Helping Children Catch Up: Early Life Shocks and the PROGRESA Experiment*

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Abstract

Children who face significant disadvantage early in life are often found to be worse off years or even decades later. Can conditional cash transfer programs mitigate the negative consequences and help these children catch up with their peers? We answer this question using data from rural Mexico, where rainfall shocks can have substantial effects on household income. We find that adverse rainfall in a child's year of birth decreases grade attainment, post-secondary enrollment, and employment outcomes. But declines were much smaller for children whose families were randomised to receive the conditional cash transfer program, PROGRESA: each additional year of PROGRESA exposure during childhood mitigated almost 20 percent of the early disadvantage in grade attainment.

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Poor circumstance in early life – even when it is temporary – often has long-lasting negative impacts (Almond and Currie, 2011; Currie and Vogl, 2012; Heckman, 2006, 2007). What role can public policy play in lessening the burden of adverse events in a young child's life? This question is of core relevance to many areas of academic inquiry, and is critical in providing guidance on the allocation of scarce public resources. Much of the related work in economics focuses on evaluating the impacts of safety net policies that provide support to low-income children and families (Aizer et al., 2016; Chetty et al., 2016; Gertler et al., 2014; Hjort et al., 2017; Hoynes et al., 2016). This body of evidence shows that providing material and financial support during childhood can have positive impacts that last well into adulthood, often generating very large social returns (Bailey et al., 2020; Hendren and Sprung-Keyser, 2020).

We study a related but distinct question, for which the evidence thus far is quite limited (Almond et al., 2018). For children who have faced significant disadvantage or trauma early in life, are social protection programs capable of helping them catch up to their more fortunate peers? We focus specifically on conditional cash transfers (CCTs), a popular type of anti-poverty policy used widely across the globe. Our question, therefore, is essentially about heterogeneous returns to CCTs *within* the lower-income populations that are typically targeted by these policies. Do these programs have higher returns among children who have experienced early-life disadvantage compared to children with less exposure to early shocks? The answer to this question is important because it determines whether additional policies are needed above and beyond existing programs, targeting children who have experienced extreme disadvantage or trauma, in order to generate adequate catch-up.

Answering this question poses a substantial empirical challenge. First, we need a causal estimate of the effect of an early-life shock on later-life outcomes. This requires isolating variation in exposure to early life disadvantage that is orthogonal to other determinants of long-run outcomes. Second, in order to measure the extent to which a policy mitigates or exacerbates the effects of early-life disadvantage, we need to isolate exogenous variation in this policy. Because exposure to public programs is determined by parents' preferences and local access to resources, which could also determine long run outcomes, comparing the outcomes of two people who faced the same shock but were differentially exposed to public policies will likely produce a biased estimate of the remediation value of these programs.[1]

[1]As Almond and Mazumder (2013) put it in their review of the literature, resolving this identification problem

Our study attempts to overcome this challenge. We leverage the combination of a natural experiment that induced variation in the extent of early disadvantage and a large-scale cluster randomised controlled trial of CCTs in Mexico, documented to have increased educational attainment. In our study's agrarian setting, where weather plays a key role in determining household income (and thus the availability of nutrition and other inputs for children), we verify that adverse rainfall lowers the agricultural wage and affects physical health. We then show that Mexican youth born during periods of adverse rainfall – years in which rainfall levels were more than one standard deviation above or below the locality-specific mean – have worse educational attainment and employment outcomes later in life than those born in normal rainfall periods. Exposure to adverse rainfall in the year of one's birth – a crucial period for the determination of long-term health and human capital – decreased years of completed schooling by more than half a year.

However, for children whose households were randomised to receive CCTs through PROGRESA, Mexico's landmark experiment in anti-poverty policy, each additional year of exposure mitigated the long-term impact of rainfall shocks on educational attainment by 0.1 years, almost 20%. PROGRESA could in theory have had both income and substitution effects, but we argue that the impacts we detect are driven primarily by the latter. By reducing the effective cost of schooling, PROGRESA enabled all children to stay in school longer than they would have otherwise, but had the largest effects on those impacted by negative rainfall shocks at birth. The negative effects of adverse rainfall become discernible after primary school, with the largest impacts measured for completion of grades 7 through 9. The mitigative impact of PROGRESA, as well as the main effect of the program, is also largest precisely in these years.

Finally, for the oldest individuals in our sample, we find a similar pattern of coefficients in regressions on post-high-school education and employment outcomes. Adverse rainfall in the year of birth leads to a reduction of 17 percentage points in the probability of working, but each additional year of PROGRESA exposure offsets nearly 8 percentage points of this impact.

This set of facts constitutes our main contribution: with respect to schooling and early employment outcomes, children born in times of hardship are the ones most responsive to CCTs provided in their school-aged years. This implies that public investment can indeed help children who faced

"may be asking for 'lightning to strike' twice: two identification strategies affecting the same cohort but at adjacent developmental stages."

adversity in early life catch up to their peers. Given that children were exposed to PROGRESA during school-aged years, its success at generating catch-up for disadvantage from the year of birth is striking. Several influential studies argue that there is little scope for catch-up when it comes to nutritional deficiencies that occur before a child's second birthday (Martorell et al., 1994; Victora et al., 2008), or test score gaps that appear by early elementary school (Heckman, 2006). However, there is other work, consistent with our findings, documenting that catch-up on both physical and cognitive dimensions is still possible after age 2 (Crookston et al., 2010, 2013; Lundeen et al., 2014; Prentice et al., 2013).

A second important implication is that safety net policies geared toward low-income families in general may in effect target the neediest children within the targeted groups. Our results are similar to heterogeneous impacts found in recent evaluations of preschool policies in Germany (Cornelissen et al., 2018) and Denmark (Rossin-Slater and Wüst, 2020); from the Head Start program in the United States (Bitler et al., 2014); and from micronutrient supplementation in Bangladesh (Gunnsteinsson et al., 2021). Other studies find the opposite result or no evidence of significant interactions (Aguilar and Vicarelli, 2011; Duque et al., 2018; Johnson and Jackson, 2019; Malamud et al., 2016). This emphasises that differences across settings – types of policies, access to resources, socioeconomic environments, and intervention timing – may determine whether catch-up is possible.

Our empirical context is particularly appealing because of the relatively high potential for external validity. Adverse rainfall is one of the most common type of shocks experienced by poor households in much of the developing world (Dinkelman, 2017), and has large short- and long-term consequences (Maccini and Yang, 2009; Paxson, 1992; Shah and Steinberg, 2017; Wolpin, 1982). Given the rising importance of wide-scale CCT programs around the world – including those modeled closely after PROGRESA itself (see, e.g., Das et al. (2005); Lagarde et al. (2007)) – it is important to learn here that these programs, if administered as successfully as PROGRESA was in Mexico, could potentially mitigate a sizable portion of the adverse impacts of poor rainfall at the time of birth.

1 Program Background

1.1 Description of Program

In 1997, the Mexican government began a CCT program called the Programa de Educación, Salud y Alimentación (PROGRESA). The program provided cash transfers to poor families (mothers, specifically), conditional on certain education and health-related requirements. Since then, the program has been expanded to urban areas and renamed, first to *Oportunidades* in 2002 and to *Prospera* in 2014.

In this paper, we focus on the education component of PROGRESA, which consisted of bi-monthly cash payments to mothers during the school year, contingent on their children attending at least 85% of school days. Appendix Table D1 summarises the monthly grant amounts by gender and grade level for the second semester of 1997, 1998 and 2003. At the program's onset, grants were provided only for children between third and ninth grade. In 2001, the grants were extended to high school.

For evaluation purposes, the program was implemented experimentally in 506 rural localities (in 191 municipalities) in the states of Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San Luis de Potosi and Veracruz. 320 "treatment" localities were randomly assigned to start receiving benefits in the spring of 1998. 186 localities were kept as a control group and started receiving PROGRESA benefits at the end of 1999. This randomised variation has allowed for rigorous evaluations of the program's effects on a wide range of outcomes, which we discuss below. For more detail on PROGRESA's health component, program targeting, and eligibility, see Appendix section A.

1.2 Previous Literature on PROGRESA Effects

An enormous body of research has explored the effects of PROGRESA on a wide array of outcomes (Parker et al., 2017). In Appendix Table D2, we attempt to summarise the key findings of this literature, categorising studies based on the age of the analysis sample (specifically, how old they were during the years of PROGRESA being used to identify its effects) and by the main outcomes examined: education, health, cognitive or behavioural, or consumption-related.

It is clear that PROGRESA was successful at improving outcomes across all of these dimensions. For school-aged children, however, the main effect of PROGRESA was educational, which is not surprising: this age group was the only one directly affected by the schooling subsidies and was too old to benefit from the main health benefits targeted toward much younger children. Consistent with this, Table D2 shows that most of the effects that PROGRESA had on health were concentrated among much younger (or much older) samples.

The main question we seek to answer is whether a government policy like PROGRESA can help remediate for disadvantage generated very early in life. We are therefore interested in studying the outcomes of children who were school-aged when the program was rolled out, for whom there is experimental variation in exposure to the schooling grant. When interpreting our results, therefore, we view the education subsidy channel as the main driver of the results we document, not the health component or the actual cash received.[2] This is consistent with what has been documented in the literature – large education effects for school-aged children but virtually no evidence of health effects for this age group – and with the design of the program.

2 Data

2.1 PROGRESA Data

The data collected for the evaluation of the PROGRESA program include a baseline survey of all households in PROGRESA villages and several follow-ups in 1998, 1999, 2003, and 2007 (Government of Mexico, 2012). As we summarise in Table 1, we use the 2003 survey to obtain the outcome variables for our main analysis, and the baseline survey to construct control variables. For supplementary analysis, we also draw on both the 2003 and 2007 waves.

For our primary analysis, we focus on individuals aged 12 to 18 in 2003 in households who were eligible for the program ("poor" households). Following Behrman et al. (2011), we drop individuals who have non-matching genders across the 1997 and 2003 waves (1.9% of the sample), as well as those who report birth years that differ by more than 2 years (1.8% of the sample). For those with non-matching birth years with smaller than 2 year differences, we use the birth year reported in the 1997 wave. We restrict to the 12-18 age range because 12 year-olds are the youngest cohort for which there is differential exposure to PROGRESA in treatment and control villages (see Table 2),

[2] Though Appendix Table D2 shows that significant consumption effects have been documented, these are on the whole relatively small in magnitude (Parker et al., 2017).

Table 1: Variables and Survey Waves

Variables	Survey Year	Ages
A. Primary Outcomes		
Education	2003	12-18
Employment	2003	18
B. Control Variables		
Household demographics	1997	N/A
Locality characteristics	2003	N/A
C. Supplementary Outcomes		
Weight, Height	2003	2-6, 15-21
Weight, Height	2007	0-2, 8-10, adults 30+, mothers of young children
Behavioural	2007	8-10
Cognitive Tests	2003	2-6, 15-18

while individuals over 18 are more likely to have moved out of the household by the 2003 survey and are therefore not surveyed (though the main respondents are still asked some questions about non-residents).[3]

For our supporting analysis, we use other physical, cognitive, and behavioural outcomes collected during the 2003 survey for specific age groups (in most cases, different from our main sample of interest). We also use child development measures collected for younger children in 2007.[4]

2.1.1 Outcome Variables

Our main education variables include educational attainment (in grades attained), a dummy for grade progression, and a dummy for having completed the appropriate number of grades for one's age. "Grade progression" is a binary variable equal to 1 if an individual progressed at least five complete grades between 1997 and 2003. We also define an indicator for age-appropriate grade completion. This is equal to 1 if an individual completed the appropriate number of grades for their age (e.g., one grade for a 7 year old). We also generate 10 dummy variables, each indicating whether the individual completed at least a certain number of grades (from 3 to 12 grades) of

[3]As Figure D1 shows, the proportion of 19-year-olds not living in the household is over 40%, and this proportion continues to grow with age.

[4]Unfortunately, high attrition rates prevent us from using the 2007 outcomes of our sample individuals. We lose over half of our 2003 sample, partially due to household-level attrition, but primarily due to individual migration (no proxy information is collected for those no longer living in the originally surveyed household) – likely to be endogenous.

school.

For individuals who are 18 years old in 2003, we also look at continued enrollment and employment outcomes. Specifically, we create indicators for whether an individual is still enrolled in school after having received a high school degree. We are also interested in whether an individual was employed in the past week, employed in the past year, and employed in a non-labourer job in the past year. This last variable attempts to separate the jobs with the lowest earning growth potential from the rest of the employment categories by grouping those working as spot labourers with the unemployed. We verify using the Mexican Family Life Survey that youths who are 18 to 20 years old and working in a labourer job during the 2002 survey have among the lowest hourly wages during the 2009 survey (amounting to about two-thirds of the average of the rest of the sample).[5]

2.1.2 PROGRESA Exposure Variable

One of our main independent variables of interest is years of PROGRESA exposure. The length of exposure to the education component of the PROGRESA program depends on a child's locality and birth year. Table 2 shows, for each birth cohort, the number of years of exposure to PROGRESA by treatment status. This variable takes advantage of the variation in exposure lengths across different age cohorts within the treatment and control groups, in addition to the exogenous variation generated by the randomization of the PROGRESA program.

2.2 Rainfall Data

In addition to PROGRESA data, we use rainfall data from local weather stations collected by Mexico's National Meteorological Service (CONAGUA, 2013). We match those rainfall stations to program localities using their geocodes. For each locality, we use data from all stations within a 20 kilometer radius and take an inverse-distance weighted average of rainfall from these nearby stations. Using this procedure, 69 of the 506 localities are still missing rainfall measurements for our study period. Thus, our final sample, after excluding individuals missing rainfall for their particular year of birth, restricting to those from poor households in our desired age group meeting the data quality requirements, consists of individuals from 420 localities.

[5]Job categories differ across the two datasets, but the labourer category is similarly defined.

Table 2: Exposure to PROGRESA

Cohort			*Age (year) when first exposed to PROGRESA*		*Number of years exposed to PROGRESA by 2003*		
Age in 1998	School Grade in 1998	Age in 2003	Treatment Villages	Control Villages	Treatment Villages	Control Villages	Difference in Exposure
5	-	10	8 (2001)	8 (2001)	3	3	0
6	1st year primary	11	8 (2000)	8 (2000)	4	4	0
7	2nd year primary	12	8 (1999)	9 (2000)	5	4	1
8	3rd year primary	13	8(1998)	10 (2000)	6	4	2
9	4th year primary	14	9 (1998)	11 (2000)	6	4	2
10	5th year primary	15	10 (1998)	12 (2000)	6	4	2
11	6th year primary	16	11 (1998)	13 (2000)	6	4	2
12	1st year junior high	17	12 (1998)	14 (2000)	6	4	2
13	2nd year junior high	18	13 (1998)	16 (2001)	4	2	2
14	3rd year junior high	19	14 (1998)	17 (2001)	2	1	1
15	1st year high school	20	-	-	0	0	0
16	2nd year high school	21	-	-	0	0	0

Notes:
- Initially, PROGRESA only applied to primary and junior high school. In 2001, the program was extended to all three years of high school. The control cohort aged 14 in 1998 aged out of the program at the end of 1999 and started receiving benefits again in 2001.
- Years of exposure is obtained by calculating the number of months, dividing by 12, and rounding to the nearest year (because there is some ambiguity about the precise month in which treatment households began receiving benefits (Skoufias, 2005; Hoddinott and Skoufias, 2004)).

2.2.1 Rainfall Shock Variable

We use rainfall as an exogenous shock to income during a child's first year of life. Specifically, we define a shock as a level of annual rainfall that is one standard deviation above or below the locality-specific mean (calculated over the 10 years prior to the birth year). We use this relative measure in order to capture the fact that the same amount of rainfall may have different consequences for different regions with different average rainfall levels. As we discuss in detail in section 3, both previous literature as well as our own data show that defining the shock variable in this way captures the contemporaneous relationship between rainfall and agricultural wages: normal rainfall is associated with better outcomes than extreme rainfall.

We use rainfall in an individual's calendar year of birth in their locality of residence in 1997.[6] To calculate rainfall levels, we simply sum all monthly rainfall during an individual's calendar year of birth. We do not use month of birth to define this annual shock because approximately 30% of our sample reports different birth months in the 1997 and 2003 surveys. Appendix Figure

[6] The data do not include locality of birth, which would be the ideal geographic identifier in this context. We therefore use locality of residence (as of 1997), which should be accurate for most of the individuals in our sample, as long as migration among these young age groups is uncommon.

D2 maps all PROGRESA localities by their rainfall status, separately for treatment and control.[7] Black dots represent localities that experienced a rainfall shock in 1987 (chosen, for illustrative purposes, because this is the modal birth year in our sample), while gray crosses represent those that experienced normal rainfall in that same year. For both treatment and control villages, we see a great deal of variation in rainfall shock status within states, and even within clusters of neighboring localities.

2.3 Summary Statistics

Table 3 reports summary statistics for individual-level variables from the 2003 survey for our sample of interest: individuals aged 12 to 18 (and for employment outcomes, only those aged 18) who live in households eligible for PROGRESA and satisfy the data quality requirements described in section 2.1. Panel A shows all education measures are significantly higher for treatment than control villages, while panel B shows employment outcomes for 18 year olds do not differ by treatment status on average.

Panel C reports summary statistics for the two independent variables of interest: PROGRESA exposure and birth year rainfall, which vary at the locality by birth year level. By experimental design, treatment villages were exposed to PROGRESA for longer than control villages. Mean rainfall, both in raw levels and in normalised terms, is not significantly different across treatment and control villages.

However, there is a small but statistically significant difference in the prevalence of a one-standard deviation shock. Since PROGRESA treatment was randomly allocated and rainfall shocks are arguably exogenous, this difference in the prevalence of a shock does not necessarily indicate an identification issue. However, this imbalance could be problematic if it resulted from a lack of common support across the treatment and control rainfall distributions. Accordingly, we verify in Appendix Figure D3 that the rainfall distributions for treatment and control localities indeed share a common support and are similar overall. Moreover, in Appendix Figure D2, though there are more shocks in control villages than treatment villages, the spatial distribution of rainfall shocks is similar across the two groups (and both quite disperse).

Nevertheless, in order to alleviate concerns that this imbalance is driving our results, we also

[7] We use shape files for Mexico from GADM (2009).

Table 3: Summary Statistics

	Full Sample	*Treatment Villages*	*Control Villages*	*Treatment - Control Differences*
A. Individual Outcomes (12-18 year olds)				
Educational Attainment	6.79	6.85	6.69	0.15***
	(2.11)	(2.09)	(2.13)	(0.040)
Grade Progression	0.58	0.59	0.56	0.030***
	(0.49)	(0.49)	(0.50)	(0.0096)
Appropriate Grade Completion	0.46	0.48	0.44	0.037***
	(0.50)	(0.50)	(0.50)	(0.0094)
Number of individuals	11829	7193	4636	
Number of localities	420	257	163	
B. Individual Outcomes (18 year olds)				
Currently Enrolled w/ HS Degree	0.061	0.058	0.064	-0.0057
	(0.24)	(0.23)	(0.25)	(0.012)
Worked this Week	0.50	0.51	0.48	0.029
	(0.50)	(0.50)	(0.50)	(0.030)
Worked this Year	0.53	0.54	0.52	0.028
	(0.50)	(0.50)	(0.50)	(0.030)
Worked in Non-Labourer Job	0.35	0.36	0.35	0.0051
	(0.48)	(0.48)	(0.48)	(0.029)
Number of individuals	1597	942	655	
Number of localities	368	218	150	
C. Shock Variables (Full Sample)				
Years of PROGRESA exposure	4.84	5.57	3.69	1.88***
	(1.17)	(0.73)	(0.72)	(0.030)
Annual rainfall	1182.4	1180.6	1185.3	-4.75
	(644.3)	(654.8)	(628.0)	(26.3)
Normalised rainfall	-0.070	-0.054	-0.096	0.042
	(0.81)	(0.79)	(0.84)	(0.033)
Rainfall Shock	0.24	0.22	0.27	-0.048***
	(0.43)	(0.42)	(0.45)	(0.017)
Number of locality x birth-year observations	2519	1536	983	
Number of localities	420	257	163	
D. Shock Variables (Trimmed Sample)				
Years of PROGRESA exposure	4.81	5.58	3.71	1.87***
	(1.17)	(0.72)	(0.71)	(0.031)
Annual rainfall	1181.1	1171.1	1195.5	-24.4
	(644.0)	(654.8)	(628.0)	(28.1)
Normalised rainfall	-0.067	-0.051	-0.089	0.038
	(0.84)	(0.83)	(0.86)	(0.037)
Rainfall Shock	0.28	0.27	0.29	-0.028
	(0.45)	(0.44)	(0.46)	(0.020)
Number of locality x birth-year observations	2170	1282	888	
Number of localities	344	203	141	

Notes:
Standard deviations (in the first 3 columns) and standard errors (in the last column) in parentheses (*** p<0.01, ** p<0.05, * p<0.1). We do not cluster standard errors in these summary statistics but cluster at the municipality-level in all main results.

trim the sample by excluding localities that could be considered outliers. That is, we drop any localities that either experienced no rainfall shocks throughout the sample period or experienced rainfall shocks in every year throughout the period. As shown in Panel D of Table 3, this trimming results in a sample of balanced rainfall shocks across treatment and control. Appendix Figure D4, which maps the geographic distribution of shocks for this trimmed sample, is not noticeably different from Appendix Figure D2, emphasising that trimming did not substantially change the distribution of rainfall shocks (by removing localities only from a particular area, for example). In the appendix, we repeat our main empirical analysis using the trimmed sample and show that our results remain nearly unchanged (Table C5).

3 Empirical Strategy

To investigate whether PROGRESA can help generate catch-up for children who experienced early-life disadvantage, we need exogenous variation in early-life disadvantage as well as exogenous variation in exposure to PROGRESA. The randomised rollout of PROGRESA provides the latter. For the former, we turn to variation generated by rainfall shocks.

3.1 Early-Life Rainfall Shocks

In rural settings, good rainfall in early childhood means higher income, which may translate into increased nutritional availability during a crucial stage of development. Children exposed to negative rainfall shocks early in life often remain disadvantaged many years later, in terms of their health, human capital, and labour market outcomes (Dinkelman, 2017; Maccini and Yang, 2009; Shah and Steinberg, 2017).

Drawing on previous literature, as well as new analyses using our data, we argue that negative rainfall shocks do indeed generate substantial disadvantage in this setting. First, studying the same PROGRESA villages that we study in this paper, Bobonis (2009) finds that rainfall shocks, defined as monthly rainfall one standard deviation above or below the historical mean, reduce household expenditures by 16.7%. Next, using locality-level wages reported by village leaders in the PROGRESA data, we find evidence consistent with this. Appendix Figure D5 depicts the lowess-smoothed relationship between average male wages from the 2003 survey and rainfall in that same year, normalised using the locality-specific 10-year historical mean and standard deviation. The inverted U-shape, which peaks at around zero, shows that wages are highest around the locality mean but fall at the tails of the rainfall distribution.

We also provide evidence that rainfall shocks affect nutrition, by examining effects on BMI. As we show in Table B1 and discuss in more detail in Appendix section B, contemporaneous rainfall shocks reduce BMI. In the same table, we show that these contemporaneous nutrition effects have longer-term implications for child health. Shifting attention to rainfall shocks in the year of birth (instead of the survey year), we find that adverse rainfall increases stunting for children aged 2 and older – by 4.2 percentage points (about 20% of the mean) for those aged 2-6, and 3.7 percentage points (about 40% of the mean) for those aged 8-10 at the time of survey.[8]

Finally, we also examine whether other dimensions of human capital are affected by birth-year rainfall, focusing on cognitive test scores and behavioural measures collected in 2003. We find that adverse birth-year rainfall had no significant effects on cognitive or behavioural measures for 2 to 6 year-olds, but did increase the likelihood of behavioural problems (externalising problems, in particular) later in childhood. That income shocks in the year of birth can affect non-cognitive development is consistent with the child development literature, which documents that socioeconomic disadvantage is associated with altered maternal responses to infant emotions (Kim et al., 2017) and with other reasons for negative mother-infant interactions that could lead to behavioural problems later in childhood (Goyal et al., 2010).[9]

In sum, exposure to adverse rainfall early in life has substantial effects on household resources, nutrition, and health in our setting. In our analysis, we use adverse rainfall as a proxy for early-life disadvantage, noting that household income at the time of birth is not available in our data (and would be generally di□icult to obtain in most settings). Even if this variable were available, however, the exogeneity of the rainfall shock provides an important advantage because it enables us to obtain

[8]To put these magnitudes into perspective, these increases in stunting correspond to average reductions in height-for-age z-scores of about 0.09 and 0.03 standard deviations for 2-6 year-olds and 8-10 year olds, respectively. For comparison, an additional month of exposure to civil war in Burundi led to a 0.05 standard deviation decrease in height-for-age z-scores (Bundervoet et al., 2009); in Colombia, a one standard deviation increase in early-life exposure to violence reduced height-for-age z-scores by 0.16 standard deviations (Duque, 2017); survivors who were infants during the 1984 Ethiopian famine were 5 centimeters (almost half of the sample standard deviation) shorter than unaffected individuals by young adulthood (Dercon and Porter, 2014).

[9]Because the samples used in Tables B1 and B2 were all exposed to the PROGRESA program by the time of survey (2003 or 2007), and the young cohorts in particular (both treatment and control) were exposed to the health component of the program, the estimated effects could be underestimating the main effect of adverse rainfall if PROGRESA had any remediating effect on these health outcomes.

a causal estimate of the effect of early-life disadvantage (and therefore a valid estimate of the amount of catch-up generated by PROGRESA). Because household income at the time of birth could be strongly correlated with household conditions later in life (during exposure to the PROGRESA program), using an exogenous rainfall shock also helps ensure that we are isolating catch-up based on early-life disadvantage rather than current circumstances. Programs like PROGRESA already target recipients based on current income levels – the goal of this paper is to investigate whether these programs help those who experienced additional disadvantage (early in life) catch up to other program recipients.

3.2 Specification

Letting z_{islt} denote education or employment outcomes for individual i, born in year t and living in state s and locality l in 1997, we estimate the regression specification below. See Appendix section E for details on how this estimating equation relates to the structural parameters of a life-cycle utility model of schooling choices, endowments, and conditional transfers.

$$z_{islt} = \beta_1 R_{slt} + \beta_2 P_{slt} + \beta_3 R_{slt} P_{slt} + \alpha' X_{islt} + \mu_s \text{ x } \delta_t + \epsilon_{islt}. \tag{1}$$

R_{slt} represents the rainfall shock dummy, indicating that rainfall during the individual's year of birth was more than one standard deviation away from the ten-year locality-specific mean. P_{slt} represents the number of years of PROGRESA exposure, which varies across treatment and control villages as well as across different birth cohorts within villages.

Our basic specification includes state x birth year fixed effects (μ_s x δ_t). In some specifications we add municipality (an administrative region larger than locality but smaller than state) fixed effects. Given that R_{slt} and P_{slt} both vary at the locality and birth year level, we could technically also include locality fixed effects, though these would absorb all of the variation generated by the PROGRESA randomization, the primary source of exogenous variation in this design. Therefore, municipality fixed effects are the smallest set of geographic fixed effects that we use.

β_1 represents the causal effect of a negative early-life income shock, and β_2 provides the causal effect of PROGRESA for individuals who did not experience this negative shock. β_3 provides the differential effect of PROGRESA for disadvantaged individuals (who experienced the negative shock). A positive β_3 would indicate catch-up: larger effects of PROGRESA for the more disadvantaged individuals; a negative β_3 would suggest that PROGRESA widens the gap between disadvantaged and non-disadvantaged children.

We cluster our standard errors at the municipality level, which is a larger administrative unit than the locality. In addition to this, we also show standard errors that adjust for spatial correlation (unrelated to administrative boundaries) using the method described in Conley (1999). We report standard errors that allow for dependence up to 100km and 500km.

In keeping with previous work on PROGRESA (Behrman et al., 2011; Schultz, 2004; Skoufias and Parker, 2001), we include a rich set of controls in order to obtain more precise estimates of the treatment effects and account for some significant differences across treatment and control villages that exist despite the randomization (see Table D3). All of our specifications include controls for individual gender, household size, household head age, household head gender, a set of household composition variables (specified in Table D3), as well as locality controls for water source type, garbage disposal methods, the existence of a public phone, hospital or health center, and a DICONSA store (nutritional supplement distributor) in the locality.

3.3 Exogeneity of Rainfall and PROGRESA

PROGRESA exposure, the rainfall shock variable, and their interaction form the basis of our empirical specification. To provide support for the exogeneity of these variables, we check whether individuals are observably different across PROGRESA treatment and control villages, as well as rainfall shock versus normal rainfall groups. In Appendix Table D4, we regress each of the individual, household, and village-level characteristics that we use as control variables on a PROGRESA treatment village dummy, the rainfall shock, and their interaction. The vast majority of coefficients are statistically insignificant and/or small in magnitude relative to the means.[10]

Finally, we note some important considerations with respect to the interpretation of the rainfall shock coefficient (β_1). This coefficient provides the reduced-form effect of an early-life income shock on child outcomes in 2003. This includes any direct, biological effect the shock may have

[10]One exception is age, but as we discuss in section C.5 and show in Table C7, these age imbalances do not appear to be driving our main results.

on a child's health and human capital, in addition to any changes resulting from compensating or reinforcing investments that parents may make in response to the shock. Similarly, the coefficient on the interaction term (β_3) indicates whether there is any heterogeneity in the effect of PROGRESA with respect to this reduced-form shock – that is, how the effect of PROGRESA differed for children who experienced an income shock early in life along with any behavioural responses that resulted from this shock. Because children who experience income shocks do not experience these shocks in isolation, we argue this is a policy-relevant parameter of interest.

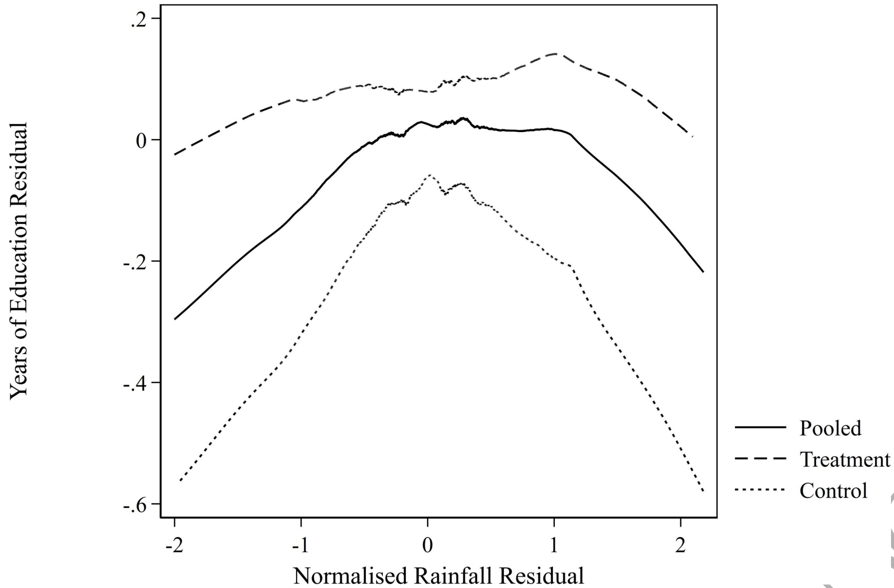
That said, in order to inform the generalisability of our findings, it would be useful to know whether parents are indeed responding to these early-life income shocks in ways that could in turn influence the effectiveness of the PROGRESA program. For example, parents could adjust their labour supply if they have a child who is less healthy due to an early-life income shock. They could also reallocate resources across siblings. In Table D5, we find no evidence of this. We regress indicators for parental employment, days worked by each parent, and hours worked by each parent in the baseline survey on the child-specific rainfall shock variable of interest (R_{slt}). We also examine average educational attainment among siblings, as well as average grade completion among siblings (which better adjusts for age) from the baseline survey, and find no significant differences across children who experienced and did not experience a rainfall shock at birth. In sum, parental responses to at-birth rainfall shocks are not large, at least in these dimensions we are able to observe.

4 Results

4.1 Education Results

Figure 1 illustrates the intuition underlying our identification strategy, using lowess smoothing to depict the non-monotonic relationship between rainfall at birth and educational attainment. We first regress educational attainment and normalised rainfall on our full set of controls (state-by-birth year fixed effects, and all household and locality-level controls described in Section 3). We then plot non-parametrically the relationship between the educational attainment residuals on the y axis and the normalised rainfall residuals on the x axis, separately for treatment and control villages.

Figure 1: Years of Educational Attainment by Birth-Year Rainfall



Notes:
All three lines represent the lowess-smoothed educational attainment residuals for the relevant group. Educational attainment and normalised rainfall residuals are calculated after regressing each variable on state by birth-year fixed effects and the control variables described in section 3. Normalised rainfall residuals are trimmed at the 5th and 95th percentiles.

The dotted control group line has an inverted U- shape, which reinforces the idea that extreme deviations from mean rainfall are harmful for children. In addition, the treatment line is above the control line across the entire range of rainfall deviations, which is consistent with our summary statistics and previous work on PROGRESA. Finally, the distance between the treatment and control lines is smallest around a normalised rainfall deviation of zero and grows larger in the tails, indicating that PROGRESA exposure mitigates the impacts of extreme rainfall at birth on educational attainment.

Table 4 reports parametric regression estimates analogous to the graphical analysis above. The first three columns show the regression results from our base specification (1), which includes state-by-year fixed effects and household and locality controls. For each coefficient of interest, we report three standard errors: first, clustered at the municipality level; second, allowing for spatial correlation using a 100km cutoff; and third, allowing for spatial correlation using a 500km cutoff.

For educational attainment in column 1, the main effect of PROGRESA is positive, the main effect of a rainfall shock is negative, and the interaction is positive; all are statistically significant at varying conventional levels. Since our sample includes children who may not have completed their

Table 4: Effects of PROGRESA and Birth-Year Rainfall on Education Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Educational Attainment	Grade Progression	Appropriate Grade Completion
Years of PROGRESA Exposure	**0.10**	**0.010**	**0.013**	**0.015**	**-0.013**	**-0.011**
	(0.038)***	(0.0097)	(0.0076)	(0.047)	(0.012)	(0.011)
	[0.030]***	[0.0069]	[0.0070]*	[0.035]	[0.0087]	[0.0082]
	{0.022}***	{0.0058}*	{0.0068}*	{0.034}	{0.0077}*	{0.0092}
Rainfall Shock	**-0.65**	**-0.11**	**-0.12**	**-0.70**	**-0.12**	**-0.14**
	(0.28)**	(0.056)**	(0.051)**	(0.27)***	(0.057)**	(0.054)***
	[0.27]**	[0.058]*	[0.049]**	[0.23]***	[0.048]**	[0.048]***
	{0.34}*	{0.065}*	{0.047}**	{0.25}***	{0.046}**	{0.043}***
Rainfall Shock x Exposure	**0.11**	**0.020**	**0.019**	**0.13**	**0.024**	**0.025**
	(0.053)**	(0.011)*	(0.010)*	(0.051)**	(0.011)**	(0.011)**
	[0.053]**	[0.012]*	[0.010]*	[0.044]***	[0.0095]**	[0.0096]***
	{0.062}*	{0.013}	{0.0091}**	{0.045}***	{0.0086}***	{0.0081}***
Observations	11824	11216	11824	11824	11216	11824
Mean of Dependent Variable	6.79	0.58	0.46	6.79	0.58	0.46
Sample Ages (in 2003)	12 to 18					
Fixed Effects	Birth year x state			Birth year x state, Municipality		

Notes:
- Standard errors clustered at the municipality are reported in parentheses, Conley standard errors using a 100km cutoff are reported in square brackets, and Conley standard errors using a 500km cutoff are reported in curly brackets. (*** p<0.01, ** p<0.05, * p<0.1).
-"Rainfall Shock" = 1 for individuals whose birth-year rainfall was more than one standard deviation from the 10-year historical locality-specific mean.
-All specifications include gender, household head gender and age, household size, household composition variables, parental education, parental language, and locality characteristics. Controls for parental language/education and locality distance include dummies for missing values.

schooling yet, we also look at the two other variables that adjust for age: similar patterns hold for grade progression and appropriate grade completion. In columns 2 and 3, the rainfall shock coe□icients are negative and statistically significant; the main effects of PROGRESA are small and only marginally significant, which means that PROGRESA had little effect on those who were not exposed to adverse rainfall. Importantly, however, the sums of the PROGRESA coe□icient and the interaction term are positive and statistically significant, indicating large PROGRESA effects for those exposed to early-life disadvantage.

These coe□icient estimates imply PROGRESA had larger effects on those disadvantaged at birth. For those who were not exposed to adverse rainfall, PROGRESA increased educational attainment by 0.11 years, but it increased educational attainment for those exposed to adverse rainfall by 0.22 years. Put differently, PROGRESA was able to generate substantial catch-up for individuals exposed to adverse rainfall. A negative rainfall shock decreased educational attainment by 0.65 years. However, one year of PROGRESA exposure mitigated this reduction by 0.11 years. At 2 years of exposure – the average difference between treatment and control exposure – the program mitigated 35% of the disadvantage caused by the rainfall shock at birth in years of completed schooling. For grade progression and appropriate grade completion, the figures are similarly high: 37% and 32%, respectively (all percentages calculated using the results in columns 1 to 3).

In the specification with municipality fixed effects (columns 4 to 6), the pattern of the results is the same: PROGRESA reduces the disadvantage generated by early-life rainfall. The effects of PROGRESA for both groups (i.e., the main PROGRESA coe□icient and the sum of the coe□icient and interaction) are close to zero, likely due to lack of variation in treatment and control status within municipalities. Although municipality fixed effects help control for location-specific unobservables on a finer level than state, the fact that over half of the municipalities consisted of either all treatment or all control villages reduces the amount of variation we can exploit. For this reason, we focus on the baseline specification for the remainder of the paper.

Table 5 examines schooling completion by grade. We create separate dummy variables for the completion of 3 to 12 grades of school and estimate specification 1 using these dummies as the dependent variables. In each column, we restrict the sample to individuals old enough to have completed the number of grades used in the dependent variable.

The impact of PROGRESA on completing grades 3 to 10 is positive and significant. The main

Table 5: Effects of PROGRESA and Birth-Year Rainfall on Schooling Completion by Grade

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Primary School				*Junior High School*			*High School*		
	3 grades	4 grades	5 grades	6 grades	7 grades	8 grades	9 grades	10 grades	11 grades	12 grades
Years of PROGRESA Exposure	**0.0043**	**0.0095**	**0.013**	**0.018**	**0.023**	**0.014**	**0.019**	**0.0090**	**-0.00032**	**-0.0038**
	(0.0027)	(0.0035)***	(0.0048)***	(0.0058)***	(0.011)**	(0.011)	(0.011)*	(0.0065)	(0.0060)	(0.0076)
	[0.0019]**	[0.0026]***	[0.0034]***	[0.0040]***	[0.0080]***	[0.0087]*	[0.010]*	[0.0065]	[0.0068]	[0.0098]
	{0.0015}***	{0.0017}***	{0.0027}***	{0.0034}***	{0.0063}***	{0.0083}*	{0.0071}***	{0.0045}**	{0.0039}	{0.0061}
Rainfall Shock	**0.012**	**-0.0090**	**-0.031**	**-0.036**	**-0.20**	**-0.23**	**-0.25**	**-0.065**	**-0.072**	**-0.10**
	(0.020)	(0.028)	(0.038)	(0.047)	(0.070)***	(0.072)***	(0.083)***	(0.052)	(0.037)*	(0.054)*
	[0.019]	[0.028]	[0.034]	[0.042]	[0.064]***	[0.069]***	[0.076]***	[0.044]	[0.035]**	[0.056]*
	{0.021}	{0.034}	{0.035}	{0.048}	{0.064}***	{0.067}***	{0.089}***	{0.042}	{0.029}**	{0.053}*
Rainfall Shock x Exposure	**-0.0020**	**0.0025**	**0.0052**	**0.0047**	**0.032**	**0.040**	**0.046**	**0.010**	**0.0059**	**0.018**
	(0.0040)	(0.0054)	(0.0072)	(0.0090)	(0.014)**	(0.013)***	(0.016)***	(0.011)	(0.0071)	(0.017)
	[0.0037]	[0.0053]	[0.0066]	[0.0080]	[0.013]**	[0.014]***	[0.015]***	[0.0096]	[0.0078]	[0.018]
	{0.0040}	{0.0061}	{0.0067}	{0.0087}	{0.012}***	{0.013}***	{0.017}***	{0.0086}	{0.0055}	{0.017}
Observations	11824	11824	11824	11824	10068	8285	6618	5002	3231	1592
Mean of Dependent Variable	0.97	0.93	0.88	0.78	0.56	0.52	0.45	0.14	0.097	0.058
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	16 to 18	17 to 18	18
Fixed Effects	Birth year x state									

Notes:

- Standard errors clustered at the municipality are reported in parentheses, Conley standard errors using a 100km cutoff are reported in square brackets, and Conley standard errors using a 500km cutoff are reported in curly brackets. (*** p<0.01, ** p<0.05, * p<0.1).

-"Rainfall Shock" = 1 for individuals whose birth-year rainfall was more than one standard deviation from the 10-year historical locality-specific mean.

-All specifications include gender, household head gender and age, household size, household composition variables, parental education, parental language, and locality characteristics. Controls for parental language/education and locality distance include dummies for missing values.

effect of the rainfall shock is negative and significant starting in 7th grade. For grades below this, early life disadvantage does not seem to drive grade completion, possibly because the vast majority of our 12-18 year old sample have completed grades 3 (97%) to 6 (78%). Also starting in 7th grade (and until 9th grade), there are significant positive interaction coefficients. As in Table 4, these interaction terms are at least as large as the main effects of PROGRESA, implying PROGRESA effects that are double the size for those who experienced adverse early life rainfall compared to those who did not.

Another important related outcome is cognitive ability, but as we explain in Appendix section C.1, the evidence we have on this is somewhat inconclusive. Our regressions reveal no significant effects of PROGRESA, rainfall, or their interaction on Woodcock-Johnson language and math scores for the sub-sample that was tested.

4.2 Employment Outcomes

We are also interested in whether rainfall shocks and PROGRESA exposure have similar effects on longer-run labour market outcomes that are not directly tied to the PROGRESA cash incentive. Unfortunately, much of our sample is too young for us to study impacts on their employment outcomes, but the oldest cohort – who were 18 at the time of the 2003 survey – were just old enough to be graduating from high school and pursuing either further education or formal employment. About 30% of the 18-year-olds in the 2003 survey were no longer living at home (see Appendix Figure D1) and therefore missing detailed employment information, but as we show in column 4 of Appendix Table C3, the likelihood of a missing employment variable in this sample is not driven by PROGRESA, rainfall, or their interaction.[11] In Table 6, we report the results of regressions on variables related to continuing education and employment after high school for this 18-year-old sample.

Our first dependent variable of interest is the continuation of education after high school: this is an indicator equal to 1 if an individual is enrolled in school (including college or vocational training) and has already completed 12 grades of school. In columns 2 and 3, we create dummies for employment in the week of survey and in the past year. Column 4 attempts to separate those

[11]The fraction living outside of the household grows even higher after age 18, which is why we do not examine those older than 18 in 2003.

Table 6: Effects of PROGRESA and Birth-Year Rainfall on Longer-Term Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Currently Enrolled w/ HS Degree	Worked this Week	Worked this Year	Worked in Non-Labourer Job	Enrolled or Currently Working	Enrolled or Worked this Year	Enrolled or Worked in Non-Labourer Job
Years of PROGRESA Exposure	**-0.0049**	**0.0010**	**0.0031**	**-0.014**	**0.0019**	**0.0034**	**-0.014**
	(0.0078)	(0.013)	(0.012)	(0.016)	(0.013)	(0.012)	(0.015)
	[0.0096]	[0.013]	[0.012]	[0.012]	[0.014]	[0.012]	[0.014]
	{0.0054}	{0.0064}	{0.0066}	{0.0088}	{0.0066}	{0.0068}	{0.0095}
Rainfall Shock	**-0.10**	**-0.21**	**-0.17**	**-0.22**	**-0.21**	**-0.21**	**-0.26**
	(0.053)*	(0.15)	(0.13)	(0.13)*	(0.15)	(0.14)	(0.13)*
	[0.053]*	[0.15]	[0.10]*	[0.096]**	[0.16]	[0.11]*	[0.10]**
	{0.047}**	{0.17}	{0.059}***	{0.072}***	{0.19}	{0.085}**	{0.063}***
Rainfall Shock x Exposure	**0.017**	**0.087**	**0.077**	**0.099**	**0.079**	**0.078**	**0.100**
	(0.017)	(0.044)**	(0.039)*	(0.040)**	(0.046)*	(0.044)*	(0.042)**
	[0.017]	[0.043]**	[0.030]**	[0.031]***	[0.049]	[0.037]**	[0.037]***
	{0.016}	{0.046}*	{0.018}***	{0.023}***	{0.055}	{0.031}**	{0.025}***
Observations	1597	1147	1143	1143	1145	1139	1138
Mean of Dependent Variable	0.061	0.50	0.53	0.35	0.56	0.59	0.41
Sample Ages (in 2003)				18			
Fixed Effects				Birth year x state			

Notes:

- Standard errors clustered at the municipality are reported in parentheses, Conley standard errors using a 100km cutoff are reported in square brackets, and Conley standard errors using a 500km cutoff are reported in curly brackets. (*** p<0.01, ** p<0.05, * p<0.1).

-"Rainfall shock" = 1 for individuals whose birth-year rainfall was more than one standard deviation from the 10-year historical locality-specific mean.

-All specifications include gender, household head gender and age, household size, household composition variables, parental education, parental language, and locality characteristics. Controls for parental language/education and locality distance include dummies for missing values.

employed in lower-skilled, intermittent jobs from the pool of employed individuals by using an indicator equal to 1 if an individual was employed and worked in a non-labourer job (and 0 if unemployed or a spot labourer). In the last 3 columns, we combine previous outcomes to create indicators for individuals either still in school or working. For instance, the dependent variable in column 5 is an indicator equal to 1 if individuals report either being currently enrolled or having worked that week.

An important takeaway from this table is the consistent pattern of coefficients across all columns: PROGRESA effects are either close to zero or positive, adverse rainfall effects are negative, and interaction terms are all positive. While none of the main effects of PROGRESA are statistically significant, the sum of this coefficient and the interaction term is positive and significant in columns 2 through 7. This indicates, as with the education outcomes, PROGRESA has statistically significant employment effects on those exposed to adverse rainfall at birth. Taken in sum, these findings illustrate the ability of school-aged CCT programs to offset the impacts of insults in early life, in dimensions not limited to school-aged outcomes directly incentivised by the program.

4.3 Robustness Checks

We run a number of checks to address concerns about selective fertility, attrition, migration, and imbalances across treatment and control villages. We discuss these in detail in Appendix section C. In short, we find no evidence that PROGRESA or birth-year rainfall shocks affected fertility (Table C2) or attrition (Table C3). Rainfall shocks do not appear to be correlated with various migration-related outcomes (Table C4). Our results are robust to the use of a trimmed sample that addresses the imbalance in the rainfall shock variable across treatment and control (Tables C5), as well as specifications that address the imbalances in other characteristics, including age (Tables C6 and C7).

We also investigate a number of alternate definitions of our main variables of interest. We find that our conclusions remain the same when we use a simple treatment indicator for PROGRESA (Table C8). In Table C9, we use a more flexible specification for our rainfall shock variable, allowing for floods and droughts to have different sized effects. We find that floods and droughts both have negative effects that are mitigated by PROGRESA exposure. The coefficients on droughts tend to be slightly larger in magnitude (though not significantly different from) the coefficients on floods.

Finally, when we include controls for rainfall shocks in the year before birth, the second year of life, and the third year of life (as well as their interactions with PROGRESA), we only see consistently significant main effects and interaction terms involving rainfall in the year of birth. All of these tables are discussed in more detail in Appendix section C.

4.4 Mechanisms

Having documented that negative rainfall shocks at birth affect educational attainment and employment outcomes, and that this effect is reduced by PROGRESA exposure, we now discuss why this might be the case.

One possibility is that parents have inequality averse preferences and when one child is disadvantaged (due to an income shock in their year of birth), they reallocate resources from other children. When schooling becomes more affordable due to a program like PROGRESA, this could result in parents choosing to increase the educational attainment for the disadvantaged child by more than for their other children.

It is difficult to identify the extent to which these kind of preferences exist. However, the evidence we do have does not provide strong support for this possibility. First, Appendix Table D5 finds no evidence that a child's exposure to a rainfall shock at birth affects the educational attainment of their siblings (in the absence of the PROGRESA program). Adding to this, the results in Appendix Table D6 show that a child's educational outcomes do not appear to be affected by their siblings' exposure to rainfall shocks at birth, nor the interaction between sibling rainfall and PROGRESA exposure. Interestingly, for the 18-year-old sample, we find that sibling rainfall shocks increase the probability of work and reduce the effect of PROGRESA exposure, which could indicate that the early-life experiences of children do affect the employment decisions of their siblings – particularly older ones. However, the coefficients on the main variables of interest (own rainfall, own PROGRESA exposure, and their interaction) are very similar to those estimated in the original specifications, suggesting this does not explain our main empirical findings. In these same regressions, we also explore how child outcomes are affected by sibling exposure to PROGRESA. Across all outcomes, there is little evidence that sibling exposure to PROGRESA (or its interaction with the child rainfall shock variable) affects education or employment outcomes.

While these results do not necessarily rule out resource reallocation across siblings as a mechanism for our findings, we are also interested in what could be driving our results in the absence of inequality averse preferences. That is, in a model that abstracts away from parental preferences regarding comparisons between their children, is there anything that could explain why PROGRESA improves educational attainment more for children born in years of adverse rainfall?

To answer this question, we extend the canonical schooling choice model in Card (2001) by allowing individuals to have heterogeneous initial endowments that affect future earnings. That is, the earnings function at period t is given by $y(\omega, S, t)$, which depends not only on years of schooling S but also the initial endowment ω. We describe the model in detail in Appendix section E and summarise the main implications in this section.

4.4.1 Rainfall as a Shock to Endowments

Rainfall shocks are incorporated into the model as a shock to the initial endowment. This is based on the evidence in Appendix Tables B1 and B2, which show that negative rainfall shocks at birth increase stunting and behavioural problems. We acknowledge that rainfall shocks in one year could affect the income-generating abilities of households in subsequent years, but we argue that the primary effects of birth-year rainfall shocks are concentrated in the first few years of life.[12]

We assume that rainfall shocks at birth do not affect current household income. This is supported by the evidence in Appendix Table D7, which shows that individuals who experience rainfall shocks at birth are not more or less likely to be classified as poor in 1997. In column 2, we also show that rainfall at birth is not significantly related to current household income.

In Appendix Table D8, we provide further support for the argument that early-life rainfall shocks capture a phenomenon that is distinct from contemporaneous household disadvantage. We estimate a regression that adds to our main specification a measure of household income and its interaction with the PROGRESA exposure variable. Specifically, we use the "poverty score" (which is increasing in household income) that is used to determine program eligibility measure in 1997. The results of this exercise show that the coefficients on our variables of interest (PROGRESA exposure, early life rainfall, and their interaction) are almost identical to the baseline results. In

[12]Bobonis (2009), for example, finds that household expenditures are affected by rainfall shocks in the previous year. Serial correlation would also imply that rainfall shocks in one year could lead to income effects in subsequent years, but – like other papers that test for serial correlation in rainfall shocks (Kaur, 2014; Shah and Steinberg, 2017) – we do not find that our rainfall shocks are serially correlated over time.

other words, the ability of PROGRESA to remediate early-life disadvantage is separate from any heterogeneous effects based on current household income. This is because current household income appears to be orthogonal to rainfall at birth (as indicated by the similarity between the rainfall-related coefficients in Appendix Table D8 and the corresponding ones in Tables 4 to 6). While there does appear to be some heterogeneity in the effect of PROGRESA by current household income for a subset of outcomes, this is separate from the heterogeneity based on early-life rainfall and does not affect our estimates of PROGRESA's ability to remediate for early-life disadvantage.

4.4.2 Theoretical Mechanisms for Remediation

Returning to the model, we assume that individuals have an infinite time horizon, attend school during the first S periods of life, and work full-time for the rest of it. While in school, the utility in period t depends on the level of consumption, $u(c(t))$, and the effort cost for the t-th year of schooling, $\phi(\omega, t)$. As we show in Appendix section E, this model allows us to predict how the optimal level of schooling should vary with the initial endowment and with a program like PROGRESA that offsets the cost of schooling. Importantly, the model also provides a mathematical expression describing how the effect of PROGRESA on optimal schooling will vary with the initial endowment. An inspection of this expression helps shed light on the primary mechanisms that could drive remediation.

First, the value of the PROGRESA transfer represents a larger proportion of foregone wages for low endowment individuals compared to high endowment individuals, as low endowment individuals have lower income potential, leading to a larger schooling response to the PROGRESA incentive among low endowment individuals. Second, because high endowment individuals obtain more schooling than do their low endowment counterparts in the absence of the PROGRESA incentive, it would be more difficult for a program like PROGRESA to increase the schooling of high-endowment individuals (vis-a-vis low-endowment individuals) if effort costs are convex in schooling levels. Finally, the shape of the earnings function also plays a role. If the initial endowment and schooling are substitutes in the production function and the marginal returns to schooling increase faster with the initial level of endowment, this would also contribute to remediation. This is because these two conditions imply that the rate at which the benefit of studying an extra year decreases is faster for high endowment individuals.

5 Conclusion

In this paper, we leverage the combination of two sources of exogenous variation – in early life circumstance and costs of schooling during childhood – to study whether (and the extent to which) it is possible to mitigate the impact of early life shocks. We find that a negative shock to early-life circumstance (adverse rainfall) lowers educational attainment and employment probabilities by young adulthood. However, exposure to the PROGRESA program helps mitigate these negative effects, indicating that remediation of early-life shocks is possible through government programs later in life. The magnitude of the interaction term is telling: in most cases, it ranges between 15% to 40% of the size of the main effect of rainfall, suggesting that cash transfer programs like PROGRESA have the potential to offset almost entirely the inequality generated by early life circumstances.

This study contributes to the large literature evaluating PROGRESA, and more specifically, to our knowledge about the program's ability to mitigate shocks. Two studies investigate the ability of PROGRESA to mitigate for contemporaneous weather shocks and find mixed results. De Janvry et al. (2006), who also focus on the education component of the program, finds that PROGRESA protects school enrollment from falling in response to contemporaneous weather-related income shocks. Aguilar and Vicarelli (2011), on the other hand, find no evidence that PROGRESA mitigated the negative health effects of El Nino flooding on young children, for whom the health component of the program was most relevant.

Our results also speak to the literature on cash transfer programs more generally (Behrman et al., 2011; Blattman et al., 2013; Haushofer and Shapiro, 2013; Schultz, 2004). While most evaluations of such programs tend to focus on average effects, we compare impacts across individuals with different early life experiences and find PROGRESA had a larger impact on those who experienced negative shocks early in life.

Supplementary data

The data and codes for this paper are available on the Journal repository. They were checked for their ability to reproduce the results presented in the paper. The replication package for this paper is available at the following address: https://doi.org/10.5281/zenodo.8206694.

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