Disastrous Displacement: The Long-Run Impacts of Landslides*

Travis Baseler Jakob Hennig

November 2023

Abstract

Natural disasters displace millions of people a year, but their economic impacts are difficult to study due to sorting out of at-risk areas. This paper estimates the long-run impacts of landslides by exploiting sharp boundaries between affected and nearby households exposed to similar levels of *ex-ante* risk. The analysis combines administrative and survey data from Uganda with exact landslide paths and a geological hazard model. Landslides increase long-term displacement and migration, and affected households have substantially worse economic and mental health outcomes years afterward. Excluding the displaced sample leads to attenuated welfare impact estimates, implying that research designs that don't track the displaced may understate disaster harms. Our findings suggest that social capital disruptions act as an impediment to post-displacement recovery.

JEL CLASSIFICATIONS: Q54, J61, H84, O15, R23
KEYWORDS: natural disasters, displacement, climate refugees, forced migration

*Baseler: University of Rochester, travis.baseler@rochester.edu. Hennig: The World Bank, jhennig@worldbank.org. We thank the International Research Consortium, and especially Dr. Daniel Kibuuka Musoke and Aidah Nakitende, for their diligent work coordinating and collecting field data, and Ande Shen for excellent research assistance. We are grateful to Lieven Claessens for sharing landslide risk data and for helpful discussions on landslide risk, to Daniel Brzovic for helpful comments on the geophysical data. We thank Daniel Agness, Doug Gollin, Nghiem Huynh, participants at the North East Universities Development Consortium, and anonymous referees for helpful comments. We also thank the International Growth Centre for funding this study. Additionally, this paper has been partially supported by the Umbrella Facility for Trade trust fund (financed by the governments of the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom) and by The World Bank research support budget. This study has received approval from the Institutional Review Board (IRB) at the University of Rochester (Study 6572), the Mildmay Uganda Research Ethics Committee (Study 2021-86), and the Uganda National Council for Science and Technology (Study SS1263ES). This paper was previously titled "The Long-Term Welfare Impacts of Natural Disasters: Evidence from Ugandan Landslides."

1 Introduction

Between 2008 and 2018, around 265 million people were displaced by natural disasters such as floods, storms, earthquakes, tsunami, and landslides, and the frequency and severity of these disasters are expected to increase (IDMC, 2019). There is thus a clear need to understand the economic impacts of natural disasters and subsequent displacement on affected individuals, as well as the factors that mitigate harmful effects. However, the nature of disasters makes estimating these impacts difficult, especially in developing countries where the vast majority of at-risk individuals reside. Exposure to natural disasters is likely to be correlated with potential economic outcomes, as richer or more mobile households sort away from high-risk areas. Displacement itself also complicates data collection—especially on long-run outcomes—as the affected population becomes dispersed. These challenges have led to a paucity of such estimates in the economics literature. The few existing studies of natural disaster impacts in developing countries focus on events with little long-term displacement. Many of the events studied also attracted massive international attention and aid, whereas most large natural disasters—including the landslides we study—receive little attention or aid (Eisensee and Strömberg, 2007, Heger and Neumayer, 2019).[1] As a result, disaster impacts in typical settings—and the role of displacement following disasters—are largely unknown.

This paper studies the long-run economic impacts of landslides in Uganda, where 300,000 people have been affected, and 65,000 displaced, over the past decade alone (OCHA, 2019). Landslides create sharp boundaries in exposure within the set of at-risk households, allowing us to overcome many of the selection issues present in the extant disaster literature. We identified six major landslide events in eastern Uganda since 2010, and gathered administrative lists of the households residing in the affected areas at the time of each landslide. We surveyed those households in 2022 regardless of their current location, with a very low attrition rate. This allows us to estimate the average impact of landslides on nearly the complete set of affected households, which may be different than the impact on the set of households that remain, especially in settings with high rates of displacement.

To identify landslide impacts, we combine exact information on the path of each landslide and households' pre-landslide locations with a risk model developed in the geomorphology literature

[1] Aid following the 2004 Indian Ocean tsunami totaled $1,250 per victim (Péchayre, 2011), far exceeding direct monetary damages in what Heger and Neumayer (2019) call "the single largest aid and reconstruction effort of any developing world region ever afflicted by a natural disaster." The average large disaster prompts far less aid, totaling about 10% of damages on average (Heger and Neumayer, 2019). This average is close to aid receipts in our setting, which amounted to about 8% of monetary damages.

specifically for our study region (Claessens et al., 2007). The model shows that around a landslide site, many households that did not experience a landslide nevertheless faced similar levels of risk compared to those that did.[2] Moreover, before the disaster, directly affected households did not differ from their neighbors on a large set of observable characteristics, consistent with limited sorting into the landslide paths within an affected site. We estimate long-run impacts by comparing landslide-affected households to their neighbors, controlling for *ex-ante* landslide risk as measured by the geomorphological model.[3] We describe our setting and study design, including our sampling frame, identification strategy, and estimating equations in Section 2.

The landslides were highly destructive, causing casualties in 28% of households residing in their paths—which we refer to as *affected households*—and average damages worth over two years of median household income. The landslides also created substantial long-term displacement: affected households were 50 percentage points (pp.) more likely to be displaced outside their home villages—nearly all to rural locations—and were displaced for about 3 years on average. Around half of displaced households had returned to their origin villages by the time of our survey. External assistance was low: the average affected household received only $34 in aid—about 8% of damages—and the government organized only 23% of displacements. The landslides also increased rural-to-urban migration of individuals within the household, which persists in the long run. Years after the landslides, affected households appear substantially worse off compared to those that were outside a landslide path: they are less likely to be economically active, they exhibit worse financial and mental health outcomes, and they live in lower-quality housing. The estimated welfare impacts of landslides are substantially diminished in a sample that excludes the displaced population, as a traditional panel survey would. We describe the immediate and longer-run impacts of the landslides in Section 3.

To investigate the mechanisms driving long-run welfare impacts—and our substantially different estimates when excluding the displaced population from the estimation sample—we exploit our detailed household survey data on pre- and post-landslide outcomes. We first summarize the landslides' impacts into an index using the methodology of Anderson (2008) and estimate how welfare

[2]While the overall risk of landslides in the area is known, the extent and path of each slide introduces plausibly exogenous variation in landslide damage. This is supported by a literature showing the difficulty of predicting landslide occurrence, and especially landslide extent, from geological variables (Broeckx et al., 2019, Roy et al., 2022).

[3]Our identification strategy does not capture broader regional impacts, such as those at the village level. To test for regional effects, we use data on households far from landslides or in villages where no landslide occurred. We find that regional effects appear small and negative, likely leading us to understate the negative impacts on directly affected households (see Section 4.5).

impacts vary based on the destructiveness of the landslide, whether the household was displaced from its origin village, and aspects of the displacement process including whether the household was displaced to a location with others from its origin social network and whether the government organized the displacement process. To address the concern that destructiveness and displacement may be partly determined by pre-landslide household characteristics that are themselves correlated with long-run welfare impacts, we gathered rich retrospective data from each household immediately prior to the landslide. We construct a pre-landslide welfare index—following as closely as possible the construction of our welfare index measure—and control for this index in our regressions,[4] following the logic of an ANCOVA specification (McKenzie, 2012). We additionally control for a large set of pre-landslide characteristics, along with several measures of the extent of landslide damages, and their interactions with landslide exposure, using a principled variable-selection algorithm to choose among potential controls (Belloni et al., 2014). We discuss the role of displacement, migration, and destruction—and rule out potential alternative explanations—in Section 4.

Two factors appear to explain why many households do not recover from the landslides: displacement and deaths. Households displaced after a landslide appear much worse off compared with those that were also located in the landslides' path, but remained in the origin village. This does not appear to be driven by negative selection into displacement: in fact, the displaced are positively selected on pre-landslide income. The worse outcomes among the displaced do not appear to be due purely to locational disadvantages at the destination, as nearly all households were displaced to similar rural areas in the same region as their origin villages, and households that remained in the destination after displacement appear much better off than households that moved back to the origin.

Rather, our evidence points to the importance of maintaining social capital after displacement. Variation in the extent of landslide damages within a village led some households to be displaced in large groups, while others were displaced to locations without any known contacts from their origin. We find that those who were displaced with households they knew in their origin village appear substantially better off than those displaced alone. Government support during displacement appears to partly substitute for these benefits. We find that maintaining social capital during dis-

[4]Recall bias is a potential concern with retrospective questions. To mitigate this concern, we measured retrospective outcomes using ranges rather than levels for monetary values, and focused on outcomes that are easier to remember such as occupation and physical assets. Reassuringly, this pre-landslide welfare index does not vary significantly with exposure to the landslide, as shown in Table 1.

placement helps households establish new livelihoods in the destination, as households displaced with their network were more likely to find employment outside of farming and to remain in the destination rather than return to the origin. We also exploit quasi-random variation in the share of households in a village hit by a landslide, which is likely to be determined largely by how far down the slope—and in which direction—the destabilized earth traveled, and which increases the likelihood of displacement with origin network members. We find that, among displaced households, those from villages where a greater share of households were hit fare better in the destination.

Although displacement appears to worsen outcomes, negative welfare impacts appear mitigated for households that sent more migrants to urban areas after the landslide, suggesting that urban migration can help affected households cope with the impacts of disaster. Finally, household casualties from the landslide strongly predict negative long-run welfare impacts.

Overall, our results indicate that natural disasters can have substantial negative long-run impacts on affected households, especially when households are displaced. This finding is in stark contrast to many studies from high-income settings, which often find positive long-run impacts of displacement on income and human capital, possibly by disrupting locational ties that have adverse economic consequences or increasing human capital investment (Sacerdote, 2012, Deryugina et al., 2018, Nakamura et al., 2021). Our findings suggest that the maintenance of social networks after displacement is key in allowing households to establish new livelihoods. A number of studies in low-income settings have also found positive long-run economic impacts of natural disasters (Gignoux and Menéndez, 2016, Heger and Neumayer, 2019), potentially driven by subsequent aid receipts. Our results are consistent, however, with a more qualitative or descriptive medical literature that documents negative mental health outcomes associated with natural disaster and displacement (Norris et al., 2002, Porter and Haslam, 2005, Makwanam, 2019), including in eastern Uganda (Kabunga et al., 2022). This contrast suggests that the positive impacts of disaster observed in other settings may be specific to disasters that attracted substantial external aid, and which did not cause high rates of displacement. Importantly, the contextual differences between our study and the extant literature mirror the variation within our setting: among households affected by the landslides, those that experienced unassisted displacement experience much worse long-run outcomes. Our findings thus suggest that the positive impacts of disaster found in the extant literature depend on well-developed institutions which can provide effective disaster insurance or relief and prevent or mitigate forced displacement. As the majority of disaster-driven displacement occurs in sub-Saharan Africa and South and Southeast Asia (IDMC, 2019, Internal Displacement Monitoring Centre, 2022), where government capacity to respond to disasters is often limited, these findings have significant implications for the global population of at-risk households.

Related literature. This paper contributes most directly to the literature studying the economic impacts of natural disasters. Nakamura et al. (2021) study a volcanic eruption which displaced households out of a high-income town in Iceland. The authors find that displaced households were better educated and earned more, with results driven by younger individuals for whom high moving costs may have precluded optimizing over locations based on comparative advantage. Deryugina et al. (2018) study the impact of Hurricane Katrina on income, and find positive long-run effects, with the largest changes observed among households who moved away from New Orleans permanently. Sacerdote (2012) studies the impact of Hurricanes Katrina and Rita on the test scores of displaced students. After an initial drop in scores, impacted students' scores were higher by the third year following displacement. These papers attribute the positive impacts of displacement to locational advantages in the destination compared to the origin.

The disaster impacts literature is sparser in developing countries. We conducted a systematic review of the literature on natural disaster impacts in low or middle income countries, detailed in Appendix B. Across a broad set of economics journals, we identified 17 papers studying natural disaster impacts, only 4 of which studied individual- or household-level economic outcomes such as income, consumption, assets, or human capital. These 4 papers come to contradictory conclusions.[5] Gignoux and Menéndez (2016) study earthquakes in Indonesia and find positive long-run effects on productivity, driven partly by substantial external aid receipts. Caruso and Miller (2015) and Caruso (2017) study the impacts of exposure to natural disasters based on location at birth and disaster timing, and find negative impacts on children's human capital outcomes. Deuchert and Felfe (2015) find that typhoon damages are associated with worse children's educational attainment in panel data, but must rely on variation in home damages within a region, which may be correlated with unobserved determinants of future educational investments. Our identification strategy im-

[5]The macroeconomic literature on natural disaster impacts in low-income settings has also come to contradictory conclusions. These studies typically compare affected regional units to unaffected ones. A disadvantage of this strategy is that displacement or subsequent out-migration make it hard to conclude that changes to regional aggregate variables reflect welfare effects on affected households. In a macroeconomic meta-analysis, Cavallo et al. (2013) find little long-term consequences of disaster. Kocornik-Mina et al. (2020) find similar results for inundations. On the other hand, Strobl (2012) and Felbermayr and Gröschl (2014) find negative growth impacts. Noy (2009a) finds that less-developed countries are less able to withstand shocks from natural disasters. Hsiang and Jina (2014) and Joseph (2022) find that cyclones and earthquakes, respectively, reduce long-run aggregate growth at the national or sub-national level. Heger and Neumayer (2019) find positive impacts, likely driven by aid receipts.

proves on methods that rely on regional variation in exposure to disasters—as individuals may sort out of high-risk regions—by exploiting sharp exposure boundaries, which is typically not possible in studies of earthquakes, tsunami, or typhoons. Our study also extends this literature by collecting detailed survey data from affected and nearby households, which was only possible by combining past administrative lists with extensive field tracking. These data offer an unusually detailed look at households' disaster and displacement experiences, allowing us to shed light on the mechanisms driving welfare impacts. Our paper is the first in this literature to bring household data to a setting with significant displacement and little external aid, which is typical in developing-country settings (Heger and Neumayer, 2019). We also add to this literature by documenting persistent negative impacts of disasters on households' mental health, which many existing natural-disaster studies are unable to evaluate. Finally, we introduce to this literature a novel measure of *ex-ante* landslide risk based on geological features such as terrain slope and soil characteristics, assisted by a model from the geomorphology literature (Claessens et al., 2007). This enables us to better address concerns about pre-disaster sorting of households out of disaster-prone areas.

The literature on natural disaster displacement sits within a broader literature on forced displacement, including by conflict or persecution. Becker et al. (2020) show that Poles whose families were forcibly displaced after World War II are more educated, driven by increased preferences for human capital investment. Sarvimäki et al. (2022) show that displacement of Finnish communities after WWII increased long-run income, possibly due to disrupted preferences for remaining in a home location that did not maximize individuals' incomes. Chiovelli et al. (2021) study the impact of displacement during the Mozambican civil war on human, social, and civic capital by comparing displaced to non-displaced siblings in census data. The authors find that displacement increases educational investment. While the greatest effects were observed in rural-to-urban movers, even rural-rural movers exhibited greater educational gains than stayers, implying that place-based effects do not entirely drive displacement impacts.[6] Given the positive economic impacts of displacement documented in this literature, one might expect including the displaced population in a study of natural disaster to mitigate estimated harms.[7] The role of displacement in driving nega-

[6]Cortes (2004), Gray et al. (2014), Chin and Cortes (2015), Dustmann et al. (2017), Aksoy and Poutvaara (2021) and Abramitzky et al. (2022) study the selection of refugees compared to other immigrants or to non-immigrants, but do not estimate the impact of displacement on the displaced. Cattaneo and Peri (2016) study the impact of rising temperatures on migration, but also do not estimate impacts on the displaced. See Becker and Ferrara (2019) for a review of the forced migration literature.

[7]The findings in Caruso (2017) also suggest that including the displaced population mitigates estimated negative impacts of natural disaster. However, that paper cannot distinguish disaster displacement from subsequent voluntary migration.

tive long-run impacts in our setting is thus potentially surprising. We contribute to the literature on forced displacement by documenting the role played by origin social networks—which may be fragmented as a result of displacement—in determining post-displacement outcomes.

Finally, our paper contributes to the study of rural-urban income gaps in developing countries, and the role of internal migration in allowing households to exploit these gaps. A large literature identifies barriers that restrict workers from moving to take advantage of income gaps, including inadequate information about destination opportunities (Baseler, 2023), financial constraints (Bryan et al., 2014, Cai, 2020), costs of migrating (Lagakos et al., 2018, Imbert and Papp, 2020, Morten and Oliveira, 2023), food insecurity in the destination (Baseler et al., 2023), and land market frictions (De Janvry et al., 2015).[8] While worker sorting also plays a role in sustaining income gaps (Lagakos and Waugh, 2013, Young, 2013, Hamory et al., 2020), several studies find that the return to marginal rural-urban migration in low-income countries is large (Beegle et al., 2011, Bryan et al., 2014, Baseler, 2023). These large, uncaptured returns are thought to explain the positive effects of displacement observed in some contexts through place-based effects on earnings (Deryugina et al., 2018, Chiovelli et al., 2021, Nakamura et al., 2021). Our results are consistent with positive returns to disaster-induced urban migration, but we find that rural-rural displacement worsens welfare impacts. This literature has also documented that migration can help households cope with disasters, by offering earnings opportunities for new migrants (Mahajan and Yang, 2020) or through increased remittances from existing migrants (Yang and Choi, 2007, Yang, 2008).[9] We confirm in our setting that landslides increase rural-urban migration, which helps households cope with the disaster.

2 Study Design

This section describes the setting of our study, our data collection methodology, and our identification strategy to estimate the causal impacts of landslides.

[8]See Lucas (1997) and Lagakos (2020) for reviews of this literature.

[9]Gröger and Zylberberg (2016) study household responses to a typhoon in Vietnam, and find that households cope with the negative shock to income through increased remittances from existing migrants and by sending new migrants to urban areas, but do not measure long-run effects. For evidence that remittances help households cope with negative weather shocks, including natural disasters, see Yang and Choi (2007), Yang (2008), Blumenstock et al. (2016) and Mahajan and Yang (2020).

2.1 Context and Geological Data

Our study area is the Mt. Elgon region of eastern Uganda, where most of the landslide-related deaths and displacement in Uganda have occurred. Between 2010 and 2020, landslides resulted in at least 1,000 deaths in the region, and in tens of thousands of displacements.[10] The main hot spot for these disasters is Bududa district. The volcanic soils and steep slopes of Bududa contribute to landslide risk, as do the increasing population density and intensity of crop cultivation (Knapen et al., 2006, Claessens et al., 2007). The primary economic activity is farming, especially of maize and bananas, and of coffee as a cash crop (Akoyi and Maertens, 2018).

Despite attempts by the Ugandan government to relocate victims and the at-risk population, the number of households facing serious landslide risk has grown over time (Independent, 2020). This risk is closely related to climate change, in particular more frequent heavy rainfall events, which destabilize susceptible slopes. The number of landslides and floods has increased over the last 30 years, and is expected to increase further (World Bank Group, 2020).

Bududa has frequently been the site of geological landslide risk assessments, for example in Claessens et al. (2007, 2013) and Broeckx et al. (2019). The authors of these studies have generously provided us with their data and advice. Their dataset includes a 10-meter-by-10-meter grid of the elevation, slope, distance to the watershed, direction, and soil type, as well as a landslide risk measure based on these features. The risk model in Claessens et al. (2007) (called LAPSUS-LS) is based on a mapping of 81 earlier landslides in the same region, which enabled the authors to determine the statistical relationship between geological features and landslide risk. The model output is a critical rainfall value, above which a plot would become unstable. As described in Section 2.3, we use this output—together with information on each site's topography—to control for households' pre-existing exposure to landslide risk, and in Section 2.5 we show that directly affected households did not differ from their neighbors before the disaster on these risk measures and a large set of other observable characteristics.

2.2 Site Identification and Household Data Collection

The sample for our study includes the largest landslide sites around Bududa in the last 10 years. For each site, we collected a list of households residing in nearby villages *at the time of the landslide*

[10] See OCHA (2019), Monitor (2019). As the sources note, the exact number of victims is hard to determine since many remain missing.

from past population registers maintained by local officials. This gives us information on the full affected population regardless of whether they moved away. These lists form our study sample. We also collected data from two other landslide sites outside Bududa district. LAPSUS-LS data were not available in these other districts, so our main analysis relies on the four Bududa sites.[11]

The population registers produced a list of 975 households, of which 652 were located in Bududa. We supplemented this list using snowball sampling by inquiring about neighboring households not on the household register during fieldwork. Only 26 new households were identified through this process, increasing our confidence that the registers were largely complete.

Of the 675 households we identified as residing in our study villages in Bududa prior to the relevant landslide, we successfully surveyed 630, or 93%. We successfully collected GPS readings at the pre-landslide location for all but five of these households, producing a final primary sample of 625 households. In the broader sample which includes Manafwa and Sironko districts, we successfully surveyed 913 out of 1,001 households, or 91%. Inclusion in our surveyed sample is uncorrelated with household size, age of the household head, or residence in a landslide path, and is similar across the four landslide sites, as shown in Appendix Table A1.[12] Our survey rate is higher for households that were still residing in their original village. Out of the 675 households, 46 (6.8%) were listed as currently living in a different village. Among these 46, we successfully surveyed 36 (78%).[13] Our surveys collected current and pre-landslide information for each individual that had been living in that household prior to the landslide, regardless of their current location. Additional details on sampling and data collection can be found in Appendix C.

Figure 1 displays our six sites on a map of the surrounding region. We surveyed households from four sites within Bududa district, where we can estimate landslide risk from LAPSUS-LS data. We also identified two sites in the neighboring Manafwa and Sironko districts.[14]

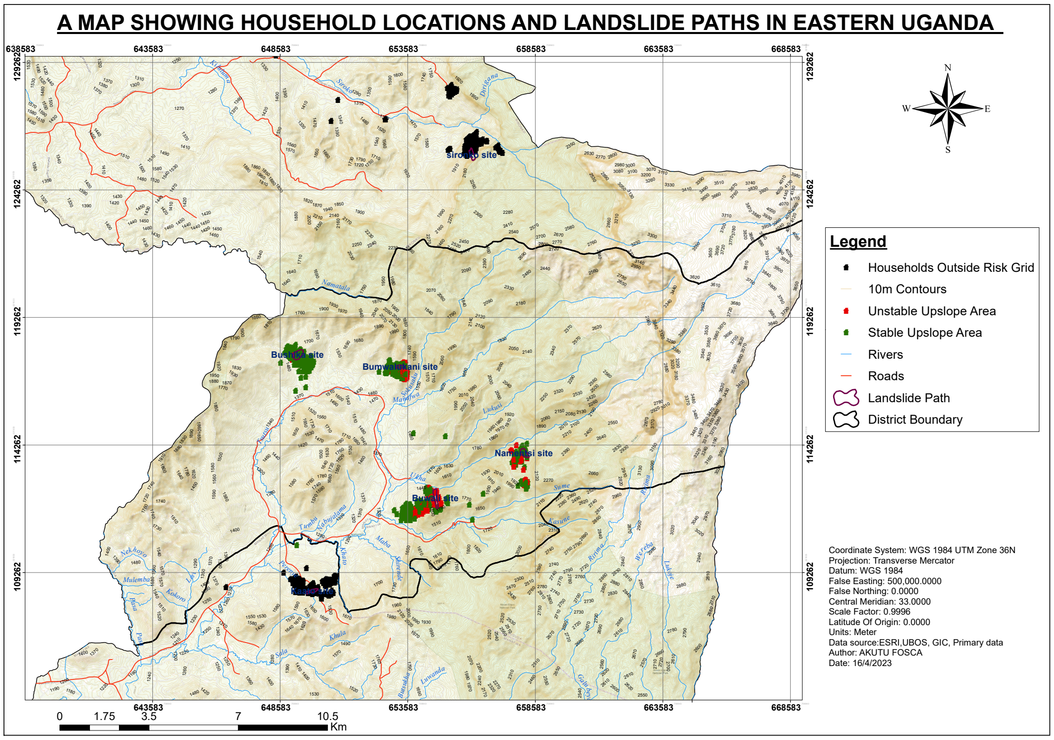
[11] Results including the two additional sites are very similar, as shown in Appendix Table A10.

[12] Since we do not observe pre-landslide location for households we could not survey, we impute an indicator for residing in a landslide's path based on whether the household is listed as residing outside its pre-landslide village in our administrative data, for unsurveyed households only.

[13] We discuss, and reject, the possibility that the lower survey rate among households listed as currently displaced is driving our estimates in Section 4.5. Estimating Lee bounds or re-weighting our estimates to account for non-response changes our findings very little, as shown in Appendix Tables A15 and A16.

[14] The Bududa sites include the large Nametsi landslide in 2010, an equally destructive event in Bumwalukani parish in 2012, and two more recent events: the 2019 landslides in Bushika and Buwali sub-counties, which together killed close to 100 residents and displaced more than 1,000. The two sites outside Bududa are a 2018 landslide in the Kaato sub-county of Manafwa and a 2017 landslide in the Bufupa parish of Sironko.

Figure 1: Overview of Landslide Sites in Our Study Sample



Notes: Each house icon is the pre-landslide location of a household in our sample. Green indicates that more than 75% of that household's upslope area is classified as stable; red indicates that 25% or more of the upslope area is classified as unstable. Exact landslide paths shown in purple polygons.

2.3 Identifying Variation

To identify which households were hit by landslides, we used satellite images to trace exact landslide paths. We add a 50-meter buffer to these paths to account for rubble or destabilized ground near the path's boundary that may not show up on satellite images.[15] We designate households that had been residing within these buffered regions at the time of the landslide as *affected households*, with the caveat that households outside a landslide path also experienced casualties and damage, albeit at a much lower rate. We show that our main results are similar when excluding the 50-meter buffer, or relying on self-reported damages, in Tables A5 and A6.

To control for possible *ex-ante* sorting based on landslide risk, we combine information from the LAPSUS-LS model with information on each site's topography to compute measures of exposure to landslide risk at each household's pre-landslide location. Specifically, we compute the share of the upslope area classified as unconditionally unstable, the share classified as conditionally

[15]We choose 50 meters as a buffer because of a clear discontinuity in how likely households are to report destruction of their home around that point, as shown in Appendix Figure A1.

unstable, and the distance to the nearest unconditionally unstable point.

Figure 2 displays our identification strategy visually for the Nametsi landslide site in Bududa district.[16] The map shows the Claessens et al. (2007) measure of soil instability at each surface-grid point, with darker shading indicating a higher risk of instability. Households in our sample are colored green or red according to their exposure to unstable upslope terrain. The exact landslide path is shown in purple. The map provides a preliminary indication that predicting landslide paths is difficult: there is significant variation in exposure to unstable upslope terrain both within and outside the *ex-post* landslide path.[17] In Section 2.5, we show that the LAPSUS-LS variables, our measures of households' landslide risk, and other pre-landslide demographic variables exhibit few significant differences between households originally located in a landslide path and their neighbors. This is indicative of limited sorting across affected and unaffected locations within sites.

2.4 Estimating Equations

We estimate the causal impacts of landslides with the following specification:

$$y_i = \beta Landslide_i + Risk_i \Omega + Site_i + \varepsilon_i \qquad (1)$$

where y_i is an outcome (which may be measured at the household or individual level); $Landslide_i$ is an indicator for whether the household resided within 50 meters of the exact landslide path at the time of the landslide; $Risk_i$ is a vector of geological variables defined at households' pre-landslide locations (elevation, slope, indicators for ground stability classifications defined in Claessens et al., 2007, the share of the upslope catchment area classified as unconditionally unstable and as conditionally unstable, the distance to the nearest unstable point, and the square of that distance);[18] $Site_i$ is a landslide-event fixed effect; and ε_i is an error term.[19] Under our assumption that $Risk_i$ cap-

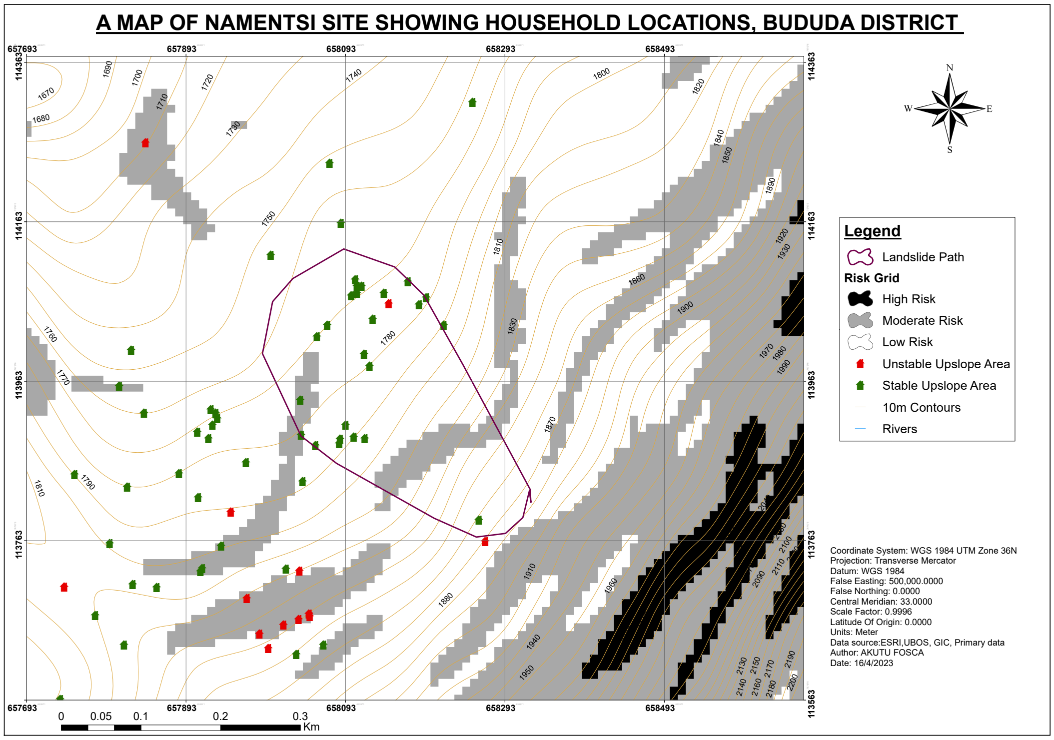
[16]Maps of our other study sites are presented in Appendix Figure C1.

[17]The unpredictability of landslide extent is also discussed in Broeckx et al. (2019), which uses an updated model to study Bududa district; while they find that overall landslide susceptibility of an area can be assessed, the size of a slide and thus its width and length are difficult to predict from observed characteristics. The changing nature of landslide risk as a consequence of climate change can make previously safe areas prone to instability (World Bank Group, 2020). This is also reflected in the government's plans to relocate all households in landslide-prone areas (Kabunga et al., 2020), rather than targeting certain households within these areas.

[18]Consistent with our finding of limited sorting around the *ex-post* landslide path, omitting this risk measure produces very similar estimates (see Appendix Table A8).

[19]When estimating impacts outside our main sample, where LAPSUS-LS data are not always available, we include

Figure 2: Map of One Landslide Site Showing Grid-Level Soil Instability, Household-Level Risk, and Exact Landslide Path



Notes: Land surface is shaded to indicate soil instability at each grid point: black indicates high risk (unconditional instability), gray indicates moderate risk (critical rainfall value of up to 0.39 meters per day), and white indicates low risk (critical rainfall higher than 0.39 meters per day). Each house icon is the pre-landslide location of a household in our sample. Green indicates that more than 75% of that household's upslope area is classified as stable; red indicates that 25% or more of the upslope area is classified as unstable. The exact landslide path is shown by the purple polygon. We apply a 50-meter buffer (not shown) when classifying affected households to account for GPS readings taken near the boundary, and for rubble or destabilized ground not captured in the exact path.

tures any pre-existing differences at the time of the landslide across households within a landslide site operating through sorting based on risk, β captures the average causal effect of landslides on y_i among households hit by the landslide compared to households within the same landslide site but residing outside of a landslide path. If households outside a landslide path are also negatively affected, for example through damages from the associated heavy rains or disruptions in village infrastructure or public services, this will lead us to understate any negative impacts of landslides on households within a landslide path, a point we return to in Section 4.5.

---

pre-landslide demographic controls as a substitute for the geological controls in $Risk_i$. Estimates in Bududa are robust to controlling for 5-year bins of the respondent's age, farm size prior to the landslide, household size and number of adult-equivalents prior to the landslide, an indicator for whether a household member had migrated outside the village prior to the landslide, and an indicator for whether the household had family living in a big city (or, separately, in Kampala) prior to the landslide, as shown in Appendix Table A7. When using these controls, we replace missing values with the mean value among non-missing responses.

By nature, the destruction caused by landslides is spatially clustered and so may give rise to spatial correlation in regression residuals. Appendix Table D1 presents Moran tests for spatial correlation in residuals for our main outcomes. As expected, these Moran tests suggest modest spatial correlation in damages. However, they indicate little to no spatial correlation in our economic and mental health measures.[20] Appendix Tables D2 and D3 present standard errors adjusted for spatial correlation using the method of Conley (1999) and Müller and Watson (2022) respectively. These adjusted standard errors are very similar to their unadjusted versions.

2.5 Balance on Pre-Landslide Characteristics

Consistent with qualitative evidence that landslide paths are difficult to predict within an area (see footnote 17), households located in a landslide path exhibit few significant pre-existing differences compared to other households in the same area, as shown in Table 1. Columns 1 and 2 show unconditional means for several pre-landslide variables separately by unaffected and affected households, and Column 3 shows the average difference for each variable estimated using (1).[21] Average differences are generally small: out of 27 pre-landslide variables spanning geography, demographic information, and retrospective income and savings, only 3 exhibit significant differences at the 10% level. Affected households were located at slightly higher elevations and slightly farther from the nearest unstable point, and had modestly larger farms prior to the landslide, but are otherwise very similar to unaffected households.[22] We thus proceed to estimate the casual impacts of landslides using (1).

[20] Out of 7 damage and displacement outcomes, we reject the null hypothesis of independent and identically distributed error terms at the 10% level for three. Out of 7 economic and mental health measures, we reject the same null hypothesis for only 1.

[21] We include the vector *Risk* to control for geological risk only in Panels B and C.

[22] We present results estimated on the set of households with common support of these three imbalanced variables in Appendix Table A9. The results are very similar to our main estimates.

Table 1: Balance on Pre-Landslide Characteristics

	(1) Mean for Unaffected Households	(2) Mean for Affected Households	(3) Conditional Difference	(4) p-Value	(5) N
Panel A: Geography at Pre-Landslide Location					
Terrain Slope (Degrees)	19	19	0.63	0.38	625
Elevation (Kilometers)	1.60	1.65	0.02**	0.04	625
Ground is Stable	0.81	0.89	0.03	0.49	625
Critical Rainfall Value for Unstable Ground	0.07	0.05	-0.02	0.28	111
Size of Upslope Area (Meters Squared)	1832	1495	-380	0.24	625
Size of Unstable Upslope Area (Meters Squared)	576	415	-109	0.58	625
Stable Upslope Area (% of Total)	0.77	0.84	0.03	0.44	625
Conditionally Unstable Upslope Area (% of Total)	0.23	0.15	-0.04	0.25	625
Distance to Nearest Unstable Point (Meters)	704	752	-105***	0.00	625
Panel B: Demographic Characteristics					
Respondent Age (Years)	44	43	-0.77	0.71	613
Average Household Age (Pre-Landslide)	29	28	-0.85	0.59	604
Household Size (Pre-Landslide)	4.7	5.1	0.22	0.49	613
# Adult Equivalents (Pre-Landslide)	3.4	3.5	0.02	0.92	613
# Members Aged 0–5 (Pre-Landslide)	1.0	1.2	0.10	0.56	604
# Members Aged 6–17 (Pre-Landslide)	1.6	1.6	-0.02	0.91	604
# Members Aged 18–50 (Pre-Landslide)	1.7	1.6	0.10	0.54	604
# Members Aged 51 or older (Pre-Landslide)	0.4	0.4	-0.04	0.66	604
Large Farm (1 Acre or Larger, Pre-Landslide)	0.47	0.54	0.07	0.26	625
Farm Size (Acres, Pre-Landslide)	1.7	2.3	0.69***	0.00	625
Had Migrated Prior to Landslide	0.33	0.40	0.06	0.33	625
Had Family in Big City at Time of Landslide	0.49	0.52	0.07	0.23	625
Had Family in Kampala at Time of Landslide	0.27	0.29	0.06	0.28	625
Panel C: Retrospective Income and Welfare					
Income (Pre-Landslide)	33	35	-1.3	0.88	625
Income per Adult-Equivalent (Pre-Landslide)	12	15	1.1	0.82	625
Savings (Pre-Landslide)	38	57	12.3	0.21	625
Savings per Adult-Equivalent (Pre-Landslide)	12	20	5.3	0.20	625
Welfare Index (Pre-Landslide)	0.00	0.37	0.14	0.27	625

Notes: An observation is a household (based on pre-landslide structure). Columns 1 and 2 show means within unaffected and affected households, respectively. Column 3 shows the conditional difference recovered from a regression of each characteristic on an indicator for whether the household was affected, controlling for a landslide-event fixed effect plus the vector *Risk* containing geologic variables (Panel A excludes geographic controls). Columns 4 and 5 show p-values from Column 3 regressions testing for equal means and the number of observations. *Welfare Index* comprises 11 pre-landslide outcomes (see Section 4). Responses of “Don’t Know” are coded as missing. Monetary units are USD/month. $^{***}\ p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

3 Short- and Long-Run Impacts of Landslides

This section presents estimated impacts of landslides on immediate destruction, subsequent displacement and migration, and long-run economic and mental health outcomes. Throughout this paper we distinguish between displacement—the relocation of an entire household due to landslide—and migration—the departure of one or more individuals from a household to seek work outside the village.

3.1 Destructive Impacts of Landslides

The landslides we study were highly destructive: households residing in a landslide path experienced extreme rates of death and property destruction. As shown in Panel A of Table 2, 45% percent of homes in a landslide path experienced major damage,[23] compared to 12% of homes outside the path ($p < 0.01$).[24] About 28% of affected households report casualties from the landslide (compared to 7% of unaffected households; $p < 0.01$), almost all of which represents death of a household member. Among affected households with casualties, the median number of casualties is 2 and the mean is 3.5. Both affected and unaffected households experienced damage to land, crops, livestock, or other possessions, although the rates are significantly higher among affected households. Affected households faced significant uncovered repair and reconstruction costs: these rise from an average of \$177 among unaffected households to \$503 among affected households ($p < 0.01$), representing over two years' worth of median household income.

3.2 Impacts on Displacement and Migration

Most households residing in a landslide path were displaced outside their home village, though a significant minority remained. Panel A of Table 2 shows that even among unaffected households, 18% were displaced; this share rises to 68% among affected households ($p < 0.01$). Nearly every displaced household moved to another village in eastern Uganda. Only seven households in our

[23] Among these, 83% report that their entire home was destroyed. The rest largely report damage to one or multiple walls, roofs, or destruction of the floor from flooding.

[24] Damage to homes outside a landslide path is most likely due to heavy rains immediately preceding landslide events. There are several reasons why some of the homes that we categorize as residing in a landslide path were not damaged. Some homes, especially those close to the boundary of the landslide, avoided major damage. There may also be classification error coming from GPS readings. If some unaffected households are miscategorized as affected, this should bias our impact estimates toward zero. Nevertheless, the large differences in reported damages between households categorized as residing in a landslide path and those that are not reassures us that our measure is strongly correlated with true landslide exposure.

sample relocated to a city (four to Mbale, the largest town in the Eastern Region of Uganda, and two to Kampala, the capital), and one left the country. Only 28% of all affected households—just under half of displaced households—remain displaced outside their home village at the time of our survey in 2022. Among those that returned to their home village, about half were displaced for less than a year, one quarter were displaced for one year, and the rest were displaced for two or more years. Among all displaced households, the average length of displacement is 2.9 years.

The landslides also increased migration among individuals in affected households. Panel B of Table 2 shows that individuals from affected households were 11 pp. more likely to migrate after the landslide (on a base of 31%; $p = 0.01$). All of these additional migrants traveled to a city, but almost none went to big cities. Most of this effect is driven by long-run migration: at the time of the survey, individuals from affected households are 9 pp. more likely to be urban migrants, almost doubling the baseline urban migration rate ($p = 0.01$).

3.3 Long-Run Impacts on Economic and Mental Health Outcomes

Landslides substantially worsen long-run economic and mental health outcomes, as shown in Panel C of Table 2. We measure these outcomes at the time of our surveys, 3 to 12 years after the landslides. To give three examples, affected household heads were 12 pp. more likely to report being seriously worried about their household finances (on a base of 51%, $p = 0.04$) and 20 pp. less likely to report being satisfied with their life in general (on a base of 54%, $p < 0.01$), and individuals from affected households were 8 pp. less likely to be economically active (on a base of 66%, $p = 0.07$). To provide coherent summaries of the landslides' long-run effects, we aggregate all of our survey information on economic and mental health outcomes into indices constructed according to the methodology of Anderson (2008). We create four indices comprising the following four categories: financial health of the household, mental health of the respondent, home amenities, and income. We also construct an overall welfare index that includes all of these outcomes. We standardize all indices to have mean 0 and standard deviation (sd) 1 among unaffected households.

Table 2: Long-Run Impacts of Landslides

Panel A: Household Destruction and Displacement	(1) House Damaged	(2) Casualty	(3) Land Damaged	(4) Any Other Damage	(5) Spending on Repairs	(6) Household Displaced	(7) Remains Displaced
Landslide	0.332***	0.214***	0.179***	0.240***	326***	0.496***	0.246***
	(0.056)	(0.049)	(0.056)	(0.037)	(72)	(0.054)	(0.045)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	625	625	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.12	0.07	0.52	0.72	177	0.18	0.03
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.113***	0.113***	0.008	0.090**	0.089**	0.003	-0.075*
	(0.041)	(0.038)	(0.029)	(0.040)	(0.036)	(0.022)	(0.042)
	[0.01]	[0.00]	[0.79]	[0.02]	[0.01]	[0.89]	[0.07]
Observations	1,814	1,814	1,814	1,814	1,814	1,814	1,814
Dep Var Mean for Landslide = 0	0.31	0.22	0.12	0.21	0.13	0.06	0.66
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.201***	0.121**	-0.402***	-0.349***	-0.222*	-0.026	-0.382***
	(0.059)	(0.059)	(0.120)	(0.113)	(0.124)	(0.125)	(0.117)
	[0.00]	[0.04]	[0.00]	[0.00]	[0.07]	[0.83]	[0.00]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.54	0.51	0.00	0.00	0.00	0.00	0.00

Notes: An observation is a household (based on pre-landslide structure) in Panels A and C, and an individual aged 18 or older who was living in a pre-landslide household in Panel B. Migrants are individuals who live outside of the village where their household currently resides, or had left the village since the landslide to seek work and then returned; displacement of the household is not coded as migration. *Landslide* is an indicator equal to 1 if the household was located within 50 meters of a landslide path at the time of the landslide. All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Across all five index measures, landslides have negative long-run impacts. The estimated impact on our financial health index is -0.4 sd ($p < 0.01$); on our mental health index it is -0.35 sd ($p < 0.01$); on our home amenity index it is -0.22 sd ($p = 0.07$); and on our income index it is -0.03 sd ($p = 0.83$). Our overall welfare summary index is lower by 0.38 sd ($p < 0.01$) among affected households.[25]

3.4 Excluding the Displaced Attenuates Welfare Impact Estimates

To assess the role of the displaced in driving our estimates, we reproduce the results shown in Table 2 but exclude the set of households that were displaced by landslides. This approximates a research design that relies on panel surveys which households exit when they leave their homes. We find that excluding the displaced leads to substantially diminished welfare impact estimates, as shown in Table 3. Our estimate of the impact of landslides on our overall welfare index falls by two-thirds, from -0.38 ($p < 0.01$) to -0.13 ($p = 0.53$). Financial and mental health impact estimates are both small in this sample (0.08–0.09 sd). Only the impact on home amenities becomes more negative, suggesting that the displaced moved to better-quality housing. Impacts on individual migration are higher in this sample, possibly because displacement prevents households from financing migration.

The role of the displaced in driving our results can be explained in three possible ways. First, there may be negative selection into displacement along pre-existing characteristics. Second, the direct impacts of the landslide may have been worse among the displaced. Third, displacement may itself be disruptive in a way that worsens long-run outcomes. In Section 4.3, we present evidence consistent with the third explanation, but inconsistent with the first or second.

[25] Appendix Table A2 shows impacts on each component of each index. Our index of **financial health** includes whether the household has enough food, can pay for basic expenses, did not experience a recent financial emergency which forced asset sale, did not pull a child out of school for lack of funds, reports that they are not seriously worried about their finances, and reports that they could find 70,000 UGX in an emergency if needed. Our index of **mental health** includes whether the respondent reports that they are usually happy, usually not nervous, satisfied with their life, and optimistic about the future. Our index of **home amenities** includes indicators for whether the household has access to an improved toilet, an improved water source, an improved cooking fuel source, did not experience any crime in the past 30 days, and the number of good friends outside their household. Our index of **income** includes total household income over the past month (computed as the sum of earnings from household businesses over the past month, earnings from individual salaries and wages over the past month, and crop production value from the most recent season converted to a monthly value), household non-farm income over the past year (computed as the sum of earnings from household businesses over the past year and earnings from individual salaries and wages over the past year), household savings over the past month, household food consumption over the past week, and the share of children who have been in school since the landslide. We divide household income, savings, and consumption by the number of adult-equivalents residing in the household at the time of the survey using World Food Programme consumption equivalence scales (Mathiassen et al., 2017).

Table 3: Long-Run Impacts of Landslides (Excluding Ever-Displaced Households)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Household Destruction and Displacement	House Damaged	Casualty	Land Damaged	Any Other Damage	Spending on Repairs	Household Displaced	Remains Displaced
Landslide	0.288***	0.133*	0.101	0.344***	215**	-	-
	(0.098)	(0.074)	(0.106)	(0.041)	(104)	-	-
	[0.00]	[0.07]	[0.34]	[0.00]	[0.04]	-	-
Observations	459	459	459	459	459	459	459
Dep Var Mean for Landslide = 0	0.08	0.06	0.50	0.70	159	0.00	0.00
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.210***	0.158**	-0.059	0.226***	0.169***	-0.004	-0.076
	(0.062)	(0.068)	(0.042)	(0.064)	(0.056)	(0.032)	(0.077)
	[0.00]	[0.02]	[0.16]	[0.00]	[0.00]	[0.91]	[0.32]
Observations	1,324	1,324	1,324	1,324	1,324	1,324	1,324
Dep Var Mean for Landslide = 0	0.30	0.22	0.13	0.20	0.12	0.06	0.66
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.169	-0.148	-0.083	-0.094	-0.400**	0.232	-0.126
	(0.111)	(0.111)	(0.234)	(0.185)	(0.175)	(0.257)	(0.201)
	[0.13]	[0.19]	[0.72]	[0.61]	[0.02]	[0.37]	[0.53]
Observations	453	452	459	459	459	459	459
Dep Var Mean for Landslide = 0	0.56	0.50	-0.01	0.02	0.00	0.00	0.01

Notes: An observation is a household (based on pre-landslide structure) in Panels A and C, and an individual aged 18 or older who was living in a pre-landslide household in Panel B. Households displaced by landslides are excluded from the sample to approximate a research design that misses the displaced. Migrants are individuals who live outside of the village where their household currently resides, or had left the village since the landslide to seek work and then returned; displacement of the household is not coded as migration. *Landslide* is an indicator equal to 1 if the household was located within 50 meters of a landslide path at the time of the landslide. All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

4 Which Mechanisms Are Driving Welfare Impacts?

What explains the landslides' large, persistent negative impacts on economic and mental health outcomes? Comparing our study to others in the literature reveals several major contextual differences which may explain our divergent results. First, affected households experienced a high casualty rate. Second, a significant share of affected households were displaced from their home villages, and the institutional capacity to assist displaced households was low. Many also turned to urban migration as a means to cope with the disaster. In this section, we examine the role of these mechanisms in driving welfare impacts.

4.1 Estimation Strategy

Studying the mechanisms behind the negative welfare impacts of landslides introduces two additional challenges, especially because displacement and migration are partly choices made by households *ex-post*. First, if a potential channel—such as migration—is correlated with pre-landslide characteristics that separately influence landslide impacts, then the differential welfare effect by migration status will reflect both the impact of migration as well as heterogeneity in direct landslide impacts along those pre-landslide dimensions. For example, if wealthier households are better able to cope with landslide losses and also more likely to migrate afterward—possibly because they can afford the upfront migration cost—then our analysis would overstate the benefit of migrating. Second, displacement and migration are likely to depend on the extent of landslide damages, which may also directly affect long-run welfare. The first concern also applies, though likely to a lesser extent, to analyzing the role of casualties and damage, since households may make investments to mitigate potential landslide damages (for example, planting trees), which could lead landslide damages conditional on exposure to be correlated with investment capabilities, even if landslide paths are as good as random.

To address these challenges, we rely on rich data collected retrospectively about pre-landslide characteristics and landslide damages. While it is not possible for us to rule out unobserved selection, our approach holds fixed a detailed set of observable characteristics that are potentially correlated with the welfare impacts of landslides. Specifically, to investigate the role of the above mechanisms in driving welfare impacts, we estimate regressions of the following form:

$$Welfare_i = \delta M_i \times Landslide_i + \xi Landslide_i + \theta Welfare_i^{PRE} + Risk_i\Gamma + Site_i + X_i\Phi + \nu_i \quad (2)$$

where $Welfare_i$ is the welfare summary index described in Section 3.3; $Welfare_i^{PRE}$ is an analogous summary index of pre-landslide welfare;[26] M_i is a mechanism of interest such as whether the household experienced a casualty; X_i is a set of pre-landslide controls (and their interactions with $Landslide$) chosen through double-lasso regression;[27] and other variables are defined as in (1). Our coefficient of interest δ estimates the average welfare index difference for a one-unit increase in M_i among households affected by a given landslide, holding constant differences in a pre-landslide welfare index, geographic variables predictive of landslide risk, and potentially additional characteristics that are correlated (or differentially correlated based on landslide exposure) with the mechanism of interest. In cases where variation in a mechanism of interest occurs independently of the landslide—such as individual migration—difference-in-differences estimates may be of interest, in which case we modify (2) to include a control for M_i. Appendix Table A11 displays the list of variables selected by the lasso procedure for each mechanism of interest M_i, which is informative of the direction of selection along pre-landslide characteristics and landslide damages.

4.2 The Role of Landslide Destruction and Deaths

The medical literature studying the psychological harms of disaster emphasizes the role of traumatic experiences such as the sudden death of a family member (Norris et al., 2002, Porter and Haslam, 2005, Makwanam, 2019), and a growing literature in behavioral economics shows that mental health and economic outcomes are directly linked (Ridley et al., 2020). The setting we study involved severe damage and death tolls, causing casualties in 28% of households residing in their path (94% of these casualties were deaths) and substantial damage to 45% of homes. In Table 4 column 1, we estimate differential welfare effects of landslides depending on whether the household experienced a casualty, damage to their home, and damage to their land using (2).

We find that experiencing a casualty substantially worsens the long-run welfare impacts of landslides on surviving household members. The estimated differential impact is 0.53 standard

[26] Although this could introduce concerns about recall bias, pre-landslide welfare is not significantly different between affected and unaffected households, as shown in Table 1 ($p = 0.27$). Our pre-landslide welfare index uses the same measures as our contemporary welfare index, except: number of friends, optimism about the future, experience of a recent financial emergency, pulling a child out of school, worry about finances, consumption, share of children in school, and yearly income. Monthly pre-landslide income excludes harvest value.

[27] The post-double lasso estimation method selects control variables that predict either $Welfare_i$ or $M_i \times Landslide_i$ from the set of characteristics shown in Table 1. This method allows us to control for potential selection along observables and improve statistical power while reducing the risk of false discovery (Belloni et al., 2014). Appendix Tables A11 and A12 show the set of variables selected by the double-lasso procedure for each mechanism of interest.

deviations in our summary index ($p = 0.02$). Damage to the home or land are also associated with worse welfare impacts, but the estimated differences are smaller and not statistically significant. Some households outside the landslides' paths report casualties and home damage from the landslide: this is likely due to flooding associated with the landslides. Column 1 of Appendix Table A13 shows that deaths and damage to households outside the landslides' path have roughly similar impacts on our welfare summary index.

Interpretation. The role of casualties in explaining our results may help reconcile our findings when compared to those of Chiovelli et al. (2021), or to other settings where death rates were lower. Reliable data on deaths caused by the the Mozambican civil war are sparse, but the death rate among the displaced was certainly lower than in our setting, where 35% of displaced households experienced the death of a member due to the landslide.[28] Most importantly, the death rate among the non-displaced was likely also high during the Mozambican civil war—possibly even higher than the rate among the displaced—whereas in our setting, non-displaced households outside a landslide path experienced a much lower death rate of around 5%. However, other natural disasters found to have positive long-run economic effects—such as the 2004 Indian Ocean tsunami (Heger and Neumayer, 2019)—caused enormous death tolls and damages, suggesting that destruction and deaths are unlikely to be the sole factors driving our results.

4.3 The Role of Displacement

Around two-thirds of households hit by a landslide were displaced outside their home villages, and about half of these remain displaced by the time of our survey. Variation in the extent of landslide damages within a village led some households to be displaced in groups, while others were displaced alone. Overall, 66% of displaced households reported being displaced along with others from their origin village. Among all displaced households, 23% reported that the government organized the move, with the rest organizing the move by themselves. In Table 4 columns 2–6, we estimate differential welfare effects of landslides depending on whether and how the household was displaced using (2).

Displaced households appear much worse off than those that were hit, but not displaced, by

[28] Estimates of the number of direct civilian killings range from early estimates as high as 100,000 (Gersony, 1988) to more recent, lower estimates of 829 (Weinstein and Francisco, 2005). Many more died of famines during the war, with an estimated number of total civilian deaths around 600,000 (Africa Watch, 1997). The number displaced was much higher, around four million (Chiovelli et al., 2021).

landslides. Column 2 of Table 4 shows a 0.40-sd greater welfare loss among households displaced by landslides ($p = 0.05$). This difference is pronounced among households displaced to locations without any origin connections: for these households, displacement is associated with a 0.71-sd greater welfare loss compared to the non-displaced ($p < 0.01$), as shown in Column 3. Displacement with members of the origin network mitigates these differences by an estimated 0.51 sd ($p = 0.01$). Government support appears to partly substitute for the support provided by social networks: households whose displacement was managed by the government experience mitigated welfare losses, by 0.27 sd ($p = 0.23$), although the difference is not statistically significant, as shown in Column 4.

Column 5 further decomposes welfare differences depending on whether the displaced household remains displaced at the time of our survey, or whether they have returned to the origin village. We find that welfare differences associated with displacement are largely mitigated among households that remain in the destination ($p < 0.01$), possibly because these households successfully established themselves and did not need to return.[29] This difference is much smaller for households displaced by the government, as shown in column 6. As shown in Appendix Table A13, these results are largely similar in difference-in-differences specifications, indicating that displacement effects for households outside the landslides' path are small relative to effects for those inside. This is consistent with displacement of households inside the landslide paths being relatively involuntary compared to moves for households outside the paths.

Interpretation. We do not find that displaced households were negatively selected on pre-landslide characteristics, or experienced a greater rate of casualties or property damage, as shown in Appendix Table A11, which shows correlates of displacement selected by lasso regression. In fact, there is positive selection into displacement along pre-landslide income. Instead, these findings suggest that displacement itself worsens long-run outcomes. In the next section, we suggest and test a potential explanation: displacement disrupts social networks, which matter for households' ability to establish new livelihoods in the destination.

[29]Indeed, only about one-third of returnees reported a reason for returning that reflects an apparently voluntary choice, such as to reclaim land, because they did not like life in the destination, or because others were also moving back. The rest move back because they can no longer afford living in the destination, or only had temporary arrangements.

Table 4: The Role of Casualties, Displacement, and Migration in Welfare Impacts

Dep Var: Overall Welfare Index	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Casualty × Landslide	-0.528**							-0.781***
	(0.230)							(0.203)
	[0.02]							[0.00]
Damage to Home × Landslide	-0.136							-0.034
	(0.235)							(0.209)
	[0.56]							[0.87]
Damage to Land × Landslide	-0.205							-0.359*
	(0.228)							(0.210)
	[0.37]							[0.09]
Displaced × Landslide		-0.397**	-0.711***	-0.467**	-0.651***	-0.736***		-0.924***
		(0.198)	(0.239)	(0.214)	(0.214)	(0.222)		(0.248)
		[0.05]	[0.00]	[0.03]	[0.00]	[0.00]		[0.00]
Displaced with Network × Landslide			0.514**					0.474**
			(0.208)					(0.237)
			[0.01]					[0.05]
Displaced by Gov't × Landslide				0.273		0.455		0.282
				(0.229)		(0.410)		(0.326)
				[0.23]		[0.27]		[0.39]
Remains Displaced × Landslide					0.663***	0.806***		0.910***
					(0.226)	(0.279)		(0.255)
					[0.00]	[0.00]		[0.00]
Remains in Gov't Displacement × Landslide						-0.665		-0.601
						(0.494)		(0.428)
						[0.18]		[0.16]
# Urban Migrants × Landslide							0.114	0.067
							(0.098)	(0.089)
							[0.25]	[0.45]
Observations	625	625	625	625	625	625	625	625
Demographic × Landslide Controls	X	X	X	X	X	X	X	X
Damage × Landslide Controls		X	X	X	X	X	X	X

Notes: Each column is a regression. Dependent variable is a standardized index of current welfare. *Displaced with Network* = 1 if the household was displaced with connections in the origin village, and *Displaced by Gov't* = 1 if displacement was required by the government. All regressions include a control for *Landslide* (not shown), a landslide-event fixed effect, geologic controls at the pre-landslide location, a pre-landslide welfare index, and additional controls chosen through double lasso regression from all pre-landslide controls shown in Table 1, the four landslide damage variables shown in Table 2, and their interactions with *Landslide* (damage controls excluded from Column 1). Robust standard errors in parentheses; two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

4.3.1 The Role of Social Capital in Displacement Outcomes

Given the apparent importance of networks in driving welfare impacts, we further investigate the role of social networks by examining households' outcomes during displacement. We collected survey data on each household's main source of income during displacement regardless of whether they had returned to the origin village by that time. Because resettling in the displacement destination—as opposed to returning to the origin village—strongly predicts better post-disaster recovery, we also analyze the role of networks in influencing return decisions. To do so, we estimate versions of (2) on the full set of ever-displaced households,[30] with outcomes during displacement as the dependent variable.

We use two approaches to measure whether households were displaced with others from their origin. The first is the survey question asking displaced households whether they knew anyone from their origin village in the destination. This measure is precise, but may be correlated with the size of overall networks prior to landslides. Our second approach uses quasi-random variation in village-level landslide damages. While the share of displaced households within a village is likely to be influenced by village-level characteristics such as mean income, or ties with other villages, the share of households hit by the landslide is likely to be driven largely by how far down the slope, and in which direction, the destabilized earth traveled.[31] This second measure is coarser, but more plausibly exogenous than the household-level variation. Our assumption is that, among displaced households, the share of households hit by the landslide is unlikely to be strongly correlated with other determinants of outcomes during displacement. Nevertheless, we control for village-level means of the same demographic and damage variables used in (2).

We find that households displaced with others from their origin village were much more likely to remain in the destination (by 24 pp., $p = 0.01$), as shown in column 1 of Table 5, Panel A. This is consistent with origin networks supporting better displacement experiences, as these households were 25 pp. more likely to find a livelihood in the destination other than farm income compared to those displaced without origin connections ($p = 0.02$), as shown in column 4. Households are also more likely to remain in government-organized displacement compared to own-displacement (by

[30]In our main analysis, we use all displaced households including those originally located outside Bududa and so exclude the control vector $Risk_i$, which is not observed for households outside Bududa. The sample size available within Bududa alone is small, and post-double lasso estimates are not identified. Appendix Table A14 shows estimates within the Bududa sample, excluding the control vector X_i but including the control vector $Risk_i$. Results are consistent, though generally noisier.

[31]The share of households hit strongly predicts the share displaced (coeff. $= 0.88$, $p < 0.001$) and, among the displaced, the likelihood of being displaced with others from the same village (coeff. $= 0.19$, $p = 0.10$).

35 pp., $p < 0.01$), as shown in column 2. There is no significant difference in non-farm employment for these households, possibly because of increased availability of government support.

Relying on village-level variation in landslide damages gives similar results, as shown in Panel B of Table 5. In specifications with the full set of control variables (columns 3 and 6), moving from 0% to 100% of households in a village being hit by a landslide increases the probability of remaining in the destination by 72 pp. ($p = 0.11$), and of making a living outside of farming by 92 pp. ($p < 0.01$).

4.3.2 Did Households Displaced Alone Go to Different Locations?

We find that households displaced with others from their origin were more likely to remain in the destination, and were more likely to find non-farm livelihoods there, compared to those displaced alone. This suggests a role for social support in determining outcomes during displacement. However, other interpretations are possible: in particular, households displaced alone may have gone to "worse" destinations where it was harder to establish a livelihood. To test for locational effects such as these, we repeat the strategy in Section 4.3.1 and estimate versions of (2) on the full set of ever-displaced households, putting various features of the displacement destination on the left hand side.

Those displaced alone were no more likely to leave

We do not find that households displaced with others from their origin villages, or from villages where a high share of households resided within a landslide path, went to systematically better locations than those displaced alone. In fact, we find some evidence of the reverse: those displaced with others went to locations with lower average human capital, measured by average educational attainment among those living there prior to the landslide.

4.3.3 Interpretation

The few papers studying natural-disaster impacts on households in developing countries focus on events in which displacement was rare (see Appendix B). Disaster studies in rich countries have found positive impacts of displacement (Sacerdote, 2012, Deryugina et al., 2018, Nakamura et al., 2021), and studies of conflict displacement have found positive impacts across a broad array of settings (Becker and Ferrara, 2019, Chiovelli et al., 2021, Sarvimäki et al., 2022). One might therefore expect the total effect of disaster and displacement to be no worse—perhaps even to improve on—the effect of disaster alone. In light of these other findings, the role played by displacement in driving welfare impacts in our setting is surprising.

Table 5: Displacement with other in-network households, or when coordinated by the government, is associated with better outcomes.

	(1) Remains Displaced	(2) Remains Displaced	(3) Remains Displaced	(4) Found Non-Farm Income in Destination	(5) Found Non-Farm Income in Destination	(6) Found Non-Farm Income in Destination
Panel A: Household-Level Variation						
Displaced with Network × Landslide	0.235**		0.178*	0.249**		0.254**
	(0.095)		(0.096)	(0.104)		(0.110)
	[0.01]		[0.06]	[0.02]		[0.02]
Displaced by Gov't × Landslide		0.348***	0.301***		0.068	-0.015
		(0.086)	(0.090)		(0.115)	(0.120)
		[0.00]	[0.00]		[0.56]	[0.90]
Observations	277	277	277	154	154	154
Demographic × Landslide Controls	X	X	X	X	X	X
Damage × Landslide Controls	X	X	X	X	X	X
Panel B: Village-Level Variation						
Share of Village Hit × Landslide	0.700***	0.700***	0.719	0.504*	0.778***	0.916***
	(0.211)	(0.205)	(0.455)	(0.292)	(0.191)	(0.284)
	[0.00]	[0.00]	[0.11]	[0.10]	[0.00]	[0.00]
Observations	277	277	277	154	154	154
Demographic × Landslide Controls		X	X		X	X
Damage × Landslide Controls			X			X

Notes: Sample includes all households displaced by a landslide, regardless of current location. Each column within a panel is a regression. *Remains Displaced* = 1 if the household has not returned to the origin. *Found Non-Farm Income in Destination* = 1 if the household reported a primary income source while displaced other than farming or agricultural labor, based on a question added to the household survey partway through field work. *Displaced with Network* = 1 if the household was displaced with connections in the origin village, and *Displaced by Gov't* = 1 if displacement was required by the government. *Share of Village Hit* is the mean of *Landslide* within the household's pre-landslide village. All regressions include a control for *Landslide* (not shown), a landslide-event fixed effect, and additional controls chosen through double lasso regression from all pre-landslide controls shown in Table 1 plus the four landslide damage variables shown in Table 2—or their village-level means, in Panel B—and their interactions with *Landslide*. Standard errors in parentheses are heteroskedasticity-robust in Panel A and clustered at the pre-landslide village level in Panel B; two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The existing displacement literature highlights the role of location-specific factors combined with mobility barriers to explain the positive impacts of displacement on economic outcomes. However, displacement effects do not appear to operate purely through location-specific disadvantages in our setting. If they did, we would expect households that returned from displacement to fare better than those that remained, when in fact we observe the opposite. Additionally, most displaced households moved to other rural locations in the same region. These findings are also difficult to reconcile with the loss of origin-specific factors, such as location-specific knowledge, being the primary driver of welfare impacts. Instead, our findings point to the maintenance of social capital as a key driver of post-displacement economic success.

We also find a role for external assistance in mitigating the negative impacts of displacement, consistent with findings in Gignoux and Menéndez (2016) and del Valle et al. (2020).[32] This may explain the divergence between our results and those in the extant literature. In high-income countries, government agencies were substantially more involved in resettlement and reconstruction efforts: in the volcanic eruption studied in Nakamura et al. (2021), households were compensated for the value of their homes, and after Hurricane Katrina support from the National Guard and infrastructure reconstruction, aid, and insurance payments were massive (Deryugina et al., 2018).[33] Displaced Finnish communities studied in Sarvimäki et al. (2022) were given land and assistance establishing new farms. In the studies of natural disasters in developing countries, the aid receipts that followed were massive, in the case of the 2004 Indonesian tsunami exceeding estimated damages (Heger and Neumayer, 2019). External assistance was much rarer in our setting—-about 8% of damages, close to the average for large natural disasters (see footnote 1)—and the government managed only a minority of displacements to temporary resettlement camps.

4.4 The Role of Urban Migration

In contrast to predominantly rural household displacement, individual migration in response to landslides was entirely urban (see Table 2). Spatial earnings gaps are substantial in most developing economies, including Uganda (Gollin et al., 2014), but gains from rural-to-urban migration may

[32]At the level of firms, De Mel et al. (2012) also finds a role for external aid in economic recovery following a disaster. At the macro-economic level, Noy (2009b) find that several measures of institutional capacity to respond to disasters predict better post-disaster recovery.

[33]Nakamura et al. (2021) find that displacement improves outcomes only for children—whereas older individuals were slightly worse off—which the authors interpret as consistent with reoptimizing education and career choices, which is harder for older individuals. Given that most households were displaced to similar, rural locations, the scope for such reoptimization is likely to be low in our setting.

remain uncaptured due to frictions such as information failures (Baseler, 2023). Natural disasters may improve economic outcomes if they encourage migration to locations with higher productivity (Deryugina et al., 2018) or that better match individuals' comparative advantages (Nakamura et al., 2021). However, it is unclear whether disaster-driven migration affords the same benefits as other migration. In this section, we estimate how landslide impacts varied based on rural-urban migration behavior afterward using (2).

We find that affected households that sent more migrants to cities after the landslide fared modestly better, as shown in column 7 of Table 4, although the difference is not statistically significant (coeff. = 0.1 sd per urban migrant, $p = 0.25$). However, this estimate masks differences between landslide-induced migration and other migration. In our data, urban migration by unaffected household is associated with lower welfare, consistent with individuals migrating in response to negative shocks. In the difference-in-differences specification shown in column 7 of Appendix Table A13, each urban migrant from a landslide-affected household is associated with a 0.18-sd increase in our welfare index, compared to an urban migrant from an unaffected household ($p = 0.09$).

Why is urban migration after a landslide associated with greater welfare compared to other forms of migration? We consider two potential explanations. First, landslides may induce higher-return individuals to migrate (relative to migrants from unaffected households), for example by lowering their relative utility from staying in the village. In this case, we would expect to see more positive selection of migrants—along variables that predict destination earnings, such as schooling or past migration experience—from affected compared to unaffected households. Second, the returns to migration may be higher for landslide-affected households if urban migration helps households cope with the negative shock. To distinguish between these explanations, we estimate selection-into-migration regressions separately among affected and unaffected households. Results are shown in Table 6.

Our findings are not consistent with landslides' inducing higher-return individuals to migrate: if anything, urban migrants from affected households appear somewhat less positively selected compared to urban migrants from unaffected households. Across these two groups, the associations between urban migration and age, gender, and household size are statistically indistinguishable. However, selection into urban migration is less positive within affected households along education ($p = 0.19$), individual migration experience to the regional capital prior to the landslide ($p = 0.02$), our pre-landslide welfare index ($p = 0.02$), and an indicator for having family living in the capital at the time of the landslide ($p = 0.01$).

Interpretation. Our finding that urban migration appears to mitigate the harmful impacts of landslides—despite less positive selection of disaster-induced migrants—suggests that the marginal return to rural-urban migration is positive in this context, at least for households affected by landslides. This is consistent with the literature showing that migration can help households cope with negative weather shocks, including natural disasters (Yang and Choi, 2007, Yang, 2008, Blumenstock et al., 2016, Mahajan and Yang, 2020). The finding that most individuals induced to migrate by the landslides remain in the destination by the time of our survey is also consistent with positive returns. However, the finding that overall welfare and employment effects are nevertheless significantly negative indicates that the benefits of migrating are not high enough to offset the negative impacts of landslides.

Our finding that individuals induced to migrate by landslides are less positively selected compared to other migrants from the same area is consistent with many studies of forced migration.[34] These patterns are suggestive of disaster-induced migration being used as a coping device.

4.5 Potential Alternative Explanations

In this section, we consider potential alternative explanations for our results, including impacts on nearby households such as through general equilibrium effects, measurement error in assigning landslide exposure, differential non-response, and differential risk mitigation, and argue that they are unlikely to be driving our findings.

Impacts on Nearby Households. Our identification strategy compares households residing in a landslide path with nearby households, which we show were exposed to similar levels of landslide risk. It is likely, however, that our comparison group was indirectly affected by the landslides. For example, around half of households we classify as unaffected report that some of their land was damaged during the landslide. There are also likely to be disruptions to economic activity resulting from school or health clinic closures, road closures, and market disruptions. To the extent that these broader market effects have dissipated by the time of our surveys, 3–12 years after the landslides, they should not significantly affect our results. Moreover, any economic disruptions affecting our

[34] See, for example, Cortes (2004), Chin and Cortes (2015), and Dustmann et al. (2017). Forced migrants may also be relatively positively selected in contexts where the value of home amenities reduces emigration among the well-off (Abramitzky et al., 2022) or when persecution threatens income or wealth directly (Aksoy and Poutvaara, 2021). In general, the sign of migrant selection is likely to depend on wealth: see Bazzi (2017).

Table 6: Impact of Landslide on Selection Into Migration

Outcome: Individual is an Urban Migrant	(1) Coefficient (Unaffected)	(2) Coefficient (Affected)	(3) Difference (2–1)	(4) p-Value on Difference	(5) N
Individual Characteristics					
Age (Years)	-0.003	-0.003	-0.000	0.97	1,814
Female	0.037	0.074	0.037	0.48	1,814
Education (Years)	0.016	0.003	-0.013	0.19	1,814
Had Migrated Before Landslide	-0.065	-0.078	-0.013	0.87	1,814
Had Migrated to City Before Landslide	-0.061	-0.069	-0.008	0.92	1,814
Had Migrated to Big City Before Landslide	-0.054	-0.132	-0.078	0.31	1,814
Had Migrated to Mbale Before Landslide	-0.107	-0.231	-0.124**	0.02	1,814
Had Migrated to Kampala Before Landslide	-0.019	0.042	0.061	0.72	1,814
Had Migrated to Nairobi Before Landslide	-0.074	-0.226	-0.152***	0.00	1,814
Household Characteristics					
Household Size (Pre-Landslide)	0.011	0.019	0.009	0.57	1,814
# Adult Equivalents (Pre-Landslide)	0.023	0.034	0.012	0.52	1,814
Large Farm (1 Acre or Larger, Pre-Landslide)	0.062	-0.017	-0.079	0.32	1,814
Farm Size (Acres, Pre-Landslide)	0.029	-0.001	-0.030	0.23	1,814
Anyone Had Migrated Before Landslide	0.009	-0.001	-0.011	0.88	1,814
Had Family in Big City at Time of Landslide	0.043	0.048	0.006	0.94	1,814
Had Family in Kampala at Time of Landslide	0.049	-0.113	-0.162**	0.01	1,814
Income (Pre-Landslide)	0.007	0.027	0.020	0.63	1,814
Income per Adult-Equivalent (Pre-Landslide)	-0.027	-0.036	-0.009	0.91	1,814
Savings (Pre-Landslide)	0.018	0.005	-0.013	0.78	1,814
Savings per Adult-Equivalent (Pre-Landslide)	-0.000	-0.089	-0.089	0.31	1,814
Welfare Index (Pre-Landslide)	0.023	-0.049	-0.072**	0.02	1,814

Notes: An observation is an individual residing in a pre-landslide household. Columns 1 and 2 show coefficients from regressions of each characteristic on an indicator for whether the individual is an urban migrant, an indicator for whether the household was hit by a landslide, and the interaction of those two variables. Columns 3 and 4 show the difference in coefficients for affected compared to unaffected households and the p-value on that difference, respectively. All regressions control for a landslide-event fixed effect and geologic controls, and cluster standard errors at the household level. Currency units are 100s of USD/month. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

comparison group will lead us to understate the overall negative impacts of landslides on affected households.[35]

To test whether impacts on nearby households residing outside of a landslide path are driving our results, we consider two proxies likely to capture indirect effects: whether the household resided nearby—outside of the 50-meter buffer, but within 1 kilometer of—a landslide site, and whether a landslide hit any households within the same village. Provided that indirect effects of

[35]Positive welfare impacts on our comparison group are highly unlikely. The literature on natural disasters has highlighted two mechanisms through which directly or indirectly affected households can benefit: displacement to more advantageous locations, and aid. In our setting displacement appears to worsen the impacts of landslides, and aid receipts were very small compared to damages. Consistent with this, the results in Appendix Tables A3 and A4 suggest small, negative impacts on nearby but not directly affected households.

landslides are stronger closer to the landslide site, or within the same village as the households that were hit, we expect these proxies to capture the most important indirect effects. We then estimate direct and indirect welfare effects of landslides by using completely unaffected households (that is, neither directly nor indirectly affected) as a comparison group, with the caveat that our identifying assumptions are less likely to hold when comparing indirectly affected to completely unaffected households. To partly mitigate this concern, we include the full set of pre-landslide geological and demographic controls when estimating indirect effects.

We find that nearby and same-village households look largely similar on our welfare index to far-away households, or to households in villages with no landslides, respectively. As shown in column 7 of Appendix Table A3, our welfare index is 0.10 sd lower among households within 1 km of a landslide site, or 0.02 sd lower among households with a landslide in the village, and neither difference is statistically significant at the 10% level. Welfare impacts derived by comparing affected to completely unaffected households remain negative and statistically significant. Appendix Table A4 replicates this analysis on the broader sample including Manafwa and Sironko districts, with very similar results. In this broader sample, the estimated indirect effects are significantly negative for households within 1 km of a landslide (coeff $= -0.20$, p-val $= 0.08$). We conclude that our key findings are unlikely to be explained by impacts on the comparison group: if anything, indirect impacts are leading us to understate direct impacts.

Measurement Error in Identifying Affected Households. As discussed in Section 2.3, our preferred method for identifying directly affected households involves overlaying landslide paths traced using satellite imagery with GPS readings taken at households' pre-landslide locations, as identified by the household members together with local leaders, and allowing for a 50-meter buffer to account for rubble or destabilized land not visible from satellite images. Our choice of a 50-meter buffer is motivated by a discontinuity in damage rates at that point, as shown in Appendix Figure A1. The much higher casualty and destruction rates in the group we identify as directly affected (see Table 2) support this method, as does the strong relationship between destruction rates and measured distance from the landslide site shown in Appendix Figure A1. Nevertheless, we consider two alternative classification methods: i) omitting the 50-meter buffer, and ii) using household survey reports of damage to their home during the landslide. Our impact estimates are robust to these alternative measures, as shown in Appendix Tables A5 and A6.

Differential Non-Response of Displaced Households. While our overall survey rate among households living in our study regions at the time of the landslide was very high, we were less likely to successfully survey households which had moved away from their home village after the landslide, as shown in Appendix Table A1. Our survey rate for households still residing in the origin was 94.4%; for households that had moved away and not returned it was 78.3%. To test whether this differential survey rate is influencing our results, we estimate Lee bounds, imputing residence in a landslide path for unsurveyed households based on administrative data (see footnote 12). These bounds are generally tight, and do not significantly alter our estimated landslide impacts on households, as shown in Appendix Table A15. We also reproduce our main results weighting observations by the inverse probability of being surveyed, estimated by lasso logistic regression. Results, shown in Appendix Table A16, are extremely similar to unweighted results. We conclude that differential attrition is not a significant factor in this study. Moreover, we note that a conventional study design would not have surveyed any households that had relocated prior to sampling; our study thus offers a rare opportunity to analyze the impact of disaster and displacement on the full affected population with modest non-response even among the displaced.

Differential Risk Mitigation. Even if our comparison group faced similar risks of being hit by a landslide—as our analysis indicates—it may still be that some households invest in landslide-mitigation technologies which influence the degree of landslide damages conditional on residing in the path of the landslide. Note that these investments are not a threat to our identification of welfare impacts unless they are correlated with *Landslide*, and our analysis—together with local reports (see footnote 17)—indicates that exact landslide paths are unpredictable. However, if these investment decisions are a function of pre-landslide characteristics that also influence how households cope with the landslides' effects, this would complicate our mechanisms analysis. However, the robustness of that analysis to a principled set of controls—including landslide damages, which mitigation technologies would most directly affect—provides some reassurance that other pre-landslide characteristics such as risk mitigation are not responsible for our findings.

5 Discussion

Natural disasters displace millions of people a year, but are difficult to study because displacement complicates the collection of reliable data on affected populations, and because people tend to sort out of high-risk areas. Most studies of natural disasters involving significant displacement also focus on developed countries with compensation schemes and welfare nets, although most displacement occurs in developing countries. We overcome these challenges by combining information on exact landslide paths and pre-existing landslide risk—which together produce quasi-random variation in destruction within affected areas—with complete administrative lists of the set of households residing in the affected villages at the time of the landslide event. This allows us to estimate the average causal impact of landslides on nearly the full affected population. Extensive tracking of relocated households made this possible in a setting with high rates of displacement. This study thus offers what we believe is the first rigorous economic analysis of the household-level impact of a natural disaster that caused high rates of death and displacement but where victims received little aid or assistance. Aid receipts in our setting were typical of large natural disasters in developing countries, which increases the generalizability of these results.

We find that landslide-affected households were more likely to be displaced to a rural location, more likely to send migrants to urban locations, less likely to have economically active members, and appear significantly worse off along several economic and mental health dimensions. The negative impacts on welfare are pronounced among households that were displaced by the landslide, although displacement with others from the origin, government resettlement administration, and urban migration appear to attenuate negative impacts. This may explain why other studies of natural disasters—which study contexts in which displacement is rare or the government or civil society actors provided substantial humanitarian and development aid—often find small or even positive long-run economic impacts.

In studies of natural disasters involving significant displacement, the economic benefits of displacement appear to be due to location-specific advantages at the destination (Deryugina et al., 2018, Nakamura et al., 2021). In our setting, most displaced households moved to villages and rural relocation centers throughout eastern Uganda, which are unlikely to offer locational advantages relative to households' home villages. Our findings, together with those of the extant literature, thus point to the importance of the destination in determining long-run outcomes for the displaced. Another contribution of our study is the apparent role of maintaining social ties during displacement in attenuating its negative effects. We also find that casualties within the household worsen the impact of the disaster. This is perhaps unsurprising, but it should be noted that such casualties are much rarer in developed-country settings. Taken together, our findings indicate that the positive economic impacts of displacement observed in the economic literature are unlikely to apply to many low-income settings.

The Ugandan government has long considered permanent relocation of households living in areas at risk of landslides, but thousands of households continue to reside in these areas. While the findings of this study bolster calls for resettlement, we note that no study we are aware of has estimated the impact of pre-landslide resettlement. In addition to better understanding the value of *ex-ante* relocation, future work could help to identify who would benefit most from *ex-post* resettlement in the event that *ex-ante* resettlement is not possible or not desirable, and help to understand the factors contributing to successful resettlement efforts.

References

Abramitzky, Ran, Travis Baseler, and Isabelle Sin, "Persecution and Migrant Self-Selection: Evidence from the Collapse of the Communist Bloc," Working Paper 30204, National Bureau of Economic Research July 2022.

Africa Watch, "Conspicuous Destruction: War, Famine and the Reform Process in Mozambique," *Human Rights Watch*, 1997.

Akoyi, Kevin Teopista and Miet Maertens, "Walk the talk: private sustainability standards in the Ugandan coffee sector," *The Journal of Development Studies*, 2018, *54* (10), 1792–1818.

Aksoy, Cevat Giray and Panu Poutvaara, "Refugees' and irregular migrants' self-selection into Europe," *Journal of Development Economics*, 2021, *152*, 102681.

Anderson, Michael L., "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American Statistical Association*, 2008, *103* (484), 1481–1495.

Baseler, Travis, "Hidden Income and the Perceived Returns to Migration," *American Economic Journal: Applied Economics (forthcoming)*, 2023.

_ , Ambar Narayan, Odyssia Ng, and Sutirtha Sinha Roy, "Does Food Insecurity Hinder Migration? Experimental Evidence from the Indian Public Distribution System," 2023.

Bazzi, Samuel, "Wealth Heterogeneity and the Income Elasticity of Migration," *American Economic Journal: Applied Economics*, April 2017, *9* (2), 219–55.

Becker, Sascha O. and Andreas Ferrara, "Consequences of forced migration: A survey of recent findings," *Labour Economics*, 2019, *59*, 1–16. Special Issue on "European Association of Labour Economists, 30th annual conference, Lyon, France, 13-15 September 2018.

Becker, Sascha O, Irena Grosfeld, Pauline Grosjean, Nico Voigtländer, and Ekaterina Zhuravskaya, "Forced migration and human capital: Evidence from post-WWII population transfers," *American Economic Review*, 2020, *110* (5), 1430–1463.

Beegle, Kathleen, Joachim De Weerdt, and Stefan Dercon, "Migration and Economic Mobility in Tanzania: Evidence from a Tracking Survey," *The Review of Economics and Statistics*, 2011, *93* (3), 1010–1033.

Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen, "High-Dimensional Methods and Inference on Structural and Treatment Effects," *Journal of Economic Perspectives*, 2014, *28* (2), 1–23.

Blumenstock, Joshua, Nathan Eagle, and Marcel Fafchamps, "Airtime transfers and mobile communications: Evidence in the aftermath of natural disasters," *Journal of Development Economics*, 2016, *120* (C), 157–181.

Broeckx, Jente, Michiel Maertens, Moses Isabirye, Matthias Vanmaercke, Betty Namazzi, Jozef Deckers, Joseph Tamale, Liesbet Jacobs, Wim Thiery, Matthieu Kervyn et al., "Landslide susceptibility and mobilization rates in the Mount Elgon region, Uganda," *Landslides*, 2019, *16*, 571–584.

Bryan, Gharad, Shyamal Chowdhury, and Ahmed Mushfiq Mobarak, "Underinvestment in a Profitable Technology: The Case of Seasonal Migration in Bangladesh," *Econometrica*, 2014, *82* (5), 1671–1748.

Cai, Shu, "Migration under liquidity constraints: Evidence from randomized credit access in China," *Journal of Development Economics*, 2020, *142* (C).

Caruso, Germán and Sebastian Miller, "Long run effects and intergenerational transmission of natural disasters: A case study on the 1970 Ancash Earthquake," *Journal of Development Economics*, 2015, *117*, 134–150.

Caruso, Germán Daniel, "The legacy of natural disasters: The intergenerational impact of 100 years of disasters in Latin America," *Journal of Development Economics*, 2017, *127*, 209–233.

Cattaneo, Cristina and Giovanni Peri, "The migration response to increasing temperatures," *Journal of Development Economics*, 2016, *122*, 127–146.

Cavallo, Eduardo, Sebastian Galiani, Ilan Noy, and Juan Pantano, "Catastrophic natural disasters and economic growth," *Review of Economics and Statistics*, 2013, *95* (5), 1549–1561.

Centre, Norwegian Refugee Council Internal Displacement Monitoring, "Global Report on Internal Displacement 2021," 2022.

Chin, Aimee and Kalena E. Cortes, *The refugee/Asylum seeker*, 1 ed., Vol. 1, Elsevier Inc., 2015.

Chiovelli, Giorgio, Stelios Michalopoulos, Elias Papaioannou, and Sandra Sequeira, "Forced Displacement and Human Capital: Evidence from Separated Siblings," Working Paper 29589, National Bureau of Economic Research December 2021.

Claessens, L, Anke Knapen, MG Kitutu, Jean Poesen, and Jozef A Deckers, "Modelling landslide hazard, soil redistribution and sediment yield of landslides on the Ugandan footslopes of Mount Elgon," *Geomorphology*, 2007, *90* (1-2), 23–35.

Claessens, Lieven, Mary G Kitutu, Jean Poesen, and Jozef A Deckers, "Landslide hazard assessment on the ugandan footslopes of Mount Elgon: the worst is yet to come," in "Landslide Science and Practice," Springer, 2013, pp. 527–531.

Conley, T.G., "GMM estimation with cross sectional dependence," *Journal of Econometrics*, 1999, *92* (1), 1–45.

Cortes, Kalena E, "Are Refugees Different From Economic Immigrants? Some Empirical Evidence On The Heterogeneity Of Immigrant Groups In The United States," *Review of Economics and Statistics*, 2004, *86* (2), 465–480.

De Janvry, Alain, Kyle Emerick, Marco Gonzalez-Navarro, and Elisabeth Sadoulet, "Delinking land rights from land use: Certification and migration in Mexico," *American Economic Review*, 2015, *105* (10), 3125–3149.

del Valle, Alejandro, Alain de Janvry, and Elisabeth Sadoulet, "Rules for Recovery: Impact of Indexed Disaster Funds on Shock Coping in Mexico," *American Economic Journal: Applied Economics*, October 2020, *12* (4), 164–95.

Deryugina, Tatyana, Laura Kawano, and Steven Levitt, "The Economic Impact of Hurricane Katrina on Its Victims: Evidence from Individual Tax Returns," *American Economic Journal: Applied Economics*, April 2018, *10* (2), 202–33.

Deuchert, Eva and Christina Felfe, "The tempest: Short-and long-term consequences of a natural disaster for children s development," *European Economic Review*, 2015, *80*, 280–294.

Dustmann, Christian, Francesco Fasani, Tommaso Frattini, Luigi Minale, and Uta Schönberg, "On the economics and politics of refugee migration," *Economic Policy*, 2017, *32* (91), 497–550.

Eisensee, Thomas and David Strömberg, "News Droughts, News Floods, and U. S. Disaster Relief*," *The Quarterly Journal of Economics*, 05 2007, *122* (2), 693–728.

Felbermayr, Gabriel and Jasmin Gröschl, "Naturally negative: The growth effects of natural disasters," *Journal of development economics*, 2014, *111*, 92–106.

Gersony, Robert, "Mozambique Refugee Accounts of Principally Conflict-Related Experiences in Mozambique," 1988. State Department, Washington.

Gignoux, Jérémie and Marta Menéndez, "Benefit in the wake of disaster: Long-run effects of earthquakes on welfare in rural Indonesia," *Journal of Development Economics*, 2016, *118*, 26–44.

Gollin, Douglas, David Lagakos, and Michael E Waugh, "The Agricultural Productivity Gap," *Quarterly Journal of Economics*, 2014, *129* (2), 939–993.

Gray, Clark, Elizabeth Frankenberg, Thomas Gillespie, Cecep Sumantri, and Duncan Thomas, "Studying displacement after a disaster using large-scale survey methods: Sumatra after the 2004 tsunami," *Annals of the Association of American Geographers*, 2014, *104* (3), 594–612.

Gröger, André and Yanos Zylberberg, "Internal labor migration as a shock coping strategy: Evidence from a typhoon," *American Economic Journal: Applied Economics*, 2016, *8* (2), 123–53.

Hamory, Joan, Marieke Kleemans, Nicholas Y Li, and Edward Miguel, "Reevaluating Agricultural Productivity Gaps with Longitudinal Microdata," *Journal of the European Economic Association*, 11 2020, *19* (3), 1522–1555.

Heger, Martin Philipp and Eric Neumayer, "The impact of the Indian Ocean tsunami on Aceh's long-term economic growth," *Journal of Development Economics*, 2019, *141*, 102365.

Hsiang, Solomon M and Amir S Jina, "The Causal Effect of Environmental Catastrophe on Long-Run Economic Growth: Evidence From 6,700 Cyclones," Working Paper 20352, National Bureau of Economic Research July 2014.

IDMC, "Disaster Displacement: A global review, 2008–2018.," *Internal Displacement Monitoring Centre*, 2019.

Imbert, Clément and John Papp, "Costs and benefits of rural-urban migration: Evidence from India," *Journal of Development Economics*, 2020, *146*, 102473.

Independent, The, "Revisiting Nametsi 10 years after landslides," *The Independent, Uganda*, 2020.

Joseph, Iverson-Love, "The effect of natural disaster on economic growth: Evidence from a major earthquake in Haiti," *World Development*, 2022, *159* (C), S0305750X22002431.

Kabunga, Amir, Ponsiano Okalo, Viola Nalwoga, and Brenda Apili, "Government relocates more landslide victims," 2020. February 15, 2020. https://www.monitor.co.ug/uganda/news/national/government-relocates-more-landslide-victims-1875474.

_ , _ , _ , and _ , "Landslide disasters in eastern Uganda: post-traumatic stress disorder and its correlates among survivors in Bududa district," *BMC Psychology*, 2022, *10* (287).

Knapen, Anke, M Goretti Kitutu, Jean Poesen, W Breugelmans, J Deckers, and A Muwanga, "Landslides in a densely populated county at the footslopes of Mount Elgon (Uganda): characteristics and causal factors," *Geomorphology*, 2006, *73* (1-2), 149–165.

Kocornik-Mina, Adriana, Thomas KJ McDermott, Guy Michaels, and Ferdinand Rauch, "Flooded cities," *American Economic Journal: Applied Economics*, 2020, *12* (2), 35–66.

Lagakos, David, "Urban-Rural Gaps in the Developing World: Does Internal Migration Offer Opportunities?," *Journal of Economic Perspectives*, August 2020, *34* (3), 174–92.

_ , Ahmed Mushfiq Mobarak, and Michael E Waugh, "The Welfare Effects of Encouraging Rural-Urban Migration," Working Paper 24193, National Bureau of Economic Research January 2018.

_ and Michael E Waugh, "Selection, Agriculture, and Cross-Country Productivity Differences," *American Economic Review*, 2013, *103* (2), 948–980.

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

Lucas, Robert E.B., "Chapter 13 Internal migration in developing countries," in "in," Vol. 1 of *Handbook of Population and Family Economics*, Elsevier, 1997, pp. 721–798.

Mahajan, Parag and Dean Yang, "Taken by Storm: Hurricanes, Migrant Networks, and US Immigration," *American Economic Journal: Applied Economics*, April 2020, *12* (2), 250–77.

Makwanam, Nikunj, "Disaster and its impact on mental health: A narrative review," *Journal of family medicine and primary care*, 2019, *8* (10), 3090–3095.

Mathiassen, Astrid, Darryl Miller, Aysha Twose, and Amit Wadhwa, "Conducting Food Security Assessments using Household Expenditure Surveys (HES)," *World Food Programme*, 2017.

McKenzie, David, "Beyond baseline and follow-up: The case for more T in experiments," *Journal of Development Economics*, 2012, *99* (2), 210–221.

Mel, Suresh De, David McKenzie, and Christopher Woodruff, "Enterprise Recovery Following Natural Disasters*," *The Economic Journal*, 2012, *122* (559), 64–91.

Monitor, "Landslides kill 1,000 in Bugisu over the past decade.," *Monitor*, 2019.

Morten, Melanie and Jaqueline Oliveira, "The Effects of Roads on Trade and Migration: Evidence from a Planned Capital City," 2023. Working Paper.

Müller, Ulrich K. and Mark W. Watson, "Spatial Correlation Robust Inference," *Econometrica*, 2022, *90* (6), 2901–2935.

Nakamura, Emi, Jósef Sigurdsson, and Jón Steinsson, "The Gift of Moving: Intergenerational Consequences of a Mobility Shock," *The Review of Economic Studies*, 09 2021, *89* (3), 1557–1592.

Norris, Fran H, Matthew J Friedman, Patricia J Watson, Christopher M Byrne, Eolia Diaz, and Krzysztof Kaniasty, "60,000 disaster victims speak: Part I. An empirical review of the empirical literature, 1981–2001," *Psychiatry*, 2002, *65* (3), 207–239.

Noy, Ilan, "The macroeconomic consequences of disasters," *Journal of Development economics*, 2009, *88* (2), 221–231.

_ , "The macroeconomic consequences of disasters," *Journal of Development Economics*, March 2009, *88* (2), 221–231.

OCHA, "Uganda: Floods and Landslides.," *United Nations Office for the Coordination of Human-*

itarian Affairs, 2019.

Péchayre, Marion, "Humanitarian Action in Pakistan 2005–2010: Challenges, Principles, and Politics," 2011. Feinstein International Center.

Pelli, Martino, Jeanne Tschopp, Natalia Bezmaternykh, and Kodjovi M. Eklou, "In the eye of the storm: Firms and capital destruction in India," *Journal of Urban Economics*, 2023, *134*, 103529.

Porter, Matthew and Nick Haslam, "Predisplacement and postdisplacement factors associated with mental health of refugees and internally displaced persons: a meta-analysis," *JAMA*, 2005, *294* (5), 602–612.

Ridley, Matthew, Gautam Rao, Frank Schilbach, and Vikram Patel, "Poverty, depression, and anxiety: Causal evidence and mechanisms," *Science*, 2020, *370* (6522), eaay0214.

Roy, Priyom, Tapas R. Martha, Kirti Khanna, Nirmala Jain, and K. Vinod Kumar, "Time and path prediction of landslides using InSAR and flow model," *Remote Sensing of Environment*, 2022, *271*, 112899.

Sacerdote, Bruce, "When the Saints Go Marching Out: Long-Term Outcomes for Student Evacuees from Hurricanes Katrina and Rita," *American Economic Journal: Applied Economics*, January 2012, *4* (1), 109–35.

Sarvimäki, Matti, Roope Uusitalo, and Markus Jäntti, "Habit Formation and the Misallocation of Labor: Evidence from Forced Migrations," *Journal of the European Economic Association*, 2022, *20* (6), 2497–2539.

Strobl, Eric, "The economic growth impact of natural disasters in developing countries: Evidence from hurricane strikes in the Central American and Caribbean regions," *Journal of Development economics*, 2012, *97* (1), 130–141.

Weinstein, Jeremy M. and Laudemiro Francisco, "The Civil War in Mozambique: The Balance Between Internal and External Influences," in Paul Collier and Nicholas Sambanis, eds., *Paul Collier and Nicholas Sambanis, eds.*, Vol. 1 of *Understanding Civil War*, The World Bank, 2005, pp. 157–192.

World Bank Group, "Climate Risk Profile: Uganda," 2020.

Yang, Dean, "Coping with Disaster: The Impact of Hurricanes on International Financial Flows, 1970-2002," *The B.E. Journal of Economic Analysis & Policy*, June 2008, *8* (1), 1–45.

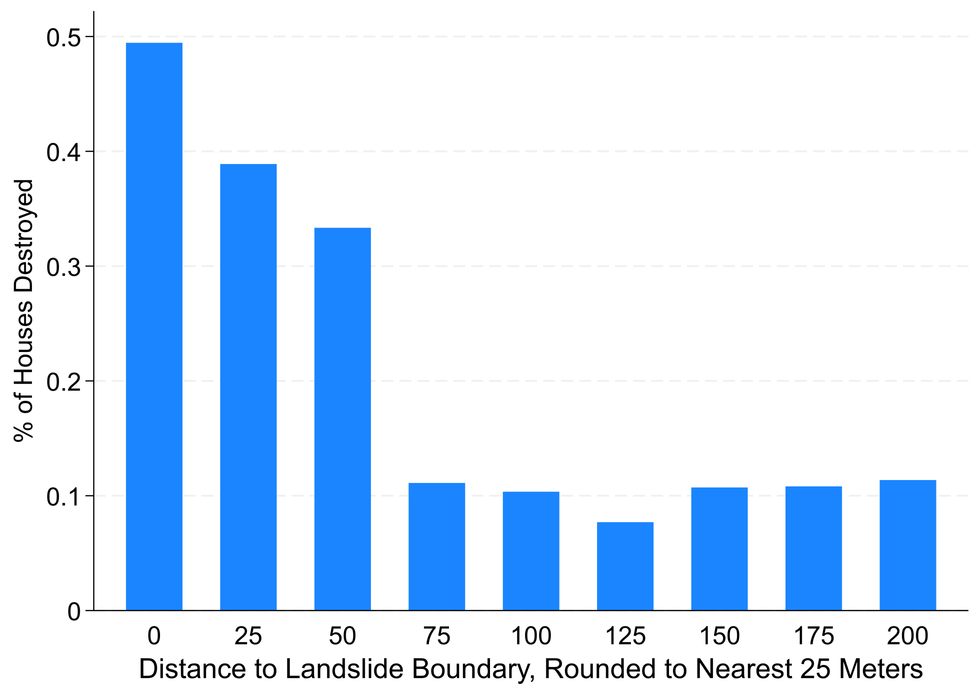
_ **and HwaJung Choi**, "Are Remittances Insurance? Evidence from Rainfall Shocks in the Philippines," *The World Bank Economic Review*, 2007, *21* (2), 219–248.

Young, Alwyn, "Inequality, The Urban-Rural Gap, and Migration," *Quarterly Journal of Economics*, 2013, *128* (4), 1727–1785.

Online Appendix for "Disastrous Displacement: The Long-Run Impacts of Landslides" by Travis Baseler and Jakob Hennig

A Additional Tables and Figures

Figure A1: Reported Home Destruction Rate, by Distance to Landslide Boundary



Notes: Houses located within the boundary coded as distance = 0. Distance rounded to nearest 25 meters.

Table A1: Tests of Selection Into Survey Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Dep Var: Surveyed = 1					
Age of Household Head (Years)	0.001					0.001
	(0.001)					(0.001)
	[0.24]					[0.32]
Household Size		0.004				0.003
		(0.004)				(0.004)
		[0.35]				[0.47]
Landslide = 1			-0.001			0.031
			(0.027)			(0.028)
			[0.98]			[0.27]
Currently Displaced = 1				-0.162***		-0.166***
				(0.038)		(0.039)
				[0.00]		[0.00]
Bushika Event = 1					-0.017	-0.024
					(0.028)	(0.029)
					[0.56]	[0.40]
Buwali Event = 1					0.012	0.010
					(0.025)	(0.026)
					[0.63]	[0.69]
Nametsi Event = 1					-0.047	-0.054*
					(0.030)	(0.030)
					[0.11]	[0.07]
Observations	663	672	675	675	675	663

Notes: An observation is a household located in Bududa prior to the landslides we study. Each column shows a regression of an indicator for whether the household was surveyed on one or more administrative variables. For unsurveyed households with missing pre-landslide GPS coordinates, we impute *Landslide* to be 1 if they are listed as displaced in administrative data, and 0 otherwise. *Currently Displaced* indicates that the household is listed as residing in a different village than it was before the landslide. Age and household size are missing from a small number of administrative records. Robust standard errors in parentheses; two-sided p-values in brackets. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A2: Impacts on Individual Welfare Measures

Panel A: Financial Health	(1) Enough Food	(2) Can Pay Expenses	(3) No Financial Emergency	(4) No Education Disruption	(5) Not Worried About Finances	(6) Robust to Financial Shock
Landslide	-0.101*	-0.106*	-0.078	-0.091	-0.121**	-0.127**
	(0.054)	(0.059)	(0.053)	(0.065)	(0.059)	(0.059)
	[0.06]	[0.07]	[0.14]	[0.16]	[0.04]	[0.03]
Observations	623	608	609	526	606	624
Dep Var Mean for Landslide = 0	0.76	0.58	0.76	0.57	0.49	0.46
Panel B: Mental Health	Usually Happy	Usually Not Nervous	Satisfied With Life	Optimistic About Life		
Landslide	-0.071	-0.141**	-0.201***	-0.014		
	(0.059)	(0.060)	(0.059)	(0.061)		
	[0.23]	[0.02]	[0.00]	[0.81]		
Observations	625	609	608	604		
Dep Var Mean for Landslide = 0	0.64	0.55	0.54	0.42		
Panel C: Amenities	Improved Toilet	Improved Drinking Water	Improved Cooking Fuel	Residence is Safe	Number of Close Friends	
Landslide	-0.049*	-0.041	0.014	-0.054	-0.157	
	(0.028)	(0.039)	(0.010)	(0.050)	(0.517)	
	[0.07]	[0.30]	[0.16]	[0.28]	[0.76]	
Observations	625	625	625	625	625	
Dep Var Mean for Landslide = 0	0.06	0.11	0.00	0.81	4.89	
Panel D: Income	Total Income, Past Month	Non-Farm Income, Past Year	Savings, Past Month	Food Spending, Past Week	% Children in School	
Landslide	-4.82	-66.85***	-5.73	0.34	0.02	
	(12.88)	(24.12)	(3.84)	(0.83)	(0.05)	
	[0.71]	[0.01]	[0.14]	[0.68]	[0.65]	
Observations	625	625	569	625	454	
Dep Var Mean for Landslide = 0	49.68	131.49	14.30	5.85	0.75	

Notes: An observation is a household (based on pre-landslide structure). Each panel shows impacts on components of the welfare indices used in Table 2 Panel C. All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Currency units are USD per month. Robust standard errors in parentheses. Two-sided p-values in brackets. See Section 3.3 for variable definitions. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A3: Indirect effects of landslides on nearby households appear too small to explain our main results.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Indirect Effects Within 1 Km of Site	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.171*	-0.035	-0.343**	-0.461***	-0.609***	0.052	-0.536***
	(0.090)	(0.084)	(0.172)	(0.167)	(0.195)	(0.183)	(0.170)
	[0.06]	[0.68]	[0.05]	[0.01]	[0.00]	[0.78]	[0.00]
Indirectly Affected: Landslide Within 1 Km	0.036	-0.161**	0.061	-0.120	-0.387**	0.186	-0.101
	(0.075)	(0.068)	(0.139)	(0.143)	(0.165)	(0.142)	(0.137)
	[0.63]	[0.02]	[0.66]	[0.40]	[0.02]	[0.19]	[0.46]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.54	0.51	0.00	0.00	0.00	0.00	0.00
Panel B: Indirect Effects Within Village	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.135**	0.083	-0.335**	-0.426***	-0.273*	-0.124	-0.460***
	(0.068)	(0.067)	(0.137)	(0.128)	(0.140)	(0.134)	(0.129)
	[0.05]	[0.21]	[0.01]	[0.00]	[0.05]	[0.36]	[0.00]
Indirectly Affected: Landslide Within Village	0.119**	-0.038	0.108	-0.121	0.004	-0.028	-0.022
	(0.052)	(0.051)	(0.106)	(0.105)	(0.096)	(0.097)	(0.101)
	[0.02]	[0.45]	[0.31]	[0.25]	[0.96]	[0.77]	[0.83]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.54	0.51	0.00	0.00	0.00	0.00	0.00

Notes: All regressions include a landslide-event fixed effect, geologic controls at the pre-landslide location, and pre-landslide demographic controls. *Landslide Within 1 Km* = 1 if the household was located within 1 kilometer of any landslide site, but outside the direct path. *Landslide Within Village* = 1 if any household in the same village was directly affected by a landslide, and the household was not in a direct path. Robust standard errors in parentheses. Two-sided p-values in brackets. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A4: Indirect effects of landslides on nearby households appear too small to explain our main results (broader sample including Bududa, Manafwa, and Sironko districts).

Panel A: Indirect Effects Within 1 Km of Site	(1) Satisfied With Life	(2) Worried About Finances	(3) Financial Health Index	(4) Mental Health Index	(5) Home Amenity Index	(6) Income Index	(7) Overall Welfare Index
Landslide	-0.119	-0.021	-0.328**	-0.323**	-0.392**	-0.031	-0.438***
	(0.074)	(0.072)	(0.149)	(0.140)	(0.181)	(0.143)	(0.143)
	[0.11]	[0.77]	[0.03]	[0.02]	[0.03]	[0.83]	[0.00]
Indirectly Affected: Landslide Within 1 Km	-0.003	-0.061	-0.031	-0.119	-0.436***	0.084	-0.204*
	(0.062)	(0.060)	(0.120)	(0.118)	(0.147)	(0.116)	(0.116)
	[0.96]	[0.31]	[0.80]	[0.31]	[0.00]	[0.47]	[0.08]
Observations	891	888	912	912	912	912	912
Dep Var Mean for Landslide = 0	0.54	0.50	-0.01	-0.03	0.12	0.01	0.04
Panel B: Indirect Effects Within Village	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.077	0.006	-0.224**	-0.245**	-0.019	-0.126	-0.257**
	(0.053)	(0.053)	(0.109)	(0.100)	(0.129)	(0.099)	(0.103)
	[0.15]	[0.91]	[0.04]	[0.01]	[0.88]	[0.20]	[0.01]
Indirectly Affected: Landslide Within Village	0.072*	-0.052	0.139*	-0.057	-0.052	-0.033	-0.011
	(0.038)	(0.038)	(0.075)	(0.075)	(0.085)	(0.072)	(0.074)
	[0.06]	[0.17]	[0.07]	[0.45]	[0.54]	[0.64]	[0.88]
Observations	891	888	912	912	912	912	912
Dep Var Mean for Landslide = 0	0.54	0.50	-0.01	-0.03	0.12	0.01	0.04

Notes: All regressions include a landslide-event fixed effect and pre-landslide demographic controls plus elevation. *Landslide Within 1 Km* = 1 if the household was located within 1 kilometer of any landslide site. *Landslide Within Village* = 1 if any household in the same village was directly affected by a landslide. Robust standard errors in parentheses. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A5: Impact estimates are robust to defining affected households using exact landslide path (without a 50 meter buffer).

Panel A: Household Destruction and Displacement	(1) House Damaged	(2) Casualty	(3) Land Damaged	(4) Any Other Damage	(5) Spending on Repairs	(6) Household Displaced	(7) Remains Displaced
Landslide (No Buffer)	0.328***	0.236***	0.240***	0.240***	232**	0.520***	0.336***
	(0.079)	(0.079)	(0.075)	(0.046)	(103)	(0.068)	(0.074)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.02]	[0.00]	[0.00]
Observations	625	625	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.15	0.08	0.53	0.74	203	0.22	0.04
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide (No Buffer)	0.066	0.060	-0.009	0.016	0.006	-0.022	-0.052
	(0.058)	(0.052)	(0.038)	(0.056)	(0.043)	(0.023)	(0.058)
	[0.26]	[0.25]	[0.82]	[0.77]	[0.90]	[0.34]	[0.37]
Observations	1,814	1,814	1,814	1,814	1,814	1,814	1,814
Dep Var Mean for Landslide = 0	0.32	0.23	0.13	0.23	0.14	0.07	0.65
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide (No Buffer)	-0.259***	0.023	-0.253	-0.339**	-0.206	-0.169	-0.389**
	(0.084)	(0.086)	(0.174)	(0.163)	(0.183)	(0.177)	(0.163)
	[0.00]	[0.79]	[0.15]	[0.04]	[0.26]	[0.34]	[0.02]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.53	0.52	-0.03	-0.02	-0.02	0.00	-0.03

Notes: Landslide (No Buffer) = 1 if the household was located within an exact landslide path at the time of the landslide. All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A6: Impact estimates are robust to defining affected households using self-reported damages.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Household Destruction and Displacement	House Damaged	Casualty	Land Damaged	Any Other Damage	Spending on Repairs	Household Displaced	Remains Displaced
House Damaged	-	0.210***	0.169***	0.217***	555***	0.384***	0.151***
	-	(0.043)	(0.051)	(0.033)	(76)	(0.049)	(0.038)
	-	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	-	625	625	625	625	625	625
Dep Var Mean for Landslide = 0	-	0.06	0.52	0.72	123	0.19	0.04
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
House Damaged	0.078**	0.046	-0.030	0.051	0.012	-0.041**	-0.018
	(0.038)	(0.034)	(0.022)	(0.038)	(0.031)	(0.014)	(0.035)
	[0.04]	[0.18]	[0.17]	[0.18]	[0.70]	[0.00]	[0.60]
Observations	1,814	1,814	1,814	1,814	1,814	1,814	1,814
Dep Var Mean for Landslide = 0	0.31	0.23	0.13	0.22	0.14	0.07	0.65
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
House Damaged	-0.265***	0.160***	-0.417***	-0.447***	-0.022	-0.072	-0.364***
	(0.052)	(0.053)	(0.103)	(0.104)	(0.115)	(0.105)	(0.100)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.85]	[0.49]	[0.00]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.56	0.49	0.02	0.03	-0.03	0.01	0.01

Notes: House Damaged = 1 if the the respondent reported that their home was damaged from a landslide. All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A7: Impact estimates are robust to controlling for pre-landslide household characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Household Destruction and Displacement	House Damaged	Casualty	Land Damaged	Any Other Damage	Spending on Repairs	Household Displaced	Remains Displaced
Landslide	0.332***	0.209***	0.159***	0.214***	294***	0.476***	0.252***
	(0.057)	(0.048)	(0.056)	(0.039)	(70)	(0.055)	(0.045)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	625	625	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.12	0.07	0.52	0.72	177	0.18	0.03
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.071*	0.080**	-0.006	0.046	0.053	-0.008	-0.065*
	(0.040)	(0.037)	(0.030)	(0.038)	(0.032)	(0.023)	(0.037)
	[0.08]	[0.03]	[0.85]	[0.23]	[0.10]	[0.72]	[0.07]
Observations	1,814	1,814	1,814	1,814	1,814	1,814	1,814
Dep Var Mean for Landslide = 0	0.31	0.22	0.12	0.21	0.13	0.06	0.66
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.202***	0.105*	-0.395***	-0.358***	-0.276**	-0.108	-0.448***
	(0.062)	(0.062)	(0.123)	(0.116)	(0.127)	(0.124)	(0.118)
	[0.00]	[0.09]	[0.00]	[0.00]	[0.03]	[0.39]	[0.00]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.54	0.51	0.00	0.00	0.00	0.00	0.00

Notes: All regressions include a landslide-event fixed effect, geologic controls at the pre-landslide location, and pre-landslide demographic controls. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A8: Impact estimates are robust to excluding geographic controls.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Household Destruction and Displacement	House Damaged	Casualty	Land Damaged	Any Other Damage	Spending on Repairs	Household Displaced	Remains Displaced
Landslide	0.325***	0.215***	0.171***	0.220***	275***	0.500***	0.236***
	(0.054)	(0.048)	(0.054)	(0.033)	(69)	(0.051)	(0.044)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	625	625	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.13	0.07	0.52	0.72	177	0.18	0.03
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.105***	0.109***	0.013	0.080**	0.084**	0.005	-0.064
	(0.040)	(0.037)	(0.028)	(0.039)	(0.035)	(0.021)	(0.041)
	[0.01]	[0.00]	[0.64]	[0.04]	[0.02]	[0.81]	[0.12]
Observations	1,814	1,814	1,814	1,814	1,814	1,814	1,814
Dep Var Mean for Landslide = 0	0.31	0.22	0.12	0.21	0.13	0.06	0.66
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.171***	0.106*	-0.329***	-0.291***	-0.190	-0.038	-0.329***
	(0.057)	(0.057)	(0.121)	(0.109)	(0.121)	(0.122)	(0.114)
	[0.00]	[0.07]	[0.01]	[0.01]	[0.12]	[0.75]	[0.00]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.54	0.51	0.00	0.00	0.00	0.00	0.00

Notes: All regressions include a landslide-event fixed effect. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A9: Impact estimates are robust to restricting to a sample with common support of pre-landslide elevation, farm size, and distance from nearest unstable point.

Panel A: Household Destruction and Displacement	(1) House Damaged	(2) Casualty	(3) Land Damaged	(4) Any Other Damage	(5) Spending on Repairs	(6) Household Displaced	(7) Remains Displaced
Landslide	0.340***	0.213***	0.153**	0.263***	288***	0.492***	0.237***
	(0.060)	(0.050)	(0.061)	(0.046)	(80)	(0.060)	(0.045)
	[0.00]	[0.00]	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	453	453	453	453	453	453	453
Dep Var Mean for Landslide = 0	0.11	0.06	0.51	0.69	154	0.18	0.03
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.119***	0.119***	0.006	0.092**	0.095**	0.006	-0.033
	(0.043)	(0.041)	(0.031)	(0.043)	(0.038)	(0.025)	(0.046)
	[0.01]	[0.00]	[0.85]	[0.03]	[0.01]	[0.83]	[0.48]
Observations	1,292	1,292	1,292	1,292	1,292	1,292	1,292
Dep Var Mean for Landslide = 0	0.31	0.21	0.13	0.22	0.12	0.06	0.64
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.218***	0.078	-0.318**	-0.380***	-0.121	0.006	-0.313**
	(0.064)	(0.064)	(0.127)	(0.125)	(0.132)	(0.143)	(0.132)
	[0.00]	[0.22]	[0.01]	[0.00]	[0.36]	[0.97]	[0.02]
Observations	440	438	453	453	453	453	453
Dep Var Mean for Landslide = 0	0.54	0.49	0.00	-0.02	-0.04	-0.02	-0.04

Notes: Unaffected household sample restricted to households with common support across affected and unaffected groups for pre-landslide elevation, distance to nearest unstable point, and farm size. All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A10: Impact estimates are similar in a broader sample including Bududa, Manafwa, and Sironko districts.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Household Destruction and Displacement	House Damaged	Casualty	Land Damaged	Any Other Damage	Spending on Repairs	Household Displaced	Remains Displaced
Landslide	0.369***	0.169***	0.169***	0.199***	138**	0.461***	0.322***
	(0.044)	(0.036)	(0.044)	(0.029)	(54)	(0.041)	(0.039)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.00]	[0.00]
Observations	912	912	912	912	912	912	912
Dep Var Mean for Landslide = 0	0.17	0.06	0.53	0.73	217	0.22	0.05
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.032	0.031	-0.025	0.036	0.032	-0.013	-0.043
	(0.032)	(0.028)	(0.022)	(0.030)	(0.024)	(0.017)	(0.028)
	[0.33]	[0.27]	[0.25]	[0.24]	[0.18]	[0.42]	[0.13]
Observations	2,683	2,683	2,683	2,683	2,683	2,683	2,683
Dep Var Mean for Landslide = 0	0.33	0.23	0.12	0.23	0.14	0.07	0.67
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.116**	0.035	-0.299***	-0.214**	0.009	-0.108	-0.251***
	(0.048)	(0.049)	(0.101)	(0.092)	(0.122)	(0.091)	(0.095)
	[0.02]	[0.48]	[0.00]	[0.02]	[0.94]	[0.23]	[0.01]
Observations	891	888	912	912	912	912	912
Dep Var Mean for Landslide = 0	0.54	0.50	-0.01	-0.03	0.12	0.01	0.04

Notes: Sample includes two additional landslide events in Manafwa and Sironko districts, where geologic data are not available. All regressions include a landslide-event fixed effect and pre-landslide demographic and elevation controls. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A11: Selected Pre-Landslide Predictor Variables in Lasso Regressions, by Mechanism

	(1) Casualty	(2) House Damaged	(3) Land Damaged	(4) Displaced	(5) Displaced with Network	(6) Displaced by Government	(7) Remains Displaced	(8) Remains in Gov't Displacement	(9) # Urban Migrants
Pre-Landslide Geography × Landslide									
Slope (Degrees)	8.82	5.92	23.04	3.96					
Elevation (Km)	-0.09			0.31					
Critical Rainfall Value (M/Day)		0.71							
Unstable Upslope Area (1000s of M^2)				0.04					
Stable Upslope Area (% of Total)	0.15								
Distance to Nearest Unstable Point (Km)	0.03								
Pre-Landslide Characteristics × Landslide									
Age of Household Head									0.003
Household Size	0.3	0.46		0.21					
# Adult Equivalents									1.31
# Members Aged 0–5			0.57						
# Members Aged 18–50									3.76
Large Farm (1 Acre or Larger)			0.22						
Income			0.1	0.02					
Income per Adult Equivalent				0.16					
Savings		0.15	0.07						
Observations	625	625	625	625	625	625	625	625	625

Notes: An observation is a household located in Bududa prior to the landslides we study. Each column shows post-lasso OLS coefficients selected from a lasso regression of a mechanism of interest on all pre-landslide controls shown in Table 1, the four landslide damage variables shown in Table 2, and their interactions with *Landslide* (columns 1–4 exclude damage controls). All regressions partial out a landslide-event fixed effect; columns 5–8 additionally partial out *Displaced* × *Landslide*. Only variables with a non-zero coefficient in at least one regression are shown. Age units are 10s of years. Household member counts are in 10s of persons. Income units are 100s of USD.

Table A12: Selected Pre-Landslide Predictor Variables in Lasso Regressions (Displaced Sample)

	(1) Remains Displaced	(2) Found Non-Farm Income in Destination	(3) Share of Village Hit by Landslide
Pre-Landslide Characteristics × Landslide			
Age of Household Head	0.02	0.03	
Average Age of Household Members	0.04		
Household Size		0.03	
# Members Aged 0–5	1.26	0.8	
Damage to Land = 1	0.31	0.09	
Village-Level Mean Landslide Damages × Landslide			
Mean (Casualty = 1)			1.01
Mean (Damage to Land = 1)			0.41
Mean (Damage to Home = 1)			0.09
Mean (Other Damage = 1)			0.12
Observations	274	274	274

Notes: An observation is a household located in Bududa, Sironko, or Manafwa prior to the landslides we study. Each column shows post-lasso OLS coefficients selected from a lasso regression of a mechanism of interest on all pre-landslide controls shown in Table 1 (except geographic variables which are not available in Sironko or Manafwa) plus the four landslide damage variables shown in Table 2—or their village-level means, in column 3—and their interactions with *Landslide*. All regressions partial out a landslide-event fixed effect. Only variables with a non-zero coefficient in at least one regression are shown. Age units are 10s of years. Household member counts are in 10s of persons. Income units are 100s of USD.

Table A13: The Role of Casualties, Displacement, and Migration in Welfare Impacts (Difference-in-Differences Regressions)

Dep Var: Overall Welfare Index	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Casualty × Landslide	-0.083							-0.396
	(0.273)							(0.254)
	[0.76]							[0.12]
Damage to Home × Landslide	0.086							0.228
	(0.260)							(0.242)
	[0.74]							[0.35]
Damage to Land × Landslide	-0.203							-0.396*
	(0.236)							(0.223)
	[0.39]							[0.07]
Displaced × Landslide		-0.391*	-0.746**	-0.465**	-0.601***	-0.671***		-0.845***
		(0.215)	(0.291)	(0.235)	(0.230)	(0.245)		(0.300)
		[0.07]	[0.01]	[0.05]	[0.01]	[0.01]		[0.00]
Displaced with Network × Landslide			0.580**					0.506*
			(0.272)					(0.292)
			[0.03]					[0.08]
Displaced by Gov't × Landslide				0.302		0.415		0.044
				(0.300)		(0.450)		(0.385)
				[0.31]		[0.36]		[0.91]
Remains Displaced × Landslide					0.319	0.434		0.439
					(0.308)	(0.367)		(0.346)
					[0.30]	[0.24]		[0.20]
Remains in Gov't Displacement × Landslide						-0.598		-0.406
						(0.651)		(0.600)
						[0.36]		[0.50]
# Urban Migrants × Landslide							0.178*	0.151
							(0.105)	(0.094)
							[0.09]	[0.11]
Observations	625	625	625	625	625	625	625	625
Demographic × Landslide Controls	X	X	X	X	X	X	X	X
Damage × Landslide Controls		X	X	X	X	X	X	X

Notes: See notes under Table 4 for details on sample, variables, and regression specifications. Each column shows a difference-in-differences regression controlling for each independent that is variable interacted with *Landslide*. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A14: Displacement with other in-network households, or when coordinated by the government, is associated with better outcomes (Bududa sample).

	(1) Remains Displaced	(2) Remains Displaced	(3) Remains Displaced	(4) Found Non-Farm Income in Destination	(5) Found Non-Farm Income in Destination	(6) Found Non-Farm Income in Destination
Panel A: Household-Level Variation						
Displaced with Network × Landslide	0.162		0.136	0.287*		0.224
	(0.115)		(0.117)	(0.163)		(0.174)
	[0.16]		[0.25]	[0.08]		[0.20]
Displaced by Gov't × Landslide		0.268**	0.250*		0.269	0.193
		(0.133)	(0.130)		(0.221)	(0.234)
		[0.05]	[0.06]		[0.23]	[0.41]
Observations	166	166	166	73	73	73
Panel B: Village-Level Variation						
Share of Village Hit by Landslide	0.925***			0.800*		
	(0.219)			(0.377)		
	[0.00]			[0.05]		
Observations	166			73		

Notes: Sample includes all households displaced by a landslide in Bududa, regardless of current location. Each column within a panel is a regression. *Remains Displaced* = 1 if the household has not returned to the origin. *Found Non-Farm Income in Destination* = 1 if the household reported a primary income source while displaced other than farming or agricultural labor, based on a question added to the household survey partway through field work. *Displaced with Network* = 1 if the household was displaced with connections in the origin village, and *Displaced by Gov't* = 1 if displacement was required by the government. *Share of Village Hit by Landslide* is the mean of *Landslide* within the household's pre-landslide village. All regressions include a control for *Landslide* (not shown), a landslide-event fixed effect, and geographic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panel A and clustered at the pre-landslide village level in Panel B; two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A15: Lee Bounds on Household-Level Landslide Impact Estimates

Panel A: Household Destruction and Displacement	(1) House Damaged	(2) Casualty	(3) Land Damaged	(4) Any Other Damage	(5) Spending on Repairs	(6) Household Displaced	(7) Remains Displaced
Landslide (Lower Bound)	0.279***	0.176***	0.153***	0.205***	272***	0.416***	0.204***
	(0.052)	(0.047)	(0.053)	(0.037)	(67)	(0.049)	(0.045)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Landslide (Upper Bound)	0.286***	0.184***	0.160***	0.213***	290***	0.424***	0.212***
	(0.058)	(0.053)	(0.053)	(0.033)	(100)	(0.054)	(0.051)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	675	675	675	675	675	675	675

Panel B: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide (Lower Bound)	-0.195***	0.082	-0.361***	-0.295**	-0.219	-0.060	-0.346***
	(0.061)	(0.060)	(0.128)	(0.120)	(0.148)	(0.171)	(0.126)
	[0.00]	[0.17]	[0.00]	[0.01]	[0.14]	[0.73]	[0.01]
Landslide (Upper Bound)	-0.149**	0.122**	-0.339***	-0.277**	-0.166	-0.003	-0.323**
	(0.060)	(0.059)	(0.128)	(0.118)	(0.132)	(0.126)	(0.126)
	[0.01]	[0.04]	[0.01]	[0.02]	[0.21]	[0.98]	[0.01]
Observations	675	675	675	675	675	675	675

Notes: An observation is a household (based on pre-landslide structure). Each column shows upper and lower Lee bounds (Lee, 2009). A landslide-event fixed effect and geologic controls are partialled out prior to estimation. *Landslide* is imputed for unsurveyed households: unsurveyed households listed as displaced in administrative data are coded as affected by the landslide, while unsurveyed households coded as living in the pre-landslide village are listed as not affected. Standard errors in parentheses; two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A16: Impact estimates are robust to reweighting observations to account for predicted non-response.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Household Destruction and Displacement	House Damaged	Casualty	Land Damaged	Any Other Damage	Spending on Repairs	Household Displaced	Remains Displaced
Landslide	0.345***	0.203***	0.178***	0.242***	336***	0.503***	0.248***
	(0.057)	(0.050)	(0.057)	(0.038)	(74)	(0.054)	(0.046)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	613	613	613	613	613	613	613
Dep Var Mean for Landslide = 0	0.12	0.07	0.52	0.72	176	0.17	0.03
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.112***	0.111***	0.007	0.089**	0.087**	0.002	-0.074*
	(0.040)	(0.038)	(0.029)	(0.040)	(0.036)	(0.022)	(0.041)
	[0.01]	[0.00]	[0.82]	[0.02]	[0.02]	[0.92]	[0.07]
Observations	1,814	1,814	1,814	1,814	1,814	1,814	1,814
Dep Var Mean for Landslide = 0	0.31	0.22	0.12	0.21	0.13	0.06	0.66
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.199***	0.124**	-0.388***	-0.304***	-0.214*	-0.050	-0.368***
	(0.059)	(0.059)	(0.119)	(0.112)	(0.128)	(0.124)	(0.118)
	[0.00]	[0.04]	[0.00]	[0.01]	[0.10]	[0.69]	[0.00]
Observations	608	606	613	613	613	613	613
Dep Var Mean for Landslide = 0	0.54	0.51	0.01	0.00	-0.01	0.00	0.00

Notes: Observations are weighted by the inverse of the probability of being surveyed, estimated on administrative household data through logit lasso regression. All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

B Systematic Review of Natural Disaster Literature

In this section, we describe our review of the natural disasters literature.

To better understand this study's divergent findings, we systematically reviewed the economic literature on natural disasters. We searched 16 top economics journals for any paper with "disaster" (or related words, using a built-in feature) in the abstract using the tool *EconLit*.[36] This preliminary search identified 120 papers. For each paper, we assessed whether it fulfilled the following criteria: 1) is set in a low- or middle-income country, 2) measures impacts on individual or household economic outcomes such as income, consumption, assets, or human capital, 3) is about a natural disaster as opposed to human disaster such as famine or depression, and 4) is not purely theoretical, or restricted to macro-economic outcomes such as regional output.[37] We also evaluated 47 additional disaster-related papers that were not identified by the search tool but which we were otherwise aware of. However, none of these papers satisfied the four inclusion criteria listed above.

Our search identified 4 papers studying individual economic impacts of a natural disaster in a developing country.[38] Gignoux and Menéndez (2016) show that household welfare improved in the long run following earthquakes in Indonesia, partly as a result of aid reconstruction efforts. In contrast, Caruso and Miller (2015) and Caruso (2017) show negative effects of diverse disasters on the human capital of children, identifying victims by their birth place. Deuchert and Felfe (2015) show that typhoon home damages within an area are associated with worse future educational outcomes for children.

[36]We searched the following journals: American Economic Review, Econometrica, Quarterly Journal of Economics, Journal of Political Economy, Review of Economic Studies, American Economic Journal: Applied Economics, American Economic Journal: Economic Policy, American Economic Review: Insights, Journal of Development Economics, Journal of Labor Economics, Journal of Public Economics, Review of Economics and Statistics, Economic Journal, Journal of the European Economic Association, Journal of Urban Economics, and Journal of Human Resources. We included all years available in the search tool.

[37]Criteria 1, 2, and 3 jointly exclude the vast majority of papers. Many papers turned up in this search used natural disasters as a shock to study asset pricing or business cycles, but did not measure impacts on affected households. The majority of this literature also focuses on high-income countries.

[38]Two additional papers evaluated impacts on firms using microdata (De Mel et al., 2012, Pelli et al., 2023).

C Data Collection

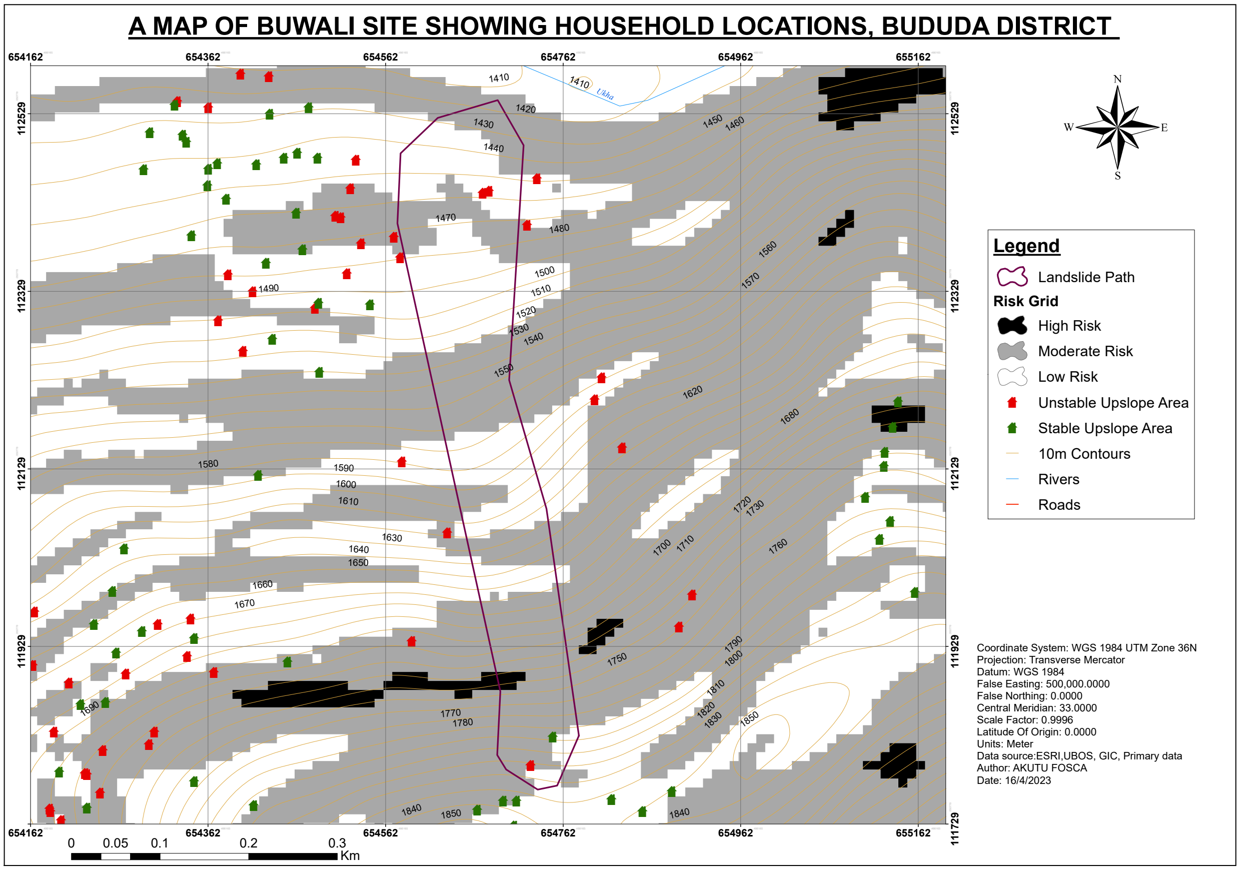
To identify sites for our study, we worked together with local leaders with insight into recent landslide events. They advised us on the sites of the largest landslides in the last 10 years, and shared lists of households that resided in villages in or near these sites at the time of the event. These lists form our study sample.

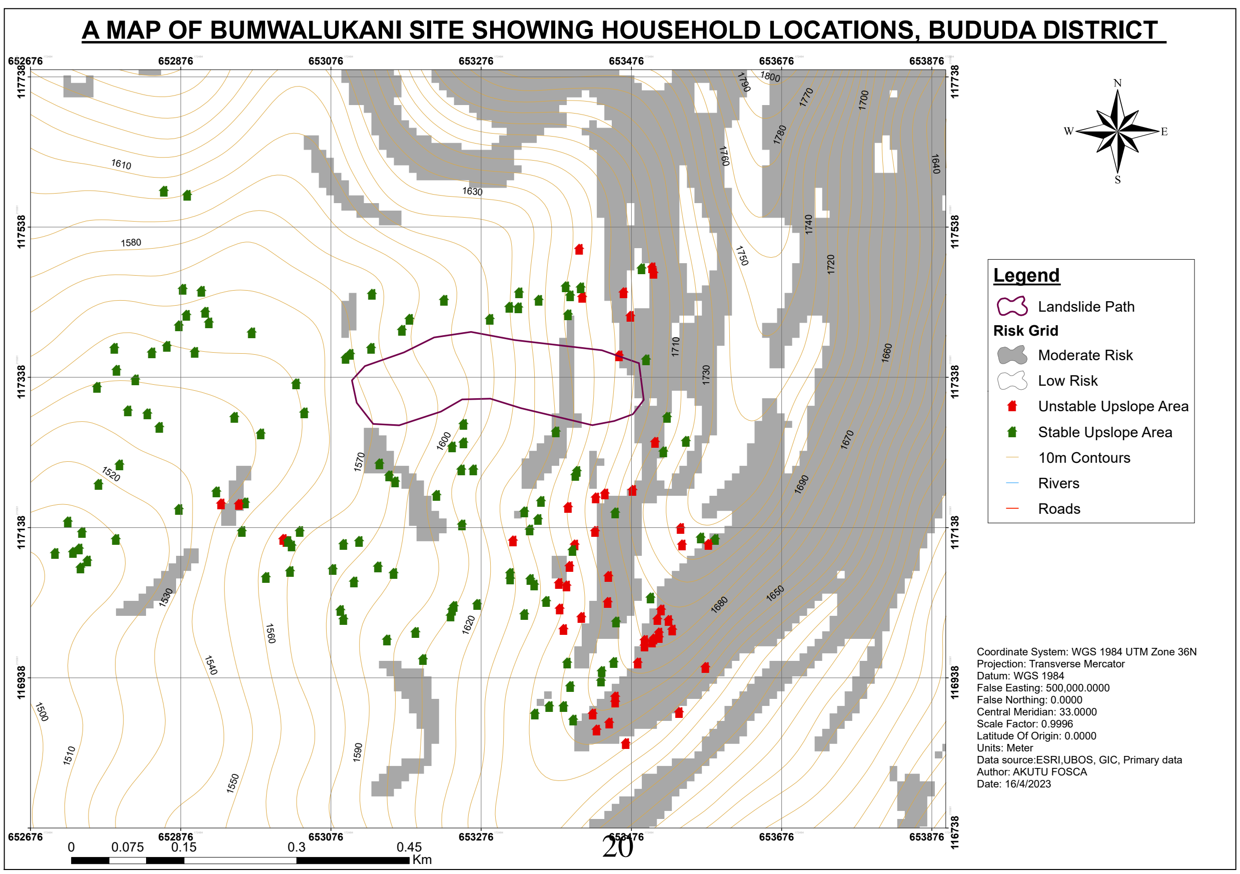
For each of these landslide sites, we established the extent of the survey perimeter by identifying directly hit villages and neighboring villages that could serve as control areas. We largely limited the scope of the survey to the villages on the slopes where the landslide occurred, to have an *ex-ante* homogenous population of affected and unaffected households. Figure C1 shows each of these sites with the villages identified for our survey (the Nametsi map is shown in Figure 2 in the main text). There is relatively little clustering of dwellings, as farmers in this region work the fields directly surrounding there homestead, rather than living closely together in a village surrounded by fields. This increases the risk that some households will be hit by a landslide compared to clustering of dwellings in stable locations.

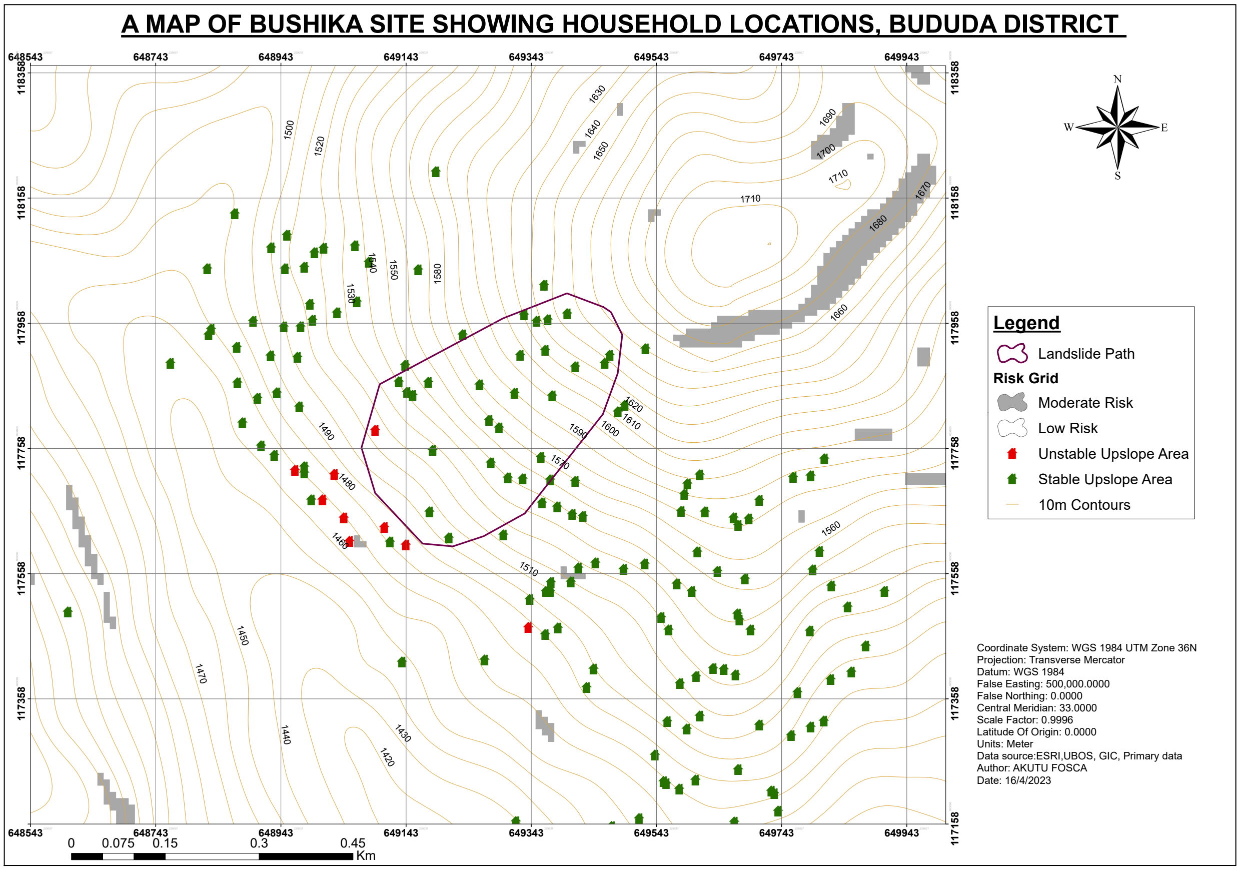
We then worked with our local contacts to collect information on the households living in these survey areas before the landslide events. For this purpose, they accessed past registers of households living in the villages, available at the offices of local village leaders. We could therefore attempt a survey of the full population which lived in these affected and neighboring villages before the landslide event.

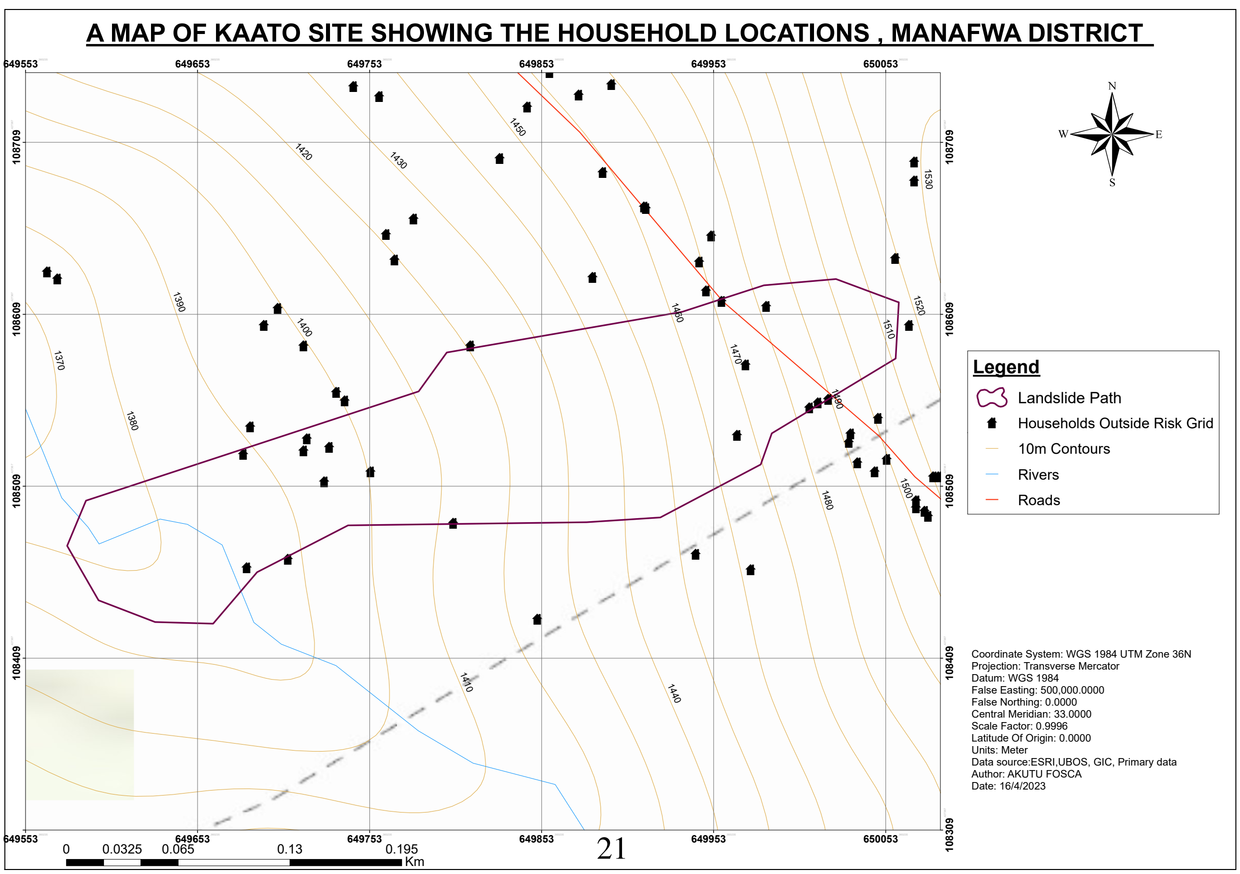
For households still living in their original dwelling, the dwelling coordinates were recorded by the survey team during interviews. For households who had moved, the coordinates at the pre-landslide location were recorded during subsequent field visits with assistance from local contacts. In cases where the original site was not safely accessible, staff were instructed to take a GPS reading as close to the original location as safely possible.

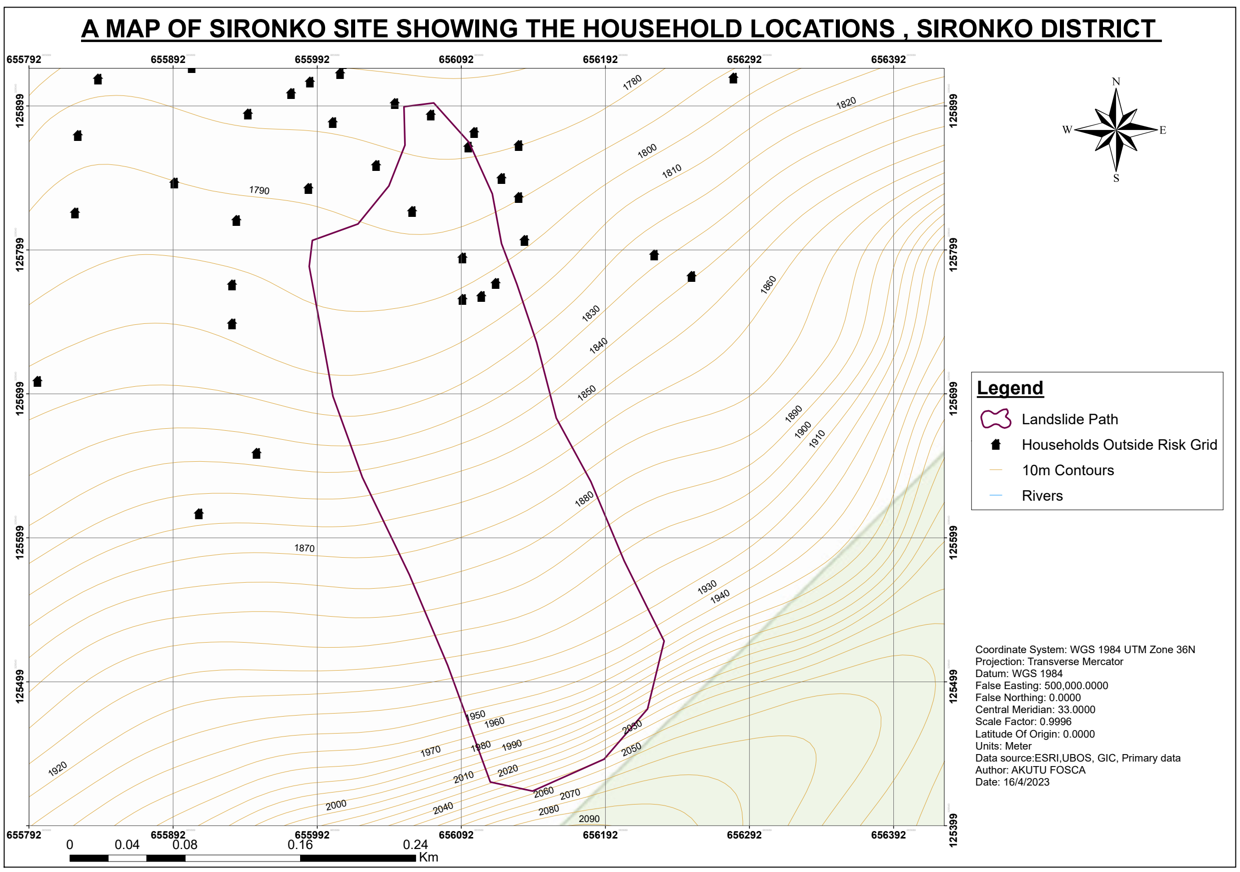
Figure C1: Additional Maps of Landslide Sites






A MAP OF BUSHIKA SITE SHOWING HOUSEHOLD LOCATIONS, BUDUDA DISTRICT
Legend
Landslide Path
Risk Grid
Moderate Risk
Low Risk
Unstable Upslope Area
Stable Upslope Area
10m Contours
Coordinate System: WGS 1984 UTM Zone 36N
Projection: Transverse Mercator
Datum: WGS 1984
False Easting: 500,000.0000
False Northing: 0.0000
Central Meridian: 33.0000
Scale Factor: 0.9996
Latitude Of Origin: 0.0000
Units: Meter
Data source:ESRI,UBOS, GIC, Primary data
Author: AKUTU FOSCA
Date: 16/4/2023
0 0.075 0.15 0.3 0.45 Km


A MAP OF KAATO SITE SHOWING THE HOUSEHOLD LOCATIONS , MANAFWA DISTRICT
Legend
Landslide Path
Households Outside Risk Grid
10m Contours
Rivers
Roads
Coordinate System: WGS 1984 UTM Zone 36N
Projection: Transverse Mercator
Datum: WGS 1984
False Easting: 500,000.0000
False Northing: 0.0000
Central Meridian: 33.0000
Scale Factor: 0.9996
Latitude Of Origin: 0.0000
Units: Meter
Data source:ESRI,UBOS, GIC, Primary data
Author: AKUTU FOSCA
Date: 16/4/2023
0 0.0325 0.065 0.13 0.195 Km



Notes: Each house icon is a household in our sample. Green indicates that more than 75% of that household's upslope area is classified as stable; red indicates that 25% or more of the upslope area is classified as unstable. Exact landslide path shown in purple polygon.

D Corrections for Spatial Correlation

By nature, the destruction caused by landslides is spatially clustered and so may give rise to spatial correlation in regression residuals. Table D1 presents Moran tests for spatial correlation in residuals for all of our household destruction, displacement, and welfare outcomes presented in Table 2. These Moran tests suggest the presence of modest spatial correlation in damages, but little to no spatial correlation in our welfare measures: out of 7 damage and displacement outcomes, we reject the null hypothesis of independent and identically distributed error terms at the 10% level for three, while out of 7 welfare measures, we reject the same null for only 1. Table D2 presents standard errors adjusted for three-dimensional spatial correlation using the method of Conley (1999), applying a cutoff of 1 kilometer. These adjusted standard errors are very similar to their unadjusted versions. Table D3 presents standard errors adjusted for three-dimension spatial correlation using the spatial correlation principal components method described in Müller and Watson (2022). Again, these adjusted standard errors are very similar to their unadjusted versions.

Table D1: Moran Tests for Spatial Correlation in Residuals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Household Destruction and Displacement	House Damaged	Casualty	Land Damaged	Any Other Damage	Spending on Repairs	Household Displaced	Remains Displaced
Moran *p*-Value	0.49	0.43	0.01	0.39	0	0.18	0.00
Observations	625	625	625	625	625	625	625
Panel B: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Moran *p*-Value	0.47	0.88	0.20	0.64	0.04	0.22	0.16
Observations	608	606	625	625	625	625	625

Notes: An observation is a household (based on pre-landslide structure). Moran *p*-values estimated from regression models shown in Table 2 using Stata command *estat moran* using an inverse-distance weighting matrix with 1-kilometer truncation.

Table D2: Conley-adjusted standard errors are similar to unadjusted versions.

Panel A: Household Destruction and Displacement	(1) House Damaged	(2) Casualty	(3) Land Damaged	(4) Any Other Damage	(5) Spending on Repairs	(6) Household Displaced	(7) Remains Displaced
Landslide	0.332***	0.214***	0.179***	0.240***	326***	0.496***	0.246***
	(0.057)	(0.049)	(0.056)	(0.037)	(72)	(0.053)	(0.045)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	625	625	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.12	0.07	0.52	0.72	177	0.18	0.03
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.113***	0.113***	0.008	0.090**	0.089**	0.003	-0.075*
	(0.041)	(0.038)	(0.029)	(0.040)	(0.036)	(0.022)	(0.042)
	[0.01]	[0.00]	[0.79]	[0.02]	[0.01]	[0.89]	[0.07]
Observations	1,814	1,814	1,814	1,814	1,814	1,814	1,814
Dep Var Mean for Landslide = 0	0.31	0.22	0.12	0.21	0.13	0.06	0.66
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.201***	0.121**	-0.402***	-0.349***	-0.222*	-0.026	-0.382***
	(0.059)	(0.058)	(0.120)	(0.111)	(0.123)	(0.124)	(0.117)
	[0.00]	[0.04]	[0.00]	[0.00]	[0.07]	[0.83]	[0.00]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.54	0.51	0.00	0.00	0.00	0.00	0.00

Notes: Standard errors and p-values adjusted for three-dimensional spatial correlation using the method of Conley (1999), estimated using the Stata package *x_ols2*, applying a cutoff of 0.01 degrees (approximately 1 kilometer). All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table D3: Standard errors adjusted using Müller and Watson (2022) are similar to unadjusted versions.

Panel A: Household Destruction and Displacement	(1) House Damaged	(2) Casualty	(3) Land Damaged	(4) Any Other Damage	(5) Spending on Repairs	(6) Household Displaced	(7) Remains Displaced
Landslide	0.332***	0.214***	0.179***	0.240***	326***	0.496***	0.246**
	(0.049)	(0.032)	(0.045)	(0.042)	(71)	(0.127)	(0.100)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.05]
Observations	625	625	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.12	0.07	0.52	0.72	177	0.18	0.03
Panel B: Individual Migration and Employment	Migrated Anywhere	Migrated to City	Migrated to Big City	Current Migrant	Remained in City	Remained in Big City	Economically Active
Landslide	0.113**	0.113**	0.008	0.090*	0.089**	0.003	-0.075
	(0.045)	(0.038)	(0.023)	(0.043)	(0.035)	(0.016)	(0.041)
	[0.04]	[0.02]	[0.76]	[0.08]	[0.04]	[0.85]	[0.12]
Observations	1,814	1,814	1,814	1,814	1,814	1,814	1,814
Dep Var Mean for Landslide = 0	0.31	0.22	0.12	0.21	0.13	0.06	0.66
Panel C: Household Welfare Measures	Satisfied With Life	Worried About Finances	Financial Health Index	Mental Health Index	Home Amenity Index	Income Index	Overall Welfare Index
Landslide	-0.201***	0.121	-0.402**	-0.349**	-0.222*	-0.026	-0.382***
	(0.049)	(0.068)	(0.117)	(0.137)	(0.094)	(0.078)	(0.079)
	[0.00]	[0.12]	[0.01]	[0.04]	[0.05]	[0.76]	[0.00]
Observations	608	606	625	625	625	625	625
Dep Var Mean for Landslide = 0	0.54	0.51	0.00	0.00	0.00	0.00	0.00

Notes: Standard errors and p-values adjusted for three-dimensional spatial correlation using the method of Müller and Watson (2022), estimated using the Stata package *scpc*. All regressions include a landslide-event fixed effect and geologic controls at the pre-landslide location. Standard errors in parentheses are heteroskedasticity-robust in Panels A and C and clustered at the household level in Panel B. Two-sided p-values in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.