Human Capital Accumulation and Disasters:

Evidence from the Pakistan Earthquake of 2005

Tahir Andrabi, Benjamin Daniels, and Jishnu Das

Abstract. In 2005 a large earthquake struck Northern Pakistan. Exposure to the earthquake was plausibly exogenous to household and individual characteristics, but households received substantial compensation after the earthquake. Four years later, there were no differences in household or adult outcomes by earthquake exposure. Nevertheless, children under age 3 at the time of the earthquake accumulated large height deficits and children aged 3–11 scored significantly worse on academic tests, unless their mothers had completed primary education. Even disasters that are accompanied with substantial compensation can lead to severe disruptions in the accumulation of human capital.

Affiliations. Tahir Andrabi is Stedman-Sumner Professor of Economics at Pomona College. Benjamin Daniels is a gui2de Research Fellow at Georgetown University. Jishnu Das is a professor in the McCourt School of Public Policy and the School of Foreign Service at Georgetown University. Correspondence to: jishnu.das@georgetown.edu. The Online Appendix can be found at http://jhr.uwpress.org/.

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

Disclosure. Funding was provided through the Norwegian Governance and DFID trust funds (World Bank), National Academy of Sciences (United States), Pomona College SURP, the Higher Education Commission (Pakistan) and through a grant from the RISE Network. Logistical support from Lahore University of Management Sciences is acknowledged. At the time of the survey (and to date), the World Bank Group did not have a formal IRB process for data collection. Following the usual protocols, we sought and obtained full approvals and comments from the government to conduct the survey. The associate dean at Pomona College had approved the project based on the governmental approval letter for the project. Use of the data for this study was granted IRB exemption by the Georgetown IRB with ID: STUDY00002237. Andrabi has received funding from Lahore University of Management Sciences (LUMS), Lahore, Pakistan (for design and setup of a school of education); and from the Department of International Development (DFID), UK, Pakistan Country Research Team, for research in systems of education. He is a Board Member (without remuneration) at the Center for Economic Research, Pakistan (a non-profit research organization). Daniels is a paid research consultant for the World Bank Group and was previously a paid research consultant for McGill University. Das has no disclosures.

Data Replication. The data and code used in this article are available online at: https://doi.org/10.3886/E128182 (data) and https://doi.org/10.5281/zenodo.4302376 (code).

JEL Codes. O15, Q54, J24, H84

doi:10.3368/jhr.59.2.0520-10887R1

1. Introduction

A large literature establishes that adverse shocks during childhood, especially in the first 1,000 days of life, can lead to worse schooling outcomes as well as poorer physical and mental health in later life, ultimately translating into a loss of productivity and earnings.[1] However, whether these adverse consequences are evident *even when the shock is accompanied with substantial compensation* remains less clear: Studies of now-adult populations trace the impacts of severe shocks in childhood at a time when households were unlikely to be compensated, especially in low-income countries. Our aim here is therefore to establish (a) the extent to which human capital accumulation may be interrupted even when there is substantial compensation and (b) the extent to which parental attributes may help mitigate these shocks.

Our focus is the 2005 Kashmir earthquake in Pakistan, one of the most physically destructive disasters in recorded history, equivalent in force to the 1906 San Francisco earthquake. Eighty percent of homes in the immediate vicinity of the activated fault line were destroyed, as was a great deal of critical public infrastructure, including schools. The earthquake resulted in 73,000–79,000 deaths and 69,000–128,000 injuries. (Encyclopedia Britannica, 2018; Department for International Development and DFID Pakistan, 2018) However, the loss to households was compensated: affected families within 20 kilometers of the fault line received 150% of their annual consumption expenditure in cash aid within two years of the disaster.

To estimate the impact of the earthquake on human capital accumulation, we compare outcomes among households and individuals at varying distances from the activated fault line, using rich survey data that we collected four years later in 2009. The causality of our findings rests on three observations. First, like in Andrabi and Das (2017), pre-determined household and village characteristics are uncorrelated with distance to the fault line. This is consistent with the fact that geological models cannot predict earthquake timing or location and the affected region is crisscrossed with multiple other (invisible) fault lines which could just as plausibly have been activated; all the households in our sample live within 11.5km of at least one fault line and half live within 2.5km of one. Accounting for hypothetical fault line exposure risk by controlling for the distance from the household to the *nearest* fault line or constructing a placebo test that simulates the activation of 40 other faults in the region does not alter our results.

Second, our survey was restricted to those still living in the region, raising potential concerns due to selective relocation. Using carefully reconstructed household rosters, we show that individual migration was not correlated with distance to the fault line at a magnitude sufficient to affect our estimates. A bounding exercise confirms that our results are robust to reasonable assumptions about selective non-responsiveness due to migration, mortality, and unavailability.

Third, we document the nature and extent of compensation and show that this compensation was strongly correlated with proximity to the fault line and did not spill over to unaffected areas.

These three patterns in our data help us estimate the full effect as a "joint treatment" of the disaster *combined with* relief aid, compared to a control group that received neither. As compensation for housing reconstruction took well over a year to implement, we 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.

Four years after the earthquake, we find that households nearest the fault line were at least as well off as those farther away in terms of wealth, consumption, and infrastructure, and were significantly more likely to be living in a permanent masonry residence. Household consumption, household wealth, and adult weight (a marker of short-term environmental stress) were at or above parity with less-affected areas. There were also no differences in access to public infrastructure as measured by geographic distance. This "back to normalcy" result mirrors findings from the U.S., Japan, and Indonesia in disasters that were followed by substantial compensation. (See Deryugina et al. 2018, Sawada and Shimizutani 2008, Frankenberg et al. 2013, and Cas et al. 2014.)

However, we continue to observe large shortfalls in the physical and cognitive development of children. Using height as an indicator for cumulative childhood shortfalls experienced during the recovery period, children who were under three years of age at the time of the earthquake and living close to the fault line were significantly shorter than those living farther away (Behrman and Hoddinott 2005). 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. It also emerges only for children in the first thousand days of development at the time of the earthquake, with the youngest the hardest hit, consistent with the timing of growth faltering documented in the literature (Shrimpton et al. 2001). This effect on child growth is comparable to those observed in the civil wars in Rwanda and Burundi, and the 1982-4 drought in Zimbabwe (Akresh et al. 2011; Bundervoet et al. 2009; Alderman et al. 2006).

Turning to education, we find no evidence of a decline in current school enrollment in earthquake-affected areas. Neither were there any differential gender effects on enrollment; although there is a difference in enrollment rates for girls and boys, the earthquake did not exacerbate these differences. We also do not find any difference in grade attainment relative to age. Thus, children remained in the same grade and were promoted at the same rate as their counterparts in unaffected regions.

Nevertheless, test scores of children living within 20km from the fault line were 0.31 standard deviations lower than those living more than 20km away. This difference does not vary by age and is equivalent to about 1.5 school grades. 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, even when households receive substantial compensation.

The earthquake also exacerbated inequalities *within* the affected areas. Children whose mothers had completed primary school were largely protected from the earthquake's negative effects on test scores. Since children whose mothers were educated *already* enjoyed a test score advantage of 0.32 standard deviations, the shock served to substantially worsen inequality in test scores between these two groups of children. We do not find a mitigating effect of maternal education for the height impacts, echoing Figlio et al's. (2014) finding that parental socioeconomic status does not mitigate in-utero biological shocks. An instrumental variable strategy that uses the availability of girls' schooling in the mother's birth village at the appropriate age as an instrument for maternal education recovers similar patterns, with maternal education mitigating test score losses, but not growth deficits among very young children (Currie and Morretti, 2003 and Andrabi, Das and Khwaja, 2012).

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

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

Second, in terms of identification, the unpredictability of earthquakes (especially in this area, where fault lines are numerous and are not visible) also satisfies several unusual requirements that may not be fulfilled with other disasters. Our estimates are unlikely to be biased by mortality selection or by selection into proximity to the activated fault line. We do not find any correlation between pre-existing characteristics and earthquake-related mortality, aside from slight excess vulnerability in the very young and very old, and even in the villages that were hardest hit, mortality never exceeds 5%.[2] The unpredictability of earthquakes also alleviates concerns arising from selection into exposure: Advance warnings for hurricanes, for instance, imply that downstream impacts depend on the degree of responsiveness in the population and its correlation with household and individual characteristics.

Third, the fact that some villages are exposed to the earthquake shock while others are not allows us to examine the causal impact of the earthquake *across the entire age-range*, instead of the relative impact on children in the critical period compared to those who are older. For stature, we can confirm the validity of cohort comparisons—we do not find evidence of any physical effects among children who were older than three years at the time of the earthquake. However, in the case of test scores, children suffer at all ages in a similar way and cohort comparisons are no longer valid estimates of overall deprivation.

Fourth, we show that maternal education mitigates the average impact of the earthquake on cognitive development. Therefore, the (in)ability of households to mitigate shocks plays a key role in the evolution of inequality *within* regions exposed to the earthquake. Our instrumental variables specification suggests that this mitigation result may reflect the causal impact of mother's education.

Our findings admittedly leave open questions related to mechanisms and longer-term effects. We do not have panel data on household investments or child outcomes. Therefore, we cannot determine whether the impacts we observe among children reflect purely biological factors or household investments or an interaction of the two. Household investments and childhood biology inevitably interact and we do not know the timing or lag structure of these investments and interactions (Bharadwaj et al., 2018). We also do not know whether children will recover from these shocks in the future through "catch-up" growth. In contexts where aid flows are small or stop after a short while (as in Pakistan), the precise conditions under which children can recover from such nutritional deficiencies are unclear and complete recovery seems unlikely, especially for those who suffer shocks in the critical period.[3] Finally, our results do not imply that cash compensation is *never* enough; we observe a single draw of how cash can be distributed (which we describe below) and we do not have evidence for the impact of other schemes that differ in timing and amounts. (Paxson and Schady 2010)

The remainder of the paper is as follows. Section II describes the dataset and the survey process and places the research in the context of existing literature. Section III presents our empirical strategy and Section IV presents our results. Section V concludes with a discussion of external validity and the consequences of these results for disaster relief.

II. The Pakistan Earthquake of 2005 and Data Description

The earthquake on October 8th, 2005 in Northern Pakistan left an estimated 73,000–79,000 dead, 69,000–128,000 seriously injured and over 2.8 million homeless. Immediately following the earthquake, organizations provided financial support as well as logistic and technical assistance, ranging from specialized services in medicine and excavation to evacuation, emergency shelter, and food. Most operations were conducted by the Pakistan Army, with support from international agencies. In this phase, affected households received PKR 25,000 in immediate cash aid as well as additional compensation for injury or death.

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

By spring, basic assessments of damage had been conducted and further compensation was given for construction, along with training on earthquake-resistant housing. The full compensation was disbursed over the next three years as houses were built and funds sequentially released following inspections of the plinth, structure, and roofing. We therefore 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.

II.A. Data

Our data were collected from 2009-10 as the aid program wound down and most reconstruction had been completed. From the four districts most affected by the earthquake, we randomly selected 126 rural villages from the most recent 1998 census of villages for the study. The selection zone ranged up to 80km from the activated Balakot-Bagh Fault in the two affected provinces, with the average household located 17.5km from the activated fault line and 36.4km from the epicenter.

We administered two types of surveys. We first completed a "short" census of all 28,297 households in the sampled villages (154,986 individuals) that captured GPS coordinates, a household roster, information on deceased household members, a listing of aid groups that assisted the household, and official cash grant programs the household participated in. For a randomly-selected 20% subsample, which covered 6,455 households, we augmented the short census with additional questions on children's education, home destruction, public infrastructure access, and a depression and PTSD screening questionnaire (we refer to this as the "extended" census). We then implemented a detailed survey to a randomly selected 10% subset of the census households, producing extended records for 2,456 households covering 15,036 individuals. This survey was similar to multi-topic household surveys, with a special emphasis on children's health and schooling as well as pre- and post-earthquake recall questions on multiple topics. In total, we have some information on 152,435 living and 4,340 deceased individuals. Andrabi and Das (2017) discuss the sampling of villages and demonstrate the validity of the randomization for the extended census; we review the sampling procedure in the Online Appendix.

Figure 1 shows the location of households covered by the detailed survey and fault lines in the area, with the activated fault line and earthquake epicenter highlighted. Figure 2 illustrates the distribution of households in the detailed survey with respect to the activated fault line along with a quantile plot illustrating various percentiles of distance. The distance to the activated fault line ranges from 0km to 75km with a mean of 19km and a median of 13km.

II.A.1 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. We attempted to collect anthropometric outcomes for children aged 3-15, school enrollment information for children aged five and up, and we administered tests in English, Urdu, and mathematics at home for children aged 7-15, regardless of their enrollment status.[4] Completion rates were 89% for anthropometric and 81% for test score measurements (Table A1a) and we do not find significant differences between the completed and eligible populations, although children who were tested were 3-4 percentage points more likely to be enrolled in school (Table A1c and Table A1d).

The mean measured height in our sample was 117.5cm and the mean measured weight was 25.6kg (Table A1b). As in other low-income settings, learning levels were low across the age range (Figure A2), although 86% of children were enrolled in school (30% in private school).

The mothers in our sample were 37 years old on average, and 26% had completed primary school, compared to 60% of fathers. As most variation in mother's education comes from a simple binary indicator of completed primary education, this is what we use in our specifications for maternal education.[5]

To examine the causal impact of maternal education on child achievement, we use maternal access to a school in the mother's village of birth by age eight as an instrument for primary education. The village of birth was recorded during the household survey and then matched to school availability in administrative data on school locations and date of establishment. We can match 92% of the mothers of tested children with complete historical data on schooling availability.

III. Econometric Approach and Identification of the Earthquake Effect

Our econometric specification exploits variation in household distance to the activated fault line as the conditionally-exogenous measure of the strength of the earthquake shock. The general form of the regression specification is:

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

where Y_i is our dependent variable (whether household or child level), $DistanceToFaultline_i$ is the continuous proximity variable, and X_i represents the vector of geographical controls, which includes district, distance to epicenter, and a measure of the hilliness of the region surrounding the village, as well as other household or individual-level controls depending on the regression. Standard errors are clustered at the village level.[6]

III.A. Assessing Exogeneity

To support our claim of conditional exogeneity, we first highlight that earthquakes are disasters with zero lead time in forecasting, and this earthquake struck after a long period of geological calm. 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.[7] Additionally, as Figure 1 illustrates, there are multiple potentially active fault lines in the region affected by the 2005 earthquake, and most of the households in our survey live close to some other fault line that was equally likely to be activated. We control for the distance to the nearest fault line in all regressions to remove effects of differential sorting by exposure to fault line risk. Thus, it is plausible that populations were randomly distributed in terms of their pre-earthquake attributes with respect to the activated fault line.

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 from the 1998 population census. In lieu of more recent pre-earthquake data from the region, we also report further correlations using data from our household survey as well as retrospective and current location data on village facilities.[8] 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 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. 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.

We do find that households farther from the fault line were less likely to report that they had electricity before the earthquake and slightly lower asset and infrastructure levels. These correlations reflect 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. There is also a potential concern of fertility selection in the case of very young children, as noted by Brown and Thomas (2018) in the case of the 1919 birth-cohort following the Spanish Flu pandemic in the United States. Appendix Figure A8 and Table A4d show that this concern is limited in our case as parental age, education, height and occupation are along the relevant trend-line for all ages with no evidence of a departure for children who were in-utero to age 2 at the time of the earthquake. Taken together, both village and household data strongly suggest that pre-existing observed (and unobserved) characteristics were not correlated with distance to the fault line.

Despite the exogeneity of pre-earthquake characteristics to the distance to the fault line, concerns may remain in terms of (a) the measurement of earthquake intensity; (b) post-earthquake migration and selective mortality; and (c) aid spillovers. We discuss each in turn.

III.A.1. Measuring Earthquake Intensity

Some studies have used alternate measures of earthquake intensity, such as the distance to the epicenter or the Mercalli intensity, which captures the actual extent of shaking at each point.

Andrabi and Das (2017) discuss why these measures are not consistent with the geology of this earthquake and/or the conditional exogeneity requirement and demonstrate that these alternate measures are, in fact, correlated with pre-earthquake characteristics, primarily because the Mercalli intensity is correlated with soil characteristics, which in turn is correlated with agricultural yield and building suitability. Although the Mercalli intensity does not satisfy exogeneity requirements, we show that our results are robust to this alternate measure of local intensity in Appendix Table A4c.

III.A.2. Earthquake-Induced Migration and Mortality

Large population movements as a response to disasters (McIntosh and Fifer 2008, Deryugina et al. 2018) can lead to a selected sample as we do not have pre- or post-earthquake characteristics for movers. There are two types of mobility responses. One option is that some members of the household relocated following the earthquake. To assess mobility-induced selection, we listed all persons who had lived in the household both before and after the earthquake in our survey modules to track both "out migration" and "in migration". Of the 5,112 living adults we listed as living close to the fault line in this inclusive method, 192 (3.8%) had moved out and 167 (3.3%) had moved in after the earthquake. The numbers and percentages are remarkably similar 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.[9] We do not find any evidence of any differential migration of adult members after the earthquake by distance to fault line; and we find a significant but small difference in child in-migration (Table A2a). Similarly, overall mortality was too low to induce severe selection bias under all but worst-case assumptions. At its highest, childhood mortality was 5%, which could not bias childhood development results in the direction we find, unless the most vulnerable children were also the tallest and the highest-achieving to a large degree. This is unlikely, as the slight excess mortality we observe is in poorer households; however, in Section IV, we compute bounds on selective attrition using mortality, migration, and incomplete surveys to demonstrate the robustness of our results.

There is also a second option, which is that entire households entered or left the village as a response to the shock. In the pilot phase, we found very few examples of whole-household migration. Even in households where most members had left, at least one remained behind to keep the property secure and to obtain compensation – meaning we could then reconstruct the household roster by surveying the remaining member. While we do not have direct measurement of the frequency of whole-household migration (this exercise was excluded from the main survey), we believe that strong cultural and institutional features of the environment worked against the migration of entire households. Most people own their land, but have weak property rights against their own extended family. 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.

III.A.3. Aid Spillovers

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

IV. Results

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

IV.A. Defining the shock: Destruction plus aid

We first demonstrate the vital role of distance to the fault line on the effects of the earthquake in Figure 3. 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 under 26% farther than 20km away. These geographically concentrated effects are also evident in mortality and the destruction of public facilities; there is a notable decline after 20km and a full levelling off at close to (but not actually at) zero. Mortality rates, even very near the fault line, never exceeded 5% and dropped off to below 1% within 15km.

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

The second part of our "joint treatment" was the receipt of aid to households from public funds.[10] This aid was delivered through three programs: A cash transfer (PKR 24,000) distributed in multiple tranches, compensation for injuries (PKR 25,000-50,000) and death (PKR 100,000), and compensation for housing (PKR 150,000), conditional on the construction of earthquake-resistant structures. Figure 5 shows the total amount of aid received from these sources, plotted against the distance to the fault line. The immediate injection of liquidity averaged PKR 42,800 which is 43% of annual per-capita expenditures among households less 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 PKR 175,000 in the villages closest to the fault line, which exceeded 150% of the annual per capita expenditures among households more than 20km from the fault line.[11] The majority of this was housing compensation, which 86% of households within 20km from the fault line reported receiving. The non-parametric specification shows that the pattern of receipts mirrors the non-linear effects observed for house destruction and mortality—it decreases slowly till 20km from the fault line, declines sharply between 20 and 30km from the fault line, and then tapers off towards (but not quite at) zero.

Although the data quality and literature on the targeting and efficacy of compensation after disasters is limited (Morton and Levy 2011), our results echo findings that well-managed disasters appear to have both greater aid magnitudes and better individual appropriateness of compensation to damages. (Becerra et al 2014, de la Torre et al 2011) In this case, Wilder (2008,

2010) documents: "*[i]nitial criticism turned to praise, however, for the army's effective leadership of the subsequent relief phase, where its decision-making skills, logistical capacity, coordination skills, and willingness to listen and learn contributed to one of the most effective humanitarian responses ever to a large-scale natural disaster.*"

This is the "exogenous" part of our joint treatment: the variation in aid arising from distance to the fault line that is not correlated with pre-existing household and individual characteristics. We also investigated variation in aid receipts by households and, although 44% of total variation in aid receipts is within village, we find little evidence of differential aid by pre-existing household and individual characteristics and only a small and negligible correlation between the mother's or father's (or other adult's) education and the receipt of aid as well as the amount received (Table A2b).[12] We recognize that additional aid mismatches may be correlated with pre-existing characteristics of socioeconomic vulnerability, even if they are exogenous to exposure. (Domingue and Emrich 2019, Frankenberg et al 2013)

IV.B. 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. To account for imprecision in the estimates, we have also included minimum detectable effects for each outcome. The coefficient on distance to fault line is negative for the asset index as well as for in-home electricity, suggesting that, if anything, near-quake households were slightly wealthier than those farther away. The quality of housing stock was also significantly better in affected areas, with more households reporting a permanent dwelling with electricity and water in the home. Across the shock spectrum there is no difference in per capita expenditures based on a detailed household consumption survey.[13] In addition, Panel B report null results for access to all types of infrastructure, including distance to schools and health clinics. These estimates are fairly precise with MDEs ranging from 0.009 to 0.016 log minutes per kilometer.

Panel C examines adult heights and weights. Adult heights are of special interest in the age range from 18 to 24, as Deaton (2008) has shown that in South Asia, adverse conditions during childhood can delay the attainment of full adult height to the early 20s. 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, although in this case greater variability implies that MDEs are correspondingly larger, at 0.09 cm/km for young adult height and 0.07 kg/km for weight. Thus, we observe a recovery that has made the affected households indistinguishable from those living further away, if not better off in some aspects. We cannot claim that this is *due* to the aid flows included in the net earthquake effect, but at least for the housing component, it is likely that this aid was important.

IV.C. Children's human capital acquisition

We now investigate whether the earthquake, despite the evidence that there were no lasting effects among adults, still impacted human capital accumulation among children and whether these effects were age-dependent. We first investigate these relationships non-parametrically, focusing both on variation by distance to the fault line and by variation in age.

Figures 6a and 6b show, non-parametrically, the difference in child anthropometric outcomes by age for children located near the earthquake and for children far from it, split at the 20km mark for illustration of an average effect (19km is the mean distance in our sample). Here, we use age-standardized height and weights, using CDC charts as the appropriate reference (Vidmar et al.

2004); the mean distance to fault line for the under-20km group is 9.2km compared to 33.9km for the over 20km category. Appendix Figure A7 verifies that the broad patterns we discuss here are robust to alternate choices of the distance cutoff.

There is no evidence of current nutritional deficits measured through weight-for-age with distance from the fault line. The top panel shows a consistent worsening of weight-for-age as children grow older, but this deterioration is similar for children closer to and farther from the fault line at all ages. In the bottom panel we show a similar non-parametric relationship, but this time mapped continuously against distance to the fault line for children who were in-utero, newborn to two years old, and three or older; these are the age groups pertinent for our height results. We have demeaned all weights by subtracting the mean weight among those who were more than 20km from the fault line. Older children are slightly heavier near the fault line, but there is no difference in slopes; the largest possible magnitudes of difference are small; and confidence intervals overlap at almost all distances. Children's weight was not significantly affected by exposure to the earthquake.

By contrast, there are large and consistent differences in stature for children below the age of three at the time of the earthquake. Figure 6b again highlights height differences by age in the top panel (split by far from and close to the earthquake) and then plots variation by distance to the fault line in the bottom panel. Relative to the U.S., reference height-for-age follows a complex pattern, first by narrowing the gap and then diverging till age 11 (age seven at the time of the earthquake).[14] Unlike weight, there is a large difference by distance to the fault line for those who were in-utero or newborn to two years old at the time of the earthquake, and this gap then diminishes smoothly, with statistical significance disappearing around age three. The bottom panel highlights this pattern and in addition shows the same marked non-linearity we found with damage, destruction, and mortality—children suffered significant and similar deficits till they were 20km away from the fault line and after this, the disadvantage decreases rapidly. By 25-30km, the gap has disappeared. This trend fits with the observation that disruptions at earlier ages interrupt the periods of most rapid growth, and older children are unlikely to exhibit large growth shortfalls.

Turning to education (Figures 7a and 7b), we find no differences in enrollment by distance to the fault line across the age spectrum; regressions below confirm that this is the case for both girls and boys and extends to grade attainment in this population.[15] By contrast, there are large and consistent differences in standardized test scores 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.[16] Figure 7b again shows (this time separated by gender) that the test score deficits follow a similar non-linear pattern.

Table 3 presents the regression equivalent to these figures; we do not have power to detect the non-linearity discussed previously, and therefore focus on linear specifications only. 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 standard deviation over a 30km interval (Column 2). The impact on those aged 0-2 at the time of the quake is half that for those in utero (0.015SD/km) and significant at the 10% level (Column 2). Children over the age of 3 at the time of the earthquake, however, show no height loss at all. Neither do we find any adverse effect on weight-for-age in any age group (Column 1).

In terms of education effects, Table 3 first confirms that there are no impacts on enrollment or grade attainment (Columns 3 and 4). The test score deficits evident in the figures amount to 0.009 standard deviations per kilometer or 0.27 standard deviations over a 30km range (Column

5) with broadly similar results when repeated within each subject (Table A2c). Column 6 then looks at the role of school closures in mediating the test score losses. Figure 9 shows variation in the length of school disruptions and when we include this a right-hand-side variable, the main effect is slightly weaker (Column 6).[17] A mediation analysis (Hicks and Tingley 2012) estimates that school disruption accounts for 5.7% (95% CI: 4.1–9.8%) of the total distance-to-fault-line effect.[18] This effect of school disruption is itself approximately equivalent to pro-rated years of learning, so that 8 weeks of disruption lead to losses that are identical to 20% of the yearly gains we see in the control group (assuming 40 weeks of school a year). Nevertheless, the difference in test scores after four years is much larger than what we would have expected to see had school closures been the only channel. In fact, these results suggest that test score gaps continued to grow *after* children returned to school.

We report three additional results. First, in the case of test scores, additional specifications that include a full set of distance-to-fault-line interactions with gender and with age show no heterogeneity in impacts for either attribute (Columns 7 and 8). Second, Appendix Table A4a includes the total aid received as an additional variable in the height and test score regressions. The aid coefficient is imprecisely estimated and small in magnitude. We caution that these estimates are difficult to interpret, as 40% of the variation in aid is accounted for by geographical controls and the remaining variation reflects a combination of household-specific shocks that may be directly correlated with human capital formation (such as death, disability, delayed housing construction), measurement error, and genuine errors in allocation. Third, Appendix Tables A4b to A4e investigate the sensitivity of our results to the choice of controls and specifications. Table A4b investigates the removal of our current control vector; the height results are robust across all specifications while the test score losses are smaller (and imprecise) when geographic controls are excluded.[19] Table A4c investigates the use of the alternative Mercalli measure of earthquake exposure.[20] Table A4d shows that both height and test score results are robust to the inclusion of a full set of parental characteristics, including age, education, height and occupation, addressing potential concerns arising from fertility selection (Brown and Thomas 2018). Table A4e includes a control for local density (estimated by the number of children per school in each village); all results are less precisely estimated but coefficients are unchanged.

IV.D. Protective Mothers

Next, we examine the effect of educated mothers on their children's test scores and heights. Using nonparametric local polynomial estimates, Figure 8a shows that, among children whose mothers did not complete primary school education, the pattern of test score losses closely mirrors the pattern of destruction. We again see that scores are substantially lower than average and flat across the 15km band closest to the activated fault line. They then increase gradually between 15–25km from the fault, and level off across the rest of the study area. As with our other results, this nonlinear progression mirrors the geographic pattern of the disaster impacts and aid receipts that we documented earlier. However, the gradient is completely absent among children of mothers who completed primary school: their scores are flat across the entire proximity distribution, and therefore substantially higher than the scores of other children in the closest 15km to the activated fault line. Figure 8b produces the same comparison for the heights of children under three at the time of the earthquake; there is no similar gap or flattening among those with educated mothers.

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

IV.D.1. Instrumental Variables Strategy

Our OLS results suggest that maternal education mediated the effect of the earthquake on test scores. 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 to focus only on the causal effect of mother's education.

To identify 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 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 Punjab, 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 specification regresses maternal education on the availability of a girls' primary school in the mother's birth village by 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 nine. The Government of Pakistan guidelines use the age of six as the normal school starting age, but school availability at age eight is, in practice, a more reasonable indicator given the widespread practice of delayed enrolment. A cutoff age higher than that is probably inaccurate since the enrollment window for girls in rural Pakistan is quite small. Our estimation results are robust to small variations in the specific cutoff, although standard errors vary.

After the primary effect of interest, γ_i 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.

Schools may also 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 still does not address 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 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. In Section IV.F., we also provide additional results from an exacting specification that includes birth-village fixed effects. Since this specification requires multiple women to be born in the same village before and after the provision of a school, the precision of these estimates is lower, but reassuringly the results are qualitatively the same.

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

As a second stage, we then regress:

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

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.

The IV regression results reported in Table 4b are similar to those reported in the OLS estimation. The height estimates are still small and insignificant. They also make a stronger case that the protective effect of maternal education observed in the test score regression are causal and not driven by other characteristics which also increase the probability of a woman becoming educated, such as greater ability or effort. The results are substantially larger in magnitude, as is common in IV regression, and remain statistically significant. 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.E. Benchmarking the Effects

Our estimated effects are at the upper end of the range found in the literature, equivalent to the most extreme recorded events like the effects of the 1990 Rwandan Civil War and the 2009 Mongolian Dzud winter (Appendix Table A5). Although we do not know how these childhood disadvantages will translate into productivity in adulthood, we can make some educated guesses under two strong assumptions: (A): that the disadvantages we see in our sample continue to adulthood in relative terms (so a child a given height percentile in childhood will remain there in adulthood), and (B): that estimates on the relationship between wages, schooling and height from Pakistan are relevant to this sample. Under these assumptions, we can use estimates from Pakistan, which suggest a 10% return to each year of schooling (Montenegro and Patrinos 2014) and 3% for every centimeter in height (Bossaive et al. 2017), to calibrate the wage equivalent of human capital losses among the children in our sample.

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

IV.F. Threats to Identification

We assess four major potential sources of contamination: (1) self-selection of households into risk exposure by fault line proximity; (2) selective missingness due to mortality, migration, or unavailability; (3) IV sensitivity to outliers; and (4) potential sources of bias due to endogenous placement of girls' schools using birth-village fixed effects.

First, to assess potential selection of risk profiles into proximity to the activated fault line, we included in every specification a variable for proximity to the *nearest* fault line to control for potential selection into risk exposure. No household is more than 11.3km from some fault line, and 50% live within 2.5km of some fault line. To investigate further whether selective location decisions could reproduce our results, we conducted a placebo test by performing identical test scores and education distance-to-fault-line regressions with respect to each fault line in our data (controlling for the true location of the activated fault line). We treated 50 other possible fault lines as though they were the location of the shock, testing the distribution of these effect sizes under the null hypothesis of "no earthquake". Appendix Figure A3 illustrates the joint distribution of these coefficients, with our results plotted for reference, as well as the 95th percentile boundary of the estimated joint distribution. The placebo distribution shows that large positive height coefficients appear in combination with large positive test score coefficients for only one specification other than the activated Balakot-Bagh fault line. Thus, outcomes for the observed activated fault lines lie at the 98th percentile ($p<0.02$) relative to the joint distribution of placebo effects for both learning and anthropomorphic impacts (Alexandersson 1998, Alexandersson 2004, McCartin 2003).

Second, we simulate unfavorable assumptions about selective missingness due to mortality, migration, and unavailability at survey time to investigate the robustness of our primary shock outcomes. 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 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 out 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 is more selective; 4.5% excess observations are trimmed and the worst-case assumptions lead to a point estimate of -0.33 SD with p=0.134, compared to an unadjusted estimate of -0.70 SD (Table A2d).

Third, following Young (2020) we assess the sensitivity of our IV estimates for maternal mitigation to outliers. Since our IV regression estimates demonstrate the high variance typical of such specifications, we re-estimate the maternal-education interaction IV regression, systematically excluding each one of our 124 clusters (villages) from the full IV regression (one of the 125 survey villages had no tested children with maternal information). Figure A5 demonstrates that our results are robust to this procedure, plotting the distribution of the 124 mitigation coefficients obtained this way. Thirteen clusters result in estimates in which the 95% confidence interval includes zero, but these results are mainly due to increased variance than an attenuated coefficient – in fact, most of these estimate a larger mitigation coefficient than our full-sample specification.

Finally, the use of Tehsil fixed effects could still miss some of the endogenous variation in placement of schools. Our final robustness check (Table A3c) includes birth-village fixed effects and replicates the specifications from Columns 1 and 2 in Table 4a and Table 4b. The variation in the data now comes from a smaller sample: 100 of 229 birth villages have only one mother; and 79 more have multiple mothers but no variation in the availability of the school at their enrollment ages, leaving 50 birth villages with 404 mothers of 690 children to supply the identifying variation. The reduced-form specification shows similar results in terms of estimated coefficients, although the precision declines with the reduction in effective sample size.

V. Channels

Our results on the impact of the earthquake on human capital acquisition among children as well as the ability of educated mothers to mitigate test score losses are silent on the potential mechanisms. Disentangling and directly measuring the impact of the shock and of maternal education on "*the production function as well as the production process*" (Behrman 1997) requires more data and precise information on the interaction between shocks, child age, household inputs, and developmental lags (Das et al 2013, Malamud et al 2016).

One channel we were particularly interested in was whether the ability of educated mothers to protect their children from test score losses reflects their ability to switch schools after the earthquake. In Table A3d we restrict our test scores sample to villages which had only one schooling option for children, to rule out school switching as a mechanism through which maternal education had an effect on child learning. These regressions suggest that the maternal education mitigation effect is even stronger in this sample than in the overall sample, although the sample size is much smaller and the IV has a very weak first stage in this restricted sample. This suggests that educated mothers were better able to handle school disruptions and compensate for a decline in the availability or quality of schooling inputs, rather than (for example) having the knowledge or resources to switch children into better or less affected schools after the earthquake.

In Table A3e, we investigate three further potential mitigating factors—maternal stress, household elevation, and household assets. Currie and Rossin-Slater (2013) and Lauderdale (2006) have demonstrated the nuanced role of maternal stress on child outcomes in the United States. Both studies are able to use exogenous events (hurricanes and the September 11th attacks) and populations that were arguably unaffected except through higher stress levels to causally identify a link between maternal stress and child outcomes. In our case, mothers were affected in multiple ways and the lack of any baseline data makes it harder to draw firm conclusions. However, we completed a mental health questionnaire with mothers in our endline that focused on depression and anxiety using the GSQ 12-item inventory. These results, while not causal, again suggest that mediating factors for education and health may be very different. Column 1 through 3 shows that neither of these three factors mitigated the test score losses, but Columns 4 though 6 show significant mitigation for maternal mental health and household elevation. Much as in our regressions for maternal education, the factors that mitigate against height losses are very different from those that mitigate against test scores, suggesting a greater possibility for biological channels in the case of the former.

VI. Conclusion

Early childhood deprivation can lead to significant interruptions in the accumulation of human capital even when households receive significant compensation and adult outcomes recover to parity. Height effects are concentrated among children who were in the critical 1,000 days of life at the time of the shock, but that test score effects are across the age spectrum. Finally, educated mothers are able to mitigate learning losses, but not height losses. We conclude that even a disaster followed by compensation has the potential to permanently scar children and increase inequalities across regions and within the areas that were subject to the shock. In conclusion, we emphasize three important points.

First, school closures alone cannot have accounted for the loss in test scores, so that children in the earthquake affected regions must have *learnt less* every year after returning to school. We do not know why this is the case. One possibility is that every child had to be promoted in the new school year, and if teachers taught to the curriculum in the new grade, they could have fallen farther behind. An influential literature suggests that teaching at a higher level compared to where children are reduces how much children learn, and this is a potential channel for our results (Banerjee et al. 2016). Detecting these types of losses can take time and it is possible that the immediate effects on children's test scores following a disaster may under-estimate the longer-term deficits.

Second, our study does not imply that height losses cannot be mitigated. Gunnsteinsson et al. (2014) have shown that children who were part of a Vitamin A trial when a typhoon struck in Bangladesh were fully protected in terms of their height losses. Similarly, in Aceh after the Tsunami, the large volume of aid allowed the worst-affected children to fully catch up (and even outgrow) children who lived farther away. Maternal mental health also appears to play a role in cushioning against height effects in very young children. This result, while not causal, points to the attraction of programs that provide mental health counselling to mothers with young children. See Baranov et al. (2020).

Finally, these results are from a specific disaster in a specific geographic, economic, and social context. Therefore the external validity of these findings to other disasters remains an open question. Our own thinking on this issue is guided by fact that the ultimate impact of disasters will depend on the extent of ex-post and ex-ante responses to the shock. In this case, the unpredictability of earthquakes and long period of calm implies that ex-ante responses in the form of precautionary investments were small; we have argued that ex-post migratory responses were also limited. This might explain why the effects on child height are so large and comparable to some of the worst humanitarian disasters in sub-Saharan Africa. The key point that we make is that compensation in the acute disaster period itself is insufficient to halt the propagation of the shock to future life outcomes through investment in human capital. These losses appear in a setting where policymakers think they *have* compensated for the disaster in an immediate sense, but have turned out to have missed longer-term recovery needs. To the extent that other disasters are not discrete events but the emergence of a permanent, ongoing season of deprivation and coping, the effects may be similar to what we identify here, with implications for long-term growth as well as the evolution of inequality in the region.

References

Akresh, Richard, Leonardo Lucchetti, and Harsha Thirumurthy. 2020. “Wars and child health: Evidence from the Eritrean–Ethiopian conflict.” *Journal of Development Economics* 99(2): 330-340.

Akresh, Richard, Philip Verwimp, and Tom Bundervoet. 2011. “Civil war, crop failure, and child stunting in Rwanda.” *Economic Development and Cultural Change* 59(4): 777-810.

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

__________. 2004. “Graphing confidence ellipses: An update of ellip for Stata 8.” *Stata Journal* 4(3): 242-256.

Alderman, Harold, John Hoddinott, and Bill Kinsey. 2006. “Long term consequences of early childhood malnutrition.” *Oxford Economic Papers* 58(3): 450-474.

Andrabi, Tahir, and Jishnu Das. 2017. “In aid we trust: Hearts and minds and the Pakistan earthquake of 2005.” *Review of Economics and Statistics* 99(3): 371-386.

Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja. 2012. “What did you do all day? Maternal education and child outcomes.” *Journal of Human Resources* 47(4): 873-912.

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

Baez, Javier, Alejandro de la Fuente and Indhira Vanessa Santos. 2010. “Do Natural Disasters Affect Human Capital? An Assessment Based on Existing Empirical Evidence.” IZA Discussion Paper No. 5164.

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

Baranov, Victoria, Sonia Bhalotra, Pietro Biroli, and Joanna Maselko. 2020. “Maternal depression, women’s empowerment, and parental investment: Evidence from a randomized controlled trial.” *American Economic Review* 110(3): 824-59.

Becerra, Oscar, Eduardo Cavallo, and Ilan Noy. 2014. “Foreign aid in the aftermath of large natural disasters." *Review of Development Economics* 18(3): 445-460.

Behrman, Jere R. 1997. “Mother's schooling and child education: A survey.” PIER Working Paper 97-25. Philadelphia, PA: Penn Institute for Economic Research.

Behrman, Jere R., and John Hoddinott. 2005. “Programme evaluation with unobserved heterogeneity and selective implementation: The Mexican PROGRESA impact on child nutrition.” *Oxford Bulletin of Economics and Statistics* 67(4): 547-569.

Berkman, Douglas S., Andres G. Lescano, Robert H. Gilman, Sonia L. Lopez, and Maureen M. Black. 2002. "Effects of stunting, diarrhoeal disease, and parasitic infection during infancy on cognition in late childhood: a follow-up study." *The Lancet* 359(9306): 564-571.

Bharadwaj, Prashant, Juan Pedro Eberhard, and Christopher A. Neilson. 2018. "Health at birth, parental investments, and academic outcomes." *Journal of Labor Economics* 36(2): 349-394.

Black, Sandra E., Aline Bütikofer, Paul J. Devereux, and Kjell G. Salvanes. 2019. "This is only a test? Long-run and intergenerational impacts of prenatal exposure to radioactive fallout." *Review of Economics and Statistics* 101(3): 531-546.

Bossavie, Laurent, Harold Alderman, John Giles, and Cem Mete. 2017. "The effect of height on earnings: Is stature just a proxy for cognitive and non-cognitive skills?" World Bank Policy Research Working Paper No. 8254.

Bozzoli, Carlos, Angus Deaton, and Climent Quintana-Domeque. 2009. "Adult height and childhood disease." *Demography* 46(4): 647-669.

Brown, Ryan, and Duncan Thomas. 2018. "On the long term effects of the 1918 U.S. influenza pandemic." BREAD, the Bureau for Research and Economic Analysis of Development.

Bundervoet, Tom, Philip Verwimp, and Richard Akresh. 2009. "Health and civil war in rural Burundi." *Journal of Human Resources* 44(2): 536-563.

Card, David. 1999. "The causal effect of education on earnings." In *Handbook of Labor Economics*, ed. Orley C. Ashenfelter and David Card: 1801-1863. Elsevier.

Cas, Ava Gail, Elizabeth Frankenberg, Wayan Suriastini, and Duncan Thomas. 2014. "The impact of parental death on child well-being: evidence from the Indian Ocean tsunami." *Demography* 51(2): 437-457.

Case, Anne, and Christina Paxson. 2010. "Causes and consequences of early-life health." *Demography* 47(1): S65-S85.

Currie, Janet, and Enrico Moretti. 2003. "Mother's education and the intergenerational transmission of human capital: Evidence from college openings." *The Quarterly Journal of Economics* 118(4): 1495-1532.

Currie, Janet, and Maya Rossin-Slater. 2013. "Weathering the storm: Hurricanes and birth outcomes." *Journal of Health Economics* 32(3): 487-503.

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

Deaton, Angus. 2008. "Height, health, and inequality: the distribution of adult heights in India." *American Economic Review* 98(2): 468-74.

de la Torre, Luis E., Irina S. Dolinskaya, and Karen R. Smilowitz. 2012. "Disaster relief routing: Integrating research and practice." *Socio-Economic Planning Sciences* 46(1): 88-97

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

Dercon, Stefan, and Catherine Porter. 2014. "Live aid revisited: long-term impacts of the 1984 Ethiopian famine on children." *Journal of the European Economic Association* 12(4): 927-948.

Deryugina, Tatyana, Laura Kawano, and Steven Levitt. 2018. "The economic impact of Hurricane Katrina on its victims: evidence from individual tax returns." *American Economic Journal: Applied Economics* 10(2): 202-33.

Domingue, Simone J., and Christopher T. Emrich. 2019. "Social vulnerability and procedural equity: Exploring the distribution of disaster aid across counties in the United States." *The American Review of Public Administration* 49(8): 897-913.

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

Figlio, David, Jonathan Guryan, Krzysztof Karbownik, and Jeffrey Roth. 2014. "The effects of poor neonatal health on children's cognitive development." *American Economic Review* 104(12): 3921-55.

Frankenberg, Elizabeth, Bondan Sikoki, Cecep Sumantri, Wayan Suriastini, and Duncan Thomas. 2013. "Education, vulnerability, and resilience after a natural disaster." *Ecology and Society: A journal of integrative science for resilience and sustainability* 18(2): 16.

Glewwe, Paul, and Elizabeth M. King. 2001. "The impact of early childhood nutritional status on cognitive development: Does the timing of malnutrition matter?" *The World Bank Economic Review* 15(1): 81-113.

Glewwe, Paul, Hanan G. Jacoby, and Elizabeth M. King. 2001. "Early childhood nutrition and academic achievement: a longitudinal analysis." *Journal of Public Economics* 81(3): 345-368.

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

Handa, Sudhanshu, and Amber Peterman. 2016. "Is There Catch-Up Growth? Evidence from Three Continents." *Oxford Bulletin of Economics and Statistics* 78(4): 470-500.

Hicks, Raymond, and Dustin Tingley. 2011. "Causal mediation analysis." *The Stata Journal* 11(4): 605-619.

Hoddinott, John, and Bill Kinsey. 2001. "Child growth in the time of drought." *Oxford Bulletin of Economics and Statistics* 63(4): 409-436.

Lauderdale, Diane S. 2006. "Birth outcomes for Arabic-named women in California before and after September 11." *Demography* 43(1): 185-201.

Lee, David S. 2009. "Training, wages, and sample selection: Estimating sharp bounds on treatment effects." *The Review of Economic Studies* 76(3): 1071-1102.

Maccini, S. Yang. 2009. "Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall." *American Economic Review* 99(3): 1006-26.

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

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

McIntosh, Molly Fifer. 2008. "Measuring the labor market impacts of Hurricane Katrina migration: Evidence from Houston, Texas." *American Economic Review* 98(2): 54-57.

Morton, Melinda, and J. Lee Levy. 2011. "Challenges in disaster data collection during recent disasters." *Prehospital and Disaster Medicine* 26(3): 196.

Paxson, Christina, and Norbert Schady. 2010. "Does money matter? The effects of cash transfers on child development in rural Ecuador." *Economic Development and Cultural Change* 59(1): 187-229.

Pörtner, Claus. 2010. "Natural Hazards and Child Health." Working paper. Seattle University, Seattle, WA. Available at SSRN: 1599432.

Sawada, Yasuyuki, and Satoshi Shimizutani. 2008. "How do people cope with natural disasters? Evidence from the Great Hanshin-Awaji (Kobe) Earthquake in 1995." *Journal of Money, Credit and Banking* 40(2-3): 463-488.

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

Shrimpton, Roger, Cesar G. Victora, Mercedes de Onis, Rosângela Costa Lima, Monika Blössner, and Graeme Clugston. 2001. "Worldwide timing of growth faltering: Implications for nutritional interventions." *Pediatrics* 107(5): e75.

Strauss, John, and Duncan Thomas. 1998. "Health, nutrition, and economic development." *Journal of Economic Literature* 36(2): 766-817.

Tauchmann, Harald. 2014. "Lee (2009) treatment-effect bounds for nonrandom sample selection." *The Stata Journal* 14(4): 884-894.

Vidmar, Suzanna, John Carlin, Kylie Hesketh, and Tim Cole. 2004. "Standardizing anthropometric measures in children and adolescents with new functions for egen." *The Stata Journal* 4(1): 50-55.

Weldeegzie, Samuel G. 2017. "Growing-up unfortunate: War and human capital in Ethiopia." *World Development* 96: 474-489.

Wilder, Andrew. 2008. "Perceptions of the Pakistan earthquake response." *Feinstein International Centre*.

__________. 2010. “Aid and stability in Pakistan: lessons from the 2005 earthquake response.” *Disasters* 34: S406-S426.

Young, Alwyn. 2020. “Consistency without inference: Instrumental variables in practical application.” Unpublished manuscript, London School of Economics.

Tables

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 Line (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 Fault Line (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

Notes: Distance to fault line and epicenter are calculated using the Haversine formula. The Standard Census is conducted among all households in the final sample of 126 villages. The Extended Census is conducted among a randomly selected subset of 6,455 households. The Household Survey is conducted among a randomly selected subset of 2,456 households. The Household Asset Index is the first principal component of household assets recorded in the Household Survey, among beds/charpais, tables, chairs, fans, sewing machines, books, refrigerators, radio/cassette recorder/CD players, televisions, VCR/VCDs, watches, guns, plows, tractors, tube well/hand pumps, other agricultural machinery, other agricultural hand tools/saws, motorcycle/scooters, car/taxi/vehicles, bicycles, cattle, goats, chickens, and mobile phones.

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

	(1) Distance to Fault Line Coefficient	(2) N	(3) R2	(4) Mean	(5) MDE
Villages (1998 Village Census)					
Total Population	-18.377	126	0.186	1,988.222	55.414
	19.625				
Male Population	-9.412	126	0.182	989.214	28.342
	10.037				
Female Population	-8.965	126	0.189	999.008	27.172
	9.623				
Muslim Population	-18.273	126	0.186	1,981.738	55.182
	19.543				
Literacy Rate	-0.000	125	0.401	0.457	0.003
	0.001				
Proportion with Primary Education	-0.002*	126	0.354	0.389	0.002
	0.001				
Proportion Females with Secondary Education	-0.000	126	0.143	0.025	0.001
	0.000				
Average Household Size	-0.024**	126	0.252	6.848	0.031
	0.011				
Number of Permanent Houses	-0.755	120	0.200	127.500	3.555
	1.259				
Number of Houses with Electricity	-2.324	112	0.130	189.670	5.731
	2.028				
Number of Houses With Potable Water	-1.269	100	0.167	60.800	2.749
	0.971				
Village Infrastructure Index	-0.013	126	0.154	0.397	0.025
	0.009				
Adults 18+ During Survey (2009 Household Census and Survey)					
Male Height (cm)	0.020	2,735	0.020	167.512	0.075

	0.027				
Female Height (cm)	0.046**	2,834	0.007	157.164	0.064
	0.023				
Male Age (Living)	0.008	36,755	0.001	36.554	0.028
	0.010				
Female Age (Living)	0.026**	33,273	0.002	35.052	0.028
	0.010				
Males Completed Primary School (Living)	-0.000	44,495	0.025	0.636	0.003
	0.001				
Females Completed Primary School (Living)	-0.002	40,474	0.024	0.315	0.004
	0.001				
Male Age (Deceased)	0.268***	1,459	0.066	56.883	0.222
	0.079				
Female Age (Deceased)	0.151*	950	0.115	45.609	0.248
	0.088				
Males Completed Primary School (Deceased)	0.000	75	0.079	0.280	0.013
	0.005				
Females Completed Primary School (Deceased)	-0.004	71	0.074	0.239	0.011
	0.004				
Male Age (All)	0.018*	38,214	0.001	37.330	0.029
	0.010				
Female Age (All)	0.024**	34,223	0.002	35.345	0.026
	0.009				
Males Completed Primary School (All)	-0.000	44,570	0.025	0.635	0.003
	0.001				
Females Completed Primary School (All)	-0.002	40,545	0.026	0.315	0.004
	0.001				
Households (2009 Household Survey)					

Recall					
Electricity in House	-0.009***	2,456	0.108	0.854	0.007
	0.002				
Water In House	-0.003	2,456	0.042	0.445	0.006
	0.002				
Permanent House	-0.003	2,456	0.103	0.380	0.006
	0.002				
Distance to Closest Market (min)	0.237	2,452	0.089	54.675	0.948
	0.336				
Distance to Closest Water Source (min)	0.056	2,456	0.030	9.660	0.145
	0.051				
Distance to Closest Medical Facility (min)	-0.086	2,444	0.069	57.861	0.820
	0.290				
Distance to Closest Private School (min)	-0.112	2,372	0.039	44.565	0.709
	0.251				
Distance to Closest Government School (min)	0.022	2,454	0.035	21.047	0.239
	0.085				
Measured					
Distance to Closest Water Source (km)	0.052	2,456	0.215	3.056	0.098
	0.035				
Distance to Closest Health Clinic (km)	0.122***	2,456	0.344	5.361	0.122
	0.043				
Distance to Closest Private School (km)	0.102**	2,456	0.255	3.355	0.127
	0.045				
Distance to Closest Boys School (km)	0.090*	2,456	0.251	1.131	0.141
	0.050				
Distance to Closest Girls School (km)	0.009	2,456	0.047	1.290	0.064
	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 line is reported, along with the number of observations, the r-squared, and the overall mean of the variable. All regressions include 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. For all regressions, we report the absolute value of the minimum detectable effect size at 80% power, calculated as the center of the t-distribution for which 80% of the probability mass falls outside the critical 5% value determined by the standard error and degrees of freedom of the corresponding point estimate.

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

	(1) Distance to Fault Line (km) Coefficient	(2) N	(3) R2	(4) Mean	(5) MDE
PANEL A: Household Socioeconomic Characteristics					
Asset Index (PCA) (Post-Quake)	-0.004	2,456	0.122	0.002	0.011
	0.004				
Household Infrastructure Index	-0.024***	2,456	0.168	0.000	0.016
	0.006				
Permanent House (Post-Quake)	-0.005**	2,456	0.089	0.635	0.006
	0.002				
Electricity	-0.008***	2,456	0.142	0.904	0.006
	0.002				
Water In House (Post-Quake)	-0.005*	2,456	0.057	0.498	0.007
	0.003				
Log Consumption per Capita	0.003	2,456	0.072	10.038	0.007
	0.003				
PANEL B: Access to Public Infrastructure					
Log Dist to Gov't School (min)	-0.004	2,454	0.039	2.781	0.009
	0.003				
Log Dist to Market (min)	0.004	2,452	0.119	3.625	0.016
	0.006				
Log Dist to Distr Office (min)	-0.005	2,449	0.240	4.834	0.013
	0.005				
Log Dist to Medical (min)	-0.003	2,444	0.048	3.789	0.014
	0.005				
Log Dist to Private School (min)	-0.006	2,369	0.037	3.396	0.016
	0.006				
PANEL C: Adult Health					
Adult Height	0.034	6,907	0.295	145.318	0.063

	0.022				
Adult Weight	0.027 0.020	6,907	0.188	45.592	0.057
Adult Height (18-24)	0.011 0.033	1,717	0.248	130.253	0.092
Adult Weight (18-24)	0.029 0.026	1,717	0.188	34.121	0.074

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 line is reported, along with the number of observations, the r-squared, and the overall mean of the variable. All regressions include controls for distance to the earthquake epicenter, local slope, distance to the nearest fault line, and district fixed effects. The adult health regressions include age and sex indicator variables. Measured distance to water is replaced by zero when recall survey notes that water was available in the house. For all regressions, we report the absolute value of the minimum detectable effect size at 80% power, calculated as the center of the t-distribution for which 80% of the probability mass falls outside the critical 5% value determined by the standard error and degrees of freedom of the corresponding point estimate.

Table 3. Child Human Capital Acquisition After the Earthquake

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Weight (Z-score)	**Height (Z-score)**	**School Enrollment**	**Grade Attainment**	**Test Scores (IRT)**	**Test Scores + Disruption**	**Test Scores + Gender**	**Test Scores + Age**
Distance from Fault Line (km)	-0.007*	0.002	0.000	0.003	0.009**	0.007*	0.008	0.013***
	(0.004)	*(0.005)*	*(0.001)*	*(0.008)*	*(0.004)*	*(0.003)*	*(0.005)*	*(0.005)*
Weeks out of School After Earthquake						-0.004*		
						(0.002)		
In Utero * Distance from Fault Line (km)	0.003	0.036**						
	(0.006)	*(0.017)*						
Age 0-2 * Distance from Fault Line (km)	0.005	0.015*						
	(0.005)	*(0.009)*						
Male	-0.041	0.034	0.077***	0.120	0.067	-0.000	0.041	0.065
	(0.048)	*(0.082)*	*(0.016)*	*(0.107)*	*(0.044)*	*(0.045)*	*(0.074)*	*(0.044)*
Distance from Fault Line							0.001	

(km) * Male		
	(0.004)	
Distance from Fault Line (km) * Age 6		-0.005
		(0.004)
Distance from Fault Line (km)* Age 7		-0.003
		(0.005)
Distance from Fault Line (km)* Age 8		-0.007
		(0.005)
Distance from Fault Line (km)* Age 9		0.005
		(0.005)
Distance from Fault Line (km)* Age 10		-0.009*
		(0.004)
Distance from Fault Line		-0.008

(km)* Age 11								
								(0.006)
Dependent Variable Mean	*-0.944*	*-2.155*	*0.903*	*4.173*	*0.131*	*0.229*	*0.131*	*0.131*
Regression R2	0.247	0.077	0.071	0.335	0.089	0.102	0.089	0.094
Number of Observations	4,002	4,001	1,874	1,875	1,875	1,547	1,875	1,875
Geographic Controls	X	X	X	X	X	X	X	X
Age Dummies	X	X	X	X	X	X	X	X

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 line. 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 Fault Line (km)	0.008**	0.009**	0.017	0.017
	(0.004)	*(0.004)*	*(0.012)*	*(0.013)*
Mother Completed Primary School	0.299***	0.422***	0.103	0.050
	(0.051)	*(0.078)*	*(0.227)*	*(0.335)*
Mother's Education * Distance		-0.007**		0.003
		(0.004)		*(0.017)*
Male	0.066	0.065	-0.151	-0.151
	(0.043)	*(0.043)*	*(0.167)*	*(0.167)*
Dependent Variable Mean	*0.131*	*0.131*	*-1.676*	*-1.676*
Regression R2	0.105	0.107	0.030	0.030
Number of Observations	1,875	1,875	1,423	1,423
Geographic Controls	X	X	X	X
Age Dummies	X	X	X	X

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 line. These regressions specifically examine the potential for mitigation by maternal education, and include the level effect and the fault line 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

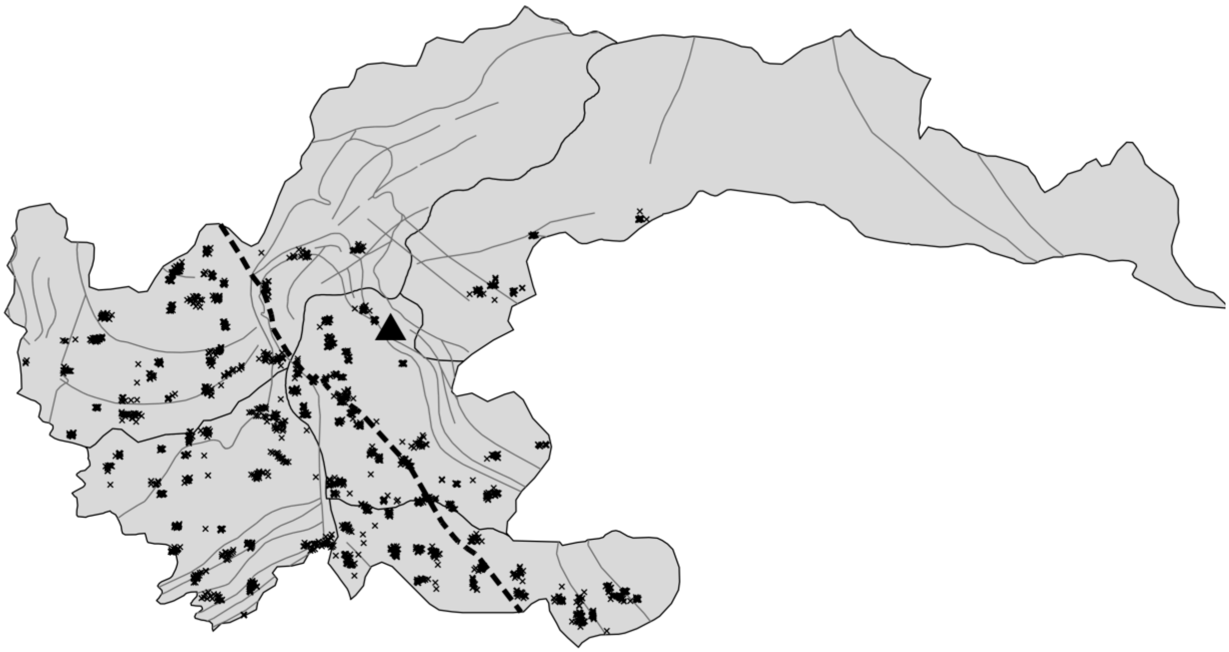
Table 4b. Maternal Education Effects (Instrumental Variables)

	(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 Fault Line (km)	0.017***	0.029***	0.009	-0.015
	(0.006)	(0.009)	(0.024)	(0.039)
Mother Completed Primary School	1.627***	4.913**	-3.488*	-5.398
	(0.553)	(1.987)	(1.906)	(3.857)
Mother's Education * Distance		-0.143**		0.096
		(0.071)		(0.133)
Male	0.059	0.041	-0.231	-0.257
	(0.047)	(0.059)	(0.176)	(0.186)
Dependent Variable Mean	*0.135*	*0.135*	*-1.657*	*-1.657*
Number of Observations	1,723	1,723	1,275	1,275
Cragg-Donald F-statistic	39.905	8.644	36.570	11.925
Geographic Controls	X	X	X	X
Maternal Controls	X	X	X	X
Age Dummies	X	X	X	X

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 line. These regressions specifically examine the potential for mitigation by maternal education using an IV specification, and include the level effect and the fault line 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

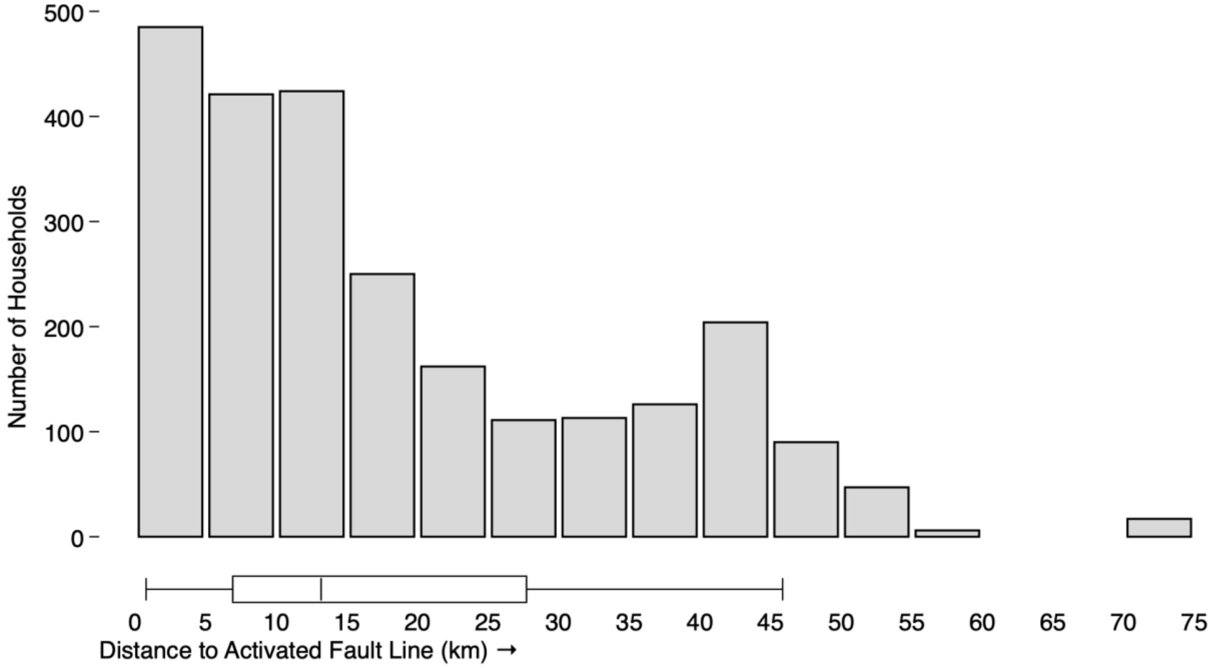
Figures

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



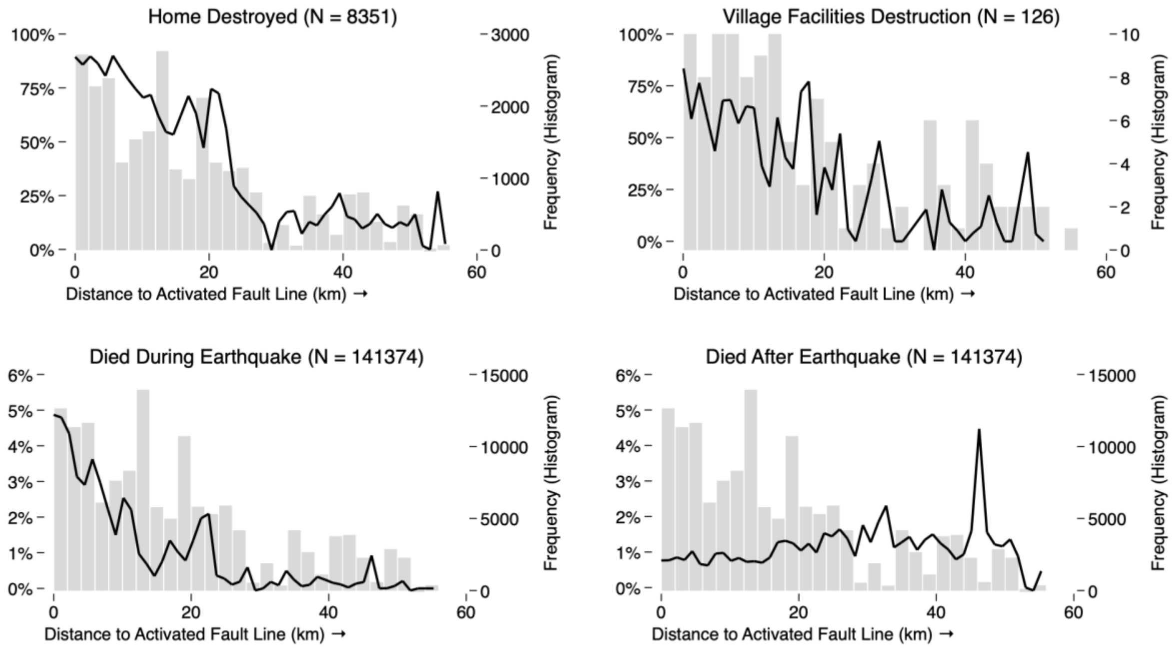
Notes: This map illustrates the location of all 2,456 households that completed the detailed household survey (X's), the location of the activated Balakot-Bagh Fault (thick dashed line), and the earthquake epicenter (black triangle). Current district boundaries are shown as thin solid black lines (Neelum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

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



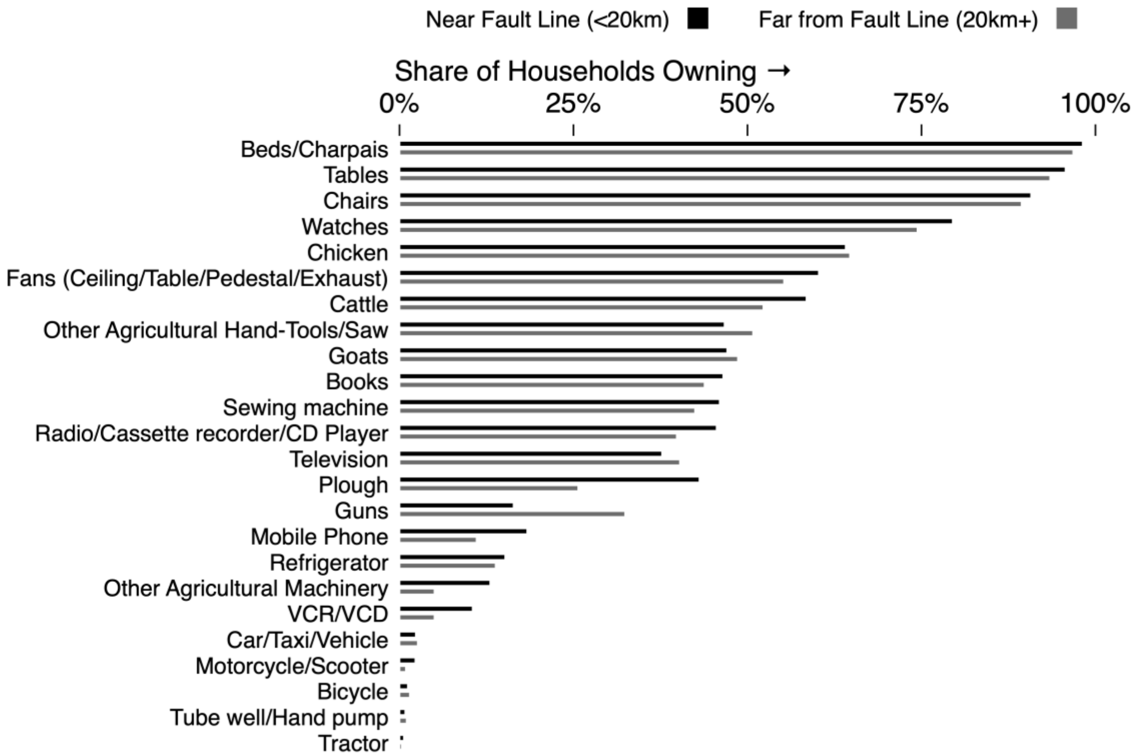
Notes: This figure illustrates the distance distribution of the 2,456 households from the detailed survey to the activated fault line (histogram), as the number of households in each 5km bin as well as the 5^{th}, 25^{th}, 50^{th}, 75^{th}, and 95^{th} percentiles of the distribution (box plot).

Figure 3. Immediate and extended earthquake deaths and destruction



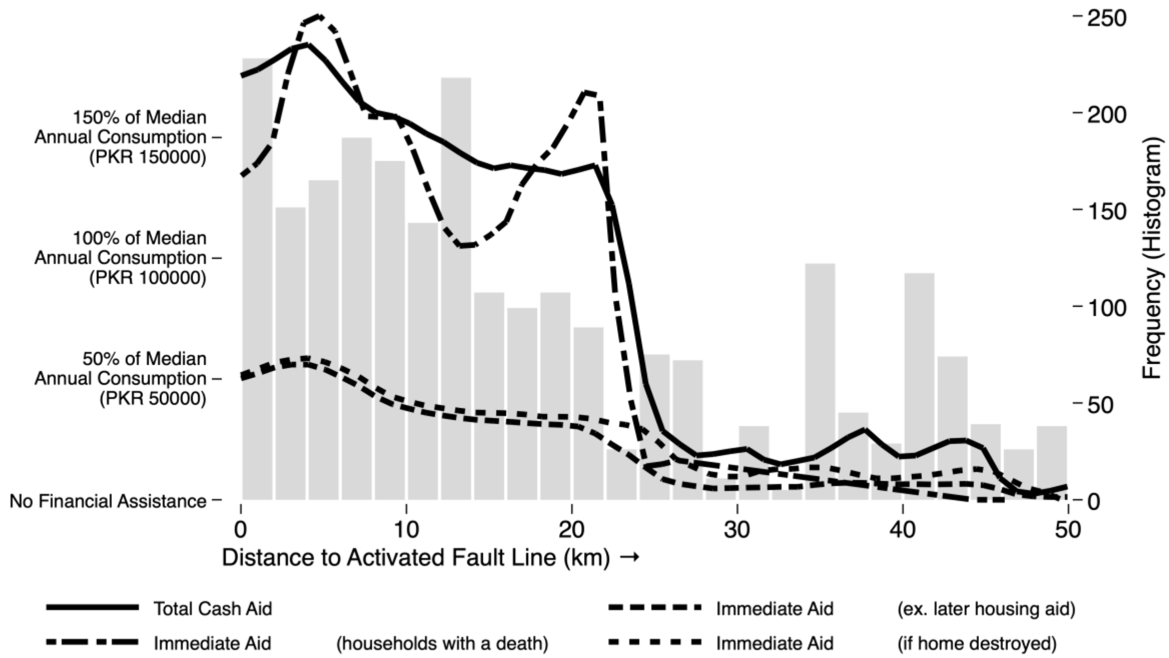
Notes: These plots illustrate the proportion of homes reported destroyed in both long census and detailed survey measures; the proportion of public infrastructure noted destroyed in village survey; and the proportion of census records reported deceased during and/or after the earthquake, as a non-parametric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

Figure 4. Pre-earthquake assets comparison



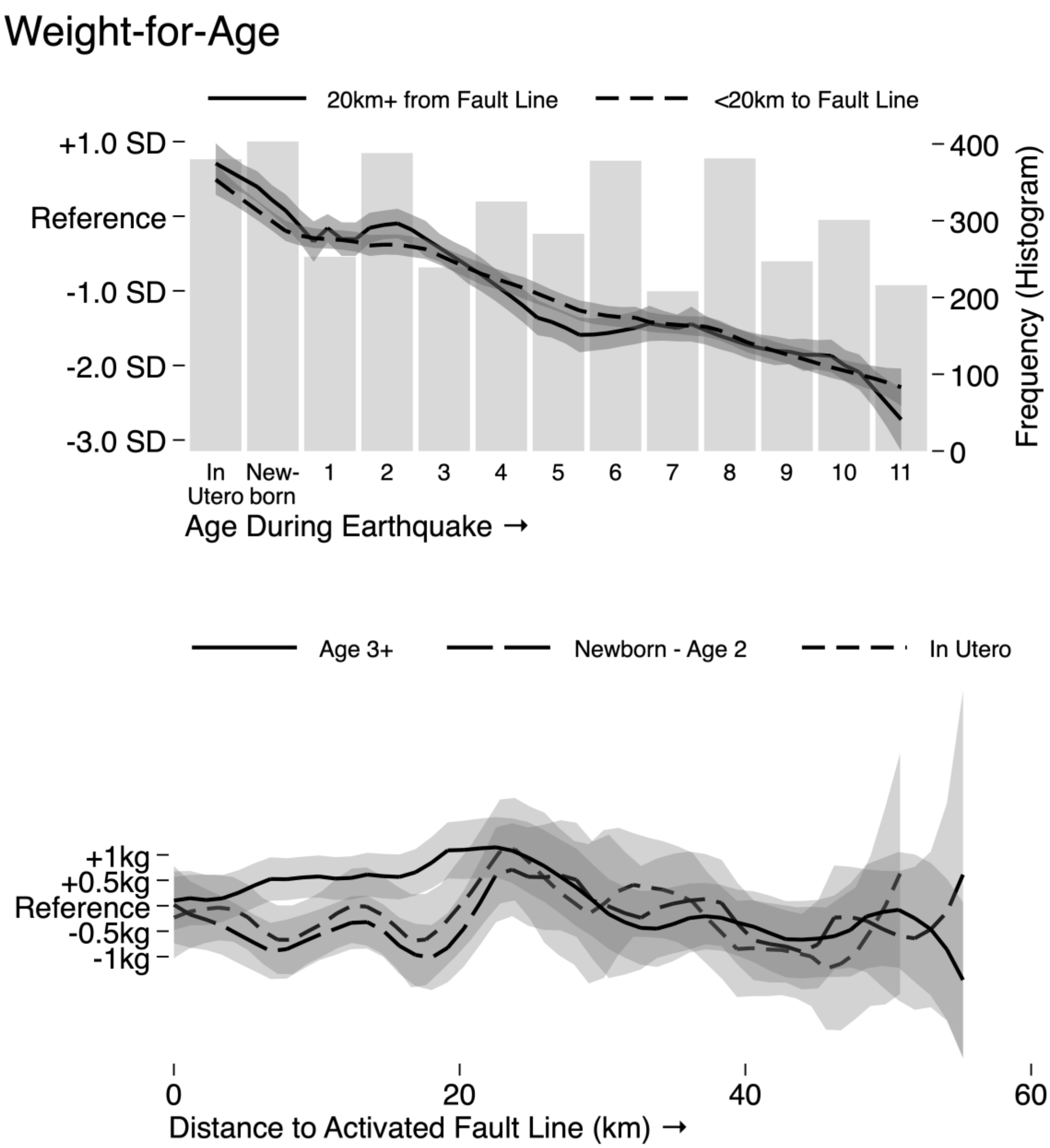
Notes: This figure tabulates the proportion of households that self-reported ownership rates (prior to the earthquake) of the assets in our wealth index are compared between near-fault-line (<20km) and far-from-fault-line (20km+) households.

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



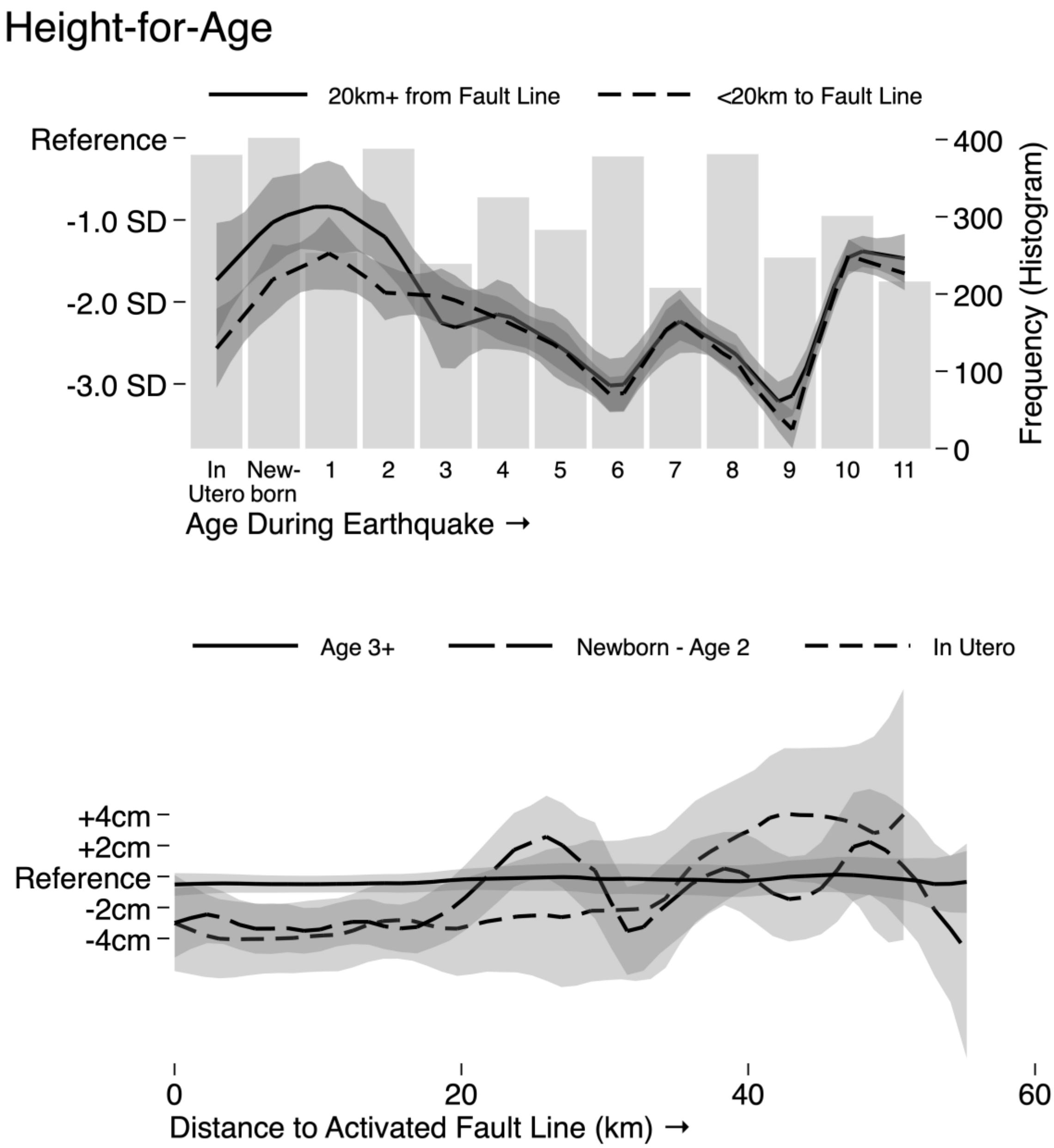
Notes: This figure illustrates self-reported aid received by households as a function of distance to the activated fault line, split into total aid (full recovery period) and immediate aid (non-rebuilding aid) for all households, households with a death, and households that reported home destruction.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

Figure 6a. Weight outcomes for children



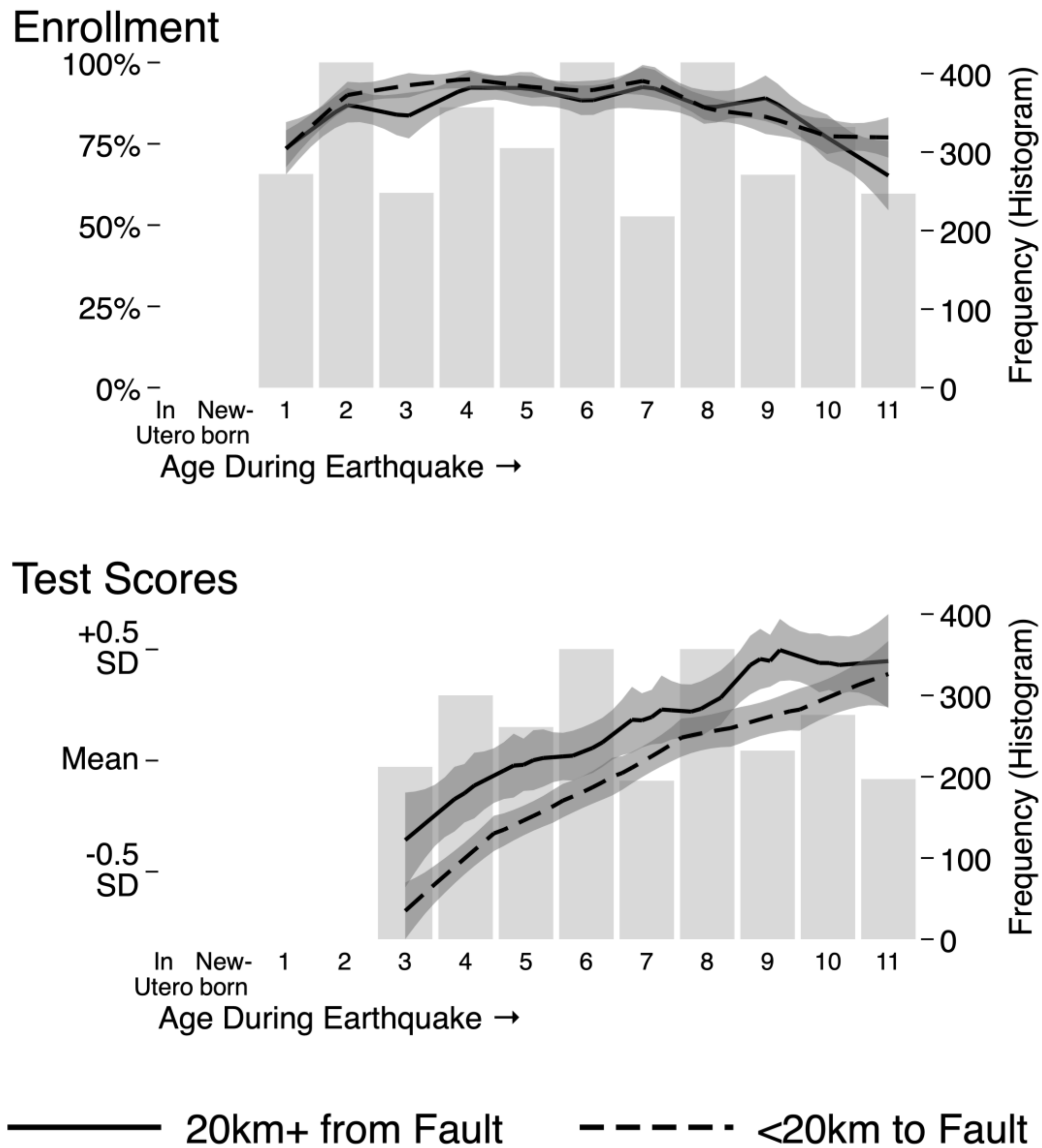
Notes: As a function of age at the time of the earthquake and distance to the activated fault line, these graphs compare the current weight-for-age (in both z-scores and kg) of children covered in the detailed survey between near-fault-line and far-from-fault-line groups using nonparametric specifications. The histogram shows relative density as the number of children at each age. Shaded areas show 95% confidence intervals.

Figure 6b. Height outcomes



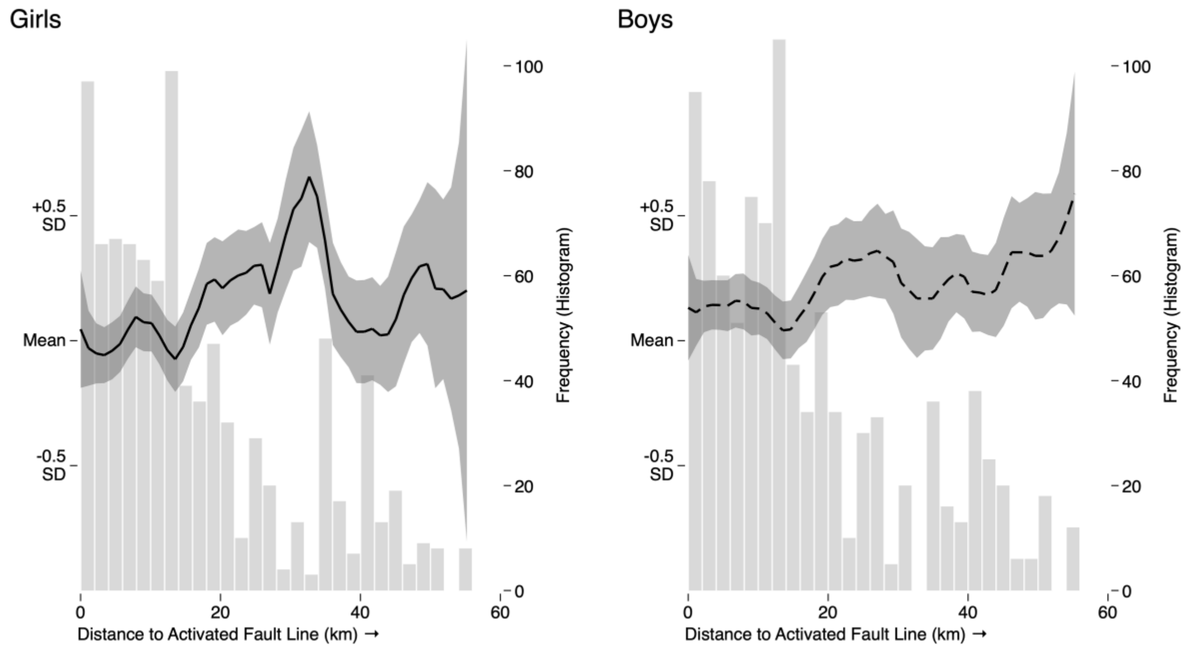
Notes: As a function of age at the time of the earthquake and distance to the activated fault line, these graphs compare the current height-for-age (in both z-scores and cm) of children covered in the detailed survey between near-fault-line and far-from-fault-line groups using nonparametric specifications. The histogram shows relative density as the number of children at each age. Shaded areas show 95% confidence intervals.

Figure 7a. Education outcomes



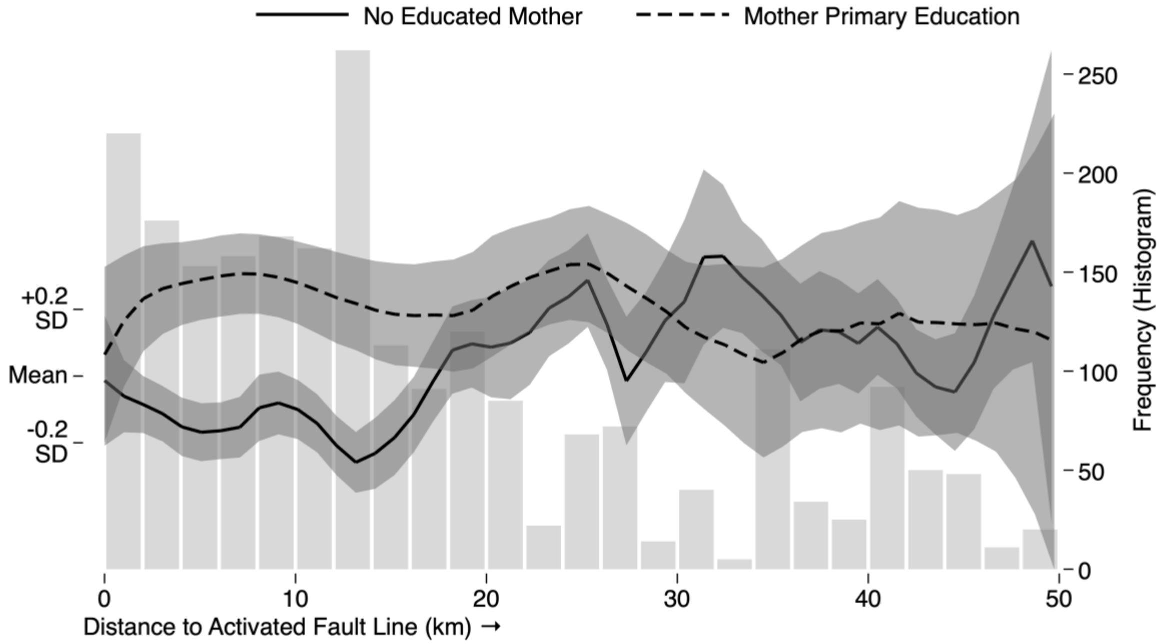
Notes: As a function of age at the time of the earthquake, these graphs compare the current school enrollment and academic performance of children covered in the detailed survey between near-fault-line and far-from-fault-line groups using nonparametric specifications. Test score results are presented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

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



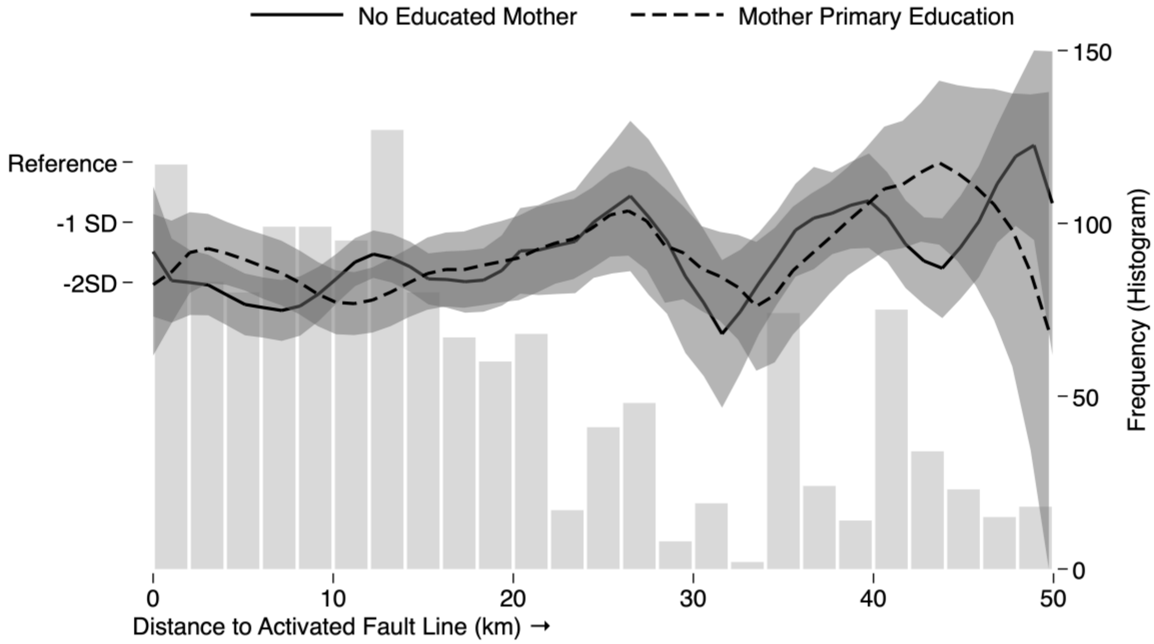
Notes: These graphs illustrate the test scores of boys and girls separately by distance to the activated fault line using nonparametric local polynomial estimation. Test score results are presented as normalized IRT scores within the observed population with mean zero and standard deviation 1. The histogram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

Figure 8a. Test scores and maternal education



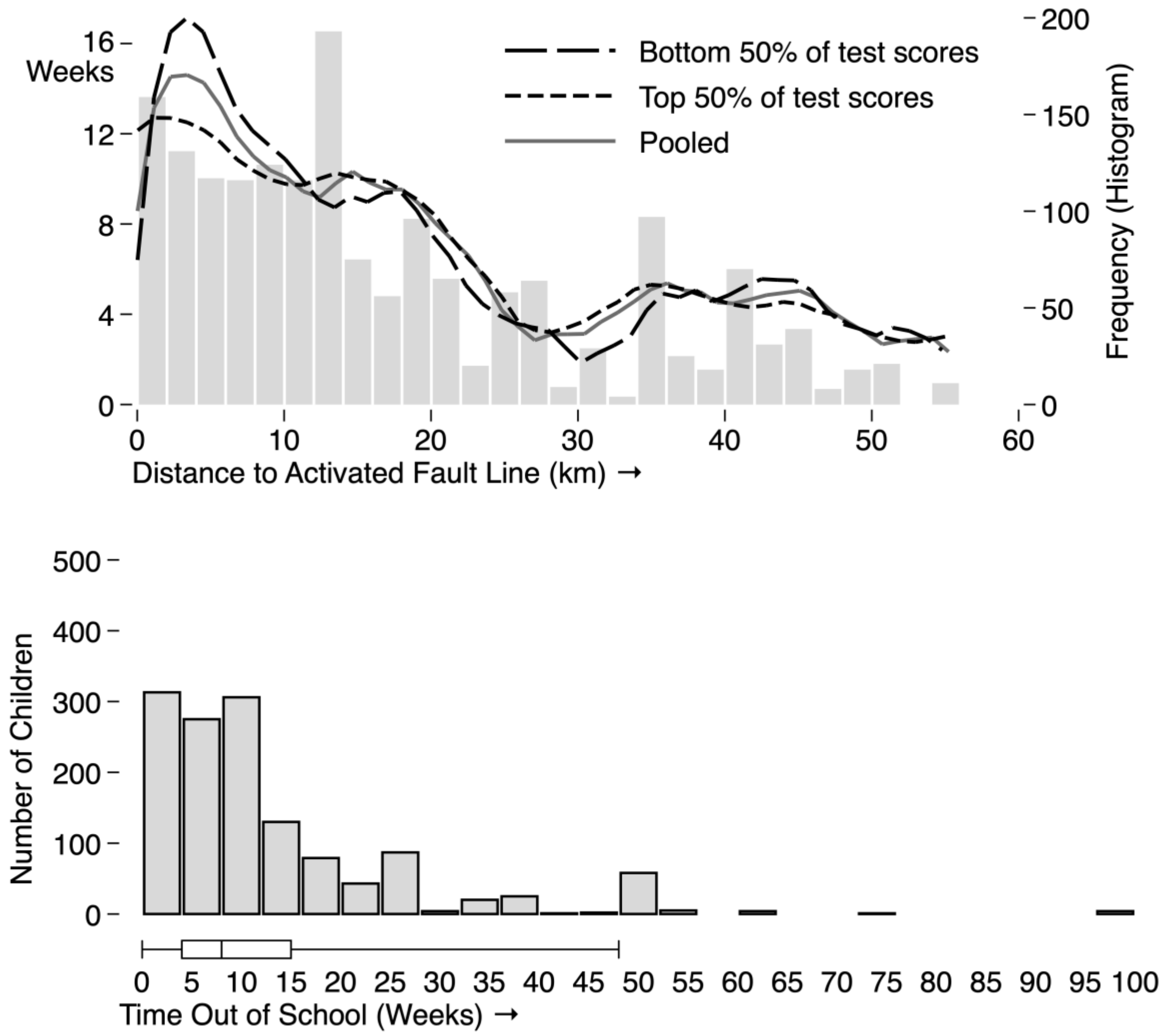
Notes: This graph illustrates the test scores of children whose mothers have or have not completed primary education, separately, by distance to the activated fault line using nonparametric local polynomial estimation. Test score results are presented as normalized IRT scores within the observed population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

Figure 8b. Height and maternal education



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

Figure 9. Time out of school after the earthquake



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

Notes

[1] Pörtner (2010) and Baez et al. (2007, 2010) summarize an extensive literature on shocks and human capital. Weather shocks (Hoddinott and Kinsey 2001, Maccini and Yang 2009), childhood disease and malnutrition (Bozzoli et al. 2009, Alderman, Hoddinott, and Kinsey 2006) and wars (Akresh et al. 2011, Akresh et al. 2012, Weldeegzie 2017) all have significant effects on height-for-age among young children with consequences persisting to adulthood, potentially through associated cognitive underdevelopment (Black et al. 2019; Case and Paxson 2010; Glewwe and King 2001; and Glewwe, Jacoby, and King 2001). In Ethiopia, for example, height losses after a famine led to predicted annual income losses of 3-8% (Dercon and Porter 2010).
[2] For the 2004 Tsunami, mortality and destruction were highly correlated with household and respondent characteristics (Frankenberg et al. 2013). Estimates of individual impact thus have to account for selective mortality rates, which were close to 30% in areas closest to the sea.
[3] Alderman et al. (2006) and Handa and Peterman (2016) suggest catch-up growth of 60% or more but it appears to be lower for children stunted between the ages of 6-17 months (Berkman et al., 2002). Frankenberg et al. (2013) show that even as children's heights declined after a Tsunami, significant aid flows allowed them to catch-up with their peers in later years. We have not found studies that examine whether physical catch-up is accompanied by recovery in test scores.
[4] Children under three were excluded from anthropometric measurements: This would have required them to be laid flat on a board, which mirrors practices during funerals and was considered traumatic after the earthquake. Testing children under the age of seven would have required oral, rather than written, examinations and specialized surveyor training which we could not fund.
[5] Mothers have 8.3 years of education, conditional on completing primary school or higher, compared to 0.17 years for those who had not completed primary school.
[6] 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)
[7] A smaller earthquake (6.2 on the Richter scale) struck Hunza, Hazara, and Swat districts in North-West Frontier Province in 1974, but these districts were mostly unaffected in 2005.
[8] Unfortunately, there are no other pre-earthquake village or household data that we can use to assess exogeneity as no surveys have been conducted in one of the provinces (AJK) and in the other (KP) household data from government surveys have been collected only on 48 villages in two districts, in which names and locations have been anonymized.
[9] Of 4,474 children, 66 (2.3%) had moved in near the fault line versus 69 (4.5%) far, and 25 (0.9%) had moved out near the fault line versus 19 (1.2%) far.
[10] 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.

[11] The value of this compensation also depends on prices at the time of receipt. Unfortunately, no routine data are collected on consumer prices in any of the earthquake-affected districts. Prices

queried at the time of our survey (Appendix Figure A6) suggest little to no change for a wide variety of food groups apart from rice and dal (lentils), both of which are not locally produced.

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

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

[14] For all children, standardized height-for-age trends downwards till age seven. This pattern is common to growth charts from South Asia reflecting 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 during the adolescent years.

[15] The gender gap in enrollment in this area is small to begin with: Household survey data from two of the four districts show that 92-98% of rural boys aged 5-15 years are currently attending school compared to 88-92% for girls (PSLM 2004).

[16] Test scores were standardized using Item Response Theory and averaged across all subjects.

[17] The earthquake struck just after the harvest had been collected, so school closures coincided with the slack agricultural season on the region during which time it was unlikely that children were engaged in agricultural activities. Fully 75% of children reported being back in school after 3 months with a median disruption of only six weeks. If we restrict the sample to households < 20km from the fault line, the median disruption is eight weeks and the 75th percentile is 15 weeks.

[18] We can only use the subsample of children who were enrolled during the earthquake, resulting in a smaller sample size.

[19] The argument for the exogeneity of the fault line variable, however, is only valid conditional on the (pre-determined) geographical controls, so removing them from the regression leads to a coefficient without a plausibly causal interpretation.

[20] If we use the Mercalli intensity instead, both height and test score results are similar, but precision is lower for the height results (Appendix Table A4c). Specifically, moving from very high intensity (“9” near the epicenter) to lower intensity (“5” near the edge of the study zone) recovers a 0.6-0.7 standard deviation impact on height for the youngest and a 0.4-0.5 standard deviation effect on test scores.