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The impact of extreme weather events on education

Valeria Groppo^a and Kati Kraehnert^{b*}

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Abstract

This paper analyzes the short- and long-term impact of extreme weather events on educational outcomes in Mongolia. Our focus is on two extremely severe winters that caused mass livestock mortality. We use household panel data with comprehensive retrospective information on households' historic experience with weather shocks. Exposure to the weather shock significantly reduces the likelihood of being enrolled in mandatory school two to three years after the shock. Similarly, it significantly reduces the probability of completing basic education ten to eleven years after the shock. Both effects are driven by children from herding households. Results are robust to measuring shock intensity with district-level livestock mortality and climate data as well as household-level livestock losses. Exposure to weather shocks during preschool age (as opposed to exposure during primary and secondary school age) yields the worst consequences for educational attainment. Overall, the evidence points toward income effects as the channel through which the shock impacts education.

Key words: human capital accumulation, weather shocks, Mongolia

JEL: I25, Q54, O12

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1. Introduction

In this study, we analyze the impact of extreme weather events on educational outcomes in Mongolia. Our analysis brings together two current topics, both of which relate to poverty and well-being with a long term perspective. On the one hand, with global warming, extreme weather events, such as droughts, floods, storms, and heat waves are expected to become more frequent and their intensity is likely to become less predictable (Murray et al. 2012). This will be particularly damaging for households in developing countries (World Bank 2010): A large share of the population draws on natural resources for their livelihood, while governments often lack the financial and technical means to manage increasing climate risk. Moreover, in developing economies, incomplete or non-existent insurance markets limit the capacity of households to smooth consumption (e.g., Townsend 1994; Udry 1994; Zimmerman and Carter 2003). On the other hand, a recent body of empirical studies documents that shocks – regardless their cause or nature – experienced early in life can influence individuals’ health and education as adults (see Cunha and Heckman 2007 for a review). These microeconomic effects can translate into long-term macroeconomic losses in terms of human capital accumulation and economic growth (Horton and Steckel 2013). However, relatively little is known about the impact of extreme weather events on individuals’ human capital accumulation in developing countries. While relevant for policy, investigating the impact of extreme weather events is also interesting from a scientific perspective. Indeed, because of their severity and unpredictability, such events provide a natural experiment setting that allows for the identification of causal effects.

Mongolia is particularly suited to address this topic, as two catastrophic weather events have hit the country since the mid-1990s. Extreme weather events come in the form of exceptionally harsh winters (called *dzud* in Mongolian) that cause mass livestock mortality. Such shocks are triggered by a range of adverse climatic conditions, including extremely cold temperatures, excessive snowfall, lack of precipitation, and fluctuations in temperature. A triple dzud, three consecutive extremely severe winters occurring between 1999 and 2002, decimated 33 percent of livestock in Mongolia. A few years later, the 2009/10 winter was the most severe winter ever recorded, causing the death of 23 percent of livestock. These two events threatened the livelihood of a large share of the rural population that depends on herding for their sustenance.

Given the timing of the events, we are able to analyze both short- and long-term effects of extreme weather events on education. Our econometric strategy exploits exogenous variation in shock intensity across space (in the short-term analysis) as well as across space and cohorts (in the long-term analysis). The database for our study is a detailed household panel survey that we collected in Mongolia between 2012 and 2015.

Our study contributes to a growing body of research exploring the impact of adverse weather conditions on the acquisition of human capital.¹ In an early study, Jacoby and Skoufias (1997) show that the school attendance of children in rural India responds to seasonal rainfall conditions that cause fluctuations in household income. Similarly, Maccini and Yang (2009) find a strong positive relationship between local rainfall conditions during the year of birth and education outcomes for adult Indonesian women. On the contrary, Shah and Steinberg (forthcoming) find that positive rainfall conditions in rural India lead to worse educational attainments. Shah and Steinberg suggest that children substitute schooling with labor market participation in years when agricultural income is high. Jensen (2000) shows that adverse rainfall conditions occurring in 1986 in Côte d'Ivoire lowers the school enrollment of children living in affected areas. Another group of studies focuses more explicitly on extreme weather events. For instance, using data from Zimbabwe, Alderman, Hoddinott and Kinsey (2006) explore the impact of malnutrition resulting from a drought and civil war experienced during early childhood on long term educational attainment. The authors find that nutritional status as preschooler has a statistically significant and economically large effect on the age of school entry and the number of grades of schooling completed as adults. Rosales (2014) shows that Ecuadorian children exposed to the 1997-98 El Niño weather phenomenon during the first semester of pregnancy score significantly lower in cognitive tests five to seven years after the shock. Deuchert and Felfe (2015) find that exposure to a typhoon significantly lowers the grades of schooling completed and IQ test scores among children in the Philippines.

We contribute to the literature in two areas. First, the data at hand allow us to minimize the bias caused by confounding factors and estimate the effect of the shock with high precision. Our estimates include a large set of controls for children's socio-economic environment and, most importantly, for pre-shock household wealth. In contrast, most existing studies on the topic can only rely on household controls measured at the time of the survey, which may be

¹ Related fields of research focus on the impact of weather shocks on household income, anthropometric outcomes, health, and child labor (see Baez, de la Fuente and Santos 2010 for a comprehensive review).

endogenous to the shock and thus likely bias the estimated coefficient of interest.² Moreover, unlike most existing studies, we base our estimates on individuals' location of residence just prior to the shock, which allows us to avoid measurement error from post-shock migration.³ Lastly, we use both aggregate- and household-level measures of shock intensity, which provides us insights on the channels through which the shock affects education.

Second, we explore at what age shock exposure has the greatest adverse impact on education. The importance of environmental conditions during pregnancy for later child development is outlined in the “fetal origins hypothesis” (Hales and Barker 1992), which maintains that the fetus adapts to unfavorable conditions in utero with consequences that manifest throughout life into adulthood, including permanent changes in physical structure, physiology and metabolism.⁴ The more recent “developmental origins” approach underlines the potential role of environmental conditions experienced over the life course (Conti 2013; Heckman 2012). The existing literature on the long-term effects of weather shocks on education mainly focuses on shocks experienced in utero or during infancy up to age three (see, for instance, Alderman et al. 2006; Maccini and Yang 2009). A small number of studies also look at weather shocks experienced later in life, such as at school entry age (Deuchert and Felfe 2015) and during adolescence (Shah and Steinberg forthcoming). In a similar vein, we assess the relative impact of shock exposure during preschool age, primary school age, and secondary school age.

We find that exposure to the extreme winter of 2009/10 significantly reduces the likelihood to be enrolled in mandatory schooling two to three years after the shock. This effect only holds for children from herding households. Results are robust to measuring shock intensity with district-level livestock mortality data, snow depth data, and temperature data. Moreover, they hold using a specification with household fixed effects and a household-level shock measure. We find the detrimental effect of the weather shock to persist over time, with estimates of similar magnitude obtained from each of the three panel waves. There is no evidence for a differential impact of the shock by the child's gender or by pre-shock household wealth. However, maternal education can mitigate the consequences of the shock. Overall, the

² One exception is the study by Deuchert and Felfe (2015), which also uses detailed pre-shock controls.

³ To the best of our knowledge, there are only two studies that also use pre-shock information on location (Akresh, Lucchetti and Thirumurthy 2012; Deuchert and Felfe 2015).

⁴ See Currie (2009) and Almond and Currie (2011) for a review of the empirical economic literature on the impact of early-life conditions on adult health.

evidence points toward a reduction in income as the channel through which the shock impacts education. Similarly, we find a negative impact of the 1999-2002 triple dzud on the probability to complete basic education ten to eleven years after the shock. This long-term effect is also driven by children from herding households. Results from the short- and long-term analyses consistently show that children exposed to extreme weather events as preschoolers are more severely affected than children exposed while already of mandatory school age.

The study proceeds as follows. Section 2 presents an overview of the educational system in Mongolia. Section 3 outlines the weather shocks that occurred in Mongolia and hypothesizes through what channels these shocks may affect education. Section 4 describes the household survey data and shock measures. The identification strategy is outlined in Section 5, which is followed by a description of the results in Section 6. Section 7 concludes.

2. The education system in Mongolia

Mongolia had a well-functioning educational system through the late 1980s, a legacy of the former centrally planned economic system. At that time, the adult literacy rate was about 95 percent and enrollment in primary schools was almost universal (Steiner-Khamsi 2007). At the beginning of the transition period in the early 1990s, a sudden withdrawal of budget support from the Soviet Union, coupled with an economic crisis brought Mongolia's education sector to near collapse (Engel and Prizzon 2014). As a result, both primary and secondary enrollment rates dropped sharply. In the late 1990s, economic recovery started and public spending on education increased significantly, leading to a recovery in enrollment rates. In 2010, 91.1 and 67.6 percent of the population aged 25 or above had at least completed primary and upper secondary education, respectively (Barro and Lee 2013). Among mandatory school aged children, 4 percent were not enrolled in primary school and 7 percent were not enrolled in secondary school (NSO and UNICEF 2011).

The provision of education in Mongolia is complicated by the extremely low population density of only 1.77 persons per square kilometer in 2012 (NSO 2013). A large part of the population follows a nomadic or semi-nomadic pastoralist livelihood. To serve the dispersed and mobile population in the countryside, the government provided housing at school for rural children in the 1970s and 1980s, which was maintained after the transition period (Steiner-Khamsi 2007). Moreover, Mongolia is characterized by an extreme continental climate with

long and cold winters, which makes high demands on school buildings. For instance, in 2004, about 18 percent of the national education budget was spent heating the schools and dormitories (World Bank 2006b, p. 43).

Since 2009, the educational system consists of five years of primary school (grades 1-5, from 6 to 10 years), four years of lower secondary school (grades 6-9, from 11 to 14 years), and three years of upper secondary school (grades 10-12, from 15 to 17 years) (Ministry of Education Culture and Science of Mongolia 2006; World Bank 2015).⁵ Mandatory schooling (referred to as *basic education* in Mongolia) ends after grade nine. Vocational and technical education is offered both in the form of specialized upper secondary schools and as post-secondary diploma programs. Tertiary education, offered by private and public colleges and universities usually involves substantial tuition fees.

Mandatory education is free of charge in Mongolia. Dormitories and the meals served in the dormitories are also free of charge to children from herding families.⁶ Yet, parents often pay fees for advanced classes, school uniforms, and textbooks (Engel and Prizzon 2014). For herding households residing in the countryside, transportation to school is often expensive.

Boys from poor herding families are considered as particularly likely to drop out of school. Two reasons are suggested to explain this pattern: On the one hand, given the gendered division of labor in herding, boys are more often required to assist in herding tasks than girls. Indeed, boys in Mongolia have poorer educational outcomes than girls at all levels of the educational system (National Statistics Office and UNICEF 2011; Steiner-Khamsi 2007). On the other hand, there is a shortage of space in dormitories in district and provincial centers. Wealthier herders can cope with this limitation by splitting the household and setting up a separate household in the vicinity of schools during the school year. Yet, this opportunity is often not available to poor herders, who are short of labor and who cannot afford to set up a second household.

Estimates of the rate of return to one more year of education range between 4.2 percent at the national level (Pastore 2010) and 7.2 percent in Ulaanbaatar (Dairii and Suruga 2006). Indeed, there are disincentives to invest in education for young people engaged in herding. As a

⁵ Since the early 1990s, several reforms were implemented in the educational sector, including lowering the age of school enrollment and increasing the number of grades both in primary and in secondary school.

⁶ There was a short period of time (between 1996 and 2000) when parents were charged for their children's food in dormitories (Steiner-Khamsi 2007, p. 7). This policy (known in Mongolia as *meat requirement*) led to increased dropout rates, which went down again once the policy was abandoned in 2000 (ibid.).

result, there are considerable differences in educational attainment and labor force participation across different strata of the country.

3. Herding and weather shocks

Herding is the single most important employment sector in Mongolia, engaging about 35 percent of the workforce in 2012 (NSO 2013). Most households living outside the capital of Ulaanbaatar own livestock and about 146,000 households (about 19 percent of the population) derive their livelihood from herding (ibid.). Extensive production techniques are commonly used in herding, with animals grazed on open rangelands that are state property. Most herders are nomadic or semi-nomadic, moving their herds between two and 25 times per year; with most herders follow similar annual movement patterns, usually returning to the same campsites every year. Herders typically hold a mix of camel, cattle, horses, sheep and goats. In addition to providing meat, milk, and dairy products to herding households, animals are an income source, through the sale of meat, milk, and wool. In 2012, herding households owned an average of 244 animals (NSO 2013).

Weather risks are an inherent part of the pastoral livelihood. In particular, the extreme continental climate of Mongolia makes herders vulnerable to extreme winter events, called *dzud* in Mongolian, which cause mass livestock losses. Dzuds are triggered by various and rather different climatic conditions; often by a combination of several conditions. Among them are excessive snowfall that prevents animals from grazing; too little precipitation that limits vegetation growth; extremely cold temperatures that sharply increase the required calorie intake; as well as fluctuations in winter temperatures above and below freezing that lead the snow to melt and then ice over, thus preventing animals from reaching the grass (Batima 2006, p. 57; Murphy 2011, p. 32-33). Meteorological conditions can vary unexpectedly, which makes it difficult to predict when and where dzuds will occur.

Figure 1 shows the annual loss rates in livestock in Mongolia from 1960 through 2014. The figure shows that in recent years, two weather events of catastrophic scale hit the country. First, a triple dzud over the winters of 1999/00, 2000/01, and 2001/02 caused the death of 10.3, 15.4, and 10.8 percent of livestock, respectively. Between 1999 and 2002, about 11.2 million livestock died. Second, the 2009/10 dzud, a single winter season, caused more

than 10.3 million livestock to perish, corresponding to about 23.9 percent of the national herd.⁷

Dzuds are a major cause of rural poverty (World Bank 2006a, 2009). Formal insurance markets were not well developed in rural Mongolia when the 1999-2002 triple dzud and 2009/10 dzud occurred. Thus, apart from emergency aid provided by the government and international agencies on an *ad hoc* basis, herding households were largely left to cope with dzud on their own. Between 1999 and 2002, the number of herding households dropped by 7.36 percent (NSO 2003). Many impoverished herders moved to urban settlements or Ulaanbaatar in search of employment. During the 2009/10 dzud, some 40 percent of all herding households lost more than half of their herd (UNDP and NEMA 2010, p. 41). The food security of severely affected households was threatened (IFRC and MRCS 2010). In turn, malnutrition experienced during the dzud months had lasting impacts on the height of children from herding households (Groppo and Schindler 2014).

Dzud events can impact education through a variety of channels. For instance, they may reduce the quality of schooling infrastructure. There is anecdotal evidence that the extremely cold temperatures experienced during the 1999-2002 triple dzud and 2009/10 dzud posed challenges in providing appropriate heating and food supplies to both the schools and dormitories (Lawrie and Dandii 2010; United Nations 2000). Snow cover and cold temperatures may also make transportation to school problematic. In addition, dzuds destroy herding households' asset base and reduce their cash flows from the sale of animal byproducts, which, in turn, may curtail the household budget available for education. Reduced income flows may also worsen the nutritional status of affected households, thus decreasing children's ability to concentrate in school. Faced with household income difficulties, children may also experience psychological distress, which may further reduce their studying activities (Lawrie and Dandii 2010). Finally, dzuds may imply a higher demand for labor in herding tasks to care for weakened animals. Exposure to extreme weather events may also change households' child labor and schooling decisions *ex ante*, i.e. in anticipation of potential future dzuds. As Colmer (2013) shows for rural Ethiopia, when climate-related risks increase,

⁷ Based on reports from the European Commission (2010), IFRC and MRCS (2010), and the United Nations Mongolia Country Team (2010), we reconstructed the sequence of events during the 2009/10 dzud as follows. Unfavorable weather conditions started with a drought in summer 2009 that prevented animals from building up fat reserves. Heavy snowfall started in October 2009, preventing animals from reaching grass. From November 2009 onwards, livestock started perishing. Record low temperatures were registered in December 2009 and January 2010. By March 2010, 5.8 million livestock had perished. Starting in May 2010, the thick layer of snow melted, resulting in flash flooding that further damaged livestock.

parents may increase child labor as a strategy to minimize losses from potential future shocks. All these mechanisms would imply a negative impact of weather shocks on education. However, following a shock, investments in education may also increase. There is indicative evidence that “concerns over the viability of herding as a sustainable livelihood saw many parents putting their children back into school” (Engel and Prizzon 2014, p. 18). Moreover, experiences of livestock losses within the household may change children’s career aspirations, with children from herding households preferring to pursue non-herding activities in order to avoid repeating the trauma. This may increase individuals’ motivation and success in school (Lawrie and Dandii 2010). Hence, the overall net impact of dzud events on educational outcomes is *a priori* undetermined.

4. Data

4.1 Household survey data

Our analysis builds on the *Coping with Shocks in Mongolia Household Panel Survey*, collected by the authors in collaboration with the National Statistical Office of Mongolia (NSO). The survey with three yearly panel waves was implemented between 2012 and 2015 in the three provinces (*aimags*) of Uvs, Zavkhan, and Govi-Altai in western Mongolia. It covers 49 out of 61 districts (*soums*) in the survey area. The sample comprises 1,768 households, of which about 1,100 are herding households, and 7,200 individuals. The survey design is based on a three-stage design, which ensures that the sample is representative of the population in western Mongolia.⁸ All results presented in the following account for survey design effects, including the clustering of standard errors at the primary sampling unit level. The household survey was collected continuously throughout the year, with interviews for the first wave taking place between June 2012 and May 2013. In each month, interviews were conducted in all districts of the survey region. To account for possible differences in school enrollment across seasons, we include interview month fixed effects in our estimations.

In addition to including standard demographic and socio-economic characteristics at the individual, household and community level, our survey data also include detailed *retrospective* information on the experience of dzuds and other socio-economic household

⁸ See Groppo and Schindler (2014) for details on the survey design.

characteristics in the past. This is a unique feature of the data at hand. In particular, for herders, precise information is available on the pre-dzud herd size in 2009 and the number of livestock lost during the 2009/10 dzud.

Moreover, the survey module on migration history records information on individuals' district of residence just before the 2009/10 dzud started. In contrast, most existing studies on this topic can only draw on information on individuals' location at the time of the survey, such that individual shock experience is measured with error. Our survey records the location of residence just before the 2009/10 dzud for individuals aged 15 or older. For children in school age (who constitute the relevant sample for the analysis of the 2009/10 dzud, as detailed below), we obtain the location before the dzud based on the pre-dzud location of their mother. This information is available for 90 percent of sample individuals in the starting sample. Our final sample for the analysis of the 2009/10 dzud includes 1,152 children.⁹

While our survey records individuals' location of residence just before the 2009/10 dzud, we lack such precise information for the 1999-02 triple dzud. Hence, we rely on the location at the time of the survey, but, based on the migration history module, we exclude individuals who moved into the current district of residence in the year 2000 or thereafter (18.2 percent of the starting sample of 2,468 individuals). The sample we focus on is composed of individuals who reported either having never changed their district of residence or having moved to the current district before 2000 (74.2 and 7.6 percent of the starting sample, respectively).¹⁰ Taking into account missing information in other covariates, our final sample for the analysis of the 1999-2002 triple dzud consists of 1,867 individuals. Further details on the composition of this sample are reported in Section 5.

⁹ For 112 children, information on the maternal district of residence before the 2009/10 dzud is missing. These cases are either children whose mothers are not identified (96 cases) or children whose maternal pre-dzud location is out of our survey region (16 cases). We further lose 6 observations due to missing values in other control variables. Within the final sample of 1,152 children, 36 children changed the district of residence between 2009 and the time of the survey. To assess the impact of using the exact location of residence just before the shock unfolded, we estimated regressions that do not include pre-dzud location of residence, but instead the location at the time of the survey. While all main results are confirmed, regression coefficients are less precisely estimated and slightly attenuated in magnitude (available upon request). Hence, we opted for reporting estimates based on pre-dzud location, gaining in precision at the price of a smaller sample size.

¹⁰ As a robustness test, we repeated the main estimations for the subsample of individuals who never migrated. All main results are confirmed. In contrast, coefficient estimates obtained from the complete sample, which includes individuals who moved to the current district in 2000 or thereafter, are never statistically significant (results available upon request). This underlines that post-shock migration, if not accounted for, would lead to biased estimates.

4.2 Measures of shock intensity

Dzuds are complex phenomena caused by the interplay of several climatic conditions over time, as discussed in Section 3. There is no consensus among researchers working on Mongolia regarding what climate data and variables can best measure dzud intensity.¹¹ Therefore, we consider four alternative proxies, which are obtained with data either on livestock losses or on climatic conditions. The measures are available at different levels of aggregation (i.e., district- and household-level) and represent complementary aspects of dzud winters. Summary statistics of each measure are displayed in Table 1a and 1b.

The first – and our preferred – district-level measure builds on the rich historic livestock data available for Mongolia. Since the 1950s, the NSO implements an annual livestock census in mid-December, collecting information on the total stock and losses of adult animals. Yearly district-level data, for each of the five major domestic species in Mongolia, are available electronically, starting from 1970. Livestock losses represent a meaningful measure of dzud intensity, because they subsume various conditions of climate, terrain and vegetation during the shock.¹² Based on the livestock census data, we derive a standardized measure of the intensity of the 2009/10 dzud in survey district d as follows:

$$dzud\ intensity_{d,2010} = \frac{(livestock\ mortality_{d,2010} - mean\ livestock\ mortality_{d,1970-2008})}{standard\ deviation\ of\ livestock\ mortality_{d,1970-2008}} \quad (1)$$

where we relate the livestock mortality occurring in 2010 to the long-term local patterns in livestock mortality between 1970 and 2008 in the same district. Negative values of the dzud intensity index indicate lower than average livestock mortality in 2010, while positive values indicate higher than average mortality rates. The index for the year 2010 varies between 1.7 and 13.2 across survey districts, with a median value of 4.2. In short, loss rates in 2010 are above the long-term average in every district surveyed. We obtain the correspondent standardized intensity measure for the 1999-2002 triple dzud. In this case, at the numerator we have the average livestock mortality over the years 2000, 2001, and 2002 minus the long term-average (pertaining to the period 1970-1998). Again, we divide the term by the long-term standard deviation. The mean index for the 1999-2002 series of dzuds varies between -0.5 and 9.9 across survey districts, with a median value of 4.5.

¹¹ Tachiiri et al. (2008) describe the difficulty of deriving a climatic conditions model that predict dzud intensity.

¹² For the same reason, the index-based livestock insurance scheme that was introduced in Mongolia in 2006 uses an index based on district-level livestock mortality and not based on weather data (Skees and Enkh-Amgalan 2002).

Figure 2 illustrates the yearly district-level dzud intensity index over 1998-2013 in western Mongolia. Each bar represents one of the 49 survey districts. In each yearly graph, we present results sorted by district identification code. In other words, the intensity index for district no. 1 is displayed in the left corner and the index for district no. 49 is displayed in the right corner in every yearly graph. Three features are remarkable about Figure 2. First, both in 2009/10 dzud and in the 1999-2002 triple dzud livestock mortality was exceptional. Second, districts that were most severely affected during the 1999-2002 triple dzud were not necessarily those that also suffered the most from the 2009/10 dzud. The district-level coefficients of correlation of the dzud intensity index for the years 2000, 2001, 2002, and 2010 range between -0.5 and 0.2. The rather low or negative correlation between the intensity of several dzuds over time in the same district supports our assumption that dzuds are unexpected by households and, thus, can be considered exogenous. Third, the graphs for the years 2000, 2001, 2002, and 2010 show that even in dzud years, there is considerable variation in the dzud intensity across districts. This illustrates the fact that dzud is a highly localized phenomenon. In each year, the dzud was particularly intense in a small number of districts.

The second and third district-level measures capture specific climatic conditions during the extreme winters, drawing on snowfall and temperature data. Snowfall mainly captures the isolation and constraints in mobility experienced by households during dzud winters, while temperature can be mainly related to livestock losses, since animals are reported to freeze to death during dzud winters. Climate data are taken from the Era-Interim dataset of the European Centre for Medium-Range Weather Forecasts (ECMWF). This dataset results from a re-analysis of climate over the past decades, in which different primary data sources are combined, including data from weather stations and satellite images. The data have a high resolution (0.75 by 0.75° grid cells), which makes them valuable for our purposes.¹³ For each of the two climate variables, we use monthly Era-Interim data to calculate averages. More precisely, we consider the months November to June, when dzud conditions are normally experienced, and then average the variable within districts across months. This is done for each dzud year (1999/00, 2000/01, 2000/02, 2009/10). Then, values for the years 1999/00,

¹³ For the 1970s and early 1980s, basically all climate data for Mongolia originate from about 60 climate stations spread across the country. Given the sheer size of the country, the resolution of these data is very poor. ECMWF uses climate models to fill the gaps between these stations. As more data sources (such as satellite imagery) become available, they are incorporated into the models, such that the climatic data are constantly being updated. Dee et al. (2011) document the construction of the Era-Interim dataset. We construct sub-district level variables, which we then average at the district level (given that we do not have information on pre-shock location at the sub-district level).

2000/01, 2001/02 are further averaged to obtain one intensity measure for the 1999-2002 triple dzud. Temperature is measured in degree celsius, while snow depth is expressed in meters of water equivalent.¹⁴ For the 1999-2002 triple dzud, average snow depth ranges between 0.001 and 0.04 meters across districts, with a median value of 0.017. The 2009/10 dzud presents more extreme values of snow depth, ranging from 0.004 to 0.184 meters, with a median of 0.039. In both cases, median snow depth is higher than in non-dzud years (for example, median snow depth in 2006 was 0.009). Hence, for both dzuds, the snow depth measure is able to capture exceptional conditions. We cannot say the same if we consider temperature. Median temperature is equal to -6.61°C and -10.25°C for the 1999-2002 and 2009/10 dzuds, respectively. This compares to a median temperature of -7.89°C in a non-dzud year (2006). Thus, cold temperatures were not among the weather conditions causing the 1999-2002 triple dzud and we do not include the results of the temperature-based measure in our analysis of the impact of the 1999-2002 triple dzud.¹⁵

The fourth shock measure, only available for the 2009/10 dzud, is the percentage of livestock lost during the dzud, relative to the household's 2009 holdings.¹⁶ This household-level measure allows us to account for within-district variation in dzud intensity. Such within-district variation is likely to be considerable, given that districts in western Mongolia have an average size of 4,811 square kilometers. Yet, one shortcoming of the household-level measure is that it is potentially endogenous, since there might be omitted variables influencing both household-level livestock losses and education investment decisions. To minimize this concern, we employ the household-level shock measure in conjunction with household fixed effects, as we discuss in Section 5.

To account for the skewed distribution of dzud intensity across districts (and households) in the multivariate analysis below, we further transform each of the four continuous dzud intensity measures into an indicator variable. Using a binary shock measure has the additional

¹⁴ The (snow) water equivalent of a given snow cover is defined as:

$$\text{snow water equivalent (m)} = \text{snow depth (m)} * \frac{\text{snow density } (\frac{kg}{m^3})}{\text{water density } (\frac{kg}{m^3})}$$

For more information on the concept of snow water equivalent, see, for example, Sturm et al. (2010).

¹⁵ In line with our expectations, the temperature-based measure of dzud intensity in 1999-2002 yields non-significant results.

¹⁶ In fact, households are asked retrospectively about their livestock holdings in 2009 and dzud-related livestock losses twice, in the first and again in the third panel wave. Our preferred measure is retrospective information recorded in the first wave, given that the recall period is shorter. As a robustness test, we carry out all estimates with retrospective information recorded in the third wave and obtain very similar results (available upon request). This test assures that the retrospectively recorded variables are of good quality.

advantage that it is less prone to measurement errors than the continuous shock measure (Justino, Leone and Salardi 2014; León 2012). We define a binary variable that takes the value one if the dzud intensity in a given district (or household) is above the median plus one standard deviation of the distribution.¹⁷ Thus we compare districts (households) in which the dzud was catastrophic to districts (households) in which it was moderate.

5. Empirical strategy

The empirical setting and the data at hand allow us to explore and contrast the impact of two separate extreme weather events on human capital outcomes. We first focus on the dzud of 2009/10 to identify its short-term effects on school enrollment two to three years after the shock. We then consider the 1999-2002 triple dzud and identify its long-term effects on the probability that individuals completed basic education ten to eleven years after the shock. Given the timing of these shocks and the data collection, we apply two different estimation approaches that are outlined in this section. Overall, our identification strategy follows studies that investigate the impact of shocks on education – but which have focused on different types of shocks, such as a locust invasion in Mali (De Vreder, Guilbert and Mesple-Somps 2015), the 1999 conflict in Timor Leste (Justino et al. 2014), the civil war in Tajikistan (Shemyakina 2011), the Maoist insurgency in Nepal (Valente 2014), the Peruvian internal conflict (León 2012), and the civil war in Burundi (Verwimp and Van Bavel 2014).

5.1 Dzud of 2009/10

To explore the short-term impact of the 2009/10 shock, we exploit exogenous variation in shock intensity across space, thus employing an approach used by, for instance, Shemyakina (2011) and Deuchert and Felfe (2015). We estimate the following reduced-form model:

$$Enrolled_{ipym} = \beta_1 shock_{2010} + \beta_2 X_{ihd} + \alpha_p + \delta_{py} + \lambda_y + \gamma_m + \varepsilon_{ipym} \quad (2)$$

for individual i living in province p just before the shock, born in year y and interviewed in month m . The dependent variable of interest is an indicator variable that takes the value one if

¹⁷ The dzud measure based on livestock mortality is only available at the district level, so the district-level distribution is used to construct this indicator. Differently, for climate-based measures, we have sub-district level information. Hence, in this case, we use the sub-district level distribution to construct the district-level indicators for severely affected districts. In particular, these indicators are equal to one for districts including at least one sub-district with intensity above the average plus one standard deviation.

the child is enrolled in mandatory schooling (grade one to nine) at the time of the survey.¹⁸ The sample consists of children of age 6 to 14 years at the time of the survey; thus born between 1998 and 2007.¹⁹

We employ three alternative district-level measures for $shock_{2010}$ that are derived from data on livestock mortality, snow depth, and temperature (as described in Section 4.2). Our main interest is in the estimated coefficient β_l , which indicates whether exposure to the 2009/10 dzud affects school enrollment in 2012/13.

Our regressions include *province fixed effects* (α_p), which control for geographic time-invariant differences, pertaining, for instance, to the quality of schools, attitudes toward education, and conditions of the local labor market. Given that our sample consists of individuals of a relatively wide age span, we also control for dynamic factors that may influence school enrollment. First, we include a *province-specific linear time trend* (δ_{pt}) capturing potential divergence or convergence in educational outcomes over time across provinces (which could result from public investments and aid targeting regions that were severely affected by the dzud or from different trajectories in economic development across provinces). Second, we include *age fixed effects* (λ_y), which control for differential incentives to enroll in school and differential exposure to reforms of the education sector across cohorts. *Interview month fixed effects* (γ_m) control for potential seasonal variation in school enrollment.

Vector X_{iht} includes a wide range of socio-economic controls. As child characteristics, we control for gender and age. Household-level controls include mother's and household head's years of education, whether the head is female, total household size, number of children in the age range 0-5 and 6-14, household assets (in quintiles),²⁰ household's location (rural or district centre, with the omitted category being province centre), whether the household

¹⁸ Following Pastore (2010), we refrain from using years of schooling as outcome as this variable may entail measurement errors due to the difference between official and actual years of schooling required to achieve a given qualification level and due to several reforms in the Mongolian educational system since 1990 that also changed the number of classes in the qualification levels.

¹⁹ The oldest children in this sample were born just before or during the 1999-2002 triple dzud. As discussed in Section 4.2, the correlation between dzud intensity across the 1999-2002 dzuds and the 2009/10 dzud is very low. The inclusion of age fixed effects and province fixed effects should control for any potentially confounding effects of exposure to the 1999-2002 dzuds.

²⁰ Using principal component analysis, an asset index is constructed from a set of dummy variables indicating ownership of durables that were bought one year or more before the survey interview (including home appliances, furniture, electronic goods, and means of transportation). The continuous asset index is then transformed into quintiles.

owned any livestock in 2009, and herd size in 2009²¹ (in quintiles). Our district-level controls are indicators for whether the district has good cellphone coverage and whether electricity is normally available 24 hours a day in the district. Table 1a provides summary statistics for these variables. Lastly, ε_{ipym} is a random idiosyncratic error term clustered at the primary sampling unit level.

The estimation strategy yields consistent and causal estimates of β_l under the assumption that the error term is uncorrelated with the incidence of the 2009/10 dzud. To explore the validity of this assumption, we first turn to the descriptive statistics in Table A1 in the Appendix, which compare pre-shock individual education between districts that were severely affected by the 2009/10 dzud and less affected districts (here we focus on individuals who most likely had completed their basic education before 2009).²² The outcome of interest in this test – having acquired basic education – is not statistically different between severely affected and less affected districts. This finding holds regardless of which district-level measure of shock intensity is employed. Next, we use the framework of Eq. 2 to carry out a placebo regression, where again the sample consists of individuals born before 1994 and whose level of education should not have been affected by the 2009/10 dzud (Table A2 in the Appendix, column 1). In line with our expectations, individuals living in districts that were later severely affected by the dzud are not significantly more or less likely to have completed basic education compared to individuals living in less affected districts, holding all other factors constant. From these tests we conclude that there are no obvious hints pointing toward omitted variables that are potentially correlated with shock intensity. Moreover, recall that all estimations include province fixed effects and province-specific time trends, which helps to remove time-invariant and time-varying differences in education across provinces.

Next, we estimate a specification that additionally includes household fixed effects, which control most comprehensively for any observable and unobservable household-level characteristics that may correlate with shock exposure. Thus, among all results presented below, this specification is the one where we can most confidently minimize potential

²¹ The number of livestock in 2009 was only collected from households that reported having been affected by the dzud. Within our baseline regression sample, this information is missing for the 47 households reporting that they did not experience the 2009/10 dzud. For these households, we estimated the number of livestock in 2009 as follows: Based on the information on the number of livestock owned at the time of the interview and on the changes in livestock in the 12 months before the interview, we obtained the number of livestock owned in 2011. Then, we imputed the number of livestock in 2009 by assuming an annual growth rate in total livestock of 0.15 (this value is the average growth rate of livestock in year 2012 – a non-dzud year – obtained from the district-level livestock census).

²² In this unconditional test, we only consider individuals living in rural areas, who are relatively homogenous in their educational background.

endogeneity. Identification now comes from comparing the shock exposure of younger and older siblings living in the same household. In this specification, we also use the household-level shock measure in addition to the three district-level shock measures.

Lastly, we explore whether the 2009/10 dzud had heterogeneous effects across different types of households and individuals by interacting the district-level shock measures with various socio-economic controls. The latter include the child's gender, parental education, pre-dzud household wealth, and remoteness.

5.2 The 1999-2002 triple dzud

To analyze the long-term effects of the series of dzuds between 1999 and 2002 on education outcomes, we exploit variation in shock exposure not only across space, but also across birth cohorts. We estimate the determinants of completing mandatory schooling with a difference-in-differences approach (similar to most existing studies in this field) as follows:

$$Completed_{idy} = \beta_1 exposed\ cohort_i + \beta_2 (exposed\ cohort_i * shock_{d,2000-2002}) + \beta_3 X_{ih} + \eta_d + \theta_{dy} + \lambda_y + \varepsilon_{idy} \quad (3)$$

for individual i living in district d , born in year y . The outcome variable of interest is an indicator variable taking the value one if an individual completed at least mandatory schooling at the time of the survey. The sample consists of individuals born between 1975 and 1994, who were 18-38 years at the time of the survey.²³ The *exposed cohort* comprises individuals born between 1985 and 1994. These are individuals who were of school age (8-15 years) when the triple dzud occurred. The control cohort consists of individuals who were at least 15 years of age in 1999 so that, under normal circumstances, they would have completed their mandatory education before the shock unfolded. Individuals of the control cohort were born between 1975 and 1984. By focusing on the completion of mandatory schooling, we keep concerns of the endogeneity of households' education investment decision to a minimum.

²³ By definition, all individuals in this sample reached age 15 by 2009. Hence, we can exclude the possibility that the 2009/10 dzud influenced the completion of mandatory schooling for these individuals (results remain unchanged when limiting the sample to individuals born in 1993 or earlier, reaching age 15 by 2008). All individuals in this sample started school before 2005, so they followed the schooling system in force before 2005, which entailed 8 years of mandatory education (from 8 through 15 years of age).

The estimated coefficient of the interaction term, β_2 , is our main focus in this estimation. The variable $shock_{2000-2002}$ represents district-level shock measures of the average dzud intensity over the triple dzud period based on either livestock mortality or snow data, as outlined in Section 4.2. It is important to note that the specification includes *district fixed effects* (η_d) and a *district-specific linear time trend* (θ_{dy}). The inclusion of fixed effects and the time trend rules out potentially confounding effects due to district-specific aspects, such as the availability of educational infrastructure, average schooling levels and investments into the educational sector over time. Again, we include *age fixed effects* (λ_y) so that results are neither influenced by differences in the educational system nor in the incentives to acquire education across cohorts.

Vector X_{ih} contains controls for socio-economic characteristics of the individual and the household. These either reflect characteristics that are very likely to be persistent over time or they build on retrospective information relating back to 1999, before the series of shocks started. Given the fact that the shocks started some 13 years before the household survey was implemented, we are constrained to using a smaller number of covariates compared to the short-term analysis. Individual-level controls include gender, age, and relationship to the head of household (whether the individual is the household head, his/her child, or his/her spouse or other relative). As household-level controls we employ parental years of education²⁴ and a dummy variable indicating whether the individual's father owned livestock in 1999.

The difference-in-differences approach provides estimates of the causal effect of the 1999-2002 triple dzud under two assumptions. First, both pre-shock trends and levels in education were the same in the severely affected and the less affected districts. We provide supportive evidence of this by considering a sample of individuals who were old enough to have completed mandatory education before the series of dzuds. Unconditional statistics in Table A3 in the Appendix show that when employing a shock measure based on district-level livestock mortality (Panel A), there is no statistically significant difference in the educational attainment across cohorts, neither across severely affected nor less affected districts. When we instead use a shock measure based on snow depth, then individuals residing in severely affected districts have significantly *higher* levels of education. If a bias still persists after including district fixed effects, we would expect it to reduce the magnitude of the estimated

²⁴ For about 10 percent of individuals in this sample, either maternal or paternal education is missing. To maximize the number of observations, we include one single education variable, which is the maximum number of years of education for either the mother or the father.

effect of the shock downwards, thus leading to an underestimation of the impact of the dzud. Next, we conduct a placebo test, again focusing on individuals who completed basic education before the shock. We focus on the control cohort of the baseline sample and define within it a ‘falsely’ exposed cohort (‘placebo treatment cohort’). We again use a difference-in-differences approach to estimate the impact of the dzud. Results displayed in Table A4 in the Appendix show that individuals of the placebo treatment cohort and who live in districts that were later severely affected by the 1999-2002 triple dzud do not have a significantly lower probability of completing mandatory education compared to same-aged individuals living in less affected districts. Hence, also this test supports our first identifying assumption for the Mongolian context.

The second assumption is that no unobserved characteristics exist that are both correlated with shock exposure and education outcomes. Again, district fixed effects and the district-specific linear time trend help us account for differences across districts. We further reduce concerns about omitted variables by controlling for socio-economic household characteristics.

Lastly, we explore the age at which individuals’ outcomes are most sensitive to shocks (Groppo 2015; Maccini and Yang 2009; van den Berg et al. 2014). We differentiate between three groups of exposed children who experienced the 1999-2002 triple dzud at different ages: Children who were of pre-school age, of primary school age, and of secondary school age when the triple dzud occurred. The treatment group includes individuals aged 4-16 years in 2002 (16-29 years in 2014/15), while the control group consists of individuals aged 17-29 years in 2002.²⁵ Thus, in comparison with the ‘baseline’ sample for the long-term analysis, we expand the sample to additionally include slightly younger treated cohorts (and also slightly older control cohorts). Treated cohorts who experienced the 1999-2002 triple dzud during preschool and primary school age may have also been exposed to the 2009/10 dzud during adolescence, which may lead to confounding effects. To exclude this possibility, we control for the specific effects of the 2009/10 dzud with one additional interaction term between dzud intensity in 2010 and exposed cohort in 2010 in this specification.

²⁵ These groups are constructed on the basis of age as measured in the third wave of the panel survey (collected in 2014/15). The dependent variable is also taken from the third survey wave.

6. Results

6.1 The short-term impact of extreme weather events

We first turn to the impact of the 2009/10 severe winter on children's enrollment in mandatory schooling. Results from OLS estimations (Eq. 2) are displayed in Table 2. For the sake of readability, the table only shows coefficients of the various shock measures, while the estimated coefficients of the full set of covariates can be found in Table A5 of the Appendix.

We find evidence of a strong negative impact of the 2009/10 dzud on the enrollment of children from herding households²⁶ two to three years after the shock (Table 2, columns 3-4). Children from herding households that resided in a district experiencing high livestock mortality during the 2009/10 dzud are significantly less likely to be enrolled in school in 2012/13 compared to children who resided in a less affected district during the dzud, holding other factors constant. This result is confirmed when we define severely affected districts based on snow depth (Panel B). Yet, this negative effect of the shock is hardly visible when we focus on the whole sample of children from both herding and non-herding households (columns 1-2). In fact, for children from non-herding households, we do not find evidence for a systematically negative impact of the shock on enrollment at all.²⁷ This suggests that the negative effect of the dzud did not come via supply side effects in the provision of education, such as problems of providing adequate heating in school buildings during the disaster winter (to which all children were exposed). In all further analyses, we therefore restrict the sample to children from herding families.

Disaggregating the sample further by age reveals surprising results: In columns 5-6, we focus on children from herding families in secondary school age (11-14 years) at the survey interview. This cohort of children was between 8 and 12 years old and thus already of the mandatory school age when the extreme winter occurred. For this cohort, the shock has no significant effects on school enrollment. Moreover, we do not find any statistically significant

²⁶ More precisely, we focus on households that were herders in 2009, before the dzud. Of the 1,094 sample households that were herders in 2009, 125 households lost a significant share of their livestock and dropped out of the herding economy in the aftermath of the dzud.

²⁷ We find similar results when using the full sample and interacting an indicator variable equal to one for children from herding households with the shock intensity measure (available upon request). The rationale for estimating the model for two separate subsamples of children from herding and non-herding households is that the livelihoods of these households are very different: mostly urban wage employees versus mostly rural nomadic herders. Therefore, important control variables, such as education and assets, have a different influence and a different distribution in each livelihood, which would require a large number of interactions.

effect of the dzud on the number of years of education acquired by these children (results available upon request).

On the contrary, the cohort of children from herding households who were in primary school age (6-10 years) at the time of the survey is significantly affected by exposure to the catastrophic winter (columns 7-8). For this younger cohort, we find consistently negative and significant effects for all district-level shock measures, regardless if shock intensity is defined in terms of livestock mortality, snow depth, or temperature. These results hold both when measuring shock exposure with continuous intensity measures and when using indicator variables for most affected districts. For children aged 6-10 years, residing in a severely affected district reduces the likelihood of being enrolled in school two to three years after the shock by an average of 6.3 to 7.8 percentage points, *ceteris paribus*. The majority of these children had not yet reached school age when the shock occurred. Indeed, the effect becomes even larger in magnitude when we restrict the sample to only those children who were of preschool age when the shock occurred (results available upon request). This result could possibly capture the effect of shock-induced losses in household income that, in turn, lead to malnutrition or health problems among preschool aged children. In fact, such interpretation would fit well with another study on Mongolia that documents a negative effect of the 2009/10 dzud on the height for age of preschool children surveyed two to four years after the dzud (Groppo and Schindler 2014).

The vulnerability of younger children is confirmed in Table 3, which presents results from a different specification that uses household fixed effects. Results in Panel A show that younger children (6-10 years at the interview) from households that suffered above-average livestock losses during the dzud are significantly less likely to be enrolled in school two to three years after the shock when compared to their older siblings (aged 11-14 years at the interview), holding all other factors constant. While all district-level shock measures have the expected sign, only some of the point estimates are statistically significant. This is as expected, given that in this specification, some of the spatial variation in shock intensity is already captured by household fixed effects. Interestingly, the household-level shock measure included in Panel D – the percentage of livestock a household lost during the dzud – is statistically significant. This indicates that in addition to the adverse weather conditions that all households face, there is a significant effect pertaining to the losses a particular household experienced. Consistent with results in table 2, this finding also suggests that the shock impairs school enrollment through an income effect.

Next, we explore the persistence of the effect of the shock over time, exploiting the fact that we have three post-shock observations for each child (Table 4). In this case, the sample is reduced by 46 children who dropped out of the panel sample in the second or third wave. To provide the basis for meaningful comparisons over time, we first repeat the estimation with enrollment measured at the first wave for this reduced sample (column 1) and then show estimates for the same sample with the outcome measured in the second and third wave (columns 2-3). Results indicate that when revisiting the children in 2013/14 (wave 2) and 2014/15 (wave 3), the negative impact of the dzud persists. The estimated coefficients of the dzud measure remain statistically significant and the magnitude is comparable to the baseline estimates obtained from first wave data. These results also serve as a robustness test. Obtaining very similar effects of the shock in terms of both magnitude and significance level from three independent observations for each child, we can rule out the possibility that our effects are driven by measurement errors in either the outcome variable or in reported age.

In Table 5, we explore the heterogeneity of the effects along individual- and household-level characteristics. In contrast to our expectations, we do not find evidence that boys were more strongly affected by the shock than girls, holding all other factors constant (column 1). Given that herding tasks follow a strict gendered division of labor, with boys being more engaged in tending animals, this result makes it unlikely that the negative effect of the dzud on education worked through a higher demand for child labor in herding. In column 2, we analyze whether pre-shock household wealth, measured with quintiles of pre-shock livestock holdings,²⁸ mitigates the negative consequences of the dzud. Only one interaction term is statistically significant, with the magnitude of the effect being small: Shock-affected children living in herding households with average pre-shock herd size in 2009 are significantly more likely to be enrolled compared to shock-affected children of poor households, *ceteris paribus*. The limited differentiation in the response to the dzud by pre-shock wealth is likely a result of the extreme severity of the 2009/10 dzud, which hit not only poorer herders, but also wealthier and better prepared ones. However, there is evidence that the higher the mother's level of education, the lower the impact of the dzud, holding wealth and all other factors constant (column 3). Yet, maternal education does not seem to influence enrolment for children living in districts with average livestock mortality.²⁹ Vice-versa, the education of the household

²⁸ Similar results are obtained when using alternative definitions of pre-shock livestock holdings, including a continuous measure, a continuous measure with quadratic terms, an indicator for livestock holdings above the median, and terciles.

²⁹ All continuous variables are centered at the mean. Hence, the coefficient on the non-interacted term, say, mother's education, represents the effect of mother's education for a child who lives in a district with average shock intensity.

head (who is the child's father in most cases) does not mitigate the negative effects of the dzud, while it significantly increases enrolment in districts with average livestock mortality (columns 4-5). Hence, our results suggest a different role for mother's and father's education, with the former being more important than the latter in mitigating the consequences of extreme weather events. Lastly, results show that remoteness – measured by the distance of a household's campsite from the district center³⁰ – reinforced the negative consequences of the shock (column 6). Note that in this specification, we proxied shock intensity with snow depth and thus use the measure that best represents constraints in mobility. One possible explanation of the negative and significant coefficient on the relevant interaction term is that constraints in mobility during the dzud winter and, thus, difficulties in accessing the nearest market to purchase livestock fodder led to higher household-level livestock losses. Interestingly, the non-interacted effect of remoteness on school enrollment is not statistically significant. Thus, it appears as if remoteness is not associated with obstacles for children's educational attainment *per se*, but only when households face a dzud.

To obtain more insights into the possible mechanisms through which the dzud affected education, we regress three additional outcome variables on the household-level shock measure and other covariates (Table 6). Those outcomes are whether the household still owned livestock in 2012 (and thus maintained its herding livelihood in the post-shock period) (columns 1-2), current household income (columns 3-4), and a subjective measure of whether the household faced difficulties paying for education expenses in 2012/13 (columns 5-6). Results indicate that severely affected households are significantly less likely to keep herding; they have significantly lower income and are significantly more likely to experience difficulties in paying for educational expenses. Although these estimates should not be interpreted as causal (as this specification does not include household fixed effect that would minimize concerns of endogeneity of shock exposure), we interpret them as suggestive evidence that the effect of the dzud on education worked mainly through the income channel.

Furthermore, we explore whether our results relate to the specific 2009/10 dzud by performing a 'placebo' dzud test. We falsely consider the year 2008 as dzud year and

³⁰ The distance variable was recorded at the time of the first panel wave. Hence, in this estimate we exclude children who changed their district of residence after 2009. Moreover, we have to assume that households did not change their location within the district in response to the dzud. Given that campsites of good quality are precious and often passed on from one generation to the next, we consider it very likely that households use similar movement patterns every year. Moreover, as herding households use various campsites within one year, we take the location of the campsite at the time of the survey as proxy for the average distance to the district center of the household's other campsites.

construct shock intensity measures for 2008 based on district-level livestock mortality, snow depth, and temperature (Appendix, Table A2, column 2). As expected, the estimated coefficients of the placebo measures based on livestock mortality (Panel A) and snow depth (Panel B) are not statistically different from zero. The estimated coefficient of the temperature-based placebo measure (Panel C) even points to a significant and positive effect. These results confirm that our analysis captures effects that are specific to the 2009/10 dzud.

In all results discussed so far, we employed OLS because it allows to control for survey design effects and to easily interpret the size of the estimated coefficients. Alternatively, one could use a penalized maximum likelihood estimation approach that yields unbiased estimates in the case of rare events (Firth 1993; Heinze and Schemper 2002). Results obtained with this alternative estimator are presented in the Appendix, Table A6. One caveat with penalized MLE is that the survey design – and, most importantly, the clustering of standard errors – cannot be accounted for.³¹ Thus, for comparability, columns 1-2 first show baseline results obtained with OLS that accounts for clustering and survey design, columns 3-4 show OLS results when ignoring the clustering of standard errors and design effects, and columns 5-6 present results obtained with penalized MLE. The latter two models yield similar results in terms of significance levels and sign of the point estimates, which is a good indication that results are not sensitive to the choice of the estimator.

6.2 The long-term impact of extreme weather events

Next, we explore whether exposure to extreme weather events also has long-term effects on education outcomes, using a difference-in-differences approach. Results from OLS regressions on the impact of the 1999-2002 dzuds on the probability to complete basic school are presented in Table 7. Again, estimations are carried out separately for the full sample of individuals (columns 1-2) and for individuals from herding households (columns 3-4).

Results show a strong and negative effect of exposure to the 1999-2002 triple dzud on the probability to complete basic education ten to eleven years after the shock. Again, results are driven by herding households. Individuals from herding households who were exposed to the triple dzud at school age and who lived in a district severely affected by it are significantly less likely to have complete basic education in 2012/13 compared to their peers of the same

³¹ Penalized MLE estimations were carried out in STATA with the user-written command *firthlogit*. Another drawback of *firthlogit* is that it does not include a post-estimation command to obtain marginal effects.

age who resided in a less affected district, holding all other factors constant (column 4). This result holds both when including a shock intensity measure based on livestock mortality or when including an intensity measure based on snow depth (although only when proxying shock intensity with indicator variables). The magnitude of the effect is large: Residing in a severely affected district reduces the likelihood to complete mandatory education by 26.5 to 31.2 percentage points for children from herding households. Given that 72.8 percent of individuals included in the herding sample obtained a basic education certificate, this represents a reduction in schooling levels by about 40 percent.

The magnitude of the effect is also large in comparison to studies examining the effect of other types of shocks on education in different empirical contexts. For instance, for women, exposure to the Tajik civil war reduces the likelihood to complete mandatory education by 7 percentage points (Shemyakina 2011). Exposure to the 1999 violence in Timor Leste reduces the likelihood that boys complete primary education by 18.3 percentage points (Justino et al. 2014). Exposure to the Burundian civil war reduces the probability to complete primary school for boys by 7 to 17 percentage points (Verwimp and Van Bavel 2014). Exposure to a locust plague has no statistically significant effect on the completion of primary schooling for children in Mali (De Vreyer et al. 2015). Nepalese girls living in districts with a relatively high number of casualties from the Maoist insurgency are even 5.6 percentage points more likely to complete primary education than those living in unaffected districts (Valente 2014).

Last, we investigate whether the impact of the triple dzud on the completion of basic education is uniform across individuals exposed at different ages (Table 8), using data from the third panel wave. Column 1 shows that children exposed to the series of dzuds as preschoolers, at primary school age, and at secondary school age are (at least marginally) significantly less likely to have completed mandatory schooling in 2014/15 than same-aged children living in non-affected districts. Yet, the magnitude of the detrimental effect is more than double in size for children exposed to the triple dzud as preschoolers when compared to children exposed during secondary school age. Adjusted Wald tests reject the hypothesis that the estimated coefficients of the interaction terms for primary and preschool ages are equal. This result is obtained defining the preschool cohort as children aged 2 to 5 during the triple dzud (who were 4-5 years old in 2002). Sensitivity tests with different cutoffs points between age groups confirm these results. When expanding the preschool treatment group to include even younger children born in 1999 (who were 1-3 years of age during the triple dzud),

results are even stronger (column 2).³² In contrast, the effect of the shock is smaller in magnitude when we also classify as preschoolers those children born in 1996 (of age 4-6 during the triple dzud) (column 3). This again underlines that younger children are most vulnerable.

7. Discussion

We provide detailed evidence that exposure to extreme weather events has severe negative consequences for the educational attainments of children in Mongolia. Our focus is on two winter disasters (*dzud* in Mongolian) that struck the country in 1999-2002 and in 2009/10. These disasters resulted in the death of 11.2 million and 10.3 million livestock, respectively. We explore both short- and long-term consequences of these two extreme weather events, exploiting exogenous variation in shock intensity in space (when focusing on the 2009/10 shock) and across space and time (when focusing on the 1999-2002 triple dzud). Our analysis draws on household panel data that includes comprehensive information on households' historic severe weather shock experience.

We find that exposure to the 2009/10 extreme winter significantly lowers the probability that children from herding families are enrolled in mandatory school by 4 to 7 percentage points. Results of similar magnitude are obtained when carrying out estimates separately for each of the three panel waves, thus documenting the persistent detrimental effect two, three, and four years after the shock. Our main finding holds after controlling for a wide range of socio-economic characteristics (including pre-shock wealth), cohort and location fixed effects, time trends, as well as household fixed effects. Drawing on exhaustive information on individuals' migration history allows us to exclude potential bias due to endogenous migration. One empirical challenge – which may also be relevant for other complex weather events – is that winter disasters are caused by several underlying climatic conditions that interact with and reinforce each other. We address this concern by drawing on unique historic livestock census data to derive a standardized measure of district-level livestock losses. While this comprehensive measure is our preferred proxy for the intensity of extreme winters, all

³² However, these results are likely upward biased. Individuals born in 1999 reached age 15 exactly in the year of the survey (2014/15), so that at that time they may be enrolled in the final year and complete basic school shortly after. Hence, our preferred specification is the one reported in column 1.

baseline results are confirmed when employing measures of shock intensity derived from climate data on snow depth and temperature.

The estimated negative impact of the dzud represents a ‘net effect’ of several possible underlying channels, which can, in principle, have both positive and negative effects on education. Because only children from households owning livestock before the shocks are affected, it is unlikely that supply side factors drive the results. We can also exclude that these are driven by an increased demand for child labor, because the effects are higher for relatively younger children. Rather, it seems that winter disasters affect children through losses in household assets and income, with children from households that lost more livestock bearing the largest consequences. In turn, those income effects may potentially result in malnutrition or health problems among children. We find that education of the child’s mother mitigates the negative impact of the shock, while higher pre-shock household wealth does not. There is no evidence for differential effects of the shock by the child’s gender.

In the long-term analysis, we find that exposure to the triple dzud winter disaster between 1999 and 2002 significantly reduces the probability of completing basic schooling. Again, this effect is most pronounced for children from herding households, for whom shock exposure reduces the likelihood to obtain a basic school certificate by 26.5 to 31.2 percentage points. Results from both the short- and the long-term analyses show that exposure to weather shocks during preschool age (as opposed to exposure during primary or secondary school age) yields the worst consequences for educational attainment.

Overall, these robust results, showing strong detrimental impacts of severe weather events on the attainment of basic education provide reason for concern and policy attention, especially in light of the following two considerations. First, Mongolia is a lower middle income country with relatively high levels of education and human development indicators well above the average compared to the Central Asian region. Moreover, there are no formal tuition fees for basic education and the government provides subsidized school dormitories for children of herding households, thus reducing the barriers for households to invest in their children’s basic education. Thus, if the population analyzed in this study does not manage to protect the human capital of their children from the negative effects of extreme weather events, such shocks may even have worse consequences in poorer economies. Second, the process of human capital formation is accumulative, with education investments in early ages affecting the productivity of further human capital investments (Cunha and Heckman 2007). Therefore, the detrimental effects of the shock, which we find in relation to basic education, are likely to

be even more serious when considering the acquisition of post-secondary education. While understanding the full effects of extreme weather events on education and labor market outcomes remains an open field of research requiring long-term data, results from this study already provide relevant policy implications. In particular, post-disaster relief should be targeted to households in severely affected districts with preschool aged children and, as suggested by Deuchert and Felfe (2015), possibly tied to later children's school participation. The implementation of preventive programs aiming at reducing these households' vulnerability to weather shocks or the provision of weather insurance may also be effective in protecting human capital accumulation in the face of severe weather events.

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Figures

Fig. 1: Annual losses in livestock in Mongolia, 1960-2014

Notes: Livestock include camel, cattle, horse, sheep, and goats. Source: Mongolia Livestock Census.

Fig. 2: Distribution of dzud intensity between 1998 and 2013 across survey districts

Notes: In each yearly graph, each bar represents one sample district, with districts being sorted by district identification code. For each district-year pair, the dzud intensity index is calculated as the difference between the pair's percentage livestock losses and the long-term average percentage losses in the district, divided by the long term standard deviation of percentage losses for that district. The period considered as long-term starts in 1970 and ends in the year preceding the year of interest. For instance, for the 1998 dzud intensity index, we consider the period 1970-1997 as long-term. However, when calculating the indexes for 2000, 2001, and 2002, the reference period is set to end in 1998. Similarly, for the 2010 dzud index, the reference period is set to end in 2008 (as from Eq. 1). Source: Mongolia Livestock Census.

Tables

Table 1a: Summary statistics (variables used in the short-term analysis)

	Mean	Std. Dev.	Min	Max	Number of children
<i>Dependent variables (measured in 2012/13)</i>					
Enrolled in school	0.96	0.19	0	1	1,152
Household owned livestock	0.75	0.43	0	1	1,152
Total household income, past 12 months (in '000 Tugrug)	8,060.12	6,769.89	240	96,01	1,152
Household had many difficulties to pay for education	0.17	0.37	0	1	1,137
<i>Measures of intensity of the 2009/10 dzud</i>					
Livestock mortality index ^a (district-level)	5.58	2.60	1.67	13.21	1,152
District-level snow depth (in meter, district-level)	0.05	0.04	0.004	0.18	1,152
District-level temperature (in °C, district-level)	-9.74	1.78	-12.32	-1.76	1,152
Percentage of livestock lost (household-level)	0.41	0.25	0	1	827
<i>Child controls</i>					
Female	0.44	0.50	0	1	1,152
Age	10.11	2.65	6	14	1,152
<i>Household controls</i>					
Mother's years of education	8.28	3.72	0	17	1,152
Household head's years of education	7.43	3.70	0	17	1,152
Household head is female	0.06	0.24	0	1	1,152
Household size	5.17	1.29	2	11	1,152
Number of children 0-5 years	0.60	0.70	0	3	1,152
Number of children 6-14 years	1.88	0.80	1	4	1,152
Asset index ^b	-0.63	2.08	-3.49	6.25	1,152
Owned livestock in 2009	0.79	0.41	0	1	1,152
Number of livestock in 2009 (in hundreds)	3.05	2.33	0.02	15.4	827
Rural	0.57	0.50	0	1	1,152
District center	0.16	0.37	0	1	1,152
Province center	0.27	0.45	0	1	1,152
Distance from district center (in tens of km)	1.74	2.21	0	21	1,152
<i>District controls</i>					
Cellphone network available in most or all areas	0.59	0.49	0	1	1,152
Has electricity for 24 hours per day	0.82	0.38	0	1	1,152

Notes: ^a Based on Eq. 1. ^b The asset index is computed using a principal component analysis of 30 dummy variables indicating ownership of assets bought more than 12 months before the interview. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table 1b: Summary statistics (variables used in the long-term analysis)

	Mean	Std. Dev.	Min	Max	Number of adults
<i>Dependent variable (measured in 2012/13)</i>					
Completed basic education	0.77	0.41	0	1	1,867
<i>Measures of intensity of the 1999-2002 dzud</i>					
Livestock mortality index ^a (district-level)	4.55	2.43	-0.54	9.89	1,867
District-level snow depth (in meter, district-level)	0.02	0.01	0.00	0.04	1,867
District-level temperature (in °C, district-level)	-6.42	1.62	-9.25	0.41	1,867
<i>Individual controls</i>					
Female	0.49	0.49	0	1	1,867
Age	26.70	5.63	18	38	1,867
Household head	0.27	0.43	0	1	1,867
Household head's child	0.25	0.42	0	1	1,867
Household head's spouse or other relative	0.48	0.48	0	1	1,867
<i>Household controls</i>					
Maximum years of education of parents	7.56	3.90	0	17	1,867
Father was a herder in 1999	0.62	0.47	0	1	1,867

Notes: ^a. Based on Eq. 1. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table 2: The impact of the 2009/2010 dzud on mandatory school enrollment (baseline), OLS

Dependent variable: 1=child is enrolled in school	Full sample		Children from herding households					
	Age 6-14		Age 6-14		Age 11-14		Age 6-10	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A - Dzud measured as district-level livestock losses</i>								
Continuous intensity	-0.002		-0.002		0.001		-0.007*	
	[0.613]		[0.516]		[0.879]		[0.096]	
Intensity > mean + 1 sd		-0.027		-0.041*		-0.038		-0.063**
		[0.158]		[0.070]		[0.262]		[0.020]
<i>Panel B - Dzud measured as district-level snow depth</i>								
Continuous intensity	-0.404		-0.594**		-0.207		-0.846***	
	[0.139]		[0.021]		[0.556]		[0.007]	
Intensity > mean + 1 sd		-0.034		-0.048**		-0.023		-0.078***
		[0.167]		[0.039]		[0.465]		[0.009]
<i>Panel C - Dzud measured as district-level temperature</i>								
Continuous intensity	0.003		0.003		-0.008		0.009	
	[0.492]		[0.605]		[0.338]		[0.166]	
Intensity > mean + 1 sd		-0.032		-0.034		-0.018		-0.074**
		[0.188]		[0.159]		[0.635]		[0.012]
Observations	1,152	1,152	827	827	393	393	434	434
R-squared, Panel A	0.162	0.163	0.166	0.169	0.183	0.186	0.273	0.276
R-squared, Panel B	0.164	0.164	0.171	0.170	0.183	0.184	0.280	0.279
R-squared, Panel C	0.162	0.163	0.166	0.167	0.184	0.183	0.271	0.276
Individual controls	YES	YES	YES	YES	YES	YES	YES	YES
Household controls	YES	YES	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES	YES	YES
Interview month FE	YES	YES	YES	YES	YES	YES	YES	YES
Province FE and province-specific trend	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table 3: The impact of the 2009/2010 dzud on mandatory school enrollment (household fixed effects), OLS

Dependent variable: 1=child is enrolled in school	Children from herding households, age 6-14		
	(1)	(2)	(3)
<i>Panel A - Dzud measured as district-level livestock losses</i>			
(Age 6-10) × (Continuous livestock losses)	-0.016* [0.094]		
(Age 6-10) × (Livestock losses > mean)		-0.119** [0.034]	
(Age 6-10) × (Livestock losses > mean + 1 sd)			-0.096 [0.224]
Age 6-10	0.019 [0.716]	0.075 [0.194]	0.041 [0.361]
<i>Panel B - Dzud measured as district-level snow depth</i>			
(Age 6-10) × (Continuous livestock losses)	-0.479 [0.360]		
(Age 6-10) × (Livestock losses > mean)		-0.044 [0.445]	
(Age 6-10) × (Livestock losses > mean + 1 sd)			-0.046 [0.470]
Age 6-10	0.018 [0.753]	0.035 [0.511]	0.026 [0.682]
<i>Panel C - Dzud measured as district-level temperature</i>			
(Age 6-10) × (Continuous livestock losses)	0.011 [0.319]		
(Age 6-10) × (Livestock losses > mean)		-0.020 [0.629]	
(Age 6-10) × (Livestock losses > mean + 1 sd)			-0.150* [0.085]
Age 6-10	0.016 [0.770]	0.028 [0.612]	0.035 [0.435]
<i>Panel D - Dzud measured as household-level percentage losses</i>			
(Age 6-10) × (Continuous livestock losses)	-0.170 [0.164]		
(Age 6-10) × (Livestock losses > mean)		-0.105* [0.052]	
(Age 6-10) × (Livestock losses > mean + 1 sd)			-0.059 [0.497]
Age 6-10	0.014 [0.801]	0.061 [0.246]	0.023 [0.702]
Observations	567	567	567
R-squared, Panel A	0.549	0.553	0.548
R-squared, Panel B	0.545	0.545	0.545
R-squared, Panel C	0.545	0.544	0.551
R-squared, Panel D	0.549	0.551	0.545
Individual controls	YES	YES	YES
Age FE	YES	YES	YES
Interview month FE, province FE and province-specific trend	NO	NO	NO
Household FE	YES	YES	YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table 4: The impact of the 2009/2010 dzud on mandatory school enrollment across panel waves, OLS

Dependent variable: 1=child is enrolled in school	Children from herding households, age 6-14		
	Wave 1	Wave 2	Wave 3
	(1)	(2)	(3)
<i>Dzud measured as district-level livestock losses</i>			
Intensity > mean + 1 sd	-0.042* [0.076]	-0.050** [0.038]	-0.048** [0.045]
Observations	781	781	781
R-squared	0.175	0.113	0.133
Individual controls	YES	YES	YES
Household controls	YES	YES	YES
Location controls	YES	YES	YES
Age FE	YES	YES	YES
Interview month FE	YES	YES	YES
Province FE and province-specific trend	YES	YES	YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table 5: The heterogeneity of the impact of the 2009/2010 dzud, OLS

Dependent variable: 1=child is enrolled in school	Children from herding households, age 6-14					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dzud measured as district-level livestock losses</i>						
Continuous intensity	-0.003 [0.624]	-0.013 [0.150]	-0.003 [0.410]	-0.002 [0.510]	-0.003 [0.417]	
(Continuous intensity) × (Female)	0.001 [0.882]					
(Continuous intensity) × (N. livestock 2009, 2 nd quintile)		0.009 [0.311]				
(Continuous intensity) × (N. livestock 2009, 3 rd quintile)		0.020* [0.096]				
(Continuous intensity) × (N. livestock 2009, 4 th quintile)		0.007 [0.463]				
(Continuous intensity) × (N. livestock 2009, 5 th quintile)		0.016 [0.214]				
(Continuous intensity) × (Mother's years of education)			0.003** [0.021]		0.003** [0.018]	
(Continuous intensity) × (Head's years of education)				0.000 [0.700]	-0.001 [0.406]	
<i>Dzud measured as district-level snow depth</i>						
Continuous intensity						-0.454* [0.073]
(Continuous intensity) × (Distance from district center)						-0.158* [0.062]
Distance from district center						0.005 [0.334]
Female	0.057*** [0.008]	0.054*** [0.009]	0.057*** [0.007]	0.057*** [0.006]	0.056*** [0.007]	0.058*** [0.007]
Mother's years of education	-0.005 [0.255]	-0.005 [0.237]	-0.005 [0.204]	-0.005 [0.226]	-0.004 [0.223]	-0.006 [0.132]
Head's years of education	0.008** [0.021]	0.008** [0.019]	0.007** [0.022]	0.008** [0.017]	0.007** [0.027]	0.008** [0.014]
N. livestock 2009, 2nd quintile	0.070** [0.010]	0.070*** [0.010]	0.073*** [0.006]	0.070** [0.011]	0.072*** [0.007]	0.091*** [0.001]
N. livestock 2009, 3rd quintile	0.035 [0.224]	0.044 [0.141]	0.041 [0.156]	0.037 [0.215]	0.038 [0.189]	0.065** [0.017]
N. livestock 2009, 4th quintile	0.027 [0.288]	0.026 [0.303]	0.031 [0.222]	0.027 [0.291]	0.029 [0.253]	0.045* [0.078]
N. livestock 2009, 5th quintile	0.017 [0.573]	0.017 [0.589]	0.023 [0.460]	0.019 [0.551]	0.020 [0.533]	0.033 [0.280]
Observations	827	827	827	827	827	794
R-squared	0.166	0.171	0.176	0.166	0.177	0.186
Individual controls	YES	YES	YES	YES	YES	YES
Household controls	YES	YES	YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES
Interview month FE	YES	YES	YES	YES	YES	YES
Province FE and province-specific trend	YES	YES	YES	YES	YES	YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table 6: The channels through which the 2009/2010 dzud impacted school enrollment, OLS

Herding households in which children age 6-14 live						
Dependent variables:	Owns livestock		Household income		Had many difficulties to pay education	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A - Dzud measured as district-level livestock losses</i>						
Continuous dzud intensity	-0.006 [0.134]		-0.018* [0.060]		0.001 [0.834]	
Dzud intensity > mean + 1 sd		0.033 [0.274]		-0.143** [0.022]		-0.043 [0.375]
<i>Panel B - Dzud measured as household-level percentage losses</i>						
Continuous dzud intensity	-0.371*** [0.000]		-0.339*** [0.001]		0.166*** [0.008]	
Dzud intensity > mean + 1 sd		-0.264*** [0.000]		-0.253*** [0.000]		0.113*** [0.007]
Observations	506	506	506	506	498	498
R-squared, Panel A	0.355	0.355	0.321	0.323	0.311	0.312
R-squared, Panel B	0.558	0.558	0.499	0.500	0.548	0.547
Household controls	YES	YES	YES	YES	YES	YES
Head's age FE	YES	YES	YES	YES	YES	YES
Interview month FE	YES	YES	YES	YES	YES	YES

Notes: Regressions in Panel A include province fixed effects and a province-specific trend, while regressions in Panel B include district fixed effects and a district-specific trend. In these estimations, the income variable has been rescaled to 10,000,000 Tugrug. Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table 7: The impact of the 1999-2002 series of dzuds on completion of mandatory education, OLS

Dependent variable: 1=completed mandatory education	Age 18-38			
	Full sample		Individuals having herding father in 1999	
	(1)	(2)	(3)	(4)
<i>Panel A - Dzud measured as district-level livestock losses</i>				
(Exposed cohort) × (Continuous dzud intensity)	-0.009 [0.605]		-0.018 [0.551]	
(Exposed cohort) × (Dzud intensity > mean + 1 sd)		-0.129 [0.124]		-0.265* [0.074]
Exposed	0.045 [0.643]	0.057 [0.561]	0.012 [0.933]	0.027 [0.850]
<i>Panel B - Dzud measured as district-level snow depth</i>				
(Exposed cohort) × (Continuous dzud intensity)	-4.041 [0.335]		-8.719 [0.172]	
(Exposed cohort) × (Dzud intensity > mean + 1 sd)		-0.158 [0.140]		-0.312* [0.077]
Exposed	0.041 [0.672]	0.081 [0.414]	-0.024 [0.869]	0.039 [0.785]
Observations	1867	1867	1005	1005
R-squared, Panel A	0.356	0.357	0.371	0.372
R-squared, Panel B	0.357	0.357	0.372	0.374
Individual controls	YES	YES	YES	YES
Household controls	NO	NO	NO	NO
Location controls	NO	NO	NO	NO
Age FE	YES	YES	YES	YES
Interview month FE	NO	NO	NO	NO
Province FE	NO	NO	NO	NO
Province-specific trend	NO	NO	NO	NO
District FE and district-specific trend	YES	YES	YES	YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table 8: The overall impact of dzuds on completion of mandatory education, OLS

Dependent variable: 1=Completed mandatory education		Individuals having herder father in 1999		
		Preschool age defined as 2-5	Preschool age defined as 1-5	Preschool age defined as 2-6
		(1)	(2)	(3)
<i>Dzud of 1999-2002, measured as district-level livestock losses</i>				
(Exposed in preschool) × (Dzud intensity > mean + 1 sd)	-0.595** [0.012]	-0.697*** [0.001]	-0.463* [0.052]	
(Exposed in primary school) × (Dzud intensity > mean + 1 sd)	-0.370* [0.097]	-0.455** [0.024]	-0.333 [0.154]	
(Exposed in secondary school) × (Dzud intensity > mean + 1 sd)	-0.248 [0.117]	-0.264 [0.104]	-0.231 [0.158]	
Exposed in preschool	0.022 [0.889]	0.046 [0.772]	-0.030 [0.841]	
Exposed in primary school	-0.026 [0.864]	-0.021 [0.889]	-0.027 [0.863]	
Exposed in secondary school	-0.053 [0.708]	-0.050 [0.724]	-0.056 [0.694]	
<i>Dzud of 2009/10, measured as district-level livestock losses</i>				
(Exposed) × (Dzud intensity > mean + 1 sd)	-0.077 [0.658]	-0.078 [0.654]	-0.069 [0.701]	
Exposed	0.003 [0.962]	0.029 [0.669]	0.005 [0.947]	
Observations	1219	1339	1219	
R-squared	0.356	0.344	0.354	
Individual controls	YES	YES	YES	
Household controls	YES	YES	YES	
Location controls	NO	NO	NO	
Age FE	YES	YES	YES	
Interview month FE	NO	NO	NO	
Province FE and province-specific trend	NO	NO	NO	
District FE and district-specific trend	YES	YES	YES	
p- value of adjusted Wald test: preschool interaction term = primary school interaction term	0.055	0.002	0.141	

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Appendix

Table A1: Descriptive statistics on pre-shock education, 2009/10 dzud

Sample	Individuals who reached age 15 before 2009
Outcome variable:	1=Completed basic education
<i>Panel A - Dzud measured as district-level livestock losses</i>	
Severely affected districts	0.63
Less affected districts	0.62
<i>Difference</i>	<i>0.01</i>
<i>Panel B - Dzud measured as district-level snow depth</i>	
Severely affected districts	0.60
Less affected districts	0.63
<i>Difference</i>	<i>-0.03</i>
<i>Panel C - Dzud measured as district-level temperature</i>	
Severely affected districts	0.57
Less affected districts	0.64
<i>Difference</i>	<i>-0.06</i>

Notes: Statistics are weighted to account for sampling design. None of the differences between affected and non-affected districts is statistically significant at conventional levels. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table A2: Placebo tests of the impact of the 2009/2010 dzud on mandatory school enrollment, OLS

Sample	Individuals who reached age 15 before 2009	Children from herding households, age 6-14
Dependent variables:	1=Completed basic education	1=Child is enrolled in school
	(1)	(2)
<i>Panel A - Dzud measured as district-level livestock losses</i>		
Dzud intensity 2010 > mean + 1 sd	0.015 [0.559]	
Dzud intensity 2008 > mean + 1 sd		0.016 [0.619]
<i>Panel B - Dzud measured as district-level snow depth</i>		
Dzud intensity 2010 > mean + 1 sd	0.001 [0.974]	
Dzud intensity 2008 > mean + 1 sd		-0.025 [0.205]
<i>Panel C - Dzud measured as district-level temperature</i>		
Dzud intensity 2010 > mean + 1 sd	-0.053 [0.274]	
Dzud intensity 2008 > mean + 1 sd		0.050** [0.046]
Observations	4023	827
R-squared, Panel A	0.258	0.166
R-squared, Panel B	0.258	0.167
R-squared, Panel C	0.259	0.169
Individual controls	YES	YES
Household controls	NO	YES
Location controls	YES	YES
Age FE	YES	YES
Interview month FE	NO	YES
Province FE and province-specific trend	YES	YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table A3: Descriptive statistics on pre-shock education, 1999-2002 series of dzuds

Sample	born 1984- 1970	born 1969- 1955	<i>Difference</i>
Outcome variable: 1= Completed basic education	(1)	(2)	(1)-(2)
<i>Panel A - Dzud measured as district-level livestock losses</i>			
Severely affected districts	0.73	0.67	0.06
Less affected districts	0.64	0.64	-0.01
<i>Difference</i>	0.09	0.03	0.06
<i>Panel B - Dzud measured as district-level snow depth</i>			
Severely affected districts	0.74	0.68	0.06
Less affected districts	0.62	0.64	-0.02
<i>Difference</i>	0.13*	0.05	0.08

Notes: Statistics are weighted to account for sampling design. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table A4: Placebo test on the impact of the 1999-2002 series of dzuds on the completion of mandatory education, OLS

Dependent variable: 1=completed mandatory education		Sample: Age 28-38, having herding father in 1999
<i>Panel A - Dzud measured as district-level livestock losses</i>		
(Exposed cohort) × (Dzud intensity > mean + 1 sd)		-0.255 [0.316]
Exposed		0.355** [0.034]
<i>Panel B - Dzud measured as district-level snow depth</i>		
(Exposed cohort) × (Dzud intensity > mean + 1 sd)		0.031 [0.885]
Exposed		0.296** [0.045]
Observations		442
R-squared, Panel A		0.504
R-squared, Panel B		0.503
Individual controls		YES
Household controls		NO
Location controls		NO
Age FE		YES
Interview month FE		NO
Province FE and province-specific trend		NO
District FE and district-specific trend		YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table A5: The impact of the 2009/2010 dzud on mandatory school enrollment (full set of controls shown), OLS

Dependent variable: 1=child is enrolled in school	Age 6-14					
	Full sample			Children from herding household		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>District-level dzud intensity measures</i>						
District-level livestock losses > mean + 1 sd	-0.027 [0.158]			-0.041* [0.070]		
District-level snow depth > mean + 1 sd		-0.034 [0.167]			-0.048** [0.039]	
District-level temperature > mean + 1 sd			-0.032 [0.188]			-0.034 [0.159]
<i>Individual controls</i>						
Female	0.038** [0.016]	0.037** [0.023]	0.038** [0.016]	0.058*** [0.005]	0.056*** [0.008]	0.058*** [0.005]
<i>Household controls</i>						
Mother's years of education	-0.004 [0.223]	-0.004 [0.213]	-0.004 [0.240]	-0.005 [0.248]	-0.005 [0.236]	-0.005 [0.247]
Household head's years of education	0.009*** [0.003]	0.009*** [0.004]	0.009*** [0.004]	0.008** [0.019]	0.008** [0.020]	0.008** [0.022]
Female household head	-0.057 [0.166]	-0.061 [0.147]	-0.056 [0.170]	-0.108* [0.063]	-0.116* [0.053]	-0.110* [0.062]
Household size	-0.001 [0.853]	-0.001 [0.924]	-0.002 [0.806]	-0.004 [0.646]	-0.004 [0.689]	-0.004 [0.623]
Number of children aged 0-5	0.011 [0.327]	0.010 [0.381]	0.011 [0.336]	0.012 [0.344]	0.011 [0.387]	0.012 [0.357]
Number of children aged 6-14	-0.002 [0.804]	-0.003 [0.730]	-0.003 [0.760]	0.000 [0.964]	-0.001 [0.912]	-0.001 [0.903]
Asset index, 2nd quintile	0.051*** [0.005]	0.051*** [0.005]	0.052*** [0.004]	0.058*** [0.002]	0.059*** [0.002]	0.061*** [0.001]
Asset index, 3rd quintile	0.067*** [0.003]	0.063*** [0.006]	0.069*** [0.003]	0.063** [0.010]	0.058** [0.019]	0.065*** [0.008]
Asset index, 4th quintile	0.056** [0.032]	0.051* [0.052]	0.058** [0.031]	0.054* [0.072]	0.049 [0.101]	0.059* [0.057]
Asset index, 5th quintile	0.078*** [0.003]	0.076*** [0.005]	0.079*** [0.004]	0.064** [0.042]	0.067** [0.043]	0.072** [0.025]
(N. livestock 2009, 2nd quintile) × (Herding 2009)	0.073*** [0.006]	0.076*** [0.005]	0.074*** [0.006]	0.067** [0.014]	0.072** [0.010]	0.069** [0.012]
(N. livestock 2009, 3rd quintile) × (Herding 2009)	0.045 [0.114]	0.050* [0.081]	0.046 [0.112]	0.035 [0.232]	0.041 [0.153]	0.036 [0.221]
(N. livestock 2009, 4th quintile) × (Herding 2009)	0.027 [0.279]	0.036 [0.154]	0.029 [0.250]	0.022 [0.373]	0.034 [0.172]	0.025 [0.332]
(N. livestock 2009, 5th quintile) × (Herding 2009)	0.016 [0.595]	0.022 [0.458]	0.016 [0.595]	0.016 [0.594]	0.024 [0.422]	0.016 [0.601]
Herding household 2009	-0.043** [0.043]	-0.041* [0.055]	-0.046** [0.035]			
Rural	0.019 [0.465]	0.025 [0.331]	0.033 [0.216]	0.015 [0.591]	0.029 [0.330]	0.035 [0.255]
District center	0.014 [0.525]	0.033 [0.128]	0.027 [0.183]	0.032 [0.241]	0.064** [0.022]	0.046* [0.081]

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Table A5, continued

Dependent variable: 1=child is enrolled in school	Age 6-14					
	Full sample			Children from herding household		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>District controls</i>						
Electricity available 24 hours per day	0.036* [0.094]	0.038* [0.074]	0.034 [0.109]	0.033 [0.190]	0.037 [0.141]	0.031 [0.218]
Cellphone network available in most/all areas of the district	0.014 [0.347]	0.017 [0.236]	0.013 [0.353]	0.007 [0.645]	0.012 [0.440]	0.008 [0.597]
Observations	1,152	1,152	1,152	827	827	827
R-squared	0.163	0.164	0.163	0.169	0.170	0.167
Individual controls	YES	YES	YES	YES	YES	YES
Household controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES
Interview month FE	YES	YES	YES	YES	YES	YES
Province FE and province-specific trend	YES	YES	YES	YES	YES	YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets. * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.

Table A6: The impact of the 2009/2010 dzud on mandatory school enrollment, OLS and penalized MLE

Dependent variable: 1=child is enrolled in school	Children from herding households, age 6-14					
	OLS with survey design effects (baseline)		OLS		penalized MLE	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A - Dzud measured as district-level livestock losses</i>						
Continuous dzud intensity	-0.002 [0.516]		0.001 [0.862]		-0.007 [0.930]	
Dzud intensity > mean + 1 sd		-0.041* [0.070]		-0.023 [0.298]		-0.458 [0.326]
<i>Panel B - Dzud measured as district-level snow depth</i>						
Continuous dzud intensity	-0.594** [0.021]		-0.610** [0.011]		-9.598* [0.054]	
Dzud intensity > mean + 1 sd		-0.048** [0.039]		-0.056** [0.018]		-0.911* [0.058]
<i>Panel C - Dzud measured as district-level temperature</i>						
Continuous dzud intensity	0.003 [0.605]		0.007 [0.421]		0.124 [0.634]	
Dzud intensity > mean + 1 sd		-0.034 [0.159]		-0.056* [0.057]		-1.084* [0.098]
Observations	827	827	827	827	827	827
R-squared, Panel A	0.166	0.169	0.130	0.131	-	-
R-squared, Panel B	0.171	0.170	0.137	0.136	-	-
R-squared, Panel C	0.166	0.167	0.131	0.134	-	-
Individual controls	YES	YES	YES	YES	YES	YES
Household controls	YES	YES	YES	YES	YES	YES
Location controls	YES	YES	YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES
Interview month FE	YES	YES	YES	YES	YES	YES
Province FE and province-specific trend	YES	YES	YES	YES	YES	YES

Notes: Regressions are weighted to account for sampling design. p-values in brackets (in columns 5 and 6, p-values are based on Likelihood Ratio tests). * p < 0.1, ** p < 0.05, *** p < 0.001. Sources: Coping with Shocks in Mongolia Household Panel Survey, Mongolia Livestock Census, and ERA-Interim dataset.