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

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Abstract

We trace the effects of a large-scale disaster on children in a poor agrarian economy with low capital stocks. Using a new dataset from a survey conducted four years after the devastating Kashmir earthquake of 2005, we document three causal results. First, public infrastructure, household consumption and adult weight recovered to parity between areas close to and far from the fault line. Second, infants in the critical first thousand days at the time of the earthquake accumulated large stature deficits in terms of height for age, with the youngest the most affected. Children aged 3 through 15 at the time of the earthquake did not suffer growth shortfalls, but they now score significantly worse on academic tests. Third, children whose mothers completed primary education were fully protected against the emergence of a test score gap. As the children most affected were those who lived with uneducated mothers, these differential dynamics contribute to the persistence of socioeconomic inequalities. Even if capital stocks fully recover, the divergence in human capital acquisition may be key to understanding the income paths of generations affected by large shocks. A full census of the sampled villages allows us to estimate that in the most affected areas, 53% of the households had children subject to the growth shock and 84% of school-age children had uneducated mothers and bore the full brunt of the learning shock.

1. Introduction

The uneducated and the poor have few opportunities and are frequently subject to large shocks from which recovery is difficult. These are two patterns that are emerging from the literature on disasters and on education mobility. (Guha-Sapir et al. 2016, Narayan et al. 2018) Further, if insufficient insurance disrupts the accumulation of human capital, these two patterns may be connected: A burgeoning literature establishes causal links between the wages, schooling, physical and mental health of adults to adverse shocks during childhood, especially in the critical period (usually defined as the first 1000 days of life)[1]. Amid the larger debate on inequality and climate change related shocks, public discussion and concern over these two connected issues is rapidly gaining traction.

Daniel Clarke and Stefan Dercon (2016) have therefore called for extensive ex-ante planning and insurance mechanisms that can, in their words, "make disasters dull". Indeed, we now know from several well-documented cases that when large disasters are followed by substantial aid, household outcomes appear to return to normalcy within four years.[2] Nevertheless, several questions remain. First, even with substantial compensation for the loss of *physical capital*, can human capital accumulation still be interrupted, affecting future achievement and the subsequent transmission of inequality? Second, to what extent can parental socioeconomic status mitigate these shocks? The answers to these questions help guide the design of post-disaster recovery programs and their effective targeting, asking if additional forms of insurance and mitigation programs are required beyond compensation for asset loss to halt the disruption in human capital formation.

This paper uses unusually rich survey data that we collected four years after the 2005 Kashmir earthquake in Pakistan to examine the physical recovery and human capital accumulation of the affected rural population. The Pakistan earthquake was one of the most physically destructive disasters in recorded history, and its 7.6 magnitude on 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 massive human loss: depending on

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[1] See Portner (2009) and Baez et al. (2011) 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 that persists to adulthood (Datar, Liu, Linnemayr, and Stecher [2011, Akresh, Verwimp, and Bundervoet 2011, Alderman, Hoddinott, and Kinsey (2006), Hoddinott and Kinsey (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) 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 (Frankenburg et al. 2013), all show that within 4 years of the disaster, household outcomes are "back to normal". 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 data source, it caused 73,000–79,000 deaths and 69,000–128,000 injuries; in our data, one in five households in the affected area lost a family member during the earthquake. (Encyclopedia Britannica, 2018; Department for International Development and DFID Pakistan, 2018) However, the loss was compensated: Billions of dollars of cash and in-kind aid were provided to the disaster region, and reconstruction efforts continued well into the following years; we will show 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.

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. We further document that external aid intensity was strongly correlated with proximity to the fault line without contaminating spillover effects of aid to unaffected areas. By comparing households and individuals living farther from the activated fault line to those living closer, we estimate the causal effects of the earthquake on a host of household, adult and child outcomes, considering the full effect as a "joint treatment" of disaster and (delayed) relief aid compared with those who received neither. We put (delayed) in parenthesis: Although relief and rehabilitation compensation started immediately after the earthquake, compensation for housing reconstruction took well over a year to implement. Thus 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 document three results. First, 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 terms of access to public infrastructure as measured by geographic distance. This "back to normalcy" result closely mirrors the findings from the U.S., Japan and Indonesia and speaks to a remarkable recovery effort led by the government and international aid organizations. (Deryugina et al. 2018, Sawada and Shimizutani 2008, Frankenburg et al. 2013)

Second, these recovery patterns do not extend to children, where we still uncover large shortfalls in physical and cognitive development. Using heights as an indicator for cumulative childhood shortfalls experienced during the recovery period, 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] (Bossavie et al. 2017)

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[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. Measured as standard deviations (SD), height-for-age z-scores (HAZ) measure a child's position in a normalized

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.

For children who had crossed out of the critical period by the time of the earthquake, there are also no differences in school enrollment. However, at all ages tested, children living 10km from the fault reported test scores that were 0.24 standard deviations lower than a child living 40km away. This gap is equivalent to about 1.3 school grades (the average 15-year old has completed 5.8). 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.

Third, children whose mother completed primary school were largely protected from the earthquake's negative effects on test scores, a result directly attributable to maternal education via an instrumental variables specification. Since children whose mothers were educated *already* enjoyed a substantial academic advantage over their peers of 0.19 standard deviations, the shock served to substantially worsen inequality between these two groups of children. We do not identify any mitigating effect of maternal education for the height impacts across the sample: a result echoing Figlio (2014) who also finds that parental socioeconomic status does not mitigate in-utero biological shocks.[4]

We must acknowledge two caveats to the height results. First, the height results emerge only for children below the age of 3, leaving us with a smaller sample for the IV estimates and therefore lower precision. The phrase "we cannot find evidence for (….)" should be taken literally, although the lack of a mitigation result in our OLS specifications as well is noteworthy. Second, these results on the lack of mitigation for child height do not imply that shocks to child height cannot be mitigated *at all*. We present speculative results showing that the effect of the shock on child height was lower in households with lower maternal stress and higher receipt of aid. However, these are both variables that are collected after the earthquake and do not necessarily support a causal interpretation.

The effects 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. In terms of vulnerability to the educational disruption, uneducated mothers comprised 65% of our sample with 84% of all school-age

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reference distribution of healthy children from the United States. See Shrimpton et al (2001) for a discussion of age-based trends in childhood anthropometrics under varying conditions of deprivation.

[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.

children, who were therefore liable to fall even more behind in cognitive achievement relative to the 16% of children whose mothers had some education.

The causal identification of our findings relies on the exogeneity assumptions that (a) proximity to the fault line was uncorrelated with household and child characteristics prior to the earthquake and (b) our instrument for maternal education is uncorrelated with other maternal characteristics correlated with child test scores. As to the first, as we have documented previously that several key requirements are fully met in our data. Baseline village characteristics from a census carried out in 1998 are uncorrelated with distance to the fault line. The region is crisscrossed with multiple fault lines and there is no model that can predict earthquake timing or location. To rule out selective location decisions based on fault exposure risk, we control all results for the distance to the *nearest* fault line, as well as conducting placebo tests by simulating the activation of 50 other faults present in the region. (All households live within 11.5km of some fault and half live within 2.5km.) We also present evidence that individual migration was not correlated with distance to the fault line at a large magnitude, mitigating selection concerns from this source, and confirm with a bounding exercise that our results are robust to reasonable assumptions about selective non-responsiveness due to migration, mortality, and unavailability.

For the second, we use as an instrument for maternal education level the availability of a girls' schooling option in the birth-village of the mother, at the appropriate age.[5] 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. 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. 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. Finally, Alwyn Young has raised a concern that, in many studies, IV estimates are not stable to the exclusion of even one or two clusters. (Young 2017) We therefore re-estimate our IV specification by dropping each village from the sample one at a time, and we find that the weakest leave-one-out estimate is still statistically significant and similarly sized as our preferred specification.

This is a case study of a disaster in a specific context where substantial aid and resources were made available to affected populations. Within this broader context, our contribution to the literature on shocks, recovery and human capital accumulation lies primarily in the identification of the disaster on household, adult and child outcomes and our ability to causally demonstrate the mitigation of education effects among the children

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[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.

of mothers with some education. Our extensive data allows 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; many studies rely on administrative data and therefore do not have the ability to separately examine these different outcomes. Even among studies that look at the educational effects of disasters on children, the typical focus is on schooling attainment as test score data are rare.[6]

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. In the case of the Tsunami, Elizabeth Frankenburg 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. Therefore, both the likelihood of exposure and mortality selection are correlated with pre-existing characteristics; this is particularly worrisome for estimates of the impact as high exposure to the Tsunami resulted in very high mortality rates, exceeding 30%. In contrast, 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 total mortality never exceeds 5%. In the case of hurricanes, there are similar concerns about non-random exposure; further, hurricanes typically 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 shocked 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, we are able to confirm that validity of the 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. This implies that older cohorts are a viable counterfactual group for those under the age of three. However, in the case of test-scores, children are hit at all ages in a similar way and here the cohort comparisons are no longer valid.

Finally, ours is one of the first studies to causally demonstrate that maternal education mitigates the impact of a shock on cognitive development. Almond and Mazumder (2013) have argued that these types of studies are extremely difficult as they require two sources of exogenous variation, akin to "lightning striking twice in the same place". By combining the exogenous exposure to the shock and the variation in schooling options in the birth-village of the mother, we credibly demonstrate that the (in)ability of households to mitigate shocks plays a key role in the evolution of inequality for these regions.

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[6] 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.

Nevertheless, we must acknowledge several open questions prompted by our findings. Most importantly, our analysis is limited by lack of data before or in the immediate aftermath of the earthquake; therefore, it is difficult for us to unpack parental decision-making at the time of the earthquake and whether the impacts we observe among children reflects poor nutrition, poor housing or significant family and environmental stress during the extended rehabilitation and reconstruction period after the earthquake. This is particularly difficult to identify because household investments and biology will interact and we do not know the timing and lag-structure of these interactions.

We also do not know whether children will recover from these shocks in the future through "catch-up" growth. 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. 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.[7]

The remainder of the paper proceeds 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 primary results. We first establish that earthquake-affected areas have recovered to parity with unaffected areas in standard measures of infrastructure, consumption, wealth, and school enrollment. We show that large physiological and educational development deficits persist in affected children of all ages, with heterogeneity by age in the growth gap. We present instrumental variable analysis that maternal education is a strong causal factor for educational resilience in affected children. 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

On October 8th, 2005, a powerful earthquake that measured 7.6 on the Richter scale struck Northern Pakistan. As a result, an estimated 73,000–79,000 Pakistanis died and 69,000–128,000 were seriously injured and over 2.8 million were left homeless. There were three distinct response phases after the earthquake. Immediately following the earthquake, relief and aid poured in from the rest of Pakistan and around the

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[7] 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 (2012) 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. We have not been able to locate studies that examine whether physical catch-up is accompanied by cognitive recovery as well.

world. In addition to financial support, organizations and individuals provided on-the-ground logistic and technical assistance, ranging from specialized services in medicine, excavation and evacuation to emergency shelters and food. Most of the operations were conducted by the Pakistan army, with critical support from international agencies.[8] 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.24000 over 4 tranches for certain eligible households as well as compensation of Rs.175000 for housing reconstruction. Although most households received Rs.25000 of the full housing grant, as well as injury and death compensation, within a month of the earthquake, the lack of pre-existing data on households in these regions precluded further speedy compensation.[9] The earthquake struck in October and 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). The photos in Figure A3, taken by Andrabi and Das in December 2005, 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. 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 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 from 2009-10 are collected just 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 17.5km from the activated fault line and 36.4km from the epicenter. We began in 2009 by conducting a complete 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

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[8] 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).

[9] Two of the authors (Andrabi and Das) were closely involved with these programs and were in the region frequently for the first year as these programs were initiated.

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 (2015) 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.[10] Further geographical data obtained from USGS and NESPAK 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. 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. 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. 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 indicated in bold. 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.

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 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.

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.[11] Height and weight measurements were normalized for children aged 3 and up according to 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 A1. The mean measured height for these children was 117.5cm and the mean measured weight was 25.6kg. This group is gender-balanced, and, as

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[10] We use the Haversine formula to compute distance, which appropriately adjusts for the earth's curvature.

[11] 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.

shown in A1B, 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; for children aged 7-15, a general knowledge exam containing sections on English, Urdu, and mathematics was also administered in the home to all children regardless of their school enrollment status.[12] Figure A1 illustrates correct response rates for several of the test questions across the tested age range. Test scores were calculated as a normalized distribution within each subject using item response theory (IRT), then averaged across subjects for each child. Each subject receives equal weight in analysis, and the results are broadly similar when repeated within each subject. In the testing performance analysis, we utilize a sample of 1,875 school-age children with data on maternal education from 1,081 households. Test coverage among eligible children with maternal data is 81% and is detailed in Table A1a. Representative sample t-tests are reported in Table A1c. Where we expect and observe differences between the tested and the eligible children – current school enrollment rates in tested children – the difference is in the expected direction at a small magnitude. Importantly, we observe no significant differences in maternal education between children who were tested and those who were eligible: 18% of children reported mothers who had completed primary school in both tested and untested children groups and 73% had mothers who reported not having attended any school at all.

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). 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 explanatory power is coming from a simple binary indicator of primary education for mothers.

To examine the causal impact of maternal education on child achievement, we 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

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[12] Testing children younger than seven was too expensive as it required substantial oral examinations which could not be funded through our survey.

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

As earthquakes are almost entirely unpredictable, the Kashmir earthquake can be utilized as an exogenous shock and the characteristics of affected and unaffected populations can be assumed to conform to a common trends assumption in its absence. This allows us to pinpoint the earthquake shock as the causal factor in our analyses. Critical for this causal interpretation is the lack of correlation between pre-quake characteristics and this distance. We rely on the unpredictability of earthquakes to assert that the shock experienced by the population was exogenous, and 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. If this claim is substantiated, then we can compare post-earthquake outcomes across the shock spectrum to provide an unbiased 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.[13] 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 was the first to strike 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. 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. Additionally, as Figure 1 illustrates, there are multiple potentially active faults in the

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[13] 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)

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 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.[14] 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. Figure 4 shows an illustrative example, indicating ownership rates of various household assets in near-fault and far-from-fault populations: across an index of 24 items, only a few of the less common items suggest systematic differences in ownership rates between near and far populations prior to the earthquake.

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[14] 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.

Despite the strong exogeneity of pre-earthquake characteristics to the distance to the fault line, several concerns remain in terms of (a) alternate specifications of intensity; (b) post-earthquake migration 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 (2015) 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:

> "*The Pakistan earthquake had a large surface rupture area along a sloping fault plane and the epicenter rarely falls along the fault-line as it is a surface marker of a subterranean event along a sloping plane (Kaneda et al. 2008). As the entire plate shifted (with the rupture point at the epicenter), the distance to the fault-line is a better predictor of local intensity than the epicenter: "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 eds. 2002). 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 did not have recourse to pre-earthquake household rosters. To assess this possibility, 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 close to the fault-line in this inclusive method, 192 had moved out after the earthquake (3.8%) 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[15]. As shown in Table A2b, 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 in magnitude difference in child in-migration.[16]

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[15] 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.

[16] 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

Similarly, overall mortality was too low to induce severe selection bias under all but worst-case assumptions. Even within 5km from the activated fault, childhood mortality was just 5%, which could not bias childhood development results unless the most vulnerable were also the tallest and smartest to a large degree. 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 that could confound the interpretation of our results is the presence of aid spillovers, which has been demonstrated in the case of Aceh after the Tsunami. If this were true, differences between affected and unaffected populations could arise from the aid delivered to groups unaffected by the disaster. Several types of cash grants were delivered to the affected region (the November 2005 exchange rate stood at PKR59.7 per USD): Rs.25000 was delivered immediately for every household; Rs.175000 was given for reconstruction of housing over 2 years, in tranches; and Rs.24000 was given starting after 6 months, in tranches, for families with more than four children. Figure 5 shows the total aid received by households declines as we move away from the fault line to near zero at distances beyond 40km. 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 who died during 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

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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.

1% within 15km. In the case of the Tsunami in Aceh, mortality rates were higher among women and higher among the poor. Although we do not have pre-existing characteristics for individuals who died, we did collect their sex and age at time of death and we also have a measure of assets prior to the earthquake based on recall. We find no difference by gender or wealth, and little variation by age other than excess vulnerability of those age 65 and up and the very young. Mortality rates could also have been higher in months following the earthquake. We therefore also collected data on additional deaths *after* the earthquake and find little evidence of excess mortality near the fault, as shown in Figure 3.

The second part of our "joint treatment" was the receipt of aid to households from public funds.[17] As discussed previously, this aid was delivered through three programs. Immediately following the earthquake (within one month) families received Rs.25000 in cash (delivered by the army) as well as compensation for injuries (Rs.25,000-Rs.50,000) and death (Rs.100,000). Public aid then stopped for 6-9 months while households were enumerated and the damage was assessed; during this time, households still received in-kind aid in the form of tin sheets and sporadic food aid on which we do not have data. Following this, a cash transfer and housing compensation program was put in place, and through these, households received a further Rs.24000 (cash transfer) and Rs.150000 housing compensation, the latter 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 PKR 42,800 which is 43% of annual per-capita expenditures among households more than 20km from the fault line (with the average household 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.175000 in 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.

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 and find little evidence of differential aid by pre-existing household and individual characteristics. Of total variation in aid receipts, 45% is within village, and, of interest for our results on mitigation of shocks, we only find a small and negligible coefficient on mothers' and fathers' education in the receipt of aid as well as the amount received.[18]

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[17] 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.

[18] 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

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 distance-to-fault line coefficient 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 due to the "Building Back Better" recovery initiative, with more households reporting a permanent house with electricity and water in the home. Across the shock spectrum there is also no difference in per capita expenditures based on a detailed household consumption survey. In Panel B, we again report null results for access to all types of infrastructure, including schools and health clinics.

In Panel C, we examine adult heights and weights. Adult heights are of special interest in the age range from 18 to 24, as previous work by Angus 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, due to the aid flows included in the net earthquake effect.

IV.3. Children's human capital acquisition

Figures 6a, 6b, 6c, and 6d show, non-parametrically, the difference in child anthropometric and educational outcomes 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 are stark differences in stature for children who were in utero at the time of the earthquake and this gap then diminishes smoothly, with statistical significance disappearing around age 3. 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. [19]

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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.

[19] 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.

Second, there is no evidence of current nutritional deficits measured through weight-for-age with distance from the fault line. We observe a consistent worsening of this condition with age in both groups, but do not observe differential effects or nonlinearities in children of different ages across space, including groups for those in utero, those ages 0-2, and all others separately.

Third, there is no difference in terms of enrollment across the age spectrum; again, if anything, those closer to the fault line were more likely to be enrolled at the time of the survey compared to those farther away. 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.

Table 3 presents the regression equivalent to these figures. 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. 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. 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. In terms of test scores, deficits amount to 0.008 standard deviations per kilometer or 0.24 standard deviations over a 30km range. In two final specifications that includes a full set of age-distance interactions, we find no heterogeneity across age or gender for the education result.

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 Rwandan civil war and the Mongolian dzud winter, suggest long-term height effects of a similar size on the in-utero or newborn cohort.

IV.4. Protective Mothers

We examine the effect of educated mothers on their children through a series of regressions that expand on the basic model. In these regressions, we restrict the tested sample of children to those 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 Table 4a. The level effect of maternal education remains strong and significant for test scores (0.4SD), and there is a large mitigation effect for *test scores* by distance in the sample (87% of the fault line coefficient). We also examine the potential for maternal education to offset height loss, both as a level effect and as an interaction specification. It does not appear that educated mothers provided any protective effect to recovery gaps, nor is there a level effect: all the coefficients involving maternal education are small and none are significant. The height result echoes Figlio (2013), pointing to the inability of family socioeconomic status to affect biological shocks. Appendix Figure A3 shows these results graphically.

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. 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) 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's 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 1000 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.

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 13%. 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. Thus we are satisfied that our first stage is capturing exogenous variation in access to schooling and satisfies the exclusion restriction. Andrabi, Das, and Khwaja (2012) provide further details regarding the instrument falsification and robustness checks.

As a second stage, we then regress:

$$Y_i = \alpha + \beta_1 * \mathit{DistancetoFaultline} + \beta_2 * \mathit{maternaleducation}_i + \beta_3 * \mathit{interaction}_i + \beta_4 * \lambda_i + \beta_5 * \gamma_i + \varepsilon_i$$

Here, $\mathit{maternaleducation}_i$ and $\mathit{interaction}_i$ 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 confirm that the protective effect of maternal education observed in the test score regression is 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, but 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.5. Threats to Identification

Despite the strong case for the exogeneity of the earthquake shock itself, there remain several potential threats to the causal identification of our results due to selective responses or outliers. We assess three 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 or “just so” observations.

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 developmental results, we conducted a placebo test by simulating the test scores and education earthquake outcomes with respect to each fault line in our data. We treat 50 other possible faults as though they were the location of the shock, therefore 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. 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. Taking these results together, we are satisfied that household distribution patterns with respect to the location of faults in general is not by chance or by selection responsible for the patterns we observe in the data relative to the activated fault.

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 A2c reports these results.

Finally, we assess the sensitivity of our IV estimates for maternal mitigation to outliers. Alwyn Young (2017) 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. Figure A4 demonstrates that 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.

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 and developmental lags. Nevertheless, at least in the case of test scores, ancillary evidence can deepen our understanding of what happened.

Figure 7 first shows that children nearest the earthquake spent more time out of school, with those in the immediate vicinity of the fault taking off up to 14 weeks on average. Although not causal, for children within 10km of the fault, those who scored in the bottom half of the score distribution stayed out of school longer than those who scored in the top half. Whether this amount of disruption could explain his result is an open question. One should note that many schools even when they opened after the earthquake, operated in less than ideal conditions and stayed that way for some time. So immediate disruption of schooling is only a small part of the explanation of the educational recovery gap.

Figure 8 then introduces a sample we have not previously used in the mitigation estimation—those under the age of 5 at the time of the earthquake who were subsequently tested in our sample. Given the age restrictions in our testing sample, these are children who were either 3 or 4 years old at the time of the earthquake. Our earlier interaction specification uses only children of school-going age; this figure illustrates the results of regressions for the mitigation specification using both the same age cutoff (age 5+) and school-enrolled children only (a similar, but endogenously selected, sample). Among both these groups, there is again a large difference in test scores between those living close to and far from the quake and between children of educated and uneducated mothers, but now there is no maternal mitigation effect for either. This is surprising since the literature on test scores typically shows building advantage for children of educated parents till they reach school followed by a subsequent stabilization of the gap. Therefore, our data suggest that maternal education effect was salient only for those who were school-going age at the time of the quake, and were thus presumably enrolled, but our data are insufficient to confirm that the primary mitigation channel was children who were in fact enrolled in school.

As a further investigation, in Table A3c 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 is weak. Taken together, all these results point in the direction of educated mothers being better able to handle school disruptions and compensate for a decline in schooling inputs. This mirrors previous work demonstrating a high elasticity of substitution between home and school inputs. (Das et al. 2013)

In Table A3d, 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. Janet Currie and co-authors as well as Diane Lauderdale have demonstrated the nuanced role of maternal stress on child outcomes in the United States. (Currie and Rossin-Slater 2013, Lauderdale 2006) 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. 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.

VI. Conclusion

In a setting of recovery to parity on typical dimensions of infrastructure, consumption, and school enrollment, our results show that development gaps persist between children affected and unaffected by the earthquake. Children affected by the earthquake underperform on tests four years later by a significant degree, and affected children under the age of 3 at the time of the earthquake area exhibit physical growth gaps which are known to be indicative of permanent cognitive underdevelopment. In our preferred regression specifications, both these gaps are in line with the largest effects noted in prior literature.

The intense devastation affected virtually the entire population of children in the most affected area. The census of households in our sample villages shows that 55% 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. In terms of vulnerability to the educational disruption, uneducated mothers comprised 75% of our sample with 82% of all school-age children (and old enough not to suffer the height shock), who were therefore liable to fall even more behind in cognitive achievement relative to the 18% of children whose mothers had some education. This large scale of losses among the most vulnerable at an age when human capital accumulation is important gives us some pause as to the lessons that can be drawn from the recovery to parity in other episodes such as the Kobe earthquake where parental education levels are uniformly high.

Nevertheless, there is variation in the ability of households to protect against these shocks. Mothers who have completed their primary school education provide both a universal gain in test scores and an additional protective effect that mitigates 87% of the earthquake proximity effect. While the channels by which this effect operates are uncertain, educated mothers tend to have more educated husbands, which may secure positional advantages for their children without producing a greater overall level of human capital. A better understanding of the production function for childhood learning will be essential to preventing these deficits after future disasters, as some of these mitigating characteristics will directly improve the production of human capital but others may only influence its distribution.

Additional research into the causes and consequences of educational disruption will also better illustrate the mechanisms through which these recovery gaps emerge. Higher-performing children missed less school after the earthquake, and the size of the gap in disruption time between them and lower-performing children increased with the local magnitude of the shock. Maternal-education effects were substantially weaker when children were younger and therefore not enrolled in school, indicating an important substitutability between these production factors, but maternal education alone seems to play little role in returning children to school after disaster. 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; Victoria Baranov and co-authors (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. In a standard macro story of recovery, there is reliance on Solow type diminishing returns to capital that result in an inflow of capital leading to catch up growth. In our case, while private capital inflows in the mountainous region have been sparse, government and international aid flows came in very quickly to fill the gap. How this large setback to human capital accumulation (albeit in a narrow localized area of 20km around the fault line) affects the long term prospects of the region and evolution of inequality remains to be seen.

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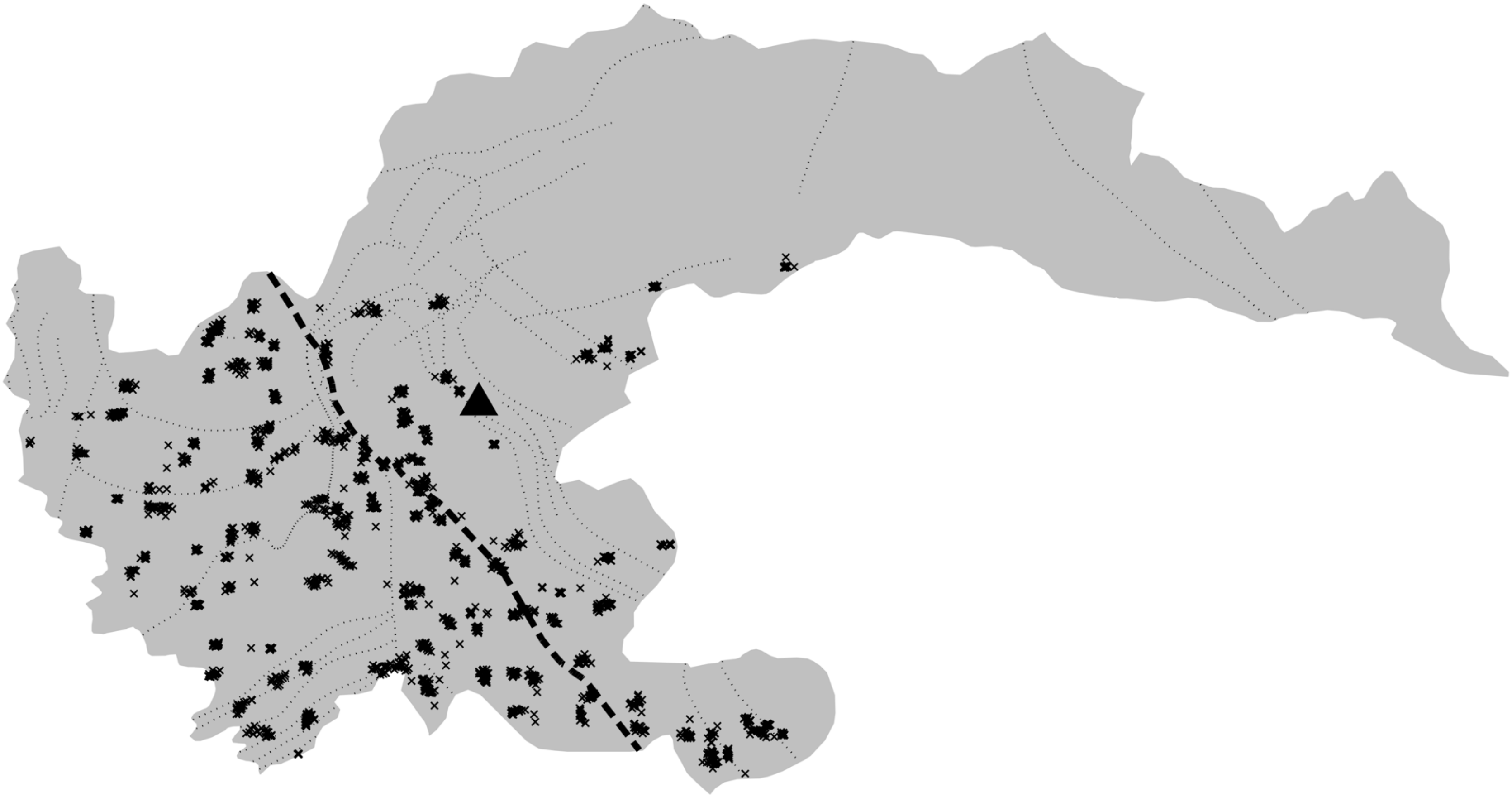
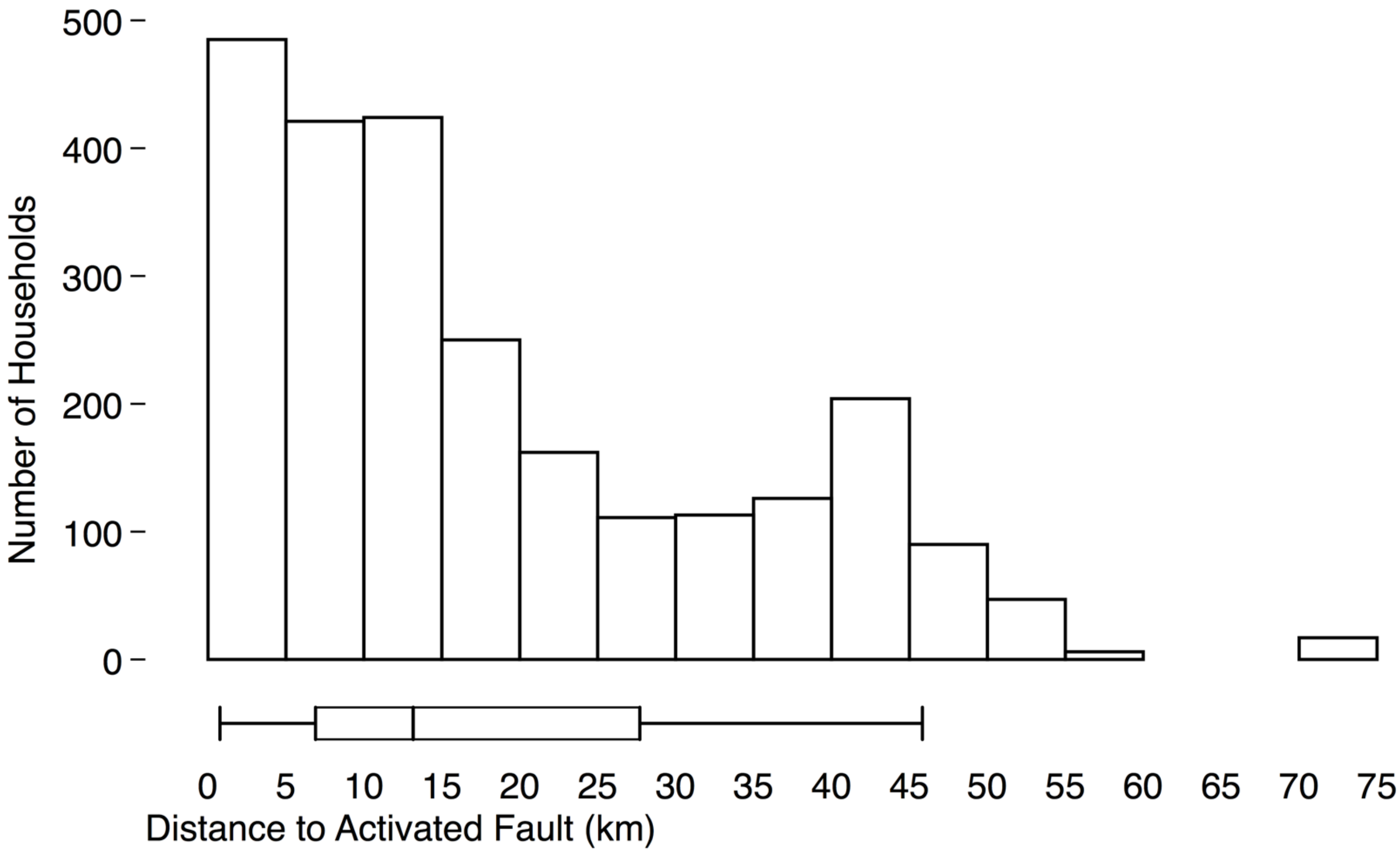
Figure 1: Map of study area, surveyed households, activated fault and epicenter, and non-activated faults



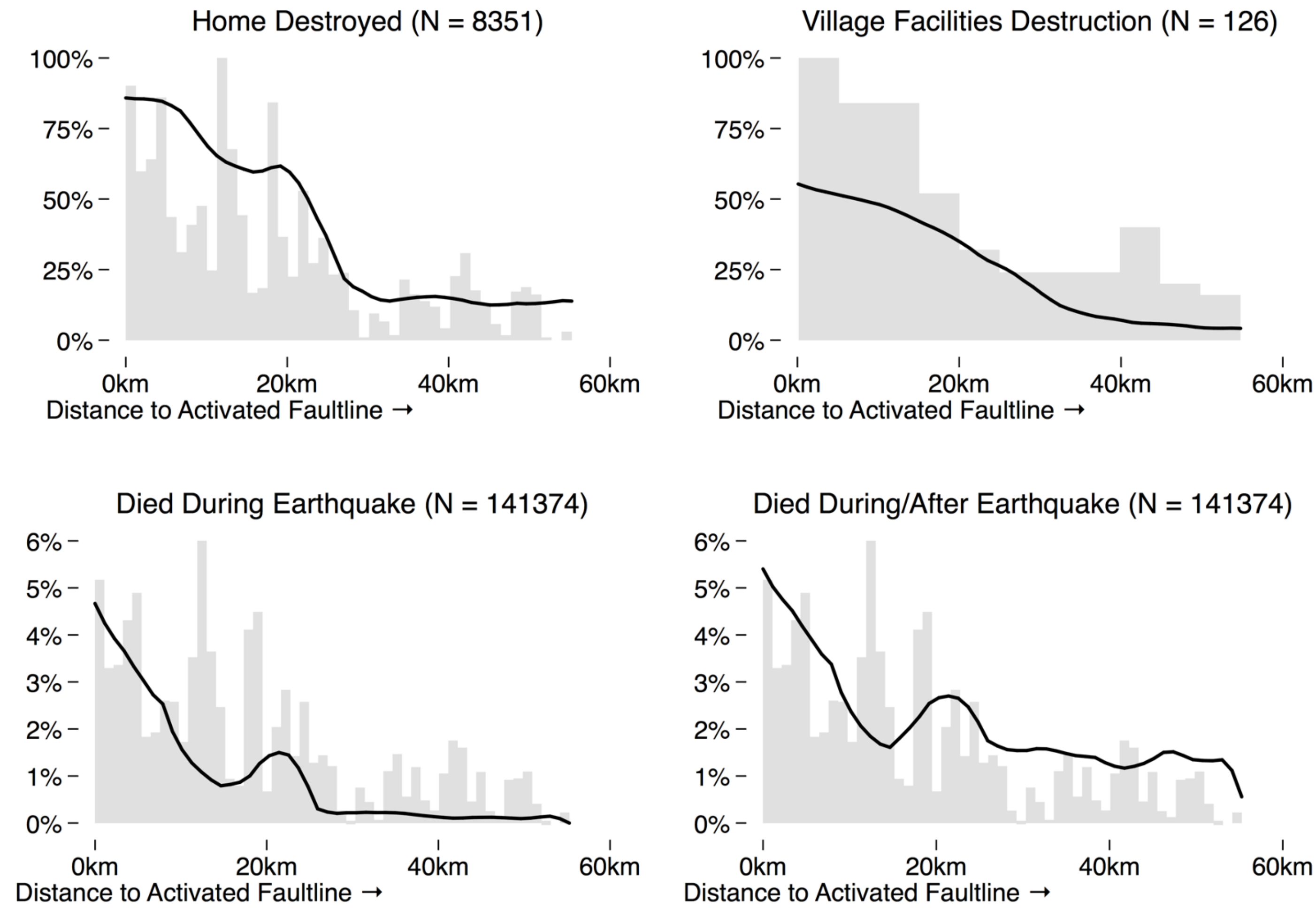
Notes: This map illustrates the location of all 2,456 households that completed the detailed household survey, relative to the activated Balakot-Bagh Fault (dashed line) and the earthquake epicenter (triangle). Fault lines which were not activated in the earthquake are shown as dotted lines.

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



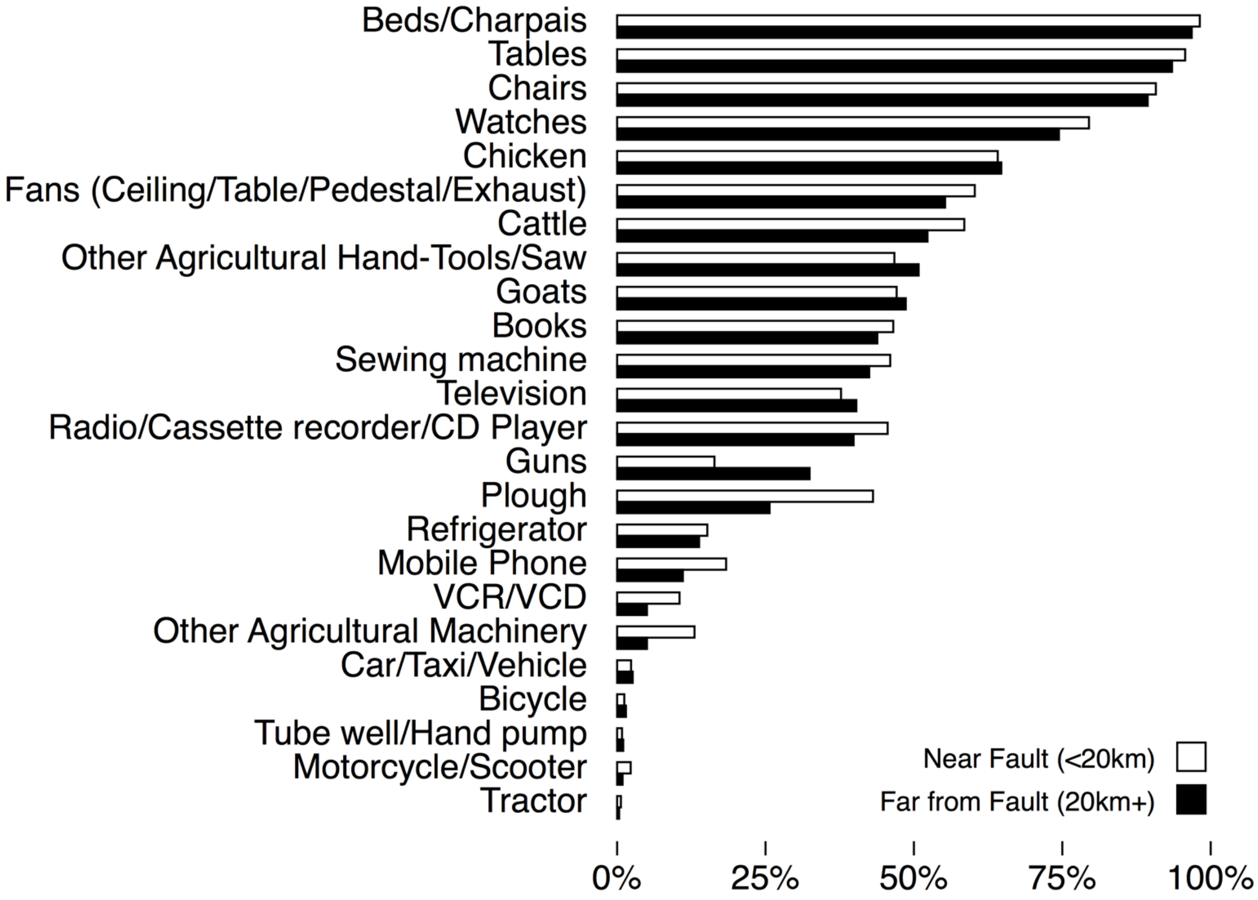
Notes: This figure illustrates the distance distribution of the 2,456 households from the detailed survey to the activated fault line, as a number of households in each 5km bin as well as the 5th, 25th, 50th, 75th, and 95th percentiles of the distribution (box plot).

Figure 3. Immediate and extended earthquake deaths and destruction



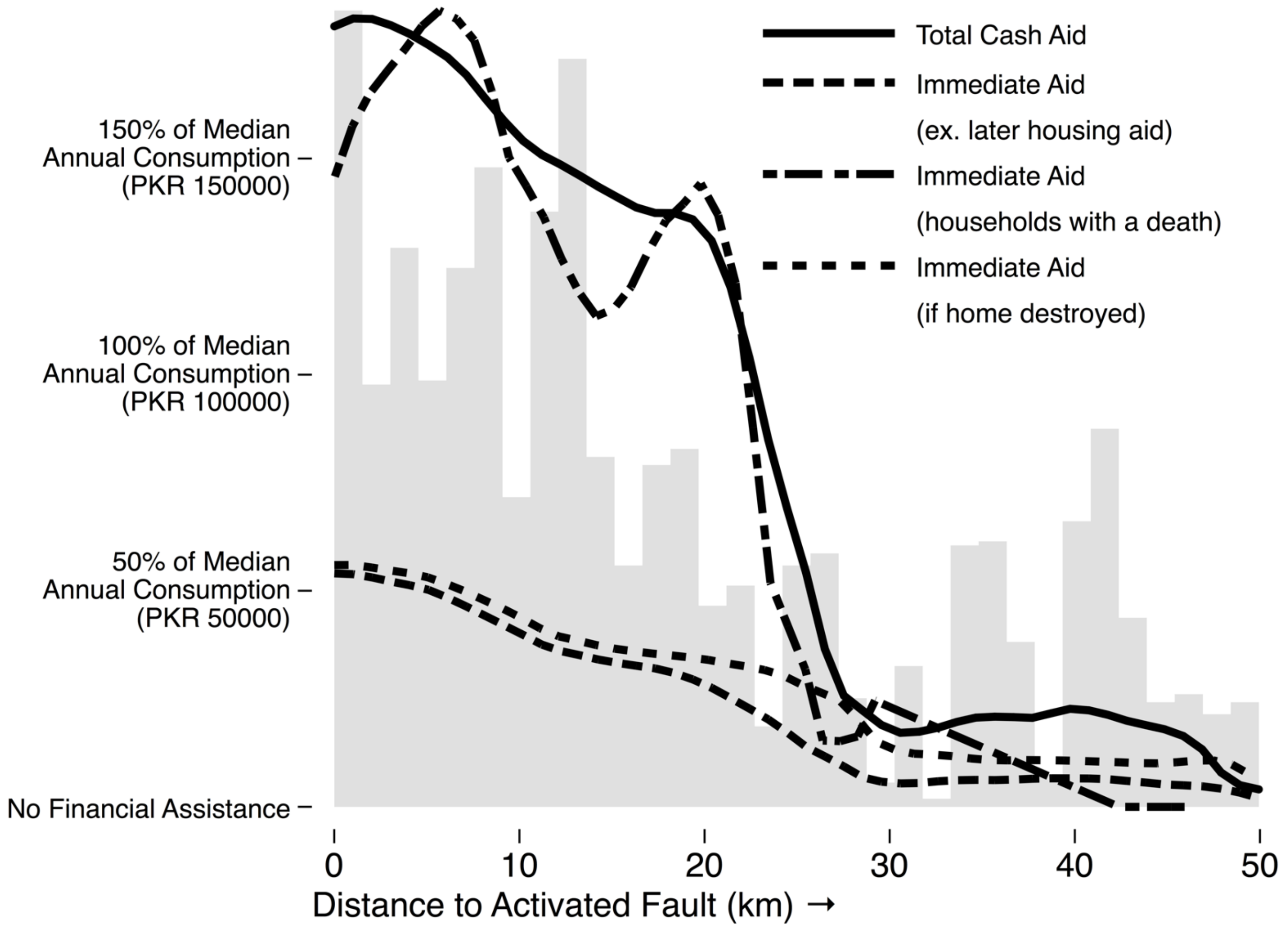
Notes: These plots illustrate the proportion of homes reported destroyed in both long census and detailed survey measures; the proportion of public infrastructure noted destroyed in village survey; and the proportion of census records reported deceased during and/or after the earthquake, as a function of distance to the activated fault. Histograms show relative density.

Figure 4. Pre-earthquake assets comparison



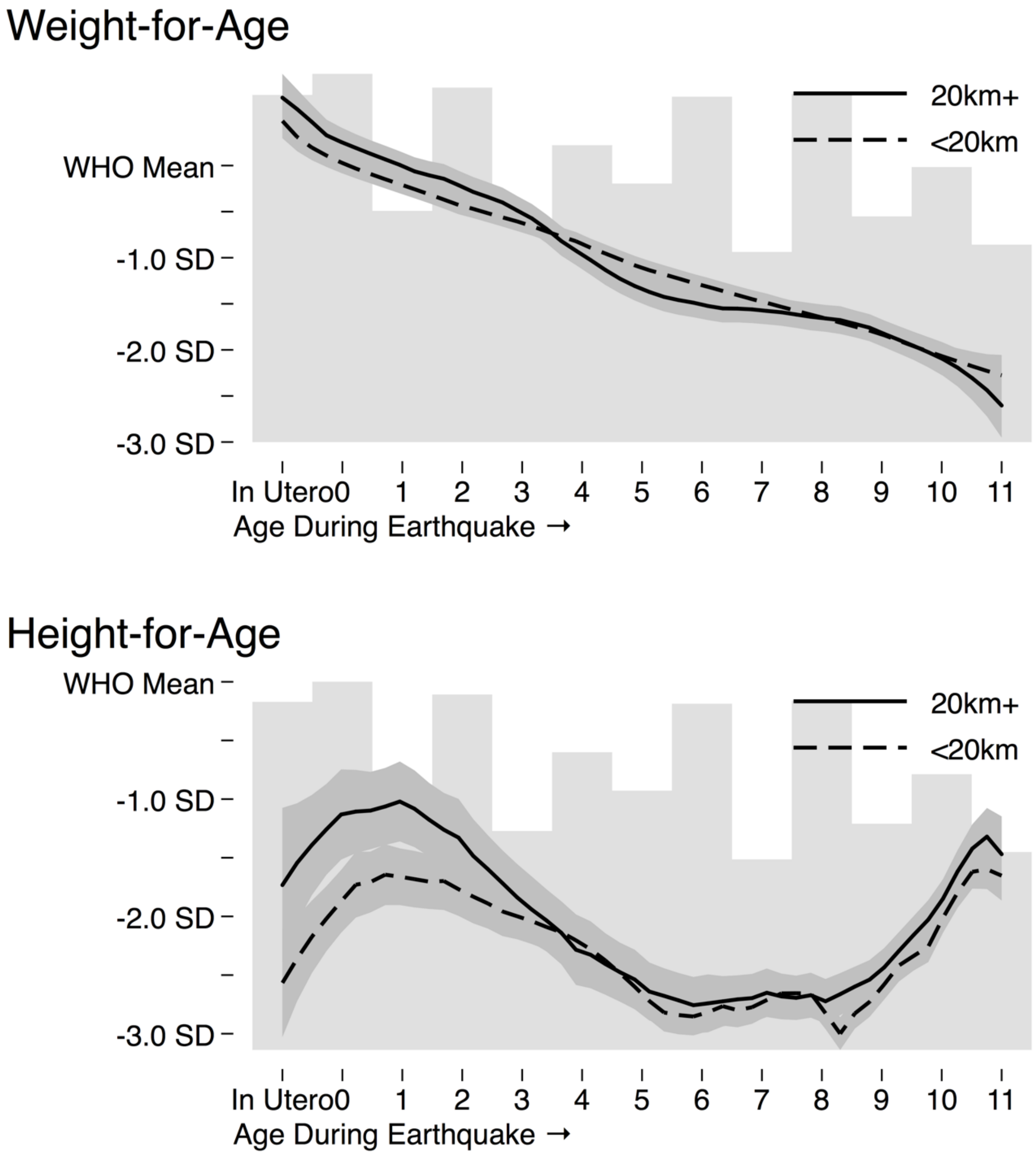
Notes: Self-reported ownership rates (prior to the earthquake) of the assets in our wealth index are compared between near-fault and far-from fault households in this figure.

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



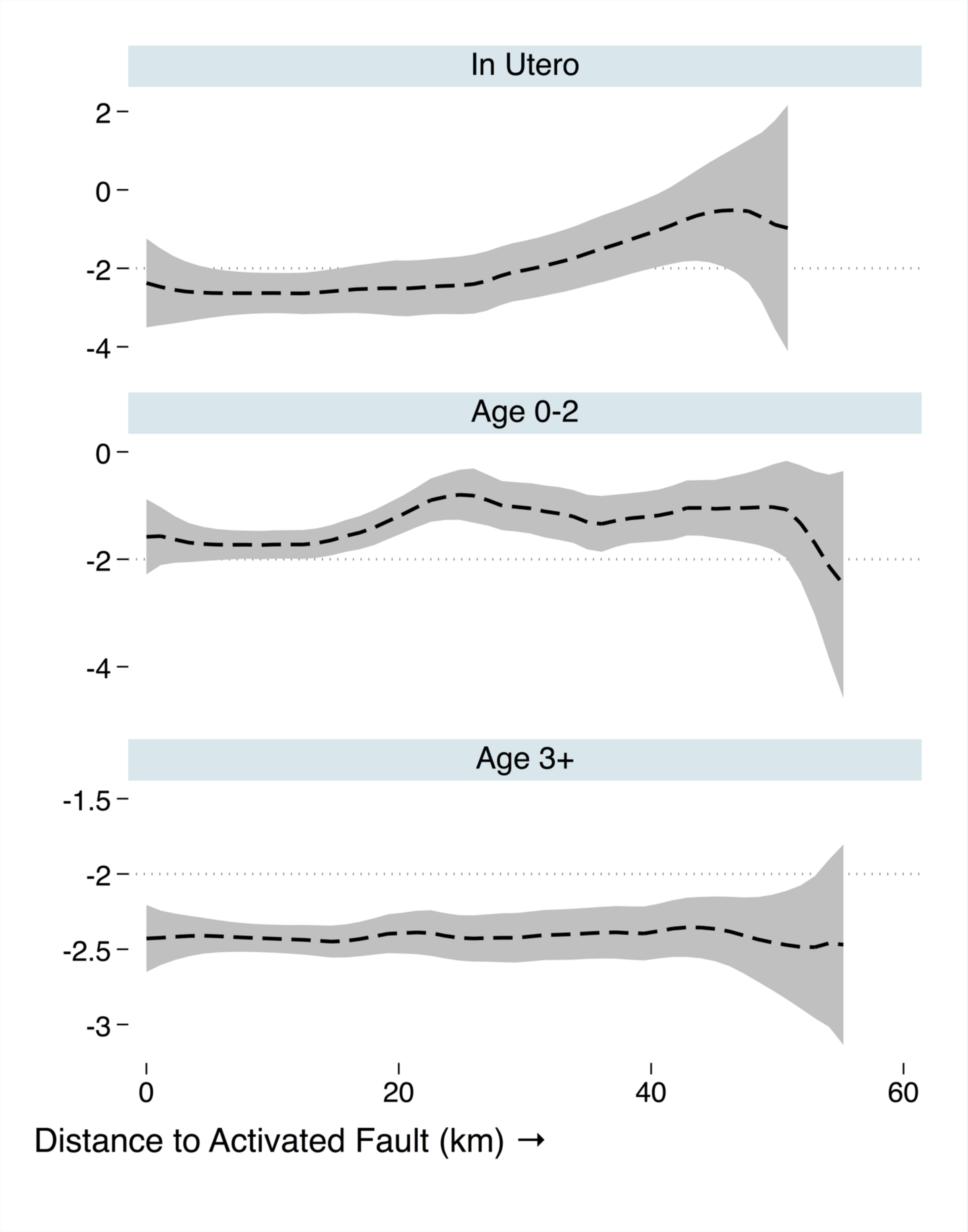
Notes: This figure illustrates self-reported aid received by households as a function of distance to the activated fault line, split into total aid (full recovery period) and immediate aid (non-rebuilding aid) for all households, households with a death, and households that reported home destruction.

Figure 6a. Height and weight outcomes for children in detailed survey



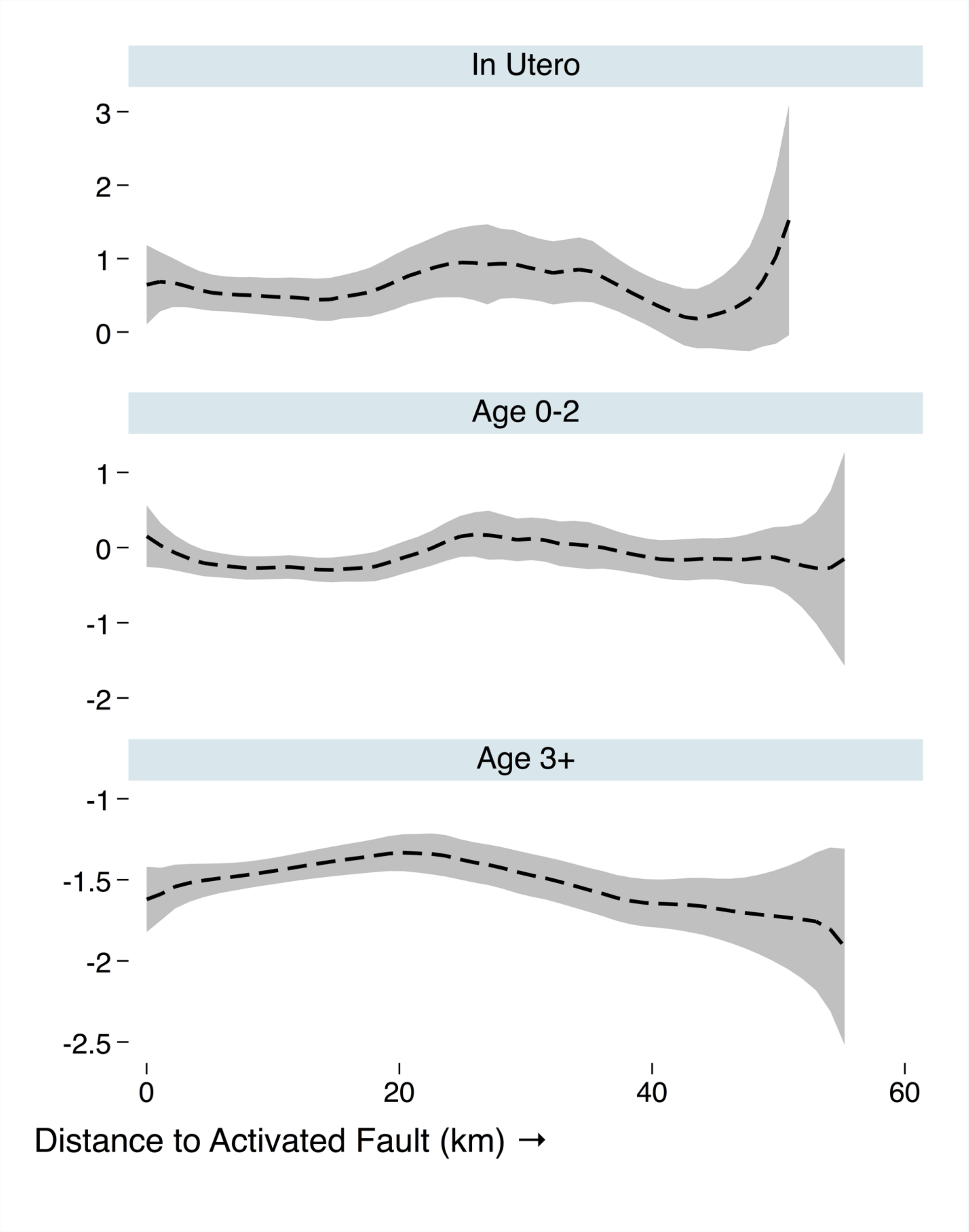
Notes: As a function of age at the time of the earthquake, these graphs compare the current height and weight of children covered in the detailed survey between near-fault and far-from fault groups using nonparametric specifications. WHO Means and Standard Deviations are referenced for the equivalent-aged US child population.

Figure 6b. Height-for-age (Z-score) outcomes by age and distance



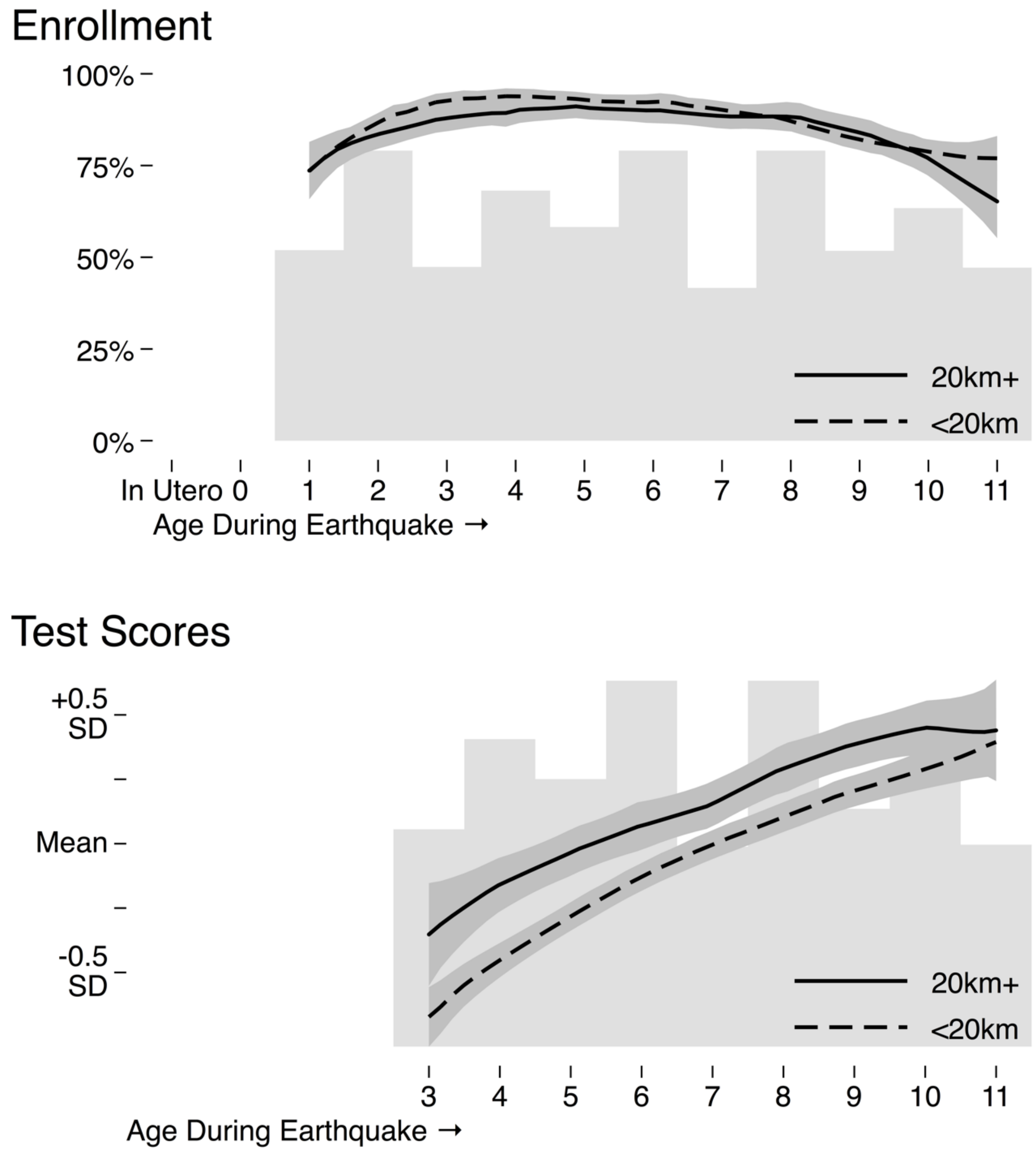
Notes: As a nonparametric function of distance to the activated fault line, these graphs illustrate the varying height impacts by age group across the range of proximity to the activated fault line. Outcomes are measured against the WHO Mean (the Y-value of zero) and Standard Deviations for the equivalent-aged US child population.

Figure 6c. Weight-for-age (Z-score) by age and distance



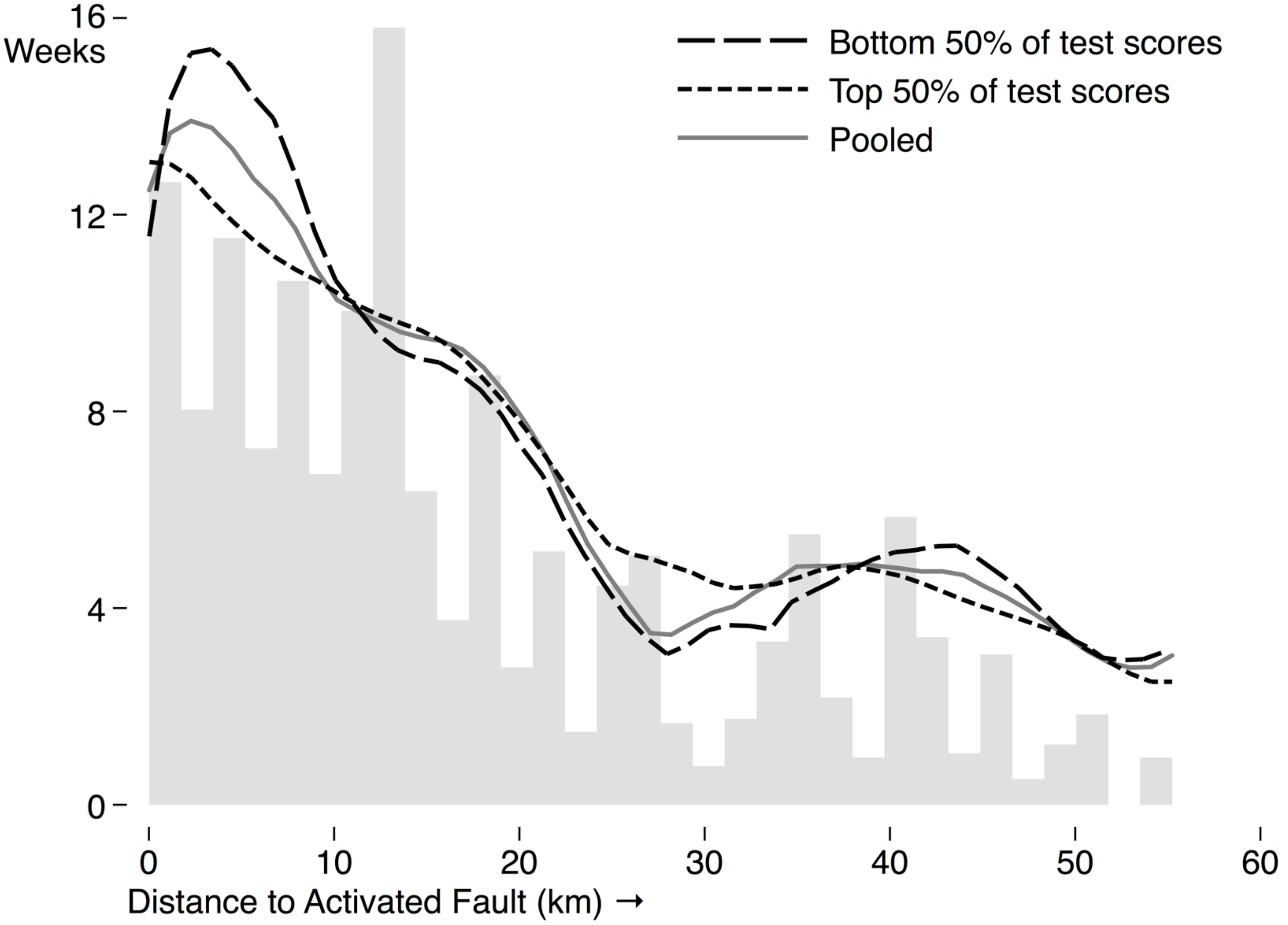
Notes: As a nonparametric function of distance to the activated fault line, these graphs illustrate the varying weight impacts by age group across the range of proximity to the activated fault line. Outcomes are measured against the WHO Mean (the Y-value of zero) and Standard Deviations for the equivalent-aged US child population.

Figure 6d. Education outcomes



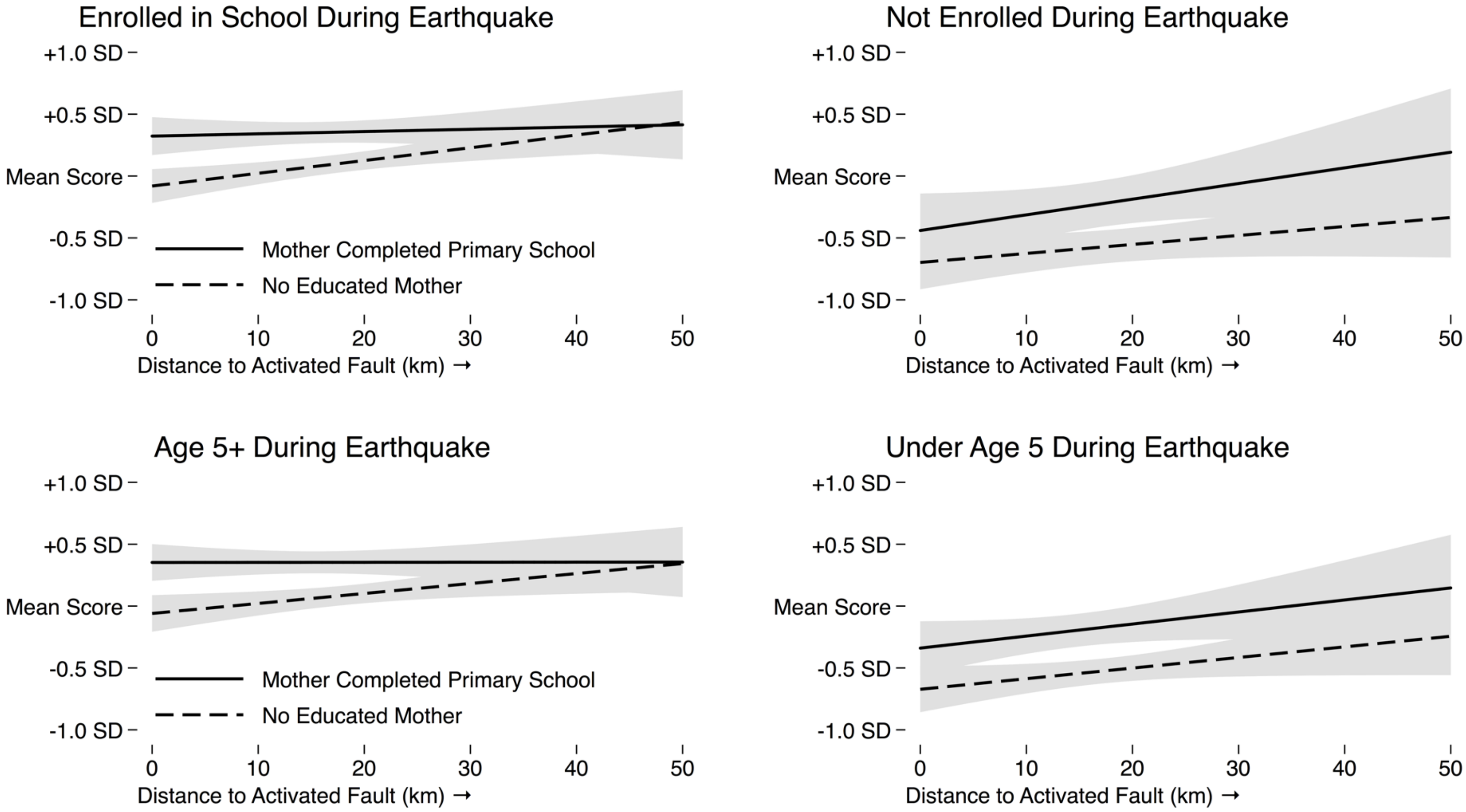
Notes: As a function of age at the time of the earthquake, these graphs compare the current school enrollment and academic performance of children covered in the detailed survey between near-fault and far-from fault groups using nonparametric specifications. Outcomes are presented as normalized IRT scores for the tested group with mean zero.

Figure 7. Time out of school after the earthquake



Notes: As a nonparametric function of distance to the activated fault line, these graphs illustrate 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.

Figure 8. Enrollment samples for mitigation: regression illustration



Notes: These graphs illustrate the predicted test scores from our mitigation regression specification for two counterfactual samples: children who were younger than minimum schoolgoing age at the time of the earthquake and children who were not enrolled at that time (of any age), comparing the mitigation effect between this and our main mitigation effect sample.

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)	-0.00	2,456	0.122	0.00
	0.00			
Household Infrastructure Index	-0.02***	2,456	0.168	0.00
	0.01			
Permanent House (Post-Quake)	-0.01**	2,456	0.089	0.64
	0.00			
Electricity	-0.01***	2,456	0.142	0.90
	0.00			
Water In House (Post-Quake)	-0.00*	2,456	0.057	0.50
	0.00			
Log Consumption per Capita	0.00	2,456	0.072	10.04
	0.00			
PANEL B: Access to Public Infrastructure				
Log Dist to Gov't School (min)	-0.00	2,454	0.039	2.78
	0.00			
Log Dist to Market (min)	0.00	2,452	0.119	3.62
	0.01			
Log Dist to Distr Office (min)	-0.00	2,449	0.240	4.83
	0.00			
Log Dist to Medical (min)	-0.00	2,444	0.048	3.79
	0.01			
Log Dist to Private School (min)	-0.01	2,369	0.037	3.40
	0.01			
PANEL C: Adult Health				
Adult Height	0.02	6,907	0.007	145.32
	0.02			
Adult Weight	0.02	6,907	0.012	45.59
	0.02			
Adult Height (18-24)	-0.01	1,717	0.012	130.25
	0.04			
Adult Weight (18-24)	0.02	1,717	0.011	34.12
	0.03			

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) Weight (Z-score)	(2) Height (Z-score)	(3) School Enrollment	(4) Test Scores (IRT)	(5) Test Scores – Gender	(6) Test Scores – Age
Distance from Faultline (km)	-0.007*	0.002	-0.000	0.008**	0.007	0.012***
	(0.004)	*(0.005)*	*(0.001)*	*(0.004)*	*(0.005)*	*(0.005)*
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***	0.068	0.041	0.066
	(0.048)	*(0.081)*	*(0.016)*	*(0.043)*	*(0.074)*	*(0.044)*
(log) Consumption per Capita	-0.001	0.084	0.026**	0.141***	0.141***	0.139***
	(0.045)	*(0.082)*	*(0.011)*	*(0.045)*	*(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*	*0.131*	*0.131*	*0.131*
Geographic Controls	X	X	X	X	X	X
Individual and SES Controls	X	X	X	X	X	X
Age Dummies	X	X	X	X	X	X
Regression R2	0.247	0.077	0.074	0.099	0.099	0.104
Number of Observations	4,002	4,001	1,874	1,875	1,875	1,875

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)*	*(0.01)*	*(0.01)*
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	0.066*	0.066	-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	*0.131*	*0.131*	*-1.676*	*-1.676*
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	1,875	1,875	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)	0.017***	0.030***	0.007	-0.014
	(0.006)	(0.009)	*(0.02)*	*(0.04)*
Mother Completed Primary School	1.629***	4.821**	-3.352*	-4.985
	(0.558)	(1.873)	*(1.840)*	*(3.600)*
Mother's Education * Distance		-0.139**		0.083
		(0.068)		*(0.126)*
Male	0.059	0.041	-0.212	-0.234
	(0.048)	(0.059)	*(0.172)*	*(0.179)*
Log Consumption per Capita	-0.027	-0.088	0.439	0.471
	(0.069)	(0.108)	*(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
First-stage F-statistic	21.632	4.340	19.374	5.127
Number of Observations	1,723	1,723	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

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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 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 A2a 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 is a migrant on distance to the fault line in Table 2b. 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

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

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 A2c. 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.

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).

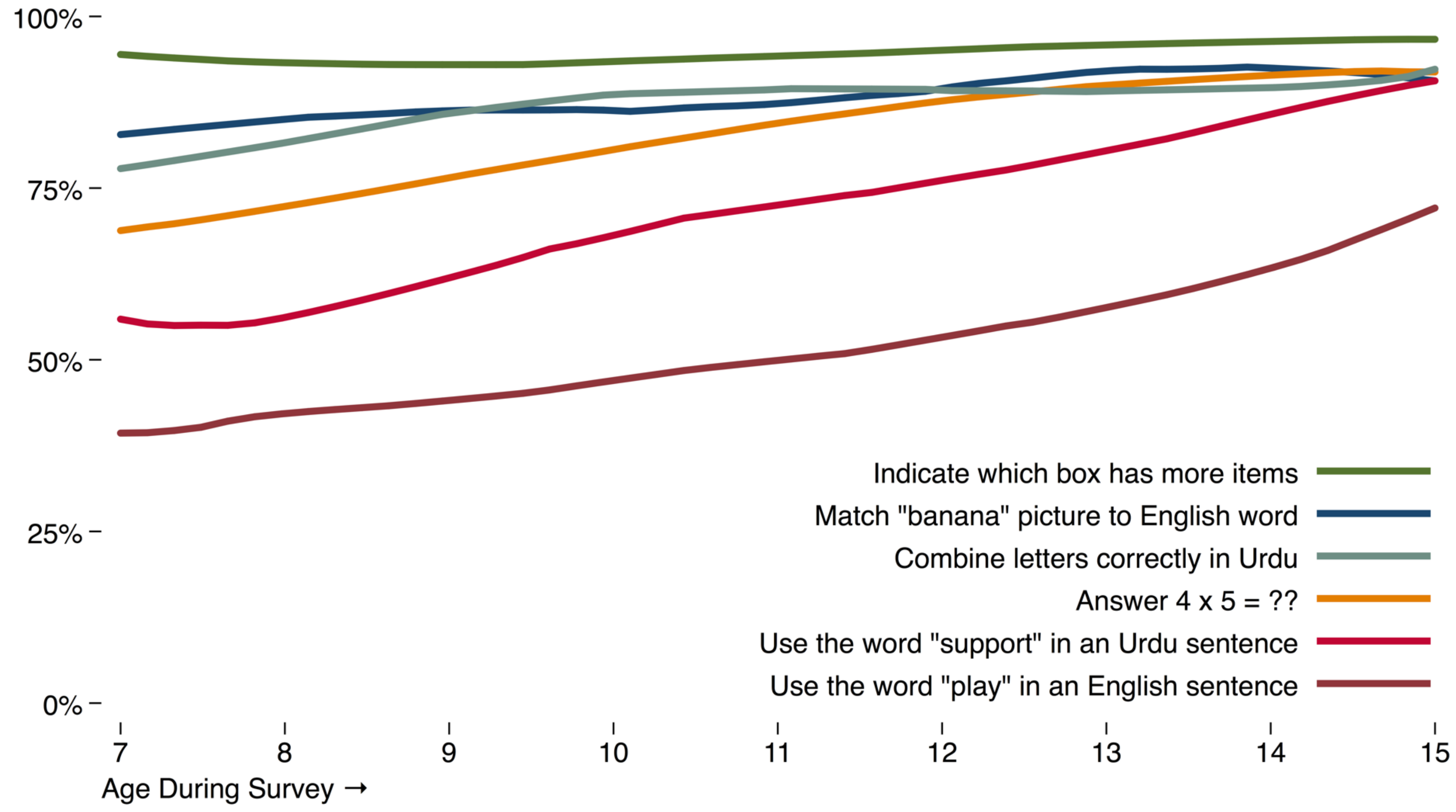
Figure A3 visualizes the results of our mitigation OLS regression for reference. Figure A4 illustrates the results of a robustness check in which we systematically exclude each cluster (village) from the mitigation IV regression on test scores; even the most extreme result remains statistically significant 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 restricts the sample of children to those in 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.

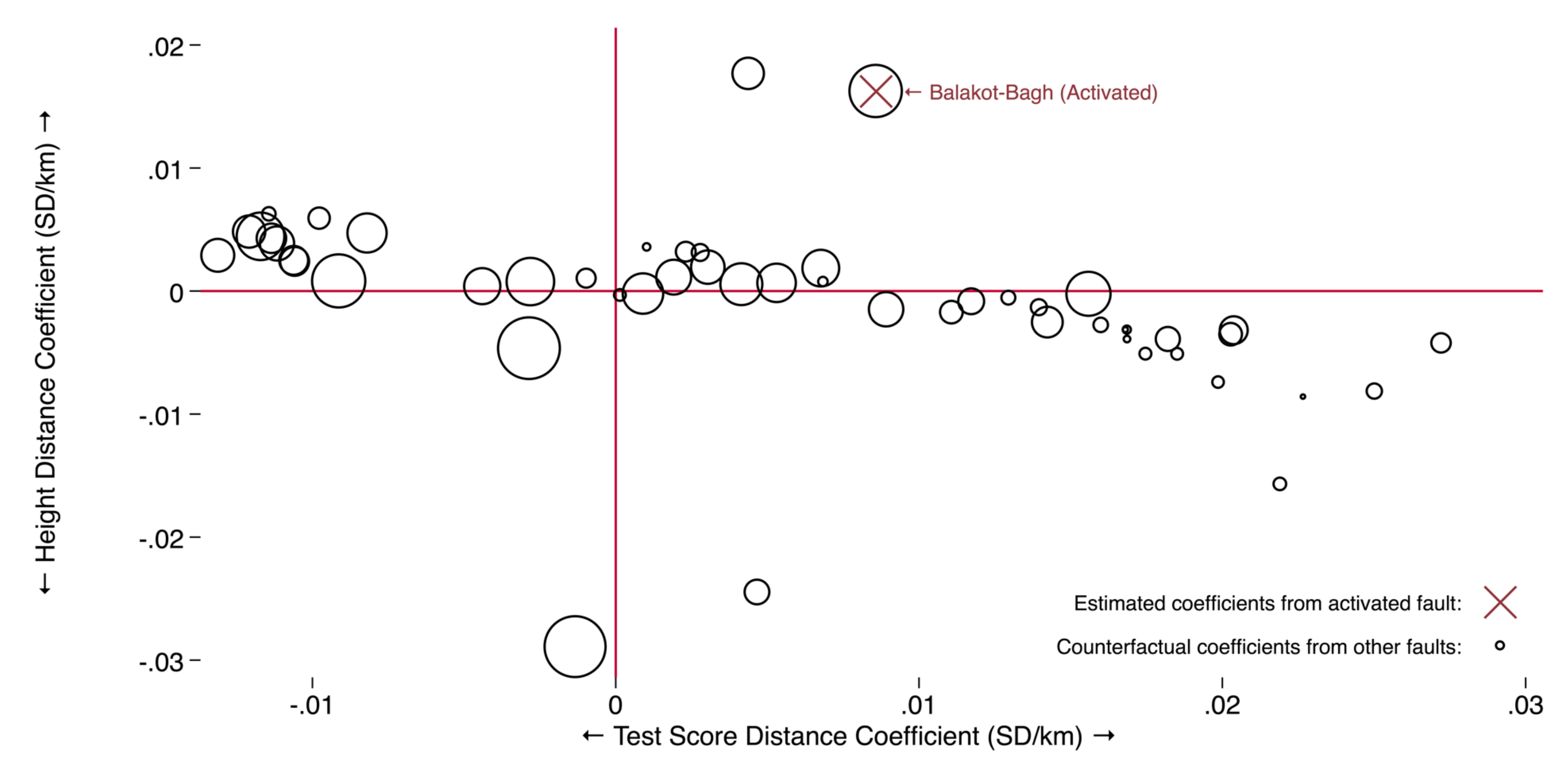
Finally, Table A3d investigates alternative potential mitigating characteristics for both height and test scores. Columns 1 through 3 show non-significant and non-causal mitigation results for the test scores results for all three potential mitigating factors. Columns 4 though 6 show significant but 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: Demonstrative test questions from educational attainment



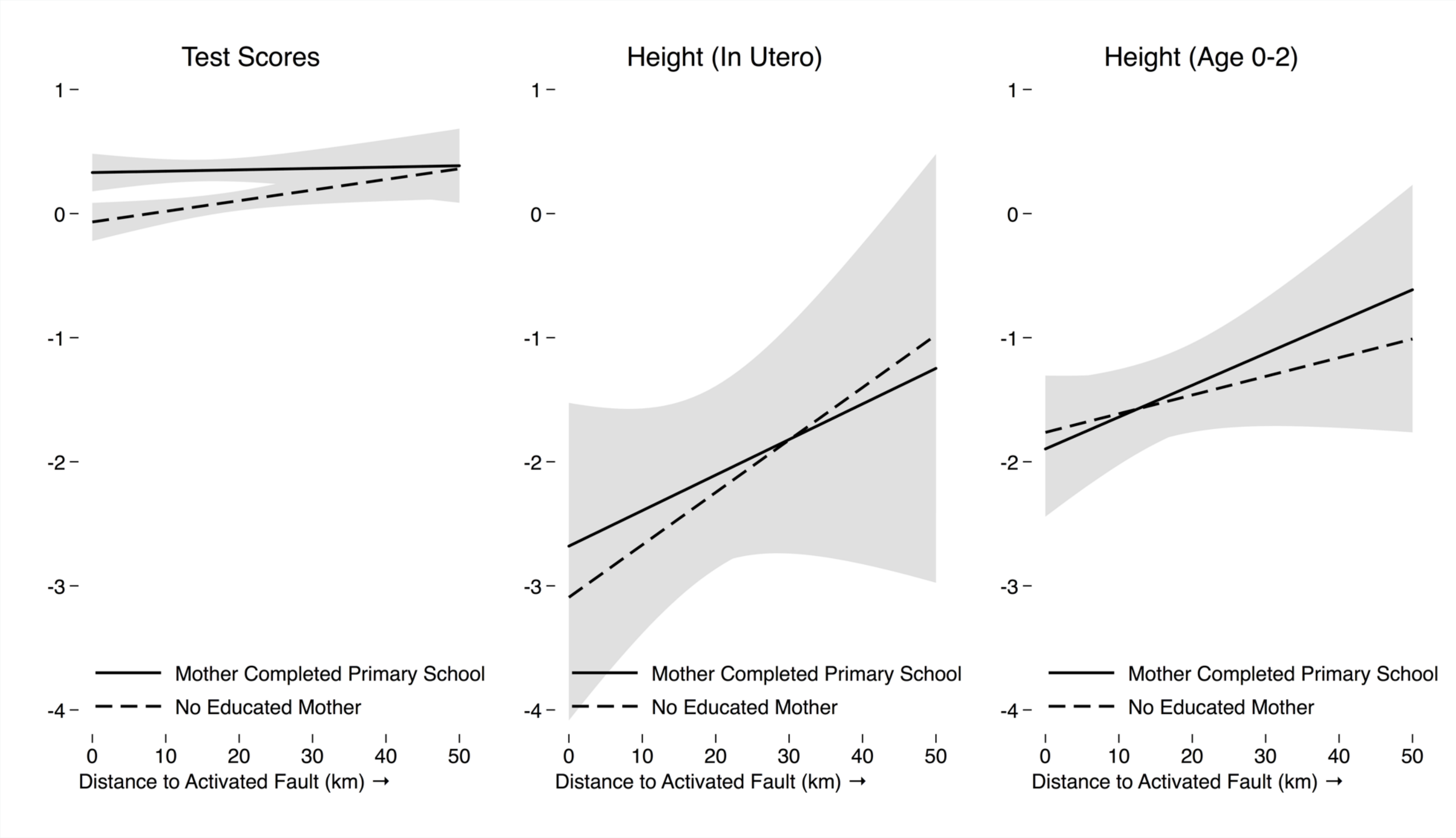
Notes: This figure illustrates results for some demonstrative questions from the knowledge exam in English, math, and Urdu administered to children in the detailed survey sample from ages 7-15 at the time of the survey. Ages at the time of the earthquake are 4 years younger.

Figure A2. Placebo fault lines simulation for primary earthquake effects



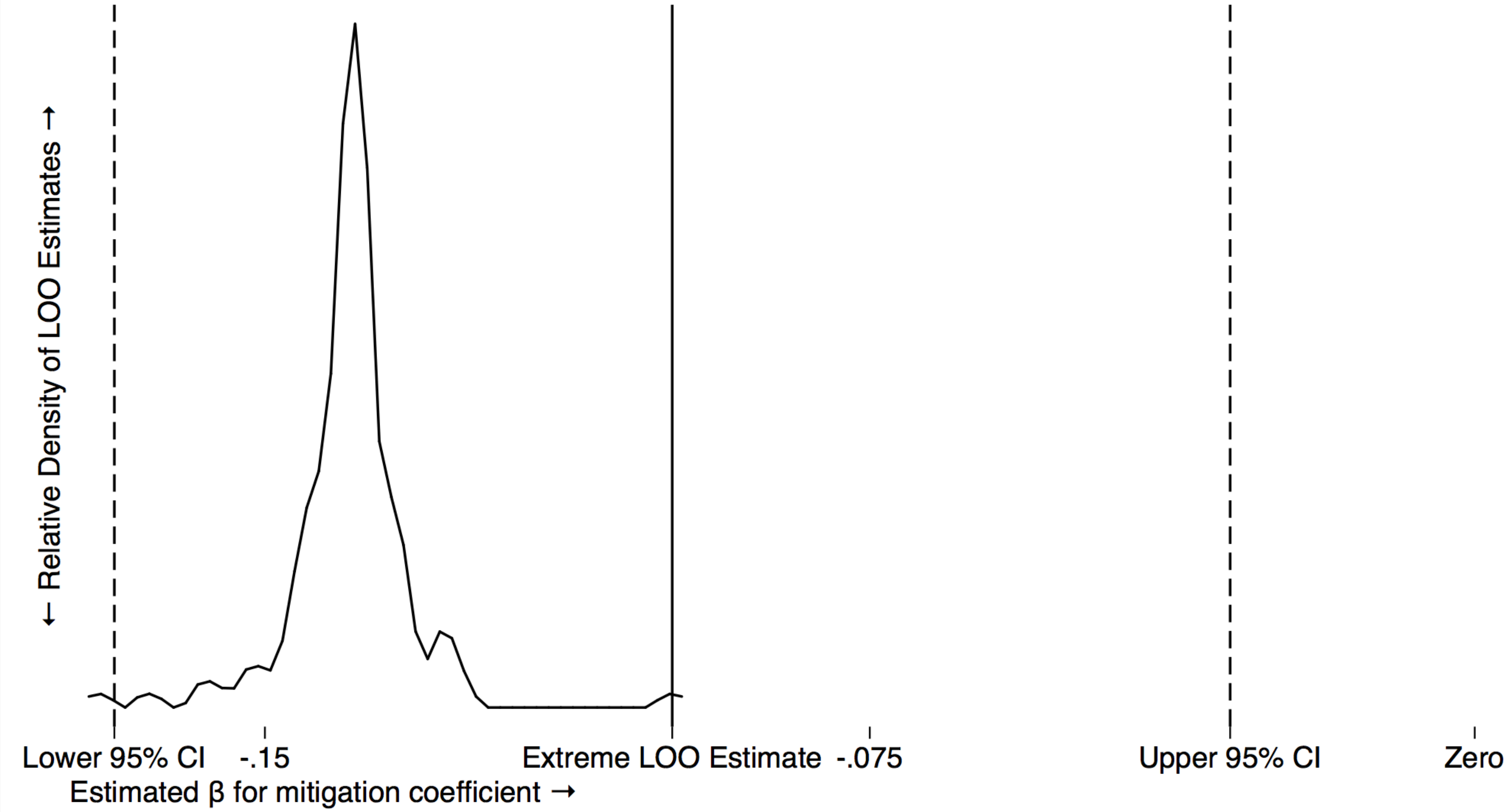
Notes: This figure illustrates the joint distribution of placebo distance coefficients for non-activated faults, for both test scores and for child heights (in utero – age 2). The estimated counterfactuals are obtained by calculating the distance from each household to the other 50 non-activated fault 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 size of the fault; the highlighted marker represents the activated fault and the corresponding effect estimates from our main specifications.

Figure A3. Maternal education mitigation visualization



Notes: This figure illustrates the predicted outcomes for children at various distances from the activated fault line, with and without a mother who completed primary school, based on the results of our primary (OLS) maternal education mitigation specification.

Figure A4. Instrumental variables leave-one-out simulation



Notes: For each village cluster in our test scores maternal education mitigation IV specification, we systematically repeat the estimation excluding that village. This figure plots the distribution of mitigation coefficients we obtain from this sequence of regressions. The vertical lines indicate the point estimate and 95% CI for estimate closest to a null result.

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	259	196	3	28	4	1	3	24
Total	2,928	2,386	66	245	43	24	9	155

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	259	216	5	153.75	-1.61	39.34	-2.40
Total	4,475	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	10.96	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	2,845	2,387	.

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	0.22	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	4,360	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. 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 A2b. Migration after the earthquake regression results

	(1) Adult Out Migration	(2) Adult In Migration	(3) Child Out Migration	(4) Child In Migration
Distance to Fault (km)	-0.000	0.000	0.000	0.001***
	(0.635)	(0.928)	(0.346)	(0.001)
Distance to Epicenter (km)	-0.001**	-0.001**	-0.000	0.000
	(0.020)	(0.016)	(0.702)	(0.891)
Mean Slope of UC	0.001*	-0.000	-0.000	0.001
	(0.098)	(0.309)	(0.627)	(0.225)
Male	-0.010**	-0.038***	-0.001	-0.008
	(0.015)	(0.000)	(0.818)	(0.115)
(log) Consumption per Capita	-0.015**	0.013***	-0.008	0.003
	(0.011)	(0.000)	(0.214)	(0.492)
Constant	0.164**	-0.065*	0.091	-0.048
	(0.011)	(0.076)	(0.242)	(0.286)
Number of observations	8,152	8,152	4,475	4,475
Regression R2	0.035	0.051	0.012	0.207
Dependent Variable Mean	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.

Table A2c. 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	-3.669	-2.445	-4.714	-1.499
P-value	0.000	0.014	0.000	0.134
Lower bound	-0.337	-0.234	-1.460	-0.758
Upper bound	-0.102	-0.026	-0.602	0.101
N observed	1,875		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) Instrument	(2) Recieved School Sometime	(3) Geographical Controls	(4) Boys' School	(5) Girls' School (Other Ages)
Distance from Faultline (km)	0.001	0.000	0.001	0.000	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Girls' school present by age 9	0.125***	0.122***	0.115***		0.136***
	(0.029)	(0.032)	(0.029)		(0.040)
Boys' school present by age 8				-0.011	
				(0.030)	
Girls' school present at age 10-14					0.033
					(0.050)
Girls' school present after age 14					0.015
					(0.033)
Constant	0.396	-0.609***	0.655	-0.480	-0.424
	(0.341)	(0.135)	(0.482)	(0.551)	(0.536)
Number of observations	991	837	991	991	991
F-statistic for Age 9 School Availability	18.470	14.980	16.020	0.140	11.280

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.

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	-2.26	1,723
	0.05	*0.11*	.	1.73	
Height (cm)	132.33	130.77	-0.50	-15.56	1,722
	0.43	*0.91*	*0.68*	13.12	
Weight (kg)	31.78	31.66	0.32	-9.12	1,722
	0.21	*0.47*	*0.42*	10.35	
Completed Primary School	0.40	0.51	0.12***	0.72	1,723
	0.01	*0.03*	*0.03*	0.52	
Father Completed Primary School	0.49	0.85	0.32***	1.74**	1,665
	0.01	*0.02*	*0.03*	0.84	
Enrolled (Earthquake)	0.86	0.93	0.05**	0.33	1,722
	0.01	*0.01*	*0.02*	0.36	
Enrolled (Survey)	0.89	0.97	0.04**	0.43	1,722
	0.01	*0.01*	*0.02*	0.38	
Private School (Earthquake)	0.16	0.33	0.15***	-0.26	1,508
	0.01	*0.03*	*0.03*	0.73	
Private School (Survey)	0.17	0.27	0.07***	-0.37	1,557
	0.01	*0.03*	*0.02*	0.64	
(log) Consumption per Capita	9.85	10.08	-0.00	-1.05	1,723
	0.02	*0.04*	.	1.09	
Household Asset Index	-0.06	0.64	0.57***	0.77	1,723
	0.02	*0.05*	*0.05*	1.40	
Log Distance to Nearest Gov't School (min)	2.79	2.65	-0.17***	-0.91	1,722
	0.02	*0.04*	*0.04*	1.25	
Log Distance to Nearest Market (min)	3.69	3.35	-0.27***	-0.39	1,722
	0.02	*0.05*	*0.05*	1.77	
Log Distance to Nearest Distr Office (min)	4.84	4.71	-0.06*	0.61	1,715
	0.02	*0.04*	*0.04*	1.09	
Log Distance to Nearest Medical (min)	3.83	3.64	-0.15***	0.08	1,719
	0.02	*0.04*	*0.05*	1.28	
Log Distance to Nearest Private School (min)	3.40	3.12	-0.24***	1.28	1,654
	0.03	*0.05*	*0.06*	1.74	
Number of observations	1,560	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 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	0.018	0.039
	(0.007)	(0.007)	(0.013)	(0.070)
Mother Completed Primary School	0.156	0.603***	0.665	27.258
	(0.117)	(0.197)	(1.592)	(82.544)
Interaction		-0.028***		-1.141
		(0.009)		(3.285)
Male	0.040	0.051	0.073	0.253
	(0.082)	(0.080)	(0.083)	(0.758)
(log) Consumption per Capita	0.174**	0.163**	0.142	-1.255
	(0.071)	(0.071)	(0.251)	(5.171)
Number of observations	573	573	539	539
First Stage F-stat			1.805	0.048

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 A3d. 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	0.056	-0.035	0.288***	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	0.062	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	1,705	1,875	1,875	1,224	1,423	1,423
Regression R2	0.109	0.121	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