*0.01*	*0.01*
Mother's Age	37.42	37.32	-0.10
	0.13	*0.13*	*0.18*
Enrolled (Earthquake)	0.87	0.88	0.01
	0.01	*0.01*	*0.01*
Enrolled (Survey)	0.90	0.91	0.01
	0.01	*0.01*	*0.01*
Private School (Earthquake)	0.19	0.20	0.00
	0.01	*0.01*	*0.01*
Private School (Survey)	0.22	0.21	-0.00
	0.01	*0.01*	*0.01*
Number of Observations	4,359	4,002	.

Notes: Differences are controlled for distance to the earthquake epicenter, local slope, district fixed effects, gender, household assets, and individual age indicators.

Table A2a. Death and migration after the earthquake regression results

	(1) Earthquake Mortality	(2) Later Mortality	(3) Adult Out Migration	(4) Adult In Migration	(5) Child Out Migration	(6) Child In Migration
Distance from Faultline (km)	-0.001**	0.000	-0.000	0.000	0.000	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Male	0.001	0.006***	-0.011**	-0.038***	-0.001	-0.008
	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)	(0.005)
Asset Index (PCA) (Pre-Quake)	-0.000	-0.001	0.004	0.002	-0.002	-0.003
	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)
Distance * Assets	-0.000	-0.000				
	(0.000)	(0.000)				
Age	-0.001***	-0.001***				
	(0.000)	(0.000)				
Age Squared	0.000***	0.000***				
	(0.000)	(0.000)				
Distance to Epicenter (km)	0.000	0.000	-0.001**	-0.001**	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean Slope of UC	0.000	-0.000**	0.001	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
District - Abbottabad	-0.012*	0.007**	0.021*	-0.007	-0.008	-0.016
	(0.007)	(0.003)	(0.011)	(0.008)	(0.006)	(0.013)
District - Bagh	-0.016	0.004	0.021*	0.016	0.007	0.000
	(0.016)	(0.006)	(0.012)	(0.010)	(0.006)	(0.011)
District - Mansehra	-0.014	-0.003	0.034**	-0.005	0.000	0.009
	(0.009)	(0.003)	(0.014)	(0.006)	(0.004)	(0.010)
Constant	0.038***	0.013**	-0.026	0.061***	0.012	-0.017
	(0.010)	(0.006)	(0.026)	(0.017)	(0.016)	(0.018)
Numbar of Observations	14,529	14,314	8,152	8,152	4,474	4,474
Age Dummies			X	X	X	X
Regression R2	0.015	0.034	0.033	0.048	0.009	0.207
Dependent Variable Mean	0.015	0.010	0.035	0.028	0.010	0.030

Notes: Regressions are controlled for distance to the earthquake epicenter, local slope, district fixed effects, and distance to the nearest fault line. Migration regressions in columns 3-6 include all available ages as dummy variables.

Table A2b. Aid to households regression results

	(1) Any Aid	(2) Cash Aid	(3) Medical Aid	(4) Shelter Aid	(5) Food Aid	(6) Supplies Aid	(7) Organizational Aid	(8) Cash Aid Total (PKR)
Distance to Fault (km)	-0.01***	-0.01***	-0.00**	-0.01***	-0.00***	-0.01***	-0.00	-1,480.51***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(235.89)
Female Head of HH	0.03*	0.04	0.04	-0.00	0.01	0.02	-0.00	-1,582.26
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(6,166.07)
Female 19-35 Completed Primary School	-0.00	-0.02**	0.01	0.00	-0.00	0.01	-0.00	606.08
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(1,804.82)
Male 19-35 Completed Primary School	-0.01	-0.01	-0.01	-0.03*	-0.05**	-0.00	0.01	-2,442.55
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(3,833.33)
Family Size	0.01**	0.00	-0.00	0.01**	0.00	0.01***	-0.00	11,475.37***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(1,182.18)
Asset Index (PCA) (Pre-Quake)	0.00	-0.02	0.02**	0.00	0.02*	0.02	-0.00	1,705.65
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(2,093.00)
House Destroyed in Quake?	0.13***	0.06*	0.10***	0.14***	0.13***	0.07**	0.03**	41,346.94***
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.01)	(5,475.37)
Eligible for death compensation?	-0.01	0.05	-0.01	-0.04	0.00	-0.03	0.01	51,119.47***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(10,830.81)
Eligible for housing compensation?	0.09***	0.08***	0.04**	0.05**	0.05**	0.02	-0.02	17,532.09***
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(5,465.65)
Eligible for injury compensation?	0.01	0.01	0.02	0.06*	-0.01	0.00	-0.01	-21,839.38**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(8,407.99)
Eligible for lcgs compensation?	-0.03*	-0.01	-0.05***	-0.05***	-0.04***	-0.00	-0.03**	-7,502.60***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(2,491.77)
N children under 6 at EQ	0.01**	0.01	0.01*	-0.00	0.01	0.00	-0.00	13,001.88***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(1,589.55)
Number of observations	2,455	2,455	2,455	2,455	2,455	2,455	2,455	2,455
Regression R2	0.600	0.307	0.146	0.259	0.333	0.223	0.066	0.484
Dependent Variable Mean	0.67	0.47	0.18	0.36	0.44	0.33	0.08	8,128.99

Notes: Regressions are controlled for distance to the earthquake epicenter, local slope, district fixed effects, and distance to the nearest fault line.

Table A2c. Distance-to-faultline regression results by subject

	(1) Averaged	(2) English	(3) Math	(4) Urdu
Distance from Faultline (km)	0.008**	0.009**	0.006*	0.010**
	(0.004)	*(0.004)*	*(0.003)*	*(0.004)*
Distance to Epicenter (km)	-0.006*	-0.008**	-0.003	-0.008**
	(0.003)	*(0.004)*	*(0.003)*	*(0.004)*
Mean Slope of UC	-0.014**	-0.016**	-0.016**	-0.011
	(0.007)	*(0.007)*	*(0.007)*	*(0.007)*
Male	0.069	0.017	0.099**	0.090*
	(0.043)	*(0.050)*	*(0.047)*	*(0.049)*
(log) Consumption per Capita	0.141***	0.158***	0.126***	0.138**
	(0.045)	*(0.049)*	*(0.045)*	*(0.056)*
Constant	-0.615	-0.679	-0.549	-0.618
	(0.500)	*(0.538)*	*(0.486)*	*(0.636)*
Dependent Variable Mean	0.131	0.098	0.156	0.139
Regression R2	0.099	0.073	0.079	0.085
Number of observations	1,874	1,874	1,874	1,874

Notes: Regressions are controlled for distance to the earthquake epicenter, local slope, district fixed effects, and distance to the nearest fault line.

Table A2d. Selection bounds for earthquake effect

	(1)	(2)	(3)	(4)
	Test Scores Bounds		Height Bounds (In Utero and Ages 0-2)	
	Lower	Upper	Lower	Upper
Near fault (<20km)	-0.220	-0.130	-1.031	-0.329
Standard error	0.060	0.053	0.219	0.219
Z-score	-3.672	-2.447	-4.714	-1.499
P-value	0.000	0.014	0.000	0.134
Lower bound	-0.338	-0.235	-1.460	-0.758
Upper bound	-0.103	-0.026	-0.602	0.101
N observed	1,874		1,423	
N not observed	442		133	
Trimming proportion	2.06%		4.53%	

Note: Results show the upper and lower bounds obtained by following the Lee (2009) procedure for trimming excess observations with adverse assumptions due to selective unavailability in either treatment or control groups.

Table A3a. IV Instrument falsification tests for mothers and first stage F-tests (Dependent variable: Probability of woman having completed primary school)

	(1)	(2)	(3)	(4)	(5)	(6)
	Instrument	Recieved School Sometime	Geographical Controls	Boys' School	Girls' School (Other Ages)	Birth Village FE
Distance from Faultline (km)	0.001	0.000	0.001	0.000	0.000	0.004
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.011)
Girls' school present by age 9	0.125***	0.123***	0.116***		0.136***	0.085
	(0.029)	(0.032)	(0.029)		(0.041)	(0.060)
Boys' school present by age 8				-0.012		
				(0.030)		
Girls' school present at age 10-14					0.033	
					(0.050)	
Girls' school present after age 14					0.013	
					(0.033)	
Constant	0.395	-0.402***	0.535	-0.651	-0.599	-1.022**
	(0.345)	(0.119)	(0.545)	(0.620)	(0.590)	(0.476)
Number of observations	987	835	987	987	987	986
F-statistic for Age 9 School Availability	18.310	15.200	16.120	0.160	10.960	1.990

Note: This table reports the first-stage regression results from our IV specification in Column 1. Column 2 restricts the sample to mothers who eventually received a school in their lifetime; Column 4 includes geographical controls for distance to the earthquake epicenter, local slope, and district fixed effects; Column 4 reports placebo results using the availability of a boys' school at the same age in place of the girls' school; and Column 5 reports placebo results using the availability of girls' schools at ages after the typical enrollment age. Column 6 includes maternal birth village fixed effects as a control.

Table A3b. Child characteristics by maternal primary education status

	(1)	(2)	(3)	(4)	(5)
	Means		Difference		
	No Maternal Primary Education	Maternal Primary Education	OLS	IV	N for IV
Age	11.88	11.59	0.00	-2.59	1,716
	0.05	*0.11*	.	1.72	
Height (cm)	132.33	130.77	-0.50	-16.07	1,716
	0.43	*0.91*	*0.68*	13.35	
Weight (kg)	31.78	31.66	0.32	-8.92	1,716
	0.21	*0.47*	*0.42*	10.35	
Completed Primary School	0.40	0.51	0.12***	0.72	1,716
	0.01	*0.03*	*0.03*	0.52	
Father Completed Primary School	0.49	0.85	0.32***	1.68**	1,658
	0.01	*0.02*	*0.03*	0.83	
Enrolled (Earthquake)	0.86	0.93	0.05**	0.32	1,716
	0.01	*0.01*	*0.02*	0.36	
Enrolled (Survey)	0.89	0.97	0.04**	0.52	1,716
	0.01	*0.01*	*0.02*	0.39	
Private School (Earthquake)	0.16	0.33	0.15***	-0.24	1,502
	0.01	*0.03*	*0.03*	0.73	
Private School (Survey)	0.17	0.27	0.07***	-0.36	1,553
	0.01	*0.03*	*0.02*	0.64	
(log) Consumption per Capita	9.85	10.08	-0.00	-0.94	1,716
	0.02	*0.04*	.	1.06	
Household Asset Index	-0.06	0.64	0.57***	0.64	1,716
	0.02	*0.05*	*0.05*	1.41	
Log Distance to Nearest Gov't School (min)	2.79	2.65	-0.17***	-1.02	1,715
	0.02	*0.04*	*0.04*	1.26	
Log Distance to Nearest Market (min)	3.69	3.35	-0.27***	-0.36	1,715
	0.02	*0.05*	*0.05*	1.78	
Log Distance to Nearest Distr Office (min)	4.84	4.71	-0.06*	0.65	1,708
	0.02	*0.04*	*0.04*	1.10	
Log Distance to Nearest Medical (min)	3.83	3.64	-0.15***	0.10	1,712
	0.02	*0.04*	*0.05*	1.30	
Log Distance to Nearest Private School (min)	3.40	3.12	-0.24***	1.17	1,647
	0.03	*0.05*	*0.06*	1.73	
Number of observations	1,559	315	.		

Notes: This table reports means and estimated differences between children whose mothers completed primary school and those who did not, using both OLS and IV specifications. Controls are included for distance to the epicenter, local slope, the nearest fault line, child gender, household assets, and individual age indicator variables.

Table A3c: Estimated maternal education effects with birth village fixed effects

	(1)	(2)	(3)	(4)
	No Clustering		Current Village Clustering	
	Maternal Education	Maternal Education Interaction	Maternal Education	Maternal Education Interaction
Panel A: Reduced Form				
Distance from Faultline (km)	0.022	0.022	0.022	0.022
	(0.020)	(0.020)	(0.016)	(0.016)
Girls' School Present by Age 8?	0.035	0.162	0.035	0.162
	(0.085)	(0.116)	(0.122)	(0.189)
Instrument * Distance		-0.008		-0.008
		(0.005)		(0.009)
Panel B: Instrumental Variables				
Distance from Faultline (km)	0.020	0.019	0.020	0.019
	(0.019)	(0.020)	(0.015)	(0.018)
Mother Completed Primary School	0.289	1.426	0.289	1.426
	(0.647)	(1.095)	(0.948)	(1.761)
Mother's Education * Distance		-0.057*		-0.057
		(0.033)		(0.067)
Dependent Variable Mean	*0.135*	*0.135*	*0.135*	*0.135*
Birth Village Fixed Effects	X	X	X	X
Geographic Controls	X	X	X	X
Individual and SES Controls	X	X	X	X
Age Dummies	X	X	X	X
Number of Observations	1,716	1,716	1,716	1,716

Notes: This table reproduces our preferred specifications from Table 4b including maternal birth village fixed effects. In Panel A this is done as a reduced form and the reported coefficients are those of the instrument and the instrument interacted with the distance to Faultline; in Panel B these are the same instrumental variables estimates with the additional control added. Controls are included for distance to the epicenter, local slope, the nearest fault line, household assets, child gender, and individual age indicator variables for mother and child.

Table A3d: Estimated maternal education effects for children with no school option

	(1)	(2)	(3)	(4)
	OLS		IV	
	Maternal Education	Interaction	Maternal Education	Interaction
Distance from Faultline (km)	0.006	0.008	0.022	0.083
	(0.007)	(0.007)	(0.016)	(0.294)
Mother Completed Primary School	0.156	0.603***	0.660	42.958
	(0.117)	(0.197)	(1.626)	(192.420)
Interaction		-0.028***		-1.783
		(0.009)		(7.639)
Male	0.040	0.051	0.070	0.354
	(0.082)	(0.080)	(0.083)	(1.563)
(log) Consumption per Capita	0.174**	0.163**	0.136	-2.246
	(0.071)	(0.071)	(0.262)	(12.243)
Number of observations	573	573	535	535
First Stage F-stat	0.035	0.035	0.033	0.033

Notes: This table reproduces our preferred mitigation specifications, restricting the sample to children who do not have another school located in their village that enrolls students of their gender. Controls are included for distance to the epicenter, local slope, the nearest fault line, child gender, household assets, and individual age indicator variables.

Table A3e. Alternative mitigation specifications for earthquake effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Test Scores			Height (In Utero & Ages 0-2)		
	Maternal Mental Health	Household Elevation	Household Assets	Maternal Mental Health	Household Elevation	Household Assets
Distance from Faultline (km)	0.008**	0.012**	0.010**	0.034**	0.047**	0.011
	(0.004)	(0.005)	(0.004)	(0.014)	(0.018)	(0.013)
Above Median Maternal Mental Health, Household Elevation, or Assets	0.055	-0.034	0.289***	0.701**	0.967**	-0.282
	(0.071)	(0.102)	(0.087)	(0.344)	(0.430)	(0.403)
Mitigator * Distance Interaction (km)	-0.003	-0.008	-0.007	-0.047***	-0.041**	0.016
	(0.003)	(0.005)	(0.004)	(0.016)	(0.021)	(0.018)
Mother Completed Primary School	0.279***	0.250***	0.246***	-0.000	0.105	0.076
	(0.056)	(0.054)	(0.054)	(0.240)	(0.230)	(0.237)
Male	0.063	0.076*	0.074*	-0.113	-0.148	-0.147
	(0.044)	(0.042)	(0.043)	(0.187)	(0.166)	(0.167)
(log) Consumption per Capita	0.116**	0.121***	0.107**	0.011	0.072	0.064
	(0.045)	(0.042)	(0.041)	(0.173)	(0.173)	(0.174)
Number of observations	1,704	1,874	1,874	1,224	1,423	1,423
Regression R2	0.109	0.122	0.124	0.042	0.036	0.031
Dependent Variable Mean	0.130	0.131	0.131	-1.632	-1.676	-1.676

Note: This table reproduces our preferred OLS mitigation specifications, in each case replacing maternal primary education with one of three alternative mitigating factors: maternal mental health measured in the upper half of the distribution; household elevation in the upper half of the distribution; and households with assets in the upper half of the distribution. The level effects and interaction terms are reported for each combination. Controls are included for distance to the epicenter, local slope, the nearest fault line, child gender, household assets, and individual age indicator variables.

Table A4. Review of effect sizes in height literature

Study	Methodology	Shock	Height Effects
Akresh, Lucchetti, & Thirumurthy (2012)	Regional exposure comparisons Measurements taken in 2002	Ethiopian war with Eritrea 1998-2000	Children exposed to war aged 0-3: –0.4 SD
Akresh, Verwimp, & Bundervoet (2011)	Comparison to exposed cohorts against those living in rest of country. Measurements taken 2-4 years after shock (1992).	Rwanda 1988-89 crop failure	Boys age 0-4 in period: Null effect Boys born in period: Null effect Girls age 0-4 in period: –0.1 SD Girls born in period: –0.6 SD
Akresh, Verwimp, & Bundervoet (2011)	Comparison to exposed cohorts against those living in rest of country. Measurements taken 2-4 years after shock (1992).	Rwanda 1990-91 civil war	Boys age 0-4 in period: –0.2 SD Boys born in period: –1.0 SD Girls age 0-4 in period: –0.2 SD Girls born in period: –0.7 SD
Alderman, Hoddinott, & Kinsey (2006)	Comparison to other children of same mother (fixed effects). Measurements taken 16-20 years after shock (2000).	Zimbabwe 1970's civil war	–0.035 SD x log(Days of exposure to conflict before 1980)
Alderman, Hoddinott, & Kinsey (2006)	Comparison to other children of same mother (fixed effects). Measurements taken 16-20 years after shock (2000).	Zimbabwe 1982-84 drought	Children aged 12-36 months: –0.6 SD
Banerjee et al (2007)	Comparison to birth cohorts born outside phylloxera years. Measurement at 20 years of age (male military service)	France phylloxera (1850s-1870s)	Born in affected year in wine-producing family: –0.5cm
Bundervoet, Verwimp, & Akresh (2009)	Regional exposure comparisons Measurements taken in 1998-99	Burundi civil war (1994-1998)	Children exposed to war aged 0-4: –0.05 SD per month
Dercon & Porter (2014)	Sibling comparisons Measurements taken in 2004 among those aged 17-27	Ethiopian famine of 1984	Children aged 12-36 months: –5cm Children in utero: Null effect
Groppo & Kraehnert (2016)	Comparison across exposure intensity Measurements taken in 2013-13 and 2013-14	Mongolian dzud winter of 2009-2010	Children exposed in utero: –1.2 SD Children exposed age 0-6: Null effect
Hoddinott & Kinsey (2001)	Comparison to children born prior to drought. Measurements taken 4 years after shock (1999).	Zimbabwe 1994-95 drought	Children aged 12-24 months: –0.6 SD Children aged 24-60 months: Null effect
Maccini & Yang (2009)	Comparison of local birth-year rainfall between 1953 and 1974 Measurements taken for adults in 2000	Indonesian rainfall variation in first year of life (per –20%/1SD below average rainfall)	Girls age 0-12 months: –0.6cm Girls age 12+ months: Null effect Boys age 0-12 months: Null effect Boys age 12+ months: Null effect
Rosales (2014)	Cohort comparisons Measurements taken in 2003-04 and 2005-06	Ecuador exposure to El Nino	Children in utero: –0.1 SD
Weldeegzie (2017)	Difference-in-difference (regional and cohort) Measurements taken in 2001-2009	Ethiopian war with Eritrea 1998-2000	Children exposed to war aged 0-6: –0.3 SD
Wierzba et al (2001)	22-month panel of children aged 0-3	Egypt diarrhea	Episode in last 90 days: –0.6 SD Eipsode in prior 90 days: null effect